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Digging Disciplines Deeper: Thoughts Arising from Commentaries by Shaw, Samdahl, Dawson, and Witt

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Introduction

The conventions of writing an academic, empirical article for a research journal such as the *Journal of Leisure Research* usually dictate a rigidly-defined content that follows a pre-determined structure. The normal "Background" section, for example, typically consists of a review of the literature most closely related to the study, and—if it is done well—identifies not only what is known about the subject-matter of the study, but also what is not known. These gaps in knowledge are then used to identify the study's objectives and to provide the foundation for a subsequent discussion of how the study has contributed to knowledge (see Kelly, 1999, p. 148, for advice that all writers of such articles should heed!).

Given the relative freedom of how to position this "reply" to four commentaries on my "Leisure research by Canadians and Americans: One community or two solitudes" article, I have chosen to begin with a personal statement and not with an academic/literature background. This should not only clarify the purpose of the article that the four commentators and I are discussing, and the objectives of the larger study of which it is a part, but it should also provide the foundation for a more generally-oriented response to the comments from colleagues, as well as to point to directions and questions that could profitably be addressed in the future. I believe that these are issues that members of the North American leisure studies community ought to be concerned about.

After I had read the four commentaries on my article by Susan Shaw, Diane Samdahl, Don Dawson, and Peter Witt, my first inclination was to decline to write a reply. This was not because I felt the commentators' critiques were indefensible. On the contrary, I thought that a simple and straightforward, "I agree with everything you say; thank you for taking the time to do this" would suffice. However, as I began to reflect on my colleagues' remarks, and realizing that, rather than writing detailed critical reviews, they had instead chosen to use my article as a springboard to raise issues that were only *implied* by my data and conclusions, I decided to respond in kind. In preparing early drafts of this reply, I also found myself reflecting on how I came to be involved in recreation and leisure studies, and how the concerns that I have arose out of observations I have made over

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the years of my involvement. Thus, I intend to use this "reply" not only as an opportunity to comment on and extend my colleagues' thoughts, but also to voice some concerns about leisure studies in general that were part of my motivation for initiating the research project in the first place, even if they were not directly addressed in the original article.

Why I Began The Study

A Personal Background

As an academic trained in urban, behavioural, and historical geography, it was never my original intention to become a "leisure researcher," either within the sub-discipline of recreation geography or more broadly within the interdisciplinary field of recreation and leisure studies. In fact my initial involvement occurred purely by chance: although hired to teach courses on environmental impacts and resource management, I was asked, immediately upon my arrival as a junior assistant professor in the Department of Geography at the University of Alberta in 1975, to supervise a graduate recreation-related thesis, which ultimately produced my first recreation research publication (Foster & Jackson, 1979). Then, I was asked to offer a graduate seminar course—"Geography of Outdoor Recreation"—previously taught by a senior colleague who had moved on to other interests.

As I am sure is the case for many colleagues in the field, my subsequent involvement in leisure studies, and the directions it has taken, have also occurred more by chance than by design. I became interested in outdoor recreation and environmental attitudes, for example, both as an extension of some work I was doing in the late 1970s and early 1980s on environmental attitudes and energy conservation, and as a result of casually browsing journals and discovering the few articles that had been published on outdoor recreation and environmental attitudes up to that time. My subsequent affiliation with research on constraints to leisure was due in no small part to the initial impetus provided by Mark Searle, who is now Vice Provost of Academic Affairs at Arizona State University West, but was at that time a manager at Alberta Recreation and Parks. He gave me a contract to analyze some leisure preferences survey data, and then some data on "barriers to participation"; this early work, together with the four papers that Searle and I co-authored in the mid-1980s, laid the foundation for research and writing that has held my attention for more than two decades. Similarly, I would likely not have produced two co-edited integrative books on leisure studies had it not been for the fortunate circumstance of having Tim Burton as a colleague at the University of Alberta (Jackson & Burton, 1989, 1999).

During the first decade and a half of my professorial career, I made it a practice to attend annual national and international conferences in my discipline of geography, and in particular sessions on recreation, tourism and, sport. Although some interesting substantive research was reported at these meetings, I was struck—and eventually turned away—by what I felt

were two serious limitations of "recreation geography," at least in terms of how it was represented at the conferences. First, the sub-discipline was much more a conceptual and theoretical follower than a leader within human geography. Second, it seemed to be almost hermetically sealed from what was being published—and what to me was much more intellectually challenging and exciting—in the leisure research journals.

It was during the early- to mid-1990s that I began my deeper involvement in leisure studies rather than geography, as I realized that the sources of ideas for what I was then considering, and a more appropriate audience for them, was to be found in the leisure research journals and conferences. While I continued to publish in the geography journals (on topics as diverse as West Edmonton Mall and the impacts on Icelandic population and rural settlement of the eruption of the volcano Laki in 1783), I found that my research was more and more being directed towards placement in leisure studies outlets. After my first paper in *JLR*, I then became a fairly frequent contributor to other leisure research journals, publishing two or more papers in each of *Leisure Sciences*, *Leisure Studies*, the *Journal of Park and Recreation Administration*, *Loisir et Société*, and the *Journal of Applied Recreation Research* (now *Leisure/Loisir*, the journal of the Canadian Association for Leisure Studies). I also attended or had a paper presented at each of the triennial Canadian Congresses on Leisure Research (CCLR) beginning with CCLR3 in 1981, and then, towards the end of the 1980s, I began to participate in the NRPA's Leisure Research Symposium on a frequent if not annual basis. The topics I wrote about were fairly divergent, but with some emphasis on constraints to leisure, outdoor recreation and environmental attitudes, conflict, and the results of a survey among leisure researchers about future directions in the field.

Observations About Leisure Studies

The reason I have told you all of this is that, while I felt that I had found my "intellectual home" in leisure studies, I also began to notice—and to become mildly worried about—some facets of the field of research and the community of scholars who have created it. Over the years of attending most of the CCLR and NRPA meetings in the 1990s, as well as reading the research journals and other literature, I made a number of casual observations. The first notable feature was that certain people's names appeared over and over again as authors or co-authors of conference papers and journal articles (and they were contributing to most of the major publications, not just one or two). They also tended to be frequently listed as members of journal editorial and conference review boards. These observations suggested two things: that a relatively small group of people remained active in leisure research over an extended period of time, and it was they who were creating a large proportion of the body of knowledge about leisure—or, at least, were responsible for a disproportionately large percentage of journal articles and con-

ference papers in the main leisure studies outlets; and that concentration of the journal article and conference paper review process was putting the field and the community of scholars in danger of "academic incest."

The second notable feature—and one that appeared to be the obverse of the first—was that there seemed to be an inordinately large number of people whose publication in the field or presentation at conferences was one-off and never repeated. There was also a smaller but nonetheless significant group who had conducted research and published or presented two or three papers but then apparently disappeared from the journals and conferences. I began to wonder about attrition and the reasons for it, and about ways in which the retention of scholars might be enhanced.

Third, a cursory comparison of journals and conference proceedings showed that some people's names were listed in almost every major leisure conference program but that their work rarely if ever appeared in the journals. Conversely, I saw that there was a group of quite prolific authors of journal articles who rarely if ever participated in conferences. I was curious about why people choose different outlets for their research, and in particular why research that had apparently been executed well and was well-presented at conferences never seemed to "make it" into the peer-reviewed journals.

Lastly, and again without empirical verification, I noticed that there were Canadian scholars who rarely if ever published or presented their work outside Canada, while there was a comparable group of Americans whose research dissemination outlets were confined to those in the United States. This led me to speculating about how well integrated—or not—the leisure research communities of the two countries are.

Development of the Study

Putting all of these observations and concerns together, I began to think towards the end of the 1990s about ways in which one could, using empirical data, "stand back" sufficiently far from the leisure studies community to investigate the size and characteristics of sub-groups within that community, their relative contributions to knowledge about leisure, other aspects of their involvement, and the awards and recognition they have received. The best way to measure this, it seemed to me, was to go to the journals and conference abstracts, and list, first on an edition-by-edition basis and then at successively higher levels of generality by source and timing, the names of each author and the number of publications on which their names appeared. There was a precedent for this approach in an article published some years earlier by Henderson, Sessoms, Chen, and Hsiao (1993). As far as the analytical strategy was concerned, a comment by Kaplan (1992) that there were identifiable "constituencies" within the leisure studies community, coupled with some success I had previously had in applying cluster analysis to leisure constraints data (Jackson, 1993), prompted me to assume that internally con-

sistent sub-groups of researchers defined on the basis of their journal publication and conference presentation activity were there to be found in the data, and that cluster analysis would be sufficient to tease out these patterns.

In the early stages of the study I was particularly influenced by a paper published by Samdahl and Kelly (1999) who, using journal article citation data, raised a number of concerns about the apparent *intellectual* and *disciplinary* isolation of leisure studies in North America. Soon after Samdahl and Kelly's paper appeared, Valentine, Allison, and Schneider (1999) published a similar article in which they expressed their concern about the lack of an international orientation in leisure research and what they saw as an unhealthy degree of ethnocentrism in the field in North America. This suggested that, in addition to the intellectual isolation identified by Samdahl and Kelly, North American leisure studies might also characterized by *geographic* isolation, perhaps thus accounting for quite different approaches to leisure research between—broadly speaking—North America and the UK/Western Europe (Coalter, 1999; Samdahl, 1999).

The specific article that is the focus of the discussion here came about as a result of Walker's (2000) commentary on the article by Valentine *et al.*, in which he took the authors to task for treating Canadians and Americans as a single entity and not recognizing the distinct contributions of Canadian leisure scholars. This criticism led me to thinking, as I said before, about how similarities and differences between Canadians and Americans might be reflected in the data I had already collected, and hence the current article that we are discussing in these pages.

Specific Points Raised by Shaw, Samdahl, Dawson, and Witt

I want to return to the above observations later, as well as draw upon my academic experience "outside of" leisure studies. However, before doing this, and with the preceding background in mind, I should now like to turn to some of the specific points raised by the four commentators on my article. In keeping with what I said at the outset, regarding the "posture" I have adopted in this "reply," I will focus more on elaborating on ideas that I see as *arising out of* the commentaries (even if these were only implicit in what the commentators said) than on responding to specific (and arguable) details.

Shaw

I tend to agree with Shaw's comment that "the importance of Jackson's research lies less in the data and analysis that he has produced, and more in the implications his findings have for our field." Indeed, this was exactly what I had in mind when I wrote that my study should be seen as a starting point for discussion as opposed to the last word. She correctly points out that the "One community or two solitudes" article was limited in its objec-

tives and that, because of the nature of the data on which the paper was based, three additional questions, which she characterizes as being "below the tip of the iceberg," remain hidden.

These three questions are "the isolation of North American research from research in other parts of the world, intellectual or theoretical isolation, and isolation from other disciplines." Shaw argues that, if my data do indeed reflect some sense of separation and solitude between Canadian and American scholars, then this separation is probably much greater between North America and the rest of the world. Others have addressed this question from both sides of the Atlantic, and it is worrisome, not because the research questions about leisure are similar in different parts of the world, but precisely *because* they *are* quite different, being rooted in sharply diverging intellectual traditions. Thus, isolation at whatever scale means that we are missing important opportunities to cross-fertilize at a philosophical and theoretical level. In some respects, what Shaw's comments boil down to are a concise re-statement of the theme of disciplinary and intellectual isolation addressed by Samdahl and Kelly (1999), and of geographical isolation addressed by Valentine *et al.* (1999), but I think what she has added is to imply that the three facets of isolation are intimately intertwined.

Shaw also raises the important question of the time crunch faced by academics, one result being that many people have difficulty keeping up with reading even in their own sub-fields, let alone more broadly in leisure studies and the social sciences in general. This, she rightly points out, is closely associated with the pressure to publish, and in turn is connected with the reward system of the contemporary university. I think that there is no question that competing responsibilities and escalating expectations, coupled with limited time and energy, represent an increasing difficulty for academics in North America, and probably elsewhere too. It no doubt contributes to the fact that apparently only a relatively small number of people in the community appear to have the drive and determination to produce a sustained and high level of research output, as well as to the isolation that is apparent in my data and which has been identified by Shaw and others.

Samdahl

Although she takes me to task for aspects of the ways in which I analyzed, presented, and interpreted my data, and suggests that she could refute any *one* of my findings, Samdahl does concede that my data and analysis produced a consistent, general picture of leisure research. Indeed, what struck me as one of the most remarkable features of the data was how such recognizable and consistent patterns emerged, despite the data being based on decisions about research, publications, and presentations by more than sixteen hundred people about thousands of publications during a ten-year period. It is on the interpretation of and explanation for the results that Samdahl bases her criticisms—although I should make it clear that I do tend to agree with most of the alternative ideas she proposes.

Like Shaw, Samdahl points to some of the social-institutional structural factors that might help to explain the patterns I observed. She rightly points out that these kinds of factors are difficult to capture in the "agency-based" approach that I used, i.e., individual researchers were the units of analysis and the patterns were detected by aggregating from these. I agree that these broader, contextual factors warrant further investigation, and make no apology for not addressing them. I think it is far more appropriate for the other commentators and I to do so here, in the context of an open debate that is less subject to the strictures of evidence and interpretation, rather than in the original article, where I would have risked the accusation of "going too far beyond the data" in drawing conclusions. I do agree that these issues are important, because they may limit academic freedom in profound ways, not so much by controlling *what* we do in our research, but *how* we do it, and *why*. For instance, pandering to the bean counters on tenure and promotion committees by padding the *vita* with MPUs (minimum publishable units, in which research projects are sliced as thin as shaved ham) is rewarded more highly than creating and integrating knowledge, or even, as Shaw points out, being able to take the time to read and reflect. I would not disagree that the patterns I detected in my data are indirectly affected by such contextual issues and that they deserve attention in the future.

Samdahl makes two other points that stimulated some further reflection on my part, in particular about next steps in the project. First, she notes that the limitation of quantitative data is that, unlike in a qualitative study, "With interviews you would be able to return to the participants using probes and member-checks to see if your understandings were correct; with statistics you are on your own to impute meaning onto numbers." This is precisely what I am planning to do in one of the next stages in my research project, and I am currently experimenting with a research survey that would examine some of the questions that Samdahl raises.

Samdahl's second point has to do with her contention that the 1990s was an unusual decade for leisure studies. Leaving aside the validity of this statement (it could be argued that *every* decade is unique!), there is no doubt that the data, the patterns that emerged, and to some extent my interpretation of them, were influenced by the time parameters of the study—1990 to 1999. In this context I plan to follow up the present study in two main ways: the qualitative survey mentioned above, as well as an extension of the data-based study to the end of 2003 (an addition of four years of journal articles and conference presentations). The latter would allow me to replicate and extend the original study and assess if and how much the patterns have changed over time.

Much more interesting to me, though, are other questions to which both the qualitative and quantitative extensions of the study would permit answers, such as changes in the personal composition and intellectual orientation of the community. A temporal breakdown of the existing data for the 1990s (which I have not published), coupled with a cursory examination of researchers listed on the contents pages of recent journals and conference

abstracts, prompts the conclusion that the field is continually being invigorated by young scholars with new and exciting perspectives and research agendas—which is most encouraging to those of us who are much closer to the end of our formal academic careers. As I casually survey these changes, the following kinds of questions spring to mind: Have the young scholars who began to enter the field during and after the mid-1990s fulfilled their promise? Who are the emerging leaders, and in which directions is their research going, with respect to substance, theory, and methodology? How will they and their fresh ideas alter the shape of leisure studies in the future? Are there, on the other hand, some newer scholars who have not lived up to their promise? Why not? Which of the more senior scholars continue to be leaders? How do they make their research and publication decisions and how will those decisions in turn affect the field?

Dawson

Dawson's commentary is particularly interesting because, going deeper than the other three, Dawson uses a theoretical framework to establish propositions in relation to "distinct historical traditions of research, different bodies of knowledge, and ... sets of solutions to problems of leisure in ... respective societies." These propositions can then be used to judge the extent to which apparently separate scholarly communities—such as I have argued is the case with leisure studies in Canada and the United States—are indeed distinct. Although I suspect he is too kind to say so directly, my interpretation of Dawson's commentary is that, using these criteria, my article falls short. There may be two reasons for this. First, the data I used were not measures of the sorts of criteria that would be needed to answer Dawson's questions. Second, the differences I identified between the two geographically-defined communities could alternatively be interpreted as shades of difference that are superimposed over a more stable set of similarities—a point which, it seems to me, is implicit in Shaw's and Samdahl's commentaries as well.

As far as the first reason is concerned, there would be more weight to the criticism if I had intended to assess the "topics, methodology, theory, practice and ethics" of leisure research, as Dawson puts it. However, I explicitly excluded these issues from my study when defining its delimitations at the beginning of the article. Thus, my article is being criticized because I did not meet objectives that it was never my intention to address in the first place. On the other hand—and leaving self-defense aside—I believe that the framework offered would be a productive one to use if one were to investigate the much more profound differences in leisure studies between North America and the rest of the world, in particular the UK and Europe, as noted in her commentary by Shaw, and as discussed elsewhere by, among others, Coalter (1999) and Samdahl (1999).

As far as the second point arising from Dawson's commentary is concerned, the decision to emphasize differences over similarities always involves gains and losses, and is shaped to some extent both by academic training

and by personal inclination (I have heard an anthropology colleague distinguish between people who are "lumpers" and "splitters"). In the present case I chose to "split" by emphasizing differences between Americans and Canadians, not because I felt that the differences were ultimately more important than the similarities, but rather because this was a "device" to "cut into" the data and ultimately to draw attention to what I felt were some troubling aspects of North American leisure research and the community of scholars that has produced it. In any event, I do believe that Dawson and I are of one mind in thinking that the problems of isolation between and among scholarly communities are worthy of discussion and that we need to find ways to break down the barriers.

Witt

Among his many points, Witt makes three that particularly caught my attention: where to publish; the changing nature of the journals; and the size of the leisure research community. As far as the first of these issues is concerned, Witt is correct when he suggests that my choice of the *Journal of Leisure Research* as the preferred outlet for my article was deliberate: I did indeed want to reach the widest possible audience of leisure scholars, who I believed should be aware of, concerned about, and eager to discuss the findings summarized in my article. But this choice was also symptomatic of a broader "Catch 22" situation that we frequently find ourselves in when choosing where to submit an article which could be published equally profitably in either a disciplinary journal (such as, say, *The Canadian Geographer*) or a non-disciplinary, "field-of-interest" journal (such as the *Journal of Leisure Research*).

This problem is exemplified by a paper I published some years ago (Jackson, 1994). As part of the work I was then doing with respect to analyzing survey data from the province of Alberta about constraints to leisure, I discovered that there were two "geographical" aspects in the data: first, there were the "geographical constraints" themselves, exemplified by distance, relative location, accessibility, and transportation problems; second, I found that there were spatial variations across the province in the "non-geographical" constraints items, such as time- and cost-related barriers. I then had a choice. Did I wish to draw leisure, and the constraints on it, to the attention of my disciplinary colleagues in geography, and therefore choose a disciplinary journal, because recreation and leisure have received relatively little coverage in the geography literature in North America? Or did I want to bring the geographical aspects of constraints to the attention of "constraints scholars" in the leisure studies field, and therefore choose a leisure research journal? There were obvious advantages and disadvantages of either choice: if I published in a geographical journal, then the article's impact on encouraging leisure scholars to think about constraints from a geographical perspective would be diminished. If I published in a leisure journal, then I would both be failing to publish within my own parent dis-

cipline (which can be important at annual review time), and failing to "raise the consciousness" of my fellow geographers about leisure research. As the citation denotes, I did indeed make the former choice, but I have often wondered since then whether it was the correct one.

Witt's second point is that the location of publication will become less important as more journals go on line. There can be no question that electronic journals, as well as Internet-based access to print journals, will make a much larger body of knowledge instantly available (although one wonders how this might relate to the time-crunch already faced by academics that Shaw laments). This prompts the thought that research is needed about the ways in which computer- and Internet-based technology will influence how we conduct, report, and discuss our research. Perhaps, in the longer term, it will profoundly change our sense of affiliation with or alienation from the scholarly community. I would not be surprised to find that, if a replication and update of my study were to be conducted a decade or two from now, the patterns I identified will have changed remarkably, and I suspect that we have been on the verge of those changes for about the last five years.

Finally, Witt cogently asks a question about what percentage of the community (probably a small one by his guess) is responsible for publishing 50% or 75% of the articles. The answer, which I plan to publish in a companion paper to the one being discussed here, is that the proportion is a very small one indeed. For example, in a cluster analysis of the journal article and conference paper data, the 207 members of the four most productive clusters combined accounted for only 12.7% of the people included in the data base but were responsible for fully 51.8% of journal article and conference paper authorships in the 1990s. In contrast, the 1138 members of the largest cluster accounted for 69.8% of the data base but only 28.4% of journal article and conference paper authorships. Based on these numbers and other detail to be included in the forthcoming paper, the inescapable conclusion is that North American leisure research is very highly concentrated, at both the individual researcher and academic institution levels, and I would argue very strongly that the intellectual, disciplinary, and geographical isolation of leisure studies as assessed by Samdahl and Kelly (1999) and Valentine *et al.* (1999) is inextricably linked with individual and institutional concentration.

The "Big Picture"

Although each of the four commentators chose her or his own approach to my "One community, two solitudes" article, it is interesting to note that several similar ideas and concerns were expressed, albeit in different ways and with different emphases. At a very general level, it seems to me that they all relate to the sorts of socio-institutional factors that were explicitly addressed by Shaw and Samdahl, that were approached theoretically by Dawson, and that are consistent with Witt's thoughts about the influence of journal readership and prestige. Factors of this kind not only influence immediate publication decisions but more deeply the nature of the conduct

of inquiry in leisure studies—or any other field of interest or academic discipline. If the commentators are correct when they raise these "structural" and contextual issues, then a socio-cultural interpretation of my findings would indeed enhance (*not* replace) our understanding of the patterns I discerned from an agency-based approach. It is also troublesome, however, because it means that taking steps to reduce the intellectual, disciplinary, and geographical isolation of leisure studies in North America, and its individual and institutional concentration is not going to be an easy task.

In any event, I think that what we are all talking about is the need, as I suggested earlier, to "stand back" from time to time and reflect on what is going on in leisure studies, and what factors (agency-based and structural) account for the patterns. Identifying the patterns themselves and then trying to account for them at either (both?) the individual and contextual levels involves discerning what I call the "Big Picture." I have always been a "big picture" person: thus, even though I have been happy to spend a large portion of my academic career attempting to understand the minutiae of detailed quantitative data on topics such constraints to leisure, and outdoor recreation and environmental attitudes, I have also always thought it necessary to try and develop a sense of context, an overview or overall framework of how things fit together—which is fundamentally what I have been trying to do in this project, albeit in only one of several possible ways.

I also try to emphasize the "big picture" in my discipline-based course on human impacts on the environment. One of the points I make in this course is the desirability of forging linkages across disciplines, not only within the natural sciences but also between and among the natural sciences, the social sciences, the humanities, education, engineering, and so on. There is an enormous diversity across the earth, atmospheric, and biological sciences, and partly because of this there is a great danger of further fragmentation of knowledge in these areas. This has been particularly the case in the last five to ten years, during which science and scientific knowledge have advanced incredibly, but during which, also, researchers have becoming increasingly more specialized and consequently less able to communicate with others. In my environmental impacts course I refer to this as "Digging Disciplines Deeper," and I illustrate the point using a cartoon drawn by my son (Figure 1), which shows the production of more and more in-depth, specialized knowledge with very little connection to anything else, and the difficulty, the deeper we get, of peering across boundaries between these disciplinary "holes." I readily concede to have been one of the "hole-diggers" illustrated on the left and center of the cartoon for a good proportion of my academic career, having published, for example, about two dozen articles on constraints to leisure. However, I have also tried, from time to time, to "look over the top of the hole," as does the third figure on the right of the cartoon. The project we are discussing here is part of that effort.

Before closing, I think it is worth noting that the trend of over-specialization in the "environmental sciences" seems to be being countered by two competing developments. I believe that those of us in leisure studies

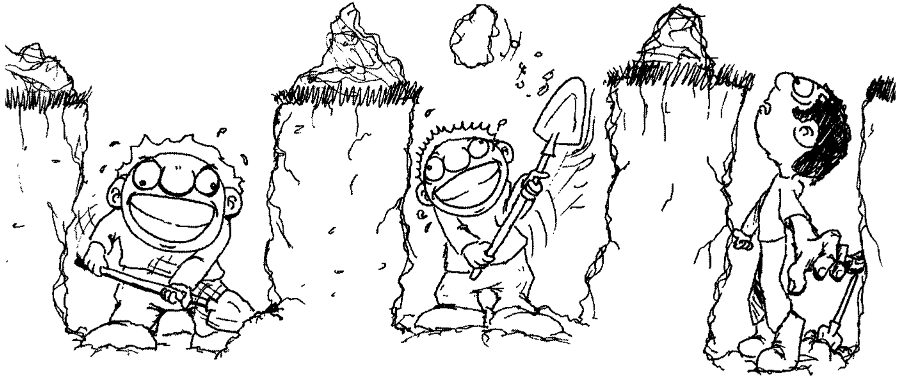

Figure 1. Digging disciplines deeper*

can learn from the environmental sciences example, and thus help to avoid some of the problems identified in my article and discussed by the four commentators. First, there seems to be a growing effort among many researchers to work with colleagues in other disciplines (and in parallel for traditional disciplinary departments to hire people with inter-disciplinary expertise; my own department, for example, which was formed a decade ago as the merger of traditional departments of geology and geography, is now attempting to hire a geomicrobiologist). The second counter-trend is that many physical-natural scientists (as well as some social scientists working on similar topics) have found (by chance or design; I am not sure which) a common focus for their research: global climatic change and its causes and consequences. This issue is simultaneously of enormous practical and policy importance and serves as a way of integrating research and encouraging scholars of diverging academic interests to communicate and work together.

Perhaps those of us in the leisure studies community would do well to emulate the two key aspects of this exemplar: to encourage, as Witt urges in his commentary, collaboration with others in cognate disciplines; and to find some sort of rubric/theme that appeals to the public and to policy-makers while at the same time integrating much of the currently fairly isolated subareas in the research that we collectively carry out. At one time I thought that the "benefits approach to leisure" (Driver & Bruns, 1999) might serve this purpose, but this does not seem to have happened. Perhaps something more general, such as "wellness" or "quality of life" would do the job.

Conclusions

In conclusion, I should like to return to a point I made earlier in this "reply," as well as in the article we have been discussing, namely that my

*Drawing by Patrick Jackson.

analysis and interpretation were not intended as the last word on the subject, but rather as a starting point for discussion. The four commentators have entered into this discussion in a very public way, and I hope that, taken together, these six pieces will provide a platform for further debate within our community. I thank my colleagues, not only for their thoughtful remarks, which they have obviously prepared with integrity, enthusiasm, and concern for the well-being of the leisure research community, but also for their indirect help in helping me to sharpen my own ideas and clarify some of the directions in which I want to go next. We all know that doing research more often raises new questions than answers the ones we started with (or the ones we *thought* we started with!). Often, however, it takes an outside stimulus to help clarify what these new questions might be, and I therefore thank Susan Shaw, Diane Samdahl, Don Dawson, and Peter Witt for helping to point out some directions in which to go.

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