

Forum

Snapshots of What, Exactly? A Comment on Methodological Experimentation and Conceptual Foundations in Place Research

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Place ideas in natural resource management have grown in recent years. But with that growth have come greater complexity and diversity in thinking and mounting confusion about the ontological and epistemological assumptions underlying any specific investigation. Beckley et al. (2007) contribute to place research by proposing a new methodological approach to analyzing attachments to place and exploring the relative importance of biophysical versus sociocultural attributes in determining place attachment. While our thinking has benefited from their contributions to place research, we see an increasing need to clarify the multiple and competing paths for place research easily obscured in the heap of similar-sounding place concepts. Our commentary cautions against philosophically unguided methodological experimentation and offers some critique of their conceptual approach to place.

Keywords commodity metaphor, critical pluralism, epistemology, mixed methods, place

Beckley et al. (2007) describe their article as a “methods experiment.” At the heart of their experiment is an attempt to wed an ethnographic/qualitative method of data collection with a positivistic/quantitative method of data analysis. However, we maintain that Beckley et al. are mixing, not methods, but distinct research paradigms. Their attempt to do so raises two important epistemological questions: (1) Can two disparate paradigms be integrated in a single research design, and (2) what evaluative standards should be applied in the case of such a mixed-paradigm research design?

Research methods are the most basic building block of research design. However, the same method can be deployed in vastly different ways depending on the research goals, researchers’ judgments about the relative importance of competing threats to validity, and other epistemological assumptions. In contrast, paradigms are approaches to science that define the normative philosophical commitments that

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guide the actual implementation of methods. While any research design inherently requires mixing methods, attempts to mix divergent paradigms across data collection and data analysis can have significant consequences with respect to internal consistency among different methodological phases. Here, we address three inconsistencies we believe are apparent in the Beckley et al. methodological experiment.

Beckley et al. state that they chose resident-employed photography accompanied by interviews as the means of data collection because this ethnographic approach provides "an extraordinarily rich device for entering into deep, detailed and meaningful conversations" (p. 926) about place attachment. However, this richness is deliberately sacrificed in the data analysis phase because coding adheres to positivistic criteria that prioritize simplicity. Specifically, simplicity was attained in part by the use of fairly generic codes and by coding only the primary response, "[thus] simplifying the complex and inter-related meanings that were articulated" (p. 925). The result is that the analysis actually presented is not of the rich ethnographic database but of the highly simplified category codes. Further, the attempt to transform the rich ethnographic database into a simplified coding scheme created what appears to be a high degree of interrater disagreement in coding (one-third of the time at least 50% of the coders disagreed and another 30% of the time at least one of the coders disagreed).

The issue of intercoder disagreement is related to a second apparent internal inconsistency. Ethnographic approaches use interviews to allow for thorough answers and to avoid imposing the researchers' categories on the subject. Indeed, Beckley et al. state that they used interviews "to avoid constraining subjects' answers to . . . predetermined responses . . . taken from the literature" (p. 918). However, there is a significant question about whose categories (respondents' or researchers') the data ultimately analyzed actually reflect. As Beckley et al. state, "Each time a category was cited by one of the four researchers, it became a data point . . . data coding . . . yielded four distinct sets of data, one for each researcher . . ." (p. 926) Thus, while the original interviews reflect rich descriptions in the respondents' own words, the analyzed data set is based on what appear to be a priori researcher-defined categories, and the analysis represents the collective amalgamation of how that coding scheme should be deployed by four different researchers who were often not in agreement. Collectively this approach to data analysis seems to negate the originally stated goals of ethnographic data collection.

A final internal inconsistency stems from the explicit instructions to interviewees versus the strategy apparently followed by coders. Interviewees were instructed "to be creative," for example, "taking a picture of a gravesite to represent history." Interviews were then used so that respondents could describe how the photographs reflected their attachments. Thus, these instructions encouraged respondents to take photographs whose content was symbolic rather than literal and suggests that the photographs and the interviews constituted a single integrated data set. In contrast, the coding scheme seems to have created a tension between the photographs and interviews that seemingly belies these instructions. For example, consider the respondent who explains a photograph by noting: "This is where I lost my virginity . . . The history really defines that spot for me." According to Beckley et al., the analysis scheme created a dilemma as to whether to code the photograph as biophysical (and within this category whether to code it as landscape or water) or social, despite acknowledging that "the photograph was taken "to depict a historical event" (p. 923). Thus, the coding scheme seems not only to have imposed predetermined, researcher-based categories, but also to have

encouraged coders to take the content of photographs literally and to treat photographs and interviews as separate sources of information with no clear priorities for which to privilege.

These apparent internal inconsistencies give rise to the second epistemological question: What standards should be used to evaluate the research? Typically, evaluative standards are developed, debated, and accepted at the paradigmatic level. To promote both diversity in perspective and fairness in the review process, a critical pluralism advocates evaluating a specific study according to the appropriate paradigmatic standards rather than the standards of some alternative paradigm (Patterson and Williams 2005). However, when paradigms as disparate as the two reflected in this study are experimentally mixed within a single research design, the question of appropriateness and fairness in evaluative standards becomes problematic.

First, positivism (which the authors indicate is the basis for the actual analysis) is often associated with an emphasis on standardization (to attain objectivity through the removal of personal judgment) and replicability (made possible in part by standardized and reproducible methods). Because disagreement among the four raters in the study seems rather high, this analysis does not seem to have a high degree of replicability through standardization. Further, positivist research values statistical inference, yet the fact that the data set for analysis represents an aggregate of ratings of the same stimuli by four different researchers seems to prohibit the use of inferential statistics due to the problem of independence. Under these circumstances it seems likely that researchers adopting positivist standards would prefer a more traditional survey approach in which the respondent, not a bank of researchers, interprets their experiences in light of the researchers' predetermined categories and for which widely accepted "critical tests" (e.g., reliability, confirmatory factor analysis, etc.) have been developed.

In contrast, "ethnographic" paradigms explicitly acknowledge the problematic nature of replication and interrater reliability, hold philosophical stances in which multiple interpretations legitimately coexist, and therefore offer alternative evaluation standards. For example, Giorgi (1975, 96) suggested that a principal evaluation criterion is "whether a reader, adopting the same view point as . . . the researcher, can also see what the researcher saw, whether or not he agrees with it." This standard seems more suited to Beckley et al. because the authors often did not agree on classification of photographs, but apparently could see each others' rationale since they included all coders' ratings in their analysis. The difficulty in applying this standard to Beckley et al. is that readers do not have sufficient access to these widely contested judgments to assess for themselves how warranted the coding is because the data reported are frequencies of codes rather than the data justifying actual assignment of codes.

Further, ethnographic approaches tend not to make as strong a distinction between data collection and data analysis as reflected in this study. For example, in an ethnographic interview, the role of the researcher is to probe and clear up ambiguity (e.g., is the picture about the landscape, the water, or the lost virginity) during the course of the interview, rather than to withhold judgment until subsequent coding by independent raters. Alternatively, other ethnographic approaches employ member checks (reviews by respondents interviewed) to assess the validity of coding. A further complication with regard to ethnographic standards is the aggregate nature of the analysis, which transforms the rich interview data set into generic and abstract categories. Ethnographic approaches that use open-ended interviews typically emphasize textual analyses that seek to maintain the complexity,

interconnections, and richness of meanings that motivated the approach to data collection in the first place.

Thus, from an epistemological standpoint, the Beckley et al. methodological experiment raises two problematic issues. The first stems from the mixed methods within a single research design. In actuality, what appear to be mixed are not methods but different paradigmatic commitments between the data collection phase and the data analysis phase. Speaking to the possibility of integrating disparate paradigms, Franck (1987, 60) argued that “what one would achieve from some integration would be a strategy still based primarily in one perspective or the other” rather than a true integration. We believe this is the situation reflected in Beckley et al. The data set that is actually analyzed seems to have lost its ethnographic character, reflecting instead a data set primarily in accord with the tenets of positivism. And while the authors are admirably transparent about their methodological decisions, neither typical positivist nor typical ethnographic standards for evaluation appear to apply and no alternatives are presented.

Methods are not passive instruments. They (and the specific logic with which they are implemented) impose a structure on empirical systems that have significant implications for the nature of the empirical test (Danziger 1985). For these reasons, we believe that methodological experimentation should be attentive to the structure that various methodological choices impose on the data and that researchers should strive to emphasize the internal consistency across approaches to data collection and analysis within a specific research design. In contrast, in recognition of the fact that any single research design is an inherently limited and imperfect representation of reality, we encourage diversity in approach across different research designs within a domain of research like place (i.e., multiple methods rather than mixed methods). To be both fair and critical, this type of pluralism requires researchers to express an acceptable set of evaluative criteria for judging the merits of research. It is possible to do this without using established epistemological paradigms; the keys are presenting a transparent, internally consistent, and defensible testing logic that makes claims about knowledge well warranted and discussing the underlying assumptions or implications of methodological choices. But adopting an established paradigm is advantageous because of the amount of prior experimental work that has gone into articulating and justifying an underlying logic, exploring internal consistency, and establishing consensus regarding standards/criteria for evaluation.

Beyond these epistemological issues, a central concern with the article as an exemplar of place research is that, in our view, conceptually it has more in common with the traditional multiattribute economic/utility theories of human–environment relations that gave rise to place research as a critique and alternative in the first place. In making our argument, it is helpful to begin by describing some of the features of place research that distinguish it from other topics.

Recent comprehensive literature reviews suggest that place embodies material form, location, and something described by various terms including sense of place and place meaning (Cresswell 2004; Gieryn 2000). *Material form* refers to the material setting through which people conduct their lives, including not just biophysical (natural properties) but also the “constructed” forms typically found in human impacted spaces (buildings, streets, offices, trails, campsites). Second, places have location and dimensionality and can be conceived as nested in scales from the subatomic to the galactic. Still, while place implies boundedness, actual boundaries are fluid and contested social constructions imposed on the world. Third, and in part

a consequence of the constructed nature of spatial boundaries, what most differentiates *place* from other spatial-material concepts (e.g., environment, resources) is the way that place organizes and even constitutes human/social relations and meanings. Places are not only “materially carved out [they] are also interpreted, narrated, felt, understood and imagined . . . [Place meanings are] flexible in the hands of different people or cultures, malleable over time, and inevitably contested” (Gieryn 2000, 467). Place is more than a setting or container: It is integral to how we organize and experience the larger world, a “fundamental means through which we make sense of the world and through which we act” (Sack 1992, 1).

While many trace this contemporary place concept back to the emergence of humanistic geography in the early 1970s (e.g., Tuan 1974), it is worth noting how place ideas took root even earlier in urban sociology. Firey (1945) was among the first to ascribe “sentiment” and “symbolism” to urban spaces as a direct challenge to the prevailing instrumental view in which “the only possible relationship that locational activities may bear to space is an economic one” (140). Referring to the Boston Commons as a sacred object, Firey noted that “its sacredness derives, not from any intrinsic spatial attributes, but rather from its representation in peoples’ minds as a symbol for collective sentiments” (Firey 1945, 144).

Contemporary theory and Firey’s early work suggest two problems regarding the Beckley et al. conceptual approach to place attachment and sense of place. First, they draw attention to the artificiality of distinguishing between natural/biophysical attributes and sociocultural attributes. In particular, there is nothing compelling, universal, or “robust” about limiting the biophysical category to natural features of place. Though places indeed have material form, compared to the sentiments and symbolism associated with intensely urban places such as Boston where Firey did his original work, Beckley et al. imply a rural/natural bias in how they conceive the material character of places. Recognizing that sense of place refers typically to the existence of nonmaterial (meaning and social relations) aspects of place, it might be plausible to distinguish between the material setting on the one hand and social relations and meanings on the other. However, this misses the point being made by Firey (1945) that it is not a place’s intrinsic attributes (biophysical, social, or otherwise) that make it special and meaningful, but that over time it has become a symbol for a particular constellation of meanings and relationships.

A second problem that Firey helps us to see is that the whole point of theorizing about sentiment and symbolism is to transcend the narrow calculus of an instrumental or goal satisfaction model of human behavior. Our own disenchantment with this “commodity metaphor” motivated us 15 years ago to apply sentiment and symbolism to understanding human relationships to wildlands, protected areas, and other natural landscapes (Williams and Patterson 1996; Williams et al. 1992). As the guiding paradigm in natural resource management going back to the early days of utilitarianism, the dominant way to explain environmental preferences had been to treat them as some form of a multiattribute utility problem (e.g., in microeconomics and consumer behavior). However, in making the assertion that attachment is “produced through personal experience with sociocultural and biophysical attributes of the spatial setting,” Beckley et al. (2007) ignore the social processes embedded in culture, discourse, and other social practices of place making (Stokowski 2002) and return us to the instrumental search for “robust” if not universal environmental determinants of affect. Their approach renders place attachment and sense of place narrowly as psychological experiences *caused* by contact with specific material qualities of the

place (in this case categorized as biophysical versus social attributes). Classifying photonarratives into one or both of these broad pregiven categories (with limited reliability) offers paltry evidence to support their deterministic assertions and, as we argued earlier, appears to have more to do with what is in the photograph than with the meaning behind it.

Throughout Beckley et al. there is a tendency to conflate the notion of attributes (or classes of attributes) with meanings. We take the two to be distinctly different. Attributes imply properties, features etc. belonging to, or produced by the material nature of the place. People may disagree about whether any given place possesses some attribute (e.g., beauty), but there is an embedded assumption that there are material indicators of such attributes. In contrast, we take meaning to be understood more broadly as something socially constructed and generally symbolic (Williams and Patterson 1996). Meaning is not a property of the person or the object, but a relationship between the two mediated through culture and individual past experience.

To return to Beckley et al. with this distinction in mind, meanings are literally *in* the photonarrative coconstituted by the respondent and the investigator and go well beyond the material attributes of the place. Take the earlier example of the photonarrative about lost virginity. To their credit, Beckley et al. admit difficulty coding this narrative in terms of biophysical versus sociocultural attributes. Losing one's virginity is not a biophysical or social attribute of this place (a beach), but a personal, intimate event that happened there, which has meaning to the respondent. Anyone looking at the photograph can tell it is a beach, but the meaning behind the location represented in the photograph is *in* the story, constructed from extant cultural and local (community) meanings interwoven with personal past events and transactions with that spot (it is yet another question as to what the story behind this locale says about the respondent's attachment to and/or sense of the community). The beach did not *produce* the meaning of this spot, although, for reasons possibly embedded in the narrative, it may well symbolize something significant about this person's relationship to and feelings for the community.

In the end we agree with Beckley et al. that resident-employed photography provides a potentially useful tool for studying the nuances and complexities of place relationships, meanings, and attachments. But their strained imposition of quantitative structure on qualitative data and insistence on finding universal environmental sources of place attachment (or sense of place) elide a theory of place that can exploit much of the value we see in this technique. The particular, relational, and storied nature of meaning gets lost in the authors' aim of "taking the esoteric, conceptual discussion of sense of place down to the empirical level of identifying specific attributes" (p. 928) that "contribute meaning" and "produce attachment," to say nothing of categorizing these attributes as either biophysical or social in content. In doing so, Beckley et al. adopt implicit and explicit positions that have important implications for the study of place and, in our view, take place research down a path that is hard to reconcile with the paths blazed by pioneers such as Firey and Tuan as well as more recent contributors (Cresswell 2004). In the interest of pluralism, we do not want to restrain them from pursuing their version of place research. On the other hand, we feel compelled to point out that their approach looks like a reincarnation of multiattribute utility theory, with utility (e.g., attitude, preference, affect) rescaled as "attachment." It amounts to a turning back to earlier instrumentalism rather than "a logical next step for sense-of-place research" (Beckley et al. 2007, p. 928).

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