“The rigours of an arctic experiment”: the precarious authority of field practices in the Canadian High Arctic, 1958 – 1970

Richard C Powell
School of Environment and Development, University of Manchester, Oxford Road, Manchester M13 9PL, England
Received 7 August 2005; in revised form 16 November 2005

Abstract. The author examines the development of the notion of the field experiment in High Arctic environmental sciences during the period 1958 – 70. After a discussion of the philosophy and sociology of experiment, the author considers a set of field practices conducted under the auspices of the Canadian Government’s Polar Continental Shelf Project. Drawing on archival and oral historical research, he argues that field scientists had to deal with a number of logistical, corporeal, and epistemic difficulties in the High Arctic. It is demonstrated that these obstacles hindered attempts to develop a scientific literature based upon experimental practices during fieldwork. In doing so, the author attempts to set new agendas for historical geographers of science around the analysis of the geographical sciences, whilst also contributing to discussions about the epistemic status of variegated field practices.

Introduction

“‘Suppose you tell me what you know,’ he [Lord Boreale] said.

“‘They’re doing experiments in the North,’ Lyra said.”

Philip Pullman (1995, page 95)

Philip Pullman’s novel, *Northern Lights* (1) concerns a young heroine, Lyra, who is drawn impulsively from her home in Jordan College, Oxford, all the way to the Polar Sea in order to investigate wicked ‘experiments’ being conducted upon kidnapped English children. It turns out, in this particular narrative, that experimental surgery on young children allows the creation of an entry bridge to another universe through the Aurora. Such constructions of the North as locales of adventure are a common and much discussed trope of Northern literature. I mention this here merely to suggest that adventurous discourse regarding the North has always been concerned with demonstrating the conquest of a nature that is exactly intemperate. I want to argue here that the idea of a *Northern experiment* has held even greater epistemic cachet precisely because it involves control over those pathological natures.

In this paper I attempt to illuminate this relationship between the Arctic as an *expeditionary space* and the Arctic as an *experimental space*. I will show that an elision is evident that goes much deeper than children’s fiction, and even entered into published discussions about the best conduct of field science during the late 1950s and 1960s in the Canadian Arctic. This is accomplished through an examination of environmental science conducted in the Canadian High Arctic under the auspices of the Polar Continental Shelf Project (PCSP), a branch of the Canadian Government’s Department of Mines and Technical Surveys, over the period 1958 – 70.

---

(1) *Northern Lights* forms the first instalment of the ambitious *His Dark Materials* trilogy (Pullman, 1995; 1997; 2000).

* DOI:10.1068/a38294
By documenting some of the field practices involved in these multidisciplinary scientific investigations of the continental shelf, I argue that what, in a 19th-century European–Alpine context, Bruce Hevly termed the “authority of adventurous observation” was still evident in this later period, despite the best efforts of Canadian field scientists to deploy a precarious authority of experiment (Hevly, 1996, page 68).

In this paper, therefore, I comment on debates about scientific practice in the field sciences; specifically, attempts to adapt experimental methods to field situations. I show how the misconstrued importance attributed to field experiments resulted in physical, emotional, and, ultimately, epistemic difficulties for PCSP scientists.

The practices of the geographical sciences and Arctic environments
A number of commentators have discussed the emergence of spatial thinking across the social sciences and humanities over the past two decades (Gregory, 1994; Soja, 1989). This was no less the case for historians and sociologists of science who began to emphasize the placing of knowledge claims (Kohler, 2002a; 2002b; 2002c; Ophir and Shapin, 1991; Shapin, 1991; 1998; 1999; Smith and Agar, 1998). These moves have inspired a number of historical geographers to plead for the important geographies of scientific activities (Livingstone, 2002; 2003; 2005; Naylor, 2005; Withers, 2001).

Such discussions have succeeded in demonstrating that scientific practices can be understood geographically. However, these revivified historical geographies of knowledge have been much less successful influencing the historiography of what have been hitherto understood as the ‘geographical sciences’. The complexity of the geographical sciences has thus only recently begun to enjoy the attention from historians of science that it deserves (Greene, 1985; 1989; 1997; Oldroyd, 1998a; 1998b; 1999; Oreskes and Fleming, 2000; Siever, 1997). As Mott Greene complains, there is still sufficient lack of interest that the “history of the geosciences will probably be written, for some time yet, mostly by earth scientists themselves” (1989, page 331). Geographers of science, for various reasons of institutional and intellectual propinquity, are strategically placed to begin to develop such accounts (Crang, 1998; Livingstone, 2002; Lorimer, 2003a; 2003b; Lorimer and Spedding, 2005). However, the complicated diversity of topographical, observational, and experimental practices involved in the geographical sciences, and their relative implications for practitioners, has often been neglected. Inspired by such discussions, therefore, in this paper I attempt to move the agenda for historical geography of science forward by examining some of the geographies of the geographical sciences.

This neglect of the epistemic and emotional practices of the geographical sciences is important for our purposes here because the connections between scientific and exploratory praxis in the polar regions have been significantly intertwined. Cultural historian Max Jones (2003), for example, has shown how scholars have often disregarded or, even worse, scoffed at, the important scientific intentions of Scott’s expedition to Antarctica under the auspices of the Royal Geographical Society. This has been no less the case for the PCSP scientists discussed in this paper.

Moreover, recent histories of cartography have stressed the need to take account of particular practices of surveying in different regions (Burnett, 2000; Collier and Inkpen, 2002; Craib, 2004; Edney, 1997; Ries, 2002). The environment of the Arctic has provided obstacles to “description, measurement, charting, and even physical penetration” by nonindigenous peoples (Collis, 1996, page 27). The geographical environment thus meant that survey of the Canadian Arctic was difficult, resulting in the adoption of specific practices.

Furthermore, in this paper I argue that the relation between geographical survey and geographical science in the polar regions has not been fully understood by
historians of the geosciences. The distinction was, for PCSP fieldworkers at least, crucial (interview, Denis St-Onge, 11 April 2002, Ottawa, ON). Although political geographer Klaus Dodds is right to stress that the “collection of accurate geographical information was essential” to justify Britain’s claims to its South Atlantic territories, it is misguided to transfer this to a wider generalization across all Polar environments (Dodds, 2002, page 24). The actual geographical practices that were to be deployed in the service of territorial sovereignty were in flux by the early 1960s, and this was no less the case in High Arctic Canada. As I will show, it is far too simplistic to argue for the High Arctic, as Dodds is able to do for Antarctica in this period, that “scientists were beginning to replace explorers” (Dodds, 1997, page 56; 2000).

In this paper I examine an epoch when, in the Canadian Arctic, geographical scientists moved from a conception of good scientific activity as involving topographical survey, to one of structured observational measurements, and then to field experiments, in a period of around fifteen years. In what follows, I attempt to reveal some of the complicated relationships between activities which were construed to be scientific whilst also allowing some sort of demonstration of territorial sovereignty.

However, before considering how these debates impacted on the PCSP, it is necessary first to understand what a field experiment was taken to be in the 1960s, and why it was deemed to be so important.

The philosophy of experiment

A number of studies by historians and philosophers of science have remarked on the need for scholars to study actual experiments (Collins, 1992; Gooding et al, 1989a; 1989b; Hacking, 1983; Le Grand, 1990; Rheinberger, 1997; Ronell, 2003a; 2003b; 2005; Shapin and Schaffer, 1985). As David Gooding, Trevor Pinch, and Simon Schaffer remark, “Experiment is a respected but neglected activity” (Gooding et al, 1989b, page xiii; 1989a). Such accounts have begun to view “experiment as an active process of argument and persuasion” (Gooding et al, 1989b, page xvi). Experiments are yet another idiom through which scientific authority is constituted (Schaffer, 1989).

This is an important injunction. However, it has led to a general lack of attention from students of science studies as to why scientists believed that they should undertake certain kinds of actual experiments. Bruno Latour (1990) collapses entirely the distinction made between observational measurements, whereby phenomena over which humans have no control are recorded, and experiments in the field, whereby phenomena are (supposed to be) manipulated by humans, usually (though not necessarily) using instrumentation, because the results brought back from the field from these two procedures appear visually alike. This reduction has been very influential in the readings of experiment by geographers. Many historical geographers of science would therefore seem to concur with Scott Kirsch (1998) when, in a study of the deployment of US nuclear technologies in the 1960s, he argues that this observational – experimental distinction is merely semantic.

This may well be the case, but it is important to state that to PCSP scientists this distinction was absolutely critical to their sense of self. In order to understand this point, it is necessary to get a better handle on what an experiment was supposed to be in the 1960s. I should stress here that the discussion of the meaning and purpose of field experiment in this paper is limited to the environmental field sciences between 1958 and 1970 in the Canadian High Arctic. Different understandings of experiment are, of course, distributed spatially, temporally, and epistemically across scientific pursuits.
However, for Karl Popper writing in 1957, scientists should attempt to emulate physicists:

“Physics uses the method of experiment; that is, it introduces artificial controls, artificial isolation, and thereby ensures the reproduction of similar conditions” (2002b, page 7; 2002a).

These requirements for control and replication were fundamental to postwar philosophies of science.

The philosopher Rom Harré provides a clear examination of the supposed distinction between structured observational measurement, or “an exploration”, and an experiment (1981, page 23). Explorations are important, for Harré, forming “a kind of intervention in the natural world which yields knowledge, but lacks the manipulative character of the true experiment” (Harré, 1981, page 23). An experimenter, Harré argues:

“is in a different relation to natural things. He actively intervenes in the course of nature. ... Experimenters describe their activities in terms of the separation and manipulation of dependent and independent variables. The independent variable is the factor in the set-up that the experimenter manipulates directly. The dependent variable is the attribute which is affected by changes in the independent variable. ... By careful design of an experiment it is possible to maintain constant all properties except those one wishes to study, the dependent and independent variables. A property which is fixed in this way is called a ‘parameter’. Fixing the parameters defines the state of the system within which the variables act” (1981, page 22).

A consequence of this requirement for the separation of variables and fixing of parameters, as Harré himself states, is that it “seriously restricts the use to which experiments can be put” (1981, page 22). Many phenomena, therefore, are simply not practically amenable to experimentation. However, such practicalities did not dampen the enthusiasm for experimental methods amongst many environmental scientists in the postwar decades.

During the 1960s, despite the obvious derivation of such models of explanation from analyses of laboratory physics, various philosophers of science, including David Harvey in *Explanation in Geography*, termed this approach to experimentation “the ‘standard’ model of scientific explanation”, under which any hypothesis with a significant degree of confirmation was a scientific law (Harvey, 1969, page 30, my emphasis; Simpson, 1963; Watson, 1966). Moreover, under this system, the definition of progress in scientific activity relies on the establishment of such laws, which normally proceed through the identification and confirmation of physical processes under laboratory experimentation.

Notwithstanding the popularity of this model, philosophers have begun to argue that the situation is very different in the environmental sciences because they involve complex systems (Oreskes and Fleming, 2000; Siever, 1997). These are systems in which all variables vary, and different mechanisms interact continuously—thus making the drawing of boundaries to effect some sort of experimental closure highly imperfect. As geomorphologists Michael Church and David Mark put it, rather than from deductive reasoning and theories, “most inquiries, at least in the field sciences, are initially prompted by the observation of formal relations” (1980, page 342).

---

(2) Environmental geographers have used the term *open systems* (Richards, 1990; 1994).
However, necessary statements in theories, as the apogee of explanation in this scientific model based on physics, were to be achieved through controlled experimentation. And a scientific experiment was thus designated, to quote a particularly succinct definition from a later paper by Church, in the following way:

“A scientific experiment is an operation designed to discover some principle or natural effect, or to establish or controvert it once discovered. It differs from casual observation in that the phenomena observed are, to a greater or lesser extent, controlled by human agency, and from systematically structured observations in that the results must bear on the existence or verity of some conceptual generalization about the phenomena” (Church, 1984, page 563).

The requirements for a classical experiment were thus based on a laboratory situation. It should be evident, therefore, that this sort of employment of experimental method is rare in the environmental sciences. As well as the relevant variables being covariant over space – time, complex, open systems are often too large, and the temporal scales of interest too long, for convenient experimentation to test theories. Moreover, phenomena of interest are often too public and the costs prohibitively high for legitimately designed experiments. Indeed, any open system reduced to elements amenable for experimentation could be construed as inherently dysfunctional, because in attaining analyzable simplicity the variation that the system depends on for stability is removed (Church, 1984). At the same time, however, the very scientific status of disciplines such as hydrology, glaciology, and geomorphology was viewed to depend upon their ability to establish such a “corpus of rational statements” (Church and Mark, 1980, page 381; Church et al, 1985). Otherwise such activities, as geographer Barbara Kennedy argued over twenty-five years ago, would remain at a natural history stage of scientific progress by relying on collection, description, and the classification of observations (Kennedy, 1977; 1979).

(3) Herein lies the dilemma: how to master the epistemic resources of experimentation on environmental systems?

Field experiments in the environmental sciences

Despite the limitations discussed, laboratory experiments are still often conducted in environmental science, but inevitably must either involve incredibly short spatiotemporal scales, or models that are susceptible to scaling effects. Precisely because of these restraints, successful experimentation in the field was deemed crucial to establishing theories for the field sciences (Church, 1984). As historian of science Robert Kohler states, field scientists are used to adapting to “a world that takes for granted that experiments are the better, or even the only, way of knowing nature” (2002b, page 1).

The concept of the field experiment as deployed by environmental scientists in the 1960s had first emerged in agricultural field trials in the 1930s (Yates, 1970). Developments in statistical techniques after 1945, coupled with increasing sophistication in the understanding of experimental design, facilitated the widespread adoption of field experiments in the environmental sciences (Fisher, 1960). As such, experiments were designed for situations “in which the effects under investigation tend to be masked by fluctuations outside the experimenter’s control” (Cox, 1958, page 1). Such fluctuations are usually meteorological in the environmental field sciences.

(3) These debates amongst physical geographers drawn from here are particularly apposite disciplinary instantiations of much wider discussions across the philosophy of earth and environmental science which first surfaced in the early 1960s, as the multidisciplinary PCSP field scientists began their research. See, for example, historians of geology George Gaylord Simpson (1963) and Richard Watson (1966).
By employing principles of experimental design sensu lato, “comparative experiments” could proceed in the field, whereby a number of alternative treatments are applied to individual “experimental units”, and observations (or a set of observations) are made for each unit (Cox, 1958, pages 4, 12, my emphases). As Cox argues in the standard textbook used by postwar field scientists, the object is thus “to be able to separate out differences between the treatments from the uncontrolled variation that is assumed to be present” (1958, page 12).

Other types of experimental procedures have been adopted by field scientists (Henke, 2000; Rees, 2001). For Kohler, a whole litany of field practices, or “practices of place” (2002c, page 204), have been developed in order to maintain the credibility of the field sciences:

“Precise measurement of environmental variables was more place dependent, but though many lab instruments were too fragile and precise for field use, they could be adapted, and suitable places found for their deployment. But experiment was different: this quintessential laboratory practice loses its power outside the controlled and placeless place for which it was designed” (2002c, page 195).

Such practices may also, for Kohler (2002c), include attempts by fieldworkers simply to find fieldsites where a single environmental variable changes while others are constant. In this way, Kohler argues, waiting for “the appropriate time and the right place to observe in nature is no less active than fixing conditions in a lab” (2002c, page 200). It is by developing these “skills of selecting and using places” that scientists are able “to operate effectively in the field” (2002c, page 205).

However, as experimental design was quickly adopted by environmental scientists in the 1960s, many field scientists were much more conservative than Kohler admits in their attempts to draw from experimental methods (Kennedy, 1992).(4) The popular approbation of experimental methods became problematic for some field scientists when many began to designate any set of structured observational measurements an ‘experiment’, neglecting the important component of controlled, anthropogenic modification of isolated variables (Ahnert, 1980; Church, 1984; Le Grand, 1990). For many field scientists, this perceived sloppiness was directly attributable both to philosophical and statistical naivety, as well as to the difficulties that harsh environments imposed on “testing theory in the field with rigour” (Church and Slaymaker, 1985, page xi). In order that the repercussions of this epistemic confusion be fully comprehended, it is necessary to examine some particular cases of fieldwork.

Experiments in High Arctic environmental science, 1958–70
Much 20th-century environmental science has constructed the Arctic as “a pristine natural laboratory for the field sciences” (Bravo and Sörlin, 2002, page vii). Moreover, this natural laboratory was to allow the performance of in situ experiments. Such desires for successfully conducted field experiments in the Arctic were exacerbated in the immediate post-1945 sciences. Indeed, such experiments formed part of a much wider ambition in the postwar Anglo-American natural and social sciences to deploy hypothetico-deductive schemata, and the impact of these influences on the conduct of contemporaneous human and physical geography has been well documented (Barnes, 1996; Gregory, 1978). Thus by focusing on environmental sciences in the Arctic during a

(4) In order that I am not misunderstood here, I should note that the widespread adoption of the field experiment in physical geography did not occur until the second half of the 1970s. It would appear that there are some intellectual links between some of the protagonists in Canada and PCSP field scientists, but they are distinct developments. On the International Geographical Union Commission on Field Experiments in Geomorphology, and its Paris Symposium of October 1978, see Slaymaker et al (1980) and Slaymaker (1980).
period when immense excitement was devoted to field experiments, we can begin to understand how the geography of science has, in David Livingstone’s words, had an impact on the “cognitive content” of science (2002, page 89). And in this way, we can develop a more empirically informed historical geography of the geographical sciences.

In order to examine field experiments in High Arctic environmental science, I focus below on some of the work done by members of the PCSP. See figure 1.

The PCSP was set up in May 1958, and, in broad brush, its foundation was in response to a set of related scientific and political developments. The late 1950s had seen the Canadian High Arctic increase in strategic importance following the development of intercontinental ballistic missile systems that had made the Distant Early Warning Line effectively obsolete and increased the importance of submarine warfare. The Soviet launch of Sputnik 1 and Sputnik 2 in October and November 1957, as part of the International Geophysical Year, had led to requests by the US for geomagnetic and geodetic data for the most northerly regions of Canada. This had prompted Canadian determination to reconfigure US–Canada relations in the High Arctic, whilst at the same time maintaining their adherence to continental defence. In part this was a direct consequence of Prime Minister John Diefenbaker’s unprecedented election

---

(5) Memorandum to Cabinet: Canadian Activities in the Polar Basin, by Alvin Hamilton, Minister of Northern Affairs and National Resources, 3 April 1958, Library and Archives of Canada (LAC), Record Group (RG) 45, Volume 300, Cabinet Document 126-58.
success of 1958, whereby his Progressive Conservative minority government of 1957 was returned with 208 seats. This was the largest majority in Canadian federal history, and had been achieved on the back of a campaign drawing heavily on the supposed ‘northern destiny’ of Canada. As such, the paucity of Canadian knowledge of the vicinity of the continental shelf had come to national attention and there was concern to remedy this. Previous Canadian activity in the region of the shelf was limited to Vilhaljmur Stefansson’s Canadian Arctic Expedition, 1913–18 (Levere, 1988), and the Geological Survey of Canada’s recent Operation Franklin (1955). Operation Franklin had comprised a 28-person expedition to the Queen Elizabeth Islands, which demonstrated the utility of combining multidisciplinary research agendas and the deployment of modern technology in the field, and had also indicated the presence of thick accumulations of sedimentary rocks and structures (analogous to those found in oil fields).

This need for action was all the more important as the International Conference on the Law of the Sea in Geneva that same year, 1958, had concluded that maritime states should have control over the resources on their continental shelves (the submerged portion of a continental landmass). The closest Canada then had to maps of the continental shelf which extended under the relatively shallow water of the Arctic Ocean had been obtained from the USSR. This situation was no longer to be tolerated.

The foundation of PCSP was recommended in March 1958 by a Technical Sub-Committee of the Advisory Committee on Northern Development and, significantly, comprised representatives of various agencies and departments of the Federal Government of Canada. For the reasons sketched, it had been deemed expedient to initiate a programme of hydrographic, oceanographic, geophysical, and biological studies of the continental shelf as a direct means of asserting Canadian sovereignty in the High Arctic (Hobson, 1990). It was important that the PCSP be an autonomous organization, separate from the requirements of other established agencies conducting Arctic research such as the Geological Survey of Canada and Defence Research Board (Hobson, 1990; Roots, 1960; 1962).

Under its first Coordinator E F (Fred) Roots, a PCSP field party began work to establish a surveyed baseline to facilitate an operational navigation system in 1959, based out of the Joint Arctic Weather Station at Isachsen, Ellef Ringnes Island; the first full-scale field season commenced during the following spring of 1960. This area of initial interest, chosen both for reasons of convenience and for the congruence of a number of channel entrances among the Arctic islands, was a band 300 miles long, centred on Isachsen, and stretching 100 miles out to sea.(6)

**The physicality of Arctic fieldwork**

It was exactly the overcoming of the traditional challenges of Arctic fieldwork that, for Roots, defined the PCSP. In promotional literature produced by PCSP, the project was supposed to have run “down the curtain on the expeditionary ‘hit-and-run’ approach to arctic research” and replaced this with “a sustained, long-term, integrated effort” (PCSP, 1974, page 5).

---

(6) “A research program to provide information on the extent and characteristics of the continental shelf and other phenomena, both physical and biological, of the Polar Basin–Polar Continental Shelf Project”, Report of Technical Sub-Committee of the Advisory Committee on Northern Development, 10 March 1958, LAC, RG45, Volume 300, File 1-1-1 [Marked Confidential].
In the course of its early years, the PCSP was able to establish a number of logistical precedents in Canadian Arctic research. These included, for example, the establishment of a 900-watt low-frequency Decca ‘6f Lambda hyperbolic’ survey and navigation system, and the marking of mid-March to May as the best period for air support of parties on sea ice. In doing so, as early as 1959 Roots was able to claim to his superior in the Department of Mines and Technical Surveys, W E van Steenburgh, that “the party has managed to do much work that some old Arctic hands had considered impossible.”

Moreover, as a consequence of its departmental autonomy, the PCSP was able to remain flexible, even footloose, not only in its organization but also in combining interdisciplinary scientific research programmes in unprecedented ways. As Roots wrote for a PCSP Steering Committee meeting in 1960, the basic principle was “to complete a thorough study of a problem, rather than general wide-ranging shallow surveys”. Roots was to reminisce in a later interview that:

“I was insistent from the beginning that whatever we did it would not be recognition for its own sake: the work we were going to do in the High Arctic should be of the same scientific quality and accuracy as work done anywhere else, regardless of latitude” (Roots in Foster and Marino, 1986, page 16).

As stated at the outset, this emphasis on scientific practices over more ostensibly adventurous pursuits is a conceit typical of descriptions of Arctic activity. As literary critic Christy Collis argues, many Arctic narratives deploy these familiar patterns “in order to construct the North as Eden, as hell, as passively empty, as savagely adversarial, as picturesque, as sublime, as female, as godless, and as God’s trial of man” (1996, pages 27–28). What was different in the PCSP case was that the progressive edge of science was to involve successful deployment of the experimental method in the field. In sea-ice studies, for example, Roots was keen to describe “a series of experiments ... carried out to determine the effect of coating the surface of sea ice with various materials that changed the index of solar absorption.” These were complementary with a similar series of experiments conducted on Meighen Island icecap by Keith Arnold, a geographer seconded to PCSP from the Geographical Branch, who used aluminium foil and chicken wire, which should have been great publicity for the PCSP “being something that the public could grip on to right away.”

Scientists employed by PCSP thus intended to move beyond arguments about establishing a physical presence in the Arctic by conducting structured observations and, ultimately, experiments. As Denis St-Onge, another member of the first PCSP

---

(7) It is a hallmark of the success of the organization that, following completion of the study of the continental shelf area, the geographical extent of PCSP research had grown to encompass the entire Canadian Arctic by late 1970s. The PCSP continues to operate today as a logistics facilitator for scientific research in the Canadian Arctic.

(8) E F Roots, Co-ordinator PCSP, to W E van Steenburgh, Director-General Scientific Services, Department of Mines and Technical Surveys, 7 August 1959, LAC, RG45, Volume 300, File 1-I-3.

(9) Notes, handwritten, by E F Roots, for PCSP Steering Committee Meeting, 16 February 1960, LAC, RG45, Volume 300, File 1-I-3, page 4.

(10) Although this paper is concerned with the research conducted by the PCSP in the High Arctic, it is important to mention that geographer J R Mackay pioneered field experiments in periglacial geomorphology from the late 1970s in the Mackenzie Delta, in the mainland Canadian Arctic (Mackay and Burn, 2002).

(11) E F Roots, “Notes on Ice Studies conducted by the Polar Continental Shelf Project, Department of Mines and Technical Surveys”, “Paper No. 32”, 4 October 1960, LAC, RG45, Volume 308, File 3-8-0. The quotation is from page 1.

(12) Copy of letter from K Arnold, Meighen Island Icecap, to E F Roots, 11 July 1959, LAC, RG45, Volume 308, File 3-8-0. The quotation is from page 1.
field parties and later President of the Canadian Association of Geographers, argued, there was little tolerance in the field for the “Boy Scout attitude to Arctic fieldwork” (Interview, Denis St-Onge, 11 April 2002, Ottawa, ON).

In order to exorcize any hints of adventurous discourses to facilitate scientific progress, the PCSP scientist had to meet a number of challenges. As historian Sverker Sörlin has argued, the “holistic hero” of the Arctic was always a “scientific omnivore”, competent across a number of disciplines (2002, page 109). This was no less the case with the PCSP, with a series of extra dimensions.

In the first place, the PCSP field scientist had to develop a corporeality which was dependent upon environmental immunity. The ability to employ instrumentation regardless of conditions was important to Roots in his selective composition of the field parties:

“I wanted Frank Hunt. He was far and away the most experienced winter topographical surveyor we had at that time. He had run the 60th parallel survey in wintertime. He had already shown that just because it is cold and the wind is blowing, there is no reason to be less accurate with a theodolite than when it is nice and sunny” (Roots in Foster and Marino, 1986, page 25).

Second, PCSP scientists had to be competent managers of logistics. This should be clear after consideration of the objectives that the PCSP had to achieve. Roots summarizes this as follows:

“We needed not only to go from place to place but to be able to know precisely where we were at all times and to return at different seasons of the year at different times with different equipment to the exact, precise, same spot. To do this we had to have a combination of surface transport and air transport equipment. If we used helicopters, ... we had to have two, in case one broke down. To fly a helicopter you had to have fuel, that meant fixed-wing aircraft that could land anywhere on the ice to carry the fuel.... In the end result, we found ourselves arranging for a small airforce with its own communications, its own navigation equipment, [and] its own supply route” (Roots interviewed on film—see Energy, Mines and Resources Canada, 1986).

Third, scientists in the field had to be skilled at various sorts of manual tasks, such as small-scale mechanical and electrical engineering. As a result of such responsibilities, St-Onge reminisces:

“I was supposed to be there to do science, I ended up being mostly mechanic the first summer” (Interview, Denis St-Onge, 11 April 2002, Ottawa, ON).

Moreover, those in positions of perceived lower status within the PCSP were also expected to be multiskilled in order to facilitate successful fieldwork. In interviews with PCSP mechanics and field-support staff, this multitasking during long working days in difficult conditions was stressed above all other recollections.\(^\text{(13)}\) In 1959, for example, an assistant cook was hired from Quebec because, despite his “modest” cooking abilities,\(^\text{(14)}\) he was fluently bilingual, and “had a very wide background of practical experience in the mechanical, electrical, and woodworking trades, plus considerable ‘bush’ experience as a trapper and lumberman”\(^\text{(15)}\).

\(^\text{(13)}\) Interviews with Christian Barmig, 14 August 2002, Ottawa, ON; Leif Lundgaard, 16 August 2002, Cornwall, ON; Bill Presley, 13 August 2002, Ottawa, ON.

\(^\text{(14)}\) E F Roots, Co-ordinator, PCSP, to R B Code, Chief, Personnel Division, Department of Mines and Technical Surveys, 31 August 1959, “[Name withheld], Labourer, acting as general assistant to Polar Continental Shelf Project”, LAC, RG45, Volume 302, File 1-2-1, page 1.

\(^\text{(15)}\) E F Roots, Co-ordinator, PCSP, to R B Code, Chief, Personnel Division, Department of Mines and Technical Surveys, 31 August 1959, “[Name withheld], Labourer, acting as general assistant to Polar Continental Shelf Project”, LAC, RG45, Volume 302, File 1-2-1, page 1.
In addition to deploying all these skills, this young man also took full responsibility for the kitchen at the Isachsen base camp for the entire season as well—as the Chief cook failed to arrive for his duties in the field.\(^{(16)}\)

This importance of being a competent fieldworker is evident in other discussions of field science in northern Canada. In a retrospective on the career of the permafrost specialist J R Mackay in the western Canadian Arctic, a contributor argued that any “adventure” by a “good scientist” was supposed to be merely the result of “some miscalculation” (Matthews, 1985, page 8; see also Jahn, 1985). It was, thus, still the cultivation of such characteristics as physical fitness and curiosity by the field scientist that allowed successful experimentation to be conducted (Matthews, 1985).

### The rigours of an Arctic experiment

As might be expected, however, such ideals regarding the reconfiguration of scientific practice and individuality by the PCSP were problematic. Moreover, these difficulties were often encountered by the scientists precisely because, despite their experimental aspirations, the ultimate reason for the funding of the PCSP was the completion of the cartography of Canada in defence of national sovereignty. This was most evident, for example, in a Government of Canada press release, which Alvin Hamilton had demanded as part of increasing public awareness of northern Canada (Robertson, 2000), and which was issued as the first PCSP field season commenced in 1959.

“Starting this spring, Canada is going to send scientific expeditions each year into the forbidding arctic wastes north of the Canadian archipelago and beyond to carry out a broad program of research on the rim of the Basin[.] ... The project, when fully organized and operating, will be the biggest and boldest scientific venture of its kind ever undertaken in northern Canada. Scientist–adventurers of rugged stock are needed to man the expeditions: men imbued not only with love of research but with the same venturesome spirit that sent men like Hudson, Frobisher, Baffin, and Davis into the trackless wastes of the Arctic to probe its secrets.”\(^{(17)}\)

It was precisely this ambivalence that made Roots’s goals for Arctic science so difficult to achieve. PCSP literature would commonly refer to the fact that the “men involved ... are conscious of making history” or that some employees were “the first human beings to set foot on some of the Arctic Islands, hitherto known to geographers only from air photographs” (PCSP, 1974, page 6). Those involved in PCSP fieldwork thus clearly constructed themselves as different from those “ordinary ‘other scientists’” working further south (PCSP, 1974, page 3).\(^{(18)}\) The boundary of field practices was an arena of contestation for science and adventure.

Indeed, Roots himself was to argue in print towards the end of the 1960s that logistics difficulties in consequence of the environment of the High Arctic had an immense impact on the research conducted (Roots, 1969). A PCSP publication was even to state that this formed “an entire psychological theory of arctic research”

\(^{(16)}\) E F Roots, Co-ordinator, PCSP, to R B Code, Chief, Personnel Division, Department of Mines and Technical Surveys, 31 August 1959, “[Name withheld], Labourer, acting as general assistant to Polar Continental Shelf Project”, LAC, RG45, Volume 302, File 1-2-1, page 1.

\(^{(17)}\) “Canada will do Research in the Polar Basin”, Department of Mines and Technical Surveys, Government of Canada, 20 February 1959, LAC, RG45, Volume 306, File 2-9-1, page 1. The masculinist discourses circulating through this statement are obvious. See, for developments of this argument in different Arctic contexts, Bloom (1993), Smith (1994), and Grace (2001).

\(^{(18)}\) The point about PCSP fieldworkers being the first human presence on Ellef Ringnes Island and Meighen Island was also made in interviews (Denis St-Onge, 11 April 2002, Ottawa, ON; George Hobson, 15 May 2002, Ottawa, ON).
articulated by Roots (PCSP, 1974, page 12). Such could be the impediment to research, argued Roots, that there are

“many investigations whose subject and priorities have changed markedly because of logistics considerations, and others where the investigators have become so dominated by the logistics challenge that they have been compelled to list as ‘results’ the number of miles travelled or samples taken, rather than show the increase in understanding of the subject” (1969, page 68).

Moreover, logistics and environmental considerations affected Arctic research to such a degree that, by the end of the 1960s, they had shattered Roots’s faith in the achievement of experimental field methods:

“Aside from influencing the general seasonal nature of the work, logistics considerations tend to impress on a northern research party a schedule that is dictated not by the research itself, but by the problems of transportation, communication, and supply... The inflexibility of schedule often forces hasty work, with no chance for consultation or repeated observation; it leads to a habit of rushed, uncritical observation and description, of wholesale gathering of information with the hope that later analysis and reconsideration will sort out the irrelevant and find that nothing really relevant has been missed. It develops a ‘now or never’ philosophy about the gathering of information, and creates a tolerance for incomplete, inaccurate or even shoddy research on the grounds that logistics constraints did not allow time or freedom to do a more careful job or to check observations and conclusions” (1969, page 69).

As Roots put it to me in one of a set of interviews:

“The methodology of travelling determines what kind of information you get... A geologist working only with a helicopter might get a different picture of what the geology was than the one who was backpacking[.]... So that’s where research and methodology gets blurred. Sometimes the methodology determines what kind of results you get” (interview, E F Roots, 6 May 2002, Gatineau, PQ).

Most telling of all, however, is the following example from the attempt by two PCSP seismologists to construct a seismic refraction profile across the continental shelf, which resulted in a paper published in the *Canadian Journal of Earth Science* in 1971. For the authors,

“The aim of the experiment was to obtain a crustal section from the coast of the northeast–southwest trending shoreline of the polar continental margin and slope to the water of the Canada Deep” (Berry and Barr, 1971, page 347).

Unfortunately for Berry and Barr, this field experiment was hampered by bad weather and poor position control:

“In the event, the experiment had to be severely curtailed. *The authors appreciate that it is not normal practice to burden the reader with the difficulties of field practice but rather to concentrate exclusively on the results. However, the rigors of an arctic experiment and the uncommon problems encountered may be of interest to any other scientists contemplating a similar endeavor. ... Suffice it to say that the unusually low temperatures encountered during April 1967, coupled with several unfortunate accidents, prevented our placing a recording station any further than approximately 220 km from Houghton Head. ... Some problems were experienced in determining the positions of the shots and of the ice station, and as these problems will be a recurring difficulty in Arctic Ocean seismic work, they will be recounted in some detail” (page 348, my emphases).

The authors go on to discuss at length the difficulties of position control using a Decca navigation system on the ice to the west of Prince Patrick and Brock Islands. ‘Camp 200’, an ice camp out on the ocean, was subject to an unexpected, radical path
through April 1967 as various large-scale pressure systems built up in the ice. It turns out that Roots had to make a special request to Computing Devices of Canada, who provided the Decca 6f Lambda chain positioning technology to the PCSP, for interpretation of location of the seismic shots.\(^{(19)}\) It appears that during the 26-day period, Camp 200 moved a total of 25 km to the northeast along a ‘random walk’ path (Berry and Barr, 1971).

As it happened, the accidents were precipitated by the destruction by fire of a shed at the Main Camp at Mould Bay, Prince Patrick Island. This resulted, a few days later, in a four-man helicopter party being marooned out on the ice for four days, following mechanical complications in flight, because due to the fire there were no operational aircraft to effect a rescue and no workshop space was available for aircraft maintenance. Bert Burry, a pilot on contract with PCSP, thus described Mould Bay in 1967 as “a camp full of troubles” when providing a detailed account of the events after his return from the field to Uranium City, Saskatchewan.\(^{(20)}\) The problem with such field projects is that the complicated logistics rely on the constant operability of the different components of the network. As Burry joked, it was fortunate that both Roots and the PCSP operations manager, Frank Hunt, were at the Mould Bay camp during this period of setbacks to the completion of the scientific programme, as “it would be most difficult to believe all the foregoing unless one was there to witness it”.\(^{(21)}\)

But what is most interesting about this example is that it should have become evident that these activities never really comprised an experiment at all. Rather, by the contemporary strictures of the philosophy of experiment, a set of structured observational measurements to determine the seismic refraction profile were performed. However, this failure in experimental design came to be of less importance than coping with unprecedented environmental conditions. And it was the documenting of these circumstances that became a necessary, scientific, activity after the field season had ended.

Rather less than deploying an authority of experiment, then, attempts by PCSP scientists to move to a conduct of Arctic field practice based on a model from physics merely succeeded in reintroducing the authority of adventurous observation in different ways. The authority of field experiment was always precarious. Nor should we be surprised at this, as historians of science have shown that experiments form part of a rhetorical armoury—like so many other scientific activities.

**Conclusion**

Despite their best efforts, then, those involved in the fieldwork of the PCSP had not discarded completely the epistemic baggage of the exploratory tradition and adventurous observation by the late 1960s. As to do so was deemed of great importance in order for the environmental sciences to become ‘scientific’, in the sense of classical laboratory physics, this was a matter of some regret amongst many of those involved in the PCSP activities in the Arctic. Indeed, perhaps this paper should be read less as a narrative of the rigours of Arctic experiment, and more as an argument about the impossibility of the

\(^{(19)}\) Letter, E F Roots to Computing Devices of Canada Ltd., Market “Attention: Mr. Peter Wilson”, 1 June 1967, LAC, RG45, Volume 374, File 4750, Part 1. The Decca 6f Lambda chain was the first Decca system that had survey, as opposed to aircraft-navigation, capability (interview, E F Roots, 22 April 2002, Gatineau, PQ).

\(^{(20)}\) Letter, [Bert Burry] to George [Burry] and Jimmy [Burry], 12 April 1967, LAC, RG45, Volume 377, File 5155-4, Part 1, page 1.

\(^{(21)}\) Letter, [Bert Burry] to George [Burry] and Jimmy [Burry], 12 April 1967, LAC, RG45, Volume 377, File 5155-4, Part 1, page 4.
dream of field experiment—at least along the lines prescribed by the Popperian readings of earth and environmental scientists. However misguided such philosophies of field experiment may appear from our intellectual vantage points, I hope to have conveyed that the development and application of such practices was of great importance to the actors involved in constituting the PCSP.

It was a consequence of the multidisciplinary nature of these activities that scientists would be at the same field bases, on board the same aircraft, and even participate in the same field parties, as colleagues with quite different backgrounds and training. Geophysicists such as Berry and Barr, although undertaking investigations into seismic refraction, would often interact in the field with topographical surveyors or those constructing geomorphological maps. There would always be dialogue in the field between experimental practice and adventurous observation. However, this laudably diverse array of scientific accomplishment notwithstanding, the desire for field experiments permeated the institutional culture of the PCSP, and Berry and Barr as geophysicists enjoyed the consequent status within the scientific hierarchy of the base camps. Indeed, as the very founding purpose of the project was the investigation of the continental shelf, the emphasis here on this seismic refraction profile is particularly apposite.

This study thus carries wider significance for historians of geography, as the debates about field experiment examined in this paper anticipated some later debates about experimentation in physical geography, and especially in fluvial geomorphology and hydrology. Moreover, as I have argued elsewhere (Powell, 2004), it was crucial for the history of the geographical sciences in Canada that many of the PCSP field scientists had read undergraduate degrees in geography. It is therefore through consideration of such cases that a historical geography of the geographical sciences might be developed. Amongst the flourishing studies of geographical field cultures (Lorimer, 2003a; 2003b; Lorimer and Spedding, 2005; Matless, 2003; Naylor, 2002; Powell, 2002), we must be wary of imposing a historiography that imposes too much uniformity on understandings of geographical field practice. Historical geographers of science have much to learn from investigations of particular cases of attempts to adopt experimental methods. Geographers of science might thereby be able to generate understandings of the geographical sciences that are more sensitive to the peculiarities of fieldwork.

As Ian Hacking put it, “Experimentation has a life of its own” that often exceeds its bounds of initiation (1983, page 150). As I have argued here, the engagement between geography and experiment has always been vivacious, not least because it depends on the contested question of ‘theory’. This is not the locale to commence that history, but perhaps by accepting Trevor Barnes’s (2001, page 550) notion of “hermeneutic theorizing”, which stresses creativity and catholicity in understanding, historical geographers of science might be able to reappropriate a much neglected slogan for our engagements with science studies which, at least some, field scientists were ready to adopt in 1969—“By our theories you shall know us” (Barnes, 2001, page 550; Harvey, 1969, page 486). Even if we no longer expect our theories to endure the various rigours of experimental control.

Acknowledgements. This research has been supported financially by an ESRC/NERC Interdisciplinary Research Studentship (R42200034029), an ESRC Postdoctoral Fellowship (PTA-026-27-0112), the Royal Society (London), HGRG and HPGRG of the RGS-IBG, the Foundation for Canadian Studies in the UK, the British Association of Canadian Studies, the International Council of Canadian Studies, l’Université d’Ottawa, and the University of Cambridge. Earlier versions of this paper were presented to audiences in Cambridge (November 2002) and at the IGC-UK in Glasgow (August 2004) and I am grateful for comments received. The generous criticisms of three anonymous referees improved the paper. I must thank Nick Scarle for producing the map.
References

Ahnert F, 1980, “A note on measurements and experiments in geomorphology” Zeitschrift für Geomorphologie Supplementband 35 1 – 10

Barnes T J, 1996 Logics of Dislocation: Models, Metaphors, and Meanings of Economic Space (Guilford Press, New York)

Barnes T J, 2001, “Retheorizing economic geography: from the quantitative revolution to the ‘cultural turn’” Annals of the Association of American Geographers 91 546 – 565

Berry M J, Barr K G, 1971, “A seismic refraction profile across the polar continental shelf of the Queen Elizabeth Islands” Canadian Journal of Earth Science 8 347 – 370

Bloom L, 1993 Gender on Ice: American Ideologies of Polar Exploration (University of Minnesota Press, Minneapolis, MN)

Bravo M, Sörlin S, 2002, “Preface”, in Narrating the Arctic: A Cultural History of Nordic Scientific Practices Eds M Bravo, S Sörlin (Science History Publications, Canton, MA) pp vii-ix

Burnett D G, 2000 Masters of All They Surveyed: Exploration, Geography and a British El Dorado (University of Chicago Press: Chicago, IL)

Church M, 1984, “On experimental method in geomorphology”, in Catchment Experiments in Geomorphology Eds T P Burt, D E Walling (Geo Books, Norwich) pp 563 – 580

Church M, Mark D M, 1980, “On size and scale in geomorphology” Progress in Physical Geography 4 342 – 390

Church M, Slaymaker O, 1985, “Preface”, in Field and Theory: Lectures in Geocryology Eds M Church, O Slaymaker (University of British Columbia Press, Vancouver), page xi

Church M, Gomez B, Hickin E J, Slaymaker O, 1985, “Geomorphological sociology” Earth Surface Processes and Landforms 10 539 – 540

Collier P, Inkpen R, 2002, “The RGS, exploration and empire and the contested nature of surveying” Area 34 273 – 283

Collins H, 1992 Changing Order: Replication and Induction in Scientific Practice 2nd edition (University of Chicago Press, Chicago, IL)

Collins C, 1996, “The voyage of the episteme: narrating the north” Essays on Canadian Writing 59 26 – 45

Cox D R, 1958 Planning of Experiments (John Wiley, New York)

Craig R B, 2004 Cartographic Mexico: A History of State Formations and Fugitive Landscapes (Duke University Press, Durham, NC)

Crang M, 1998, “Places of practice, and the practice of science” Environment and Planning A 30 1971 – 1974

Dodds K, 1997, “Antarctica and the modern geographical imagination (1918 – 1960)” Polar Record 33(184) 47 – 62

Dodds K, 2000, “Putting maps in their place: the demise of the Falkland Islands Dependency Survey and the mapping of Antarctica, 1945 – 1962” Ecumene 7 176 – 210

Dodds K, 2002 Pink Ice: Britain and the South Atlantic Empire (IBTauris, London)

Edney M H, 1997 Mapping an Empire: The Geographical Construction of British India, 1765 – 1843 (University of Chicago Press, Chicago, IL)

Energy, Mines and Resources Canada, 1986 Islands in the Midnight Sun: The Story of the Polar Continental Shelf Project (programme transcript), Carleton Productions, Toronto

Fisher R A, 1960 The Design of Experiments 7th edition (Oliver and Boyd, Edinburgh)

Foster M, Marino C, 1986 The Polar Shelf: The Saga of Canada's Arctic Scientists (NC Press, Toronto)

Goodyng D, Pinch T, Schaffer S, 1989a, “Introduction: some uses of experiment”, in The Uses of Experiment: Studies in the Natural Sciences Eds D Goodyng, T Pinch, S Schaffer (Cambridge University Press, Cambridge) pp 1 – 27

Goodyng D, Pinch T, Schaffer S, 1989b, “Preface”, in The Uses of Experiment: Studies in the Natural Sciences Eds D Goodyng, T Pinch, S Schaffer (Cambridge University Press, Cambridge) pp xiii – xvii

Grace S E, 2001 Canada and the Idea of the North (McGill-Queen’s University Press, Montreal and Kingston)

Greene M T, 1985, “History of geology” Osiris (2nd series) 1 97 – 116

Greene M T, 1989, “Afterword”, in From Hutton to Hack Ed. K J Tinkler (Unwin Hyman, London) pp 325 – 331

Greene M T, 1997, “What cannot be said in science?” Nature 388 (14 August) 619 – 620

Gregory D, 1978 Ideology, Science and Human Geography (Hutchinson, London)

Gregory D, 1994 Geographical Imaginations (Blackwell, Oxford)
Oldroyd D, 1998b Sciences of the Earth: Studies in the History of Mineralogy and Geology (Ashgate, Aldershot, Hants)
Oldroyd D, 1999, “Non-written sources in the study of the history of geology: pros and cons, in the light of the views of Collingwood and Foucault” Annals of Science 56 395 – 415
Ophir A, Shapin S, 1991, “The place of knowledge: a methodological survey” Science in Context 4 3 – 21
Oreskes N, Fleming J R, 2000, “Why geophysics?” Studies in the History and Philosophy of Modern Physics 31 253 – 257
PCSP, 1974 Islands in the Midnight Sun: The Story of the Polar Continental Shelf Project (Polar Continental Shelf Project (Information Canada, Ottawa))
Popper K, 2002a The Logic of Scientific Discovery (Routledge, London)
Popper K, 2002b The Poverty of Historicism (Routledge, London)
Powell R C, 2002, “The Sirens’ voices? Field practices and dialogue in geography” Area 34 261 – 272
Powell R C, 2004 Intemperate Spaces: Field Practices and Environmental Science in the Canadian Arctic, 1955 – 2000 unpublished PhD thesis, Department of Geography and Scott Polar Research Institute, University of Cambridge, Cambridge
Pullman P, 1995 Northern Lights (Scholastic Children’s Books, London)
Pullman P, 1997 The Subtle Knife (Scholastic Children’s Books, London)
Pullman P, 2000 The Amber Spyglass (Scholastic Children’s Books, London)
Rees A, 2001, “Practising infanticide, observing narrative: controversial texts in a field science” Social Studies of Science 31 507 – 531
Rheinberger H-J, 1997 Toward a History of Epistemic Things: Synthesizing Proteins in the Test Tube (Stanford University Press, Stanford, CA)
Richards K, 1990, “‘Real’ geomorphology” Earth Surface Processes and Landforms 15 195 – 197
Richards K, 1994, “‘Real’ geomorphology revisited” Earth Surface Processes and Landforms 19 277 – 281
Ries C, 2002, “Lauge Koch and the mapping of north east Greenland: tradition and modernity in Danish Arctic research, 1920 – 1940”, in Narrating the Arctic: A Cultural History of Nordic Scientific Practices Eds M Bravo, S Sörlin (Science History Publications, Canton, MA) pp 199 – 231
Robertson G, 2000 Memoirs of the Very Civil Servant: Mackenzie King to Pierre Trudeau (University of Toronto Press, Toronto)
Ronell A, 2003a, “The experimental disposition: Nietzsche’s discovery of America (or why the present administration sees everything in the form of a test)” American Literary History 15 560 – 574
Ronell A, 2003b, “Proving grounds: on Nietzsche and the test drive” Modern Language Notes 118 653 – 669
Ronell A, 2005 The Test Drive (University of Illinois Press, Urbana, IL)
Roots E F, 1960, “Canadian Polar Continental Shelf Project, 1959” The Polar Record 10(66) 275 – 276
Roots E F, 1962, “Canadian Polar Continental Shelf Project, 1959 – 62” The Polar Record 11(72) 270 – 276
Roots E F, 1969, “The role of logistics in northern research”, in Proceedings of the Second National Northern Research Conference Ed. J J Bond, Boreal Institute, University of Alberta, Edmonton, AB, pp 65 – 77
Schaffer S, 1989, “Glass works: Newton’s prisms and the uses of experiment”, in The Uses of Experiment: Studies in the Natural Sciences Eds D Gooding, T Pinch, S Schaffer (Cambridge University Press, Cambridge) pp 67 – 104
Shapin S, 1991, “The mind is its own place: science and solitude in seventeenth-century England” Science in Context 4 191 – 218
Shapin S, 1998, “Placing the view from nowhere: historical and sociological problems in the location of science” Transactions of the Institute of British Geographers, New Series 23 5 – 12
Shapin S, 1999, “Rarely pure and never simple: talking about truth” Configurations 7 1 – 14
Shapin S, Schaffer S, 1985 Leviathan and the Air-pump: Hobbes, Boyle, and the Experimental Life (Princeton University Press, Princeton, NJ)
Siever R, 1997, “Doing earth science research during the cold war”, in The Cold War and the University: Toward an Intellectual History of the Postwar Years Ed. A Schifferin (New Press, New York) pp 147 – 170
Simpson G G, 1963, “Historical science”, in The Fabric of Geology Ed. C C Albritton (Addison-Wesley, Reading, MA) pp 24 – 48
Slaymaker O, 1980, “Geomorphic field experiments: inventory and prospect” Zeitschrift für Geomorphologie Supplementband 35 183 – 194
Slaymaker O, Dunne T Rapp A, 1980, “Geomorphic experiments on hillslopes” Zeitschrift für Geomorphologie Supplementband 35 v-vii
Smith C, Agar J, 1998, “Introduction: making space for science”, in Making Space for Science: Territorial Themes in the Shaping of Knowledge Eds C Smith, J Agar (Macmillan, London) pp 1 – 23
Smith N, 1994, “Geography, empire and social theory” Progress in Human Geography 18 491 – 500
Soja E W, 1989 Postmodern Geographies: The Reassertion of Space in Critical Social Theory (Verso, London)
Sörlin S, 2002, “Rituals and resources of natural history: the North and the Arctic in Swedish scientific nationalism”, in Narrating the Arctic: A Cultural History of Nordic Scientific Practices Eds M Bravo, S Sörlin (Science History Publications, Canton, MA) pp 73 – 122
Watson R A, 1966, “Is geology different: a critical discussion of ‘The fabric of geology’” Philosophy of Science 33 172 – 185
Withers C W J, 2001 Geography, Science and National Identity: Scotland Since 1520 (Cambridge University Press, Cambridge)
Yates F, 1970 Experimental Design: Selected Papers of Frank Yates (Charles Griffin, London)
Conditions of use. This article may be downloaded from the E&P website for personal research by members of subscribing organisations. This PDF may not be placed on any website (or other online distribution system) without permission of the publisher.