

Volume 7, Issue 1 • June 2010



**INTERDISCIPLINARITY:
METHODOLOGICAL
APPROACHES**



<http://gjss.org>



Graduate Journal of Social Science June 2010, Vol. 7, Issue 1

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The Graduate Journal of Social Science (ISSN: 1572-3763) is an open-access online journal focusing on methodological issues of interdisciplinary relevance. The journal publishes two issues per year, one of which is thematic and one of which groups innovative and instructive papers from all disciplines. GJSS welcomes submissions from both senior and junior academics, thus providing a forum of publication and exchange among different generations engaged in interdisciplinary research. GJSS is published by EBSCO publishing.

For subscription inquiries, requests, and changes, please visit: <http://gjss.org/index.php?/Contact-us/Graduate-Journal-of-Social-Science.html>. or Email: gjss.editors@lse.ac.uk.

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Editorial

Inter/Trans/Post-Disciplinarity: Explorations of Encounters Across Disciplines

“What is needed are respectful engagements with different disciplinary practices, not ... portrayals that make caricatures of another discipline from some position outside it.”

- Karen Barad, *Meeting the Universe Halfway*

As new editors of the GJSS, we would like to use this editorial not only to introduce ourselves and to discuss the themes of the current issue, but also to take the opportunity provided with a change in editorial leadership to provide a retrospective of sorts. In the course of this introduction to the first issue of the new decade, then, we will explore: Where has the GJSS been? Where is it today? Where is it going? In so doing, we hope to provide the reader with an overview of some of the important and reoccurring themes of the journal, including the current issue on the broad topic of interdisciplinary methods and methodologies.

Interdisciplinary Foundations

In 2004, frustrated by the lack of institutional space for interdisciplinary exploration, graduate students at several universities formed the

GJSS ‘out of the conviction that different tools for the acquisition of knowledge should be confronted, compared and brought together in order to analyse the most complex aspects of our social reality’ (Leonelli 2004: iii). Six years on, the GJSS continues to work with this conviction, as it explores the transformatory implications of interdisciplinary dialogues, work and research on issues as wide-ranging as environmental policy, gender and mental health issues, and translation practices (of both the language and disciplinary variety) in Europe and beyond. The journal has covered the disciplines that are ‘inherently’ interdisciplinary (gender studies, queer studies and genomics) as well as the more traditional disciplines to which an interdisciplinary focus is more challenging (criminology, economics and biology).

Over the course of the six years of GJSS dialogue it has become

clear that when we are talking about inter-/trans-/post-disciplinary methodologies, we are not only talking about speaking across languages, but also through and among ontological and epistemological foundations (Bruusgaard et al, this issue; Peireria et al, 2009). We are concerned with reflecting upon the political and social implications of knowledge production and “its relations to action and social change” Liinason and van der Tuin, 2007: 1). We are recognizing the need for a “transfer from dichotomizations such as disciplinary/interdisciplinarity, empirical/theoretical as well as quantitative/qualitative” into a thematic organization of research and exploration (Liinason and van der Tuin, 2007: 8). And we are drawing from an understanding that, at its very root, inter-/trans-/post-disciplinary practice is about the “willingness to express a plurality of viewpoints, to mediate between different perspectives in a context-sensitive and overtly goal-directed way” (Leonelli, 2005: 1). If we take these gestures to their ultimate conclusion, we are talking about alliance politics – building alliances across barriers. In working with and across such boundaries through a recognition of what distinct standpoints have to offer, the GJSS is not masking the chasms that lie between them. Instead, the goal is to acknowledge how those divisions may become sites for productive inter-/trans-/post-disciplinary dialogue; to challenge the

secluding tendencies of traditional academic practice by critically addressing the possible difficulties or incongruities that turned them so in the first place; embracing those tensions as sites of potential opportunities and correspondences.

Challenges and Charms: Entering the Second Decade of Interdisciplinary Investigation

We open the issue with Marina Franchi’s review of the seminar, “Interdisciplinarity: Desire and Dilemma in Contemporary European Gender Studies.” The seminar, held at the Gender Institute of the London School of Economics, featured several up-and-coming academics in the field of European Gender Studies, including former GJSS editor Mia Liinason. The questions posed and themes explored at the seminar on the ongoing debates around the meanings and practices of interdisciplinarity echo those posed by this and former issues of the GJSS: Is there a limit to interdisciplinarity? What are the political and social implications of interdisciplinary practice? How can the (feminist) objectives of interdisciplinarity in gender studies be recognized in today’s (neo-liberal) political climate? Beyond these important questions, the review also reminds us of the need to critically examine the ways in which we label “interdisciplinary” practice, as the panellists pointed out one of the

“paradoxes” of interdisciplinarity in its use as a “buzzword” in European higher education policy; a rhetorical integration which, in practice, can cement powerful divisions. Franchi relates such concerns to an essay by Sabine Hark in Vol 4 (2) of the GJSS, viewing the use of inter- or transdisciplinarity as a “magic sign” or “empty signifier” whose meaning is dictated according to positionality and power of interdisciplinarity in the academic setting (Hark, 2007). The review which commends the challenging framework of the conference therefore suggests a pressing and persistent need in academia to similarly engage and question the terms through in which interdisciplinarity is being debated and put to practice.

In the first essay of the issue, Delia Dumitrica explores the power struggles inherent in the very practice of choosing a methodology as a graduate student, in “Choosing Methods, Negotiating Legitimacy: A metalogue on autoethnography.” The innovative use of the nascent method of auto-ethnography allows Dumitrica to present a “metalogue” between a graduate student and advisor in which she explains the importance of auto-ethnography as an interdisciplinary practice. Here, Dumitrica’s work draws important links between method and writing, as it highlights how concerns with writing form and style, including language of dialogue and reflexivity become part of the overall method.

Further, in exploring academic power through the intersections of disciplines, departments, universities, and individuals, Dumitrica marks the method of “autoethnography as a site of struggle for and against power in terms of knowledge production” (Dumitrica, this issue), and highlights the complexities, difficulties and possibilities of engaging with interdisciplinarity.

Exploring the complexities, difficulties and possibilities of interdisciplinary research is the goal of Emily Bruusgaard, Paula Pinto, Jennifer Swindle, and Satomi Yoshino’s article, “Are we all on the same page? The Challenges and Charms of Collaboration on a Journey through Interdisciplinarity.” A reflection on the practice of interdisciplinarity research in a group setting, Bruusgaard et al use their experience in a Social Sciences and Humanities Research Council of Canada (SSHRC) funded project as “a valuable starting point for the production of knowledge about theories and concepts, as well as about the social practices and relations that we study” (Peiria et al, 2009: 4), much like the editors and contributors of the last GJSS Special Issue, *Lost (and Found) in Translation*, who looked beyond viewing issues of translation (including translation across disciplines) as a “problem to be solved.” The authors, hailing from different disciplines themselves (Human Ecology, Nursing, Sociology and English), transform their experi-

ences into “lessons learned” that would be valuable in embarking on any project that seeks to take interdisciplinarity seriously. Among other aspects necessary for engagement across disciplines, Bruusgaard et al cite the acknowledgement and acceptance of differences “from the outset” as crucial to an interdisciplinary effort built on “mutual trust and respect.” This trust and respect is called for by Karen Barad (see the opening quote to this editorial), and is akin to that called for by Donna Haraway, in her concept of “situated knowledges” (1988) and her more recently- elaborated practice of “diffraction” (1997; 2008).

These authors are important to mention here not only for their dedication to engaging across disciplines with “mutual trust and respect”, but because they are both dedicated to broadening interdisciplinary work beyond the traditional focus in the social sciences and humanities and into the natural sciences, something that Bruusgaard et al note was lacking in their own engagement, as all project team members were from the humanities and social sciences. Barad (2007: 93) offers transdisciplinarity as a possible avenue to achieve a more profound interaction between disciplines, suggesting that “unlike multidisciplinary or interdisciplinary approaches, a transdisciplinary approach ‘does not merely draw from an array of disciplines but rather inquires into the histories of the organization of knowledges

and their functions in the formation of subjectivities... mak[ing] visible and put[ting] into crisis the structural links between the disciplining of knowledge and larger social arrangements’ [citing Hennessey 1993: 12]”. Similarly, Bruusgaard et al’s understanding of transdisciplinarity is that it transcends the traditional boundaries of interdisciplinarity by putting the “humanities into a natural, social and health sciences context” and vice versa. And, while the authors close by noting that such an element was not present in their own project, they agree that this is something that they aspire to in future cross-disciplinary interactions.

Working in the tradition of Haraway and other feminist science scholars (notably Londa Schiebinger), Rachel O’Donnell’s essay “Imperial Plants: Modern Science, Plant Classification and European Voyages of Discovery” offers an interdisciplinary review of literature on botanical classification and European colonialism. In so doing, O’Donnell explores the ways in which science, nature, and gender were co-constituted during the height of European colonialism. O’Donnell’s review makes clear that, in exposing the connections between politics and science, what is at stake is nothing less than the power to create knowledge (and who has it and who does not). Further, O’Donnell argues that recognizing such connections is not only historically important, but critical “in light of

contemporary biotechnological efforts and international development practice” (O’Donnell, this issue).

The “charms and challenges” of interdisciplinarity are taken up in four book reviews that close out the issue, expertly edited by Katherine Harrison. Hilde Jakobsen reviews Monique Hennink’s *International focus group research: a handbook for the health and social sciences* (2007), providing a useful overview of the ways in which focus groups can be used to their potential, while noting some of the method’s shortcomings. The next two reviews take a look at recent work from a more transdisciplinary approach. First, reviewing Teresa Ortiz Gómez’s *Medicina, historia y género. 130 años de investigación feminista* (Medicine, history and gender: 130 years of feminist research) (2006), Agata Ignaciuk offers a review of the work of “one of the pioneers in applying and teaching feminist interdisciplinary methodology in the field of history of medicine and science in the Spanish context.” Second, Beatriz Revelles Benavente reviews Karen Barad’s *Meeting the Universe Halfway: Quantum Physics and the Entanglement of Matter and Meaning* (2007), offering readers a brief glimpse into the complex work of the feminist physicist philosopher and examining the ways Barad’s work has been taken up in new materialist theory. Finally, Francois Briatte’s review of Jonathon W. Moses and Torbjørn

Knutsen’s, *Ways of Knowing. Competing Methodologies in Social and Political Research* (2007), focuses on the distinctive historical approach to methodological inquiry advanced by this text. Specifically, it underscores the relevance of tracing the intellectual and philosophical lineage of social science disciplines, and their associated methodologies, in order to situate the current divisions, connections and debates emanating from them.

Beyond Interdisciplinarity?

As the founding editor of GJSS stated six years ago, “Discourse over interdisciplinarity is thus an essential, if largely unrecognised, part of academic life, insofar as it encourages the necessary flexibility of boundaries and connections among disciplines” (Leonelli 2004: iii). As we have seen in this and past issues, the boundaries between disciplines are indeed unclear. This issue continues to blur the remaining boundaries, asking: how can we make cross-disciplinary encounters more productive? What new methods might lend themselves more readily to cross-disciplinary engagement? And finally, should we move past “interdisciplinarity” into a “trans” or “post” disciplinary world?

Bruusgaard et al. conclude their essay by stating that “we do not yet consider ourselves to be transdisciplinary, but we do believe that this is the path on which we are headed.”

Like the members of the SSHRC team, we hope this issue of the GJSS takes us further along on our voyage toward this goal. We also want to acknowledge that there is no clear path on this road, and that we might not even want to move toward yet another category – even one as seemingly flexible as “trans-disciplinarity.” Ultimately, however, we are eager to continue toward a place where all interactions across disciplines have as their base “mutual trust and respect.” We open this issue, then, with the words of Gloria Anzaldúa, one border-crosser who has inspired us both:

*Caminante, no hay puentes,
se hacen puentes al andar.*

(Voyager, there are no
bridges, one builds them as
one walks).

Acknowledgements

We would like to thank both the editorial board and the reviewers. We are indebted to the entire GJSS Editorial team for their commitment, dedication and hard work. We also offer enormous gratitude to the anonymous student reviewers and academic reviewers, who offer their time and energy to help ensure that articles submitted to the GJSS are reviewed to the highest standard. Gwendolyn would like to acknowledge Max Tremblay, at the New School for Social Research in New

York, and the other members of the Spring 2010 feminist theory reading group of which she was a part, and whose discussions of Donna Haraway’s and Karen Barad’s work led to many fruitful and animated conversations on ways in which the “hard” and “soft” sciences can better “intra-act” with one another. Both Melissa and Gwendolyn thank the London School of Economics, not only for being a wonderful institutional home, but for offering institutional support for the journal by hosting the editorial email account and providing many other small but important day-to-day necessities. Last, but certainly not least, we thank our former editor, Mia Liinason for her guidance during our initial time as editors here at the GJSS. We are inspired by the work of Mia and the other GJSS members who have come before us and we hope that we can continue on in the tradition that has made the GJSS a home for graduate students who seek to critically engage with inter/trans (or post!) disciplinary methodological inquiry in the social sciences. Any editorial mistakes are ours alone.

**Gwendolyn Beetham, New York
Melissa Fernández, London
May 2010**

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Interdisciplinarity: Desire and Dilemma in Contemporary European Gender Studies

Thursday 21 January 2010
Gender Institute Research Seminar

Marina Franchi

Key Words: interdisciplinarity, gender studies, Bologna process, Higher Education, research, GJSS

On January 21st the Gender Institute¹ at the London School of Economics (LSE) held a seminar titled *Interdisciplinarity: Desire and Dilemma in Contemporary European Gender Studies*. Interdisciplinarity is one of the current key terms within the field of Feminist, Women and Gender Studies. Although the term is ubiquitous, a single definition remains elusive, and debates around the meanings and practices of interdisciplinarity are ongoing. The structure of the research seminar fit perfectly within these contemporary and contested understandings.

A glance at the curricula of the three scholars on the panel, Maria do Mar Pereira, Sabine Grenz and Mia Liinason, shows how they have all thoroughly engaged with the issue. Mia Liinason is a PhD student at the Centre for Gender Studies at Lund University. She was one of the editors of GJSS in 2007, when the

Journal dedicated a special issue to feminist interdisciplinarity. In the editorial of that edition, she and co-editor Iris Van der Tuin reflected on the importance of interdisciplinarity within women's and gender studies. Sabine Grenz holds a PhD in Gender Studies and her research interests include feminist criticism of science, the history of sexuality, prostitution and masculinity. The discussion was led by Maria do Mar Pereira, PhD student at the Gender Institute at London School of Economics whose research focuses on the epistemic status of women's, gender, feminist studies in Portugal.

The three scholars engaged in a rich and full hour of discussion, pushing the audience to reflect upon the term and practice of interdisciplinarity. Never missing the wider picture, the panel guided the audience through their "personal career trajectories" highlighting the

point at which the concept of interdisciplinarity became relevant both for their work and for their definition as scholars. Academia operated as both the site in which one shapes her own expertise, and where one meets the criticism to a given set of practices.

From the beginning, the panel tried to unpack the “buzz word” of interdisciplinarity, a term not confined within methodology chapters but which, as Liinason has previously pointed out, has become “a buzz-word in the current higher education policies of the European Union” (Liinason, 2009: 52). The panel provided the audience with an interesting “panoramic view” of how interdisciplinarity became valued within the European Union policy-making process. Focusing on the Bologna process of harmonization of higher education in Europe, they discussed how different countries coped with the request for interdisciplinarity that the European Union put forward. During this process, the ‘buzz word’ became a necessary skill for maintaining a competitive position in the research market².

After an overview of the policy use of interdisciplinarity, Maria do Mar Pereira invited the panel to think through interdisciplinarity as a paradox. As described by Sabine Hark in *Magical Sign: On the Politics of Inter- and Transdisciplinarity* (published in the above mentioned issue of *GJSS*), the “magical sign” of interdisciplinarity is, paradoxi-

cally, used both by critical scholars and neo-liberal inspired European Higher Education Reforms. “Hence, one could indeed argue that inter- and transdisciplinarity function like magical signs (Katie King 1994), that is, as empty signifiers meaning whatever their users want them to mean.” (Hark 2007). The panellists made clear how the neo-liberal definitions and aims produced through the Higher Education policy debates hugely contrast with the definitions and practices of interdisciplinarity that flourished within Queer Studies or Postcolonial studies.

When the discussion moved to the core of the topic: the field of Gender Studies, the audience was presented with another paradox of interdisciplinarity. The panel provided insightful examples of practicing interdisciplinary research, while at the same time discussing “the paradoxical position of disciplining a field of research and education we have proudly dubbed inherently interdisciplinary” (Holm 2003). In what I personally consider the most appealing part of an utterly intriguing talk, the focus on Gender Studies led to a interesting reading both on the practices of the field, and on the narratives that permeate those practices. The speakers explained that, in the last few decades, Gender Studies - the discipline that used to occupy a space within various departments, and hence was “inherently interdisciplinary”- acquired a “physical” independent status through the con-

tinuing growth of departments and programs. Disciplinary boundaries were produced as those physical spaces were defined, leading to the ultimate paradox: the interdisciplinary Gender Studies becoming a discipline. As a result, Gender Studies is beginning to face many of the same disciplinary constraints of the “traditional” disciplines. These constraints resonate with the patriarchal organization of knowledge, a foundational critique of gender studies itself.

Overall, the panellists demonstrated amazing command of the literature, which allowed them to avoid the often-observed ‘short-cut’ of giving ready-to-use answers to the recurring questions within the field. Instead, the panellists provided an appealing picture to the debate, while also challenging the audience to nail and unpack the above-mentioned paradoxes. Those present were left with stimulating questions to reflect upon, questions which resonate with those posed by many of the contributions to the *Graduate Journal of Social Sciences*: Does one need to be grounded in a discipline before ‘moving’ to interdisciplinarity? Is there a limit to interdisciplinarity? Is it accidental that these debates are primarily taking place in Gender Studies?

The debate is, luckily, still open.

Endnotes

¹ The Gender Institute GI at the London School of Economics is undoubtedly one

of the leading European institutions in the field of Gender Studies in Europe. Along side the vibrant post graduate teaching programmes it is characterized by a diverse research tradition. The latter is mirrored in the Research Seminar Series that the GI runs throughout the academic year. The Series provides the academic community with the chances to meet and discuss the later work of scholars both from within and outside the Institute. Looking at the 2009/10 programme it appears evident how the topics addressed in the series reflect the key contemporary debates within the field of Gender Woman and Feminist Studies.

² The Bologna process has been at the core of speculation within Gender and Women’s studies. Clare Hemmings in 2006 in the *European Journal Of Women Studies* (EJWS) discussed the opportunities that the Bologna process was holding for UK Women and Gender Studies. In the same issue Mary Evans appeared more sceptical and less optimistic (2006). In 2008 on EJWS Clare Hemmings resumed the debate and published a interesting note on the Bologna Process in which she suggested “ways forward for women’s and gender studies in its negotiation with European institutionalization of the field” (2008:119).

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Choosing Methods, Negotiating Legitimacy. A Metalogue on Autoethnography

Delia D. Dumitrica

For a doctoral student, choosing a research method is not a simple, rational act. It is an act that involves an assessment of our position and power within the academic setting, as well as a negotiation of the legitimacy of the method. It is also an act of expressing our values and political commitments. Thus, this choice becomes an opportunity to investigate the ways in which power relations may come to shape both our understandings of 'legitimate research' and our performance of that legitimacy. This paper looks into these issues by means of an imagined dialogue (a metalogue) between a student and a supervisor on the possibility of choosing autoethnography as a method for a doctoral project. As a contested method located within the qualitative paradigm, autoethnography allows me to explore the question of what makes a method a legitimate way of inquiry within the academic context. My interest here is to show how the networks of power within which I am positioned as a doctoral student, with a particular set of values and commitments, come to play into the negotiation and performance of the legitimacy of the method. Using Foucault's discussion of power/knowledge, I am arguing that such networks of power are both external to me, constituting the institutional context within which I am acting, and part of my own self, shaping my values and my performance as an authorized speaker within the academic setting.

Key Words: autoethnography, methodology, qualitative research, doctoral dissertations, legitimacy, power/knowledge

As a graduate student, I have often been advised to choose a method that is able to tackle the research question I am asking. Yet, this choice is "something that reaches into the assumptions about reality that we bring to our work" (Crotty 1998, 2). In other words, this choice is not only a simple, rational act of matching your research question to the ways in which you will investigate it. It also involves your worldview,

your beliefs about the nature of society and the ways in which it can be known. But acknowledging this is often at odds with the 'regime of truth' (Foucault 1977/1980b, 1997/1995) of our modern world that equates truth with science. In this equation, the latter stands for rational and rigorous testing that can explain the nature of things (Fay 1992; Hamilton 1992; Hollis 2002). To the extent that this regime of truth has become part

and parcel of the network of power in modern societies, the choice of a method within the framework of a doctoral dissertation becomes interwoven with the politics of the disciplines and of the wider academic setting. The purpose of this paper is to look into this process by means of an imaginary metalogue on autoethnography. It is contended here that such an enterprise may be able to shed some light on the ways in which academic settings (schools of thought, disciplines, departments, universities) and personal contexts intersect, marking a method such as autoethnography as a site of struggle for and against power in terms of knowledge production.

Autoethnography is a qualitative research method that takes the researcher/ author as the subject of research (Denzin and Lincoln 2002; Ellis 2004; Richardson 2000, 2002). Autoethnographers reflexively examine their own feelings, meanings and understandings of the social world in order to “connect the autobiographical and personal to the cultural, social and political” (Ellis 2004: xix)¹. Thus, researchers are both the subject of their own analysis, and the analysts examining the data to understand wider social dynamics. The method is firmly rooted in a constructivist epistemology (Crotty 1998), retaining a strong commitment to critical social science. Although related to a variety of qualitative methods, such as critical ethnography, reflexive eth-

nography or performance narrative (Denzin and Lincoln 2002), autoethnography is different from them in that the only empirical data used to trigger the critical analysis is the researcher’s own experience.

As a method, autoethnography is also contested primarily for its lack of theory, its relation to subjectivity and its forms of writing (Denzin & Lincoln 2002; Ellis 2004; Holt 2003; Sparkes 2000, 2002). It is mostly the charge of being “too personal” that challenges autoethnography’s legitimacy as “proper academic research” (Sparkes 2000, 2002). These accusations are also made against other qualitative methods, such as ethnography, in what Alexander has described as “residual ideas of truth and objectivity [that] remain stubborn features of much ethnographic research and writing on ethnicity in Britain” (2004: 137). As such, contesting autoethnography may be seen as part of the wider ongoing debates between quantitative and qualitative approaches. Autoethnographers respond to such accusations by relying on the repertoire provided by constructivist epistemologies, building on established critical reflections on the status of knowledge and the role of the researcher vis-a-vis the production of (academic) knowledge.

A possible reason why the legitimacy of the method is more problematic in the case of autoethnography (as compared to, say, ethnography) may have to do with autoethnogra-

phy's relative newness. While a historiography of the method remains to be written, autoethnography has been established as an academic method primarily through the work of Carolyn Ellis and Art Bochner in the early 1990s (Anderson 2006; see also Ellis 2004)². Although now autoethnographers often draw from feminist epistemologies (e.g. Code 1991; Collins 1990; Haraway 1988; Harding 1991), postcolonial theories (e.g. Bhabha 1994; Spivak 1999; for a discussion of autobiography and postcolonial theory, see Huddart 2008), sociology of illness (e.g. Frank 1991, 2004) and the 'cultural turn' in anthropology (e.g. Geertz 1983; Clifford and Marcus 1986), Ellis and Bochner were located within the field of sociology and thought of autoethnography in the context of symbolic interactionism (see Anderson 2006; Ellis 2004). The history of autoethnography is also intrinsically connected to methodological debates in anthropology and to the use of personal narratives in traditional social science (particularly anthropology and sociology); the word 'autoethnography' was coined by an anthropologist, while the term 'autobiography' was used in literary studies to designate a specific writing genre (Ellis 2004). Thus, it is fair to say that autoethnographers reclaim historical origins that often disregard (and thus challenge) disciplinary boundaries. This trend continues, as autoethnographers cross disciplinary boundaries

and borrow from a variety of theoretical frameworks to legitimize their choice of method and to frame their approach to the research problematic. Most importantly however, these scholars 'perform' the legitimacy of the method, by submitting their work to peer-review processes and publishing it in academic journals and books (Bochner 2002; Denzin 2006; Ellis 1993, 1997, 1998, 2004; Holt 2002; Richardson 2002; Sparkes 2000, 2002). In this process, autoethnography's contested position presents an opportunity to inquire into the power dynamics through which the 'academic norm' becomes constructed and perpetuated.

In this paper, I offer a personal account of what the choice of autoethnography as a method may look like from the point of view of a doctoral student. My own take to the method cannot be divorced from both the 'politics of the method'³ (Frank 1983; Clifford and Marchs 1986; Eisner 1988; Gitlin et al. 1989) and my own position within the academic system. Informed by Foucault's discussion of power, discourse and authority (Foucault 1966/1970, 1972, 1976/1980, 1977/1980, 1977/1995), I begin by asking 'what is autoethnography and why is it such a contested method?', only to realize that this question should be situated within a larger context of inquiry which includes asking: what constitutes 'academic' knowledge; who grants it legitimacy; and how am I, as a student, relating

to these processes in my own work? My methodological choices, as well as my ambivalence on autoethnography appear as part and parcel of the networks of power within which I am trying to position myself as an academic in the hopes of gaining access to the higher-education professions and to positions of authority within disciplines or departments. Such networks of power are at the same time external to me (such as the relation with supervisors, professors, reviewers etc.) and part of my own self (such as my professional goals, values and worldviews).

Instead of tackling these questions within the format of the traditional academic paper, I propose to look at them by means of a personal - yet imagined - narrative: a metalogue between a student and a supervisor⁴. It is fair to point out from the beginning that the two characters come to be quite unequally constructed: while the student is filled with doubts and uncertainties, the supervisor appears to have moved beyond such dilemmas, expressing only a pragmatic attitude to the dissertation writing process. My intention was not to pit an enthusiastic and ethically troubled student against a pragmatic supervisor, worn out by the vicissitudes of the system. Rather, these characters should themselves be understood as part of my own position in (as well as understanding of) the power networks within the academic system. In my case, being a doctoral student

is not an experience located entirely within the 'student' arena: I am also a sessional instructor, teaching my own courses. In this position, I am required to constantly shift between being a student and teaching students. This may be interpreted as a self-disciplining process, through which I internalize the norms of academic scholarship and evaluation as both a student and an evaluator. Yet, just as Foucault has noted, such processes of self-disciplining are never smooth: they are also the moments of resistance, or, in my case, of ambivalence, uncertainty and questioning. In this sense, autoethnography has allowed me to both acknowledge and reflect upon this process, and preserve its emotional depth.

The discussions presented here have been part of my academic experience. My own graduate background is interdisciplinary, which may be one of the reasons why I have not engaged here with a specific social science discipline. In my own doctoral research, I am located within a communications studies department. The field of communication is itself contested and interdisciplinary, drawing its theoretical positions from a variety of social science thinkers (see Craig 1999). Many of the discussions below rest on insights from this field, along with cultural studies, sociology and anthropology. Of course, this is also a major limitation: trying to keep the discussion on a more general level

leads to glossing over the particularities of methodological debates in specific disciplines. Yet, I do not think this undermines the validity of this autoethnographic exercise: the major contemporary theoretical and epistemological debates in social theory have a meta-disciplinary aspect (e.g. Delanty 2000)⁵. As already indicated, the historical contexts reclaimed by autoethnographers, as well as the use of autoethnography has always involved such meta-disciplinary theories and epistemological debates. This does not preclude the fact that autoethnographic projects are employed and legitimized within the context of specific disciplines⁶.

While this paper deals with the networks of power within which I find myself as a doctoral student, it also represents an act of direct engagement with them. After all, I am writing a paper for the purpose of publishing it within an academic journal and my ability to do so comes from being part of this expert system (Giddens 1990). For this reason, the paper takes the form of a metalogue, which is a “conversation about some problematic subject” (Bateson 1972, 1) in which both the topic and the form invite the writer and the reader to navigate between layers of understanding and order. As a submission to a peer-reviewed journal, I also had to compromise on the metalogue: although part of the paper is written in a nonconventional, dialogical format, the other

part follows some of the “rules” of academic writing, such as resting a case upon prior academic literature or the citation style. Where traditional academic writing insists on the separation of the author/ text, logical sequencing and (linear) flow of the argument, a metalogue is a personal story where the argument does not necessarily follow a well-rehearsed path (from premises to conclusions). Its role is to reveal the complexity of the problematic, provoking readers to make sense of it through their own frames. This is by no means something new; for instance, post-modernism has challenged the traditional author/ reader positions, arguing for the need to develop a new aesthetic of academic writing that takes all texts as ‘oriented’ by the intentions and contexts of its producers and readers (e.g. Hutcheon 1983).

By taking this form, the paper allows me to follow more closely my thinking flow, which often times has a tree-like structure simultaneously branching in various directions. It also allows me to bring forward the values that accompany specific ideas, exposing the feelings, questions and uncertainties brought along by the act of choosing a suitable method. This personal struggle is an often- ignored aspect in academic publications on methods⁷. Yet, the selection of a method remains an important mechanism of situating oneself within particular schools of thoughts and disciplines.

The metalogue is thus able to contextualize a reflection about autoethnography within a view from below of the 'politics of the method' (Clifford and Marcus 1986; Eisner 1988; Frank 1982; Gitlin et al. 1989) and the specific emotional space that accompanies such politics. The choice and understanding of the method, together with the emotions that accompany these processes, are means by which we insert ourselves into the complex networks of power that make up the social world in which we exist.

* * *

Student: I have finally found a method for my dissertation that really suits me. I would like to do an autoethnography!

Supervisor: Autoethnography? Let me remind you that you will present this work to a defense committee. You need to be cautious of such highly subjective methods... they may be inspirational, but they are hard to defend. Besides, if you want to become a scholar, you have to learn to distance yourself from your own beliefs. With an autoethnography, you can only talk about your own beliefs, your own views. And that's the problem right there: if it's about you, it cannot be empirically falsifiable (Popper 1965).

Student: Why is it such a bad thing if I am the object of my own inquiry? Autoethnography would re-

ally work for me, because my own research is driven by my personal background. I should acknowledge that, shouldn't I? My project deals with identity issues. Doesn't it seem strange to talk about identity as if it's something that the researcher can study, without her own identity to come under microscope? Many autoethnographic projects deal with identity questions, precisely because this method gives the researcher an avenue to question how their own identity comes into play in the research process and then connect this to wider social structures (Ellis 1998; Richardson 2000; Sparkes 2000, 2002; Stapleton and Wilson 2004).

I read this autoethnographic piece about Asian women who married US servicemen after the Second World War and came to live in the US. Initially, the researcher wanted to map the problems these women encountered in the US. But she was also the daughter of one of them, so she was afraid that her own identity would "bias" her research. Her fear made her "overlook[...] the possibilities for exploring what a more self-reflexive ethnographic representation might look like – one based upon a lifetime of talk story with [her] mother and her circle of friends." (Creaf 2002, 80). In the end, she did an autoethnographic project where her own life became the lens through which the stories of these women were linked to the wider social structures in which they

lived. It was this personal lens that allowed her to tell the story of how identity and race feel like within those structures. This also allowed her to question her own relation, as both a researcher and a daughter, to the subjects of her research. She did not produce yet another account where identity was reduced to numbers and cases to be examined. With her mother becoming “her most willing chief informant”, both author and readers are prompted to question their own ethics of researching and consuming ‘the other’. As she narrated identity and race, we, as readers, re-constructed and lived them through her. The personal lens forced her to question the ways in which writing as an outside researcher transformed these women into ‘cases’ and ‘objects’ of research, further denying them their individuality and agency.

Supervisor: Well, it seems like an interesting story. But this is also the source of the problem: it sounds more like a story and less like a piece of research. Unless autoethnographers are part of your committee, this may get you in trouble. The committee members may not share your enthusiasm for this method. What will you do when they will ask about the generalizability of your results? What can you do to defend a project where the method through which your results are reached is under question? No, I do not think autoethnography is a good idea.

Not to mention that it will be very hard to get any funding for such a project. Grant-giving agencies want to see reliable results, that can be extended and used. You have to be more strategic here and think in terms of your final goals: to write a defensible dissertation that will get you what you need for now, the doctoral title.

Student: I know, I do want to write a good thesis. But I feel I owe it to myself to stay true to who I am and how I insert myself in my own research, because to “know an object without considering the way [I] participate in the production of that knowledge” (Gitlin et al 1989, 245) seems a bit unfair to me⁸. I do not want to write a thesis fearing the committee won’t like it. I want to write a thesis that I feel brings something new, and most importantly, addresses social inequality and structural oppression. I am motivated by strong feelings here. I start from a political position, and it seems only fair to acknowledge it and incorporate it in a reflexive manner in my research, don’t you think? Why is it that if it’s a personal story, it is suddenly less defensible? What makes a thesis defensible anyway? Just because you follow the ‘standard’ research steps it doesn’t mean your personal story is not inserted into the whole research project. It’s not as easy as coming up with a research question that can be investigated, defining its terms and building a methodological

design that can address the question; making sure the design is replicable to ensure results are reliable. Then, crafting a clever argument as to why only this research design really works for my specific question (hence, why others do not work). And finally, doing the research and presenting the findings in a concise and clear manner, by connecting them to the theory I have used (Iowa State University n/a). As long as the method is rigorous, the conclusions are defensible!

I am a qualitative researcher and I am espousing a particular political stance. I think this is how I can defend the method if the committee and I do not see eye to eye on the legitimacy of this method⁹. Autoethnography is only another form of the “reformist movement” in social science research introduced by qualitative research from the 1970s on (Denzin and Lincoln 2002). If I position myself firmly within this paradigm and within a constructivist epistemology, then shouldn’t this be enough to make a strong case for my choice of method?

Supervisor: There is a difference between making a strong case for your method and the acceptance of that method as legitimate. Remember that legitimacy is contextual: you try to establish it in relation to the prevailing forms that are considered legitimate. Writing autoethnographies for doctoral projects remains quite rare, and I have never

sat on any committee evaluating an autoethnography. Yet, in my experience, the method is one of the most scrutinized aspects of your research project. You may position yourself as a qualitative researcher, but you are still doing a research project and you are still writing an academic dissertation. A thesis where you are both the author and the object of inquiry seems self-indulgent (Sparkes 2002). It comes into conflict with some of the most entrenched values of academic work: the ability to arrive at conclusions by means of a rational argument that can be explained and then tested by logical means. Autoethnography may be a qualitative research method, but it remains contested even within (qualitative) ethnography (see Anderson 2006; Atkinson 2006).

Ethnography is in fact a good example here. Ethnographic accounts existed before the method itself became accepted as scientific. But scholars persuaded the academic communities that ethnography can be done in a scientific manner if it uses narrative realism. To the extent that a description remained true to what people were doing, then ethnography was a reliable and scientific method. Thus, the earlier accounts were dismissed as ‘literature’ and the author became absent from the descriptive account he provided (Clifford and Marcus 1986; Gitlin et al. 1989). Thus, the qualitative shift you talked about earlier also affected debates on ethnogra-

phy. From the 1960s on, we have witnessed an increased recognition that no description is independent from its interpretation and that the author/ researcher is always using her own perspective in describing something (Gitlin et al. 1989). In this shift from description to questioning how people make sense of things - and how researchers intervene in this process- ethnography moved from being considered a descriptive method to being evaluated on the basis of the 'thick descriptions' and constant symbolic translation it achieved (Geertz 1983). Yet not even these discussions completely opened the door to embracing autoethnography, as the question of analysis remains a contentious issue (on similar questions around the evaluation of ethnography, see also Clifford and Marcus 1986). How is analysis to be done? What constitutes a good, academic autoethnography? Is autoethnography to be used in an evocative manner, to emphasize the journey and to expose the flow of "lived experience", without engaging in its direct analysis (Ellis and Bochner 2006)? Or should autoethnography be an analytical tool, "committed to an analytic research agenda focused on improving theoretical understandings of broader social phenomena" (Anderson 2006, 375)?

Student: The legitimacy of this method is what seems to be in my way here. How legitimate is autoethnography? How is this legitimacy

being constructed? To what extent will the committee members see it as an established method or reject it as non-academic or self-indulgent. Should I understand that, in spite of the qualitative turn, the debate is still one about objectivity, reliability and validity?

Supervisor: It is a question of legitimacy. You know, "each society has its regime of truth [...] the type of discourse which [society] accepts and makes function as true" (Foucault 1977/ 1980b, 131). It is this regime of truth that provides us with the criteria for deciding what can count as 'truth'. Or, in our case here, as a method to access the 'truth' about social reality. To a certain extent, the legitimacy of a method is still measured against this 'regime of truth'. Of course, what one takes to be the 'regime of truth' depends on one's epistemologic and disciplinary position. For example, an understanding of 'race' as a biological category is considered as a fallacy from a constructivist point of view. These 'regimes' are not something immutable. They do change as they have to always respond to criticism stemming from new social circumstances.

Student: In my thesis and in my defense, I need to prove that I know the 'regime of truth' and the criteria it imposes. This would authorize me as a speaker within the academic setting (Foucault 1972). To a cer-

tain extent, this is what I think the defense is all about: prove I master the rules of the game, the intellectual legacy of my discipline and the debates around my chosen method. And that I am able to combine them, so that I come up with something new and original. I should be honest and admit that I do want to get the 'doctor' title!

Supervisor: That is exactly what I am trying to tell you: that you need to think in advance about your defense and about your career. I may be too harsh on autoethnography here, because there is a lot of room for the author/ researcher in the qualitative paradigm, especially when compared to positivist epistemologies. But with autoethnography it's almost like the boundaries of this qualitative paradigm are being pushed a bit too far. I guess it reads too much like literature (Richardson 2002, 39-50; see also Clifford and Marcus 1986)! Nobody says you should not be reflexive about your own position as a researcher. Insert a section on this in the methods chapter! But to make it into the method itself, I am not sure about that.

Student: It's true that I've also wondered about the whole literary aspect. I mean, I have a hard time confronting my own "academic" self, whispering in my ear that my writing doesn't even count as poorly written fiction, let alone academic work! But then I'm back to my earlier question

about legitimacy: what counts as academic work and why?

Let's take what you said that autoethnography may read just like literature. The good part of it is that it makes academic research more accessible to people. Geertz said that the power of a text comes from its ability to move the reader, its horror as a lived case and the morality it carries (Geertz 1983, 36). Academic texts are not supposed to make you cry, organically scare you or psychologically disturb you! But it is precisely those pieces that are able to move us while at the same time bringing up the social dynamics in which we live that make us more critically engaged with these dynamics (Ellis 1997). Some scholars want to recuperate this evocative power, and this is where they locate the strength of the method (Ellis and Bochner 2006). For others, this evocative power has to be accompanied by a theoretical reflection that enables us to simultaneously construct and question our own meanings, as well as the problems they bring to light (Anderson 2006; Atkins 2006). The personal narrative layer is like a drawing in which you produce line upon line thus creating "layered accounts [which] leave traces of a play of differences for other selves who read to apprehend. This, in turn, makes it possible for selves to identify with other selves, bringing us closer together in the understanding that we are all the same, located in different

positions in the play of difference that is existence” (Ronai 2002, 123).

Supervisor: Well, you are not writing a novel here, but a piece of research. How will the committee evaluate it? Aesthetic qualities aside, a thesis is about research. We come back to the question of what counts as legitimate research and what are the means through which it can be evaluated.

Student: I have to say you struck a chord here. In spite of the case I am making for autoethnography, I am also ambivalent towards it. I think my ambivalence stems precisely from this problem of the evaluation: how can autoethnography be assessed? Particularly when I am in the position of the instructor, evaluating student assignments, the question of evaluation becomes more important. I am not sure why, but when I am the evaluator, I feel more compelled to reinforce the boundary between academic thinking as ‘skilled research’ and fiction (or any type of knowledge and writing not based primarily on empirical proof, logical arguments, and critical thinking). Now that I think of it, it is precisely this boundary between academic research and other forms of knowing and writing that gives me the authority to be an evaluator; and when I evaluate, I find myself espousing more strongly the regime of truth we were talking about earlier, because this is partly the source of

my authority!

Maybe I am a hypocrite because just the other day I was talking to a fellow doctoral student in political science and I was arguing for the need to have a clear and valid method of analysis. My colleague wanted to do a discourse analysis, and I was clearly advocating for an analytic method that will spell out in detail how the text was to be assessed, and based on what criteria. I was pushing for a design that was to be evaluated in terms of reliability and validity. When I disagreed with the interpretation of a certain phenomenon, I wanted to know how she has observed the phenomenon, what were the criteria she used in analyzing it. In retrospect, I realize that whenever one disagrees with a political position, questions of the validity of the analysis tool become foregrounded as more important.

Autoethnographers make the case that there are criteria that can be used for evaluation, even if they see the concept of ‘criteria’ as positivist, as something that is “beyond culture, beyond ourselves and our conventions, beyond human choice and interpretation” (Bochner 2002, 259). For instance, instead of looking for validity, reliability and generalizability, autoethnographers look for reflexivity, impactfulness, aesthetic merit, substantive contribution and degree to which the text clarifies a lived reality (Holt 2003). The merit of such a piece lies in the level of detail or “thick descrip-

tion”, in the complexity of the writing and the emotional credibility and honesty of the author. I know we want to avoid simplistic descriptions and that we need to question intuitive or ready-made explanations, but I think autoethnography allows for this in providing a space for our ‘many selves’ or contradictory selves to become visible in the text (Ellis 1997). Finally, autoethnography espouses a particular political goal, that of addressing inequalities and injustice. In this sense, its evaluation could consist of asking whether the narrative speaks about empowerment and resistance to oppressive norms (Bochner 2002).

So, a good autoethnography needs not indulge in the cozy space where the self thinks highly of her/himself (Sparkes 2002). A good autoethnography is one that contributes to understanding the society in which we live. Its value lies in the ability to render the complexity of issues and make it appealing to the reader, because the knowledge we gain through empathy is just as important as the knowledge we get from numbers. And a good autoethnography needs to be reflexive and to make us want to engage in the dialogue (Ellis 2004; Sparkes 2002).

Supervisor: How do these criteria measure up when you try to use them in evaluating student work?

Student: You are right, it’s not very easy because whenever I try to

evaluate such work, it is hard to escape my own feelings towards the piece. If I disagree with the interpretation, it becomes more difficult to evaluate it, and I find myself looking for the coherence of the argument, for the ‘proof’ provided by the author.

I feel very ambivalent on autoethnography now. And I wonder if it has to do with the fact that I have to identify with the position of the evaluator. The pressures I face now are different: I want to ensure that the arguments and the ensuing knowledge they propose are indeed ‘valid’. To consider them as such, I need to make sure they are based on a rigorous observation or logical argumentation. At the same time, I know that “in the social sciences, we have never overcome our insecurities about our scientific stature. In our hearts, if not in our minds, we know that the phenomena we study are messy, complicated, uncertain, and soft. Somewhere along the line, we became convinced that these qualities were signs of inferiority which we should not expose” (Bochner 2002, 258). When I do my own research, I feel more inclined to acknowledge this messiness and the results of my own position in filtering it. I think of this as reflexivity and I tell myself it is an important part of the critical interpretation (Richardson 2002). But when I evaluate other people’s research, things are not always the same. Yes, I still ask questions around the position

of the researcher, but the way I ask such questions sounds more as if the researcher can get to the 'essence' of things if her own biases do not get in the way.

Supervisor: On the one hand, you are talking about criteria for evaluating autoethnographic work. On the other, you are talking about the politics of the profession. Let's get back to the question of the legitimacy of the method: it's hard to think of what counts as a method without considering the politics of the discipline in which you are writing. It matters a great deal if you are positivist or constructivist; if you are interested in causal relations and effects, or if you are more interested in understanding meaning-making practices. In terms of authority, if you are a famous scholar like Bruno Latour, presenting your theory by means of a funny dialogue between a student and a professor, you can say things in a quite different manner than if you are only a graduate student doing an autoethnographic dissertation²⁰. Your future depends on how you are evaluated in the defense! The way in which you establish yourself as a scholar within a particular discipline and using a specific method will matter a lot in terms of what kind of departments will want to hire you and what research funds you can access.

Student: Maybe I am not thinking very strategically here. I see

your point about legitimacy and the networks of power behind it. It makes me think that, to a certain extent, autoethnography is so appealing and yet so problematic precisely because it has not achieved full legitimacy. Its marginal position is both a promise of expanding what counts as academic research, and a threat to it. I can see how the whole discussion about criteria of evaluation is in fact one in which the boundary of academic work is both challenged and reinforced; for in order to legitimize autoethnography, I borrow from the vocabulary and tactics of the established methodological corpus, whether quantitative or qualitative (Ellis 1997; Ellis and Bochner 2006; Sparkes 2000). The discussion we have here is of a quite different nature from the act of reading autoethnographic pieces. In many published autoethnographies, the legitimacy of the method is not necessarily put under question, but performed by the fact that the pieces are published in academic outlets. In my case, I am doing an autoethnography from a different position: as a doctoral student, worried about my own defense; thus my choice of a method becomes crucial to my professional future. I need to be strategic here, not only on my method, but on my politics as well.

* * *

The choice of autoethnography as a method is neither a simple nor a purely rational act. It involves

my worldview, my political commitments but also my position within the various networks of power permeating the process of academic research. Through the metalogue above, I wanted to reveal how this choice looks like from my perspective as a doctoral student and how, through my choice, I become the node where various lines of power intersect. The choice thus becomes the mechanism through which I claim my authority as an aspiring academic.

Because autoethnography is a method at the margins of academic research, constructing its legitimacy is a very important stage in this process of claiming authority. Autoethnography is a contested method not only from the vantage point of positivist methodologies, but also from within the qualitative paradigm. Thus, constructing its legitimacy needs to be done contextually and planned strategically. For example, by virtue of its full embrace of subjectivity, autoethnography clashes with methodologies that assume the separation between the scholar and the social world (Denzin & Lincoln 2002; Ellis 2004; Holt 2003; Sparkes 2000, 2002). In such cases, autoethnography may be evaluated through traditional positivist criteria, such as validity, reliability and generalizability (Neuendorf 2002, 11-13). As an author, I need to build the legitimacy of my autoethnographic work in relation to the scientific paradigm,

largely defined along the lines of reason and demonstration (Fay 1996; Hamilton 1992). A leading figure of this paradigm, Francis Bacon once observed that “there are and can be only two ways of searching into and discovering the truth. The one flies from senses and particulars to the most general axioms [...]. The other derives axioms from sense and particulars, rising by a gradual and unbroken ascent, so that it arrives at the most general axioms last of all” (in Hollis 2002, 23). As Bacon tells us, there can only be two forms of scientific knowledge - induction and deduction. Thus, the question now becomes: where may autoethnography fit here and what elements can be of use in claiming legitimacy for this method?

As discussed above, there's also a need to construct the legitimacy of autoethnography in relation to constructivist paradigms. In such cases, autoethnography would rely on other type of 'criteria' like credibility, transferability, dependability, and confirmability (Lincoln 2002, 329) and on the intellectual frameworks provided by an array of critical theories such as feminism, post-modernism, post-structuralism and cultural studies. Interestingly enough, autoethnography's embrace of subjectivity is also a point of contention within the field of autoethnography, with some scholars trying to counter its emotional aspect with an emphasis on the analytical dimension (Anderson 2006; Atkins 2006).

Such attempts make an explicit effort to justify this method by situating it within the tradition of symbolic interactionism and by distinguishing between evocative (concerned with producing compelling descriptions) and analytic forms of autoethnography. The latter are then positioned as the “viable and valuable” (Anderson 2006, 378) forms of this method. In this process, analytic autoethnography’s subjectivity is being tamed and the method is made consistent with the ‘regime of truth’ of academic research: “the defining characteristic of analytic social science is to use empirical data to gain insight into some broader set of social phenomena than those provided by the data themselves” (Anderson 2006, 387). By espousing this analytic goal, subjectivity becomes enlisted under and reduced to “theoretical development, refinement, and extension” (ibid).

The shifts within the autoethnographic movement and its connections to other fields and power dynamics suggest that the legitimacy of a method is never a given thing. Instead, the process of choosing criteria of evaluation and intellectual legacies becomes a performance of legitimacy in itself, an act through which I establish myself as an authoritative speaker. As a student - and particularly in the context of a doctoral thesis - this performance is crucial: the method acts as a way of inserting myself within particular schools of thought and within par-

ticular disciplinary/ institutional networks of power. Drawing on multiple conversations and experiences as a doctoral student in the interdisciplinary field of communications, the metalogue above tries to capture the ways in which my position as an imagined student facing an imagined supervisor and an imagined doctoral committee becomes part and parcel of this negotiation of my authority as a speaker. As Crotty argues, at the same time, it is connected to my position within the academic system; a system that, implicitly or explicitly constructs legitimacy based on where you are located within the hierarchy, what type of research you are doing and who is reading your paper.

Last, but not least, the choice of a method and the ways in which the author may need to construct its legitimacy are also affected by the fact that this paper is a submission to a peer-review journal. Thus, through this paper, I enter into a relation with the potential reviewers, the institutional format of the journal and its take on academic writing. To what extent will the format of this paper will be accepted as a potential submission? Will it upset the imagined/ potential reviewers/ readers who, while sympathetic to autoethnography, may remain unconvinced about its scientific status or contribution (as Holt (2003) describes)? I would like to suggest that the autoethnographic nature of this paper and its metalogue format are soliciting the

reader to actively engage with the established norms and expectations of academic research. In fact, the compromise of this paper - part metalogue, part incipient analysis - reflects the ongoing exchange between the author and the (imagined and real) reviewers, who often require the re-writing of autoethnographic pieces so that they clearly outline these works' expected contribution to knowledge (Holt 2003; Sparkes 2000). By contrast, autoethnographic contributions by already established scholars are often published in dialogical or even poetic formats (e.g. Denzin 2006; Ellis and Bochner 2006; Pelias 2005).

These various dimensions of power networks are, of course, both contingent and contextual. But so is my own position on autoethnography. I have tried to capture this by referencing my own ambivalence towards autoethnography, an ambivalence that I link to my varied position within the academic system. On the one hand, I am not yet a legitimate member of this system. On the other hand, in certain roles (such as being an instructor or a reviewer), I am asked to act on behalf of the system. Doing an autoethnography may challenge the processes through which the boundary of the academic system (and particularly the boundary between academic knowledge and other forms of knowledge) is being maintained. What autoethnography seeks to do is precisely to create a "new quali-

tative research tradition" (Denzin 2006, 422) and to open a new space for analysis which is not tied to the explicit arguments, but rather stems from "how stories work" (ibid.).

This form of analysis resists reaching "some conclusion about the human condition or something that holds true for all people at all time" (Ellis and Bochner 2006, 438). Where does this leave my status, as an aspiring academic? While this intellectual effort of opening new spaces is appealing, its implications are also problematic. I am trying to enter this profession precisely because, in the end, I do espouse the Enlightenment's argument on reason as the means through which we can oppose dogmatism and taken-for-granted beliefs. While I find that our values and politics are always with us and therefore in our work, I also believe that there is a universal quality to reasoning that can transcend them. In particular instances, I do see that the methods of scientific inquiry are only one out of many possible modes of inquiry, "a rhetorical style" and that other forms of inquiry, focused on emphasizing the human dimension rather than causal logic, are also possible (Pelias 2006, 417-8). Yet, on the whole, I remain committed to forms of reasoning drawing from logic as well as from the ongoing questioning of the proof (Popper 1965).

As much as I may protest against some of the totalizing aspects of the established 'regime of truth', I am

not entirely against it. Indeed, I am part of it. Thus, when I have to act as an evaluator of academic work, for instance, my ambivalence to autoethnography is heightened. This ambivalence has also been noted by previous autoethnographers, particularly in instances where they realized that their own defense of the legitimacy of the method borrows from the established norms of academic argumentation (e.g. Ellis and Bochner 2006; Holt 2003). In my case, I try to rationalize it as an indicator that the 'regime of truth'- or the hegemonic claim over what can constitute knowledge - is never fully dominant, but also resisted. In my case, I both challenge and internalize and use it to establish myself as an authoritative speaker. Therefore, this 'regime of truth, which supported the various lines of power exposed in the metalogue, should not be understood simply through the conceptual binary 'enforcement'/ 'submission', but as a node through which power flows which involves processes of internalization and resistance (Foucault 1977/1980). Ironically, it is in those nodes that the hegemony of the 'regime of truth' is being both re-established and contested, keeping this regime flexible enough to be able to deal with new contingencies, contexts and positions. Choosing a method is not merely a logical deduction from the research question I am asking; it involves a negotiation of what counts as a legitimate method for my proj-

ect, a negotiation that brings together my values and my position within the academic system, as well as the networks of power within which I am trying to insert myself.

Endnotes

¹ Other qualitative methods also bring the researcher to the forefront of the research process, retaining this commitment to reflexivity and critical engagement. In the case of feminist inspired reflexive ethnographies, Suki Ali notes that researchers have to be reflexive not only in terms of how their identity comes to intersect with the research process, but also in terms of how "that relates to issues of power, and impacts on research and respondents" (2006: 476). However, unlike autoethnography, they are still using other people's experiences as data.

² Other prominent advocates of autoethnography are sociologists Laurel Richardson and Norman Denzin. The latter is an important figure in the legitimation of autoethnography as a qualitative method through his work on qualitative methodology in social sciences (see for instance Denzin and Lincoln 2002).

³ The 'politics of the method' refer to the argument that methods cannot be separated from particular worldviews - or discourses, in Foucault's formulation - which are part of the social distribution of power. Foucault argued that some scientific methods (such as those characterizing medicine or psychology) are an intrinsic part of the modern forms of social control (Frank 1982, 66). Similarly, Clifford and Marcus (1986) have discussed the impossibility of separating ethnography, as a method, from interpretation. The latter always implies our position and worldview.

⁴ The two characters presented here (the

supervisor and the student) are both reflections of my own persona. They do not represent any specific people; they grew out of my own struggles with academic work. I should point out that my own doctoral project is not an autoethnographic one, although I have been using autoethnography in a collaborative project (detailed in Dumitrica and Gaden 2009).

⁵ I am thinking here of theories such as post-structuralism or post-modernism, which cannot be confined to disciplinary boundaries. Similarly, feminist or post-colonial epistemologies are often used to formulate research projects in specific disciplines. For a more detailed discussion of meta-theory in social sciences see Delanty (2000).

⁶ I have been introduced to autoethnography within the context of a course on research methods in communication studies. Within this disciplinary field, autoethnography may be seen as a means to access meaning-making processes. This marks autoethnography as a method able to address concerns specific to communication scholars (such as how we make sense of the world around us). For example, my colleague and I have used autoethnography as a method of research in virtual worlds. We argued that this method allows us to tackle the dynamics of online gender construction and performance, and we made a case for its legitimacy by using both feminist theories and previous work on gender in virtual worlds (see Dumitrica and Gaden 2009).

⁷ Qualitative methodologists talk about the relation between the researcher and his/her work (see for instance Denzin and Lincoln 2002; Seale 2004). However, there are also many methodology textbooks still presenting the process of selecting a method as a logical one, deriving from the type of the research question asked.

⁸ In fairness, a certain degree of reflection on how researchers become inserted into the research process has always been present, even in quantitative methods. For instance, concepts such as 'nation', 'ethnicity' or 'identity' have always been recognized as connected to the researchers' personal values and political commitments. Nevertheless, this did not prevent scholars from trying to develop models that would limit the subjective aspect of these concepts and provide an objective definition that would make them amenable to 'proper' inquiry (see for instance Karl Deutsch's attempt to build a scientific model of nationalism, modelled after cybernetic theory). However, for the purposes of this argument, I have not engaged with this problematic here.

⁹ Andrew C. Sparkes describes these hardships in two different settings: in the defense of an autoethnographic thesis (Sparkes 2002) and in the review of an autoethnographic journal article (Sparkes 2000). For Sparkes, the question of how to judge a piece that does not fall within the traditional boundaries of academic work needs to be accompanied by an awareness and willingness on the part of reviewers/ defense committee to move outside their "own particular paradigmatic position" (Sparkes 2000, 29).

¹⁰ I am referring to the section in Latour's book *Reassembling the Social* (2005), where a student meets a professor to talk about doing an actor-network research project.

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“Are we all on the same page?”: The Challenges and Charms of Collaboration on a Journey through Interdisciplinarity

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Over the last decade, the Social Sciences and Humanities Research Council has been actively working to encourage interdisciplinary and collaborative approaches to acquiring and disseminating knowledge in Canada. How interdisciplinarity is understood and how it is translated into practice has been a source of debate, however. In this paper, we examine how we problematised interdisciplinarity and collaboration and how we learned from this process as a student group in the context of Hidden Costs / Invisible Contributions, a large multi-university research project based at the University of Alberta.

Students have been involved at a number of levels in this project: our MA, MSc and doctoral research have become intertwined with and integral to the project; we have authored and co-authored papers and presentations, we have assisted in other members' research, and we have been involved in the SSHRC mid-term review. As emerging scholars, in a project which has combined the research and knowledge of both the social sciences and the humanities, we have had to develop our own strategies for negotiating differences. In this paper, we will investigate four key areas that we have identified as potential challenges to successful collaboration: conceptual, methodological, pragmatic and personal differences. In our examination of the difficulties and rewards that we faced as students in each area, we will argue that successful collaborative and interdisciplinary work across the social sciences and humanities requires a reconfiguration of the ways that we are taught to “see” our particular disciplines. We have had to challenge how we understand the language, practice and function of our disciplines and the manner in which we approach this work as individuals. This has been a transformative process for each of us, but also one that has lent a renewed rigour and expanded scope in our own individual work.

Key Words: interdisciplinarity, collaboration, multidisciplinary, SSHRC, MCRI, student research, translation, relationship-building, power, personal epistemology

“The real voyage of discovery consists not in seeking new landscapes, but in having new eyes.”

-Marcel Proust (quoted in Clark, 2006)

Introduction

Since its inception in 1977, the Social Sciences and Humanities Research Council of Canada (SSHRC), a Canadian government-funded agency which supports Canadian and international scholarship, has been actively working to encourage interdisciplinary and collaborative approaches to acquiring and disseminating knowledge in the research it funds (Klein 1996). How that “interdisciplinarity” is understood and how it is translated into practice has been a source of debate, however. In the academy, the term, “interdisciplinarity,” has multiple meanings, with different risks and implications for each stakeholder in the research project (Klein 2005). For student researchers, in particular, whose future careers are closely tied to SSHRC funding, and often dependent on research positions within SSHRC-funded initiatives, collaborative interdisciplinarity can be new and difficult terrain to negotiate. Surprisingly, while there has been a significant body of research on interdisciplinarity and collaboration within the academy, there is almost nothing that focuses exclusively on student perspectives. In this paper we will examine

the charms and challenges of translating a SSHRC policy of interdisciplinary collaboration into practice by relating our own student experiences working on a SSHRC-funded program of research, *Hidden Costs/ Invisible Contributions (HCIC)*¹. We are going to explain why we believe that interdisciplinary collaboration is important for student training and how this process has given (and continues to give) depth and richness to our individual work, without glossing over the difficulties inherent to interdisciplinary collaboration or the challenges that we face going forward from this project.

Our collaborative journey officially began in January 2003, with the start-up of the *HCIC* program of research. Drawing upon research and knowledge from the social sciences and humanities, this program considered both the costs of caregiving for older adults and adults with disability and the contributions of these individuals to society. The *HCIC* team drew together various researchers, practitioners, NGO partners, policymakers and students from across Canada and internationally, who were united in their interests in aging and disability, and who were willing to work collaboratively with others. *HCIC* posed several major research questions: What are the hidden costs of care to caregivers and what are the invisible contributions made by older adults and adults with disability? How do we define “care” and

“contribution” in modern society? Are these definitions limiting or exclusionary in any way? *HCIC* research has included an annotated bibliography of the representations of care in Canadian literature, historical studies of Canadian and international government programs for older citizens, a study of media representations of disability, and a policy review on caregiver compensation for 10 countries, among others. Although all stakeholders were equally significant, this paper represents the experiences of *HCIC* student members, whose involvement SSHRC explicitly encourages.

SSHRC is the largest single source of funding for social sciences and humanities research in Canada. One of SSHRC's objectives is to “provide unique opportunities for training students and post-doctoral fellows in a collaborative, interdisciplinary research environment” (SSHRC). In addition, the formal application for major research projects requires that researchers address the number of students involved in the project, the overall quality of the proposed training activities, career development opportunities, and the potential to provide student training in a “well-structured, cross-disciplinary research environment” (SSHRC). As *HCIC* students, we were a diverse group from different universities, different disciplines, and were at different stages of our own personal careers when we became involved in *HCIC*.

Our MA, MSc and doctoral research work became intertwined with and integral to the project; we assisted in other members' research; we authored and co-authored papers and presentations (locally, nationally and internationally); we were fully involved in annual team meetings and symposiums; and we had a student representative on the executive committee.

In his seminal text, *The Reflective Practitioner*, Donald Schön argues that, “in real-world practice, problems do not present themselves to practitioners as givens. They must be constructed from the materials of problematic situations that are puzzling, troubling, and uncertain” (1984, 308). From the first *HCIC* meeting, the student group was concerned with the issue of how we were going to work together across disciplines and research interests. We had a strong desire to work with others and to learn how our work could both complement and be complemented by other team members. We also had a concern for self-reflection, were always thinking about interdisciplinary work, and would get others talking about collaboration. As a result, the student group took a lead throughout the course of *HCIC* research in studying interdisciplinarity and collaborative practices within the larger group. For most of the project, however, this interest was more practical, intuitive and hands-on, than research-centred. We wanted to know how we, as in-

dividuals and as a group, changed and adapted through the process of collaboration and interdisciplinarity. It was not until much later in the project that we began to corroborate our findings with other research in interdisciplinarity. This paper examines how we problematised interdisciplinarity and collaboration as a student group and how we learned from this process.

In all our various conversations with *HCIC* student participants, we found that three key areas stood out as being potential challenges or charms to collaboration. We identified these areas broadly as conceptual, methodological and pragmatic differences. We also noted that personal experiences were often included in discussions of the collaborative process. To further explore the challenges and charms resulting from these areas, we questioned current and former *HCIC* students about their experiences in each of these four areas. Eight of eleven students contributed their opinions, which were then amalgamated for review and discussion. Although this was a self-reflective exercise, we felt that these views and experiences were important to share, and we draw upon some of these thoughts throughout this paper.

Given SSHRC's interest in collaborative approaches and student training, and the gap in the literature, research on the experiences of students working in the context of a large project is timely and relevant.

Our journey will begin by exploring how our way of working together has transformed conceptually, methodologically, and pragmatically areas which we recognise are interdependent and may have some overlapping ideas and themes. Further on in the paper, we will suggest that beyond our conceptual, methodological, and pragmatic training, we have also developed as individuals. Our involvement in this project has taught us what it feels like to be valued and to know good mentorship and it has also significantly changed how we approach learning, teaching, and relationship building. Finally, we will also examine closely the significance of student involvement, what we have learned about the process of collaboration, and how it may facilitate the evolution of future collaborative work. The lessons we learned are useful to anyone thinking about or exploring the possibilities of collaboration, whether student, professor or researcher of any kind, both inside and outside the academy.

Interdisciplinary collaboration – a brief review of the research

Julie Thompson Klein argues that interdisciplinarity is neither a subject matter nor a body of content. It is, she suggests, “a process for achieving an integrative synthesis, a process that usually begins with a problem, question, topic, or issue. Individuals must work to overcome

problems created by differences in disciplinary language and world view" (1990, 188). Before we reflect on our own journey through this process, let us briefly flag what the literature says about interdisciplinarity and how it works.

A growing body of research on this topic addresses the benefits or advantages of collaborative research. Scholars investigating these issues highlight that collaboration among different disciplines fosters creativity (Levine & Moreland 2004), and promotes innovation (Cummings and Kiesler 2005) by bringing together ideas, tools and people from different domains. Others argue that the need to address increasingly complex problems in nature and society calls for interdisciplinary approaches (Massey et al. 2006, Beers et al. 2006) as these are better equipped to integrate "depth with breadth of interests, visions and skills" (Committee on Facilitating Interdisciplinary Research et al. 2004, 2).

While support for interdisciplinary approaches is on the rise, especially among funding agencies and policymakers (Massey et al. 2006), there is also widespread recognition of a particular tension "between the benefits to innovation of working across disciplinary and organizational boundaries versus the risks that arise from the costs of coordination and relationship development in these collaborations" (Cummings and Kiesler 2005, p.704). The chal-

lenges most often discussed as posing significant difficulties to the integration of multiple disciplines and methodologies in a research project include differences among researchers in terms of 'worldviews' and approaches to field practice, and the lack of a common vocabulary (Klein 1996).

Slatin et al. (2004), for instance, describe difficulties in communicating across disciplines due to unfamiliarity with disciplinary language. Qin and colleagues made similar comments a decade ago, suggesting that particular attention in collaborative projects should be paid to differences in "disciplinary terminologies and working norms" (1997, 914). And Fairbairn and Fulton importantly argue that the responsibility of the individual participant in interdisciplinary projects is not so much to learn the disciplines of the others, but "to interpret one's own discipline to the others" (2000, 35). All these scholars underline the importance of quality communication in interdisciplinary research processes, although we recognize, as do Pereira, Marhia and Scharff (2010), that communication across disciplines is neither straightforward nor a simple one for one translation.

To ensure that communication barriers do not affect the success of collaboration, several studies recommend that differences are acknowledged and respected from the onset and that a common conceptualisation of key concepts and

understandings is also achieved (Massey et al. 2006; Larson 2003). As Beers and colleagues (2006) argue, in order to function as a group and bridge their differences in perspective, team members need to negotiate a common ground. The more individual members identify themselves, their goals, and their perceptions about the outcome of their work, the more successful the group becomes. In short, effective communication can help teams to develop shared ideas and concepts (Klein 1994), and allow the establishment of connections among researchers from different disciplines and sites.

Other studies have pointed out that being flexible to a diversity of perspectives rather than judgemental or prejudicial are useful skills when working with people from a variety of disciplines and cultures (Stead & Harrington 2000). Slatin et al. (2004) specifically link the issue of power and disciplinary differences, and point out that not all team members view all disciplines involved in their project as equally important. They explain that some members place greater emphasis on concepts and methods of specific disciplines over others and, intentionally or unintentionally, create a hierarchy of values. Failing to establish the sense of shared power discourages facilitation of trust among team members and their commitment to the project (Stead & Harrington 2000). This indicates

that there are socio-emotional facets to the interdisciplinary process that are integral to a successful outcome. Indeed, in their examination of seven-year research collaboration, Engebreston and Wardell (1997) note that acceptance, validation, commitment, synergy, and having fun are fundamental characteristics of thriving partnerships. In the absence of mutual trust and respect, they conclude, research projects are less likely to reach their potential.

The emphasis on flexibility and openness as components of successful collaboration further suggests that epistemological transformation may be a part of the cognitive makeup of interdisciplinary processes. The study of personal epistemology (or conceptions that individuals have about knowledge and knowing), a flourishing area since William Perry published his first study in the 1960s, has more often examined the relevance of these constructs for teaching and learning (e.g. Schommer-Aikins, Duell and Barker 2003; Schommer-Aikins 2004; Hofer and Pintrich 1997; King and Kitchener, 1994; Khun 1991), but it seems reasonable to similarly consider their potential in the context of interdisciplinary work. Citing a number of authors in the field, Barbara Hofer (2004) maintains that beliefs about the nature of knowledge influence comprehension, cognitive processing and conceptual change learning, but also appear to promote

epistemological development by fostering one's competency to critically evaluate information, resolve competing knowledge claims and coordinate theory and evidence. Along similar lines, we would argue that interdisciplinary work, by engaging a dialogue among researchers of different disciplines both calls upon and stimulates the development of more sophisticated perspectives on knowledge. Studies that have addressed interdisciplinarity in the context of post-secondary education (Newell 1992; Fairbairn and Fulton 2000) seem to provide support to this claim. For instance, William Newell (1992), in his discussion of undergraduate interdisciplinary education, argues that students exposed to interdisciplinary work learn to go beyond logical skill sets and become strong critical thinkers, reflexive of self and discipline. Fairbairn and Fulton (2000) assert that in contrast to the oft-repeated observation that established academics are able to take more risks than less-established academics and are therefore more likely to become interdisciplinary, it is, in fact, "junior" academics that are most often open to learning newer, more interdisciplinary approaches to teaching and learning. While this is an area certainly deserving further attention in the literature, these studies begin to offer some evidence on the multiple intersections at the individual level between participation in interdisciplinary processes and

epistemological development.

Studies in interdisciplinarity have also examined the role of organizational issues in collaboration. Two factors reappear several times in this literature: physical distance and time. In a study examining successful coordination of collaborative research, Cumming and Kiesler (2005) found that multi-university projects tend to be less successful, on average, than projects located at a single university. They also argue that multi-university projects require more complex types of communication systems, including workshops and meetings, because distance and organizational boundaries tend to interfere with such coordination mechanisms that involve frequent and spontaneous conversation and/or problem-solving. And finally, while some studies pointed out time issues as a barrier to collaborative research (Fox & Faver 1985; Katz & Martin 1995), others view time, particularly for relationship-building, as a necessary element of element of team work (Larson 2003).

In sum, an emphasis on quality communication and the socio-emotional aspects of collaboration as foundational axes of successful interdisciplinary work, a growing interest in personal epistemological processes in the context of interdisciplinarity, and attention to organizational aspects of multi-site, multi-disciplinary research are some of the critical issues raised in the literature on interdisciplinarity. These

broad topics roughly correspond to the four areas (conceptual, personal, methodological and pragmatic) that the student group in our project identified as crucial to our journey toward interdisciplinarity. We now turn to the analysis of this experience.

A journey through collaboration: expanding the conceptual landscape

The first area identified in our discussions around interdisciplinary collaboration, the conceptual, was also the most difficult to isolate and define clearly. Paradoxically, however, it was also the area where students felt the most profound changes in the course of the research project. The Oxford English Dictionary defines a concept very loosely as “an idea of a class of objects; a general notion; an invention” (OED). The concept is what lies behind the research project: it is simultaneously the idea or “invention” around which the research project is built and the main research question that becomes the framework of the project. At the conceptual stage we look at and think about all the given data on a particular subject and ask ourselves, “What is missing?”

It is important to note, however, that the concept behind a research project is also in large part that “idea of a class of objects.” In the conceptual stage disciplinary foci are most apparent, because the ques-

tions asked and the things asked of them are informed by the theory central to each discipline. In thinking about care and caregiving, for example, where a literature scholar asks questions about the depictions of older adults in fiction over the last twenty years, a sociologist might ask questions about the statistical data of a particular segment of the population. Each approach is perfectly valid for their individual disciplines, and indeed, the questions they each pose of their subjects might be quite similar, but the questions are limited to a particular class of objects, which in turn circumscribes and limits how each individual sets up a research project.

Students were integrated into the *HCIC* project from the earliest stages of development. Much of our anxiety at the beginning revolved around how were going to work together across these disciplinary boundaries and much of our time was spent in thinking conceptually about how we were going to build working relationships with each other. How were we going to communicate, express ideas, and work across our very different disciplines?

These anxieties were very real and appeared at first almost insurmountable. Since we were all, more or less, thinking within a discipline, it was difficult to think conceptually outside of those boundaries. Would it not be a challenge to balance all the ideas and viewpoints of the student group and then synthesize them into

one research project? Would it not be difficult for each of us to find an individual place within the research, and would it not be even more difficult to focus – to not be so inclusive that the work would lose its impact? Or would one form of research, one discipline, take precedence over the others? Where would we draw the line? Consequently, student presentations, in these early stages of *HCIC* research, tended to focus on how the lines of communication and responsibility between researchers might operate over the course of the project.

What we were really modelling, as it turns out, was how to build trust. Paramount to a successful working relationship was to understand, at the conceptual level, how we each approached a problem, how we defined that problem, how we communicated it to the rest of the group, and how we could use this productively as a group. As the student group began to work together collaboratively on posters and panel presentations, and with other team members on *HCIC* research and individual dissertation work, we had to come up with our own practical strategies for overcoming our seeming differences. Somewhat ironically, the students we interviewed said that the more the students worked together, the more they trusted each other – and the more they trusted each other, the more faith they put into the process. It was surprising how quickly and how fully that trust

developed within the student group as a discrete entity within *HCIC*. Perhaps, as Fairbairn and Fulton (2000) suggest, because we were students, at an early stage in our academic careers, and able to immerse ourselves fully into an experience like *HCIC*, we were able to try on and discard new ideas and new approaches without inherent risk to our academic futures. It may also be that we were individually very open to the process of collaboration and found that it fit our own personalities and learning styles. It likely also has a lot to do with the leadership of more senior colleagues on this project; colleagues who welcomed student ideas and student input and enthusiastically supported our initiatives. We believe that the SSHRC funding for graduate research assistants and the ability to work long-term on this project also helped. The students within *HCIC* were all involved with the project for two or more years, while the authors of this article were involved, whether directly or indirectly, for 3 or more years.

It is important to recognize, too, that *HCIC* included only disciplines from humanities and social sciences. Engineering and science, which Biglan (cited by Schommer-Aikins, Duell and Barker 2003) calls the “hard disciplines,” were not a part of the collaboration team. According to Alexander (1992), in the humanities and social sciences, answers to problems are often incomplete, and

naturally encompass multiplicity. As humanities and social sciences researchers, we have been trained in relativist thinking. It is possible that such exposure has helped us develop, even before participating in *HCIC*, a personal epistemology that was more prone to accept an understanding of the nature of knowledge as complex, interrelated and fluid. Had the team included hard disciplines, it is very likely that reaching an agreement on basic concepts would have been more difficult.

Students within *HCIC* all clearly identified significant changes in their conceptual thinking as a direct result of their collaborative and interdisciplinary work on the project. All of them said that the theoretical base from which each is working is richer for having listened to, and worked with, each other. The “literature” person now sees historical, anthropological and sociological implications to literary texts, the “sociology” student is incorporating philosophical concepts and literary images to expand and enrich her social analysis, and the “human ecologists” have expanded the interdisciplinary theory they had already absorbed into a larger context. We do not suggest that these approaches have magically synthesized into one overarching or meta-discipline, nor, would we want them to. Rather, we have instinctively begun to integrate other approaches into the questions we ask – and into the class of objects we apply them

to. We are no longer content with limiting ourselves to the approaches defined by our disciplines – and this has transformed, expanded and reshaped our individual research outside of the *HCIC* project.

Addressing methodological challenges

This process has not been easy, clearly, and it has required some time, patience and a willingness to try out new approaches. Once embarked on that journey though, we very quickly realized that we were questioning our epistemological stance: what constitutes knowledge and how do we get it? In a very practical sense, these interrogations translated in our group into issues of scientific methodology. Methodologies are modes of procedure, or systematic ways of “doing things.” Methods, in turn, are the techniques researchers use to access and interpret their data (Hesse-Biber and Leavy 2004). Methodologies and methods are very important parts of the research process, and therefore also make up some of the charms and challenges of working with an interdisciplinary team.

In the social sciences and humanities, researchers apply a variety of methodologies: some rely on quantitative approaches drawing from national or even multinational surveys, performing complex statistical analysis and elaborating graph-

ics and tables to interpret and depict their data. They work with large numbers and anonymous samples that make possible the generalisation of results to a wider population with an estimated degree of error. Others are more closely associated with qualitative methodologies and assemble their data based on observation, interaction, interview, narrative and discourse analysis, and other unobtrusive modes of gathering information and knowledge. They work with smaller samples, collecting stories, meanings and worldviews, accumulating field notes, searching archival documents, examining images and texts. Their data is interpretative, process oriented and holistic.

Academics from the two traditions work within very different paradigmatic frameworks. The quantitative paradigm is often associated with positivism (and post-positivism), which assumes that all phenomena can be reduced to empirical indicators, that an objective reality exists independent of human perception, and that the investigator and the investigated are independent entities (Sale, Lohfeld & Brazil, 2002). In this sense, positivists assert that research can be conducted within a neutral, value-free framework. In contrast, the qualitative paradigm is based on interpretivism and constructivism. It claims that reality is socially constructed and constantly changing, and therefore is multiple and cannot be accessed indepen-

dently of our minds. In this sense, for qualitative investigators, the positions of both the researcher and the researched are intrinsic to the research process (Sale, Lohfeld & Brazil 2002). In short, there is no such thing as value-free, neutral knowledge-production processes.

In most disciplines, one of these approaches is usually privileged over the other. Students are frequently more familiar with one tradition or the other, as the two paradigms tend to be taught as independent of one another, and it is uncommon that graduate programs emphasize both to the same degree. *HCIC* encompassed a large, multidisciplinary team and involved multiple projects that employed qualitative and quantitative methodologies. Not surprisingly then, from a methodological perspective we found that participating in *HCIC* was beneficial as it provided us with exposure to a wide range of approaches. In sum, as one student put it, being a member of *HCIC* has helped us in developing research skills and becoming "more competent researcher[s]".

Curiously, student responses did not express many concerns in relation to collaboration around methodological issues. Part of this may actually stem from the way that we approached collaborative research. We spent a lot of time just talking in *HCIC*: talking as a research group, talking as student researchers, and talking as colleagues. The unintended effect of these dialogues

was that it allowed us to explore and overcome methodological differences both before and as we were collaborating on research projects.

While the *HCIC* initiative offered students great exposure to a variety of methodologies, it was not necessarily a “multi-methods approach”. Collectively, the various research projects conducted under *HCIC* employed different methodologies, but individually each one of them was more closely associated with a qualitative or a quantitative approach. It was only towards the end of the project, and especially in the context of discussions regarding a possible second application for funding, that the possibility of mixing and matching methodological approaches in single studies in order to acquire a more holistic perspective of a particular problem became more prominent. This shift, again, was one that students seemed to experience more acutely than any other members of the *HCIC* team. Certainly, with Bryman (2006) and others, we are aware that mixed approaches are not a panacea to all research, and that ultimately it is the question under investigation that should guide our methodological decisions. But by using multiple methods we will be able to find broader, more comprehensive answers to our problems because we will be asking different, more substantive and exciting questions. To us, the student group, this is perhaps one of the most valuable experiences *HCIC*

has offered us, in terms of enriching our academic training and enhancing our career prospects as future researchers.

‘Against all odds’: confronting pragmatic difficulties in collaborative research

The third key area identified in student discussions around interdisciplinarity and collaboration was the pragmatics of working together. After the conceptualisation of a project, and the subsequent planning, there comes a time when the project has to be put into motion. This is the pragmatic stage of collaborative research, which we define as the actual practise of implementing a project. This was the area which students most often identified as being a challenge and over the course of the *HCIC* research, we developed a number of strategies to overcome (both real and imagined) differences.

Power-sharing was principal among student-identified concerns. Because *HCIC* included scholars from a variety of disciplines, as well as practitioners, policy makers, and community partners, this meant differences, not only in discipline, but also in member investment in the outcome of the project, goals for the research and interests in the project. This had particular significance for students, especially at the beginning of the project, who perceived themselves as the least senior members

of the research team. The physical distances between team members was also identified as a major challenge, since the project involved a number of universities and community agencies across Canada and other countries.

We believe that there was a strong commitment to interdisciplinary collaboration and to fostering student involvement from everyone involved in *HCIC* during the five years of the project. All the students consistently reiterated that the encouragement they received from senior team members and community and government partners was a major reason for our positive experience with *HCIC*. We were given the opportunity and the funding to do week-long "campus exchanges," so that we could each visit another university campus and work with other researchers involved in *HCIC*. Student-initiated research was welcomed and supported. One student, in their response to our questions, suggested that this encouragement gave students the belief that we can and will be able to resolve differences and solve problems. We were able to work successfully within the project largely because it was a strong expectation of the project that we could and would contribute in a real way. We were expected to offer insights and make suggestions, to integrate our research into the larger project and to publish this work abroad. Because we knew that our ideas and our research were re-

spected and valued not as "student" research but as research in its own right, we pushed ourselves to think more broadly and more deeply. When we presented our ideas to others in the project, the response we invariably got, is not "can we make this work?" but "how do we make this work?" and "what are the tools with which we can provide you to make this work?"

This is not to say, however, that there were no individual challenges with power-sharing. Although the base for the project was at one university, *HCIC* research was managed by a number of partner universities geographically distant from each other. As a result, it was sometimes difficult to interpret directives and suggestions from more senior partners at other universities. With multiple projects operating simultaneously, there was also the potential for some students to feel lost and isolated in the larger group. To combat this, during annual team meetings, especially, we allotted a large portion of our time to come together as a group (both as a whole group and as a student group) to play games, exchange ideas and discuss issues. This practice was fostered by the senior researchers and team leaders in the *HCIC* project and, as one student put it: "Meetings are great opportunities to put faces on names and re-connect with the team and the project. It's great to hear about what each one of us is doing. Meetings really boost

my energy to continue for the following year". Meeting face-to-face during the annual meetings made subsequent discussions through email and the telephone easier.

We had the luxury of working together over an extended period, which also allowed us to develop strong relationships with one another. The challenge, however, is that the multi-year period of SSHRC special project funding also meant we had to sustain our energy and enthusiasm over months and years. This was where the planning and the practise sometimes diverged: while we thought initially that WebCT and email would be effective tools to help bridge the physical distance between us, it was difficult at times to sustain conversations through this very impersonal form of communication. What came across in student responses was that between team meetings, we could quickly lose momentum, and it was easy to fall back into the feeling of isolation. We made it a consistent and conscious effort of the student group to try to keep in contact with each other, even if it was just to discuss minor issues. We found that frequent telephone and Skype meetings, where we could hear the nuances of each other's voices, helped us with this.

The personal impact of collaborative and interdisciplinary research

The importance of the personal

in collaborative research is often undervalued, as studies on collaboration tend to focus on the academic aspects of research. Yet experiences at the personal level are as important as those in other areas because the personal actually underpins the whole process of collaboration. In our journey together, we learned that the energy of interdisciplinary collaboration may actually be embedded in the socio-emotional dimension, including the process of building and maintaining personal relationships and trust.

The *HCIC* project encouraged students not only to gain knowledge of new academic-related skills such as multiple perspectives, methodologies, concepts, and process of research, but also to develop less obvious, but equally important skills. Working in a team helped students to learn how to interact with others at both professional and personal levels, to develop negotiation and problem-solving skills, and to learn to ask people for help. Furthermore, we found a sense of community and belonging within this interdisciplinary project, and took inspiration from the personal and working experiences of other researchers and partners. For example, over the last year, while working on this paper, our academic, topic-centered conversations were often permeated with personal anecdotes, providing deeper insight into our respective worldviews. This ongoing process of exchange and sharing was ex-

tremely important in fostering mutual understanding and bringing us closer to each other. We began to understand the world through each other's eyes, to respect the unique contributions that each brought to the project while at the same time allowing ourselves to transform with the new concepts, methods and perspectives that emerged through and with this dialogue. These socio-emotional experiences not only apply to interdisciplinary collaborations, but will be relevant to the development and maintenance of our future personal and professional relationships.

Discussion: lessons learned and challenges ahead

The authors of this paper have travelled together along a collaborative journey as members of the *HCIC* team. Through the research process we have broadened our conceptual approach to issues, learned the epistemological value of methodological pluralism, and have developed effective ways of working together. All of these skills will not only help us in our own individual work, but will also enhance our skills as future researchers and collaborators.

While SSHRC requires that major funding initiatives involve students to provide training opportunities, we have gotten far more out of this experience than research training. An unexpected result, and one

which is not given enough attention in the literature on collaboration and interdisciplinarity, is our evolution as individuals. Through our active engagement in the *HCIC* team we learned more than simply how to do good research. Our constant exposure to each other's work styles, life views and disciplinary influences, has quite simply transformed us. We are less ego-driven and more open to the input and approaches of others. We are more supportive of our colleagues, and less inhibited in how we learn and teach. As we reflect on this journey against the backdrop of the literature on interdisciplinarity, we want to reiterate the key lessons that have helped us, both as students and scholars, to overcome challenges and contribute to successful collaboration:

- *Openness*: For us as a group, successful collaboration required flexibility to other perspectives, a genuine interest in understanding and learning from others, a non-judgmental attitude and a willingness to take the risks inherent in trying new approaches. In other words, we came to realize that successful collaborative work builds upon personal epistemologies that accept intellectual pluralism and the relativism of knowledge and ways of knowing (Hofer and Pintrich 1997; Schommer-Aikins, Duell and Barker 2003; Hofer 2004). While all members of the *HCIC*

team were open to the process of collaboration, as students we were particularly eager to try on new ideas and new approaches. Our experiences substantiate previous studies, which found an increase in interdisciplinary programs of study among undergraduate and graduate students who are often open to learning new perspectives (Committee on Facilitating Interdisciplinary Research et al. 2004).

- *Sharing power*: In a multidisciplinary, multi-stakeholder project like *HCIC*, it is very important to create a sense of shared power and equal value among all disciplines and team members (Stead & Harrington 2000). As students who are typically at the bottom of the academic hierarchy, we were fortunate to be involved at all levels in *HCIC* and we always felt that our opinions and ideas were respected and valued. Working in a less hierarchical environment encouraged us to work harder and more creatively. This is an area that would benefit from further attention in research programs involving students.

- *Ongoing communication*: Referred to in some studies as “negotiating common ground” (Beers et al., 2006), a conscious and consistent effort to maintain open communication was fundamental for our collaborative work to be successful. From the onset it was necessary to recognise and

respect differences among team members and to define (and re-define) clearly the common goals of the project. These experiences were not unique to our student group, as the importance of establishing common goals and language is well documented in interdisciplinarity literature (i.e. Klein 1994, Massey et al. 2006; Larson 2003). However, as we reflect back on our experiences in *HCIC*, we realize the negotiation of that common ground came largely through our willingness to dialogue openly with one another.

- *Trust*: Trust was another key element in our experience. The more we worked together, the more we came to trust one another; the more we trusted one another, the more we were committed to the process of collaboration. Successful interdisciplinary collaboration requires a trust both in the process and in each other (Engelbreton and Wardell, 1997). Project participants have to come to the project with a commitment to working together and a willingness to resolve differences openly. At a certain level this also requires a belief that the other members of the team are as equally committed.

- *Time*: Needing more emphasis in the literature on collaboration and interdisciplinarity is the issue of time. None of this would have been possible without the

luxury of working together over an extended period. Learning about each other and building trust, relationships and consensus takes large commitment of time and a considerable level of patience (Larson 2003). Again, this requires a commitment from the project participants, both to the process and in their investment in the outcome.

We would like to point out that there are challenges ahead for us. As the funding for this project has ended and we are each finishing our respective degrees, the authors of this paper are faced with the challenge of how to continue to work together. Since the *HCIC* program began, SSHRC has shifted from funding 5 year programs to 7 year programs. This is a positive move, as it allows for an evolution conceptually, epistemologically, and pragmatically into more integrated ways of working together. Importantly, it also allows team members to learn from each other and grow as individuals. Yet as we begin to look outside of this one program of research, we see difficulties ahead. As acknowledged by the Committee on Facilitating Interdisciplinary Research et al., "home departments [that] do not recognize, encourage, and reward such activities may not be willing to make the extra effort required for interdisciplinary activities" (2004, 62). Indeed, in the humanities (at least in Canada) there is still

little recognition of (and low funding for) collaborative work. In the sciences and the social sciences there is still not enough recognition of the value of a holistic perspective that includes the arts, history, and philosophy. In some cases, we face the problem of how to translate what we have learned for our individual (and occasionally sceptical) departments.

Yet the process of interdisciplinary collaboration continues to motivate and excite us. We did not realize, for example, that interdisciplinary work might offer us more career choices down the road: with the economic downturn and a hiring freeze in tenure track job postings, several of us are looking outside of traditional disciplines (and outside traditional academic jobs in several cases) for our careers. We remain firm in our belief that there is room in the academy for this kind of work. Choi & Pak (2006) argue that there are varying degrees of collaborative involvement which occur along the same continuum:

Multidisciplinarity draws on knowledge from different disciplines but stays within the boundaries of those fields; Interdisciplinarity analyzes, synthesizes and harmonizes links between disciplines into a coordinated and coherent whole; Transdisciplinarity integrates the natural, social and health sciences in a humanities context, and in so doing transcends each of their traditional boundaries. (359)

We would argue that transdisciplinarity also works in the opposite direction, by putting the humanities into a natural, social and health sciences context. Is this perhaps more than SSRHC bargained for? As a student group, we do not yet consider ourselves to be transdisciplinary, but we do believe that this is the path on which we are headed: unsure both of the final destination and if we will get there, but fascinated by the process. And we are looking forward to having more people join us on this exciting adventure.

Endnotes

¹ www.hcic.ualberta.ca

Acknowledgements:

The authors wish to sincerely thank Dr. Janet Fast at the University of Alberta for her guidance and her unwavering support of all the students in *HCIC*. We would also like to thank Dr. Pat Armstrong and Dr. Jim Struthers for reading drafts of this article, as well as the two GJSS reviewers and the GJSS editors for their helpful comments and suggestions. Finally, we would like to thank all the *HCIC* students, past and present, who have also offered suggestions and commentary over the past five years and who made this research project such a wonderful experience, particularly Dr. Linda Cook, whose enthusiasm and energy has been a vital part of this process.

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Imperial Plants: Modern Science, Plant Classification and European Voyages of Discovery

Rachel O'Donnell

This review essay considers Linnaeus's system of botanical nomenclature and the eighteenth-century 'voyages of discovery' to the Americas within the framework offered by contemporary feminist science studies. The author uses a feminist methodological approach toward concepts of natural knowledge and knowledge production and summarizes here basic ideas that are part of a larger project that looks at knowledges of particular plants from the Americas and their properties, focusing on one plant still used for fertility in the Guatemalan highlands. In this essay, the author investigates the centrality of natural knowledge to the development of differing historical perspectives on nature as well as the relationship between the development of European botanical sciences and 'voyages of discovery' to the Americas.

Key Words: empire, post-colonial, bioprospecting, Linnaeus, botany, feminist science studies, history of science

Botany became an important science during three centuries of European empire-building from the sixteenth to the nineteenth centuries. Ships from England, France, the Netherlands, and Spain sailed to their colonies to make discoveries in the service of the state and for profit. These profits did not arise from precious metals as much as they did from other natural resources: tropical plants, fruits, trees, and flowers from the Americas and the East and West Indies¹. Great fortunes awaited those who grew and handled colonial luxuries and

valuable plants such as cinnamon, cloves, coffee, maize, nutmeg, pepper, rubber, sugar, tea, and tobacco. Europeans wanted to know what plants looked like and where they grew; they needed to know they found the plants they were looking for and had 'discovered' the most valuable ones.

Botany grew and promoted European voyages. Trade and capital, more than science, drove collecting, classifying, and naming plants in the late seventeenth and eighteenth centuries. As it became more profitable to extract botani-

cal knowledge from native peoples, Europeans created a modern history of cultural exchange and colonial bioprospecting, i.e. Western endeavors to capitalize on indigenous knowledge of natural resources. Science and the development of capitalism converged on the discipline of botany as ornaments in European gardens, sought-after medicaments, and profitable plants became the most important materials in the building of empire, but only after a new 'objective' science had taken ideological hold².

This review essay takes an interdisciplinary approach to the relationships among science, nature, and gender in Europe in the early modern period and explores the role of Carl Linnaeus as one of the key developers of modern science, placing his role in the context of political, economic and cultural changes in Europe in the sixteenth and seventeenth centuries. Beginning from the central historical analyses in the field of feminist science studies, the first section of this essay will outline historical associations of nature and science in order to put in context the second part of my essay and the bulk of my argument. In order to fully understand the historical and ideological justifications for plant classification and European voyages of botanical discovery, it is imperative to begin with a discussion of early feminist science studies works, such as Carolyn Merchant's work on the history of the origins of

science, noting the relevance of botanical classification to a gendered history of science and the origins of such 'science' into account. While some science historians argue that "historians of science take an almost universally negative tone... seeing modern science' as all-too ready to assist the powers-that-were, whether domestic or imperial," (Drayton 2000, 128) feminist science studies often considers the political implications of the production of particular historical scientific knowledges. We can only look at these specific material events in light of their ideological context since, as Merchant articulates, "Descriptive statements about the world can presuppose the normative; they are then ethic-laden" (Merchant 1990, 4). Linnaeus's classification system and its connection to the voyages of exploration by botanists both prompted and expanded much of this classification. Indeed, constructions of gender are relevant to all this history. As Ruth Watts (2005, 89) argues, not only were scientific impulses of women restrained by gendered notions of science from the origins of modern science, but the position of women was in line with conflicting modern principles that underlay a contested terrain in science for the centuries that followed.

It is in this light that I attempt to illustrate the centrality of narratives of empire to the production of recognizable and legitimate narratives of science. I focus on the construc-

tions of 'exploration' and 'science,' examining not only the ways in which ideologies are created and perpetuated, but also the ways in which they make certain responses, actions and attitudes permissible and censor others. Scientific narratives are understood here as systems of meaning-production, rather than simply statements or language, encompassing texts and images and systems that 'fix' meaning, however temporarily, and enable us to make sense of the world. Keeping in mind the particular histories that shape our knowledge, feminist science studies allows us to demonstrate how the actions and priorities of a few dominant decision-makers (in many of these cases, European scientists such as Carl Linnaeus) have had repercussions historically and in our contemporary lives for what we understand of the natural world. This paper takes science to be both part of culture and humanistic knowledge, since part of the history of science is the formation of disciplines; that is, what is known as 'science' was specifically constructed in a particular time and for particular individuals, to be elaborated in the following essay. In its essence, then, this paper moves toward a central question about the politics of knowledge: how is it that some theories become dominant over others?

A Mechanized Marketplace: The Origins of Modern Science

Feminist writing has helped to re-evaluate the Western scientific revolution as an essentially masculine enterprise that served to classify and dominate nature. Carolyn Merchant played an early role in elaborating this history, focusing on the early modern era in her 1990 text *The Death of Nature*. Merchant's efforts were to "show how, in the context of commercial and technological change, the elements of the organic framework—its assumptions and values about nature—could be either absorbed into the emerging mechanical framework or objected as irrelevant" (Merchant 1990, 5). Feminist science studies as a field has become a valuable source of information for those who challenge the hegemonic epistemology of value-free research, and an asset for all scholars, especially feminist scholars, who deeply value the kind of scientific inquiry that breaks the power of gender.

Londa Schiebinger is one of these scholars, and as she has correctly articulated, what is partially at stake in reconceptualizing the history of science is access to the missing world of knowledge, missing as a result of science's disciplining of knowledge as well as a lack of consideration of 'science' as a set of ideologies produced at particular times in history when European knowledge was considered superior and non-European cultures inferior. Knowledge from Europe was preserved and knowledge from other

cultures purposefully ignored, particular knowledge about the properties of plants was systematically removed from Western public understandings throughout the modern period. Colonial powers persisted from the sixteenth century onward, the natural resources of the Americas were transferred to Europe while the bulk of plant knowledge was not. Decision-making about science, therefore, must be analyzed as it was guided by a certain set of assumptions made by scientists about people, lands, and traditions of knowledge and served to reinforce some and ignore others³.

Also fundamental to feminist scholarship has been a historical approach to theorizing the body, one that understands bodies as the sites of dynamic social processes, and brings presumed medical and scientific conceptions of human bodies more closely into view. Only an historical approach to the body will enable us to truly understand the strategies and violence by means of which Western science has disciplined and appropriated women's bodies, and has done so in light of ignorance of the medicinal properties of plants. As Karen Harvey (2002, 204) writes in her history of gendered science, "Bodies were thus reassessed by scientists in the context of political imperatives." Some feminists correctly posit a relationship between the participation of women in science and the his-

torical scientific conclusions about women's bodies and minds, as well as the nature of scientific work and the language of science (Kohlstedt 1995, 41-42). As Daniel Sherman (2000, 712) writes, the study of colonialism's deployment of various kinds of knowledges and their construction as 'scientific' has led to a related area of investigation: "the ways in which the colonial enterprise has fostered, nurtured, and decisively shaped disciplines, institutions, and practices in the metropole" and new analyses of how these dominant understandings developed. Other scholars have noted how colonial historians of science often wrote larger social and intellectual histories of Europe, not only histories of the colonies (Chambers and Gillespie 2001, 222), consciously or subconsciously detailing "how societies are structured so that certain knowledges become reviled and their development blocked" (Schiebinger 1989, 232).

What are these dominant formations of science that developed and blocked others? In modern scientific study, patterns of order and laws of nature are of utmost value. Sixteenth-century Europeans, however, considered nature without such stringent patterns within the prevailing ideological framework as an "organismic" understanding, where the "subordination of individual to communal purposes in family, community, and state, and vital life permeating the cosmos to the lowli-

est stone" (Merchant 1990, 1) was of highest importance. Such understandings of the world involved identifying nature and the earth with a nurturing mother, which gradually disappeared with the mechanization and rationalization of prevailing ideologies during the seventeenth century, what would later be called the scientific revolution. Nature as female earth and spirit was subsumed by the development of the machines of capitalism; the image of a natural earth had previously severely constrained what could be done to nature. With the disintegration of feudalism and the expansion of European colonialism and capitalism, commerce and profit became more ideologically important to the development of science than anything else.

Nature that was once seen as alive, fertile, independent and holistic devolved into a mechanized science during the sixteenth and seventeenth centuries that created new attitudes toward land. Such intellectualized science led to the domination of both nature and the female: mechanistic approaches to nature brought about the creation of objective knowledge developed by experiment and the "active subject/passive object" we know today as the modern sciences. Merchant calls our voyeuristic approach to nature "ocularcentric," (Merchant 1990, 2) describing the way in which Western sciences look 'out' at nature as separate from us in order

to uncover its secrets. Both nature and women began to be represented as subordinate and passive. The Aristotelian and Platonic conception of the passivity of matter could be incorporated into the new mechanical philosophy in the form of inert "dead" atoms, constituents of a new machine-like world in which change came about through external forces, a scheme that readily sanctioned the manipulation of nature. The Neoplatonic female world soul, the internal source of activity in nature, would disappear, in order to be replaced by a carefully contrived mechanism of subtle particles in motion. Indigenous conceptions of the land and a previous ethic of restraint disappeared as the ongoing exploitation of resources available for any nation's use was justified by the new science.

During the scientific revolution, a grand narrative emerged of the earth not as center of the universe but as something available for industrial science. Tools were now used in which to uncover this "natural philosophy" with empirical and experimental methods and mechanical law. It was only in very recent history that science has come to represent a field of study much more specific than its original general meaning or "knowledge that one has of things." Science lost such a broad understanding by the nineteenth century and acquired specific meaning based on mathematics and controlled observational experi-

ment: “Scientific method came to mean particular techniques requiring particular training, while mathematical descriptions of the universe came to be acknowledged as more exact models of the observed world” (Zinsser 2005, 4). How did “natural philosophy” become “science” and move toward classification and scientific exploration?

Francis Bacon (1561-1626), the celebrated “father of modern science,” developed an interest in industrial science and an inductive method to reveal ‘true’ science: “Female imagery... permeated his description of nature and his metaphorical style and were instrumental in his transformation of the earth as a nurturing mother and womb of life into a source of secrets to be extracted for economic advance.” Bacon saw dominating nature as part of ensuring the good of the entire human race:

“She [nature] is either free and follows her ordinary course of development as in the heavens, in the animal and vegetable creation, and in the general array of the universe; or she is driven out of her ordinary course by the pervasiveness, insolence, and forwardness of matter and violence of impediments, as in the case of monsters; or lastly, she is put in constraint, molded, and made as it were new by art and the hand of man; as in things artificial (cited in Merchant 1990, 165).”

By the time Bacon wrote his *New Atlantis* in 1624, significant class divisions motivated by capitalism and perpetuated by the industrial revolution were common throughout Europe. Changing relationships between local and large manufacturers prompted a doctrine of “scientific progress” associated with the rise of technology in support of capitalism. Further, as scientists became guardians of ‘scientific’ knowledge and technical language, valuing the objective over the subjective (in which the philosophical disappears) became the dominant European ideology. Bacon’s efforts to define experimental method in these terms found the bodies of animals and humans secondary to developing ‘true’ understandings of nature.

From the 1650’s onward, Bacon worked in developing a methodology for the manipulation of nature, including a tendency to charge women with medical knowledge with witchcraft and celebrate particular constructions of femininity that were not knowledge-based. Sciences that women traditionally operated in, such as midwifery and alchemy, were soon considered subjects that could be relegated to the periphery in search of ‘true’ and ‘objective’ science: an experimental and objective new science served the needs of capital and its accompanying ideology, the “privileged first-born twins of modern science: the myth of the natural body and the myth of value-neutral knowledge” (Schiebinger

2000, 4). Nature became “feminine” as it developed along the lines of European ideologies that reinforced a connection between masculine and objective. Such an analysis suggests a new model for politically-oriented historical analysis of science, as Bacon’s model allowed for particular constructions of knowledge—which included the classification of plants and the colonial exploration in search for these valued plants—that would, in turn, come to reinforce a masculine and objective construction of science.

**Kingdoms and Classes:
Linnaeus’s System of Plant Classification, Natural History and European Voyages of Discovery**

The need to look for “pure” systems of classification came about during seventeenth century colonial expansion in Europe and was prompted by the desire to collect plants for their economic and medicinal value, amid the general interest among naturalist explorers to uncover the botanical secrets of the world⁴. Mary Louise Pratt suggests in *Imperial Eyes* that the key moment in the development of a Western classification system for plants came when in 1720 Carl Linnaeus, a Swedish natural historian, elaborated his system for classifying and naming species. This system helped trigger a rapid increase in natural history exploration and stimulate “syntheses” of the botani-

cal knowledge it produced (Beinart 1998, 778). Scientific findings and literature, as a result, served to naturalize an ‘objective’ and scientific approach to travel for plant exploration and took little consideration of the human encounters that came with it. Pratt describes the writings of these botanical explorers:

“The landscape is written as uninhabited, unpossessed, unhistoricized, unoccupied even by the travelers themselves. The activity of describing geography and identifying flora and fauna structures as an asocial narrative in which the human presence ... is absolutely marginal, though it was, of course, a constant and essential aspect of the traveling itself” (Pratt 1992, 51).

In addition, the traveling naturalist had the ability to “walk around as he pleases and name things after himself and his friends” making “European authority and legitimacy uncontested” where “indigenous voices are almost never quoted, reproduced or even invented” (Pratt 1992, 63-4). Indeed, feminists have long contemplated the particularly gendered nature of the way in which colonial plants were named⁵. These “heroic narratives” explorers sent home to describe their findings and adventures went about naming plants, so each plant name became a celebration of European men, many of whom were upper-

class physicians. The heroic narratives they wrote “served to heighten a new version of heroic masculinity” (Terrall 1998, 225-7) and highlight the adventures of naturalists who encountered the dangers of the natural world. One German naturalist explorer who dramatized the difficulty of his passage: “the weather was severe, the rain continual, the mud thick and stagnant. Food was scarce along the long road and places to lodge nonexistent. Few people of means go by foot in these conditions,” he concluded, “they arrange instead to be carried in a chair tied to a man’s back” (Schiebinger 2004, 67).

European respect for traditional knowledges lessened over the eighteenth and nineteenth centuries. Interest in indigenous knowledge degenerated to “superstition” that coincided with the development of commercial crops and botany’s goal of charting commerce and state politics from the sixteenth through the eighteenth centuries⁶. Such understandings of plants as primarily profitable derive from early conceptions of the nature of science itself, where claims of objectivity coincide with little question of how findings are evaluated, who has access and authority to the knowledge, or to whom scientific findings are presented⁷. A consistent botanical language was crucial to the success of the expeditions of European empires to investigate the flora of the colonies: “Linnaeus’s system was efficient

since among its merits was its ability to disregard local circumstances, such as climate and soil conditions, without renouncing its claim to be describing a natural, or universal, order (Lafuente and Valverde 2005, 137). ‘Kingdoms’ of plant species, which Linnaeus imagined were ruled by laws similar to those that governed empires, were further divided into Classes and then into Orders, which were then broken into Genera and Species. Global expansion, as much as it served to shape the science of plants, included certain forms of knowledge accompanying global botanical exchange, and depended on local negotiation and cultural encounters, and dealt with the failures of transportation, disease, and adaptations. Still, what remained most important were plants that could easily be transported and turned into profit, such as coffee and opium. As Lafuente and Valverde conclude, “Linnaean botany was a form of biopolitics, what we might call ‘imperial biopower’ devoted to turning diversity, local variation, and qualia into data” (2005, 46). Indeed, as others have argued, “Empire requires that scientists and their patrons share the belief that the stuff of nature can be captured in words, figures, lines, shading, gradients, or flows” (Lafuente and Valverde 2005, 141). In fact, national identities among European empires often became centered around precise natural knowledge of New World regions they colonized:

“European naturalists, of whom Linnaeus was only one, tended to collect only specimens and specific facts about those specimens rather than worldviews, schemas of usage, or alternative ways of ordering and understanding the world. They stockpiled specimens in cabinets, put them behind glass in museums and accumulated them in botanical gardens...They collected the bounty of the natural world, but sent ‘narratively stripped’ specimens into Europe to be classified by a Linnaeus... supporting once again the notion that ‘travelers never leave home, but merely extend the limits of their world by taking their concerns and apparatus for interpreting their world along with them’” (Schiebinger 2004, 87).

Linnaeus taught that the purpose of natural history was to render service to the state. He was among many scientists in the service of the colonial empires to desire, first and foremost, to grow plants that could yield high profits like coffee and opium. The science of botany itself was defined as “expertise in bioprospecting, plant identification, transport and acclimation” that mirrored colonial expansion (Schiebinger 2004, 7). Botanical exploration followed trade routes, and naturalists and physicians worked to improve commerce and served empire in three ways: cheap supplies of drugs, food and luxury items for domestic markets, as colonial substitutes for such luxury goods,

and the growth of plants for profit within the empire itself (Schiebinger 2004, 7-8). These “biopirates” often named such items and operated within “a narrative of imperial nomenclature⁸.”

It is important to recall that Linnaeus’s naming practices came about at a point in history in which naturalists had the ability to regulate who could and could not do science and the restriction that scientific knowledge is only that generated by scientists. Such “professionalization” of knowledge of the natural world also developed as European science was establishing its power vis-à-vis other knowledge traditions. As a result, Linnaeus closely guarded the power to name and wrote, accordingly, “no one ought to name a plant unless he is a botanist.” Linnaeus admonished that “he who establishes a new genus should give it a name,” strengthening the priority of discovery as a chief scientific virtue. Further, he saw it as his “religious duty to engrave the names of men on plants, and so secure for them immortal renown” (Schiebinger 2004, 201-3).

Linnaeus’ system of naming that excluded native names proved instrumental for colonial conquest: “It was precisely this type of information—medicinal usages, biogeographical distribution, and cultural valence—that was to be stripped from plants in Linnaean binomial nomenclature as it has come down to us” (Schiebinger 2004,

197). What should be clear is that Linnaeus's system of plant classification and its repercussions never would have been accepted had it not been already clear what constituted scientific knowledge and who was responsible for its production, but what should be elaborated further are the definite links between Linnaeus's system and the voyages of exploration. Seventeenth and eighteenth century voyages of discovery brought European culture into contact with a variety of world cultures, but it is important to recall that European sciences were then being developed to enable the expansion of European empires at the expense of those Europeans encountered⁹. Certainly, European expansionism changed the "topography" of global scientific knowledge (Harding 1991, 29), and the underdevelopment or decline of scientific traditions in other cultures:

"Those aspects of nature about which the beneficiaries of expansionism have not needed or wanted to know have remained uncharted. Thus, culturally distinctive patterns of both knowledge and systematic ignorance in modern sciences' pictures of nature's regularities and their underlying causal tendencies can be detected from the perspectives of cultures with different pre-occupations. For example, modern sciences answered questions about how to improve European

land and sea travel; mine ores; identify the economically useful minerals, plants, and animals of other parts of the world; manufacture and farm for the benefit of Europeans living in Europe, the Americas, Africa and India; improve their health and occasionally that of the workers who produced profits for them; protect settlers in the colonies from settlers of other nationalities, gain access to the labor of indigenous residents..." (Harding 1991, 43-4).

Epic scientific voyages sponsored by colonial powers explored the natural riches of the 'new' world. Political economic thinkers of the day who touted Western European expansion found that amassing great wealth and power relied on exact knowledge of nature and celebrated the resources could be obtained for European powers through conquest and colonization. Indeed, in the eighteenth century, there was a close alliance between medicine and colonial botany or, "the study, naming, cultivation, and marketing of plants in colonial contexts—was born of and supported European voyages, conquests, global trade, and scientific exploration" (Schiebinger and Swan 2005, 2). Plants were important all kinds of New World travel, even missionary work—as a food source, in order to combat disease, and for building materials (Bravo 2005, 63). Botanists were

active in colonial politics, so “natural historical observation must therefore be viewed as a form of colonial government, in which cataloging existing resources and acquiring new ones served the ends of European imperialism” (Spary 2005, 193). As a result of the new ideologies of science that took hold during the centuries of empire, however, and centered on its noble search for ‘truth’ and ‘objectivity’ based in empirical method, historians of science often construed Europeans as the producers of knowledge and indigenous peoples as mere suppliers of the material artifacts from which that knowledge was born.

Conclusions: Toward a Political History of Plants

European sciences were developed to enable the expansion of European empires at the expense of those Europeans encountered, and the continued expansion of empires justified the continuing exploitation of nature’s resources. Naturalists who were able to bear witness to flora “in the field,” provided a certain authority to travelers’ observations, allowing them to represent the plants they ‘found’ and claim scientific authority over them. Consistent botanical language was necessary in these endeavors, and Linnaeus’ classification system was regarded as most efficient since it enabled scientists to disregard local culture and use and claim botany’s natural

and universal order (Schiebinger 2004, 36). As Carolyn Merchant articulates, natural history and nature had been previously represented to conform to particular gendered notions of colonizing social and economic systems. Technologies such as instruments, books, maps and tables, now continue to mediate between people (as subject) and nature (as object). Linnaeus’ system of classification and the botanical exploration that both prompted and follow from it, proved instrumental for colonial conquest and served to reinforce science and botany along particular gendered lines.

Such research into classification systems and scientific exploration remains historiographically significant because it indicates that the history of science and botany in particular, has moved from the margins of a historical field to take center stage in critical historical processes such as capitalist expansion, globalization, and colonization. Botanical exchange, therefore, was a highly contested and complex procedure previously taken for granted in political analysis and provides a possibility for demonstrating insights into indigenous understandings of nature and worldviews before Western disciplinary specialization took hold, especially in light of the contemporary focus that incorporates these plants from the colonial world and their applications into pharmaceutical research as well as biotechnology and international development ef-

forts. The commodification of crops and plant life continues as those once imported to the West from the rest of the world are exported again today to those former colonies in expensive, genetically modified forms. The question of bioprospecting and the status of indigenous knowledge and intellectual property rights is also very much a present-day and relevant question, and a complete analysis of the gendered history of these plants, as detailed by those in the field of feminist science studies, allows us to reaffirm the need for gendered understandings of natural history and explore new possibilities for conceptualizing the natural world and the political history that surrounds it.

The colonial world still remains marginalized by an overriding focus on European naming and colonization, and international botany is still regulated by politics, not science. Certainly, botany “both facilitated and profited from colonialism and long-distance trade” (Schiebinger 2004), but we must further analyze the links among botany, science history and classification, and European commercial and territorial expansion in light of contemporary biotechnological efforts and international development practice. Such research provides us new possibilities for understanding the natural and theoretical world, and science’s perpetuation of certain ideologies of gender, race, empire and science that we often take for granted.

Endnotes

¹ In 1494, when Columbus brought sugarcane cuttings into the West Indies, he provided the Spanish empire with what would become one of the world’s most successful cash crops.

² For more on specific historical instances of scientific sexism and racism, see Londa Schiebinger’s *Feminism and the Body*.

³ See Schiebinger’s “Feminist History of Colonial Science” (2004), in which she looks at the “culturally induced ignorances” of the peacock flower, as the plant itself traveled to Europe but pre-colonial knowledge of the plant’s abortifacient properties did not, one example of many Schiebinger cites in her work of the ways “bodies of scientific and medical ignorance...molded the very flesh and blood of real bodies.”

⁴ Many scholars have provided readings of European botanical gardens based in their incorporation of plants from the colonized world. During the time of empire, Jill Casid argues, even the presentation of nature became imbedded with ideologies of empire and gender: “Landscaping... was the primary means by which particular formations of family, nation, and colonial empire were engendered and naturalized.” Casid, pg. xxii.

⁵ The search for female amazons was part of the imperial inquest into South America as were the “heroic narratives” or the botanical explorers themselves. Schiebinger, *Plants and Empire*, pgs. 62, 65.

⁶ New ideas of agricultural “improvement” developing in the seventeenth century provided the right conditions for appeals to transform Kew Gardens in London from a royal pleasure garden for a garden with “use beyond beauty” (Drayton, 92). An account of this history uses specific details of Kew’s development as links to a wider

range of more specific historical and cultural shifts in the global and local economies of horticulture. If we consider this within the development of science and the gendered ideologies shaped by the broader social and political context, we see that economic botany was, in part, dependent on gender norms and sexual divisions.

⁷ Schiebinger makes a point to explore the hierarchical system of sex difference that Linnaeus's practice of plant classification actually represented, which I do not explore here, in *Nature's Body* (2004).

⁸ In *Plants and Empire*, Schiebinger explores the politics of early colonial bioprospecting in the West Indies, employing the metaphor of "biocontact zones" to look at the theoretical frameworks of local indigenous botanical worldviews in contrast to those of Europeans. In similar ways, but dealing with Creole elites, Antonio Lafuente and Nuria Valverde (2005) have shown how the Linnaean system was contested outside of Europe.

⁹ Sandra Harding (1991) concludes that modern forms of racism developed precisely as remnants of colonialism that justified the conquests: "It is impossible to separate racism from colonialism and imperialism and the development of modern science in Europe," In addition, she argues, the standards for objectivity, rationality, and "good" method have been constituted in relation to qualities and practices associated with non-European cultures. Harding, pg. 29.

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Hennink, Monique M. 2007. *International focus group research: a handbook for the health and social sciences*. Cambridge: Cambridge University Press. 248 pp.

ISBN: 978-0-521-60780-3.

Key Words: focus groups, developing world, development research, methodology, positionality, fieldwork

How can focus group discussions (FGDs) be used to their strengths in the Two-Thirds World? While handbooks for using focus groups in One-Third World contexts abound, researchers and students seeking guidance on this question have been limited to sub-chapters, articles and footnotes in publications on other topics (Lloyd-Evans 2006; Vissandjee 2002). 'Standard' focus group literature assumes a One-Third World setting, which limits its applicability to other contexts (Bloor et al. 2001). This dearth of relevant guidance on how to use the method in the Two-Thirds World has been accompanied by an unprecedented surge in its actual use there. Focus group discussions are now a staple - and sometimes the default - qualitative method in evaluations and assessments by aid agencies as well as in applied and pure research by

academics in the Two-Thirds World.

Monique Hennink's *International Focus Group Research* handbook is a welcome and long-awaited response to precisely this need. The author, Associate Professor of Public Health at Emory University, draws both on her own experiences in African and Asian countries and on interviews with other researchers who have conducted FGDs in Two-Thirds World contexts. The preface promises to combine this nimble 'feel for the field' with a commitment to data quality. One significant albeit obvious advantage over 'standard' FGD handbooks is the ease with which readers will be able to relate it to their own research situations in the Two-Thirds World. Illustrative examples and photographs from Africa and Asia help readers visualise the principles in the situations where they will actually be applying

them.

Another major strength of the book is its clarity: in structure, layout, and precision of language. It comprises twelve chapters, each for a separate stage in the research: planning, participant recruitment, data analysis, etc. One significant addition to the 'standard' FGD texts is a chapter devoted to 'training the research team'. This is pertinent because researchers are more likely to find themselves hiring and training assistants when conducting FGDs in the Two-Thirds World. The reasons for this are relatively mundane, but Hennink's no-nonsense commitment to lived fieldwork challenges is undeterred by their apparent banality. This unpretentious approach will be appreciated by graduate students who wonder whether their struggles are too quotidian to qualify for advice.

The handbook is an unparalleled resource on FGD methods. However, does it fully live up to its promise of addressing the main challenges to methodological rigour in FGD research in the Two-Thirds World? Readers who are familiar with the 'standard' focus group literature, and concerned with using the method to its strengths in the Two-Thirds World, will unfortunately recognise the bulk of the content as reviews of that same literature. The chapter on data analysis is specific neither to focus groups nor to Two-Thirds World research, and would not be out of place in a 'standard'

qualitative methods book. The chapter on discussion guide is similar, excepting a sub-section entitled 'Discussion guides for international focus group research', which deals exclusively with translation (64).

The challenges dealt with that are specific to the Two-Thirds World are logistical, for example translation and recruitment procedures, applying for research permits, and seating and recording arrangements. However this privileging of practicalities has three drawbacks. Firstly, it reinforces the very tendency she is countering, namely to focus on 'the management of fieldwork challenges' rather than methodological rigour. Secondly, it implies that apart from such practicalities, the challenges to methodological rigour in FGD research are identical in, say Senegal and Switzerland. Lastly, it fails to address challenges to methodological rigour specific to FGDs in the Two-Thirds World.

Two such challenges that are central to the literature on qualitative methods in the Two-Thirds World, and more difficult there than in One-Third World research, are power gradients and positionality (Apentiik et al. 2006; Madge 1997; Scheyvens et al. 2003). Power gradients refer to unequal power relations between researcher and researched. Positionality in this case refers to how the identity the researched assign to the researcher influences what they say to him or her (Bell et al. 1993; Henry 2003;

Rose 1997; Srivastava 2006). The steeper the power gradient, the greater an interest the researched have in adjusting their responses to who they perceive the researcher to be. Researchers' self-deployment may change who respondents perceive them to be, but this does not change the extent to which responses reflect respondents' perception of them, regardless of what that perception is. The absence of these two challenges is conspicuous because the 'standard' FGD literature claims the FGD method, when used to its strengths, can shift researcher-researched power relations and thus reduce the extent to which positionality determines what data can be generated (Kambrelis et al. 2005; Kitzinger 1999; Smithson 2000; Wilkinson 2006). Hennink's handbook gives no advice on this. The advice it offers on moderation seems to blithely gloss over this challenge:

The deference effect ... (where participants say what they think a moderator wants to hear rather than their own opinion about an issue)... can be avoided by clearly reinforcing to participants at the outset of the discussion that all views are valued and it is participants' own views that are being sought. (184)

Proponents of FGDs emphasise that most guidance on how to moderate a discussion relies on 'natu-

ral' conversation norms that are context-specific (Bloor et al. 2001). This makes it especially problematic that also the chapter on moderating discussions is largely a repetition of the 'standard' FGD literature. The few conversational norms that are mentioned are more about topics that may be tricky to elicit responses on in general, than about challenges specific to the FGD method. They are content-specific rather than method-specific, and say little about how the interactive processes on which the method hinges may play out differently in Two-Thirds World contexts, and how to handle this.

Given the challenges of steep power gradients, positionality and different conversational norms, this book does not adequately explain how FGDs can be used to their strengths in the Two-Thirds World. However this does not detract from the book's immense usefulness for one large group of readers. Researchers familiar with the practicalities of working in the Two-Thirds World, and with the 'standard' focus group literature, will find little new here. In particular, readers of the 'skeptical enthusiastic' literature that followed the method's surge in popularity, which hones in on what types of data FGDs can reliably generate, and how to conduct them in order to generate this type of data, will miss this level of epistemological awareness (Barbour et al. 2001; Bloor et al. 2001; Parker and Tritter 2006). Nevertheless, One-Third World re-

searchers embarking on their first FGD research project in the Two-Thirds World will find the handbook an invaluable companion. While the extent to which the textbook consists of reviews of other textbooks is problematic, this does also have its advantages when researchers pack their bags for countries where books may not be readily available. For this reason, despite its neglect of central methodological challenges to rigour in Two-Thirds World FGD research, if you have never worked outside the One-Third World, and do take only one methods book for your focus groups in Colombia, Cambodia or Cameroon, Hennink's handbook is a practical choice.

Endnotes

¹ The term 'Two-Thirds World' refers to the social majorities who "have no access to most of the goods and services defining the average 'standard of living' in the industrialised countries", as defined by Gustavo Esteva and Madhu Suri Prakash (1998, 16-17) and used by Chandra Talpade Mohanty (2002). 'One-Third World' refers to the social minority in both the North and the South who do enjoy such a standard of living.

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**Ortiz Gómez, Teresa. 2006. *Medicina, historia y género. 130 años de investigación feminista*. Oviedo: KRK. 368 pp.
ISBN: 84-96476-52-9/978-87-96476-52-3**

Key Words: women's studies, feminist historiography, history of medicine, academic teaching and research, Spain, twentieth Century

Medicina, historia y género by Teresa Ortiz can be considered a successful attempt at transferring the author's knowledge and experience in researching and teaching the history of medicine, science, and women's/gender studies. This research was gathered during her long career at the University of Granada, Spain, where she is at present professor at the Department of the History of Science at the Faculty of Medicine¹. Ortiz currently teaches the history of science and medicine to undergraduate medical students, and is also one of the key lecturers in postgraduate women's and gender studies in Spain.

Teresa Ortiz is one of the pioneers in applying and teaching feminist interdisciplinary methodology in the field of history of medicine and science in the Spanish context. In Spanish academia, feminist interest

in women's and health issues is represented within various disciplines, with the most prolific areas being anthropology (such as Mari Luz Esteban), psychology (like Silvia Tubert) and sociology (e. g. Eugenia Gil Garcia). Generally, these works provide either the theoretical background and methodological tools necessary to study various aspects of women and health, or present the results of research conducted on concrete aspects of women's health, especially those related to reproduction, violence against women or bodily issues. On the other hand, feminist historians of medicine such as Montserrat Cabré i Pairet or Consuelo Miqueo have published important works on the history of women in medical professions and androcentrism in medical discourses. Teresa Ortiz herself has published important works on

women and medicine, especially on midwives and female medical professionals. However, *Medicina, historia y género* is a reflection on the histories of women and medicine as disciplines. It offers an excellent initial reading of the development, scientific interests, production, methods, and intersections of the history of women in/and medicine².

Fundamental feminist epistemological concepts, such as 'situated knowledges' (Harding 1991), have most certainly influenced the author while working on this book, whose presence in the narrative is strong, as she shares her inspirations, reflections, and experience in co-forming the Spanish feminist historiography of science and medicine. This quality, together with clear and comprehensive language in which the book is written, makes it very readable. Thus, it can be recommended to readers with intermediate level of Spanish.

The main question posed by Ortiz concerns how the history of medicine as a discipline should be delimited, and how it interacts with gender studies, and especially with women's history and feminist theory. The book is divided into three parts. The first part broadly presents the theoretical and academic context of the feminist historiography of science and medicine, or a historiography pursued from a gender perspective, within the last 130 years in Spain. In the four chapters that comprise this part, Ortiz focuses on the his-

tory of Spanish feminist academia, explains and dismantles common inaccurate or imprecise uses of terms such as *women, gender, gender relations, sexism and androcentrism*, and finally discusses the implication of women's studies in the re-elaboration of the concepts such as *body, authority, and authorship*. The second part of the book is dedicated to a feminist historiography of health and medicine from its early days in the late nineteenth century to the end of the twentieth century. Here, the author traces the academic traditions of feminist historians of medicine and health, and critically reports on the contributions of the most distinctive authors, outlining the important phases in the development of the discipline. The author also points to the most recent trends in gender and health (or women and health) studies that have flourished in the Western context and particularly in Spain during recent decades, such as the deconstructive studies of medical discourse in relation to women. The final part of the book is dedicated to the history of medicine from a more general perspective. Here, Ortiz critically examines her own discipline and also discusses the most outstanding features in its current development. Finally, the author proposes a series of recommendations regarding the academic teaching of a non-androcentric history of medicine, resulting from her vast experience as a lecturer, and based on feedback she had re-

ceived from her postgraduate students.

Medicina, historia y género is primarily about interdisciplinarity, which has been considered one of the main characteristics of the methodological toolbox of women's and gender studies. Throughout the book Ortiz discusses possible intersections within the disciplines such as women's history, history of medicine and history of science both in research and teaching, providing an excellent practical example of how to pursue interdisciplinary scholarship. This dimension of Ortiz's work is emphasised in the third part of the book, where she refers to interdisciplinarity as, along with pluralism, one of the most distinctive features of the contemporary history of science and medicine, which seeks to include and combine different theoretical and methodological paradigms.

I would highly recommend Ortiz's book to all scholars and students who work on issues related to women and medicine, especially within or in reference to the Spanish feminist framework. Above all, this work provides an excellent and neatly organized bibliographical revision of the most prominent works from Anglo-Saxon, French and Spanish context. The extended reference list can be useful to those interested in (feminist) history of science and medicine, and also to all who wish to learn about the origins and development of feminist scholar-

ship in Spain. The book is based on rich documentary sources including quantitative and qualitative publications by and on women in scientific journals, under- and postgraduate courses in gender and history of medicine offered by Spanish universities, the proportion of men and women professors in Spain within the field of the history of science, and more. Furthermore, the methodological comments and observations are of great value especially to less experienced scholars, especially as far as academic teaching is concerned.

With regards to drawbacks of this text, in Chapter 1.2 Ortiz extensively discusses rather basic notions such as *gender, sexism, or androcentrism* in a passage which is too rudimentary for researchers with some experience in the field of gender studies. Meanwhile, the next chapters (1.3 and 1.4), which are dedicated to feminist re-conceptualizations of the body and the concepts of *feminist authorship, authority and sexual difference*, only scarcely mention these concepts. Development and critical revision of these would have increased the usefulness of this book for feminist researchers. Ortiz does situate herself as a feminist scholar, but this book would have benefited from more emphasis on her own position within feminist theories and academia. However, the simplicity and underdevelopment of the mentioned parts of the text can be justified by Ortiz's consideration

for the broader public at which the book is aimed. These are, as she explains in the introductory part of the book, scholars who work in the field of the history of medicine, possibly with scarce knowledge of women's studies or contaminated with the common misuses of these terms, and postgraduate students of history of medicine and women's studies. Beyond any doubt, both will certainly find this work of great utility in their research and studies.

Endnotes

¹ The book I review was published in Spanish only. The author's publications in English include chapters on the history of Spanish midwives in Marland (1993) and Marland and Rafferty (1997). She has also recently published a chapter on female medical professionals in Spain during Francoism (Rodríguez-Sala & Zubieta García 2005) and co-edited *Dynamics of health and welfare* (published in Lisbon by Colibri in 2007), a collection of commented sources in history of medicine, in which she co-edited, together with Denise Bernuzzi Sant'Anna, the part entitled *Perspectives on gender and health*.

² Teresa Ortiz is also one of the founding members of Instituto de Estudios de la Mujer [Women's Studies Centre], an interdisciplinary body established at this university in 1986. It is now host to GEMMA: Joint European Master's Degree in Women's and Gender Studies, a prestigious European postgraduate programme, which is being developed simultaneously since 2007 in seven European universities (Granada, Oviedo, Utrecht, Lodz, Ljubljana, Hull and CEU-Budapest) under auspices of the European Commission. *Medicina, historia y género* is used as a textbook in the

Spanish edition of the Master's.

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Re(con)figuring the ethico-onto-epistemological question of matter

**Karen Barad, *Meeting the Universe Halfway: Quantum Physics and the Entanglement of Matter and Meaning*
Duke University Press: Durham, 2007, 524 + xiii pp.
ISBN 978–0–8223–3917–5**

Key Words: apparatus, diffractive, new materialism, methodology, quantum physics, language

Meeting the Universe Halfway is an answer to the reflective methodology found in representationalist scholarship which has previously characterised not just feminist studies but social and scientific studies in general, and which presumed the separate ontological existence, however mute or devoid of agency, of the object which is being represented. The author's aim is to configure a 'diffractive methodology [...] to provide a transdisciplinary approach' (25) which cuts across 'quantum physics, science studies, the philosophy of physics, feminist theory, critical race theory, postcolonial theory, (post-) Marxist theory, and poststructuralist theory' (25). This new methodology is necessary

in order to challenge representationalism, which has, for all fields mentioned, unwanted consequences. In so doing, Barad joins the theory of new materialism, which, though still in the process of being constructed (see Sheridan, 2002; Colebrook, 2008; DeLanda, 2006), is part of a wider movement in critical theory away from theories associated with the linguistic turn. New Materialism is an epistemological/methodological trend which has entered the academic arena not as a contestation, but as one of the theoretical frames of third wave feminism (Van der Tuin, 2009), which postulates affirmative readings instead of critical ones of past theories.

Karen Barad is a professor of

feminist studies, philosophy, and history of consciousness at the University of California, Santa Cruz. She has a Ph.D. in theoretical particle physics, which forms the background to this book. Her previous work, and particularly her 2003 article, 'Posthumanist Performativity: Toward an understanding of how matter comes to matter', paves the way for this book. Drawing mainly on Bohr's philosophy of physics, as well as the work of other major theoreticians such as Foucault, Butler, Haraway, and Fernandes (among others), she develops a new 'ethico-onto-epistemological' (185) theory called 'agential realism', which is explained throughout the book, but in more detail and with practical application in chapter six. Her list of references not only demonstrates her balanced reading of current theoretical debates relevant to her agential realist account, but could also be considered essential reading for any new materialist researcher.

Meeting the universe halfway enters the academic arena in the 'transitional' period from second- to third-wave epistemologies, offering an agential realist ontology which can help feminist studies to demonstrate the 'entangled' state or complexity of 'matter'. 'Agential realism', the term that Karen Barad uses for her new ontology, is meant to provide sensitive descriptions of 'material-discursive practices' which promote differences that matter. That is to say, this ontology rejects

the foundational separation between 'object of observation' and 'observer' because this division assumes the object as passive and the observer as active. Her ontology describes the world by means of 'apparatuses' in which both object and observer, human and non-human, are connected. As such, the differences that matter are provided by the boundaries of the apparatus (140 & 148), and not just by the researcher: 'apparatuses are specific material reconfigurings of the world that do not merely emerge in time but iteratively reconfigure spacetime-matter as part of the ongoing dynamism of becoming' (142). This is why specific intra-actions (different relations produced within the apparatus) matter, the materialization of reality depends on all the entanglements and is how the world acquires its meaning (333).

The structure of the book is very complicated since the author moves back and forth in order to produce more complex explanations, although each chapter can be understood by itself since the paramount concepts are repeated throughout the book. The last two chapters are devoted to the entanglement of the philosophy of physics with social theories and, as such, are the more complicated ones for an audience which is not familiar with quantum physics. The first chapter presents the problem of the present theory and methodology, while the second one moves to her solution to this

problem: a diffractive methodology which is very precisely explained in contrast to the reflective method throughout an intra-active, yet binary, table (89-90).

Diffraction is understood by Barad as 'a material-discursive phenomenon that makes the effects of different differences evident', 'a way of understanding the world from within and as part of it' (88). It is a 'physical phenomenon' (91) which entails a commitment by the researcher to understanding 'which differences matter, how they matter, and for whom' (90). The researcher is responsible for the different practices which construct different understandings of the world. Drawing on Haraway's work with technoscience (94), Barad proposes a reading of different theories which, instead of opposing them, engages them with one another (92-3). That is to say, the researcher engages with different theories realizing affirmative readings of them in order to provide more sensible accounts of the world. Instead of looking at differences, she wants the researcher to explore boundaries since they are what provides meaning. This methodology proves to be not only an analytical tool of critical engagement (as traditional methodologies are), but also part of the 'phenomenon', or object of investigation, since instruments of investigation produce differences that matter in the results. It helps to explain power relationships and how they are entangled in bodies,

subjectivities and identities (35). Making the methodology part of the object of investigation involves an awareness and inclusion of the different effects of instruments in an investigation in both human and social sciences.

In this book repetition does not become synonymous to fixity of concepts. The book can be considered a perfect materialization of Barad's own theory; concepts are entangled everywhere and their definitions are not entirely stable. For example, the elements intra-acting are sometimes described as 'agencies' (333) and sometimes as 'components' (269). In addition, the many neologisms required to describe the apparatus (such as 'intra-action'), can further complicate reading of this book. Though these difficulties are to some extent resolved after reading the whole book, they can cause reader to have doubts about what is meant by 'phenomena', 'apparatuses' and 'agential cuts'. In other words, reading and re-reading of the entire book is beneficial for understanding Barad's ideas in their full complexity. Ideally, clearness goes hand in hand with conciseness - something which is occasionally missing in this book.

To conclude I would like to turn towards one of the most controversial aspects of Barad's work which she tries to clarify throughout this book: her take on language. This issue has created a strong debate between some new materialists who

follow Barad's agential realism and some poststructuralists in feminist epistemology (see Ahmed 2008). It is true that strong negative attributes attached to Language (with a capital L) pervade her work. However, this is focused on the erroneous conception of language as a mediator by representationalism (470, n. 41). Instead, language is seen here as part of the apparatus (205); it is another entangled agency as important as the rest in configuring the phenomena itself since matter and meaning are always inseparable (as highlighted by the subtitle of this volume). Thus, matter is made out and understood through language and so is language for matter.

This book is valuable not only for understanding new materialist theory in general, but also for rethinking perceptions of dichotomies such as nature/culture, subject/object or reality/representation. In addition, it provides us with a new ontology based on previous social and scientific theories. It is a move towards the present new paradigm which allows us to leave infinite paradoxical dichotomies which often (and especially in posthumanist accounts) have stopped feminism, and social movements in general, in their political fight.

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Jonathon W. Moses and Torbjørn Knutsen, *Ways of Knowing. Competing Methodologies in Social and Political Research*, Basingstoke: Palgrave Macmillan, 2007. 344 pp.

ISBN-13: 9780230516656 (pbk).

Key Words: research methods, epistemology, research design, positivism, interpretivism, constructivism

While many textbooks conveniently opt for a shopping-list approach to the description of research methods in the current social sciences, *Ways of Knowing* retraces their historical and intellectual lineage, focusing on the evolution of their philosophical underpinnings through time. The book is a successful attempt “to encourage students to be sensitive to the methodological priors of social scientists, and to become more conscious and aware of how these priors affect [their] work” (p. 15). Those interested in the development of the social sciences since the time of Francis Bacon have much to gain in reading this rich contextualisation of ideas.

The book’s division deliberately accentuates the contrast between two major perspectives in the study of society. The first section empha-

sises the importance of naturalism in the initial constitution of social inquiry as a scientific practice, structured around the experimental, statistical, case-oriented, and historical approaches. The authors provide a brief discussion of the various limits and perceived flaws of the naturalistic approach, most notably in relation to “law-like patterns” and visions of the world “as a single entity” (pp. 148-149). These critiques help to explain the emergence of alternative frameworks of understanding grounded in constructivist and interpretive methods, which are the subject of the second section of the book. The authors’ historical overview transcends disciplinary boundaries, making it an appealing approach to social scientists, regardless of his or her research topic and preferred method of inquiry.

A great strength of the book re-

sides in its in-depth coverage of both classic texts and recent scholarship in social and political theory. The works of Leopold von Ranke, John Stuart Mill or Émile Durkheim — to point out only a few — are given extensive attention by the authors, alongside contemporary discussions shaping the current methodological debates in academia. Similarly, the work of Karl Popper, Carl Hempel, Thomas Kuhn and other essential readings from the philosophy of science find their way into all chapters. From a practical perspective, then, their writing elegantly solves the dilemma that the postgraduate student faces when having to select classic texts versus up-to-date readings by putting them in dialogue with each other. The author handles an eclectic range of sources superbly, carrying the reader from Galton's exploration of statistical research (p. 76-sq.) to Immanuel Kant's thoughts on sense perception (p. 170-sq.). The one topic that arguably receives a slightly less informed treatment is comparative analysis, as the authors refrain from engaging recent literature on it. Specifically, their coverage of classical approaches (from John Stuart Mill to Przeworski and Teune) eludes some of the important debates in this field, such as the work of Giovanni Sartori on concept formation and comparing 'small-N' cases with 'low degrees of freedom' (Sartori 1970; see also Collier and Gerring 2008). More precise ac-

counts of comparison as a method of analysis can be found in Newton and van Deth's (2005) excellent history of comparative politics.

The authors conclude their inquiry with a call in favour of methodological pluralism, which they had already announced in their opening pages. But the final chapter also develops another interesting argument that distances itself from the epistemological equivalent of the *e pluri-bus unum* motto. That is, they do not attempt to reconcile the successive historical narratives presented by suggesting broad principles to follow in order to produce valuable knowledge on society. Instead, the authors claim that they "are skeptical of any attempt to create a new hegemonic vision of science", which leads them to stress "the need to encourage problem-driven (not methods-driven) science" (pp. 289-290). Accordingly, the authors' subsequent argument on "methodological rapprochement" suggests reflexive cross-fertilisation instead of an uncanny marriage of approaches. This final statement resonates with the authors' own methods pursued in *Ways of Knowing*.

At this point, one might underline the role a philosophy of history plays in defining one's approach to science. *Ways of Knowing* does not describe a Hegelian narrative of scientific knowledge, driven by dialectical steps from naturalism to constructivism and then into a joined-up version of both. Instead, the book

supports a Kuhnian representation of “wondrously different” (p. 285), and eventually incommensurable approaches, historically connected by paradigm shifts rather than continuities. Any idea of a final synthesis sealing the tension between naturalism and constructivism would seriously contradict the message supported by all the previous chapters: that disagreement over analytical perspectives is essential to the dynamics of science itself. The authors’ reasoned pluralism is hence very distinct from any “hegemonic” project over the intellectual dispositions of scientific inquiry.

In *Ways of Knowing*, the reader will find an ideal springboard from where to situate more specific discussions about the contemporary issues generating some of the most passionate debates within the social sciences (for an excellent example of such a view from the perspective of an American political scientist, see Hall 2007). The book stands out as a particularly valuable addition to the methodological and philosophical curriculum of the social sciences through its provision of a detailed historical inquiry of approaches to its practices.

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