

# The Messy Process of Research: Dilemmas, Process, and Critique

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## **Abstract**

*The unabashedly messy aspects of the research process are often hidden from published view, and are therefore not available to encourage and instruct. The authors tell specific stories about “messy” research, arranged around:*

- *evolving research questions,*
- *methodology or methods surprises,*
- *problematic answers, and*
- *publication dilemmas.*

*These stories are from work amongst diverse categories and frames of inquiry, and are meant to encourage fellow students to persist and problem-solve mid-process. We hope that having real researchers tell their own stories of mucking around the methodologies and ethics of inquiry can encourage others to engage and persist, and can be the beginning of a more process-oriented and perhaps more nuanced conversation that opens active discussion and scrutiny amongst critical friends.*

## **Résumé**

*Tous les moyens sont bons pour que les aspects compliqués du processus de recherche soient habituellement masqués de la version publiée et ils ne peuvent donc pas servir à stimuler ou à former. Les auteurs racontent des histoires particulières au sujet de recherches compliquées qu'on adapte :*

- *au développement de questions de recherche*
- *aux surprises rencontrées en méthodologie ou dans la méthode*
- *aux réponses problématiques*
- *aux dilemmes de publication*

*Ces histoires proviennent de travaux de catégories et de cadres de recherche divers et sont sensées encourager des confrères à persévérer et à résoudre des problèmes pendant la recherche. Nous espérons, qu'en ayant de vrais chercheurs qui racontent leurs propres histoires de décanation afin de rendre correctes les méthodologies et l'éthique de recherche, que cela en encouragera d'autres à s'engager et à persister et peut être initiera un processus plus méthodique et possiblement un dialogue mieux nuancé qui permettra une discussion active et une recherche minutieuse avec des collègues critiques.*

*Keywords:* environmental education; evolving research questions; methodology surprises; research problems; publication dilemmas

## Introduction

The error spectrum in publishable research, ranging from slight “undetectable” flaw to insurmountable foible, is known to exist, but is seldom acknowledged in print unless such an admission is followed by the victorious tale of how the blemish was erased, overcome, or adeptly sidestepped on the way to a research publication. This is not a new tradition; Figure 1 contains a 50-year-old tongue-in-cheek research glossary (from metallurgical research) of such sidestepping prose.

Sidestepping Research Prose	
Common words and phrases	Their real meanings
It has long been known that...	<i>I haven't bothered to look up the original reference.</i>
...of great theoretical and practical importance	<i>...interesting to me</i>
While it has not been possible to provide definite answers to these questions...	<i>The experiments didn't work out, but I figured I could at least get a publication out of it.</i>
Three of the samples were chosen for detailed study...	<i>The results on the others didn't make sense and were ignored.</i>
Typical results are shown...	<i>The best results are shown.</i>
Presumably at longer times...	<i>I didn't take time to find out.</i>
These results will be reported at a later date...	<i>I might possibly get around to this sometime.</i>
It is suggested that... It is believed that... It may be that...	<i>I think...</i>
It is generally believed that...	<i>A couple of other guys think so, too.</i>
It might be argued that...	<i>I have such a good answer to this objection that I shall now raise it.</i>
It is clear that much additional work will be required before a complete understanding...	<i>I don't understand it...</i>
It is to be hoped that this work will stimulate further work in the field...	<i>This paper isn't very good, but neither are any of the others in this miserable subject.</i>

Figure 1. Sidestepping research prose (Graham, 1957).

We believe that too little attention is given to acknowledging and (especially) disseminating the “stubbed toes” that happen on the research path. In this article, we seek not to sidestep but rather to highlight, and perhaps even celebrate,

the unabashedly messy business that is quality research. To do so, we tell stories from our own experiences that illuminate our belief that research happens not at a failure/success binary/dichotomy, but rather along a path where many points can't (and shouldn't) be described as failure or success.

The variety of experience level among authors, from highly published tenured professor to unpublished graduate student, is meant to show that this is true for researchers at every experience level, and that a key is to engage and persist. Writing often tidies up the loose and ugly ends of research, and yet the process of dealing with those parts is often one of the most productive for a project. Our hope is that stories of our flaws and foibles will generate thought and provoke discussion. We wish for others to be heartened, and to see their own difficulties as both more useful to themselves, and perhaps instructive to be shared with others.

The impetus for this paper was provided by a session held at the Third Annual Research Symposium in October 2006, prior to the annual conference of the North American Association for Environmental Education in St. Paul, Minnesota. The session, entitled "What Happens When Research Goes Bad?" featured a panel composed of Justin Dillon, Paul Hart, Joe Heimlich, and Michael Brody, and was moderated by Charlotte Clark.

As we did at that conference session, we will parse our stories into four themes:

- evolving research questions,
- methodology or methods surprises,
- problematic answers, and
- publication dilemmas.

### Evolving Research Questions

In qualitative work, we expect our research questions to evolve as we work and to be preconceived as emergent, rather than preordained. However, the back story of that evolution in research work is seldom articulated, at least in print (however, see Lather & Smithies, 1997; Russell, 2003). We provide two such back stories here. In the first, the question evolved through external influence through a journal review process; in the second, the question evolved through internal inquiry.

#### *Welcoming External Influence on Research Questions (Michael and Justin)*

*Michael:* This tale emerged from a dynamic editorial process resulting in a career-defining article that led to further development of learning theory related to informal settings, and challenged me to grow professionally by leading me to greater insight and understanding that I otherwise would have overlooked (Brody, Tomkiewicz, & Graves, 2002). The text is presented as a

reflective dialogue on the editorial process and subsequent evolving world-view, research questions, research concept, and published paper.

The research contract asked the authors to ascertain if a new Midway Geyser Basin Visitor Guide would have any effect on visitor outcomes; that is, would casual visitors learn anything as a result of using the brochure at the site. I went into this research project with an existing understanding of learning derived particularly from my previous work on misconceptions about the environment, and influenced primarily by Ausubel and Novak (Ausubel, Novak, & Hanesian, 1978). This led me to ask what people already know about Midway Geyser Basin and the associated life forms. I asked what science content visitors brought to the event, how that content might change, and what views and science misconceptions people might have about

- the National Park Service,
- geothermal features at the park, and
- associated microorganisms (especially biopiracy of microorganisms for the biomedical industry (Brody et al., 2002)).

*Justin:* Michael's first draft was sent to two reviewers of the *International Journal of Science Education* that Bill Scott (co-editor) and I had asked to help with the special edition. The first review, undertaken by a leading researcher in science learning in informal contexts and completed in September 2000, concluded by stating that the paper:

raises interesting questions, but needs to be more firmly grounded in relevant non-school based research and recent research on alternative conceptions. It also needs a more thorough defense of the research design and an introduction that sufficiently supports the study conducted.

The second review, undertaken by a leading science education researcher, noted that the literature was somewhat out-of-date and suggested other studies should be used:

Specifically, there are now strongly recognized alternative theories about learners' conceptions. These alternative theories are grounded in research in cognitive science and learners' epistemologies. The more contemporary research (e.g., Strike & Posner, 1992; Pintrich, Marx, & Boyle; diSessa, Minstrell) is showing that the conditions of the learning environment and the motivation for learning are as important as the initial conceptual schemes held by the students. Simply stated, the conceptual ecology is much more complex than originally proposed by Novak, Ausubel, Champagne, Klopfer, Osborne, etc. Some of the new research maintains that the initial conceptual schemes are not important at all. Here I am referring to the idea of "knowledge-in-pieces" put forth in a theoretical framework by Andrea diSessa and in a practical framework by Jim Minstrell. This research has shown that individuals' conceptual frameworks change with regard to the conditions of the task. This perspective challenges some of the conclusions drawn

in the present study. For example, why would we expect a senior citizen with no formal education in geology or evolutionary theory to have a knowledge base about an emerging scientific theory about the origins of life on Earth? This expectation on the part of the author(s) is a serious shortcoming of the present paper because it is a shortcoming of the theories about alternative conceptions and conceptual change teaching.

Both reviewers also noted that there were some issues concerning the sample size and the claims the authors were making about the generalizability of their findings. So, Michael and colleagues were asked to make a major revision to the paper. Although the review process might be seen as a gate-keeping exercise by editors and/or reviewers, editors do (or should) expect authors to defend their submissions if they think the reviewers have missed the point or are mistaken. So, Bill and I waited for Michael's response.

*Michael:* The comments by the reviewers were the most insightful and telling of any that I had received in my entire career, but they were also the most difficult to accept and, in the end, accommodate. To say that the authors went back to the "drawing board" is an understatement.

I read new literature, attended conference presentations by people cited as important to consider, and constructed concept maps to integrate the new ideas into a more robust conceptual framework. These efforts led to a reformulation of the basic research question, moving from the traditional outcomes-based approach to the more complex task of understanding the experiences of visitors to Midway Geyser Basin. The questions evolved in this way:

- *Old:* What are park visitors' understandings about geothermal features and associated microorganisms?
- *New:* What are the experiences of visitors to Midway Geyser Basin?

As my attention increasingly focused on the experience of visitors, the new theories and principles (both of concept and methodology) the editors introduced to me began to make more and more sense. Figure 2 depicts my evolution by showing components of the old (standard text) and the new (italicized text). For example, in the conceptual framework, I had to integrate disparate ideas from various domains into a comprehensive view of learning in this situation—not an easy puzzle to solve!

From the authors' point of view, the reconceptualization of the underlying philosophy, theory, principles, and concepts was the hardest part of the rewriting. Once this had been clarified, it was clear that the data could be reinterpreted in more meaningful ways. This led to different knowledge and value claims, which are depicted in the boxes on the right in Figure 2. Fortunately, there were some aspects of the research and final paper that did not change, such as research events, actual data, and transformative emergent themes. A conservative estimate of the amount of change that took place in this paper would be about 75%. Thankfully, the editors allowed approximately six months for the authors to resubmit the paper.

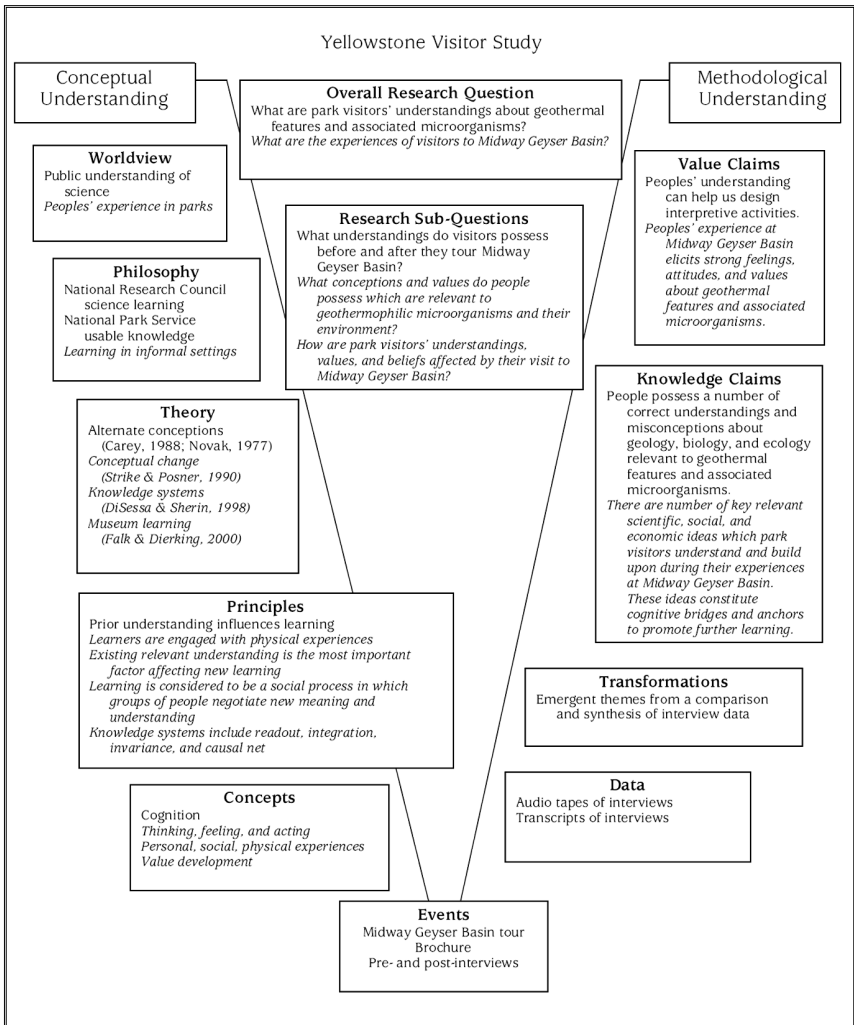


Figure 2. Yellowstone visitor study (Brody et al., 2002). Plain text indicates original ideas and framework of study; italicized text indicates new understandings.

*Justin:* The final paper, published in 2002, is far stronger than the version that was originally submitted. From an editor's point of view, Michael and colleagues were exemplary in the manner in which they responded to the reviewers' comments.

*Michael:* The process of producing the final Yellowstone paper will remain a watershed event in my career. On one hand, it was always humbling to be led down a new trail by expert guides—that is, a trail where a part of your brain

scolds you, saying “you should have traveled this before.” On the other hand, this article is about quieting that scolding voice, and recognizing how empowering it is to know that you can trust your colleagues to give you insights that, with patience and hard work, can result in a career-defining publication.

### *Welcoming Internal Influence on Research Questions (Paul)*

Good research takes time to engage both theory and methodology intensively. Although many university programs work against this longer-term study through fee structures and scholarship award limitations, thoughtful inquiry is not so much a matter of learning the theory as learning to conceive oneself in terms of theory. In this case, a doctoral student’s interest shifted over a period of about three years, resulting in new research questions, a significantly different dissertation product, and deep changes to the student’s own personal belief system (Barrett, 2005).

Her original interest arose directly from her experience as a secondary teacher who coordinated and taught alternative, semester-long outdoor/environmental education programs. Early in her doctoral program she expressed a narrative interest in understanding what sustains teachers who work somewhat outside the regular high school program. She was curious about what motivates teachers (much like her) and where the energy, persistence, and patience had originated. Her original research questions, as she presents them, are as follows:

- What does it mean to be a teacher of an intensive interdisciplinary outdoor/environmental education program?
- Why do teachers of integrated programs choose to teach the way they do?
- How did they get there?

By mid-course, she began to engage intensively in both poststructural theory and fieldwork. The combination of conversations with one teacher in particular, together with deeper thoughts about how teachers’ identities and sense of agency are produced, how teachings are discursively framed, and how life histories are culturally produced, led to a significant shift in research questions. Her new questions were positioned as a critical examination of how environmental education that espouses change can be understood and enacted within the conservative tendencies of the educational system. Thus, stories of experience are not simple tellings, and her evolved questions ask:

- How do discourses of teaching, learning, and “nature” work to constrain and enable environmental educators?
- How have teachers’ identities been produced by discourse?
- How can life history research within a feminist poststructural framework be useful in addressing these two questions?

Beyond these evolving questions, she has also constructed a critically reflective re-engagement with her work as questions themselves:

- What assumptions are embedded in the research questions (e.g., about self, agency, knowledge)?
- What kind of answers will the questions produce?

These last two questions, together with St. Pierre's (St. Pierre, 2000; St. Pierre & Pillow, 2000) admonition that poststructuralism needs to turn back on itself to investigate its own sets of assumptions, opened the study to another layer of examination. The study (which is represented in hypertext at [www.porosity.ca](http://www.porosity.ca)) has now been extended to investigate ways in which research itself, through its often unacknowledged anthropocentric framing, might be limiting (successful) enactment of outdoor/environmental education.

### Methodology or Methods Surprises

Optimally, published works should discuss the methodology, methods, and instruments used, but this does not always happen; methodologies may be the most oft-confused and neglected component (Dillon & Wals, 2006). Published works whose aim is to discuss the perils and pitfalls of these choices are also rare, but not completely absent (Raven, 2006). This section provides three stories of surprises discovered in the midst of research involving methodologies and methods. One describes the unfortunate discovery that a "well-tested" instrument was useless, a second tells of the researcher's realization that the chosen method did not fit the question after all, and a third admits to a study where the frame of the study changed so often after data collection had begun that every type of external validity error was violated.

#### *The Useless "Well-Tested" Instrument (Joe)*

We are always seeking ways to ensure that our studies are well-grounded and rigorous. One long-practiced approach is to find an instrument measuring a component of what you hope to be studying, and to use it. Of course, there are the obvious challenges of ensuring that what is being measured is actually the domain of what you want to know, but often there are more hidden challenges as well.

For example, a graduate student was planning to use an instrument as part of a larger study. This instrument was comprised of several scales, had been used in at least 12 studies, and had good reliability measures in each of the studies (ranging from around a 0.69 to a 0.82). However, as we began the necessary work to translate the instrument, we found that several items could be considered positive or negative, other items were neutral-



ly stated, and some were clearly directionally framed. What a shock that this instrument had been replicated and used in other studies, but had construction errors! The reliability measures were strong because there was a clear inter-item relationship, but the validity of the questioning structure would be questionable, as would any analysis on the findings.

Although this original instrument was flawed for our use, sometimes going back to the original instrument is far more valuable than creating a new one or even using a more contemporary adaptation. For example, we've found far stronger reliability and distributions in populations using some instruments developed by psychologists measuring a particular construct, as compared to environmental education instruments measuring the same construct. As a second example, I have found some basic state and trait measures that are better than those that have been adapted specifically for environmental education and science.

In sum, caution is urged both when modifying existing instruments, where errors can be amplified through repetition without critical reflection, and in using existing ones without change, where application to a new field must be carefully considered.

### *Evolving Epistemology, Methodology, and Methods (Paul)*

In this case, a sticking point in data analysis led a veteran teacher to not only a change in methods or even methodology, but in fact an epistemological shift in what counted as legitimate data (not necessarily in that order). The setting for this story is the secondary school physics classroom (not the environmental education classroom), but the story and its outcome are well-applied to the environmental education field.

The teacher in this case developed his doctoral work out of a persistent conundrum in his work with students on vector mathematics (Wessel, 1998). The fact that many high school students experience difficulty in the transition from concrete scenario (e.g., two soccer players each kick a soccer ball simultaneously from different angles) to abstract math symbols and graphical representations, led him to question how adolescents "learn" to conceptualize complex concepts. He came to investigate students' lived experiences in physics problem-solving by creating classroom conditions where students could verbalize their learning experiences interactively with peers and the researcher-as-teacher in an attempt to make their reasoning processes more explicit. Although difficult, he was able to collect rich narrative text through many days of problem-solving.

So while context question and research process were clear enough, the sticking point in this study concerned the method of analysis. What to do with mounds of narrative text is a relatively common dilemma in many emergent interpretive research studies. In this case, given a background in scientific research, the student's first instinct was to organize the data by dwelling inside

the text and looking for regularities as patterns or themes. He elected to code the transcripts of his data using an elaborate colour-coding scheme—attempting to make sense of the interactive (reasoning) talk. In my discussions with him during this process, questions arose about what counts as legitimate data and data analysis in various forms of inquiry (quantitative and qualitative). Despite rhetoric to the contrary, it was difficult for this teacher/researcher of science to make the interpretive turn—from viewing data in terms of categories to viewing it differently, perhaps holistically, so that the actual content of the conversations could be brought into sharper relief.

After about three months of coding, he abandoned that analytic process for a form of conversation analysis within a more interpretive frame. I believe the shift was analogous to his physics students' insight into the transition from concrete to abstract thought in solution of their vector problems. A highly independent person with a mind of his own, who would not be convinced of method except through his own experience, produced a thesis rich in conversation analysis. This case, for me, represents an example of distinctive shifts in methodological thinking brought on by a sticking point in analysis that had ramifications far beyond method—in fact, in order for a shift in method of analysis to occur, an epistemological shift in what counted as legitimate data was prerequisite. Such shifts in thinking and data analysis methods are important to consider for questions that are at the concrete/abstract border, many of which exist in environmental education, such as the study of instruction on global warming.

### *The Case of the Disappearing Frame (Joe)*

In a national study, several organizations provided names from which a random sample would be generated. Each group was part of a blocked sample of these organizations and was selected to represent that type of organization based on size of members on their lists. Initially, each group was asked to provide the number of names on their list. The total number was determined and a proportional sample number was assigned to each group. The result was an initial frame, or list of accessible respondents, of around 900 names.

Because the study was under a strict timeline and the volunteer-led groups did not respond in a timely manner, the study began as soon as the first frame was received. This first list, when received, included duplicate names and multiple ways of addressing the same person for some businesses, organizations, and individuals. The total  $N$  for the study was adjusted, the proportions rerun, the sample drawn, and surveys sent. The second frame was larger than the first, but also had many frame errors. And so it went through all 10 frames. Each time a list arrived it was edited, corrected, and the numbers recalculated. One list of 600 names was actually under 300 discrete names, and another list had over 60 names that were corporate sponsors rather than members.

Clearly, we had a problem. We (the researchers) were in a perpetual state of panic each time a new list of names arrived and differed from the earlier promised list. This created a multitude of emergent threats to validity, among them:

- differential selection-experimental variable interaction,
- selection-maturation interaction effects,
- maturation,
- differential selection of subjects, and
- instability (see, for example, Smith, 1980).

The study could have been discarded, but as most of the threats related to interaction effects and differentiation among the populations, all respondents were considered as one pool. The sample was changed from a blocked design to a single population with no blocks. In doing this, we were able to eliminate two of the major threats. To address the instability threat, significance measures were done using ANOVAs in order to look within and between groups, with the groups being defined by demographics rather than the block design. Finally, the threat of maturation was addressed by looking at differences between late respondents from one frame and comparing them to early respondents of the next frame.

The final result: a national study of a random selection of individuals from 10 organizations that could be considered representative of the larger population. Statistics were nonparametric or, in the case of the ANOVAs, parametric but for the purpose of more clearly describing the population, rather than generalizing. Although the study was published in peer-reviewed journals without a hint of the frame error, the real learning for me was the challenge of addressing the compounding problems.

### Problematic Answers

In theory, studies ask questions or state hypotheses without knowing the answer(s) that will be generated. In reality, most students, researchers, supervisors, funders, and sponsors have an opinion or wish as to what an answer might be. Quality study design aims to enable that wish to either be subsumed (as much as possible) under statistical methods, or to be acknowledged and articulated to the extent that it could impact the study findings. Nonetheless, stories of some problematic answers may be useful and interesting to others in the field. These stories illustrate the jeopardy of discovering things a research sponsor doesn't want to know, of learning that the funder (or implementer) is not interested in what you have found, of losing access to some answers mid-stream, and of being caught in political mire.

### *Finding Answers the Funder Doesn't Want to Hear (Joe)*

There is an ethical dilemma when doing evaluation work because the work belongs to the organization paying for the evaluation, rather than the evaluator. This can lead to real problems when someone agrees they want an evaluation to “find out what’s working and what can be changed to improve the program” upfront, but when the report is completed decides that they want everything to look good for the funder. On numerous occasions, this shift has led to the organization asking to remove any of the findings that are even slightly critical, and *all* recommendations for improvement. Two alternate approaches could be tried:

- provide the report and allow the organization to pull from the report those things they choose to share, but do not allow alteration of the report itself; or
- offer to present the findings verbally to the funder.

The former creates additional tension around the findings and can lead to resistance to good evaluation. The latter takes additional time, but demonstrates to the program people how to manage findings with funders. It also helps the funders see why a program is truly worthy of additional or continued support, as it is continually improving.

In one study, a board member did not want membership of the organization studied as part of the larger organizational review, and blocked progress on the study. Repeated discussions and messages to stop gathering data led the study team to realize that this individual feared members would not be in favour of some pet projects (changes) of this board member. To satisfy the member and remain ethically true to the research (which needed the input of members), members were not surveyed, but a random selection from the membership roster was interviewed by telephone. This kept the member component of the study separate from the study of staff, board, administration, and the community at large. Not surprisingly, some of the members interviewed liked the changes, and some didn’t. However, major discontent was expressed by members over the way the board and administrators were handling the changes. Members felt that too many important things happening were being kept from them, as exemplified by the reluctance to include members in the study itself! Although this observation was not included in the final report, the findings were presented and one bullet point noted the dissatisfaction of the membership. In this way, we feel we were able to include the concept without being confrontational and/or risking that key stakeholders would reject the entire document because they didn’t like one piece.

### *Murky Participant Permission Process (Charlotte)*

In a study of a group of people planning to build (and live in) a neighbourhood, I was granted initial permission for the research by the community through

a consensus decision process (and a permission form signed subsequently by community leaders). This permission permitted me to attend community meetings, have access to neighbourhood documents, and participate in the neighbourhood Internet list serve. It also allowed me to invite individuals to participate in interviews (I obtained separate written permission on an individual basis). About half the households in the community agreed to (and participated in) an individual interview. Notwithstanding the fact that this study was exempted from the review of the Institutional Review Board at my institution, I have made regular and thorough attempts throughout the years to re-ascertain permission from the community as a whole, and with individuals (especially newcomers) in the community.

After many years of data collection and analysis of these data, I found that some of the most interesting stories and findings feature members of the neighbourhood who did not elect to participate in an individual interview. This produced a dilemma for the research—some findings of high potential interest to the study existed in a murky access situation. These individuals had agreed, through a group consensus decision, to being observed at meetings and to having their emails read that they posted to the list serve. However, I believe that their lack of permission for an individual interview might indicate a discomfort with a more public illumination of their particular participation. Therefore, if I want to highlight those stories and to remain comfortable that permission has been granted, I must decide how, when, and whether to approach these individuals to obtain a second level of permission. A risk exists that a participant may decline permission, which could have implications for many levels of data. Therefore, the decision is actually whether to put some of the data at risk of becoming unusable (with unknown “domino-style” implications), or to use other data (featuring those participants who have given individual interviews) to reveal similar or different findings. In the one case that inspired this anecdote, I asked for and was granted permission to tell the story (“I didn’t agree to an interview because I was just busy,” she said.) Nonetheless, I wonder whether in the end, I will believe that my initial and ongoing permission process was not thorough enough, or that my data and findings were better and richer as a result of the less invasive approach.

### Publication Dilemmas

Many experienced and aspiring academics can tell stories of trying to guess unsuccessfully what is in the editor’s head or, when a piece is rejected from a journal, knowing when to work to change the article to fit, and when (and how) to submit the piece to a different journal where it might fit better.

### *Resubmitting to the Same Journal (Justin)*

Different journals operate different review procedures. The *International Journal of Science Education*, for example, has seen the number of submissions rise, particularly with the introduction of an electronic system (Manuscript Central). The electronic system obviates the need for posting printed papers across the globe which, as well as having environmental benefits, also has concomitant time savings. The journal now publishes 15 editions each year and editors can afford to be robust in attempting to raise the quality of the journal by sending papers to two or more experienced researchers. It is extremely unusual for a paper to be accepted without any revisions. In my experience, almost all papers are either rejected (with an encouragement to resubmit) or they are sent back for major revisions. The message is: persist, listen to the reviewers' comments, and be prepared for a lot of extra work after you think you've finished a paper!

### *Resubmitting to a Different Journal (Michael)*

Sometimes, a paper may not need rewriting as much as it may need resubmitting to a different journal. The reason can be a lack of fit with the type of work the journal publishes, or it may be a more amorphous or invisible reason. In the case of the former, an author should try to identify a journal's theme before submission and, where a misfit occurs, hope that comments from the journal will steer the article in a better direction. Two stories of the latter (amorphous or invisible reasons for rejection) follow.

In one case, an editor rejected an article of mine out-of-hand, and without review, because it was based on a well-known author's perspective. Specifically, I had taken the author's perspective on local, social, cultural, and political aspects of a watershed and its potential role in guiding ecological governance and sustainability, and had related it to place-based curriculum development in environmental education. Apparently, the journal editors believed the application of the perspective to a different curricular situation was not unique enough to warrant publication. I chose not to revise but to resubmit to a different publication, where the article was published without any editorial changes.

In a second case, I realized that several papers from early in my career could be combined into a synthesis paper focusing on student understanding of ecological crises. The new paper addressed science and natural resource management concepts across several ecological phenomena and provided insight across disciplines. As with the example above, the first submission was again rejected out-of-hand, and was not sent out for review by contributing editors. The only reason given was that the research involved work over five years with the assistance of approximately 30 graduate students. Again, a subsequent submission of the same paper to an international journal resulted in publication with little revision.

These two experiences have helped me develop a “thick skin” when it comes to reviews that seem to simply dismiss my work. My current perspective is that editors have power that they may choose to wield in a variety of ways, some not as fair or just as others. From my experience, it appears second opinions are just as good in publishing as they are in medical diagnoses.

## Conclusion

This article is about encouragement. We identify with the challenges researchers face in exploring their questions and interests in the face of new theoretical perspectives—interpretive, critical, and postmodern. More traditional pre-ordinate frames have been supplanted. In their place, new kinds of questions require diverse theoretical perspectives that often cross disciplinary boundaries. Indeed, we have gone beyond the days when a cookbook approach to methods was sufficient. Researchers now employ a range of methods across a wide spectrum of methodologies that advocate emergent methods more consciously responsive to changing conditions in the field, as well as sensitive to theoretical issues of ethics, power, and authority. The issues, methodology, and methods needs cannot always be anticipated in design.

The authors have attempted to provide insights from work amongst diverse categories and frames of inquiry that are meant to encourage fellow students to persist and problem-solve mid-process. We encourage an emergent trend for authors to write about, and publish, their own struggles in a way that anticipates critique. We hope that increasing instances of researchers telling stories of mucking around the methodologies and ethics of inquiry can be the beginning of a more process-oriented, and perhaps more nuanced conversation that opens active discussion and scrutiny amongst critical friends.

## Notes on Contributors

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