Masculinism, Emplacement, and Positionality in Peer Review*

Lawrence D. Berg
Okanagan University College

In this article I examine peer review (refereeing) of manuscripts for geography journals, focusing in particular upon the discourse of refereeing. I suggest that this discourse is constituted through seemingly banal practices and that it constructs and positions referees, the conceptual space of geography, and the knowledge produced by academics about specific places. Drawing upon feminist theory, I suggest that the dominant practice of “blind” (and “double-blind”) refereeing relies upon a masculinist model of “objectivity” that is disembodied, impartial, and unlocated. This approach to peer review, I argue, genders geographic theory, reconstitutes abstract Cartesian space, and effaces place.

Key Words: emplacing knowledge, feminist theory, masculinism, partial knowledges, peer review.

Introduction

A little more than five years ago, I undertook my first ever task as a peer reviewer of a manuscript for a journal—an experience that had a profound impact on my thinking about peer reviewing and the journal publication process. In my role as a neophyte reviewer, I was struck by two aspects of the reviewing process. First, I noted how powerfully my task positioned me as a subject of knowledge. Second—and perhaps more importantly—I was quite astonished by my own embodied reaction to this discursive positioning. I felt that the discourse of refereeing—that is, the hidden set of expectations, norms, and ideologies that give refereeing meaning (Barnes and Duncan 1992) had constituted me as an outside expert, brought in to pass judgment on the quality of the manuscript under review (see, e.g., Polak 1995). I was also struck by the forceful way that the discourse of peer reviewing required me to say something significant and critical about the paper in question (Bakanic, McPhail, and Simon 1989). What really amazed me, however, was the visceral reaction I had to this positioning as an ostensible expert. A range of emotions—excitement, pride, fear, desire—rushed through my body when I read the letter requesting my participation as a referee. Interestingly, and in spite of the numerous times I have acted as a referee since, I still have similar embodied reactions to being asked to referee papers.

This article arises from these very personal experiences as a first-time peer reviewer, and from my response to the underlying discourse of peer review in geographic publishing. I argue that this discourse is constituted through seemingly banal practices and that it constructs and positions referees, the conceptual space of geography, and the knowledge produced by academics about specific places. Although it is now commonly accepted—by feminist and post-structuralist geographers, at least—that knowledge is partial and situated (Haraway 1991), the dominant practice of “blind” (and “double-blind”) refereeing relies upon an older, masculinist model of “objectivity” that is disembodied, impartial, and unlocated. This approach to refereeing, I argue, genders geographic theory (as masculine), reconstitutes abstract Cartesian space, and effaces place.

In order to counter this, I argue that it is time to transform the anonymous (blind) review of manuscripts submitted to geography journals to a more dialogical approach (Bakhtin 1981). Although I am uncertain about the best way to proceed, I would like to suggest two possible options. The first is to adopt a system of signed reviews for geography journals, an approach that would accord with similar calls by a number of other authors (Goodstein 1995a, 1995b;
Symanski and Pickard 1996; but compare Bondi 1998). A second option, which I believe is more preferable, would see the retention of “anonymity” in the reviewing process, but also the adoption of explicit structural measures to position authors and referees.

I believe it is important for authors to know something of the position (social, epistemological, and material) of those who have reviewed their manuscripts. Likewise, given that geography matters, it might also be useful for referees to know the position of those who authored manuscripts, thereby helping to embody and emplace the process somewhat. I suggest that some knowledge of the particular perspective that a given embodied scholar brings to their review of manuscripts will better equip authors to respond to the issues raised by referees. I am not suggesting that we can (transparently) know the spaces and positionings that authors and referees occupy as part of the processual relations of academic publishing. However, I would like to suggest that some measures to position authors and referees might work to contest the abstract transparency of Cartesian notions of space implicit in the blind refereeing process. In this way, we might extend a dialogic process in order to effect some changes in the masculinist production of space in academic texts. My aim is to subvert, in some small way, the hegemonic notions of disembodied masculine knowledge and false “objectivity” that underpin the anonymous refereeing process.

**Writing Refereeing**

Peer review can be traced back at least to 1656, when the Royal Society of London began publication of the *Philosophical Transactions* and implemented a system of scientific peer review to ensure high standards of publication (Chubin and Hackett 1990; Hirschman 1994). The process as we know it today came to the fore in the postwar U.S., where it was institutionalized along with the relationship between the state and science (Hirschman 1994). The term “peer review” refers to a number of related processes: review of proposals for research funding, evaluation of ongoing or completed research projects, evaluation of teaching, and the review of manuscripts for publication in academic journals. It is the latter process, one which many geographers term “refereeing,” on which this article focuses. Much has been written about refereeing, most of it focusing upon the questions of impartiality, fairness, and the quality of the process. For example, studies have been undertaken that have attempted to ascertain if the process of refereeing can distinguish quality among submissions to journals (Peters and Ceci 1982). Others (Blank 1991; Laband and Piette 1994) discuss the differences between single- and double-blind approaches to refereeing in terms of their impact on manuscript quality (also see Yankauer 1991). A few studies have attempted to construct measures of the “reliability” of the refereeing process—that is, the ability of referees to differentiate good quality (as measured by citation counts) from poorer quality work (Hargens and Herting 1990; O’Brien 1991).

By far the largest number of papers—including several by geographers—have discussed the “gatekeeping” role of referees. These works focus specifically on the process of refereeing itself—the “rules of the game,” and whether or not referees fulfill expectations of “impartiality,” “objectivity” and “timeliness” in their reviews (e.g., Hamermesh 1994; Berry 1995; Polak 1995; Boots 1996; Symanski and Pickard 1996). Finally, a number of people have written descriptive accounts of refereeing in order to inform prospective authors about the editorial and review process of journals (e.g., Brunn 1988; Wheeler 1993, 1995; see also the series of related articles by Hart 1990, Taylor 1990, and Orme 1990), or to “improve” the refereeing process itself (Glesler 1986). With few exceptions (e.g., Bondi 1998), these works and the arguments they present draw on well-rehearsed (positivist) understandings of the practice of “science.” They can thus be seen to gain their meaning from the discourse of science. This discourse emphasizes objectivity, rationality, impartiality, and disconnectedness.

Interestingly—and in spite of the fact that most analyses of refereeing understand it to be a social process of some sort—very few studies have discussed social aspects of refereeing in any thoroughgoing sense. The theme of scholarly misconduct seems to be most prevalent among those works that discuss the social implications of peer review. For example, Brian Berry (1995) provides anecdotal evidence of the “misuse and manipulation” by referees reviewing work that he and some colleagues submitted to a leading housing journal. David Good-
stein (1995a, b) discusses the increasing lack of ethics in peer review arising from the competitive character of publishing. He (1995a, 618) argues that “. . . referees are never called to account for what they write in their reviews. As a result, referees are able, with relative impunity, to delay or deny funding or publication to their rivals.” Richard Symanski and John Pickard (1996) echo these sentiments in places, although they present a much more nuanced general discussion of refereeing. They (1996, 177) suggest that, while there are a great many individuals whose honesty cannot be questioned, there are those “enemies and competitors” of authors for whom the anonymous refereeing process “provides a good opportunity to belittle the work of another through an apparently ‘objective’ attack on the merits of a manuscript.”

Within such works, social process is understood within a discourse of liberal individualism that locates social action at the level of a bounded, unified, individual subject. Analyses of the problems of anonymous refereeing thus center upon psychologistic explanations, leading to a focus on such things as the (dis)honesty of reviewers, manipulation of the process, personal vendettas by enemies of authors, and competition for scarce resources (journal space). There is little sustained examination of social or epistemological structures that might constitute refereeing, so as to frame the knowledge produced in journals in specific ways.

This is not to say that there have not been at least a few analyses of refereeing that examine specific social aspects of the process in more detail. For example, Charles Leslie (1990) examines peer review, science, and ideology as they relate to the publication of a racist article by Canadian psychologist J. Phillippe Rushton and his colleague Bogaert (1989) in the journal Social Science and Medicine. Leslie argues that the referees of the Rushton and Bogaert manuscript were affected by ideologies of positivism, liberalism, and taken-for-granted notions of “race” in a way that “blinded” them to the racist character of the manuscript. Focusing upon “race” as a socially constructed category (as opposed to a natural, biological one), Leslie (1990, 904) observes that “Rushton's paper may have appealed to the reviewers because it affirmed a commonsense way of thinking about race.”

Much the same could be said about the way that gender has been treated by almost all of those few works that discuss its role in refereeing (e.g., Key 1990; Blank 1991; Boots 1996). For the most part, such works make a now unsustainable distinction between biological “sex” and socially constructed “gender” (see Butler 1990), although some do not do even this. Ultimately, conceptual slippage between sex and gender means that absolute sexual difference comes to underpin a binary division between “male” and “female” that is taken for granted as natural and immutable. Some works, such as Diana Dinitto’s (1990) brief discussion of the question of gender in journal refereeing, do begin to ask questions about the relationship between a socially constructed gender and refereeing. However, even Dinitto’s work focuses on the impact that the gender of authors and referees will have upon the “objectivity” of the manuscript review process.

Although she focuses her argument on the issue of “anonymity” in the refereeing process, Liz Bondi (1998) presents a nuanced discussion of the complex social context of refereeing in geography. She bases her discussion on her experiences as a coeditor of Gender, Place and Culture and a recent informal survey that journal did of over 200 of its contributors (both referees and authors) asking for feedback on the refereeing process. I will return to her comments later.

Notwithstanding Bondi’s (1998) contribution, I have yet to find any analyses of the manuscript refereeing process that center on the way that the discourse of refereeing constructs and positions both knowledge and subjects. Drawing on poststructuralist understandings of the mutually constitutive effects of language and social practice, I attempt in the remainder of this article to illustrate how this discourse genders geographic thought, reconstitutes Cartesian space, and effaces place.

Before doing so, however, I wish to make clear that my comments focus upon the discourse of refereeing—that is, on the structures of signification and practice that give refereeing meaning in academic life. I am not focusing directly on refereeing as actually undertaken by individual referees. I should also make it clear that it is difficult, if not impossible, to separate the discourse of refereeing from actual practices. The practice of refereeing may be framed by the discursive meaning of refereeing, but new practices can also transform discourses. In making such a distinction, I draw upon the
path-breaking work of J. K. Gibson-Graham (1996, 252, note 2), who distinguishes between “prominent ways of representing capitalism” and “actually existing capitalism.” In this sense, I argue that there is a hegemonic discourse of refereeing which draws upon certain taken-for-granted understandings of objectivity, rationality and universality that constitute our understanding of refereeing. Of course, individual authors, editors, and referees may articulate with this hegemonic discourse in different ways, such that the “actual” practice of refereeing may not always match its discursive counterpart. As Gillian Rose (1993, 10) argues, geographers “are caught in a complex series of (historically and geographically specific) discursive positions, relations and practices.” At best, the relationship individual authors and referees have to the hegemonic discourse of refereeing is likely to be ambiguous, contradictory, and problematic.

The theme of contradiction can describe my own analysis of refereeing through “normal” peer-reviewed academic publication channels such as this journal. In this sense, there are significant tensions inherent in contesting hegemonic practices of academic peer review from within academia (see Bondi 1997 for a fuller examination of such tensions). Suffice it to say that critical academics must often make choices about what kind of hegemonic practices to contest and how to contest them. Working outside academic practices often results in marginalization. Contestation from within academia may be equally marginal, but at least it offers the prospect for different kinds of change than those produced externally. Ultimately, however, we should not overestimate the opportunities for critical or progressive change within an academy largely dominated by conservative approaches and interests. Nevertheless, in focusing on the contradictions and tensions of peer review, I will draw on some of the exemplary (both good and bad) aspects of peer review, which I experienced during the production of this article. In doing so, I hope to illustrate how individual experiences may articulate with wider discursive structures of meaning in academia.

**Scientific Objectivity**

The attachment of human geography and other disciplines to “science” is evident in the very term “social sciences” that many human geographers use to describe their work. The term arose partly as a response to the scientific-technological revolution of the postwar era in the West. This far-reaching revolution, which saw the increased coupling between science and technology and in turn between science and the state, in some ways compelled human geographers to adopt a more “scientific” approach in order to better compete for their share of increased state funding for sciences (see, e.g., Johnston 1991).

This provides a partial explanation for the “quantitative revolution” in geography. I would like to suggest, however, that the coupling of science and masculine rationality (see, e.g., Keller 1985; Seidler 1994; Connel 1995) provided an equally important impetus to the adoption of science as the dominant model for human geography in the 1960s. Just as it was in the scientific revolution of the seventeenth century—where science was conceived “as a masculinist practice, as part of the development, as Francis Bacon thought of it, of a new masculine philosophy” (Seidler 1994, 6)—the scientific-technological revolution of the twentieth century was closely linked to hegemonic masculinities (Cockburn 1985; Keller 1985). I shall return to this point later.

Although the science model dominated during the quantitative-theoretical period of the 1960s and 70s, its hegemony is now contested in geography by postpositivist approaches (e.g., Barnes 1993; Dixon and Jones 1996). Yet, despite the decreasing importance of “science” in geography, many geographers continue to believe that the external world can be understood through disinterested objective study. This is what Rose (1993) terms geography’s “architectonic impulse.” This impulse is reflected in the discourse of refereeing manuscripts for journals, which tends to remain locked into a scientific model. In their recent discussion of refereeing in geography, for example, Symanski and Pickard (1996, 175) “premise [their] arguments on well-known tenets concerning the nature and practice of science, broadly conceived.”

The notion of objectivity comprises a keystone of this scientific model. Such objectivity is supposed to be translated into the refereeing process by ensuring that manuscripts are refereed anonymously. The chain of signification is structured as follows: objectivity = impartiality = disembodiedness = anonymity. With this in mind, it is now “commonplace in the discipline
of geography, and the other social sciences (if not the sciences in general), to have articles or essays submitted for publication reviewed ‘anonymously’” (Symanski and Pickard 1996, 176). In the science model, anonymity in the refereeing process is not only acceptable, because knowledge is conceptualized as universal; it is also required in order to ensure impartiality and objectivity, and to prevent the intrusion of bias or subjectivity (Blank 1991; Hirschman 1994; Laband and Piette 1994). Importantly, anonymity provides some useful protections for both reviewers and authors (for junior faculty to comment on work by more senior colleagues, for example).

However, objectivity is usually constituted in opposition to subjectivity, and it is founded on interrelated and mutually constitutive notions of rationality, disembodied reason, and universality. All of these ideas have important implications for the gendering of geographic theory and the epistemological constitution of space in geographic writing.

**Disembodied Masculine Knowledge**

Within the Enlightenment tradition, “we inherit a view of knowledge as ‘objective’ and it is through a faculty of reason, sharply demarcated from nature, that we strive for objectivity” (Seidler 1994, 23). The separation of reason from nature arises from the Cartesian mind/body distinction (Berg 1994; Longhurst 1995). The Body is part of nature—an object to be transcended by Mind, which stands apart from nature as a subject of knowledge. Body is immanence, while Mind is transcendence.

Such dichotomous concepts are powerfully gendered. Masculinity has long been associated with the mind, culture, reason, and rationality. In striking contrast, femininity has long been associated with the body, nature, unreason, and irrationality (Jay 1981; Lloyd 1984; Bordo 1986; Seidler 1991, 1994; Berg 1994). The association of mind with masculinity and the opposition of these concepts to the body and femininity form a part of what Susan Bordo (1986) terms “the Cartesian masculinization of thought,” which came about through Descartes’ separation of knower (subject) from known (object). “The mutual exclusion of res extensa and res cogitans made possible the conceptualization of complete intellectual transcendence of the body” (Bordo 1986, 450). Since the mind was already associated with the masculine and the body was associated with the feminine, Descartes’ mind/body dualism laid the conceptual groundwork for the masculine rational transcendence of the feminine irrational. In this framework, “objective” knowledge becomes a matter of masculinity. Moreover, since science is now so closely associated with objective knowledge, objectivity has come to constitute a part of what Evelyn Fox Keller (1985, 8) terms the “science-gender system.”

However, there is more at work here than the masculinization of “objective knowledge.” Cartesianism also leads to a spatialized ontology concerning the “location” of the knowing masculine subject and the kind of knowledge available to it. In Cartesian thought, both knowledge and space are wholly external to the observing mind. In this “objectivist” (Keller 1985) discourse, both space and the objects of knowledge are to be transcended. Space becomes a transparent grid in which objects are trapped. Knowledgeable masculine subjects are able to transcend this space—in effect, to rise above the Cartesian grid—to obtain an “objective” view of the world (Barnes and Gregory 1997; Bondi 1997). Scientific geographers, as Trevor Barnes and Derek Gregory (1997, 15) suggest, imagine themselves as a person—significantly, almost always a man—who had been elevated above the rest of the population, and who occupied a position from which he could survey the world with a detachment and clarity that was denied to those closer to the ground (whose vision was supposed to be necessarily limited by their involvement in the mundane tasks of ordinary life).

From this disembodied vantage, objective knowledge looks the same from any perspective: it is monolithic, universal, and totalizing. This concept of unlocated and disembodied rational knowledge draws on powerful metaphors of mobility and transcendence for its rhetorical power (see Haraway 1991; Barnes and Duncan 1997).

I would like to suggest that the discourse of blind peer review draws upon these same masculine concepts of transcendence, detachment, and disembodiment. Ostensibly, anonymity in the refereeing process is designed to ensure detachment and impartiality. Anonymous refereeing is acceptable—or, more properly, made
necessary—in order to bolster the supposed universality of scientific knowledge. In this discursive frame, a social “fact” is the same in Manchester (U.K.) as it is in Manurewa (N.Z.). Accordingly, the notion of anonymity both helps to constitute and is itself constituted by ideas of universality, transcendence, and detachment. If “true” objective knowledge must be disembodied and impartial, then anonymity is a required part of the refereeing process. The detachment of anonymity helps to ensure that the resultant knowledge meets the appropriate standards of disembodied objectivity. Given the already asymmetrical character of the political economy of academic publishing (Barnett and Low 1996) and the discursive hegemony of Europe and America (Berg and Kearns 1998), this reliance upon an ostensibly universal masculine knowledge has significant implications for the production of geographies in academic publishing.

**Anglo-American Hegemony**

Gayatri Spivak (1988) has argued that the subject of theory is Europe. In part she means that the “West” is the constitutive referent for philosophical and theoretical reflection. However, she also means that the knowledgeable agent of theory-formation is the Euro-American subject (Berg and Kearns 1998). Donna Haraway (1991) has similarly pointed out that the “master subject” of scientific knowledge is constituted as white, Western, bourgeois, able-bodied, heterosexual, and masculine (also see Rose 1993). Yet, despite the specificity of this subject, “he” remains both unmarked and unlocated in scientific discourse. This subject is the ultimate expression of disembodied Cartesianism. Moreover, I would like to suggest, it is the very same subject that constitutes the “anonymous” referee. Indeed, if the arguments of Spivak and Haraway are extended, then it becomes clear that the agent of geographic refereeing is also Euro-American (or, more specifically, Anglo-American), white, bourgeois, able-bodied, heterosexual, and masculine.

There are two aspects to my argument. First, I suggest that the unmarked subjectivity of The Geographer is Anglo-American and masculine, and by extension, so is the unmarked (read “anonymous”) subjectivity of the referee. In this sense, I focus upon the discursive constitution of the referee. However, there are some important material implications of this discursive construction of refereeing. These material considerations lead to the second aspect of my argument: that, for the most part, geographic referees are physically located in the U.K. and U.S. and are predominantly “male.” Empirical evidence for this second point is difficult to obtain, given that many journals do not list their referees, while others provide no institutional location for those they do list (Boots 1996). Nonetheless, we can find evidence that is at least indicative of the patterns I am suggesting.

On the question of gender and refereeing, Daniel Hammermesh’s (1994, 156) analysis of refereeing in seven economics journals during the period 1989–1990 found that 90 percent of the referees were male. Hammermesh (1994, 156) puts this dominance down to the fact that it closely mirrors “the 91 percent male faculty in Ph.D.-granting economics departments.” Given a similar predominance of male faculty in geography departments (McDowell 1990; Longhurst 1994), the overwhelming prevalence of male referees in economics is likely to obtain for geography journals as well. Barry Boots’ (1996) study of refereeing in seven geography journals between 1988 and 1992 tends to confirm this speculation. His work shows, for example, that of the twelve geographers who acted as referees on more than ten occasions, only one (Susan Hanson) was female (1996, 180, table 2). Moreover, his list of those nineteen geographers who acted as referees for more than five of the seven journals examined contained no women referees (1996, 181, table 4). Just as referees are principally male, so too are they predominantly located in the U.K. and the U.S. As Boots (1996, 180) observes, the group of nineteen geographers who acted as referees for more than five journals “contains a preponderance of individuals who were resident in North America during the period studied.” Ironically, Boots reinforces this Euro-American hegemony by deciding not to select *Australian Geographical Studies* for his study. He (1996, 184, note 1) argues that, “since [Australian Geographical Studies] is ‘primarily concerned with the geography of Australia and its neighboring regions,’ it was felt that its referees would be much more localized geographically than those of the other journals examined.” Interestingly, to be centered in Australasia is to be “localized
Masculinism, Emplacement, and Positionality in Peer Review

geographically” (read “parochial”), but the same does not apply if one is centered in North America (see Morris 1992; Berg and Kearns 1998).

Accordingly, the blindness of blind refereeing is perhaps more extensive than we might have thought. I argue that, as currently conceptualized, anonymous refereeing reconstitutes the hegemony of the unmarked “master subject” of geography: bourgeois, white, Anglo-American, heterosexual, able-bodied, and masculine. Thus, the anonymous refereeing process is also blind to the location and specificity of referee(s), author(s), and their embodied knowledge about specific spaces and places. Indeed, anonymous refereeing reconstitutes reliance upon a universal form of (Cartesian) knowledge that faces the specificity of places. This is made worse by of the extant asymmetries in academic knowledge production that emphasize the centers at the expenses of the margins (Slater 1992, 1993; Dodson 1995; Berg and Kearns 1998).

The end result, as Meaghan Morris (1992, 471) observes, is that “Americans and Europeans often assume that we are abstracted like a footnote from their history and devoid of any complicating specificity in intellectual and cultural history.” Morris speaks specifically about Australia, but her comments apply equally well to most other nonmetropolitan locations.

Worlding the Referee

Over the last decade, social geographers have conceptualized space not as merely a container in or through which humans move—what Henri Lefebvre (1991) terms “abstract space”—but instead as a concrete and constitutive element of social life (e.g., Gregory and Urry 1985). There has also been a significant “spatial turn” in the human sciences more generally, with a resultant emphasis upon the spatiality of social life. Emblematic of this turn to space are the various spatial metaphors—“politics of location” (Anzaldúa 1987; Frankenberg and Mani 1993), “cartographies of struggle” (Mohanty 1993), “power-geometries” (Massey 1993), and “situated knowledges” (Haraway 1991)—that have been adopted primarily by feminist theorists in order to critique masculinist and Eurocentric concepts of universal knowledge (Berg and Kearns 1998). As Bondi (1997, 248) points out, “[such theorists] contribute to critiques that challenge the mastery of dominant knowledge systems, that emphasize the corporeality of knowers, reject the radical split between mind and body associated with the notion of transcendence and that argue that all claims to a singular, universal position are fraudulent. They argue instead, partly via their distinctive spatial metaphors, for critical, situated knowledges.”

Haraway’s (1991) evocative metaphor of situated knowledges provides perhaps the most useful trope to contest unlocated forms of knowledge. She argues that, within the present hegemonic ideologies of scientific knowledge, objectivity must be seen as a “God Trick” of seeing everything from nowhere. She posits a different concept of objectivity, one that attempts to situate knowledge by making the knower accountable to their position. All knowledge is the product of specific embodied knowers, located in particular places and spaces: “there is no independent position from which one can freely and fully observe the world in all its complex particulars” (Barnes and Gregory 1997, 20). Haraway uses spatial metaphors of location to highlight epistemological spaces of knowledge. Wendy Larner extends this argument to include “real” material spaces and places. She (1995, 177) argues that positionality “involves not just positioning in a theoretical and ideological place, but also in a geographical location and, by implication, the politics of that place.”

Given these arguments about situating knowledge, it seems rather ironic that we continue to accept the “scientific” rationale for anonymous refereeing in geography journals. This irony is even greater in geography given that we are ostensibly interested in the difference that space makes. If, as Doreen Massey (1984) has so cogently argued, “geography matters,” then it is time to focus our geographical imagination on some of our own banal academic practices, including refereeing. With this in mind, I suggest that it is long past time for us to rethink the way we conceptualize the role of anonymity in the refereeing process.

As I have already suggested, a number of options exist for rethinking the blind review process. First, we might abolish the anonymous refereeing process, opting instead for a system of signed reviews. Symanski and Pickard (1996, 177) promote this approach, albeit for different reasons from those I have presented here. In-
indeed, they approach the issue with two underlying premises:

(1) everyone has agendas and prejudices, and some people not only dislike certain members of their profession but may also be quite prepared to apply their agendas and prejudices against those whom they dislike; and (2) people vary immensely in their willingness to be honest with themselves and with others . . .

Using these premises as the basis for judgments about the motivations of referees, Symanski and Pickard (1996, 177) suggest that referees “are much more willing to make use of their agendas and prejudices if their identities remain concealed.” If we accept Symanski and Pickard’s two premises about the hidden agendas and (dis)honesty of referees, then their conclusions about anonymity in the refereeing process are logical ones.

However, there are reasons to be wary of their premises. Their arguments, for example, draw upon well-rehearsed liberal humanist understandings of human subjectivity that have come under fire from poststructuralist and psychoanalytic geographers (e.g., Pile and Thrift 1997). Further, my own limited experiences as guest editor of the *New Zealand Geographer* and as editor of *SITES: A Journal for South Pacific Cultural Studies* leave me with a very different impression of the motivations of referees. Referees with whom I have come into contact either as editor or as author have been fair, diligent, and committed to providing constructive criticisms for authors.

Similarly, Bondi’s (1998, 293–94) experience as coeditor of *Gender, Place and Culture* “diverges so substantially from the picture portrayed by Symanski and Pickard that [she was] prompted to present another perspective.” Drawing upon responses to an informal survey sent out to over 200 contributors (authors and referees) to *Gender, Place and Culture* asking for input on the refereeing process, Bondi (1998) makes a cogent argument for the retention of anonymous refereeing. An excerpt from one of the submissions of a respondent to that informal survey is quite useful in illustrating the thrust of those arguments:

I think anonymous reviewing is the best—or least worst—arrangement. It means that reviewers can say exactly what they think without feeling compromised by friendship or other associations. I myself have made fairly critical comments on a paper written by a friend or colleague which simply couldn’t have been expressed as directly had my identity been known. Of course, it is possible to imagine situations where referees use this system to make unfair and negative attacks on people—in such cases, it is the editors’ job to decide whether the tone and spirit of the referee’s report is consistent with the journal’s policy, and to intervene or edit them if necessary. (cited in Bondi 1998, 295)

I am convinced by the argument that Bondi (and her respondent) make concerning the positive aspects of anonymous refereeing. However, to accept their argument is not to conclude that the anonymous refereeing process should go unchanged. Indeed, if my arguments above obtain, then a need still exists to alter the structure of the refereeing process in order to allow for the partial, embodied, and emplaced character of geographic knowledges. Yet, as Bondi (1998, 295) observes, “to insist that knowledge is always situated, local, and partial is quite different from equating it with its author’s (or enunciator’s) identity.” This is very true, and it seems to me that there must be other ways of positioning authors and referees that, ultimately, transform the discourse of refereeing.

### Transferring the Structures of Refereeing

Within the present structure and discourse of refereeing, most editors ask referees to make one of four recommendations on manuscripts: (1) acceptance without revision (rare); (2) acceptance with minor revision; (3) rejection with an invitation to resubmit after major revisions; or (4) outright rejection. Referees are also asked to make substantive comments on manuscripts, including such issues as suitability for the journal, contribution to theory or techniques, and contribution to understanding of the substantive research question. None of these questions positions the referee in any way. With this in mind, I would like to suggest a number of structural changes to the peer review process that might link the discourse of refereeing with recent understandings of partial and situated knowledge.

As one aspect of such structural change, I suggest that journal editors should require, as part of the referee’s duties, that they attempt to “po-
position” themselves in a couple of ways. First, referees would need to position themselves within present theoretical debates, which might involve identifying with a specific theoretical approach. At the very least, given the wide range of theoretical approaches, referees should make clear the epistemological and theoretical perspectives upon which they draw for their review of a given manuscript. Referees should also be required to position themselves in a material sense, actually locating themselves geographically. I realize this might seem naïve to readers. Certainly it struck a chord with reviewers, one of whom, for example, asked “what difference [it would] have made to have prefaced my comments” with the above kinds of positionings. It is difficult to respond to such questions except to say that, as a geographer who trained and worked for a significant amount of my academic career in the Southern Hemisphere, I maintain that they highlight the normative and unmarked character of Eurocentrism. For those who work outside hegemonic centers, it very clearly makes a difference to know that a reviewer is speaking from one of the geographic centers—or from the margins. Surely, if geography matters, it must make a difference in the social relations of academic knowledge production (Larner 1995; Berg and Kearns 1998). Theory travels, but not very well; thus, social and spatial theories need to be understood according to their specific historical permutations and geographical contexts.

A second structural change to the peer review process should focus on the journal editor: journal editors should be required to outline their rationale for choosing a specific set of referees. The importance of this structural issue was highlighted by Stuart Aitken’s reasons for choosing the four referees who reviewed the manuscript:

Blind review is the policy of the PG, and so I sent your work out anonymously. To attain a relatively balanced set of reviews I sent the paper out to ex—journal editors, feminists/poststructuralists and (for balance) a structuralist. The reviewers were U.S.- and U.K.-based (how’s that for “Worlding”) and I reveal their identities only because I have their permission to do so. (Aitken 1999, personal communication, 1)

As he (2) went on to explain, “[c]hoosing reviewers is a difficult task for an editor [and] how heavily an evaluation is weighed will be up to the editor.” Indeed, the choice of reviewer is highly significant to the final outcome of any peer review process. This is so precisely because there are important differences in theoretical orientation, substantive experiences, and academic placement in any given set of reviewers. If authors have an understanding of why editors have chosen particular reviewers, then they can respond to a review with more knowledge of its context. More importantly, such a requirement may have an impact on the structuring of knowledge in academic geography, highlighting as it does the partial character of all knowledge.

Another structural change relating to editorial practices would be designed to respond to some authors’ concerns regarding professional conduct (Berry 1995; Symanski and Pickard 1996). In this regard, editors should be required to send copies of the reviews, and their cover letter to the author(s), to all the referees. This is common practice at a number of journals. It assists with quality control, and it provides another check on the referees’ professional conduct.

As Rose (1997) has argued, we cannot know fully our position(s) or all the reasons for our actions. Moreover, the search for positionality through transparent reflexivity is bound to fail because it relies on specific “notions of agency (as conscious) and power (as context), and assumes both are knowable” (Rose 1997, 311). Poststructuralism and psychoanalysis have certainly taught us that this is not possible; the subject is fragmented, contradictory, and not fully knowable. In addition, identities and positions are always produced as relationships, rather than as some kind of essential characteristics. To respond to this dilemma, we need to think of positioning, not in terms of “identity,” but rather in terms of location with specific institutional practices, economic processes, material contexts, and geographic settings (e.g., Nagar and Geiger 2000).

While certainly not perfect, the structural changes I have outlined above—attempts at “worlding the referee”—might result in a more dialogical relationship between authors, editors, and referees. While we are not able to transparently position ourselves, we can engage in a process of identifying institutional, material, and geographic locations within a dialogical process. Such practices might, in some small way, lead to transformation of the discourse of refereeing—the discourse that con-
structs knowledge as universal, monolithic, dis-embodied, and unlocated. At the very least it might help to situate knowledge and the theories produced by geographers, leading to a more contextual understanding of place. In this way, it might help to subvert the masculinism and Eurocentrism of academic texts by contesting hegemonic notions of disembodied knowledge and false objectivity that underpin anonymous refereeing.

**Literature Cited**


LAWRENCE D. BERG is an Associate Professor of Geography at Okanagan University College, Vernon, BC, Canada. E-mail: Lberg@Okanagan.bc.ca. His principal areas of interest are geographic epistemologies, identity politics and place, masculinities and space, and geography in higher education.