

Rachel Carson Center

Perspectives

How to cite:

Hall, Marcus. "Conspicuous Elevations and the High Art of Posing the Right Question." In: "Crossing Mountains: The Challenges of Doing Environmental History," edited by Marcus Hall and Patrick Kupper, *RCC Perspectives* 2014, no. 4, 65–73.

All issues of *RCC Perspectives* are available online. To view past issues, and to learn more about the Rachel Carson Center for Environment and Society, please visit www.rachelcarsoncenter.de.

Rachel Carson Center for Environment and Society
Leopoldstrasse 11a, 80802 Munich, GERMANY

ISSN 2190-8087

© Copyright is held by the contributing authors.

SPONSORED BY THE



Federal Ministry
of Education
and Research

Deutsches Museum 



Marcus Hall

Conspicuous Elevations and the High Art of Posing the Right Question

In the first sentences of his 1936 treatise on mountains, geographer Roderick Peattie confidently declares that “a mountain, strictly speaking, is a conspicuous elevation of small summit area.” Conspicuity, he explains, is an essential yet indefinite element of a mountain and “depends upon the personal evaluation or the standard by which it is measured.” A mountain may therefore be a few hundred feet or a few thousand feet high, depending on the observer.¹ We begin to realize why mountains have been so inspirational to hermits and heretics, soothsayers and soul searchers, poets and scholars. If *nature* is the primary concern of environmental historians, eliciting dozens of interpretations, *mountain* is also rich with connotations and denotations. Mountains are what we want them to be, and so make ideal subjects for learning about ourselves and our histories.

I have always lived in or near conspicuous elevations, or longed for them. For whole years at a time, whether at the foot of the Rocky Mountains, the Alaska Range, the Andes, the Alps—or Blue Mounds, Wisconsin (a barely perceptible rise on the horizon that only flatlanders learn to appreciate)—I thought I saw conspicuity. Swept up in a “transnational” world and the call for “comparative” histories in graduate school, I decided to write my own doctoral dissertation comparing Europe’s Alps with America’s Rockies. Surely here were two famous mountain ranges that deserved more simultaneous attention. Both ranges had served as my refuge, and putting them side-by-side seemed an ideal way to make sense of the world and my place within it. All histories are ultimately biographies of sorts. So I might as well embrace my own past by writing about something that I knew. Besides, academic advisors oft repeated that one must choose a doctoral project according to its ability to enthrall and entice its creator, even on the rainiest of days, for it was a long journey between topic selection and book publication. Juxtaposing these two mountain systems was therefore my subject as well as my method. Now all I needed was a question.

In confronting the challenges of “doing” environmental history—as in most pursuits—the practitioner is always better off when asking the right question. Of course one of the first ways to know which question to ask is to read widely and converse with experts in the

1 Roderick Peattie, *Mountain Geography: A Critique and Field Study* (Cambridge: Harvard University Press, 1936).



Figure 1:
Restoring
watersheds in the
Italian Alps with
check dams, ca.
1890. G. di Tella,
*Il Bosco Contro il
Torrente* (Touring
Club Italiano,
1912).

field. By learning more we realize what isn't known, and so we begin to identify what cries out for greater understanding. But there is more to identifying the right question than going to the library. In what follows, I offer my own ruminations on how one best approaches a research topic and then selects a crucial question. I tidily (and somewhat facetiously) categorize the challenge of selecting the right question according to the five *Ps*: Personal, Practical, Procedural, Professional, and Public criteria. Although such issues will be second nature to many scholars, reviewing some of them here may aid those who are just setting out toward their own conspicuous heights. A bit of reflection now may save a few from pursuing unrealistic, uninteresting, or unanswerable questions later.

Personal Interests

As mentioned, the best question is often one that is integral to one's own background, so that one can already identify some of the questions that need asking. During our 2013 Graduate Summer Workshop in Switzerland, scholarly climbers investigated climbing

histories, scholarly skiers investigated skiing histories, and scholarly environmental activists investigated environmentalist topics: their passion and knowledge reflected more than impersonal learning. My own dissertation topic had been about restoration history, using mountains as places that had been deforested and degraded, and then in some areas, reforested, restabilized, and rejuvenated. I felt that the conservation pursuit of ecological restoration deserved historical reflection, and I believed that mountains were a good place to explore the restorative enterprise. What had been the experience of mountain restorers? Had Rocky Mountain and Alpine restorers practiced their craft differently? Even if my dissertation topic was not immediately relevant to my advisory committee, I felt that this research was still worth pursuing, for it was satisfying to me personally. I was and am concerned about ways to fix a damaged earth, and I was intent on offering rigorous insight into stories that did not show merely decline and despair. Mountains became my laboratory for exploring how humans had wrestled with environmental repair.



Figure 2:
Restoring watersheds in the Utah Rockies with contour trenching, ca. 1930. W. Bailey and A. Croft, "Contour Trenches Control Floods and Erosion on Range Lands," *United States Forest Service Publication 4* (1937).

There was the other pragmatic fact that in those graduate school days, I consumed most of my jet fuel traveling back and forth between my selected mountain ranges. My family lived at both ends of this mountain divide, and I would be making my way between these ranges regardless of which doctoral project consumed me. Such family issues also meant that language, too, was working on my side, for I realized that dealing with obscure archival records would require a good command of the local vernacular; fortuitous earlier circumstances meant that I could handle Romance tongues reasonably well but not Germanic ones. Clearly my research questions and my study sites in the Alps were better centered on valleys draining into the Po and Rhone than into the Rhine and Danube. While I admire those who simply throw a dart on the world map and then set out to learn more about that place, I did not want to spend extra graduate school years just acquiring another language or two before I could begin to wonder which questions might be asked.

Practical Matters

Challenging logistics are part of every research project and question. Thus, winter is a bad time to carry out field research in temperate mountains, unless snow and ice are fundamental to one's research question. August is a bad month to expect easy access to Italian archives. Pursuing almost any question that requires viewing Italian Renaissance military maps, moreover, will require special perseverance, as it is certainly easier to be granted an interview with the Pope than to gain admittance to the library of Florence's Istituto Geografico Militare. Part of my own project would eventually trace landscape changes over the last one hundred, but not the last five hundred years, as the availability of cartographic data modified the questions that I asked. In his own graduate school days, one of my dissertation advisors spent a year as a research assistant bringing order to a dusty storehouse of land-use records, which then became a primary source for answering his own dissertation questions. Certainly Machiavelli would have used his own contacts and influence to plan his historical career, including the way he crafted the questions that he might have pursued. Of course, this is not to say that graduate students should be devious or immoral in identifying their own questions—but at the same time, opportunism should not be excluded from a researcher's toolbox.

Thinking about scale (as shown by Emily Wakild's essay) is also key to crafting the right question, for our projects cannot be too large or too small. "Global" environmental histories should be attempted only by the brilliant or the foolish, as it would seem (despite Braudel's call) that there can be no such thing as *total* history. We always need to choose pieces of the past to focus on: a three-year project requires a three-year question; a three-month project, a three-month question. The scope of a master's thesis is not that of a doctoral thesis, and the question being answered must be adjusted accordingly. With his book *In 1926*, Hans Gumbrecht demonstrated that it is possible to write a whole book about a single year (and presumably other books about a single month or a single day), but most histories still address a handful of years or centuries.²

Students in my own "Topics in Global Environmental History" course often pose sensational questions for their own research projects, but ones that would require much more time than they have allocated. Rather than advising young researchers to "narrow their

2 Hans Ulrich Gumbrecht, *In 1926: Living on the Edge of Time* (Cambridge, MA: Harvard University Press, 1998).

topic,” it may be more fruitful to suggest that they narrow their question, for example by time, space, or scope. In my own first major research project, I asked how reforestation and erosion control may have differed between a valley in the Alps and one in the Rockies. In retrospect, I am sure that I was lucky to have not sought to compare a third valley, or to extend my study much beyond the last century or two. Research questions are often reduced, refined, or redirected as one confronts practical limits of time and funding. One’s *magnum opus* is usually best left until after graduate school.

Procedural Concerns

Linked to the practical considerations of selecting a good question is the process of identifying the steps needed in order to answer it. First off is usually the required literature review, or historiographic survey, which typically reveals how little is known about one’s topic, but how much is known about everything else. Yet almost without one realizing it, this background reading often reveals obscure archival repositories, noteworthy people or places, and (crucially) ways for honing one’s original questions. After reviewing this historiography, one can better settle on an optimal case study, identify fruitful narrative techniques, and reveal the extent to which comparison may be necessary or superfluous. As an aside, I have heard claims that all history is comparative, whether between past and present, between ideas of one person and the next, or between landscapes here and there: as a result, self-conscious comparative history is not often needed for presenting useful answers to a question. In the end, the project of “reviewing the literature” may in the best of scenarios lead to other, seemingly unrelated bodies of literature. Indeed, demonstrating links between two seemingly disparate bodies of knowledge is what the best history projects do.

The process of crafting an exciting question should never be formulaic, as research always presents serendipity. In the throes of a different research project, I remember sitting down one morning at the long oaken table of the big state archives in Rome, waiting for “my” boxes of records to be carted out, and then being notified that the boxes were already in use. At the other end of the table, I spied a senior scholar who was immersed in those very records. With some inquiry, I found out he had been wrestling with some of my same questions—except that he was far ahead of me on this pursuit, just then crossing t’s and dotting i’s on a manuscript heading to press. Luckily for me, a subse-

quent conversation with him showed that he had steered clear of one area that seemed particularly fruitful, and so I realized then what would become my own research focus. A chance encounter in Rome shifted my whole project. Perhaps knowing when and how to act on such encounters is the artistic side of carrying out research.

There are the databases that one must check, such as Google Scholar or JSTOR. There are the writing manuals to glance over, such as Atchity's *A Writer's Time*.³ Advanced graduate students will be way ahead of the curve if they thumb through any of several how-to guides in converting theses to books. All such exercises can highlight how one's initial question may be too large, too obscure, or too academic. Does your question catch the attention of your friends, or are you almost embarrassed about your obsession with a particular puzzle? Instead of explaining to them what your research is about, try announcing the questions that you aim to answer. Expect such questions to change as you work further on your project.

Professional Considerations

My own comparison of the Rockies and the Alps was an apples and oranges affair. Probably every comparison is strained, because deeper inspection reveals that both sides are always different from one another—but that is why you compare them. The Alps are jagged and populated; the Rockies are mostly rounded and uninhabited. The Alps underwent gradual deforestation and sustained grazing while the Rockies witnessed dramatic changes in land cover and land use. Pressures to rehabilitate and reinstate wildness were much greater in the American mountains. It was precisely the differences that made this transnational comparison so appealing to me. I believe that almost any kind of comparison is possible if one can identify the necessary constraints on how to go about doing it. In comparing disparate mountain ranges, especially through the eyes of travelers, I had responded to my own guild's call to consider two or more phenomena or regions simultaneously. In retrospect, the decision to consider both mountain ranges was a good choice, one that required an extra year or two in the archives, extra practical and procedural considerations, and one that may have produced extra insights. But the comparative choice was also influenced by peers and

3 Kenneth Atchity, *A Writer's Time: Making the Time to Write* (New York: Norton, 1995).

mentors. Is trendiness a legitimate motive for selecting a question? How much should we mold our interests to meet the pressures imposed by our colleagues?

Professional realities typically mean that historians, for example, need to pose questions that can lead to one substantial monograph; that promote the development of intriguing opportunities for teaching and outreach; that are sufficiently captivating to a non-expert to attract outside funding; and that can intrigue the upper echelons of one's field, university, or established community.

But the problem with pursuing *hot* topics—going where there is money—is that by the time one is deeply immersed in the relevant questions, such questions may be stale. Besides, there are so many other unknowns out there begging for attention. Climate history is fascinating stuff, but today's budding climate historians should be seeking more than better proxy information for estimating the nineteenth century's warmest summers. At the same time, rugged individualists who set their own research courses risk becoming irrelevant to the greater community of colleagues who may want to hear about their work. It would be too simple (and too simple-minded) to brush aside the trendy work of others and head off blindly into unknown lands. Perhaps one's stage in the professional ladder helps influence how bizarre one's question may be, with the most securely employed scholars being able to pursue the most unorthodox research agendas. But I would like to think that those posing the most unlikely questions should enjoy some of the best prospects to continue funding their projects. Perhaps one should still pursue topics acknowledged as being crucial, but utilize unusual methods or sources when seeking to answer them.

In the best of all worlds, budding researchers would be able to develop their own questions, rather than be handed one from an advisor, book, or other authoritative source. The more a researcher is constrained by the question handed down, the more creativity is compromised. We must be suspicious of the practice of allowing senior scholars to set research agendas. Alternatively, if the only way of securing funding or employment is to play lip service to Kuhn's Normal Scientists, so be it. But when those checks finally start rolling in, researchers should be free to readjust their course toward more pressing questions at hand.

Public Issues

Beyond acknowledging professional considerations, one must also pay attention to public relevancy. I have heard it argued (by someone I admired) that today's researchers can no longer afford the luxury of asking merely theoretical questions, for we have far too many other pressing questions confronting us. Of course the easy rejoinder to this plea is that we cannot easily predict which questions will turn out to be most useful. The historical profession itself is not very relevant to a large fraction of the public, for it seems that historians are mostly preoccupied with a few dead men and (fewer) dead women. Ongoing public involvement is required to convince our skeptics that revealing the roots of a problem is a crucial step in figuring out how to resolve it.

What the above promoter of practical research meant to say, I think, is that researchers must seek to answer exciting questions, ones that stimulate us to think in new ways, that uncover strange facts or trends that we did not know existed, and that may provide different angles for viewing familiar scenes. Questions that aim to add "building blocks" to a pyramid of knowledge that we already know the shape of are not very stimulating. Researchers isolated from society's current challenges are likely to be pursuing tired questions, unconnected to the real world. Environmental historians foster delicate relationships with environmental activists, and our research questions should animate both groups. And yes, I believe one of our highest achievements is to gain the attention of the general public. Whoever that public may be, it is certainly our highest critic and the one that is most difficult to satisfy. Dissertations are better crafted as drafts of books, rather than as academic treatises. The disparaged "journalistic account" may actually teach a historian a thing or two about fresh writing.

Answering questions for our publics may also include not writing at all, or at least not writing on paper. I am convinced that audiences are not becoming more illiterate, even though one source says that just a fourth of Americans read a book last year. Europeans are apparently better bookworms, but they too increasingly forego books.⁴ More and more, our readership absorbs their ideas by other means: orally, visually, and if in written form, more briefly but more frequently. TED talks have now exceeded a billion view-

4 Jordan Weissman, "The Decline of the American Book Lover," *The Atlantic*, 21 January 2014. Reportedly, European bookless rates last year varied from 10 to 50 percent depending on country. See "Europeans Turning Their Backs on Culture, Survey Says," *EUobserver*, 5 November 2013, accessed 27 February 2014, <http://euobserver.com/news/121987>.

ings.⁵ Clearly we need to put more of our answers on webpages and social media, even if this requires reshaping the length and style of our messages. We might pursue more questions that lend themselves to 10,000 words and a Vimeo spot rather than 100,000 words and two hard covers. People are more curious than ever, but they are less willing to plod through dense prose offering opaque answers when there are more captivating and more informative sources available.

Mountains make captivating subjects. Our graduate seminar in eastern Switzerland did not spend all of its hours inside a climate-controlled conference room. On one sunny afternoon, the group of us went outside and hiked a trail to a meadow. We watched each other admiring the scenery, where conspicuous peaks stood above and a lake lay below. Inside that national park, we were not permitted to leave the trail, as this protected area is a strict nature reserve, and our controlled admiration of it demanded questions. Why were most of us glorifying mountains more than wetlands or the plains? Who had decided we could not leave the hiking paths, and whose interest did the park's present status best serve? Why in that moment did we cherish a hot sun, and appreciate nibbling our sandwiches away from the luxury indoor comforts that awaited us that evening? How did a glance from that viewpoint represent a landscape—a *Landschaft*—that had been *crafted* to conform to our expected view? And where would that side-path following the ridge take the more adventurous hiker?⁶ These were all good questions, questions that could lead to other questions, and might stimulate a few of us, if not to try to answer them, then to find out how others might have answered them.

5 "TED Reaches Its Billionth Video View," *TED Blog*, 13 November 2012, accessed 27 February 2014, <http://blog.ted.com/2012/11/13/ted-reaches-its-billionth-video-view/>.

6 On the environmental history of the Swiss National Park, see Patrick Kupper, *Creating Wilderness: A Transnational History of the Swiss National Park* (New York: Berghahn, 2014). My own mountain answers are reported in *Earth Repair: A Transatlantic History of Environmental Restoration* (Charlottesville: University of Virginia Press, 2005).