

THESIS FOR THE DEGREE OF DOCTOR IN ENGINEERING

In search of maritime energy management

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Skäms inte för att du är människa, var stolt!
Inne i dig öppnar sig valv bakom valv oändligt.
Du blir aldrig färdig, och det är som det skall.

Tomas Tranströmer

Abstract

This thesis deals with a transdisciplinary research project that concerned the implementation of an energy management system standard – ISO 50001 – in two shipping companies. This project was partially successful insofar as we implemented the standard in one of the two companies. It is a thesis by papers: the introductory text discusses the research process, to explore in hindsight how transdisciplinarity was made possible. The appended papers bring up issues that were suitable to fit into such a structure during the course of the implementation project, and after. These issues are related to various aspects of “maritime energy management”. The papers can be read in any order, with or without reading the introduction first, but my intention is that you continue reading, page by page, beyond this abstract and onwards until recommended otherwise. On the other hand, as Umberto Eco wrote: “books are not made to be believed, but to be subjected to inquiry.”

Keywords: maritime energy management, action research, transdisciplinarity, energy efficiency gap

Appended papers

This thesis is based on the following appended papers:

- 1 **Johnson, H.**, Andersson, K. (2014). Barriers to energy efficiency in shipping. *WMU Journal of Maritime Affairs*. doi:10.1007/s13437-014-0071-z.
- 2 **Johnson, H.**, Johansson, M., Andersson, K. (2014). Barriers to energy efficiency in short sea shipping: an action research case study. *Journal of Cleaner Production* 66, 317–327.
- 3 **Johnson, H.**, Styhre, L. (2015). Increased energy efficiency through increased port efficiency. *Transportation Research Part A – Policy and Practice* 71, 167–178.
- 4 **Johnson, H.**, Johansson, M., Andersson, K., Södahl, B., (2013). Will the ship energy efficiency management plan reduce CO₂ emissions? A comparison with ISO 50001 and the ISM code. *Maritime Policy & Management* 40, 177–190.
- 5 Taudal Poulsen, R., **Johnson, H.** (2016) The logic of business vs. the logic of energy management practice: understanding the choices and effects of energy consumption monitoring systems in shipping companies. *Journal of Cleaner Production* 112, 3785–3797.

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1 The introduction

William the Conqueror, it is said,
began by eating a mouthful of
English sand.

Salman Rushdie

There are an overwhelming number of stories to be told, to relate to, to tell, before one can say with reasonable confidence what to do when it comes to research on energy efficiency in shipping. During the course of this PhD project, I've gathered stories of ships, of machinery rooms, of canteens insulated with styrofoam, of bridges, of captains, of engineers, of little sleep, of waiting, of full speed ahead, of forms to fill, of bribes to give, of measurements to take. There are stories of offices, of lay-offs, of Excel sheets, of no time, of the Baltic covered in ice, of secrecy, of Russian mafia. Of propellers, bunker fuel flow meters, of oil delivered at high temperatures to scam the shipping company because they pay for volume rather than weight, of engines, of torque meters on propeller axes, Froude numbers, slip, fouling, regression analysis, sensors, data. Reality is a mess. And there are many of them: reality in front of my computer is not the reality of a hectic day in the commercial operations group of a shipping company, nor is it the reality of a ship bridge in a storm. The stories from these realities often differ.

"Do you remember two months ago?" the captain asked. We were discussing whether there was much difference in energy efficiency between different ship crew; a common opinion amongst onshore managers.

"There was a storm here," he continued. "We used eight mooring ropes, forward and aft. I didn't sleep that night. I checked the metrology station. Wind, thirty-two meters per second. The whole quay was under water. The second officer cried: 'Look, water is coming!' I said 'Easy, we are on a vessel. We are floating. Don't worry.' Then I received a message [from the office]: 'Hurry, go to Lappohja!' But after loading, we will have a draft of six meters. The deviation from the metrology station is eighty centimetres because of the wind. This is a dangerous situation for the vessel in this area. So I have to go around, through the Great Belt instead. As a result, five more hours at sea and a large increase in consumption. For the same cargo and the same port. So it is very difficult to compare and calculate correctly. It depends."

Realities differ; realities are the effects of many things. Realities, John Law would say, are heterogeneous (Law, 1992). Sometimes they are connected, sometimes they share common grounds. In the context of energy efficiency, common grounds are often missing. The quote above is one such illustration: energy efficiency is difficult to quantify in practice and for people to mutually agree on and make sense of. It appears there, in report after report, to the European Union (EU) or the International Maritime Organization (IMO). Or in a presentation to the board of directors in one of the shipping companies in my project. These appearances contain promises of millions

of USD savings to a shipping company; billions to an international industry. To what extent and how energy efficiency is performed in reality is less clear.

There are many who would argue that the role of the researcher is to explain the order behind all of these stories, to show a more correct reality that explains all the others; a reality in which the technical factors are separated from the social, the environmental factors from the economical, and from the organizational. Reduce stories to simpler things. Point out the “drivers” and “barriers”, for example, as has been very common in energy efficiency research. But a challenge I encountered was that when stories were actually told, these “factors” were often quite mixed up.

“There’s just this constant wind against us, and when you’ve run your head into the wall enough times, you start thinking: well, let’s just take this another day.” The manager seemed exasperated. We were discussing transparency in terms of environmental information within the manager’s company.

“What kind of wind is this” I asked, taking a clumsy shot at the metaphor.

“Well, we are going to have a new webpage. And then, as usual, you just have to wait until it comes. A year ago, I think, it was going to have some kind of SharePoint approach, but in the end we adopted another way, since the webpage and the intranet was going to be in the same system somehow. So, transparency is the goal, but I talked to the communications officer recently, and they’re working on it, but it’s...” The manager trailed off.

“But these are technical reasons, it’s not...” I asked, having grown more, not less, curious.

“No, no, the transparency, we’ve always had that wish, and that hasn’t changed. We’ve said for years that we wanted to be transparent. But then it stopped in the world of technology.”

Not only people inhabit these realities; we co-habituate nature with the technologies we’ve created. We live in a world where hybrids proliferate (Latour, 1993). Does transparency lie in SharePoint, in our hopes and wishes, or somewhere else? Reality is quite a mess. But this should not be seen as a limitation. Law (1994) argued that researchers simply need to be much more modest in telling their story.

Bruno Latour (2005) would add that this is essential not only in order to produce better science, but also for political and moral reasons: who knows more about a particular part of reality, the analyst on temporary visit or those who live, work, dwell there all the time? In a text, should the latter be silenced and “explained” in terms of the researcher’s framework, or should they be brought to the frontlines? What would the people I’ve worked with say if I told them they were really just puppets in a hidden scheme with mechanisms that kept them from doing things to improve energy efficiency? And should I wish that the research I produce has a positive impact on the community I have studied and been a part of, dare I come with ideas for improvement or should I remain with describing and problematizing? Where does research end and politics begin?

1.1 Stories of motivation

Energy efficiency in shipping is a complex beast of a PhD topic. I feel at times there are too many things that are interesting; there truly is something for everyone.

Let's start with the question as to *why* study energy efficiency in shipping: it has many answers. These are stories of climate change, of international policy, of regional policy, of bunker fuel costs, of poor charter rates, of lobbying, of sustainability, of power, of science.

"It's not even three per cent, boy!" a not entirely fictive industry spokesperson boasted, referring to the relative contribution of shipping to global green house gas (GHG) emissions. "Not even three per cent, and we carry ninety per cent of the world trade - how's that for efficiency?"

He was right of course, I knew these numbers.¹ But what was it we were supposed to be discussing?

"And look here at my graphs, which show how inefficient an aircraft is compared to a ship when it comes to transporting large quantities of iron ore."

Oooh, nice graphs.

"All shipping companies are working on energy efficiency now; we need no more 'incentives'. Why would we, when the cost of fuel is so high?"²

I started nodding, agreeingly. If it makes economic sense to do certain things, surely they will be done?

"Besides, we are swamped with costs already, due to your high-brow, elitist, western-centred environmentalist lobbying: billions and billions, and you want to place an even higher burden? Think of the rest of the world! It needs the cheap transportation we provide!"

But as one would perhaps expect, these stories also contradict one another. Many of them even appear as having been created with the sole purpose of contradicting some other story. In that sense, not only the stories I collected from the field are messy; so are these. It could look like this:

"We need to start somehow in the coming decade to incentivise the industry to reduce emissions," say the European policy-makers. "And if you can't agree in London, we'll agree on your behalf in Brussels."

"Get off our turf," an industry representative counter-argued. "Any further legislation needs to be international legislation. But not too much of it, thank you very much; we're quite happy with what we already have. Obviously, since we took part in creating the existing legislation ourselves."³

"Oh no no no, you're both way off," interjects a group of British researchers. "We need to radically change business models in the entire industry right this minute or the world as we know it will perish."⁴

Rittel and Webber (1973) would perhaps say this is a typical "wicked problem". But possibly the strangest contradiction lies in the reports that detail the "potential" for

¹The most recent attempt to estimate the contribution of shipping to global GHG emissions for the IMO was done by Smith et al. (2014)

²This argument of course no longer used.

³On the role of industry influence on IMO processes, see Svensson (2014) or Campe (2009).

⁴Anderson and Bows (2012) produced GHG emission scenarios for the shipping industry and also discussed what kind of transformations would be necessary.

improving energy efficiency in shipping. This potential is substantial in monetary terms; an important part of the communication on the legislation directed at energy efficiency in shipping has been the savings it will bring to the industry (Bazari and Longva, 2011). From the perspective of an engineer or economist, this is of course most peculiar. It should be rational to improve energy efficiency at a far greater pace than what is actually seen. The situation is not unique to shipping and has often been referred to in literature as an energy efficiency gap (Jaffe and Stavins, 1994). The scientific literature generated around this problem, from all sorts of disciplines, forms the basis of this project. When I started in 2010, the notion that there was something similarly strange going on in shipping had been treated in reports to the IMO (Buhaug et al., 2009) and the EU (Faber et al., 2009), but the topic had not been thoroughly treated in peer-reviewed literature.

1.2 The research question; theories used and misused

Perhaps there are those that started reading this looking for advice on good maritime energy management practices. My apologies: this is not the topic of this text. As I started out in the introductory paragraph, it is many things; too many things to fit in a PhD thesis. Some specific parts of maritime energy management are treated in the appended papers. My intention with this introductory text is rather to look more broadly at my research project; to explore lessons learned from a three-year action research project on implementing energy management systems in two shipping companies.

In 2009, I was hired to do a one-year project at Chalmers on the applicability of energy management system standards in shipping companies, standards that had previously been used in the energy-intense production industry in Sweden (SS 62 77 50, replaced by EN 16001, which in turn was replaced by ISO 50001). That project ended with plans for implementation in two shipping companies, and based on that momentum, we planned for another longer project that would actually see through that this standard was implemented. The companies were Laurin Maritime, a family-owned chemical tanker company based in Gothenburg and Houston, and Österströms, a family-owned short sea dry bulk company. We were also able to attract a consultant from DNV to act as mentor to the PhD student in this longer project (me). An action research approach was chosen, and two-and-a-half years later, in Autumn 2012, one of the companies had an implemented system. The other hadn't, but many other interesting things had happened instead.

If there is a linear logic to this PhD project, it goes something like this: I did a literature review together with a series of interviews,⁵ did field research i.e. the action research project, discovered that the theory established through the literature review did not match the data, despair. I later discovered, thanks to a comprehensive course in qualitative field research, new and more suitable groups of theories. This led to a short, fleeting, sense of harmony, before I realised that by using these new theories, my initial ambitions on doing good, solving problems, "saving the world" etc. were rendered more problematic to achieve. The bittersweet End.

⁵This "pre-project" was documented in Johnson and Andersson (2011) and later as Johnson and Andersson (2014).

The research question addressed in this introductory text to the papers relates to the broader class of research to which action research belongs – that of transdisciplinary research, which aims at transcending scientific disciplines including even knowledge outside academic discourse into solving problems. It could be argued that this question is far too general and already should have plenty of answers. I’ve found it quite tricky, however, and have struggled with answering it in practice for years.

- How is transdisciplinary research made possible?

The writing of this text started with the idea of exploring the consequences of rejecting the theoretical framework I established initially. I was a slow learner; had I better understood the criticism that had already been mounted against my initial framework when I started (Shove, 1998; Weber, 1997), I could perhaps have left it alone. And these “new theories” that I found more useful are embarrassingly well applied in other research areas, in particular in Scandinavia (Czarniawska and Hernes, 2005; Czarniawska-Joerges and Sevón, 2003). Lately some of them are starting to become empirically realised in energy efficiency research as well (Palm and Reindl, 2016).

In short, the idea is to follow the movement started by those who study science and technology, which in turn inspired many who studied organising in general (Czarniawska, 2004b; Woolgar, Coopmans, and Neyland, 2009). The starting point is not the question of what, when, how much, why, or why not; the crucial questions of much of the energy efficiency gap research field. Latour (1988) called such questions accusations: “Everyone is looking for who or what is responsible for some state of affairs; ‘accusation’ is thus implicated in all attempts at explaining something.” Instead, the key question should rather be *how* (Knorr-Cetina, 1981).

I do not see this shift of focus as an argument *against* of the results of previous research, namely that one can argue there are aspects of how people organise their organisations, or of what we can call markets or institutions, that could be seen as hindering the implementation of technologies or procedures that improve energy efficiency. That it is reasonable to sometimes speak of “barriers to” or “drivers for” energy efficiency. The alternative proposed here is more of a problematising character.

To get straight to the point, a main theoretical conclusion of this project is based on the observation that when some phenomenon is referred to as “a barrier” or “a driver”, a metaphor has been introduced. Some phenomenon is said to be something else; “a bridge [has been built] between something that is less known—the object of the study—to something that is better known.” (Czarniawska, 2009, p. 158).

The point is this: “barriers” and “drivers” are not necessarily things that are better known. To paraphrase Latour (1986, p. 265): if the notion of a “barrier” may be used as a convenient way to summarise the consequence of collective action, it cannot also explain what holds the collective action in place. Or a later, more catchy, Latour (2005, p. 97): The barriers have never explained anything; the barriers have to be explained instead.

It was upon this reflection I realised I was in trouble. If I cannot contribute using a theory which summarises and simplifies for policy-makers and others what their problems are (i.e. the barriers) or what processes to enhance or push further (i.e. the drivers), what is really my role? The theory and the method must match (Czarniawska, 2014a); I was in trouble because I had tried to contribute to a reductionist framework

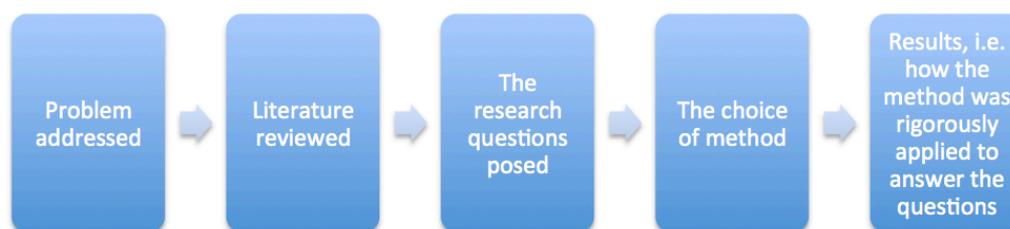


Figure 1.1: The linear model of research

using an interpretative methodology. I had rediscovered that “the close examination of living institution is the enemy of all logical classification.” (Arnold, 1935, p. 90).

1.3 The form of the thesis

In the first sentence of this chapter, I alluded to Alasdair MacIntyre, who wrote that it is not possible to answer the question “What am I to do?” before one answers the prior question “Of what story or stories do I find myself a part?” (MacIntyre, 1981/2013, p. 250). MacIntyre wanted to describe man as a story-telling animal. In justifying and making sense of what we do, we set our actions in relation to other actions, other stories. Following Rorty (1991), the aim is not to demonstrate objectivity, but solidarity.

In this introductory text of my PhD thesis, I wish to justify and make sense of a couple of years of work with organising energy efficiency in shipping companies. The norms of presenting research would require that I follow a certain order in explaining what I did. A reviewer of one of our papers argued along these lines:

The most important improvement, in my view, would be to give the reader a better understanding of the flow from the problem addressed, to the literature reviewed, the research questions posed, the choice of method, and how the method was rigorously applied to answer the questions. This would remove a sense of anecdotalism that I felt lessened the value of the contribution.

This logic can be summarized as in Figure 1.1; I will refer to it throughout this thesis. But this was far from how this project was carried out in practice. The MacIntyrian processes of justification, of sense-making, of setting-in-relation-to-other-stories never stopped. In a flow-chart, it rather looked something like Figure 1.2. In the published paper, the following text was inserted to address the reviewer’s concern

During the work with implementing the standard, literature was searched and read in the hope of finding information, theories, models, etc. that could be useful in the implementation process. The barriers discussed later in this paper were thus informed by the pre-understanding generated during the project; they arose iteratively through going “back and forth”—abductively—between theory and different activities in project (Dubois and Gadde, 2002). Greenwood (2002) summarised this, perhaps complicated, description well:

Action researchers, precisely because of the on-going dialogue between theorization, action, and re-theorization, cannot sep-

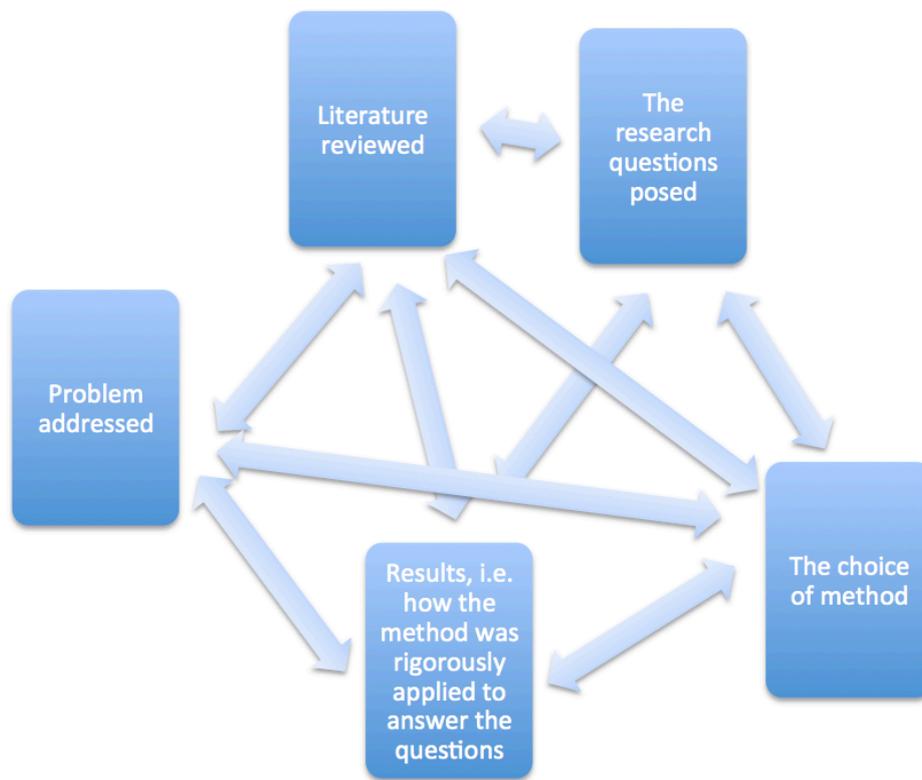


Figure 1.2: The research model applied in this project.

arate the research process from the findings in the dichotomous way that conventional researchers do.

(Johnson, Johansson, and Andersson, 2014, p. 320)

But who are these “conventional researchers” that Davydd Greenwood (2002) referred to, and whom I suppose the reviewer suggested I follow? Alexander Styhre (2013), in an introduction to academic writing, argued that these researchers might not actually practice research differently. Rather, the difference lies in the rhetoric they apply in writing: “[e]ven though... linearity is a fabrication, it is still expected that the researcher is capable of telling the story as if it had occurred via such prescribed linearity.” (p. 49).

In using the passive form – “it is still expected” – Styhre is referring to the customs, the institutions of science. These customs expect us to, amongst other things, adhere to certain standards of writing. Styhre again:

The written text that the reader encounters is linear and unfolds forwards; however, the actual research work is more messy and circular, and bringing these two activities together demands some rhetoric, leading to the polishing of, for instance, research questions at the very end. (p. 35)

But needing to follow *certain* standards in order to conform to scientific practice doesn’t mean following *a single* standard. There are others to follow.

In writing this thesis, I was inspired by many but perhaps first by the standards of writing set by John Law (1994) in *Organizing Modernity*, a study on how performance

indicators affected scientific practices in a British scientific laboratory. It is a reflexive text: the researcher and the (actual) research process is not hidden away by using a passive voice or fabricating linearity. The researcher and the process are very much in the forefront, present in the text. Due to this inspiration it is certainly possible there are readers who have already closed this thesis in despair, or even disgust. At first reading I myself hated his writing, and possibly him as well, as he was everywhere in the text. Like in *Being John Malcovich*, I felt as if I was inside his head (and I must say it is strange to write this thinking you may now experience you are inside mine).

Reflexivity in writing—of which of course there are many types (Latour, 1988)—can be experienced as being irritating, narcissistic, exclusive and indecisive (Law, 1994); strangely self-contained, [sealed] off from comment and criticism (Law, 1994); too cute, too clever by half (Woolgar, Coopmans, and Neyland, 2009); best consumed at home, behind closed doors (Czarniawska, 2014b). Indeed, “for better or worse, it is the commentator who has the last word” (Nabokov, 1962/2011).

There is a rationale however, beyond the wish to annoy and exclude readers (and if this were a paper for a mainstream journal, perhaps ensure immediate reject): it is a response to the recognition that the world is messy and multiple and that research is hard. It is done in order to increase the transparency of the text, making it more clear what has been done and why. Following Latour (1987), it is an attempt to show science in the making, a move “from final products to production, from ‘cold’ stable objects to ‘warmer’ and unstable ones.” (p. 21).

It can also seem a bit risky. In the postscript to *Organizing Modernity*, Law responds to various comments on his work, amongst others from junior colleagues:

“It is all very well you writing like this. You’re a Professor. You’re well established. But I’m not. I still have to get my PhD. So I can’t possibly write like that.”
(Law, 1994, p. 191)

Law acknowledged that he is indeed privileged being a tenured professor. But he stressed that when people in his position write in new ways, and they contrast these new ways to the old, new standards, conventions and institutions are set. He took a punch for the likes of the PhD student in the quote above. For the likes of me.

For if the privileged perform and embody a mode of ordering that, self-reflexively, also tells that it is a mode of ordering, then possibly, just possibly this new way of being will become more performable by those who are less constituted in privilege. It will start to spread.
(p. 192)

More than twenty years later, it has indeed started to spread in some parts of academia. In others not. That the rhetoric matters has been shown in many fields, perhaps most interestingly in economics (McCloskey, 1998), where the assumed rationale was perhaps the opposite: argumentation was expected to be done through equations and logic. Still some reviewers express sincere disapproval at non-standard ways of writing. Below an excerpt from a reviewer of one of our papers.

The paper lacks a scientific approach throughout and reads as if it is almost totally anecdotal in nature and has been written over a cup of coffee in the canteen... The conclusions are likely to be perceived as totally subjective

and actually rather arrogant - who are the authors that the reader might be interested in their subjective views or opinions?

We deserved some of the reviewer's criticism for sure, and we rewrote parts of the paper for submission to another journal. But for this introductory text to the appended papers, I am taking the luxury to write according to other conventions. For proof I can also write according to mainstream academic conventions, I figured I can refer to the appended papers.

Finally, and most importantly, a reflexive mode of writing is a natural consequence of the action research methodology used in the research project. On writing up action research, Greenwood and Levin (2006, p. 109) stressed that action research texts simply "cannot adopt the form of conventional social science." They particularly recommended a narrative mode of writing to be able to cover the learning processes that have taken place during the process. In a chapter on action research PhD projects, Morten Levin provided some other words of warning on PhD theses, and advice in line with the above.

The major challenge is to integrate both the deep involvement and reflective distance, to write a thesis that captures the richness of active engagement in the social change process and at the same time lends enough weight to the researcher's critical distance to the process that he or she has been engaged in. One mode of writing is to organise the thesis to communicate the gradual learning that takes place in an action research process, singling out major incidents, identifying what has been learnt through the practical achievements and what new actions have been taken. In this way the thesis shifts from the traditional linear structure to a cyclical spiral of reflection and action...
(Levin, 2009, p. 679)

In a guidebook to action research thesis writing, however, Herr and Anderson (2014) reminded the PhD student that while there are certain specific standards that action researchers adhere to, the committee that will pass judgement on it often come from other fields. They recommend that the dissertation should be viewed partly as a "political document". For example, "what might be acceptable and even encouraged in an avant-garde women's studies program might not fly in a traditional department of psychology." (p. 113). Similarly Greenwood and Levin (2006, p. 110) who above advocated narrative modes of writing for action research texts also warned that conventional (social science) researchers may find such writing to be "incomprehensible, uncontrollable, trivial, and probably just a little scary."

Baumann (2009) stressed the importance of "educating" one discipline about the other in interdisciplinary research. To facilitate reading for a broader range of readers, I have tried in this text to thoroughly motivate my choices and point towards relevant references. The reader will ultimately decide on whether I succeed in this balancing act of writing in ways that pleases both action research conventions as well as others.

1.4 The structure of the thesis

Some of the work done in the scope of this PhD project has been transformed into text and published in journals, and have been appended to this thesis. Parts of this work

are in draft article forms, parts are still raw field material. But crucially, this being an action research project: the most important part of my work is not a text, it is out there, in the form of an implemented energy management system according to ISO 50001 in a shipping company. Hopefully in the form of less bunker fuel used to transport chemical products. There are also a number of former M.Sc. students out there, working in a shipping company or in a consultancy or on board a ship or in a university, who took my course in maritime energy management. “Research that produces nothing but books will not suffice” wrote Kurt Lewin, often attributed as the founder, more or less, of action research methodology (Lewin, 1946, p. 35). There is no doubt, however, that there are only so many waking hours of the week; following Lewin’s credo also means less books and papers are produced.

This kappa has been structured as a series of short but connected essays organised in the way PhD theses are usually written; thus straying from the action research conventions mentioned above. After this introduction, there are four additional chapter, with the following themes:

2. The literature review. I go through the criticism mounted against the “barriers to energy efficiency” framework that I used in my earlier studies but later found troublesome. I borrow some concepts from the field of Science and Technology Studies (STS) and Actor-Network Theory (ANT) to propose a way forward, from generalisability to more local action.
3. The method. I go through the problem of access as seen from ethnographers and action researchers and why access is crucial. I continue with introducing action research and how it was chosen in theory and in practice.
4. The results. I explain the occurrence of the appended papers in the context of the action research project: why certain papers have been written and why some haven’t yet. I make some reflections on the research process.
5. The conclusions. I answer the research question.

Finally, there are five appended papers to this thesis.

1.5 To what discipline does this text belong?

There are many things this thesis is not. It is obviously not a work of engineering (look through it: barely any equations). But neither is it a work of social science. For sure, it will borrow unashamedly from some key authors. But to a social scientist, who will recognise these authors and perhaps their arguments, it will be evident I’m neither well versed in the classics, nor up to date in recent developments. My supervisor likes the (derogatory) term scientific shopper; I feel a lot like a scientific thief. Following the method of Mol (2002, p. 15), I’ve taken a lot of half hidden remarks, inflated them, and run away with them.

This thesis is possibly best read as a story about what happens when you expose an engineer to a problem that doesn’t seem to have boundaries. It is a highly unique and personal tale. It is some sort of transdisciplinary mongrel. To paraphrase the above anonymous reviewer: one long, very subjective, positively arrogant anecdote.

Do such texts have any value in a scientific discussion? It is the privilege of the reader to decide. For myself, writing this kappa has foremost been an act of therapy. I am doing it also in the hope that should there be others like me out there, and they by luck find this thesis, it may contain some useful words of caution; that it may serve as a confessional tale (Van Maanen, 1988). I've met and heard of a number of PhD students who have been directly discouraged by their seniors to pursue interdisciplinary projects and ideas. And in my opinion, rightly so. Interdisciplinary research is often presented as something unconditionally positive – “new knowledge lies in the border between disciplines” and so on – but it is not something I could recommend without serious caveats.

“We are not students of some subject matter,” Karl Popper (1963/2002, p. 88) now tries to reassure me, “but students of problems.” I agree, even after these years, but it needs to be said: I found research that crosses boundaries to be very, very hard. Being trained foremost in the natural and engineering sciences, I haven't really been expected to read since I did a year of philosophy at Linköping University, almost fifteen years ago.⁶ Some have argued that for such reasons, interdisciplinary research—perhaps especially research which lies in the borders between natural and social sciences—is best carried out in collaboration between researchers specialised in different disciplines (Wilson, 1996). In that sense, leaving my investment in an engineering education behind, an investment I started at the age of 14 when I chose to apply for the natural science programme for three years of Swedish gymnasium, and start dabbling with social science may seem not only risky, but stupid.⁷

A curse of my engineering physics degree is that I'm conditioned into thinking anything is possible to understand, or at least digest, given some time and effort. No matter how difficult the equations appear, after eight weeks they are typically solvable. Possibly, this trait of perseverance (some could say hubris) has now taken me too far astray. Do I still have a place to call home?

One day soon after he started at the school he came down to breakfast to find a kipper on his plate. He sat there staring at it, not knowing where to begin. Then he cut into it, and got a mouthful of tiny bones. And after extracting them all, another mouthful, more bones. His fellow-pupils watched him suffer in silence; not one of them said, here, let me show you, you eat it this way. It took him ninety minutes to eat the fish and he was not permitted to rise from the table until it was done. By that time he was shaking, and if he had been able to cry he would have done so. Then the thought occurred to him that he had been taught an important lesson. England was a peculiar-tasting smoked fish full of spikes and bones, and nobody would ever tell him how to eat it. He discovered that he was a bloody-minded person. “I'll show them all,” he swore. “You see if I don't.” The eaten kipper was his first victory, the first step in his conquest of England.

Salman Rushdie's *Saladin Chamcha* tried to conquer England, but he couldn't become *English*, no matter his years in boarding school (Rushdie, 1988/2010). Similarly, I cannot count on being told how to be a social scientist. I can read plenty, I can go live

⁶What Thomas Kuhn (1962/1996, pp. 155-156) pointed out, that students of natural sciences never have to read original work, only textbooks, still holds.

⁷A similar situation is described in Snow (cf. 1959/1998, p. xvi).

with them (some readers can read: “with you!”) and hopefully learn something from them, but I will still have been born in the wrong country. On the other hand, I’ve seen too much of London to go back to Mumbai unchanged. I cannot simply go back now. I am an engineer in self-chosen exile.

But one of the things that helped me convince myself I was still on an OK track was Czarniawska’s suggestion to self-diagnose myself into one of the two kinds of thinkers (Czarniawska, 2014a). The first kind is perhaps what a researcher is often said to be like, and probably especially in the natural sciences: a person who likes to dig deep into a specific topic using a specific framework. A person who knows one big thing. Self-diagnosing was useful because I realised that I was not like this, not at all. I was of the second kind, and “a great chasm” (Berlin, 1953/2013, p. 2) separated me from the other.

2 The literature review

The scientist looking at
non-scientific problems is just as
dumb as the next guy.

Richard Feynman

There are hedgehogs. Hedgehogs “relate everything to a single central vision, one system, less or more coherent and articulate, in terms of which they understand, think and feel – a single, universal, organising principle in terms of which alone all that they are and say has significance.” (Berlin, 1953/2013, p. 2). I had thought that in order to be a good researcher, one had to be a hedgehog.

But there are also foxes. Foxes “pursue many ends, often unrelated and even contradictory, connected, if at all, only in some de facto way, for some psychological or physiological cause, related to no moral or aesthetic principle.” Foxes “lead lives, perform acts and entertain ideas that are centrifugal rather than centripetal; their thought is scattered or diffused, moving on many levels, seizing upon the essence of a vast variety of experiences and objects for what they are in themselves, without, consciously or unconsciously, seeking to fit them into, or exclude them from, any one unchanging, all-embracing, sometimes self-contradictory and incomplete, at times fanatical, unitary inner vision.” (p. 2). Foxes know many things.

Foxes die, in the original fable, and hedgehogs survive. Undoubtedly, there are situations where it may be more beneficial to be of one kind rather than the other. But as Barbara Czarniawska, Richard Feynman and others have proven, whether one is a fox or a hedgehog is not what makes a good or a bad researcher.¹ Hopefully, a fox’s approach to energy efficiency in shipping is just as fine as that of a hedgehog’s. Just different.

As mentioned in the previous chapter, energy efficiency is a topic with much at stake. It can be a much needed cost-reduction strategy for a shipping company, or a mitigation pathway towards reduced climate impact of the whole international shipping sector. It can even be a card to play in international climate negotiations. On top of dealing with such a contested topic, I’ve employed an action research method: I’ve tried to directly solve a problem found relevant for two shipping companies. Some would say I’ve joined the “group of ‘do-gooders’ telling nice idiosyncratic stories of no scientific value.” (Greenwood, 2015).

To many academics, every warning bell and whistle is now sounding. LOUD.² So,

¹Czarniawska (2014a) diagnosed herself, but Feynman was called out as a fox by fellow physicist Freeman Dyson (2005) in a book review of a compilation of Feynman’s letters. He contrasted Feynman with Albert Einstein, who he saw as a hedgehog. Both were of course Nobel Laureates in Physics.

²Though possibly more so to a social scientist than an engineer. Engineers really want to solve problems; method literature for beginning qualitative researchers can extensively discuss the pitfalls of accepting problem formulations created outside peer-reviewed literature (e.g. Silverman, 1993/2006). The result can perhaps be seen in the observation of Henrikke Baumann (2009, p. 48), who is originally

to a researcher such as myself, the literature review becomes very important. If it is true that “[a] paper that does not have references is like a child without an escort walking at night in a big city it does not know: isolated, lost, anything may happen to it” (Latour, 1987, p. 33), what is then a transdisciplinary introduction to an action research project without some serious back-up? Sources of authority are important, explained Annemari Mol (2002, p. 30), and quipped: “If Latour says papers need references, then so they do, or would you want to disagree with him?” I’ve already blamed John Law for my style of writing; now I need to defend my text’s substance. But how?³

Before I get on with the actual review, I shortly cover two main strategies for doing so. Both are proposed to solve deficiencies in common review practices, although the deficiencies identified are different. As a result, the two alternatives are more or less diametrically opposed. My review will borrow mostly from the second alternative.

2.1 How to do a literature review, suggestion #1

In the linear model shown in Figure 1.1, the literature review comes first, and determines the methodology employed to answer the research questions motivated by the review. Following this model, “systematic” reviews are now being preferred by many researchers, a method borrowed from the health and medical sciences into for example “evidence-based management” (Tranfield, Denyer, and Smart, 2003) or “evidence-based policy” (Pawson et al., 2005). Below I’ve included an excerpt from the method section of such a review, incidentally of energy management literature in the production industry (Schulze et al., 2016). I find it worth quoting at length since it aims to defend systematic reviews while highlighting drawbacks of more traditional reviews.

The aim of such a systematic review is to locate relevant existing studies based on a prior formulated research question, to evaluate and synthesize their respective contributions and to report the evidence in a way that clear conclusions with regard to further research and managerial practice can be drawn (Denyer and Tranfield, 2009).

Systematic reviews exhibit significant advantages compared to traditional narrative approaches of literature reviews. Those traditional reviews generally do not follow a formal methodology, thus resulting in lacking transparency and replicability by others. Researchers can focus on “preferred” literature sources and base their review on a personal, purposive selection of materials they believe to be important. Systematic reviews help to reduce those implicit researcher biases (Denyer and Tranfield, 2009). Through the adoption of search strategies, predefined search strings as well as inclusion/exclusion criteria, systematic reviews effectively force researchers to search for all relevant studies beyond their own horizon of experience.

a chemical engineer: “Over the years I’ve noticed that when engineers initiate interdisciplinary work, it often resembles attempts at social engineering, while when social scientists put forward their work, it is not unusual that many engineers find it completely irrelevant.”

³“When?” is another important issue. But as mentioned in the previous chapter, analysis of literature is always on-going in action research (Herr and Anderson, 2014). In other fields, there are two main schools of thought: the literature review is conducted either before (i.e. the linear model) or after the data collection (e.g. the grounded theory approach)

Furthermore, the application and extensive documentation of a clear review protocol improves the methodological transparency of the review and enables future replication by other researchers. (pp. 3693–3694)

A number of mainstream scientific virtues are mentioned: rigour, transparency, and replicability. The rationale is that should researchers only follow these in their literature reviews, practices will improve and well-grounded knowledge will be easier to transmit to decision-makers, for example managers of firms.

2.2 How to do a literature review, suggestion #2

But now for something completely different. Sandberg and Alvesson found that most researchers in their field, which is management and organisation studies, prefer to present their research questions in terms of “gap spotting” rather than to problematize existing research (Alvesson and Sandberg, 2011; Sandberg and Alvesson, 2011). The latter is defined as being research that “challenges assumptions underlying existing literature,” as opposed to research that aims at filling in the gaps that other researchers have missed. This strategy is in some senses the opposite of the systematic review, for example in terms of what should be read:

Problematization research typically involves a more narrow literature coverage and in-depth reading of key texts, with the specific aim of identifying and challenging the assumption underlying the specific literature domain targeted. In this sense, the prevailing norm to relate one’s own study to all the relevant literature works against problematisation and needs to be resisted. (Alvesson and Sandberg, 2011, p. 256)

Another point of departure is that this strategy emphasises the unique role of the researcher and his or her specific abilities, rather than replicability by others. Problematization has no transparent and rigorous recipe to be followed. Rather it requires a special set of tacit skills, such as “creative efforts, some boldness, patience, self-critique, support from theoretical stances other than one’s own, and sometimes even luck” (p. 257). What was argued to be a vice in the previous strategy is here a virtue.

Sandberg and Alvesson find the overweight of gap-spotting studies interesting, as it has also been shown that the latter strategy produces more interesting research. They give many reasons why the former is still the dominant, one of which is political. Researchers need to publish papers and cannot afford upsetting their peers, who, amongst many other important things, review papers:

Many organizational theorists are... likely to adopt gap-spotting in order to ‘play safe’, and even be prepared to ‘sacrifice relevance for a careerist credibility’ (Mir and Mir, 2002: 107). More specifically, challenging assumptions often means questioning existing power relations in the field, and most scholars are probably unwilling to ‘rock the boat’ as it may trigger negative feelings and hamper their careers. Dominant actors within the scientific field, such as journal editors or reviewers are vigilantly guarding their positions from potential challenges (Bourdieu, 2004; Breslau, 1997). For instance, in his study of peer reviewing in several journals within the social sciences *as well as in the natural sciences*, Starbuck (2003: 347) found

that reviewers tend to ‘criticize the methodology of studies that contradict the theories they prefer, and they applaud the methodology of studies that support the theories they prefer’.

(Sandberg and Alvesson, 2011, p. 35, my emphasis)

In other words, while researchers may benefit from conforming to common trends and practices, from not getting into conflict, progress in research does not. Billig (2013) made a similar argument while writing extensively on how social scientists write badly. In particular, Billig dislikes the habits of filling texts with “jargon, nouns, and acronym”, “avoiding to say who did it,” and through “turning people into things”. These habits, according to Billig, are understandable given working conditions in academia, for example due to the mantra to “publish or perish” within specific sub-disciplines. Billig also incidentally seems to have spoken to someone very similar to the PhD student from the previous chapter, who responded to John Law twenty years ago.

“Yes, I agree with you, but what can I do? It’s worse than you say. I have to publish my thesis and get a research contract; I can’t do anything now; perhaps, when I feel secure, I’ll be able to speak out.” (p. 210)

Friedrich Nietzsche (1882/2001) would possibly protest and argue that this has nothing to do with current working conditions in academia. New ideas are never “good ideas”.

The strongest and most evil spirits have so far done the most to advance humanity: time and again they rekindled the dozing passions – every ordered society puts the passions to sleep – time and again they reawakened the sense of comparison, of contradiction, of delight in what is new, daring unattempted; they forced men to pit opinion against opinion, ideal model against ideal model... What is new... is under all circumstances *evil*, being that which wants to conquer, to overthrow the old boundary stones and pieties; and only what is old is good! (p. 32)

Nietzsche objected to the idea that “good” was often equated with “expedient”, and stressed that what we consider to be “evil” also fills an equally important function, albeit a different one. But when is it useful to be evil? Alvesson and Sandberg (2011) explored in-depth what problematisation entails, and when it is particularly suitable:

The proposed methodology seems particularly relevant in situations of political dominance... that easily follow from a dominant and established tradition... [In other words, when] a social interest bias and/or political factors govern knowledge production rather than good ideas. But also the domination of a particular school of thought can stifle new ideas and call for politically motivated problematizations. (p. 20)

As will be evident in the next section, all of these factors easily apply to energy efficiency research.

2.3 Exploring and problematising the barriers to energy efficiency framework

The point I wish to make is neither that a particular method for reviewing literature is superior to the other, nor that “anything goes”, but that there are different conven-

tions that serve different purposes (though of course reviews can also be carried out according a certain convention but badly according to the criteria of the convention). As McCloskey put it:

What distinguishes good from bad learned discourse... is not the adoption of a particular methodology, but the earnest and intelligent attempt to contribute to a conversation. (McCloskey, 1998, p. 162)

In the case of this thesis, I foremost wish to account for the development visible in my own papers. This was a slow process of both reading literature in new fields and performing field research: Paper I uses the barrier framework rather unproblematically; in Paper II, I had difficulties reconciling the field material with the framework but perhaps without understanding why; and, in Paper III and V I start to understand what could be alternatives.

The point of the review below is not to get into conflict, but rather to faithfully account for the criticism which has already put against the barrier framework. And perhaps sharpen it a bit for the sake of argument. But I take my cue from Czarniawska (1997) and stress that I much prefer to argue *for* an alternative, rather than *against* anything else.

Three main critical articles are the focus of the below review. The first is actually in support of the framework but also open to its limits (Weber, 1997); the second possibly been too frank in its criticism (Shove, 1998); the third is possibly too careful (Palm and Thollander, 2010). I will then elaborate a bit further leaning on literature from the studies of science and technology (STS) field and actor-network theory (ANT). This will lead naturally to the next chapter, which discusses method. But to provide some context to the general reader, I will first provide a short historical overview of the literature. Also here, I will perform a more in-depth reading of a select few key articles and books, that have had an element of theoretical development.

2.3.1 A very brief history of the barriers to energy efficiency literature

The field was essentially established after the oil crises of the seventies (York et al., 1978). Governments of oil-importing states all over the world started programs to reduce their reliance on these imports, through encouraging energy conservation as well as alternative energy sources. A peculiar result of the many assessments that were carried out in order to understand the technical and economical potential for energy conservation, was that they showed that energy use could be reduced at very low cost. It seemed as if households and companies were not behaving in economically rational ways. This determined a research question which has been used with small variations since then: why aren't cost-effective technologies being implemented? At the end of this chapter, I'm going to argue for different kinds of questions.

It is perhaps telling that one of the first group of researchers to address barriers to energy efficiency referred to themselves as “policy-analysts”, rather than “researchers” or “academics” (Blumstein et al., 1980). Their goal was to contribute to policy-making processes that would lead to improved energy efficiency. Rather than being based on any underlying theory, their framework of barriers to energy efficiency was foremost a result from empirical studies; Blumstein et al. (1980) introduced them simply as “commonly occurring”. Barriers were divided into six classes: “misplaced incentives”, “lack of

information or misinformation”, “regulation”, “market structure”, “financing”, and “custom”. When research is carried out in practice, they stressed that the “actual” barriers will be a mix of these.

Blumstein et al. (1980) highlighted the importance of going beyond “classifying” barriers in the field, to “exploring their causes”, which may require the researcher to “untangle a tangled causal chain”. Sometimes such an analysis will not end in a clear idea for a solution, but instead reveal complex political situations, for example that labour unions may “protect obsolete building codes that provide jobs for their members.” As a result, policy-makers need to make a political assessment of the feasibility of addressing a particular barrier.

The paper ends with a call for interdisciplinary work:

The policy maker can have little hope that he will be able to confront social and institutional barriers to energy conservation with a complete understanding any time in the near future. [In order to get closer to this understanding] the efforts of many other researchers from a variety of disciplines must be enlisted before really substantial progress can be made. (p. 371)

Following Blumstein and their predecessors, a range of studies were performed from different theoretical perspectives. Shama (1983) for example, focused on behavioural perspectives and especially the diffusion of innovations framework to discuss energy conservation in US buildings. According to Sutherland (1996), it was not until the 1990’s that these kinds of studies started to be more thoroughly scrutinized by economists. This intervention would have a profound effect on the field, as according to (neo-classical) economics, public policy intervention can only be motivated in the presence of market failures. Sutherland (1991), for example, argued that the barriers discussed by many researchers were not market failures. As such, if public policy were to address these, markets would become less efficient, consequently leading to less efficient use of resources. The finding that many measures appeared to be “cost-effective” to implement would, given no market failures, instead imply that there were other, more lucrative, areas where the investments were being made.

This gap between the engineers and the economists took some time to bridge, before having also a large influence on climate policy. Sorrell et al. (2004) described how studies made from different perspectives were used in the discussion leading to the US withdrawal from the Kyoto protocol. A study by the US Energy Information Administration (EIA) showed how US GDP would drop by more than 4 per cent, while another study showed that the US could meet their Kyoto commitments whilst *raising* GDP. The differences can be explained through these different ways of viewing “cost-effectiveness”.

A special issue of the journal *Energy Policy* was directed at bridging the gap between engineers and economists (Huntington, Schipper, and Sanstad, 1994). I believe it is not too far-fetched to claim that after the economists’ intervention it became crucial for many researchers, though as we will see in the next section not all, to discern between barriers in general and those that were also market failures. In contrast to other forms of criticism, the re-framing by economists was quite easily assimilated over time into the barrier framework.

A paper by Adam Jaffe and Robert Stavins published in this special issue has been especially well cited (Jaffe and Stavins, 1994). Here, the “social”, the “institutional”,

and the complex political situations found in studies like that of Blumstein et al. (1980) have all but disappeared. The barriers are there, but translated into economic rhetoric. *Market* barrier now means any factor that “may account for” the energy efficiency gap (though there is also a small set of “non-market” barriers that contains little of interest, e.g. “inertia”). The role of the policy-maker no longer includes for example to consider political feasibility of measures; everything in the Jaffe and Stavins framework is a matter of costs and savings. In Figure 2.1, this framework is visualised, adapted from their paper. What the engineers have showed in their assessments constitute the “hypothetical potential”, and somewhere in between this level and today’s “baseline level” lie the “narrow social optimum”, which is reachable if market failures are removed. A “true social optimum” can be realised if externalities are accounted for, e.g. through a tax.

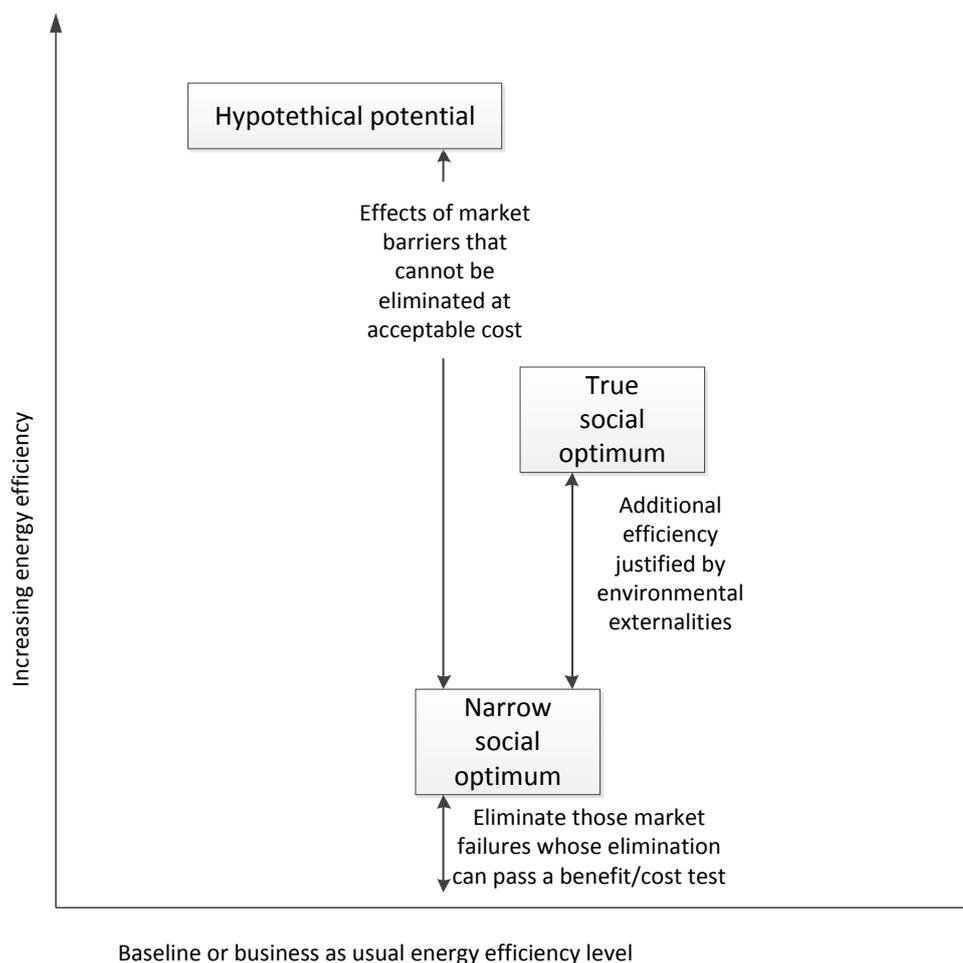


Figure 2.1: The energy efficiency gap, adapted from Jaffe and Stavins (1994).

Sorrell et al. (2004) conducted a comprehensive series of studies using interviews and surveys, in what has become a much referred-to contribution to the barrier framework. In total 48 organisations in brewing, mechanical engineering, and higher education were approached with the goal to “examine how barriers to energy efficiency operate, the circumstances in which they arise and the extent to which public pol-

icy or organisational change may overcome them.” (Sorrell et al., 2004, p. 5). Their book has perhaps been more popular not for the reporting of this empirical data, but for the theoretical chapters that precede it. Sorrell et al. (2004) takes the framework as established by Jaffe and Stavins and other economists and expand it using transaction cost economics, and to some extent behavioural economics. They introduce a taxonomy of six barriers, inspired by preceding barrier studies, and then interpret these using concepts from the expanded economical framework. Their goal is to “obtain a clear theoretical understanding of the nature of the proposed barrier.” (p. 27).

A barrier, in their terminology is “a postulated mechanism that inhibits a decision or behaviour that appears to be both energy efficient and economically efficient.” (p. 8). Similarly to other researchers, however, they are aware of the difficulties in establishing a true mechanistic model, and add that “each of these barriers may have a number of contributory mechanisms, and several of these mechanisms may coexist in different situations.” (p. 9).

While Sorrell et al. (2004) argue that extension of the neo-classical barrier framework into transaction costs economics and behavioural economics has certain advantages, as it “provides a richer and more realistic account of economic behaviour and organisation” (p. 50), they also acknowledge that it makes it more difficult to draw clear policy conclusions. Neo-classical economists interested themselves mostly with markets, but these new fields started to open up the “black box” of the organisation. Sorrell et al. (2004) point to the difficulties in determining what are barriers or failures in markets and what may be problems in organisations. They note that if organisations can be arranged differently so that transaction costs associated with implementing energy efficiency measures can be lowered, one can start discussing also organisational failures. Similarly to Jaffe and Stavins, they thus align themselves with the economists’ perspective that everything can be monetised: “Barriers can be overcome if there exist alternative institutional arrangements that can lower the net transaction cost of improving energy efficiency.” (p. 50)

The framework has since then of course seen additional development along different pathways (e.g. Cagno et al., 2013; Thollander and Palm, 2012), but I draw the line for this review here. Concluding, the metaphor of a barrier to energy efficiency has been used in different ways during the past decades. While recent reviews such as that by Cagno et al. (2013) describe the history of this framework as progressing in a linear manner – e.g. “economists clarified the difference between market barriers and failures” – I have here emphasised, inspired by Alvesson and Sandberg (2011), that no such progression has taken place. It is rather the case that underlying assumptions behind different key pieces of literature have been very different. I would rather say economists “redefined” the barrier framework; for better or for worse, more or less clear, are value judgements, dependent on one’s own favourite theoretical framework.

2.3.2 Criticism of the barrier theory

Criticism was mounted against the engineers from economists, which was accepted and adapted into the framework. But criticism has also come from another perspective which has not had an impact to the same extent. I present three papers:

- Weber, L., 1997. Some reflections on barriers to the efficient use of energy. *Energy Policy* 25, 833–835.

- Shove, E., 1998. Gaps, barriers and conceptual chasms: theories of technology transfer and energy in buildings. *Energy Policy* 26, 1105–1112.
- Palm, J., Thollander, P., 2010. An interdisciplinary perspective on industrial energy efficiency. *Applied Energy* 87, 3255–3261.

Although Weber’s paper is frequently cited in barrier studies (with its criticism neglected), Shove’s paper seems to have been largely ignored by the researchers who use the barriers to energy efficiency framework for about a decade, until it was picked up by Thollander and Palm.⁴

Weber, L., 1997. Some reflections on barriers to the efficient use of energy. *Energy Policy* 25, 833-835.

Lukas Weber wrote not a full paper but a two-page “viewpoint”. It introduces the barrier model, which is what most researchers quote from this paper.

A barrier model is asked to specify three features: (1) the objective obstacle; (2) the subject hindered; (3) the action hindered. The methodological question of how to determine a barrier model is the following: What is an obstacle to whom reaching what in energy conservation?

- (1) What is an obstacle...: persons, patterns of behaviour, attitudes, preferences, social norms, habits, needs, organisations, cultural patterns, technical standards, regulations, economical interests, financial incentives etc.
- (2) ...is an obstacle to whom...: consumers, tenants, workers, clerks, managers, voters, politicians, local administrations, parties, trade unions, households, firms, NGOs, etc.
- (3) reaching what?: buying more efficient equipment, retrofitting, decreeing an energy tax, establishing a public traffic network, improving operating practices, etc.

(Weber, 1997, p. 834)

However, Weber also discusses its limits.

It is empirically impossible to find the “true” reason behind an energy-conserving action which has out been taken. Barriers cannot be empirically classified because they are invisible. They are real, but not observable... Nonetheless, practical measures can be realised for better institutional, organisational, behavioural and market conditions to make energy conservation more successful...

[B]arrier models assume that improved efficiency is the result of a particular action (e.g. buying more energy efficient equipment, retrofitting building shell or decree of an energy tax). Energy conservation which results from the omission of an action (e.g. not buying a certain machine) or doing

⁴Science direct gives 174 hits for “barriers to energy efficiency” between 1998 and 2010, two of which cite Shove’s paper.

something in a different way (e.g. integrated instead of isolated planning), cannot be described by a barrier model. Barrier models are limited insofar as they can only describe energy conservation in the sense of positive actions. Thus, they do not represent the whole range of energy conservation options. (Weber, 1997, p. 834)

The paper has been well-cited but few take these points into account; most are satisfied with referring to the definition of the model. This is not the place to go into this in-depth to the implications of the limits Weber introduces, but to take one example: A common “barrier” in literature is “lack of time” or “other priorities”. In other words, someone has explained to a researcher that they didn’t do a certain action that would improve energy efficiency because they didn’t have time or equivalently that they had other things to do. This is in Weber’s framework a situation where the barrier model is not a very good match.

Weber stresses that the quality of a barrier model is determined by the extent it is ‘more or less promising’ with regard to the problem the researcher is trying to solve. It seems Weber has a pragmatic view of models: their purpose is not to be true but to be useful. To follow his argument, the reader can test the proposed barrier: is it for example reasonable to *a priori* believe that whatever action was being discussed would be done if more time was made available? In my view, leaning on Weber, these kinds of “barriers” become close to tautological, and add very little of explanation.⁵

Shove, E., 1998. Gaps, barriers and conceptual chasms: theories of technology transfer and energy in buildings. *Energy Policy* 26, 1105-1112.

Elizabeth Shove (1998) reported on a project directed at understanding how governments in different countries choose what research projects to fund related to energy efficiency in buildings, based on sixty interviews with people from academia as well as project funders. She found that her interviewees shared similar beliefs, and summarises these into three areas: the notion of a technical potential; the notion that there is a gap between this potential and reality, and that this can be explained in terms of market barriers as well as “non-technical” barriers; and finally, the notion that technology is linearly “transferred” from research to development to practice (supported by government policy). Shove problematizes these beliefs and goes on to construct an alternative, drawing from the field of sociology of science and technology (e.g. Bijker, 1995; Callon, 1990; Callon et al., 1992), an alternative which is not compatible with the views held by her interviewees.

Proponents of the conventional view talk of technical potential, believing that energy related choices are made by free, but more or less knowledgeable agents whose rational economic actions may be clouded by a variety of “non technical” or psychological barriers. Countering this we have an analysis which instead suggests that technical change is an unremittingly social, and thus contextual, localised and temporally specific, process... From this perspective, the notion of pure technical potential makes as little sense (after all, the technical is social), as the notion that non technical barriers

⁵“Lack of competence” – we didn’t do X because we didn’t know how; “Inertia” – we didn’t do X because we don’t do things what we don’t usually do. Etc.

and obstructions limit realisation of that potential (Bijker and Law, 1992).
(Shove, 1998, p. 1109)

What seems to constitute a problem today is not of much concern in her framework – e.g. what are the present barriers – but rather, what could the alternatives actually look like? The argument is not that people do not find it troublesome to implement energy efficiency measures, but rather that summarising these experience in a barrier model might not be very helpful.

[O]ne of the problems of the conventional approach is that insufficient thought is given to the shape of the future. What is routinely missing is any analysis of what the rest of the energy efficient utopia might look like: what new techno-economic networks do technological visions presume and what forms of social re-alignment are required along the way? More prosaically, what would everyday life be like in a world which used 50 or even 20% less energy? What organisations are involved, what sorts of companies are implied, and how are the details of energy consumption to be arranged? In other words, there is real work to be done in articulating the features and characteristics of social worlds presupposed by proposed energy-saving scenarios (de Laat, 1996). (p. 1100)

As I understand Shove's position, the role of the researcher is not to look for generalisable data and apply reductionist theories, but to have a more local and specific interest. Shove argues for a practical and direct role: following Callon et al. (1992), researchers could take active part in developing new "techno-economic networks".

Equipped with a careful, localised understanding of contemporary patterns of interest and opportunity, sociotechnical analysts might well be able to specify ways of advancing the cause of energy efficiency within specific settings (Morrill, 1994; Lutzenhiser, 1994). Going further, they might spot and perhaps engineer social and organisational circumstances in which it made sense for insulation manufacturers and boiler makers to work together, or in which professional fees and relationships were re-configured in such a way that architects and engineers had a common interest in avoiding the introduction of air conditioning. (Shove, 1998, p. 1110)

Shove ends with stressing that although the researchers' approach as well as rhetoric needs to change, such a change will not come easy: policy-makers may prefer (and fund) research that concludes with broad and generalizable "results" that reproduces their own view of their role in society. The arguments from her paper are used (in less polemical form) in further empirical studies, reported in Guy and Shove (2000).

Palm, J., Thollander, P., 2010. An interdisciplinary perspective on industrial energy efficiency. *Applied Energy* 87, 3255-3261.

It took more than ten years for researchers using the barrier framework to take up on Shove's criticism. Palm and Thollander (2010) combines engineering and social science approaches to energy efficiency in comparing and reinterpreting results from different industrial sectors. They note that although classifying barriers so that it is clear

which are market failures has been important in the past, to motivate policy intervention according to neo-classical economics, a change of mind has taken place e.g. amongst EU policy-makers. Through the Energy Service Directive (ESD), both market barriers and failures are given equal attention.⁶

Thollander and Palm revisit previous studies on barriers to energy efficiency in different Swedish industries, and found differences in the way barriers are prioritised by companies. Some examples:

[A]mong the industries in Oskarshamn the risk of production disruption was considered the greatest barrier to implementing energy-efficient technologies, while this was ranked fourth by the studied Highland SMEs. Among the Highland SMEs, lack of time was deemed the greatest barrier, while the Oskarshamn industries ranked that barrier second and the Swedish foundries ranked it only eleventh. For the Swedish foundries, lack of capital was the largest barrier, while the Oskarshamn industries ranked this ninth and the Highland SMEs ranked it third.

(Palm and Thollander, 2010, p. 3258)

Palm and Thollander suggested that the fact that different barriers are ranked so differently in different industries implies that they could be results of local values, routines etc. that are unsupportive of energy efficiency. Understanding that barriers are the result of such social processes can be used as leverage by researchers who want to problematize barriers together with practitioners.

Researchers may also facilitate and encourage the formation of networks across sectors and traditional professional boundaries. It is important to approach barriers from a new perspective, using non-traditional analytical tools that can contribute new understandings or questions as to why a particular barrier is perceived as important in a company. Analysing a company's culture and existing networks, that is, understanding the context in which energy efficiency goals and measures are discussed, is important in order to take industrial energy efficiency a step further. (p. 3260)

They also argue that a more interdisciplinary view of barriers to energy efficiency suggests new roles for policy-makers.

Common policy instruments such as taxation and subsidies could arguably be combined with information and discussion across established professions and sectors. For example, creating actor networks crossing established sector boundaries would challenge established norms and routines...[P]urely economic incentives would perhaps prove not to be the most efficient policy instruments. Instead, it may be workshops, seminars, energy clusters or other open networks in which established norms, routines, and tacit knowledge are highlighted and challenged. (p. 3260)

In conclusion, Palm and Thollander build upon Shove's arguments, but in contrast to Shove, they do not disregard the barrier model. In their view, the barrier concept

⁶Directive 2006/32/EC of the European Parliament and of the Council of 5 April 2006 on energy end-use efficiency and energy services and repealing Council Directive 93/76/EEC. Brussels: EC; 2006.

is still useful, but researchers need to take into account that the barriers are socially constructed.

2.3.3 Concluding the problematisation and moving onwards

The barriers to energy efficiency framework has now been introduced along with three forms of criticism: that the barrier model should be viewed in terms of usefulness and is not always applicable (Weber); that the barrier model should be rejected because it builds upon a false separation of social and technical and also because it puts focus on presently perceived problems instead of understanding how change could happen (Shove); and finally, that the barrier concept is useful but we need to take into account their social construction when designing our studies (Palm and Thollander).

It seems the clear that the economists' criticism lead to wide-spread adoption of their definition of barriers and the demarcation between failures and barriers. Equally clear is that criticism from a social science perspective has been little understood and/or ignored. Alvesson and Sandberg would probably find the field ripe for a thorough problematisation.

Of course, many researchers since the barrier framework was established have stated openness towards the contribution of other disciplines to their problem. In an editorial to the journal *Energy Policy*, Lorna Greening and Thomas Jefferson comment on the lack of contribution to the field from the perspective of social scientists, on the basis of the energy efficiency gap special issue referred to above:

Of the 10 papers compiled in this issue, eight discussed hypothesized economic reasons including market failures which served as obstacles to investment or caused misallocation of resources; information problems; and, inefficient pricing of energy. Only two of the papers were written by social scientists; and, these emphasized decision environments, institutions, and organizational networks as explanations. However, the three groups of researchers, technologists, economists, and social scientists, never came together to formulate an integrated explanation or a useable framework for policy makers. This lack of an integrated framework largely continues today and hampers the formulation of effective policy making.

(Greening and Jefferson, 2013, p. 1)

The problem is of course that some disciplines do not ask these types of questions. Contributions from some disciplines simply do not fit into a barrier framework; in this view, the goal of creating an “integrated framework” is flawed. Some disciplines, wrote Elizabeth Shove (2011), are simply as different as chalk and cheese. The consequence is that researchers need to be more attentive and sensitive towards other paradigms and frameworks. Not because they can be used to contribute to one's own worldview, but because *different* world-views are important:

Making use of this range [of theories] is important not because divergent approaches can be added up to provide a more “holistic” view, not because there is some absolute notion of strength and weakness in terms of which contrasting paradigms can be evaluated, and not because contributions from different disciplines provide policy makers with a more colourful palette of

responses to matters of urgent concern. The critical point is that contrasting paradigms are valuable because they generate different definitions of the problem. When some problem definitions dominate and others are edged out of the frame, interpretations of relevant research are immediately constricted. Although it is easy to see how this comes about, the consequence is to limit and sometimes exclude controversy and debate about how questions are formulated and framed. (Shove, 2011, p. 264)

Going back to the beginning of this chapter, there are roles to be played for both hedgehogs – who know one big thing – and foxes – who know many – in energy efficiency research. It appears that there are quite a few hedgehogs who believe themselves to be foxes: seeking plurality but still trying to fit everything within in a single framework. Pure foxes are not as common.

2.4 Beyond barriers (and drivers, hinders, triggers etc.)

The purpose of problematization is not to proving barrier theory “wrong”. Nor that some other theory is “right”. My main conclusion is what I stated in the introduction: that the framework is possibly useful but may also metaphorically deceptive in the way it simplifies complex processes into structures. It seems to imply that there are concepts that can travel (Czarniawska and Joerges, 1996) from researcher (from theory) to interviewee/respondent, back to researcher and then onwards to policy-maker essentially unchanged. That in the end, everyone is in agreement what the problem is, and that “removing the barrier” is only a matter of cost.

While I don't have very good alternative metaphors to offer, the ones that exist (“barriers” and “drivers”) do a poor job of making sense of what is going on in organisations when it comes to energy efficiency. As suggested by Czarniawska (1997), complex phenomena need complex metaphors. Of course simplification is necessary when telling stories, whatever the rhetoric, but there are ways to do it which are more modest; “less prone to heroic reductionism” (Law, 1994, p. 9). As will be argued below, better metaphors and model could be processual rather than structural.

The framework of barriers to energy efficiency is a field which apart from e.g. the thorough set of case studies performed by Sorrell et al. (2004), is dominated by surveys or interviews carried out with a single person per organisation (recently by e.g. Rohdin and Thollander, 2006; Rohdin, Thollander, and Solding, 2007; Sardianou, 2008; Schleich and Gruber, 2008; Thollander and Ottosson, 2008). Such approaches, where “bias” and personal opinions of interviewees are preferably “minimized” by e.g. standardised questions and granting anonymity, has lead to projects where barriers grounded in theory can e.g. be “ranked” by participants. That the researcher constrains the responses of the interviewees based on a well-used but in essence *ad hoc* theoretical framework does not count as bias in such approaches.

I had material of a very different kind than what is usually gathered in energy efficiency research, and it was certainly full of “bias” in the above sense. How could I possibly contribute? It was not until the action research project was more or less finished that I found ways to make sense of my work. Researchers such as Barbara Czarniawska, Bruno Latour, and John Law have had a major influence on how to formulate ways to discuss what I had been doing. I will use some texts from them below to my cause.

2.4.1 A modest alternative

Law (1994) listed four ways in which to make sociology more modest. The reader may ask what relevance modest sociology has in thesis for the degree of doctor in engineering but bear with me: Law's intention was not to study what sociologists usually study, but to study science and technology, scientists and engineers. Further, these four ways to modesty arise from a problematisation of four key assumptions many sociologists make; assumptions that have much in common with how energy efficiency gap research has been carried out.

First, modest sociology is concerned with not giving privilege to certain phenomena when it comes to what needs to be explained and how. Law refers to this as the principle of *symmetry*. Following Bloor (1976/1991), false and true knowledge, for example, needs to be explained on the same terms.

In the context of energy efficiency this could mean discarding the notion of a “cost-effective potential”: it is just as interesting to study why energy efficiency measures are implemented than why they are not. Law writes:

If you start of assuming that some knowledge is true and some false [e.g. that some measures are “cost-effective” and “rational” to implement], then you never get to analyse how the distinction is constructed and used.

(Law, 1994, p. 10)

The crux of the matter here is thus that the notion that some range of measures are “cost-effective” was typically *not* constructed by the people the researcher has approached for a study on the barriers to these measures. Barrier researchers have somewhat addressed this assumption by adding “drivers to energy efficiency” to their taxonomies, making also the implementation of cost-effective measures a topic of study. This addition, while possibly an improvement in Law's view, is still at odds with what will be discussed further below. The key phrase in the quote above is “constructed and used”; the point is not to add more similar theoretical constructs, but to start treating them as products of some process. Again, the question is now *how* (are people driven, are people hindered etc.). Paper IV is a practical example, where we attempt to re-create the potential for energy efficiency due to increased port efficiency from the perspective of a specific shipping company (Johnson and Styhre, 2015).

Second, modest sociology is *non-reductionist*, which is the anti-thesis to barrier-like frameworks. Reductionism, Law (1994, p. 12) writes, is “the notion that there is a small class of phenomena, objects or events that drives everything else—a suggestion often linked to a belief by the analyst that he or she has understood these root phenomena.” This goes against the above principle of symmetry:

First, you need to draw a line between two classes of phenomena by distinguishing the those that drive from those that are driven. And second you claim that the behaviour of the latter is explained – often you say caused – by the actions of the former.

(p. 12)

Non-reductionism requires a move into a different and more modest form of rhetoric, found for example in narratives (e.g. Czarniawska, 1998). Such a move is made possible through rejecting the correspondence theory of truth: the idea that it is possible to say whether a statement is true or false by comparing it to “reality”. Instead, this requires

following the pragmatists by replacing it with a coherence theory of truth (Rorty, 1991): a statement can only be compared to other statements; a text only to other texts. The argument is not that all attempts at “explanation” should be replaced with “descriptions” – noting of course that some see little difference between description and explanation: “only bad descriptions need explanation” (Latour, 2005, p. 147) – but to view the issues brought up by reductionist researchers as effects, not as causes. The non-reductionist researcher does not shy away from why-questions, but first converts them into *how*:

The job of the investigator is not to discover final causes, for there are no final causes. Rather, it is to unearth these schemes and expose their contingency. (Law and Bijker, 1992, p. 292)

Third, modest sociology sees the social as not made up of structures, but *processes* that are *recursive*. The first concept is possibly more simple: seeing processes instead of structures, organising instead of organisations, using verbs instead of nouns (unfortunately, *barriering* instead of barriers doesn’t work as well). But recursive? We need to get away from the notion that something “drives” these processes: “The image that we have to discard,” Law argues, “is that of a social oil refinery: society is not a lot of social products moving round in structural pipes and containers that were put in place beforehand.” (Law, 1994).

The fourth way has already been mentioned. It concerns *reflexivity*, and is perhaps especially easy for a physicist to fathom. As physicist Werner Heisenberg put it: “We have to remember that what we observe is not nature herself, but nature exposed to our method of questioning.” (Heisenberg, 1958/2007, pp. 31–32). According to Law, reflexivity be seen as an extension of the principle of symmetry: also we, the researchers, are the result of various processes. In accounting for our work, we need to take into account (not “minimise”) our own effects on the things we study and the texts we write.

The above paper by Palm and Thollander (2010) can serve as an example: they argued for noting that “barriers” are the results of social processes in companies and sectors. However, following the principle of reflexivity, these “barriers” are also the result of the researchers’ own queries. How the practitioners themselves related to energy efficiency before (and after) the researchers arrived and put the framework to them is not known.

Concluding, the goal of this PhD project was not so much something we wanted to *know*, as it was something we wanted to *do*: to see to that an energy management system standard was implemented in two shipping companies. To improve energy management practices. In this way, rather than collecting material on how people perceive their problems (or their drivers) and interpreting these in a theoretical framework to propose general (policy) solutions, we’ve applied action research into taking a more local problem and attempting to solve it in practice, informed by theory throughout. Figure 2.2 is an attempt to visualize these differences. The kind of theories that fit better with my method seem different from those that researchers have traditionally used, in particular in terms of its their aims. Czarniawska (2013) followed Blumer (1954) in proposing her view:

[T]he aim of theory remains the offering of some kind of understanding of the world, its possibilities for development, and the directions along

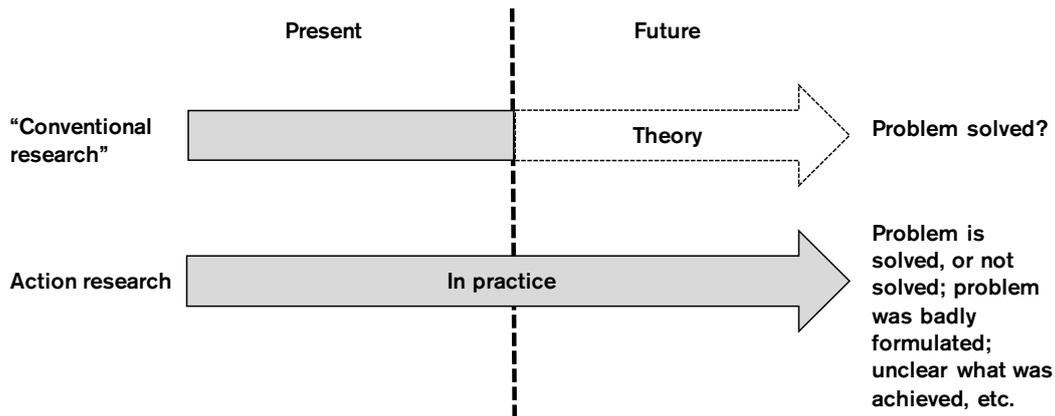


Figure 2.2: Two forms of research.

which it can move. The main concerns of such a theory should not be the highbrow sociological abstractions, reified into entities, but the ongoing construction of the world or certain parts and aspects of it.

(Czarniawska, 2013, p. 106)

The last sentences in Sorrell et al. (2004) are as follows:

There is clearly more work to be done in understanding barriers to energy efficiency and in identifying their determinants in different contexts. But while this is an appropriate objective for academic research, the ultimate goal for climate policy is to overcome these barriers. Research should aim to contribute to both the objectives. (p. 313)

What I have argued is that research that meets the second objective, of “overcoming barriers” can – dare I say should? – be of a very different kind. To return to the question of what modest sociology could possibly offer, consider the situation I’ve described in this chapter and possibly sharpen it a bit to provoke reaction. At least to me, the situation is somewhat unexpected: The people who propose work with generalisable reductionist theory and hands-off, “non-biased” methods, leading to problem formulations grounded in practice but solutions only in theory, are mostly engineers. The people who prefer getting their hands dirty with practical work and practical solutions to specific problems are sociologists. The next chapter will deal with how to get going with such work.

3 The method

Everyone has a plan until they get punched in the mouth.

Michael G. Tyson

Studying shipping comes with certain challenges. Not in the least because it is sometime said to be one of the most secretive industries (George, 2013). Or as an interviewee put it: “speculative”.

“Shipping is very speculative, you know,” the elderly CEO of a small but successful shipping company explained.

“It’s no secret really that it’s only the weapons industry that is more speculative—no tax and all that, nobody revealing who they are. When a ship sinks, there are never any owners. They are only found when chased with a blowtorch. One could say that this is the call sign of shipping,” he continued. “It’s a bit sad, one can think sometimes. But on the other hand, it is what it is.”

The difficulties in gaining access for research and in particular field studies in shipping have been duly noted. In an editorial to *Maritime Policy & Management*, David Moreby (2004) elaborated on contemporary maritime researchers, who, in his view, were producing papers that were “well presented, well referenced and well argued pieces of work, and far more academically vigorous than the early papers [published in *MPM*]” (p. 90). Unfortunately, Moreby continues, these papers were on topics that were “so extensively researched and reported that they are now as dry as dust.” (p. 90). Billig (2013) as well as Alvesson and Sandberg (2011) from the previous chapter saw such processes in different ways as inherent of academia. Moreby, on the other hand, seemed to have seen this change in the nature of the papers as a result of it becoming increasingly difficult to perform field research.

[In the good old days, it] was relatively easy to conduct research work in the field, provided one knew some good jokes and remembered to take a gift of all that days’ newspapers, it was relatively easy to join ships for short voyages for observational research. Also, provided one could hold one’s liquor and keep one’s mouth shut it was not uncommon – yet very valuable – to share in leisurely lunches at the Baltic (Exchange) or in owners’ and brokers’ offices.

The result was that, in the early years of *MPM*, many contributors were able to report results from the field... Then suddenly, in the early 1970s, fundamental changes took place in global shipping. Oil price rises and the blockages of Suez Canal caused tanker rates to boom. Incredibly, new buildings could be paid for completely by two trips, PG–West around the Cape! Owners started borrowing heavily and various financing whiz kids

moved into ship-owning. Severe shortages of seafarers emerged in traditional maritime nations so owners started recruiting seafarers from the Far East...

Then just as suddenly as the rates had gone through the ceiling they fell through the floor – and through the basement! Banks found themselves the not-so-proud owners of re-possessed ships. Ship-management companies emerged to help the poor bankers in their time of need (!) and to take over the operation of vessels owned by the financial whiz kids who knew all about money but nothing about running ships. Compliance with national regulations was a costly inconvenience so ships were flagged out to some good and some not so good flags of convenience; or, as some leading American-Greek owners insisted, to flags of necessity!

(Moreby, 2004, pp. 89-90)

Access to the world out “there” is crucial to most researchers, in one way or the other. It is a necessary condition to performing field research, and can be notoriously difficult to negotiate (Feldman, Bell, and Berger, 2004). Not only due to changes in organisational structures and geographical outsourcing, as Moreby argued above. It can also be difficult because “access” in itself is hard to define. Do you have access just because you are let inside the company door? Evert Gummesson (2000) gave the following example of a conversation between himself (A) and an executive (E).

E: Some type of professor is over here again from the USA wanting to interview us about strategy. He had a long questionnaire with him containing about fifty different factors. He wants to find out which factors are important when we decide to enter a new market.

A: What are these factors?

E: Just the usual sort of thing – market potential, competition, political stability, et cetera.

A: What did you reply?

E: Well, you know, you go through the list and tick off a few factors, show him some marketing plans, and then send him off to meet a few other people. I have no idea what he gets out of it all.

A: You don't seem too enthusiastic about his research.

E: No, it doesn't really work like that in practice, does it? Let me tell you what happened when we decided to enter a country in Latin America. Four of us got together over dinner in New York: a divisional director, the Vice President of R&D, a department head, and myself. We sat and chatted around the problem but just couldn't agree. In the end we had to take a vote: two in favour, one against, and one undecided. Well, that was it; in we went. Two men flew down on the following Thursday to check the lay of the land.

A: Did you tell the professor about this?

E: Of course not! He might have thought we were not serious.

(p. 26)

What managers say they do when for example being in an interview, and what they do when observed do not necessarily coincide. Argyris and Schön (1974) called the former the managers' espoused theories, and the latter their theories-in-use. In Gummesson's example, the interviewee explains that the questionnaire didn't conform with his reality ("it doesn't work like that in practice") but also that what is done "in practice" can be difficult to account for when companies also need to keep up appearances ("He might have thought we were not serious"). In other words, there may even exist reasons *not* to give the feedback that the particular means of gathering data is inadequate. In a similar way, Brunsson (1989/2006) argued that differences in decisions, talk and actions are only to be expected from organisations who have conflicting demands placed upon them; events of "hypocrisy" as displayed in the above conversation is natural part of this process. Some stories from the field may be deliberately hidden (Czarniawska, 1998, p. 21). So, how do you know if you have access? Unless you're already a native, or an employee of the organisation you are studying (Brannick and Coghlan, 2007), you might never know: it is the privilege of the people who were in your study to decide to let you know.

The argument proposed here is not that certain data collection techniques are not useful. People are nowadays quite used to interviewing for example; we live in an interview society (Atkinson and Silverman, 1997). The argument is that results can be difficult to interpret if the setting or the interviewees etc. are unfamiliar to the researcher. Rosaline Wax (1971) argued that only through long-time fieldwork can a researcher generate the kind of understanding necessary for performing research involving people outside your own culture, class or profession; this kind of understanding is a "precondition of research" (p. 13). Moreby argued similarly for beer and kicking down doors:

If, young researcher, you want to investigate real shipping, get into the habit of reading Lloyds List every day and learn who are the real "movers and shakers" in shipping. Becoming friendly with shipping journalists and brokers will be helpful as will mixing with practitioners at trade shows and conferences. Try to keep up-to-date with market trends and respect various cross-cultural courtesies... [I]f you want your eyes opened brush up on your jokes, get in a good stock of calling cards, have a few beers then go and kick down a few doors in London, Piraeus, Mumbai and Hong Kong. (Moreby, 2004, pp. 90-91)

Action research, the method employed in this project, can be seen as a strategy for negotiating access (Gummesson, 2000); "action" can be used as leverage when negotiating a mutual research project, for example by proposing aid in a project that would aim to save bunker fuel.

3.1 Action research and its ethnographic relatives

The term "action research" is used broadly in literature (Cassell and Johnson, 2006). Chandler and Torbert (2003), for example, identified 27 "flavours". Before I offer one of these definitions, I will define action research through relating it to other disciplines.

The closest neighbouring researcher is perhaps the ethnographically inspired organisation researchers, or the "ergonomers" as coined by Czarniawska (1997). Ethno-

graphers go live with a tribe or a community, ergonographers go to an organisation to know more about what is going on there. Action researchers do something similar, but with a different purpose: to test a mutually interesting idea, for example to solve a problem of mutual interest. Action research in the sense used in this project has much in common with management consultancy (Gummesson, 2000).

These differences in purpose is reflected in literature on the researcher's role in the ongoing research project. For example, ethnographers and ergonographers can have periods of anxiety and stress because they do not have a natural place; they need to constantly re-negotiate it (Czarniawska, 2007). In this sense, an ethnographer slowly works his or her way up to being “‘a well-intentioned but harmless researcher’—a euphemism for ‘harmless idiot’” (Czarniawska-Joerges, 1993, p. 127). The action researcher has no such anxiety as he or she is there for a purpose.

Pro-longed visits to the field is no guarantee for access to information of what is going on, however. Ethnographers of organisations sometimes complain that even though they are always present, action always seems to be were the ethnographer is not (Law, 1994). By contrast, an action researcher is where the action is by definition. For good and for bad: the researcher might not succeed with the task at hand, generating a different kind of stress. This can be disappointing for the people expecting positive results from the action, but it doesn't make it useless for the researchers themselves, who can always learn something (e.g. Olesen and Myers, 1999).

A demarcation between action research and certain types of ethnographies needs to be made clear; that between action research and *participant observation*. In participant observation, the researcher is already, or assumes the role of, an insider, getting rid of the anxiety associated with having to enter a new environment. Czarniawska (1998, p. 25) gave a number of examples of such studies, referring to them as “no doubt superior to all other types [of ethnographic research]”. One example is Michael Burawoy (1979) who worked as a machine-tool operator. Burawoy's case can also serve an example of Law's recommendation for symmetry in explanations: he turned the traditional question of “Why do not workers work harder?” into “why do workers actually work as hard as they do?” An action researcher by contrast can also be an insider (Coghlan, 2001), but is there to induce some sort of change; to test some idea for improvement.

There are certain things action researchers share with ethnographers, in particular data collection methods. As such, action researchers

derive their theoretical insights from naturally occurring data in the form of the expressed experiences, views, action-centered dilemmas, actual actions of participants, and events in the life of the practitioners rather than through interviews or questionnaires. (Huxham and Vangen, 2003, p. 385)

When later analysing this data, grounded theory is often used, possibly for the same reason as the ethnographers: because it is also known as the “common sense of field-work” (Czarniawska, 1997), what everyone was already doing before Glaser and Strauss (1967) gave it a name. The grounded theory approach was used in this project with increasing complexity over time, approaching the method of Charmaz (2014).

A major drawback of action research in comparison with other forms of field studies is that the effort required in order to serve a professional role *and* the role of a researcher can be too much. It can become too difficult to gather data in practice: to observe, listen and take notes while also working with something else (Czarniawska, 1998, p. 25). One

way action researchers address this point is that the action aspect is crucial to testing and developing their theory (Gummesson, 2000, pp. 208–210). And in any case, the point is not to act and reflect at the same time, but to work in iterative cycles (Coughlan and Coughlan, 2002). Gummesson (2000) listed three points on these “conflicts of loyalties” between being a good researcher and a good consultant.

- A researcher who concentrates on satisfying the requirements of the researcher role will be, first and foremost, an academic researcher, and considerations related to the consultancy role will be of secondary importance. From the client’s point of view, there is a risk of becoming an inferior consultant.
- A researcher who concentrates on satisfying the requirements of the consultancy role may well come in conflict with the demands laid down for good research.
- Once the research role comes into conflict with the role of the consultant, the latter must be given priority. If this does not take place, the change process will itself be affected by the academic research. This creates an unsatisfactory situation for both the client and for science.

(p. 216)

As for a straight-up definition of action research, I cited the following on what Gummesson (2000) called “management action science” in Paper II to explain my position:

On the basis of their paradigms and pre-understanding and given access to empirical, real-world data through their role as a change agent, management action scientists develop an understanding of the specific decision, implementation and change process in the cases with which they are involved. They generate a specific (local) theory, which is then tested and modified through action.

The interaction between the role of the academic researcher and the role of the management consultant, within a single project as well as between projects, can also help the scientist to generate a more general theory, which in turn becomes an instrument for increased theoretical sensitivity and an ability to act in a social context, a theory that is never finalized but is continually being transcended.

(p. 208)

From this definition, what makes action research unique is its focus on local issues, which, given that the researcher keeps on working on similar issues, leads not to greater statistical generalisability, but to a greater ability to solve new local issues more successfully. By including “increased theoretical sensitivity,” I believe Gummesson is saying that the researcher is not producing necessarily better theories but is becoming better at discussing data in terms of theory as well as improving his or her ability to modify theory in light of new data. To what extent I myself now have a more general theory produced from several local ones is not so clear to me; however, my ability to act and being more sensitive to theory has possibly improved.

3.2 Why action research? Answers in theory and in practice

The conventional way of motivating method would be to say that action research was the best fit for answering the research questions I constructed from my literature review. This would actually not be too difficult.

- Improving energy efficiency in an organisation is a process of organisational change. Action research has been highlighted as superior in terms of studying change processes: “intervention settings can provide rich data about what people do and say—and what theories are used and suable—when faced with a genuine need to take action.” (Huxham and Vangen, 2003, p. 385). It is also an advantage that the researcher is forced to learn the practice his or herself instead of only observing (Gummesson, 2000, p. 80)
- Improved energy efficiency is a crucial part of possible mitigation pathways for the shipping industry to reduce its impact on climate change. Knowledge that would support such a development would be essential. Action research and other forms of transdisciplinary research are often argued to be more useful in informing policy-makers on such issues since the knowledge has been grounded and tested (one could say peer-reviewed) in the community which would be affected by policy (Clark and Dickson, 2003).

There are also more general arguments on the proposed failure of social science as a whole in producing knowledge useful for society, and that action research and similar activities provide a way forward (Funtowicz and Ravetz, 1993; Gibbons et al., 1994; Greenwood, 2002; Hirsch Hadorn et al., 2006). Bent Flyvbjerg (2001), for example, invoked the Aristotelian division of knowledge into *episteme*, *techné*, and *phronesis* in arguing that social science needs to stop trying to emulate natural science in trying to produce predictive and general theory (i.e. *episteme*). Social science as *phronesis*, argued Flyvbjerg, means asking other kinds of questions, such as Where are we going? Is this desirable? and What should be done? Flyvbjerg later stressed the crucial connection that research needs to have to practice.

In public affairs, reason is made capable of action by effectively having reason enter the public sphere and public deliberation. It is reason times exposure in the public sphere that matters, not reason alone.

(Flyvbjerg, 2012, p. 95)

Finally, action researchers often connect to the pragmatist philosopher Richard Rorty (to whom I referred to earlier to explain narrative forms of knowledge). Below for example, as quoted by action researcher Peter Reason (2006).

We cannot regard truth as a goal of inquiry. The purpose of inquiry is to achieve agreement among human beings about what to do, to bring consensus on the end to be achieved and the means to be used to achieve those ends. Inquiry that does not achieve coordination of behaviour is not inquiry but simply wordplay.

(Rorty, 1999, p. xxv)

But action research was not chosen as the result of research questions generated from a literature review. In general, as discussed in Chapter 1, action research can be difficult to

account for in such conventional terms. I was, however, much encouraged by those who pointed out a gap that could be filled with more in-depth studies of energy management practice (e.g. Palm and Thollander, 2010; Thollander and Palm, 2012). Indeed, Herr and Anderson (2014) likened action research to designing a plane why flying it, which very much applies to this project.

The project began with an idea for a collaborative project between myself being employed by the university and two shipping companies. The project goal—to implement an energy management system—was thus set quite early and the actual research aspects were of a second nature. An email from the period preceding the PhD project, from the environmental manager in one of the shipping companies follows below. I had been quite sceptical of my abilities to fulfil (or even of my interest in fulfilling) the academic potential of the project.

Hi Hannes!

I just want to tell you I've emailed [the funding agency] and [your boss] and told them we want a practical project and not an evaluation project of some sort [e.g. where I would be observing their process]. Your boss called me and reassured me that the project will indeed be practical. Where you have the opportunity to work in the project while we will also support, tutor and make decisions. Also the funding manager called and he is very keen on starting this project and he wants it to be clear we can start straight away. Remember it is not [the funding agency] that requires research elements [but your employer]. I've promised them both that we will support with co-financing. Think through it once again.

Kind regards,

[The environmental manager]

I thought it through and went through with it of course—it was much too interesting to turn down. Concluding, I think it is fair to say action research was chosen because it was least common denominator between the wants and needs of the companies' in the project, of the university, and of me. But there was much negotiation preceding the project, for example on just how much “action” they could expect me to do, and how much they were going to do themselves—was I a resource that they could plan with or something else?

“Facilitate?” To my surprise, the senior manager reacted to a certain line in my project proposal.

“Facilitation,” he continued, “is when we work and you bring us coffee. We need more than facilitation.”

In the end, the formulation in the mutual project charter (see Chapter 4) included obligations for the shipping companies as well as the university (“Chalmers expects XYZ, Chalmers offers ABC”). In terms of the shipping companies, we expected that they “independently design and implement an energy management system.”

Other key considerations in the start-up phase were related to concerns that I had about the risk of lacking competence in operational energy efficiency in the project, i.e. someone with practical technical know-how in how to actually achieve improved

energy efficiency in shipping companies. This concern was a stroke of luck: what I should have been concerned over, according to literature, was everything else.

Action research is also challenging for two further reasons: (i) the uncertainty and lack of control creates anxiety for anyone other than confident and experienced researchers; and (ii) doing action in action research demands experience and understanding of methods for consulting and intervention. This second challenge suggests the need to face up to conceptual issues about the nature of problems in organizations and the concomitant demands for change, the nature of a client-centered activity, the issues involved in building and sustaining a consultant–client relationship, and so the nature of power and politics in the context of intervention. As an aside, the above suggests that action research is likely to be a problematic research methodology for doctoral students. (Eden and Huxham, 1996, p. 85)

I will return to these challenges, and add two more, in the next chapter. Interestingly, Eden and Huxham continue with referring to PhD students at their universities who did action research, who had supervisor who were also experienced action researchers. This was apparently not a good idea:

Our experience... suggest that research students can be debilitated by the demands made by those supervisors who have more experience of intervention than they give themselves credit for. Moreover, these supervisors significantly have, through their own research experience, a much more sophisticated understanding of the methodological issues of doing action research than they are aware they have. (p. 85)

Instead, Eden and Huxham recommend that an action researcher is made part of a research team involving the PhD student. Returning to the “stroke of luck”: (i) my supervisor was not an action researcher; and ii) the person who would be providing the technical competence was also a successful consultant. Levin (2009) emphasised “mentor/apprentice” relationships in educating action researchers, which became the case here: not only was it possible for me to discuss with him separately on various issues; he was also contracted to do energy audits in two cases where I could work alongside (the first is reported in Paper 2, the second will be part of a future paper).

3.3 “Action research is a stance”–but what kind of stance?

Up until now, I’ve discussed action research as if it were the main “method” employed in this project. It would be more correct to say that action research was the main “stance” or “research strategy”. The practical tasks of the project were something like the list below, visualised also in Figure 3.1; these will be discussed in the next chapter.

1. See to that an energy management system is implemented in two shipping companies according to a specific standard
2. See to that the people in each company can learn from each other
3. Try to learn as much as possible about energy efficiency in shipping companies from the consultant who is going to be your mentor

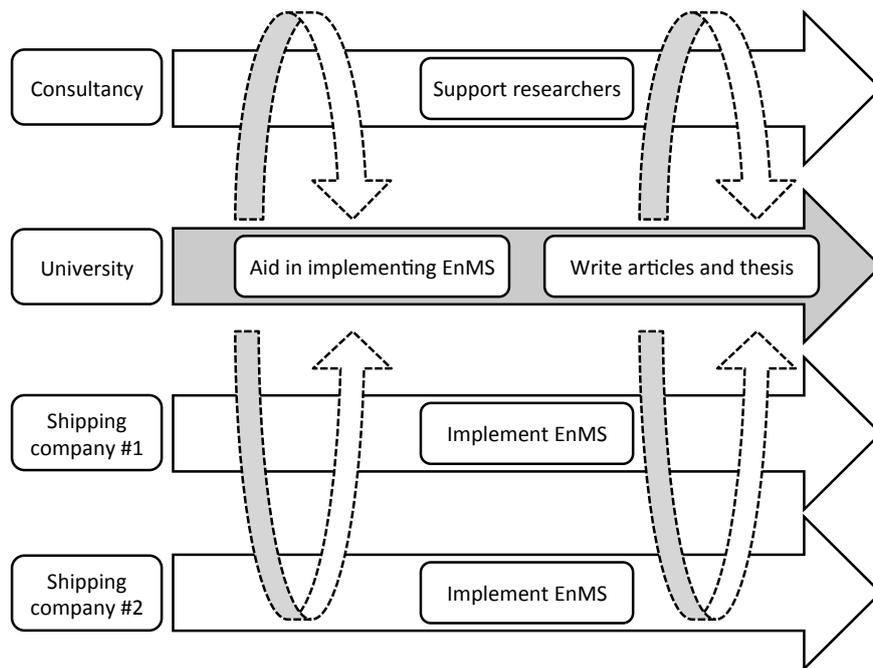


Figure 3.1: The action research process, from Johnson et al. (2014)

4. Do research about the above

As will be evident upon reading the appended papers, a range of tools were employed to collect, analyse and discuss data. In Paper IV, for example, we had to invent a way to determine the potential for energy efficiency due to decreased time in port that used quantitative data already available in a shipping company (Johnson and Styhre, 2015). In this way action research is not so much a technique for collecting data, or even solving problems, but rather more of an overall research strategy. Along the way, I had to acquire a range of other skills, as required, and used both quantitative and qualitative techniques. I will not cover these here but refer to the individual papers.

Finally, what *kind* of stance is action research? If it is *phronesis* and not *episteme*, as I argued above, what are the consequences? How is local and practical knowledge valued compared to knowledge that is general, predictive and abstract? A newly appointed editor rejected a paper after a round of minor revisions with the following motivation.

Unfortunately, and this is something that should have been pointed out to the author at the outset, this paper focuses on the internal organisation and motivation within a small shipping company. Therefore, interesting though the paper is, it has little or no relevance to energy policy at the broader level.

The editor made me realize that despite calls for more in-depth studies from those who were using the barrier framework to support policy-making, performing such studies and discussing the findings was far from trivial. Action research as *phronesis* was a different activity with different kinds of results than conventional research, which are often based upon ideas of natural science practices. But different in which ways; how

do action research practices relate to those of natural science? Greenwood and Levin (1998) argued that action research and “scientific method” has much in common. I’m not so sure.

Contrary to what many philosophers thought for a long time, progress in natural science has had very little to do with following rigorous “scientific methods”. Or as Albert Einstein put it, “whoever undertakes to set himself up as a judge in the field of Truth and Knowledge is shipwrecked by the laughter of the Gods.” (Einstein, 1955, p. 28). Thomas Kuhn, physicist-turned-philosopher-of-science was part of that movement to re-examine progress in science. Rather than progressing linearly, Kuhn (1962/1996) demonstrated that science moved one from one set of practices (“paradigms”) to another, the new being incommensurable with the old. In between, “normal science” was carried out in accordance to the current paradigm. In doing this, Kuhn offered many glimpses of natural science practice. This is one:

Just because he is working only for an audience of colleagues, and audience that shares his own values and beliefs, the scientist can take a single set of standards for granted. He need not worry about what some other group or school will think and can therefore dispose of one problem and get on to the next more quickly than those who work for a more heterodox group. Even more important, the insulation of the scientific community from society permits the individual scientist to concentrate his attention upon problems that he has a good reason to believe he can solve. Unlike the engineer, and many doctors, and most theologians, the scientist need not choose problems because they urgently need solution and without regard for the tools available to solve them. (Kuhn, 1962/1996)

Kuhn’s glimpse is easy to de- and reconstruct (Martin (1990) adapted and quoted in Czarniawska (2004a, p. 97). I find that the resulting negated text describes my own time as a PhD student rather well.

Since I’m not working for a single audience of colleagues, but a larger audience that might not share and may even oppose my own values and beliefs, I cannot take any single set of standards for granted. I must always take into account what other groups or schools may think. I cannot therefor simply dispose of one “problem” and get on to the next, something which can be done more quickly for those who work for a more homogeneous group. Even more important, the interaction of the scientific community with society forces me to concentrate my attention upon problems that I’m not sure whether I can solve. Like the engineer, and many doctors, and most theologians, I need to choose problems without regard for the tools available to solve them, because they urgently need solution.

There you have it. I’ve been doing the opposite of science. No wonder it has been difficult.

4 The results

Everything is there for those who are looking and have the time, that is, can afford to look, and while looking they are free and they find things they never expected.

Tove Jansson

Action researchers, as discussed in the previous chapter, typically point out two main problematic areas for beginners in the field:

1. understanding intervention; and,
2. understanding interpretative research methodology

In this kind of more technical project there is also a third problematic area: that of learning the subject matter:

3. understanding technical aspects of energy efficiency in shipping companies

Either of these by themselves is a profession. Research-wise, (2) and (3) could have made their own disciplinary research projects: one from an engineering perspective (e.g. Baldi, 2016) and the other from social science perspectives (e.g. Guy and Shove, 2000; Palm and Reindl, 2016). Things could have been made slightly more complicated by combining them. This could then have been performed as a more of a detached study, either on a small sample of organisations (Armstrong and Banks, 2015; Schøyen and Bråthen, 2015) or interviews or surveys in a larger sample (e.g. Rehmatulla and Smith, 2015; Poulsen and Johnson, 2016).¹ Finally, combining (1) and (2) could have also have been possible, but in a research team with at least one technical expert.²

But in this project, I tried to do (1), (2) and (3); I've attempted a transdisciplinary project. Being an engineer, I thought that (3) would be most difficult. (1) is just to deliver the facts to the management of the company, right? And (2) is writing it all down later? It would perhaps be appropriate to leave a quote here from the research plan I drafted in Autumn 2010, but it is embarrassingly simple and doesn't say too much.

There is also a fourth problematic area, which became more apparent at the time of writing this kappa:

4. Accounting for an action research PhD project in a text

As a result, there are many ways to fail an action research project.

¹I am aware of the fact that the differences between “interdisciplinary” and the engineering and social science “perspectives” are not clear cut.

²I could have also left academia and done (1) and (3) as a consultant implementing energy management practices in shipping companies. Alas, the company that would have allowed this, and wanted to hire me, had a hiring stop at precisely the right time.

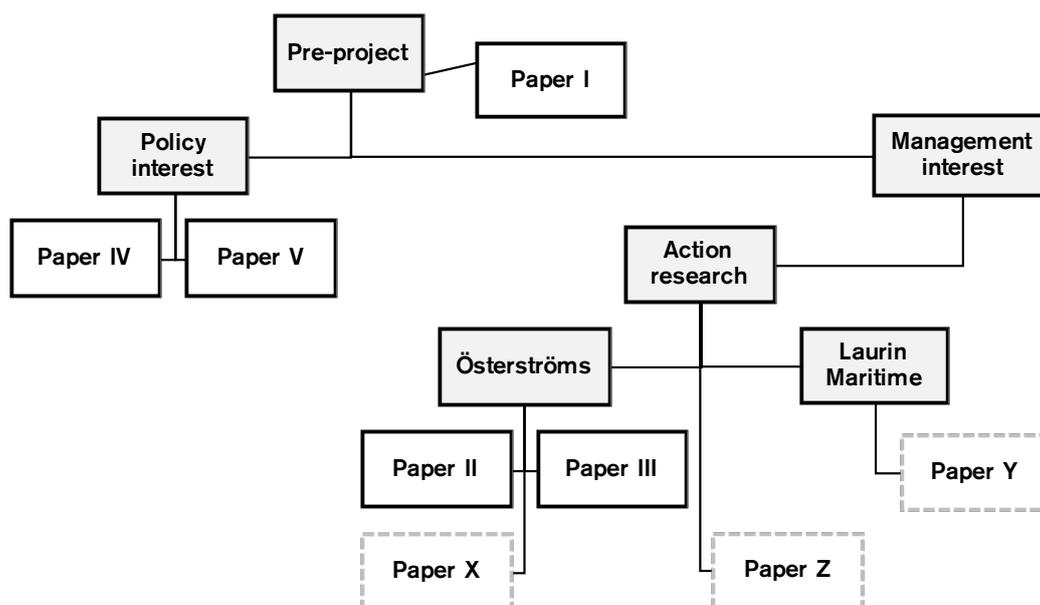


Figure 4.1: Papers in this project (some not yet written) and their connections.

4.1 Motivating the appended papers

There are five papers appended to this thesis. As much as an account for the whole project would have benefited from being in a single, more coherent text, instead of divided into publishable parts, this is what I have now.

The reader who knows something about this project already must be wondering: of all that I could write about, why these papers? The most glaring omission is a paper on the work we did at Laurin Maritime, represented in Figure 4.1 as Paper Y. This omission is perhaps a bit ironic, given that it was only here that an energy management system according to ISO 50001 was actually implemented; it was here we achieved the goals of the project.³ The short explanation is that the individual papers were not planned ahead, in the sense of what topics or events I would write about. Rather, the plan was for them to arise naturally throughout the project, as mentioned in the previous chapter. In other words, more akin to the garbage can model (Cohen, March, and Olsen, 1972) than any “rational” model.

Figures explaining rhetorical or “logical” connections between papers common in PhD theses. I’ve made one myself; Figure 4.1. To enable the reader to make sense of the order of appearance and scope of the papers and in the hopes that it makes the realities of an action research project more clear, I believe that a short story is called for. I finish this chapter by providing the points of the story. The reader could for example take a break to read the appended papers as they are mentioned. Due to the focus this introductory text has on the research process rather than the results, I won’t summarize the individual papers.

³In my defense, I also introduced my “twin” PhD student Francesco Baldi to this company, who was able to publish a range of papers based on his work with them, one to which I made a small contribution (Baldi and Gabriellii, 2015; Baldi, Larsen, and Gabriellii, 2015; Baldi, Theotokatos, and Andersson, 2015; Baldi et al., 2015).

I became a PhD student only very reluctantly. I was on closing in on the end of an un-paid volunteership in a UN organization when I started to run out of funds. An email appeared with an offer to apply for one of two shorter project assistant positions at my past university. I was invited for an interview, and expressed my interest in the position that was allegedly less academic and was not expected to lead directly to further research. It was about exploring the potential role for energy management system standards in shipping companies, that at that time were common in land-based energy intense industries. I was offered and accepted this position.

During this project, I was inspired by research that had been carried out in Swedish industry (Thollander and Palm, 2012). I made a series of interviews in the energy intensive sector as well as in shipping in order to understand to what extent energy management standards, as well as theoretical frameworks and methods used in previous research on land-based industries would be applicable to shipping.

This material later became my first conference paper (Johnson and Andersson, 2011), which even later became a journal article: **Paper I** appended to this thesis (Johnson and Andersson, 2014).

One of the project goals was very practical: to deliver “plans for test implementation of an energy management system in 2-3 shipping companies.” For various reasons, these companies turned out to be Laurin Maritime, a chemical tanker company, and Österströms, a short sea dry bulk shipping company. Both were family-owned; the second generation had just taken over Laurin, while the first and second was working in Österströms. Based on interviews ashore and visits to ships, I produced two short reports on what an energy management system could entail for each company.

The idea to continue the project into actually implementing an energy management system came naturally. After applying for funds, and some arguing which I described in the previous chapter, this continued project could begin. A crucial point was to bring in Mikael Johansson, a consultant from DNV (now DNV-GL) to act as mentor to me in matters related to energy efficiency.

We met, all of us (Figure 4.2) at the DNV headquarters in September of 2010, in Hovik outside Oslo, to get to know each other and agree on a project charter. We started with Figure 4.3, and came up with the following dates:

- Laurin Maritime and Österströms prepare a project charter by 1st of November 2010.
- Chalmers will present a research plan by the same date.
- A “diagnosis phase” will take place until the 31st of March 2011, whereafter an action plan will need to have been decided upon, containing specific energy efficiency projects to test
- A decision on what projects to implement fleet-wide needs to have been taken by 1st of November 2011.
- By Q2 2012 there should be a “certifiable” energy management system

Meeting this first goal was fine; one research plan (albeit in hindsight not very good); and two project plans were produced.



Figure 4.2: Participants at kick-off, Hovik.

For reasons which are explored further in Johnson, Johansson, and Andersson (2014), Österströms wasn't able to reach the goal set for 31st of March, and they decided that they would have Mikael Johansson from DNV help them with the energy review. The review was carried out in April-May 2011. At roughly the same time, it was announced that Österströms would be bought by the larger company Transatlantic, and its operations would be completely taken over by this company.

This made the energy review a suitable “ending” in time in order to produce a paper, which was first submitted to a conference (Johnson, Johansson, and Andersson, 2012), and later to a journal (Johnson, Johansson, and Andersson, 2014), appended to this thesis as **Paper II**. I wrote the paper in its entirety, co-authors gave constructive comments.

The third meeting was held in September 2011. Österströms had tried to keep up with the implementation plan and were on track again thanks to the energy review, but with a time lag. Laurin Maritime had strayed from the plan but in other ways made progress. Their energy review would not be completed until Fall 2012, but the company had already begun implementing continuous monitoring systems for measuring energy use onboard their ships.

In Santiago de Chile where I presented the first conference paper (Johnson and Andersson, 2011) at the 2011 Conference of the International Association of Maritime Economists (IAME), I met with Linda Styhre, who had funding to do a small project on logistical aspects of energy efficiency in shipping. Once back in Sweden, we started drafting a project together based on one

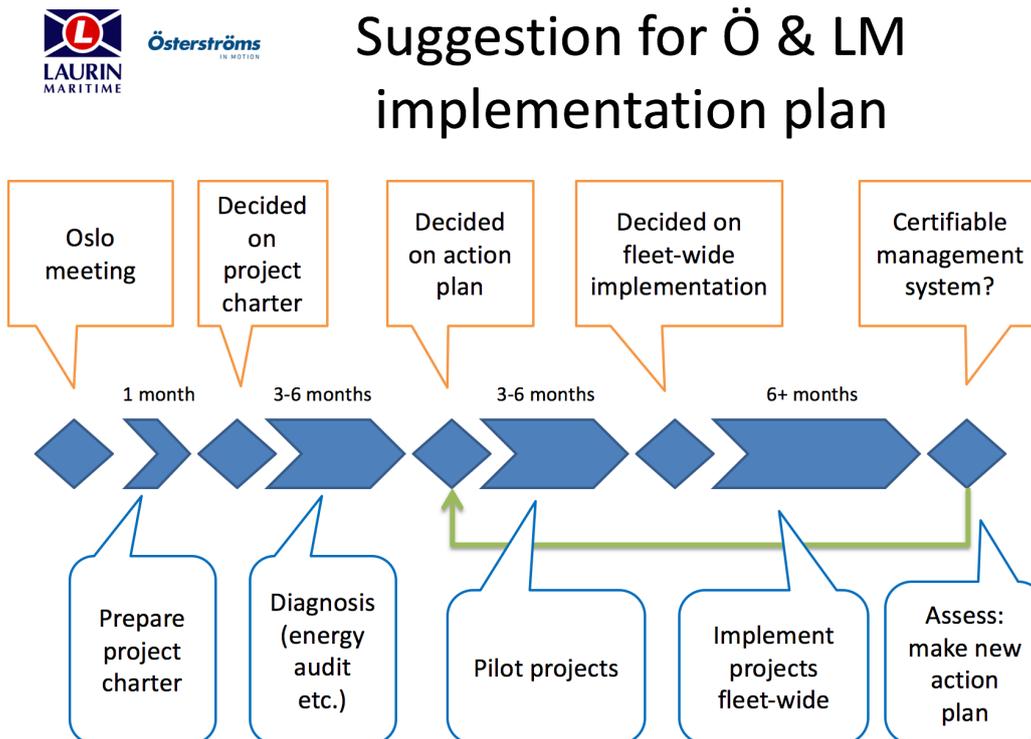


Figure 4.3: From presentation at Hovik.

of the measures deemed very cost-effective in the energy review performed for the short sea shipping company: that of decreasing time in port to be able to slow down while at sea.

This fit perfectly into the merger process that still was going on at that time in Transatlantic; they were still in different offices, and some roles, like that of environmental manager, had two people from each organisation. It fit perfectly because it was a bit difficult to move forward with the whole idea of implementing an energy management system in this new company. It gave me something to do in the meanwhile.

This became a report to the project Linda was working on (Styhre and Johnson, 2013). After expanding and submitting this material for a journal, it was published as Johnson and Styhre (2015), **Paper III**. My specific contribution was to develop the MATLAB script for analysing port efficiency, enter the data and carry out the calculations. I performed, transcribed, and analysed the interviews together with Linda Styhre, and we wrote the paper together.

The deadline for the next year's IAME conference was a short time after I got back from Santiago. I wanted to go to the next conference as well, and thought of a paper idea where we could say something clever about the new Ship Energy Efficiency Management Plan.⁴

This paper was presented at IAME 2012 in Taipei (Johnson et al., 2012), and later also chosen for a special conference issue of *Maritime Policy & Management* (Johnson et al.,

⁴One of two policy instruments introduced to address GHG emissions from shipping.

2013), which is appended as **Paper IV**. I discussed the concept of the paper with the co-authors, then wrote the paper. Co-authors gave helpful comments.

A few months later, the larger company decided to have the DNV consultant perform a new energy review. The previous one was deemed interesting but not applicable to the new company. I could participate and aid the consultant in this project as well when this was performed in June 2012. We presented our results to the board of directors of the company later that August.

The material from this review is still in raw format. I also did a new round of interviews in 2015, three years later, to see what had been done. My intention is to turn this into an article, Paper X in Figure 4.1, but it was not possible in time for the PhD.

During late autumn 2012, the environmental manager of Laurin Maritime retired. We had achieved a lot: we had finished with the management system; we had done the energy review and presented it during a officers conference, and gained approval for fleet-wide installation of energy performance measurement and analysis systems. At the same time, I had a lot of work to do on the kappa to the licentiate thesis, me and Lindas report and also to start discussing a course in maritime energy management on M.Sc. level.

In March 2013, I presented the licentiate thesis (Johnson, 2013).

From then until now, I've had two parental leaves, I've made and carried through a M.Sc. course in Maritime Energy Management, I've taken a course in qualitative methods (which largely determined the direction this project has taken). And that's it. The implementation project was largely phased out, perhaps due to the fact that the system was up and running in Laurin Maritime and because I was never able to find a natural way to start working together with Transatlantic again.

Completely disconnected from the project narrative is **Paper V**. René Taudal Poulsen contacted me in early 2012 to discuss cooperation around his 3-year study on energy management practices in the Danish shipping sector. This paper is the result of his field work and a series of discussions and co-writing.

Finally, I was originally planning on writing Paper Z, on the project in its entirety, which became this introduction.

4.2 Concluding reflections on the research process

In Section 3.2, I listed two reasons for pursuing action research: that it is superior in studying change processes, and that the knowledge produced is more useful for society at large – for informing other shipping companies as well as policy-makers. To answer to what extent this was the case for my project is difficult, as the only option in practice was action research; the companies were not interested in being followed or evaluated. But for the sake of argument, what if I had been given the option to follow a project without anyone requiring my participation? In hindsight, it sounds terribly convenient. The fact is, during my next project about the “misunderstood” business of shipbuilding (Bruce and Garrard, 1999), I will follow shipbuilding processes without any pretensions regarding my abilities to improve said processes. What I look forward to is to have more time to observe and document than what I've had during this project. Like Gummesson

(2000) pointed out, there is often a judgement call between performing the role as a researcher and the role as consultant – and the consultancy role needs to have priority for the action research project to continue.

Action research is highlighted in literature from its inception by Kurt Lewin for the opportunity it gives for a researcher to make a positive impact on a community. In this project, I tested the idea that implementing an energy management system according to the international standard ISO 50001 was beneficial for a shipping company. Ironically, I haven't completed the documentation of the efforts done in the company in which we succeeded with implementing this standard. The company which I *have* documented did not go through with implementing the standard, due to reasons which have nothing to do with energy efficiency.⁵

Concluding, I am not in a position to say if implementing the standard was more helpful than any other way to address energy efficiency. Most shipping companies that do work with energy efficiency seem to do just fine without ISO 50001, Transatlantic included. At this stage can only assess my contribution to the community indirectly – they have not thrown me out yet, so I must be doing something right. In this sense, a clear value of the action research process lies not in the immediate improvement of research results, e.g. in the way some action researchers highlight the way it enables “testing theory”, creating “actionable knowledge” etc. I rather want to stress the way it has enabled me to participate in the local shipping community going forward beyond this individual research project. Introspectively the action research stance has enabled a lot of tacit personal development in the way it has put me and my ability to acquire new competences on the line. I am slowly developing the knowledge Wax (1971) called the precondition of research. It is also very satisfying to be able to point to the world outside my desk in academia and say – “I took part in making that.”

One can ask what is the general value of local specific studies that cannot be replicated, and as the editor who rejected one of our papers stated – a case study of a small shipping company may be interesting but is of no consequence for a discussion on energy policy “on a broader level”. A for me unexpected result of carrying out my work, was that it became increasingly difficult to point out general ideas for policy-makers or management of firms to address in order to improve energy efficiency. Things became increasingly complicated – everything depended on something else – and many aspects of shipping in general that had an impact on energy efficiency looked increasingly rational and hard to change. This is very much the theme of Paper V. Palm and Reindl (2016), in their study of energy efficiency in the planning and design phases of building renovation, seem to have made a conclusion along similar lines.

Using [a practice theory framework] it is possible to understand why the planning and design phase more or less repeated itself and why the doings seemed quite unaffected by the decided energy reduction goal. *There was nothing in the studied processes that could dislodge the inertia of the practice.*

(p. 254, my emphasis)

Critics could possibly say what the anthropologists sometimes point out as dangerous: that the researcher adopts the values of those under study, becomes “one of the tribe”

⁵Transatlantic is a publicly listed company – its efforts to reorganise are well documented and publicly available.

and thus also becomes unable to be critical. But it can also be argued that expecting more, expecting the researcher to be able to solve problems for the specific organisation studied, or a more general policy-maker, is to misunderstand how academic knowledge can contribute to practice. This argument is important also to the action researcher, who hopefully *does* contribute to practice through action, in the way it affects the rhetoric to be employed in communicating the results of such endeavours in academic writing. The argument is that the people you study already know what they are doing. While this may seem a trivial and common-sensical argument, it has far-reaching consequences.

Latour (2005) has discussed the role and the mission of the researcher along the above lines extensively, illustrated here in the form of a dialogue between a PhD student (S) and himself (P). The excerpt below starts somewhere in the middle of their conversation – the student wants to know how to “apply” actor-network theory (ANT) to his study, something Latour argues is to misunderstand what ANT is about. I’ve made cuts [...] for brevity.

S: But I have to make the actors learn something they didn’t know; if not, why would I study them?

P: You social scientists! You always baffle me [...] They are the teachers, you learn from them. You explain what they do to you for your own benefit... not for them, who don’t care one bit. What makes you think that a study is always supposed to teach things to the people being studied?

S: But that’s the whole idea of the social sciences! That’s why I’m here at the [London School of Economics]: to criticize the ideology of management, to debunk the many myths of information technology, to gain a critical edge over all the technical hype, the ideology of the market [...] I have to provide some reflexive understanding to the people...

P: ... Who of course were not reflexive before you came to honor them with your study!

S: In way, yes. I mean, no. They did things but did not know why... What’s wrong with that?

P: What’s wrong is that it’s so terribly cheap. Most of what social scientists call “reflexivity” is just a way of asking totally irrelevant questions to people who ask other questions for which the analyst does not have the slightest answer! [...]

S: But if I have nothing to add to what actors say, I won’t be able to be critical.

P: See, one moment you want to explain and play the scientist, while the next moment you want to debunk and criticize and play the militant... Tell me, Master Debunker, how are you going to gain a “critical edge” over your actors?

S: Only if I have a [theoretical] framework. That’s what I was looking for in coming here, but obviously ANT is unable to give me one.

- P: And I'm glad it doesn't. I assume this framework of yours is hidden to the eyes of the informants and revealed by your study?
- S: Yes, of course. That should be the added value of my work, not the description since everyone already knows that. But the explanation, the context, that's something they have no time to see, the typology. You see, they are too busy to think. That's what I can deliver. [...]
- P: [...] But you are not claiming that in your six months of fieldwork, you can by yourself, just by writing a few hundred pages, produce more knowledge than those 340 engineers and staff that you have been studying?
- S: Not 'more' knowledge but different. Yes, I hope I can. Shouldn't I strive exactly for that? Is this not why I am in this business?
- P: I'm not sure what business you are in, but how *different* is the knowledge you produce from theirs, that's the big question. [...] You want your bundle of a few hundred pages to make a difference, no? Well then, you have to be able to prove that your description of what people do, when it comes back to them, does make a difference to the way they were doing things. Is this what you call having a 'critical edge'?
- S: I guess so, yes.
- P: But you would agree that it wouldn't do to provide them with an irrelevant appeal to causes that make no difference to what they do because they are too general?
- S: Of course not. I was talking about real causalities.
- P: But those won't do either because if they existed, which I doubt very much they do, they would have no other effect than transforming your informants into the placeholders of other actors, which you call function, structure, grammar, etc. In effect, they wouldn't be actors anymore but dopes, puppets—and even that would be quite unfair to puppets [...]
- S: So where do we differ? You, too, want to have a critical edge.
- P: Yes, maybe, but I am sure of one thing: it's not automatic and most of the time it will fail. Two hundred pages of interviews, observations, etc. will not make any difference whatsoever. To be relevant requires another set of extraordinary circumstances. It's a rare event. It requires an incredibly imaginative protocol. It requires something as miraculous as Galileo with his pendulum or Pasteur with his rabies virus.

(pp. 151–155)

To me, Latour was possibly more clear on what being political *shouldn't* be than what it *should* (pp. 258–262). But as for the former, it is clear he means that being political, being relevant for practice or policy, does not come for free; does not come from “applying

a framework”. It requires a lot more work than putting together “implications for practice” or “... for policy” at the end of a paper.

But how much work, and what kind? Following Luhmann (1989/1998), Czarniawska (2015) pointed out that research and practice may be seen as two distinct and closed systems who are unable to communicate with each other. But what *can* happen is that researchers can “irritate” the practitioners, so that they themselves come up with a solution to their problem (acknowledging that this may require, as Latour put it above, “extraordinary circumstances”). But findings such as “[t]here was nothing in the studied processes that could dislodge the inertia of the practice.” (Palm and Reindl, 2016, p. 254) is not a problem (and this is not their argument either). The reports that researchers write should not be directed at the practitioners that they studied – but to others!

In that sense, I’ve been relevant for practice in the way that I’ve initiated and participated in change processes, but the greater – the generalised – value of this kind of work lies in bringing the different realities I started sketching in the introduction closer to each other. By annoying the policy-maker with the messiness of practice as done in Paper V; by provoking academics into becoming interested in such practical matters such as the SEEMP, as was the underlying purpose of Paper IV; by highlighting the need for new forms of cooperation between ship, third-party management, office, and port – and that researchers need to be very mobile and study this across organisations, as discussed in Paper III. Indeed, if I were to write down a theory of good maritime energy management, a great deal of it would concern how to effectively bring together practitioners of the various “guilds” of the shipping business. Following McCloskey, already quoted on p. 17: I’ve made the an “earnest” attempt to contribute to a conversation. The reader will decide to what extent it is “intelligent”.

And finally, I also hope to bring value by writing this thesis; to allow for others to make useful generalisations (in a few select cases, judgments) of my experiences doing action research.

5 The conclusions

Ah, said Rieux, one cannot cure and seek the truth at the same time. So let us cure as quickly as we can. It is what is most urgent.

Albert Camus

“Maritime energy management” – what does it mean? Perhaps something like this

The activities performed from the office of a shipping company, with the aim to have the various activities that affect energy use performed and coordinated on land and on sea in a way that results in more efficient use of energy.

How to study this, a field of practice which has only begun to exist?¹ A field of knowledge which has only just now started to find its way into academic research and educational curricula. Our idea, our solution, was to try to create it in two shipping companies, inspired by the ISO 50001 standard for energy management systems, but essentially, in practice, creating it from scratch. Hence the title, “In search of...” Then we tried to document it in academic form. I can only say we did it – to what extent it was successful, useful, relevant etc. is not for me to say.

Research can be disciplinary. For an example of what that could be like, see the quote from Thomas Kuhn on p. 40. Research can be *multidisciplinary*, gathering researchers from different disciplines so that they can contribute with their disciplinary knowledge on a common problem or topic. *Interdisciplinary* research can be viewed as the merger of a number of disciplines to create a new (disciplinary) field. There are many definitions of *transdisciplinary* research, but most have an element of a definition which transcends beyond research disciplines into the world out there; many also deal with solving problems.

It is time to address the research question. There are many that want more inter- and transdisciplinarity from universities. Research projects are for example funded with the prerequisites of industry co-funding or collaboration. Some calls for going beyond disciplines are described in such positive terms one is inclined to believe the caller has no idea what transdisciplinarity is.

5.1 How is transdisciplinary research made possible?

My best explanation of how transdisciplinarity projects work is that they are not just “done”. They require work – perhaps it is better to say they are “made possible”. Which explains the somewhat awkward question formulation. I will go through three dimensions of making transdisciplinarity possible that in hindsight have been especially important in this project, as supported by the previous chapters.

¹Practitioners and researchers active in the 70's and 80's will say *exist again!*

Transdisciplinary research is made possibly through a process of constant (re-)negotiation with other academics, practitioners and with society at large.

From my deconstruction of Kuhn's text – which I claimed showed that transdisciplinarity is in some sense the opposite of normal natural science practice – one can see that the transdisciplinary researcher quickly runs into problems in dealing with a multitude of requirements and standards, for example of what is good, what is relevant etc. The excerpts I've cut from various reviewers and editors throughout the text can serve as such examples. The two different strategies for performing literature reviews is another – with opposite views of what are academic virtues. A third example could possibly play out during the defence of this thesis. In order to perform transdisciplinary research, strong negotiating skills are required.

In contrast to discussions on requirements on interdisciplinary research, which can take place within an academic discourse, standards for transdisciplinarity are set beyond the reach of the university. As the deconstruction also showed, the problems addressed by the transdisciplinary researcher may not be well-formulated and may even, as was discussed in Chapter 1, be formulated in intentionally contradictory ways. Part of the transdisciplinary researchers' job description is then *not* to rush to pointing out what problem formulation is wrong or right, but rather to show how these are each constructed – in the hope of provoking reflection and hopefully change. Whether or under what circumstances this is appreciated is another matter (Czarniawska, 2001). This can in turn lead to a normative statement by the researcher – why something should be the case – but the constructionist question of *how?* needs to precede it.

Transdisciplinarity requires seeing things as the results of various negotiations performed by others. When academic disciplinary standards for what constitutes a relevant problem for a researcher to address are confounded with many other standards, the researcher needs to proceed with great caution. This has not been the case in the barriers to energy efficiency discourse, where, as discussed in Chapter 2, one has tried instead to bring in all sorts of disciplinary knowledge while keeping the same problem formulation. “A main cause of philosophical disease – an unbalanced diet” wrote Wittgenstein (PI, §593). It is through acknowledging and pursuing plurality in problem formulations that we can move forward.

Negotiating skills are important for gaining “access”. The researcher is paid to do research, but everyone else cannot be relied on participating for the altruistic cause of forwarding the academic knowledge frontier. Regarding negotiating access, however, I probably have little of value to add. My project contained implicit promises of large monetary savings. Agreeing on *how* it should be carried out was difficult to agree upon, as discussed in Chapter 3, but that it *should be done* was not so hard. Initial *and* renegotiated access however, is not a prerequisite for success, as one of the projects showed.

These processes of negotiation result in a specific constitutional logic in transdisciplinary research. I discussed in Chapter 3 that I joined those who argued that the social sciences are stronger when they do not try to emulate natural sciences in attempting to produce predictive theory (Flyvbjerg, 2001). I find social science as Aristotelian *phronesis* to be an intuitive framework in which to make sense of transdisciplinary research. Or as Richard Rorty put it, already quoted on p. 36: “We cannot regard truth as a goal of inquiry. The purpose of inquiry is to achieve agreement among human beings about

what to do, to bring consensus on the end to be achieved and the means to be used to achieve those ends.” The intention is not to portray the transdisciplinary researcher as a pure political activist, but rather as an actor who is open and reflexive about his or her terms of participation in the discussion – in contrast to innocently delivering “implications for policy” as a result of “applying a scientific theory” at the end of a paper.

Transdisciplinarity stands on frail grounds. Negotiations can suddenly fail.

Transdisciplinary research is made possible through a multitude of communication strategies

If transdisciplinary projects transcend disciplines and symmetrically includes knowledge as constructed by non-researchers, how should they be reported? Using what language, publishing in which formats? I’ve possibly been too “conventional”; I’ve chosen to separate reporting done in the world of academia from communication with the local shipping community. I’ve also had to make choices on what parts of the project would suit for a paper. The peer-reviewed articles that came out of the project have likely had little or no influence on the project itself (and especially since we sometimes had to wait more than a year for peer-review). I don’t think very many energy efficiency experts out there have read our papers, but they have invited me for various conversations and projects.

From the definition of management action science by Gummesson (p. 35 in this introductory text), it is clear that part of the knowledge gained in an action research project is tacit, non-publishable, only visible indirectly in the longer term as the researcher improves his or her “theoretical sensitivity” and “ability to act in a social context”.

Transdisciplinary research is made possible through an equal attention to “hard” and “soft” sciences.²

I began Chapter 4 with listing various challenges of action research. The one I underestimated the most was point 2, understanding qualitative methodology. In other words, not so much the actual techniques for analysing data but understanding the kind of choices that need to be made in terms of method and what the consequences of these choices are. While this might seem a trivial point to address, I was taken aback with the sheer volume of work required to read up on basic social science literature, even when making the choice of limiting myself to the quite narrow field that lay in the cross-section between actor-network theory and management and organisation studies.³

There were a number of unexpected positive aspect of reading up on social science literature, beyond trying to getting methodology right. One was getting an appreciation for disciplinary self-reflection, problematization and criticism. I’m not so sure that the lack of papers discussing the limits of the relevance of the advice given to policy-makers

²I don’t really like this dichotomy – was *my* research hard or soft, for example? – but it is a common way of contrasting the natural sciences with the social sciences

³A common way for engineers to be interdisciplinary etc. is, as Baumann (2009) pointed out, to adopt a “systems perspective”. One of the first things that an engineer would learn upon reading up on social science literature is that many left this line of thinking – metaphorically inspired by biology – decades ago. Upon understanding this I was a bit lost for words as to how poorly engineering PhD students are being prepared for work with social scientists. I really had to go out of my way to find this out.

and managers in the energy efficiency field really signifies the engineers' superiority in this matter compared to e.g. management and organisation scholars (Kieser and Leiner, 2009; Hodgkinson and Rousseau, 2009). In Chapter 2, I argued that the barriers to energy efficiency framework had seen far too little of problematizing activities. One of the few critical papers even made certain to stress that they were not actually criticizing the framework. It was also interesting for me to see, as discussed in Chapter 3, that the people who spent the most time in a specific environment to do field research were also the ones expressing most concern regarding whether they had really understood what was going on there, compared to e.g. researchers who employ surveys.

Engineers, on the other hand, have a huge advantage in entering the world of transdisciplinary research projects in the way they can take on projects and problem formulations which are technically complex and "relevant" in a way that a social scientist could not do. Bruno Latour (1996), who by the time he looked into Aramis – a failed personal rapid transit system under development for almost 20 years in Paris – must have been quite used to science and technology studies. Still he needed an engineer by his side to find out "who killed Aramis".

Concluding, transdisciplinary research can thrive when different worlds are brought together. Perhaps foxes are especially suited for such tasks.

5.2 The end

The year 2050 is crucial in many projections of global GHG emissions, as it is a year where many goals are set. The EU, for example, plans to cut emissions by 80% from 1990 levels by this year. Sweden's "All party committee on environmental objectives" ("Miljömålsberedningen") recently proposed that Sweden should have net zero GHG emissions by the year 2045. Meeting these targets will require fundamental and radical but not inconceivable changes in the way we live here, on this planet, together.

GHG emissions from shipping have risen exponentially year by year for decades. International trade grows as fast as the global economy, and shipping carries most of that trade, around 90% by volume. In the absence of more stringent policies – which need to include substantial costs related to GHG emissions – they are expected to continue to rise. Today shipping contributes to around 2-3% of global GHG emissions, but a recent report to the EU suggested that shipping may contribute to up to 17% of global GHG emissions by 2050 (Cames et al., 2015). This report and others have noted that in order to reduce emissions from shipping in a way commensurate with its contribution, fundamental changes are required – beyond technical and operational measures to reduce emissions into "demand-side measures", measures to reduce the increased growth of the sector by reducing the demand for international trade (Bows-Larkin et al., 2015). So far, however, neither countries nor the industry have been able to suggest anything more demanding in the IMO process than a system to *measure* emissions. One can applaud the work that has been done so far, but that doesn't make the mountain of work left to be done any more easy.

Incidentally, the year 2050 is about the same time as I am expected to retire. On the 21st of March 2050, I will turn 68. I have two children now; I might have grandchildren by then. I would much like to be able to look them in the eye, and to be satisfied with what we achieved. We will be in dire need of science, of course, but if Feynman was right – as quoted on p. 13 – we will also need a lot of its opposite.

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