The Canadian Geographer
Le Géographe canadien
Canadian Association of Geographers
Association Canadienne des Géographes

Slow science, the geographical expedition, and Critical Physical Geography

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Key Messages

- Isabelle Stengers' critique of 21st-century science points to the need to change how we relate to the subjects that we study, under the umbrella of "slow science."
- Human geographer William Bunge's notion of geographical expedition may be a means of doing this, even if "expedition" is a term to be used cautiously.
- Slow science may allow a more creative and critical Physical Geography centred on the very curiosity that makes being a scientist so interesting.

Physical Geography has evolved to become a highly productive mainstream natural science, delivering on the metrics required by the accounting systems dominating the neoliberal University. I argue that the result has been: (1) a crisis of over-production (of more articles than we are capable of consuming); (2) a risk of under-production (growing scarcity in our ability to produce the research questions needed to sustain our productivity); and (3) a "disciplinary fix" involving either pursuit of the problem-solving implicit in the neoliberal impact agenda or creative destruction, aligning ourselves less with geography and more with the natural sciences. Using Isabelle Stengers' critique of 21st-century science, I argue for a slowing down in Physical Geography, by changing how we relate to the subjects that we study. I use the ideas of William Bunge to discuss the notion of geographical expedition as a means of achieving slow science, even if "expedition" is a term to be used cautiously. I illustrate these points from one of my own projects to show how slow science may allow creation of those moments that might lead to a more creative and critical Physical Geography centred on the very curiosity that makes being a scientist so interesting.

Keywords: Isabelle Stengers, David Harvey, William (Bill) Bunge, Critical Physical Geography, slow science

« Slow Science », l'expédition géographique et la géographie physique critique

Au fil du temps, la géographie physique est devenue une science naturelle conventionnelle et très productive répondant aux exigences des paramètres des systèmes comptables du monde universitaire néolibéral. Je suis d'avis qu'il en ressort: (1) une crise de surproduction (d'un nombre d'articles qui dépasse notre capacité d'appropriation); (2) un risque de sous-production (une attrition croissante de notre capacité à poser les questions de recherche requises pour soutenir notre productivité); et (3) un « positionnement disciplinaire » caractérisé soit par la résolution des enjeux prioritaires de recherche appliquée inhérents aux préoccupations néolibérales, soit par la destruction créatrice en se conformant aux sciences de la nature aux dépens de la géographie. Partant de la critique que formule Isabelle Stengers à propos des sciences au 21e siècle, je plaide en faveur d'un ralentissement de la géographie physique en faisant évoluer la façon dont nous nous

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The Canadian Geographer / Le Géographe canadien 2017, 61(1): 84–101
DOI: 10.1111/cag.12329

rapportons à nos sujets d'étude. Les idées défendues par William Bunge forment le point de départ d'une discussion portant sur la notion d'expédition géographique pour le développement de « slow science », même si le terme « expédition » doit être interprété avec précaution. Les projets que j'ai réalisés servent d'exemples pour montrer de quelle manière « slow science » pourrait contribuer à la mise en place des conditions favorables à l'émergence d'une géographie physique plus créative et critique qui met en valeur le sens de la curiosité propre aux scientifiques.

Mots clés : Isabelle Stengers, David Harvey, William (Bill) Bunge, géographie physique critique, slow science

Introduction

> *We are scientists. We don't blog. We don't twitter. We take our time.*
>
> The Slow Science Movement 2010

In 1972, David Harvey presented a paper that was to become seminal to the discipline: "Revolutionary and counter revolutionary theory in Geography and the problem of ghetto formation." Whilst its origins can be traced back into the 1960s, by the early 1970s there was a growing sense that geography needed a second revolution, one which would allow "a greater professional involvement with matters of contemporary social concern ... geography in the universities needs to become more responsive to the social awareness of the new generation of students brought up in the modern era of protest and activism" (Smith 1971, 154). Smith goes on to argue that a "critical review of the scope of geography is long overdue" because "we appear to have been so tightly locked into traditional ways of structuring our knowledge of the world, so deeply committed to existing paradigms, and recently so preoccupied with techniques and methodology" (1971, 156). This shift to a more radical and more critical geography was not just about making geography more relevant, it was "a political critique of the entire mode of social scientific knowledge production and the professional expertise that promised social scientists privileged access to power" (Heyman 2007, 101).

Notwithstanding this shift in Human Geography, and despite important forays into epistemological alternatives to conventionally assumed modes of (natural) scientific knowledge production within Physical Geography (e.g., Rhoads and Thorn 1996), the revolution was largely but not exclusively anodyne to physical geographers who carried on as normal (Demeritt 1996; Lane 2001). Even in an era, the Anthropocene, when the human drivers of environmental change have become more strongly the focus of research attention in Physical Geography, it is largely assumed that the professional expertise of physical geographers gives them privileged access to accounts of that change (see, for example, Johnston 2012). Recently, more than four decades after what went on to become a revolution in thinking in Human Geography, Lave et al. (2014, 2–3) call for the "active integration of physical and critical human geography, as demonstrated in the work of geographers who combine critical attention to relations of social power with deep knowledge of a particular field of biophysical science or technology in the service of social and environmental transformation" in a "new integrative intellectual practice" that they call Critical Physical Geography. Debates over the relative merits of a better integration of the discipline are commonplace (e.g., Johnston 1983, 1998; Gregory 1992; Liverman 1999; Sluyter et al. 2006) but I find myself deeply seduced by the argument of Lave et al. (2014). This is not only because it captures the notion that humans and the environment are at once both constitutive of and constituted by their continual interaction—and as such are co-produced in ways that cause us to interrogate simplistic accounts of human impacts upon the environment. It is also because I find myself increasingly convinced of the need for a more critical account of current Physical Geography (see also Tadaki et al. 2015). My aim in this paper is not to revisit Lave et al.'s case. Rather it is to argue, following Harvey (1972, 110) for a revolution in (Physical) Geography that might address what I see as the deeply disturbing form that Physical Geography as a discipline has become in the 21st century.

My argument has three stages. First, I describe a political economy of Physical Geography, after Harvey (1981), as an analysis of the state that I

perceive the discipline to be in. Second, I draw upon the ideas of a Belgian philosopher of science, Isabelle Stengers (2013), to make the case that central to a Critical Physical Geography(ical) project is a ralentissement in Stengers' phrasing, that is a slowing down of geographical enquiry. Third, I reflect upon ways in which this might be done and, paralleling my own experience in a project where we had to slow down our normal ways of working, I revisit Bunge's (e.g., 1979) notion of a "geographical expedition." This latter stage is important because the forces that run counter to the kind of slowing down that I believe Physical Geography needs are so powerful (e.g., see Halffman and Radder 2015) that change will only be achieved through our physical displacement to the sites of geographical knowledge production that bring us, as physical geographers, to challenge the ways of working that these forces sustain. Thus, following Tadaki et al. (2015), I propose one kind of site that might enable physical geographers to approach their work in a more critical setting.

Towards a political economy of Physical Geography

Implicit in Harvey's account is that invoking a revolution in (physical) geographical thought must attend to both the analytical (that is the state that we are in) and the normative (the state that we should endeavour to become). Whilst a perceived lack of a shared geographical project is nothing new for the discipline (e.g., Demeritt 1996; Sluyter et al. 2006), my reflection on 21st-century Physical Geography suggests to me a number of traits. First, in a turn-of-the-century review of Physical Geography in the United Kingdom (UK), Thrift argued how it had evolved into "big" and "mainstream" science:

> Recently, physical geography has come out fighting and the battleground it has chosen has been mainstream science ... This success is built on the basis of a different model from the one of everything model which tended to operate in the past. Now departments are trying to build up science groups of five or six good people and appropriate technicians who can then seek out large amounts of research money with which to fund equipment, postdoctoral fellows and postgraduates. And the money they have been able to draw on has produced some spectacular scientific products. (Thrift 2002, 291)

Whether this applies to 21st-century Physical Geography beyond the UK could be debated but as Whatmore (2013, 85) observed, "As the contents of disciplinary journals and the publication habits of those working in the two wings of the discipline attest, both are commonly more conversant with work in cognate disciplines through common fields of interest (such as urban studies or glaciology) than with each other's," something that is increasing the outward valence rather than the internal cohesion of the discipline (Clifford 2002). Clifford argues that this big science model "relies on the culturing-in of its adherents, and the culturing-out of its opponents" (2002, 432), that is, the active creation of a particular set of acceptable scientific questions and associated methodologies. The extent to which this is being actively encouraged is reflected in my own experience. Shortly after I arrived in my current University I was informed that Geography was to be restructured, resulting in social science–facing geographers joining researchers in planning and environmental ethics to create an Institute of Geography and Sustainability, and physical geographers joining soil scientists, ecologists, and geochemists in an Institute of Earth Surface Dynamics. Crucially, I was not a passive actor in this process, but actively and constructively involved in questioning the logic of maintaining a research institute (Geography as it was) at the interface of the natural and social sciences. I found myself swept along by something that I found rather attractive (a project that would unite a number of natural science–facing colleagues with largely shared research interests and approaches)—albeit paradoxically so, given that we apparently find ourselves in a new geological epoch, the Anthropocene, where we cannot separate out humans from the kinds of physical geographical questions that interest us, and despite claims otherwise (see Johnston 2012).

Second, a key element of this mainstream natural science is the progressive engagement with questions regarding global change (Castree 2014) and human impacts upon global systems. Yet, and with some notable exceptions in relation to, for example, urban ecology (e.g., Francis 2014), hydrology (e.g., Lane 2014), and rivers (e.g., Ashmore 2015), there has been a marked lack of engagement of physical geographers in questions that genuinely integrate

social and natural science, particularly within global change projects. My sense, in very general terms, is that the global change agenda is valued in Physical Geography as a justification for investment in natural field experiments. The locus of such experiments (e.g., a glaciological focus on the world's large ice sheets) increasingly reflects this justification (e.g., ice sheet impacts on sea level rise), perhaps to the exclusion of other loci of enquiry[i] (such as cities, which might be equally threatened by reduced reliability of glacier-stored water supplies[ii]). There are scale effects here (Richards and Clifford 2008): how do we reconcile the chronic (i.e., distant in space and time) but potentially catastrophic (e.g., sea level rise) impacts of global change with the acute preoccupations of those whose livelihoods are already marginal, whether due to the natural environment or the human environment, or some combination thereof? Of course, global change questions may become more acutely visible in places where there are fewer people. The choice of landscapes that either largely exclude humans or that allow the human content of scientific enquiry to be stripped of its human dimensions (e.g., reduced to discrete effects like rising greenhouse gas concentrations, land use change, dam removal), allows the physical geographer more easily to mimic the kind of scientific experiment-control characteristic of other natural sciences. But this leads to a tension between the continued, perhaps growing need to justify society's investment in Physical Geography, and the impacts of this investment, as a contribution to answering chronic questions surrounding global change; and those acute challenges that are to be found in the messy interactions between people and their environment, and where the multitude of interacting factors makes conventional ("scientific") research design difficult, even impossible. Perhaps this is why Physical Geography remains fundamentally desensitized to more meaningful engagements at the interface between the natural and the social. This is not to belittle physical geographical investigations under the umbrella of global change science but more to challenge them, from two dimensions. First, a focus on global change science is not neutral if it begs its own question (the principle of *petitio principii*)—that is, global change is used to justify investment in certain kinds of research only for that research to argue that global change should be a primary concern of policy. Second, as Castree (2016, 14) has recently argued, the "raison d'être" of a truly geographical project may well be the intellectual spaces that allow us to "connect the epistemological 'fragments' of our discipline and thereby make visible life's socio-environmental fabric (where other disciplines fixate on the threads)." What other discipline has this kind of intellectual space?

These first two broader concerns regarding the contemporary focus of Physical Geography combine with further issues that apply to the academy more generally. Third, Physical Geography has become incredibly productive, if productivity is interpreted as academic journal articles (Gregory et al. 2014). When I was starting doctoral research (1991), Thomson Reuters' analysis tells me that the category "Physical Geography" was producing around 1,200 scientific articles per year. By 2014, this had risen to 8,603, a growth that is well described by an exponential curve (Figure 1).

Fourth, this productivity is not simply concerned with more publications, but also an increase in the speed with which data can be collected and analyzed and by which publications are produced in Physical Geography and across academia more generally. The ease with which we can now generate data in Physical Geography may have increased to the point that we have more data than ideas with which to

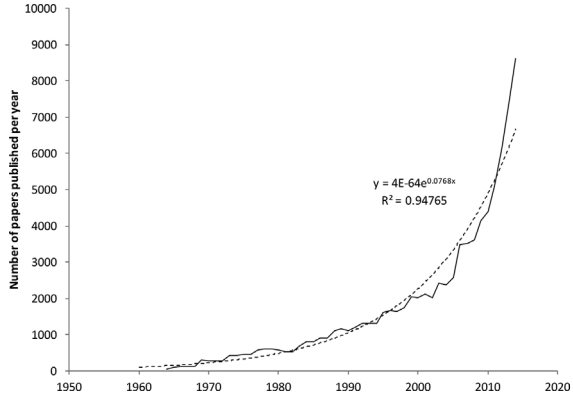

Figure 1
The growth in publication rates in Physical Geography based upon ISI analysis showing an exponential fit (dashed line).

[i] A possible example is the way in which UK glaciologists decamped from their groundbreaking focus on Alpine glaciers in the 1990s to the big ice sheets of Greenland and Antarctica in the 2000s.
[ii] Reynard et al. (2014) provide a good example.

interpret them. As a graduate student, I could measure about 200 data points per hour, to represent the morphology of the surface of a river channel. The most advanced laser scanning technologies can now measure upwards of 10^6 points per minute. There has been a revolution in information technology, making data analysis and writing quicker and easier. Internet and associated communications mean that reviewing and publishing work is more rapid.[iii] The journal *Nature* recently trialled a system where authors could speed up the peer review process by paying reviewers to turn articles around more rapidly.[iv] Open Access journals have pioneered Open Review where a paper is published on line, after some editorial checks, even before it is reviewed. Not only do we have much more to read, but the quality of what we are reading is becoming much less clear, and finding those papers that really do change the way we think has become harder. Writing and seeking citations seem to have been given more credence than reading (Halffman and Radder 2015).

Fifth, as with many other disciplines, physical geographers have had to contend with a growing accountability in their work. This has two distinct dimensions. The first (e.g., Pain 2014; Halffman and Radder 2015; Mountz et al. 2015) is the way in which this accountability manifests itself as audit. Institutions are quantitatively assessed with respect to one another, whether as a compulsory element of securing research funding (e.g., as in the UK), or as part of evaluations where the subjects of those evaluations have no say regarding their inclusion or not (e.g., World University Rankings). But this accountability is not just inter-institutional, it is increasingly intra-institutional, applied to departments, groups, and individuals, as universities seek to maximize their performance in the competitions to which they are exposed (Halffman and Radder 2015). The second is perhaps more insidious because it increasingly makes research the subject of political direction, undermining long-established faith that researchers and not politicians should decide research funding.[v] Research Impact has become a new unit of currency, where impact is defined as that work which benefits society (or more narrowly the jurisdiction of the government that is funding the research) and not academia (e.g., teaching or scholarship).[vi] The impact shift has paralleled the growth of Mode 2 science (Nowotny et al. 2001), in which curiosity-driven enquiry is increasingly replaced by "identified problems" as the motivation for research (Brewer 1999; Tress et al. 2005). As the need for research to have impact has developed, it is not surprising that policy-relevance and interdisciplinarity have co-evolved: interdisciplinarity has a clear logic (Barry et al. 2008) for problem-driven research where the supposed complexity and urgency of particular problems "precludes the luxury of assessing things from a unique perspective using a unique conceptual framework" (Aligica 2004, 68). Crucially, the shift towards Mode 2 science, which is rarely criticized (but see Barry et al. 2008 as an exception), raises fundamental questions regarding what and who is identifying the problems that then invoke the interdisciplinary analysis necessary for their resolution, as well as what and who then defines the impact that results.

In reflecting upon the above points, I found myself revisiting Harvey's (1972) critique of the Kuhnian account of scientific paradigm shifts. Harvey argued that the observation that there has been some kind of shift is perhaps less important than understanding why the shift come about, and in what way. Thus, if the focus of 21st-century Physical Geography is natural science–facing, problem-oriented research that must be held to account in certain ways, then how has this change in focus come about? Who is defining the kinds of research that have become the focus of Physical Geography, whether this is in terms of new ways of working or problems being defined? Harvey (1972, 114) wrote: "Immediately the question arises as to who is going to control whom, in whose interest is the controlling going to be, and if control is exercised in the interest

[iii] As a journal editor, it is now not uncommon to get requests for authors on the progress of their papers after only four weeks.

[iv] It is perhaps ironic that even this form of rapid publication has itself to be reported through a device symptomatic of the rapidity of modern science: the blogosphere. http://blogs.nature.com/ofschemesandmemes/2015/04/21/fast-track-peer-review-experiment-first-findings.

[v] The classic example of this in the UK is the Haldane principle, which originated in the early 20th-century. It distinguished between general research (to be decided upon by politician independent Research Councils) and commissioned research where there was legitimate political intervention in research priorities.

[vi] See http://impact.ref.ac.uk/CaseStudies/FAQ.aspx for example.

of all, who is going to take it upon themselves to define the public interest?" Whilst Harvey's particular focus was upon Human Geography and the social sciences, he made his thesis equally with respect to the natural sciences in describing:

> the harnessing of scientific activity, by a process of patronage and funded research, to the special interest of those who are in control of the means of production. The coalition of industry and government heavily directs scientific activity. This manipulation and control means manipulation and control in the interest of a particular group in society rather than in the interests of society as a whole. (1972, 111)

In Figure 2, I try to use Harvey's (1981) ideas to make sense of these observations, somewhat crudely, to explain the state that Physical Geography is in as a political economy (Tadaki et al. 2015), a discipline situated within and structured by the broader neo-liberal changes observed within the wider academy (e.g., Meyerhoff et al. 2011; Ball 2012; Mountz et al. 2015; Halffman and Radder 2015). If science has become progressively aligned towards producing commodities that can be monetarized (e.g., Mirowski 2011), and this alignment is increasingly used as the hallmark of "good science" (e.g., its impact on economic growth), then it makes sense to see the practice of science as part of a political economy and to subject it to such analysis. My entry point is the academic system, rather than the multitude of other sites where science is practiced, reflecting my interest in a discipline, Geography, whose science is dominated by Universities. I conceive of this system as involving two entities, Labour (academic physical geographers) and Resources (research questions and finances), both needed to fuel the means of production so as to produce a Surplus. By measuring, analyzing, and writing, physical geographers produce scientific articles. The latter are a form of Surplus because scientific articles are generally framed as an excess of knowledge over what we know already, that is, in terms of their originality and significance. Universities, who own the primary means of production (Labour), have increasingly required articles to be the key output of University research activity to the detriment of other forms of academic publication (Woodward 2015; Keighren 2016), not least because of the growth of an associated audit culture (Pain 2014; Rogers et al. 2014; Halffman and Radder 2015). There is, then, a symbiotic relationship between Labour and Surplus: owners of the means of production need Labour (academics) both to produce the Surplus (scientific articles) and also to consume it (notably to cite them), not least because certainly in Physical Geography, scientific articles are rarely read beyond the academy.

Under the assumption that the driver of the system is the need to maximize Surplus, owners of the Means of Production, University authorities, find themselves in competition, whether for Labour (e.g., hiring of those academics most able to produce Surplus) or Resources (e.g., funding). They may also maximize their Surplus by improving the efficiency of the Means of Production through ever-greater levels of intervention (e.g., targets, mentoring) (Halffman and Radder 2015; see Meyerhoff et al. 2011; Mountz et al. 2015). Whilst the history of the

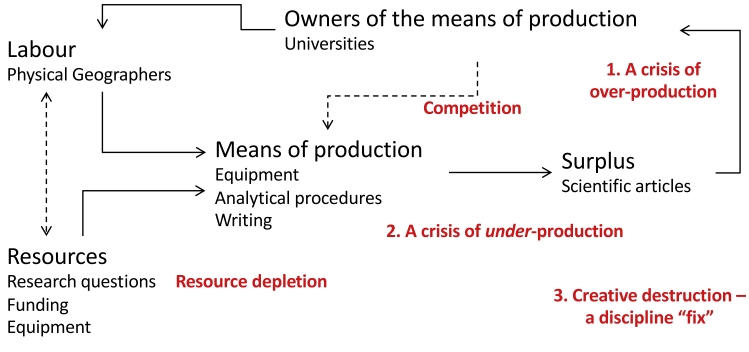

Figure 2
A political economy of Physical Geography in the 21st century.

labour movement did much to address the alternative to efficiency, that is the lengthening of the working day, the academic community (i.e., Labour) has continued to provide an additional and substantial subsidy through those academics who work many more than their contracted number of hours.

Following Harvey (1981), the result of the growing rate of Surplus is the first of three crises, one of over-production: we are now producing many more scientific articles than it is possible to read (Figure 1). This is partly sustained by the fact that we cannot only access articles more easily (on-line) but also search their content without having to read them. It explains the apparent paradox that we are not only producing more, we are also citing more. However, I argue that it threatens the necessary symbiosis between Labour as producer and Labour as consumer—unless the owners of the Means of Production can make sure that the fruits of its Labour are read (and cited) more than others. Coping with the crisis first involves reinforcing competition, such as through the invention of new ways of diffusing scientific articles that attempt to get certain articles read ahead of others, for example through use of social media such as Twitter, LinkedIn, Facebook, even ResearchGate (see Beven 2016 for a discussion of this in hydrology). The end point is no longer the publication of an article but rather the work Labour must do to get their article read (enable access, blog, tweet, etc.). Reinforcing competition is also achieved through the progressive reinforcement of accountability by qualifying (e.g., journal hierarchies through impact factors, rejection rates, etc.) what kind of Surplus that Labour should produce—that is, further intervention in how Labour is implicated in the Means of Production (see Pain 2014). Certain Surplus has become better appreciated than others, and some kinds of Surplus (e.g., inter-personal relations, new collaborations, etc.) receive no credit at all (Pain 2014; Mountz et al. 2015). In addition, there is the progressive extension of the Means of Production into the sphere of Resources, and the whole academic industry developing within universities and learned societies (e.g., grant writing workshops) concerned with improving Resource availability (e.g., grant success rates). Rather than funding being the input and the article (or even the book) the end point, Labour and Resources become strongly coupled, even interchangeable, as successful grant applications become increasingly an end in themselves. Grant earners get bought out of all activities other than research, at best allowing the owners of the Means of Production to increase investment in more Labour, at worst transferring workload to those who have not obtained funding, leaving them "slogging in the trenches" (Mountz et al. 2015, 1248).

However, there is a second crisis that follows from the crisis of over-production, which is more fundamental and which without some kind of response, leads to a condition of under-production. This defines Resources more widely and shows them to be finite. Given the growing speed with which high quality research can be undertaken and published, it is possible that the research questions that are needed to sustain research within a discipline, perhaps the key Resource, are being produced at a slower rate than they are being answered. A condition of disciplinary scarcity then arises. Physical geography is a relatively mature discipline, as it is primarily concerned with the surface and near-surface processes that have been relatively accessible in terms of measurement for some decades. Even if innovation in the Means of Production allows new questions to be asked or old questions to be considered in new ways, if the rate of Surplus production is faster than the rate of innovation then original research questions will become progressively more scarce, and a crisis of under-production will arise.

Following crises of over-production and under-production, it is my thesis that the 21st-century state of Physical Geography can be explained as a third crisis, not unlike Harvey's spatial fix (Harvey 1981), but in this case disciplinary. Faced with a condition of Resource scarcity, in relation to research questions, two choices follow. The first is to seize interdisciplinary opportunities, those questions at the margins of disciplines that are there simply because they have traditionally been difficult, or where the "hard work" of framing a question has been already done by those with a problem that needs to be solved. It is not surprising that physical geographers have found it valuable to sign up to the interdisciplinary agenda because it is precisely this agenda that provides new research questions around which to develop a focus and often wider Resources (e.g., funding) to support them. The second option is to create the critical mass of researchers necessary to seize other kinds of questions, perhaps those seen traditionally as in the domains of other disciplines, even building new

communities with little or no cohesion to traditional geographical institutions. Such changes may be interpreted at once as destructive (see Clifford, 2002; Richards and Clifford 2008) for the ways in which they force physical geographers to develop new alliances outside of the discipline, an outward valence rather than an inward cohesion (Clifford 2002); yet creative for the ways in which they generate new resources (e.g., research domains or research hypotheses) to be exploited. It is my view that physical geographers have had no choice but to embark upon a path of creative destruction given the wider evolution of the academy. We increasingly find ourselves with a strange sense of scholarship, one where we: "self-identify as broad social or environmental scientists rather than 'narrow' geographers" but "rely on the continuation of something called 'geography' in the universities for our employment" (Maddrell 2010, 151).

From rapid science to slow science and the thesis of Isabelle Stengers

Whilst Figure 2 provides a reasonable but partial analysis of the state of Physical Geography, the second part of my argument becomes more normative. That is, what form should a more critical and reflexive Physical Geography take? What should the revolution look like? I believe that the forces associated with Figure 2 are sufficiently embedded and strong that resistance to them is going to be a challenge. One way to interpret those challenges comes from Stengers' (2013) *Une autre science est possible!* and as her argument is normative, I have used a reading of her work[vii] as a basis for my own argument. Others have reached similar readings (e.g., Caracao 2013). Stengers is concerned with what science should become, based upon a critique of the practice of science, broadly defined, which leads to the notion that we can talk of a political economy of scientific knowledge. In response to this critique, and building on earlier ideas (e.g., Stengers 1999), she advocates resistance to the notion that scientific knowledge should be subject to a particular authority (Stengers 2013, 15) and to the "unhealthy character of a science, unable to nourish an amateur milieu, and (a kind of science) which is today sorely lacking" (this author's translation of Stengers 2013, 26). What is needed is a ralentissement, captured in a quote from one of Stengers' earlier papers: "How can we present a proposal intended not to say what is, or what ought to be, but to provoke thought, a proposal that requires no other verification than the way in which it is able to 'slow down' reasoning and create an opportunity to arouse a slightly different awareness of the problems and situations mobilising us?" (Stengers 2005, 994), the kind of interruption advocated by Meyerhoff et al. (2011). Finally, she argues that this ralentissement can only come about if science can escape the constraints of its current practice, through changing the relative position of scientists with respect to their science. The formal separation of the researcher and the researched has to be replaced with a more nuanced relationship, that allows the researched to inform, more directly, how it should be researched, and where the power of experience[viii] is re-recognized and re-appreciated.

Science in the 21st century: A critique

Stengers' (2013) critique begins with observations regarding the process of scientific abstraction. She argues that most science proceeds by identifying solutions to pressing problems through abstraction, one that purposely ignores other kinds of solutions (Stengers 2013, 11). She uses the example of genetically-modified crops, which may indeed be the solution to world hunger if a series of other solutions to the problem (e.g., the redistribution of wealth; changes in western consumption patterns) are assumed out of consideration. Mirroring my description above of how the study of/with people is rarely a part of physical geographical enquiry, Stengers critiques science as a process of "purification," where good questions become those that can be "cleaned up" by abstraction out of local context (see also Richards and Clifford 2008): "Ce qui signifie en retour que la question est une 'bonne question', s'adressant à une dimension du

[vii] Stengers (2013) is written in French. I develop my argument in English but leave in the relevant French phrases, extracted from the book, as evidence to support my translation and interpretation.

[viii] It is perhaps interesting that "experiment" in English translates to "expérience" in French reflecting the ways in which the Anglophone notion of "experiment" has become more formalized and restrained than the wider linguistic origin of the term. This formalization is very much the subject of Stengers' critique.

phénomène étudié qui est effectivement susceptible d'être 'débrouillée', et donc d'être attribuée à ce phénomène indépendamment de son milieu" (Stengers 2013, 64).

However, Stengers' criticism is not of abstraction itself, because she sees this as the strength of scientific enquiry and the basic of scientific proof (Stengers 2013), but more the manner by which abstraction, or purification, is undertaken. Her argument is that abstraction has become too sensitive to what particular fields of academic research define as admissible questions, and so to the academy. Scientists argue that other questions cannot and should not be considered because they will waste time:

> D'une manière ou d'une autre, explicitement ou non, les scientifiques apprennent à définir comme 'non scientifiques' des questions qui n'appartiennent pas à leur 'sillon', y compris des questions qui importent d'autres champs académiques, parce que, du point de vue de leur discipline, s'y intéresser signifierait une 'perte de temps'. Ce serait cédé à une tentation à laquelle ils doivent résister s'ils veulent se comporter en 'vrais scientifiques'. (Stengers 2013, 101)

This focus on some questions at the expense of others is only made possible by developing insensitivity to the reality that is the supposed focus of the question being posed: "Interroger l'étoffe qui fait le 'vrai chercheur' à partir d'une telle hypothèse, c'est interroger une construction au pouvoir redoutable car elle ne déforme pas la réalité mais exige une insensibilité déterminée aux questions que pose cette réalité" (Stengers 2013, 32).

The need for resistance to this lack of sensitivity

The second element of the critique argues for a resistance to this insensitivity to reality, and more particularly to those who argue for this insensitivity. Stengers constructs this as an "economie de la connaissance" in ways not dissimilar to the political economy of Physical Geography that I present above. It has three elements: (1) scientists; (2) scientific communities; and (3) the wider society within which science is found.

Stengers' resistance in relation to scientists is not a resistance to the notion that scientists should exist to make a contribution to the world in which we live. Rather, it is a resistance to the peculiar privilege that some scientists seek where, on the one hand, they wish to maintain their separation from that which they wish to study, to protect their independence, but on the other hand they wish to contribute to the reconfiguration of the societies within which we live through the results of their research. They want a double power, both to be independent *and* to make interventions because they claim privileged knowledge as they are independent: "Les scientifiques n'ont jamais été innocents. La plupart ont pris une part active, ou non adhéré, à la construction permanente d'une frontière asymétrique, qui protégerait leur autonomie, les défendrait contre les intrus, mais leur permettrait de quitter librement leurs espaces protégées pour participer à la redéfinition de nos mondes" (Stengers 2013, 128). She argues that this is understandable, a reaction of fear, because they imagine that by being perceived as independent, their interventions, supposedly unaffected by their personal bias, somehow have greater worth:

> . . . ils ne peuvent pas le dire en public, car ils craignent que si le public partageait leur savoir de la manière dont la science 'se fait', il perde confiance, réduise les propositions scientifiques à l'expression d'intérêts particuliers. 'Les gens' doivent continuer à croire à la fable d'une recherche 'libre', animée par la seule curiosité, à la découverte des mystères du monde . . . . (Stengers 2013, 12)

However, Stengers argues that ultimately, this position of authority is sustained by conformity to a model, one defined and policed by the scientific community through its own systems of peer review (in writing, in promotion). She argues that this conformity is based upon a brutal competition, of the kind implicit to the analysis in Figure 2: "les jeunes chercheurs.e.s, doctorant.e.s. et post-docs doivent accepter des conditions de travail proprement sacrificielles, une compétition sans merci. Ils sont censés serrer les dents : à la grande aventure de la curiosité humaine présentée aux enfants s'est substitué le thème d'une vocation exigeant un engagement corps et âmes" (Stengers 2013, 29). It is one that is not just academic but also personal, forcing academics to sacrifice their non-academic lives for academic ones (see Mountz et al. 2015 for similar arguments by geographers), through over-long working days and overseas travelling: "de passer les nuits blanches au laboratoire et de s'absenter lors des nombreux stages et déplacements à l'étranger que suppose la carrière

d'une chercheur" (Stengers 2013, 29). It forces scientists to conform, to grasp at opportunity and to be continuously flexible:

> ... la compétition pour la reconnaissance d'une 'excellence' qui est désormais condition de survie académique aura pour enjeu la ressource rare que constitue la publication dans une revue de rang A, et cet enjeu leur imposera de concevoir leur recherche à partir de ce que demande les revues et de se conformer aux normes qu'elles imposent: conformisme; opportunisme et flexibilité; telle est la formule d'excellence. (Stengers 2013, 52)

Stengers goes further and argues that resistance is needed not only to the working practices that have grown up around scientists and science, but also to the way in which scientists have become increasingly locked into the progressive re-orientation of scientific products towards industry and commerce (see also Mirowski 2011). This is one dynamic behind the impact agenda: the technical and methodological advances of industry sustain the rapidity of research; and the rapidity of research in turn produces those who have mastered those technical and methodological advances that industry then employs. Public funds, as reflected in the ways in which the impact of research is increasingly factored into their allocation, nourish this scientist-industry symbiosis: "Les chercheurs sont tenus à ce qui est une quasi-loi du silence à propos des liens que tissent leurs collègues avec l'industrie. Ils sont tous nourris par des subventions publiques pour contribuer à l'innovation industrielle ..." (Stengers 2013, 100). We arrive at a "tyranny of relevance" (Lave 2014, 509) in which research that is not commercially valuable is deemed to have little intellectual value.

Two potential problems follow. The first is that symbiosis leads to mutual dependence, with the theoretically separate interests of industry and research indistinguishable: "Une situation de symbiose implique des intérêts divergents, au risque permanent que les uns capturent les autres, créant un rapport simple de dépendance" (Stengers 2013, 98). The second problem is that we become unable to identify the unintended consequences of apparent scientific progress, how the "eggs" that the academic "chicken" produces might go on to be used, something that has to be of profound concern for any discipline related to human relationships with the environment: "En d'autres termes, la poule se doit de 'traiter de manière superficielle' ce qui ne participe pas directement à l'avancée de sa science et d'ignorer les questions qui pourraient la faire ralentir et hésiter, s'inquiéter de ce qu'on va faire de ses œufs" (Stengers 2013, 101). It is for this last reason that we see the basic societal argument for a need for a science that is more reflexive, able to think beyond the confines of the simple cause-effect models that it so often espouses. For Stengers, this is about a need for "slow science."

Towards a slow science: The need for ralentissement and the repositioning of the researcher with respect to the researched

To understand the nature of slow science, Stengers makes it clear that there are two things that it is not. First, it is not a call to re-establish the privilege that scientists are left alone to do what scientists think is important: "La lenteur n'est pas une fin en soi et elle ne résume pas à l'exigence 'qu'on nous laisse tranquille' de chercheurs qui continuent à se penser en droit de bénéficier d'un traitement privilégié" (Stengers 2013, 80). Being left alone would attend to neither the fundamental question of how scientific questions are posed, notably given wider evidence of the ways in which scientific practices are socially informed, nor would it make scientists think through the consequences of the fruits of scientific enquiry. Second, it is also not a call to restate the traditional ideal of free and non-partisan research as all science is motivated in part by private interests: "ce que l'économie de la connaissance est en train de démanteler est la possibilité d'une recherche qui ne soit pas directement au service d'intérêts privés" (Stengers 2013, 91).

Reading Stengers, I found little argument that ralentissement or slowing down is just about taking more time, notwithstanding wider concerns that science (e.g., The Slow Science Academy 2010; McCabe 2012) and geographical enquiry (e.g., Keighren 2016) needs to be slowed. Rather, Stengers' notion of ralentissement points to a series of actions that I have identified in Table 1, which are less about doing science slowly (see also Mountz et al. 2015), and more about re-establishing the kinds of situations that scientists can place themselves in relationally with respect to what they study, that can lead to the combination of curiosity, creativity, and innovation that makes science so exciting. In this sense, the ralentissement may well

Table 1
Interpretation of Stengers (2013) as actions for making a slow science

Action	Commentary	View of Stengers
1. Acts of disobedience	Reflecting wider ideas regarding the value of controversy in scientific research, the role that controversies can play, notably where they allow a scientist to disobey conventional wisdom, and to raise other kinds of questions. Stengers uses the example of GM crops where environmental safety concerns open up wider questions regarding global food security, spatial justice, and dominant accounts of biotechnical solutions to food shortage.	*On peut dire, de ce point de vue, que les actions de désobéissance civile ont créé, et maintenu ouvert, un espace de production d'intelligence collective à laquelle je me définis d'ailleurs comme redevable.* (Stengers 2013, 88)
2. Repositioning of the scientist	A refocusing of the scientist: away from their normal communities of practice (as scientists) and the abstraction of investigation out of the milieu of which it is normally a part; and towards those who ask different questions or bring other kinds of understanding, normally excluded from scientific enquiry.	*Se situer n'a rien à voir avec le point de vue qu'offre Google Earth, où l'on voit la Terre entière, puis on peut situer sa ville, sa rue, sa maison. Être capable de se situer, de situer ce qu'on sait, de le lier activement aux questions que l'on fait importer et aux moyens mis en œuvre pour y répondre, implique d'être redevable à l'existence des autres, de celles et ceux qui posent d'autres questions, font importer autrement une situation, qui peuplent un paysage sur un mode qui en interdit l'appropriation au nom de quelque idéal abstrait que ce soit.* (Stengers 2013, 49)
3. Readmission of consequence	The readmission into scientific enquiry of those for whom scientific research has consequence, ones normally removed via scientific abstraction.	*Il concerne en effet la capacité des scientifiques à participer à la recréation d'une fiabilité qui implique l'entrée en scène de plein droit tous ceux que les conséquences d'une innovation peuvent concerner, de tous ceux qui sont porteurs de préoccupations que le mode d'abstraction des scientifiques ignore.* (Stengers 2013, 108)
4. New ways of working	Dismantling of the normal ways of working that strive to exclude judgement from scientific practice.	*Rien dans leur formation actuelle ne prépare les chercheurs à ce genre de participation, qui devrait exclure tout jugement superficiel, toute connivence entre ceux qui savent. C'est bel et bien un démantèlement des habitudes du professionnel qui est requis.* (Stengers 2013, 108)
5. The "art of consultation"	The need to be able to engage with the subjects of our research that gives them the power to make us think differently about them…	*C'est pourquoi il vaut mieux parler 'd'art de la consultation' que de 'libre débat' car il ne s'agit pas d'évaluer les mérites des différentes positions, mais de donner à la situation qui rassemble le pouvoir de faire hésiter chacun quant à la manière dont il formule sa position.* (Stengers 2013, 110)
6. The "right of reply"	… and to evaluate what it is we think we need to know about them, that is, to give them a "right of reply" to the definition of the questions that we pursue.	*On pourrait décrire cette pratique comme une très particulière opération 'd'enrôlement' des phénomènes … En effet, il ne s'agit pas d'obtenir d'eux des réponses aux questions que les scientifiques se posent, mais aussi, et même d'abord, d'obtenir des réponses qui vérifient la pertinence, pour le phénomène, de la question elle-même!* (Stengers 2013, 127–128) *"… non certes leur droit de dicter aux scientifiques comme ils veulent être décrits, mais leur capacité à évaluer la pertinence de la mise en rapport qui leur est proposée."* (Stengers 2013, 68)

be spatial or even scalar (e.g., when we bring other disciplinary perspectives into our work). It is a relational repositioning that helps us to challenge what it is that we think. At base, slow science is about a commitment more to the subjects of our study and what is researched, and less to the forces that shape contemporary academic environments (Figure 2). The approach being suggested is profoundly empirical (but not in a formal sense) because it is grounded in the power that experience has to challenge what it is we think we know and to resist those forces that might prevent us thinking

otherwise. Thus, slow science is also a direct challenge to certain interpretations of Mode 2 interdisciplinary science because our experience(s) should force questions about what the problems are, which ones matter, how they should be studied, and with what consequence, as much as they should provide definitive answers to questions posed by others.

Slow science as "geographical expedition"

If the essence of slow science is a relational change in the practice of research, the question becomes how could it and should it come about? There is no straightforward response to this question because, following from the historical and structural account summarized in Figure 2, there is an element of "lock-in" to existing ways of working: more senior physical geographers may have a sufficiently sound position in the subject to take risks, but they are more likely to be locked into a Mode of Production whose existence needs to be sustained; more junior physical geographers are constrained by the serious consequences of not conforming to what the system expects (e.g., in relation to tenure, promotion, salary). However, if the change is relational, one way to think about it is to think through the ways in which a material relational repositioning of physical geographical enquiry can enable slow science.

To illustrate this argument, I draw upon an example of a relational change associated with an advocate of the quantitative revolution and theoretical geography in the 1960s. Bunge (1979) provides an autobiographical account of what became known as the "Detroit Geographical Expedition and Institution." When I read Stengers (2013), and identified the actions in Table 1, I was struck by the parallels with Bunge's (1979) personal account of how he dissociated himself from the axioms of theoretical geography and associated himself with a very different kind of community. This was a community that theoretical geography was at pains to say something about but which, through a material engagement with that community, slowed down Bunge's conventional reading, unsettled his accumulated wisdom, and caused him to think in a markedly different way.

In Table 2, I interpret Bunge's (1979) personal account through the ideas in Table 1. He describes how much of what he did involved acts of disobedience, whether theoretically in relation to the state of the United States in the 1960s, or more specifically through the series of academic moves he took until he left academia altogether. His motivation to "disobey" arose from a physical repositioning of himself with respect to the subjects of his research, one that forced him to readmit into his research those for whom the research mattered. As Bunge reflected, he had to "sight" rather than "cite" (Bunge 1979, 172). The methods needed were increasingly defined by what he was studying rather than a supposed academic value of the methods themselves. He was forced to find new ways of consulting with what he was researching, one where the subjects of his research were given a much stronger "right of reply."

Bunge, and co-workers, conceived their physical repositioning as a geographical expedition, one as concerned with teaching as it was with research (Heyman 2007). At first glance, the term "expedition" is unfortunate because of its association with the utilitarian purpose of bringing the world under the sovereignty of science, notably in the 19th century (Withers 2007)—and the association of 19th- and early 20th-century academic geography with the justification of sovereign expansion more generally (Couper and Ansell 2012). The kind of expedition that Bunge advocates and which I support here is not a colonizing one. Rather, it is a means of placing oneself in a situation where that situation can speak back, engendering the kind of slowing down of reason that Stengers advocates, and which raises within us a slightly different sense of awareness of what we think it is we know, and hence what it is we set out to do (Stengers 2005).

Expeditions to Pickering and Uckfield

In the final section of this paper I want to interpret my own engagement in a project, *Understanding environmental knowledge controversies,* within a wider interdisciplinary research programme Rural Economy and Land Use (RELU), as a kind of geographical expedition of the sort that Bunge envisages and which (at least in retrospect) can be interpreted as a kind of slow science. The project has been described widely elsewhere (e.g., Landström et al. 2011; Lane et al. 2011). In brief, the project was an experiment in participatory

Table 2
The interpretation of Stengers' (2013) actions through a reading of Bunge (1979)

Stengers' actions	Illustrations from Bunge (1979)
1. Acts of disobedience	*I threw myself into the peace movement when it was supported at that time by less than four percent of the American people. Having lived through McCarthyism, I fully expected to be in a concentration camp within a year. (Bunge 1979, 170)*
2. Repositioning of the scientist	*I went to Chicago for the Martin Luther King demonstrations in 1966. While there I stayed in the black ghetto in a hotel at 67th and Stony Island. (Bunge 1979, 170)*
3. Readmission of consequence	*It is easier to rouse working class mothers and fathers to maps showing the region in which children have shrunken heads due to lack of protein ... the actual human misery the money transfers cause is more difficult to perceive than the flows. (Bunge 1979, 172)*
4. New ways of working	*The campus geographers tend to separate theory from practice. They read too much and look and, often, struggle not at all. They cite, not sight ... In science the methodology does not endorse itself. Only the substance recommends the methodology. (Bunge 1979, 171)*
5. The "art of consultation"	*I worked with a young black woman, a union worker, and former peddler on 43rd Street, Rene Spears. She hated my concern about the three dimensionality of the species and our need to protect the world's children. Her people's children were starving. (Bunge 1979, 170)*
6. The "right of reply"	*Another young black woman, Gwendolyn Warren, from Fitzgerald in Detroit ... was teaching me similar lessons, filled with hatred toward me because I did not notice the children being murdered by automobiles in front of their homes or children starving in front of abundant food. "Immediacy" was their cry, "To Hell with the World"! (Bunge 1979, 170)*

knowledge production (or co-production). The project sought to understand the ways in which controversies surrounding scientific knowledge, in our case in relation to flooding, might be harnessed and worked with so as to generate the new collective competencies (in members of the public, in scientists, in action groups) that might move a problem on. This was done through an experimental method using "environmental competency groups," one in each town. This method had five elements (see Lane et al. 2011 for more detail): (1) a focus on actively producing new knowledge relating to flooding in the two towns rather than just discussion and debate; (2) a collaborative process involving academics and community members, as well as "things" (e.g., computer models, maps, measurements), the latter providing a critical role in mediating between members of each group and with the place within which the groups were working; (3) a sustained engagement over 12 months with a "matter of concern," i.e., flooding, that can bring into sharp focus the prevailing framings associated with flooding and the people and things bound to it, so mobilizing and enabling those people and things otherwise excluded from flood management; (4) the acceptance that expertise regarding flooding is deeply distributed amongst experts (members of the public and scientists) rather than being confined to those certified as such (i.e., academics); and (5) an approach that did not seek to represent any pre-existing constituency with regard to flooding, but rather that sought to constitute a new public capable of intervening in flood risk management.

The notion that we embarked upon geographical expeditions is reflected in the material ways in which elements (1) through (5) were put into practice. We spent sustained periods of time in both towns, Pickering between 2007 and 2008, and Uckfield between 2008 and 2009, holding competency group meetings in each (six evening meetings per expedition, each 3 to 4 hours long), jointly collecting data with local members, attending additional inter-meeting events (e.g., reading groups), and undertaking inter-meeting interviews with local members as well as with other key stakeholders with an interest in flooding in each place. In retrospect, we should have lived in each town for a period of time, something that may appear to be impossible given current University schedules, but which some disciplines at least (e.g., Anthropology) are able to sustain. But the expedition was not simply a "field trip." Table 3 attempts to show some of the shared characteristics with Bunge's notions of a geographical expedition (Table 2) and the ideas of Stengers summarized in Table 1. The expeditions themselves grew out of a project that advocated a very different kind of interdisciplinarity to that sought by the programme

Table 3
The interpretation of Stengers' (2013) actions through my own expeditions to Pickering and Uckfield

Stengers' actions	**Illustrations from the *Environmental Knowledge Controversies* project, a project that ran from 2007 to 2010 in the 2004 to 2013 Rural Economy and Land Use programme, an initiative straddling three UK research councils (Natural Environment, Economic and Social, Biotechnology and Biological) and whose origins can be traced back to the UK government's response to the (rural) foot and mouth crisis of 2001 to 2002.**
1. Acts of disobedience	Getting the project funded: Whatmore (2013) explains the challenges associated with getting a project funded where we (the applicants) clearly had visions of different kinds of interdisciplinarity to those of the civil servants associated with managing the research programme (the PMG, Programme Management Group). For instance, in response to cuts to the project funding, the PMG refused to countenance our proposition to focus on floods and insisted that the entire project be re-reviewed. They wanted us to avoid collecting new data and focus on the analysis and interpretation of existing data.
2. Repositioning of the scientist	The realignment of the project scientists away from the hydrological community and towards those of the communities with whom we were doing the science: Landström et al. (2011) describe how the two project scientists (Stuart Lane and Nick Odoni) were forced to turn away from their conventional network of scientific practice (based upon a particular kind of hydrological model) to the network of academics and local people created by the project, and where the hydrological models used were place specific, hybrid, and largely unpublishable.
3. Readmission of consequence	In both expeditions, we were dealing with communities living with flooding and its consequences. In the Pickering case, we advertised for community members to work with on Friday 22nd June 2007 and the major flooding began in the town on Monday 25th June 2007. Consequence was materially readmitted and the approaches we developed became sensitive to wider impacts (e.g., the importance of storing water, but not in a way that would damage the local tourist railway; the need to show attention to the effects of flow attenuation by upstream storage for the relative timing of flood waves downstream, and hence downstream flood risk).
4. New ways of working	Central to the expeditions was the redistribution of admitted competence: Lane et al. (2011) describe this as an experiment in radical scientific method because, rather than restricting engagement with communities to public understanding or public deliberation of extant scientific findings, the science that we did was co-produced. Through a series of working meetings we scoped the issues; together developed and used a hydrological model; and discussed the results leading to: (1) a public exhibition in Pickering, and (2) proposals for distributed instream and floodplain interventions to attenuate river flow in Uckfield.
5. The "art of consultation"	A critical challenge for the academics involved in both expeditions was overcoming the perception of academics as part of the establishment (e.g., funded by the Environment Agency, even though we weren't) and as experts. We were both chastised for solving the wrong problems. As one community member put it: "And particularly about flood science. My concern is why when so much money is poured into research and mapping etc. ... it doesn't get better. It has to be possible to make something better, to actually solve problems that people need solving and so this is my concern and that also a concern of this project, which I have really bought into it.". We had to learn to recognize the deep and sophisticated (if not certified) knowledge of flooding held by community members (see Lane et al. 2011).
6. The "right of reply"	We attempted to go into both locations with a minimum of prior framing (the only framing was that flooding should be a matter of concern), and then to shape the research questions that we wished to ask through the negotiation and deliberation by all members of the environmental competency groups.

that funded it. It required a continual articulation between what we wanted the project to achieve and what the programme was increasingly emphasizing, such as research impact (for further details, see Whatmore 2013). Those scientists involved in the project were forced, through direct contact with the matter of concern, to turn away from their normal working communities and to focus on those who lived in the place in which they were based (see Landström et al. 2011). Doing so meant that the local consequences of knowledge produced were brought to the fore. Much of this was achieved through a very radical way of practicing science which avoided the supposed separation of the scientist from the subjects of their research, where the latter were actively involved in both deliberating on the questions that should be asked and developing the tools (e.g., mathematical models) to answer them (see Lane et al. 2011). We also had to learn to consult in new ways, to come to terms with the inadequacies of our own conceptualization of flooding problems, and ultimately to become sensitized to the questions posed by those for whom flooding was a matter of concern.

Since the project ended in 2010, and being somewhat distant from some of the follow-up work and studies that have resulted, I can now reflect more critically upon what the project achieved. At one level, the project did produce a number of research publications (e.g., Landström et al. 2011; Lane et al. 2011) that certainly fulfilled RELU's objective in their 2004 call for proposals of developing: "... integrated perspectives on problems and to understand complex processes and issues involved in achieving sustainable rural development ..." through "... an approach that effectively combines research staff, methods and perspectives from social and natural science disciplines." Perhaps reflecting RELU's emphasis, these publications have appeared almost exclusively in social science–facing journals. If judged in terms of conventional natural science, the project was a failure. We have not published any kind of new (commercial) modelling tool. The hydrological/hydraulic modelling work done for Pickering and Uckfield, which was hybrid, has not been published. The only real publication based on the modelling involved collaboration with a different research group, in a different location, to test the effects of log jams on downstream flood risk (Dixon et al. 2016). We produced three reviews (two book chapters and a journal review article) prompted by our experience with our modelling, none of them addressing the model approach/results themselves and none of them eligible in the kinds of accounting systems that UK universities now use (e.g., publication of primary research results in Physical Geography or natural science journals with a high Impact Factor). With reference to Figure 2, the project produced very little natural science Surplus, at least in the way Surplus has come to be valued in the University audit culture of the 21st century, and this despite a significant expenditure of Labour and Resource.[ix] Producing such publications might have been possible and perhaps, had I been an early-career researcher, critical to some of the disciplinary and sub-disciplinary modes of evaluation I might be subject to (e.g., Research Council grant applications, Research Excellence Framework). But such production would have required the science that we did to be discipline-facing, i.e., based around the state of hydrological modelling as it currently is, and not subject-facing, i.e., addressing the concerns of the flood risk communities with which we worked. Whilst the flood risk management solutions that our project developed in Pickering, in particular, went on to be implemented and nationally recognized and to sustain debates over a more catchment-based approach to flood risk management,[x] there was no Surplus in the natural science terms that now dominate the academy. My point here is that following the kinds of principles that Stengers (2013) advocates (e.g., Table 1) is not just about an epistemological shift regarding the nature of the knowledge production process, but also about a deeper ontological shift in the nature and purpose of scientific enquiry, one that challenges directly what research in the academy has become. The challenges facing early-career researchers are such that the risks of pursuing such an ontological shift are serious, even if an early-career researcher may be highly motivated by the excitement that comes from doing something differently. More senior researchers, better able to cope with the negative consequences of this risk, are more likely to be locked into the kind of system shown in Figure 2, that is they have become closer to being an owner of the Means of Production and the management of the crises that the system is producing. Yet, it is probably those of us who are more senior that are most able to support and to sustain the wider changes in working practice (e.g., Pain 2014; Rogers et al. 2014; Mountz et al. 2015) and to value the kinds of geographical imagination (e.g., Castree 2016) that would provide the safe and secure University context needed for the ralentissement that Stengers (2013) advocates.

But, labelling our contributions as a failure in terms of 21st-century Surplus would hide a much deeper set of challenges that this work threw up. Landström et al. (2011) described how the natural scientists in the project (my colleague and I) turned away from our academic research networks and towards the local communities with whom we were

[ix] The project was funded before application of the UK's Full Economic Costing of research projects. The direct costs of the project allowed employment of a project administrator and three post-doctoral fellows for three years, associated travel/subsistence costs, and a fourth post-doctoral fellow to assist with dissemination of project results during the final year.

[x] See http://www.independent.co.uk/news/uk/home-news/uk-flooding-how-a-yorkshire-flood-blackspot-worked-with-nature-to-stay-dry-a6794286.html.

working whilst developing the hydrological models used in the project. This repositioning was not just about model making, it was about a deeper shift in the locus of our entire research activities. Pickering at first, then Uckfield, took over. My other research projects fell behind and suffered, and other commitments could not be properly honoured, because of the ways in which we had become imbricated within the day-to-day rolling on of those living with flooding risk in these two towns. I could not find more time, but there was a sense in which these expeditions were "eventful" time per Meyerhoff et al. (2011) where, through a process of collective innovation, we instigated a wider debate about how to do flood risk science and to address flood risk problems. It was the readmission of the subjects into our work as scientists that shifted us towards what Mountz et al. (2015, 1245) call a more "care-full" scholarship. In that sense, I have concluded that a lack of a natural science Surplus simply did not and does not matter.

Conclusion

To conclude, I want to return to some of the lessons of David Harvey (1972) where he argues that in a normative account of the world, the goal must be not to produce largely correct accounts of that world (in his case, Von Thünen's theory of urban land use, where competitive bidding leads to the urban poor moving where they can least afford to live), but rather to identify precisely those conditions that might cause the largely correct models to be incorrect (i.e., in his case, those conditions that might allow competitive bidding to be replaced by a socially controlled urban land market). I find this to be precisely the moment when as a scientist I make some kind of progress in my understanding. In relation to our work as physical geographers, conceptual statements or quantitative predictions are not the end point. Rather, our challenge is to reorient attention away from pursuit of (largely) correct (but uncertain) predictions of how the environment is changing (or will change). We need to engage much more closely with two challenges. The first challenge centres on those points that don't fit the model and so cause us to look slightly more closely at why, slowing down our reasoning, to the point at which other explanations might become meaningful and new questions important. It is right to be wrong (see Beven 2016). The second is to become more willing to address the plethora of constraints, contradictions and feedbacks, political, socio-cultural, economic, and environmental that need to be challenged such that those predictions become invalid. I sense that Physical Geography contributes significantly to the first challenge through what might be called "normal science," notwithstanding what the University system has become and the pressure for what we do to be impactful, and the creative destruction of Geography that has been needed to do this. But the contributions to the second challenge remain the exception to the norm, not least because sustaining the creative destruction of modern Physical Geography requires the reinforcement of a model of scientific enquiry that separates scientists from the subjects of their science. Critical Physical Geography (Lave et al. 2014) is exciting here. But, the arguments of Stengers (2013) and the parallels with Bunge (1979) seem so interesting because they remind us of the power of the subjects of our research, human or non-human,[xi] to speak back, if we allow them to do so, and hence engender that slightly different understanding of the world around us, one that makes the curious practice of science so creative and exciting. Even if it is unlikely to break completely the model shown in Figure 2, a 21st-century vision of the (physical) geographical expedition, that can be sensitive to the serious issues that notions of expedition imply for our research practices, merits careful attention.

Acknowledgements

Sarah Whatmore introduced me to the world of Isabelle Stengers and opened my eyes to many of the ideas in this paper. The support of the UK research councils' Rural Economy and Land Use programme between 2007 and 2010 is kindly acknowledged. The very kind invitation of Rebecca Lave to speak at a Critical Physical Geography workshop before the Chicago AAG in April 2015 forced me to bring my thinking on Stengers together, aided by a later workshop under the umbrella of the Conférence Universitaire de la Suisse Occidentale, held in Lausanne in November 2015. Both Christian Kull and Marc Tadaki drew my attention to valuable literature used in this paper. Three reviewers provided very helpful comments on an earlier draft of the manuscript and Ellen Randall provided very careful proof

[xi] I use the term "non-human" here to capture the much richer range of things that influence the practice as a science, that is, of being empirical in the broadest sense.

reading and correction. I hope that my revisions have produced, particularly for the reviewer who described themselves as a "senior physical geographer," a piece that is more nuanced and less offensive than the original version.

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