

Knowing and Living in Academic Research

Convergence and heterogeneity
in research cultures
in the European context

Edited by
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Ulrike Felt

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Institute of Sociology of the Academy of Sciences of the Czech Republic

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Convergences and Heterogeneity in Research Cultures
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Authors:

Alice Červinková

Lisa Garforth

Anne Kerr

Dagmar Lorenz-Meyer

Susan Molyneux-Hodgson

Tereza Stöckelová

Contributors:

Jutta Ahlbeck-Rehn

Magdalena Górska

Ismo Kantola

Zuzana Kiczková

Ľubica Kobová

Anne Kovalainen

Marcela Linková

Morgan Meyer

Seppo Poutanen

Veronika Řepíková

Lisa Sigl

Mariana Szapuová

Veronika Wöhrer

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GENDER**
:
an east-west comparative study

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Preface

The Contexts of Knowing

Matters of knowledge, of institutions and of gender are inextricably linked and not easy to tease apart. The research presented in this book draws attention to the complexity of the linkages, yet reaffirms both the possibility of generating knowledge on these relations, and, the key importance of doing so. The implications of this work should be felt at many levels within the academy and well beyond.

Every decade someone declares a crisis in some aspect of science. The fact that this has happened for many decades is an indication of the intractability of the issues that the research described here is dealing with. Whilst numerous policies and initiatives come into being, have some effect (sometimes), then fade, there remain even more numerous questions that endure. Concurrently, the nature of politics and of policy is changing because the world is changing. Radical shifts in how policy is informed, formed and accomplished require all of us to re-evaluate our understanding of the reproduction and maintenance of particular sets of relations. The task of understanding these relations is acute within the arenas of science, given the centrality of the scientific project within the world.

So, what produces knowledge? The approach one takes to answering this question will depend on what one understands as 'producing' and as 'knowledge'. Crucially, it will also depend on where you look, who does the looking and the optics through which the view is taken. Within this book and the project upon which it is based, notions of contexts and cultures provided the lenses through which sense could be made of a huge range of knowledge production processes. From everyday practices in bioscience laboratories, to the circulation of discourses between academics' offices, the institutional structuring of viable careers, self-descriptions of being a researcher, to issues of competition and excellence across Europe. The range and vitality of the issues addressed by this work attest to the power and import of such collaborative research projects.

Many countries within Europe have seen the rise of managerialist and other interventionist approaches to the work of the academy. These approaches and policies are responses to contexts and cultures, and have effects on the ongoing contexts and cultures into which they seep. We need to continue to ask upon what evidence these approaches are based. And, how do they become enacted and have effect on the ground? As just one example, despite numerous years and a plethora of policies and laws, the 'equity credentials' of science remain questionable. It continues

to be difficult to see what has changed, for example, in the gendered distribution and character of scientific work.

This book arises from research conducted across five countries, into the production practices and contexts of several disciplines. Whilst other reports and projects have touched on aspects of similar concern, this work differs, indeed rises above, others in a number of ways. For example, it articulates some of the relations between the fine texture of knowledge making work and the broader sweeps of policy. On the one hand, understanding the creation, nature and deployment of ‘policy’ is central to any attempt to analyse knowledge, institutions and gender; on the other, analysis of practices and meanings is essential to making sense of policy effect and policy change.

Through the categories of Boundaries, Time and Togetherness, we are offered a way into this complex web of relations. The richness these themes provide cannot be underestimated and they provide new ways into understanding central policy concerns. Inevitably, absences are created by the choices of analytical categories. Thus, the researchers’ main themes highlight matters of crucial importance, yet at the same time mean that other issues are less attended to. However, one should not interpret this lack of emphasis as a lack of significance. Rather, it is the case that all categories are imperfect, and thus fashion visibilities and invisibilities.

Having stated upfront that ‘relations’ between knowledge, institutions and gender were a focus of the project recounted here, we nevertheless need to be mindful of what kinds of things can be brought *meaningfully* into relations with each other. It is not necessarily the case that anything can be related to everything, nor that what counts as a ‘relation’ has universal meaning. What hope therefore for the notion of ‘comparison’ in such collaborative projects? The original intent to somehow bring ‘East’ and ‘West’ into a form of relation – by anticipating difference between the contexts and cultures implied by these geopolitically influenced words – was found in the end to offer little that was meaningful. Similarly, the term ‘gender’ would prove to be (unsurprisingly to many) problematic. When conducting research within nations, within Europe, within a globalising knowledge economy context, the identification of *important* patterns can become an exercise in disregarding those terms and categories that appear to make so much sense to policy makers and bureaucrats at all levels of governance. What gets counted is not always what counts.

Instead, the work here can be understood as the gathering and re-telling of stories; of different countries, different institutions, different disciplines, different labs and offices. What stories get to be told is dependent on different cultures and logics in different places. In practical terms, this

may mean that which teams and individual researchers could and could not have access to, such as “physical spaces” and “collaborators’ time”. Conceptually, it implies that each of us will see and hear some stories louder than others. Perhaps comparison, then, may reside not in the comparing of data, or results or findings, but in consideration of what questions it even made sense to ask in the first place. In this way, it is the questions that were asked within the research, as much as the questions that their reporting raises, that signal the patterns to which policy makers and others should attend.

In the closing sections, the authors provide ‘policy recommendations’. Given the complexity of relations between knowledge making and policy making – some of which we are already aware, others which this report illuminates – what future is there for this project and this text, one of its many products? In my view, the strength lies in how it tells stories that relate matters at micro, meso and macro levels. In taking this approach, the work creates openings where previously there were assumptions; and it highlights partialities where universalism had been presumed valid.

Finally, any work that attends to matters of knowledge production needs also to reflect on its own processes and practices of knowledge production. Indeed, we, the researchers, are products of the systems that were studied. This reflexive task too, is begun in the pages of the book. We need to draw boundaries around our work (what did we mean by ‘social science’, for example?); we have a ‘timed’ project (but drew on experiences and work beyond those times); we performed different kinds of ‘together’. The reflexive project is central to the epistemological stands that informed the researchers throughout. From my own position, this stand is exemplified by the words of Donna Haraway:

“I am arguing for politics and epistemologies of location, positioning, and situating, where partiality and not universality is the condition of being heard to make rational knowledge claims.” [Haraway 1988: 589].

I hope that the valuable work achieved as part of this project will be heard and that all those involved will continue ... continue to ask questions, to draw together stories and to reflect on the vital project that is *knowing*.

Susan Molyneux-Hodgson
University of Sheffield, UK
November 2008

Chapter I

Knowing and Living in Academic Research

Ulrike Felt

During my early studies I had no idea about research – I had no scientist in my family, everything was new and I had never thought about it. Then I did my Masters, I struggled with the technical work ... but I realised it had grabbed me, it wouldn't leave my head anymore. ... I stayed in the same lab for my PhD, but then the circumstances were not good and I left. I changed my subject ... I realised I liked it ... I got good feedback. ... I didn't reflect much on if I was really suitable for research. It was a different time; there was nothing like career talks and no strategic future planning.

A female senior researcher (biosciences)

The multiple articulations of the relations of knowledge production, institutions and researchers in changing academic research environments are at the centre of this study. As the quotation nicely captures, this implies talking about coming into and being in a field, about places, structures, time regimes, relations, interests and people. This book is about change, about “a different time”, about futures, pasts, and about the master narratives that generate, frame and accompany the present. And it is about gender in all this. It aims at investigating how institutions of research and society at large, with their different histories and contingencies, frame contemporary academic knowledge production. It explores how policy structures research, while also reflecting the ways in which policies are simultaneously articulated along with imaginaries of economic and societal development. The latter is understood as closely linked to the future potentials of knowledge, and thus also to potential, partly contested and contradictory, futures. It is about places of research, and how they open up or close down possibilities for being a researcher and doing research. It addresses the issue of spaces – social, epistemic and symbolic ones – in which collective and individuated ways of working emerge and find expression in the multiple forms of

knowledge produced. But it is also about the partly contradictory time and funding regimes structuring academic lives, how they are imposed upon, performed, resisted, managed and reconciled in different places and moments. Yet, above all, this book is about researchers, how they live in academic research, how they inhabit the different cognitive and material landscapes and participate in giving shape to them, how they organise their social, spatial or temporal environments and are organised by them. We address how they embody the norms and values of their workplace and the epistemic culture they are part of, while, at the same time, also resisting and disputing them, how they make and break social ties, and move in and out of places. Eventually this study is also about the widespread myths researchers share on what it means to be a researcher working in a field or participating in creating new ones. The genesis of this book is rooted in a European project¹ – KNOWING: Knowledge, Institutions and Gender – that gathered researchers from five European countries to investigate the complex and multilayered relationships between researchers, knowledge production and institutional contexts under changing research conditions. Given the multiplicity of forces at work and complexity of the phenomena to be observed, we decided to employ a “comparative optics as a framework of seeing” [Knorr Cetina 1999: 4] in our analysis, on mainly two levels: The first relates to geopolitical locations within Europe. Choosing Austria, the Czech Republic, Finland, Slovakia and the UK allowed us to cover an interesting spectrum of histories, traditions and cultural

1 KNOWING – Knowledge, Institutions and Gender. An East-West Comparative Study. FP6, SAS6-CT-2005-017617.

As editor of this book, my thanks go first of all to all those researchers in the biosciences and the social sciences in the five participant countries who have given us their time and shared their thoughts with us; without them this project would not have been possible. I would like to express my gratitude to the project collaborators for sharing their insights, discussing, commenting on various versions of the manuscript and, above all, for having been supportive throughout the process. Susan Molyneux-Hodgson and Helen Longino accompanied the whole project with their most valuable and generous advice, making it an intellectually challenging and humanly delightful exchange. The book also owes a lot to Martina Merz for her critical and supportive reading as a reviewer.

Special thanks go to Marcela Linková, the coordinator of this project, who steered us through all the difficulties of such multinational knowledge enterprises, made valuable input, cheered me up when things seemed all too complex and reminded me gently and with a lot of humour that something usually labelled “private life” also existed. My thanks also go to my colleague, Maximilian Fochler, who was not part of the KNOWING project, for being my intellectual sparring partner as well as for his critical “external” reading of the introduction and parts of other chapters. Last but not least, I would like to thank the two guys central to my life – Sébastien and Yves – for their love, tolerance and support and for allowing me to carve out my epistemic living space and inhabit it in a way that gives me pleasure and satisfaction.

contexts in which research takes place and to reflect European diversity. The second level focuses on two epistemic cultures: on specific subsets of the 'social sciences' and the 'biosciences'. We use these generic terms without claiming that each of them is a homogeneous territory but rather to make our analysis sensitive to differences between sets of practices, self-understanding, epistemic histories and relations to society.

1. Epistemic living spaces

In a nutshell, we are interested in grasping how researchers imagine, encounter and (re)produce what I would label *epistemic living spaces* [Felt 2007]. Through developing this conceptual frame we try to draw our attention to the multi-dimensional structures – symbolic, social, intellectual, temporal and material – which mould, guide and delimit in more or less subtle ways researchers' (inter)actions, what they aim to know, the degrees of agency they have and how they can produce knowledge. But the notion of epistemic living space also wants to capture dimensions such as feeling intellectually and socially 'at home', holding an understanding of the often non-codified sets of values which matter, feeling subjected to, being part of and performing certain temporal regimes, tacitly sharing a repertoire of practices to address knowledge questions, adapting to specific often complex funding arrangements and many more. It addresses the intertwinedness of the personal, the institutional, the epistemic, the symbolic and the political. As a consequence, epistemic living spaces are always both opening up and closing down possible degrees of agency; they create the feeling of being on safe ground from which unknown territories may be explored and claims made, while, at the same time, they impose limitations; they give tacit guidance while they simultaneously potentially curtail more radical forms of innovation.

Our use of the concept of epistemic living spaces also aims to draw attention to the constant reordering taking place, as well as to make us alert to the continuous efforts of stabilising, extending or protecting the spaces researchers occupy. Thus we observe what Thomas Gieryn [1995] has labelled boundary work, i.e. instances in which boundaries, demarcations or other divisions within and around fields of research and knowledge are created, dissolved, advocated, attacked or reinforced. This work is performed simultaneously by researchers, institutions, policy makers as well as by society at large. Talking about epistemic living spaces tries to point at the messiness, embattledness and practical significance of what seems to delimit researchers' capacity to act in, to think and imagine research. It should also prevent us from naively using

seemingly clear distinctions like those captured in the notion of “work-life balance” which seems to delimit different spheres of researchers’ lives and conceals some of the complexities of working and living in/with research. And it makes us aware of the importance of the more implicit dimensions of ‘being in a field’ and of the multiple tacit orders omnipresent there. Finally, using this notion of epistemic living space also points at the fact that in this book we want to move away from narrowly focusing on the core knowledge-producing activities, and to direct our attention to the many different ways of living in a field both in its more global dimensions as well as its local reconfiguration.

Knorr Cetina’s [1999] concept of “epistemic cultures” resonates with what we aim at doing in this study. “Epistemic culture” captures “the strategies and policies of knowing that are not codified in textbooks but do inform expert practice” [ibid.: 2]. Rather than talking about discipline or scientific speciality, this term seems to be better suited to “make visible the complex texture of knowledge as practiced in the deep social spaces of modern institutions” [Knorr Cetina 1999: 2]. Our interest in practices and in the ways in which they order potential action within these knowledge-producing contexts, as well as our attention to the more symbolic components of research, would thus nicely go together with Knorr Cetina’s understanding. Yet we aimed at focusing our analysis on aspects that got less attention in the epistemic culture approach: the broader framing of research through policy discourse and practice, the societal imaginaries that penetrate the research world, the changes in the monitoring and assessment practices and many more institutional aspects and normative imaginaries, such as mobility, speed, excellence, etc. Thus epistemic cultures – both global and local ones – play their part in our investigation. However, we understand the “machineries of knowledge production” [Knorr Cetina 1999] in a much broader sense including beyond the epistemic, also the social, political, structural, temporal and institutional machineries. In looking at the social sciences as well as the biosciences, we both analyse the different architectures of epistemic environments, as well as how differently socio-political and institutional machineries are at work there. We will thus focus on the “multiplicity, patchiness and heterogeneity of the space in which science works” [Pickering 1992: 8]. This will allow us not only to grasp the different epistemic cultures, but also the multiple other structuring forces and thus understand and give a more fine-grained meaning to what the “disunity of science” [Stump, Galison 1996; Hacking 1992] might actually mean in every-day research contexts. Finally, we aim to understand such epistemic living spaces through focusing on the perspective of the researchers, which allows us to grasp

the subtleties of the 'personal' and how it gets entangled with epistemic and more structural elements.

Consequently our attention is not so much centred on science as the assemblage of objectively validated, temporarily stable knowledge, but much more on research as the activity which leads to the very production of scientific knowledge. Here we embrace Latour's [1998] critique of focusing too much on the culture of science while knowing too little about the culture of research. He distinguishes between "science" and "research" as follows: "Science is certainty; research is uncertainty." – How do researchers deal with the tension between promising outcomes and the profound impossibility of planning them and thus also careers? "Science is supposed to be cold, straight, and detached; research is warm, involving, and risky." – How are involvement, the emotional entanglements and struggles handled on an everyday level? "Science puts an end to the vagaries of human disputes; research creates controversies." – How to deal with contradictions, tensions, hierarchies on institutional and cognitive levels? "Science produces objectivity by escaping as much as possible from the shackles of ideology, passions, and emotions; research feeds on all of those to render objects of inquiry familiar." – What place is given to engagement, feeling or belief in making choices? [Latour 1998: 208] In Latour's account, the social has been absorbed in the scientific, and research has become an inseparable hybrid socio-epistemic entity. Thus it will be essential to our work, to look at the messiness, at the uncertainties in the social and epistemic processes, at the involvement, the taking of risks, the passion and emotion at work when producing knowledge and living in research.

Gender dimensions are omnipresent in the analysis of epistemic living spaces, though often in rather tacit ways, woven into the social, institutional and cognitive tissues academic life is made of. It thus does not seem adequate to treat gender as a separate, clear-cut and well-delimited category to be elaborated on each level of analysis. Rather it seems much more promising to understand gender as something more vague and indefinite, that gleams through many of our observations, that is being de- and reconstructed simultaneously in different places and at different moments, is imposed, performed or refused as an explicit category, thus creating effects which matter. Gender is thus addressed explicitly through the issue of women being underrepresented and in need of special support actions, while at the same time gender is 'neutralised' or silenced through dominant norms in the research system such as rationality and universalism [Heintz, Merz, Schuhmacher 2004: 13]. It is precisely this indefiniteness and diffusion that enables gender markings – as we will show in several places in the book – to

do important ordering work. Hence gender needs careful reassembling work in order to be made visible.

Throughout our analysis, we inscribe our reflection into the corpus of feminist literature that has drawn our attention to the gendered dimensions of working in research, to the situatedness of knowing and to gender as a major mode of ordering. Thus we assume that gender is an integral and inextricable part of the ways in which research is organised and performed, focusing our attention on the multiple more or less visible structural aspects of research – both on the policy level but also in its institutional realisations –, on the ways in which contemporary institutions of research imagine academic work, how value structures get institutionalised and become a dominant part of researchers' imaginaries, how careers are conceptualised and implemented and how time regimes and accompanying imaginaries are performed and what all this means in terms of gender.

Even though these more structural aspects of gender relations in research are the central focus of our analysis, it seems essential not to forget that the very ways in which science works – issues addressed, concepts, theories, methods and practices – have been largely based on and built up in a highly gendered environment [Shapin, Schaffer 1985]. Thus not only feminist epistemologists, but also scholars in science and technology studies, have highlighted the importance of engaging with concepts of knowledge which are much more situated and context-sensitive and consequently would lead to more socially responsible epistemic agency. Such approaches underline the importance of the knowing subject and stress that what and how we know is largely shaped by our respective subject-position. These theorists have drawn our attention to historically changing relations of power in which knowledge-production is situated. They highlighted the central role of epistemic communities and cultures for the generation of knowledge and pleaded for the necessity for more open epistemic negotiations across a variety of discursive spaces of the socio-political world. Thus we need to systematically recognise that knowledge is produced through interaction of researchers with the world to be investigated, as well as among themselves, within their respective communities. This means that when certain perspectives are not included in such a community or are not granted equal intellectual authority, this also has an impact on the community's cognitive practices as well as the methods and conclusions researched [e.g. Rose 1994; Schiebinger 1999, 2008; Keller, Longino 1996, Longino 1999, 2002]. For our analysis, this implies that the ways in which researchers conceptualise, experience and enact their epistemic living spaces is closely intertwined with their possible ways of knowing.

2. The co-production of research and society

Jasanoff's [2004] notion of co-production is of key importance for our understanding of the relation of epistemic living spaces to scientific knowledge production. The idiom of co-production stands for the close intertwinedness of science and society and draws our attention to the idea that "the ways in which we know and represent the world (both nature and society) are inseparable from the ways in which we choose to live in it" [Jasanoff 2004: 3]. Thus, without claiming to be a full-fledged theory, this approach attempts to interpret and account "for complex phenomena so as to avoid the strategic deletions and omissions" which might result from focusing too much on the entities "science" and "society" while neglecting the entanglements of both. Transposed to the context of our research, living in the world might take on at least two distinct meanings. First it means inhabiting an epistemic living space. The way people choose to inhabit this space is closely intertwined with the institutional embedding, with the discursive framing of what it means to do research as well as with narratives and imaginations of the objects they are working with and the knowledge to be generated. Stretching Jasanoff's reflections, we could argue that in this epistemic living space "knowledge and its material embodiments are at once products of social work and constitutive of forms of social life" [Jasanoff 2004: 3] in academia.

In its second dimension, living in the world in our study, on the one hand, means researchers' being part of society at large as individual citizens but, on the other, also points to the ways in which contemporary research systems and thus researchers are framed by society. Discourses on the knowledge society/economy, the role attributed to research-driven innovation in the advancement of contemporary societies, the importance attached to knowledge work and knowledge workers, all these are important forces shaping research. But also changes taking place in the world of work, more broadly speaking, such as the transition to more flexible work structures, shifts in the way time is organising, aligning and accelerating our lives (e.g. time/output relations, the central role played by future scenarios, projectification) or how accountability is built into contemporary societies – catchword audit society [Power 1997] – interfere with epistemic work and its outcomes. In that sense, and we will argue this in more detail below, co-production of science and society happens, as society seems to rely heavily on scientific knowledge while, at the same time, this knowledge is framed by societal imaginations and expectations as well as structural conditions.

The researcher is a key figure in these entanglements of science and society. Indeed it is essential to stress that scientists always – willingly

or not – assume at least a double role: being a researcher and being a citizen, living in two closely intertwined cultures. Already as early as 1935, Ludwik Fleck [1935] addressed this issue in underlining that each individual is part of multiple thought collectives (sharing certain thought styles) that overlap and hybridise. As a researcher, one is part of a community within which work relations are shared, but at the same time one also might belong to a political party, come from a certain social background, country or place. In a similar vein, Nowotny and co-authors [2001] argue for the need to recognise that “social knowledge is already present in what they [researchers] do”, in both the processes of knowledge production, but also in the knowledge itself. Thus necessarily any research scrutinising “the place of people in knowledge must include scientists” [Nowotny, Scott, Gibbons 2001: 235]. Wanting to understand how society leaves traces in research thus means dealing in a fine-grained manner with the ways in which researchers narrate their knowledge-producing activities, with how they live in the field, what practices emerge, as well as with their efforts of making sense of all this. Thus we could conclude with Jasanoff [2004: 2-3] that scientific knowledge “embeds and is embedded in social practice, identities, norms, conventions, discourses, instruments and institutions – in short, in all the building blocks of what we term the social”. In terms of our research, this means that we will carefully assemble observations not only on the ways researchers organise their work socially and epistemologically, on how they experience, handle and structure time in research, on the multiple implicit and explicit value systems which are at work, on the symbolic maps they use to orient their epistemic and social positioning in the field, but also on the larger imaginaries and narratives deployed by diverse policy actors and how all this intertwines with knowledge production.

3. New modes of knowledge production

A closer look at the major systemic changes in how research is organised and how it is governed institutionally in universities and other academic research institutions is crucial for understanding the configuration of epistemic living spaces. Over the past years the most influential debate, both on the policy level as well as in academia, has been the one around “the new production of knowledge” or “mode 2” knowledge production, triggered by Michael Gibbons, Helga Nowotny and co-authors [1994, 2001]. Underlining the deep entanglement of knowledge production and societal imaginations, their central argument gravitates around the observation that knowledge production is undergoing a process

of change. Above all, this change is characterised by stronger societal contextualisation of research and its outcomes. Research thus should be understood as “part of a larger process in which discovery, application and use are closely integrated” [Gibbons et al. 1994: 46], as no longer strictly confined to disciplinary and institutional territories, and as bringing about experimental processes which are “increasingly guided by the principles of design, originally developed in the industrial context” [Gibbons et al. 1994: 19]. Beyond this, research is also revising its time structures in fundamental ways, fostering focused temporal cross-disciplinary collaborations, mobility of researchers and omnipresent reflections on potential futures.

The authors argue strongly that, along with these basic changes in the production of knowledge, the ideals of “a context-free and universal science and its objectivity are unlikely to have much appeal in the future”. Much rather, they highlight that “the authority of science ... becomes more closely tied to concrete practices, their results and impacts. Reliable knowledge ... will be tested not in the abstract, but in very concrete and local circumstances” [Nowotny, Scott, Gibbons 2001: 117]. Context-sensitivity of knowledge, also termed “socially robust knowledge”, thus becomes a central concept, which needs to be considered when reflecting on change in research. Paraphrasing Latour [1997: 232], such an approach to science may lead to being liberated from “the politics of doing away with politics” in science and to openly consider the mutual implications of science and society – an approach embraced by the KNOWING project.

Yet, as many of the critics have expressed, these reflections and diagnoses propose a quite radical discontinuity in academic development while lacking more fine-grained empirical grounding. They homogenise change, and at times it remains unclear whether these findings are to be understood as descriptive or prescriptive [e.g. Shinn 1999; Pestre 2000; Weingart 1999]. Something all the mentioned critics hold in common is the fact that much of the mode 2 analysis offered could be read as having a quasi-political function, “a partisan political agenda and ideology” [Shinn 1999: 172]. For approaches such as the one taken in this book, the challenge is hence to observe and analyse the multiple forms and formats of change – and to ask what concepts such as mode 2 knowledge production might actually mean in and for different epistemic, institutional and national contexts. This implies moving away from a homogenised and homogenising vision of change in epistemic living spaces to a more subtle reflection on the variations in and multiplicity of research environments and what this might produce both as effects on researchers and also on the knowledge

produced. Putting heterogeneity at the centre of our attention, we could thus follow Law [1992: 2] in expressing the interest of our research as understanding the "process of 'heterogeneous engineering' in which bits and pieces from the social, the technical, the conceptual and the textual are fitted together, and so converted (or 'translated') into a set of equally heterogeneous scientific products".

4. Places of knowing and maps for orientation

Drawing attention to heterogeneity means also putting the physical, geographical and symbolic arrangements at the heart of our study. As Livingstone [2003: 179] reminds us: "Like other elements of human culture, science is located. It takes place in highly specific venues; it shapes and is shaped by regional personality; it circles the globe in minds, on paper, as digitalised data. For these reasons alone science is as conspicuous a feature of the world's geography as patterns of settlement, the distribution of resources, or the configuration of cultural landscapes." Taking on such a perspective means addressing the power of different geographies at work, while simultaneously understanding them as cultural maps of science [Gieryn 1999] about which a constant struggle takes place. This makes obvious what Benedict Anderson [1996] has taught us: maps, and thus also the symbolic maps researchers and policy makers refer to, never simply represent something pre-existing. Rather maps produce what will come to be regarded as 'reality', they are the model for what they simply pretend to represent. Those who participate in developing such maps hold power and often become invisible architects not only of social, economic and epistemic orders in science but also of moral ones.

The (symbolic) maps that will appear in this book refer to very different sets of coordinates, organised according to categories such as buildings, institutions, countries or regions. Accordingly, perceptions of similarities and differences will emerge (e.g. countries might share a part of history, research institutions run through similar reforms, or institutions delimit their development from those of other institutions), become visible and give meaning to social organisation, research processes and the output produced in different places. Places have also physicality, which invites reflection on how they have been constructed, what imaginations of knowledge production are inscribed in them, how they open up or close down potential dynamics. Finally, places come into being not only because they are named, identified and represented, but also because they are recognised and referred to.

We thus will, arguing with Gieryn, try to understand how place matters in researchers' accounts of being and doing this together,

i.e., what we call forms of togetherness, how it becomes a force with “detectable ... effects on social life” [Gieryn 2000: 466] and how it is tied to imaginaries of epistemic work practices. Places express the (power) relations of people inhabiting them, while at the same time only taking form through a continuous negotiation between “material forms and interpretative understandings or experiences” [ibid.: 471] of those who inhabit them – in our case, researchers. “Places bring people together in bodily co-presence”, which may cover a repertoire ranging from engagement to estrangement” [ibid.: 476]. Research places thus might spawn collective action while at the same time they could allow for micro-spatial separation. But place also shapes the way behaviour is classified as adequate or deviant; it allows for specific forms of social control. In our case, for example, what is felt as adequate time and timing, continuity and rupture are strongly tied to the place where research is performed. And finally, place is closely linked to constructing and continuously reworking identity, performing memory and creating commonly shared values.

5. Institutions of knowledge production

Beyond these broader reflections, the institutional contexts in which research takes place need consideration – in our case academic research institutions (universities and academies of sciences). They are the central nodes where many of the observations made so far converge. At the time this book is being written, quite a substantive patchwork of reordering and reforms have taken place in many countries recently, or are still happening. In many cases, this concretely means the State backing away from its role as central direct financier of research, which leads to the need for considerable third-party funding and to the emergence of entrepreneurialism on different levels, such as the introduction of more flexible employment practices or curriculum adjustments to the (imagined) demands of the labour market. Managerialism is palpable, through the introduction of multiple assessment exercises, monitoring tools and a dense discourse on efficiency and accountability (see for example [Deem, Hillyard, Reed 2008]). Audit society [Power 1997] as well as the quasi-market imperatives generated by the knowledge-society/economy – which Slaughter and Leslie [1997] aptly label “academic capitalism” – have thus left quite visible traces in these institutions.

While for some this is a reason to leave, those researchers who have decided to stay in the system largely seem to comply with, or at least somehow to accommodate to, these changes. In the logic of

heterogeneous engineering, researchers have been “engineered, too – persuaded to suspend their doubts, induced to provide resources, trained and motivated to play their parts in a production process” [MacKenzie 1990: 28]. Yet we would miss out some of the complexities if we simply assume that researchers do so either driven by fear of being negatively sanctioned or because they feel obliged towards the institution. Indeed, debates in new-institutionalism [Hasse, Krücken 1999] have much more hinted at the fact that members of institutions – in our case, researchers – do follow the changes “because other types of behaviour (have become) inconceivable; routines are followed because they are taken for granted as ‘the way we (now) do these things’” [Scott 2001: 57]. Thus changes in the research environment and the moral orders that come along with them are incorporated by researchers and become regarded as ‘normal’, a benchmark, a commonly accepted point of reference.

In order to understand the epistemic living spaces researchers manage to imagine and carve out, it thus seems essential to reflect institutional reform debates around universities and other academic institutions. Over the past decades these debates have more or less gravitated around the notion of increased autonomy, associated with new models of self-governance and accountability towards stakeholders and society at large [Felt 2002, 2003, 2005]. While, for a long time, autonomy has been understood as a value underpinning academic identity and a synonym for academic freedom, now a clear shift towards much more technical meanings can be observed. Today, autonomy is frequently conceptualised as a more operational tool necessary to run the university, granted formally by the State through clearly defined legal processes. But this new autonomy might also simply be interpreted as relative independence achieved through multiple dependencies on external financial resources and partnerships. Thus university-internal decision-making could be seen as taking place under clearly defined external value structures. Routine decisions are left to the universities and other academic institutions, while strategic choices often remain with bodies composed of actors from ‘outside’. As a consequence, we simultaneously observe a trend towards greater deregulation on a macro level across Europe, while at the same time more invisible regulatory forces are at work on the meso and micro levels of institutions. In our analysis we will point to some of them, such as discourses of excellence and relevance of research, to mention but two examples.

Overall, a shift in central values of contemporary academic institutions may be observed, towards cost-effectiveness, ever better input-to-output ratios, closer relations to knowledge users and many more. These similarities in the rhetoric accompanying R&D as well as university

reform across Europe addressed above, might be understood as the result of more global socio-economic and socio-political shifts as universities are repositioned as institutions of research and higher education in knowledge societies and economies. Yet, on closer examination, also on the institutional level clear national variations across Europe emerge within this seemingly homogeneous discourse on research and higher education. They reflect different national histories of higher education and its relationship to the State, widely different political cultures and different positions taken by universities in relation to other knowledge-producing and distributing institutions in the national context.

Central to our analysis in this volume are the contradictions and tensions for individual researchers created by these changes. While making a career has become a highly formalised, normative and technical endeavour, simultaneously the complexities and messiness of living in research, of the multiplicities of new job categories, of the need to move around and of developing a position on your own, make people feel rather lost. While output categories are defined ever more finely and weighed in order to describe 'quality', at the same time people feel uneasy about their more broader biographical developments as researchers [Felt, Fochler, Müller 2008]. While time-structures gain a growing presence, researchers feel the need to carve out niches where they may perform 'slow research', meaning integration and articulation work of the knowledge produced in different sites and at different moments. While the myth of science still puts vocation and vision at the core of research activity, research is increasingly equated with work needing entrepreneurial and management skills. This then leads to two strands of reactions: a more optimistic narrative about the emergence of a new class of knowledge workers which might even overshadow traditional elites, and a more 'decline and fall' narrative expressing "the fear and thrill that the best days are past" [Scott 2007: 206].

6. Changing frames: audit society and a growing trust in numbers

Many of our observations have to be understood as framed by what Michael Power [1997] calls the audit society. In his analysis of contemporary regulatory imaginations in the UK and North America, Power elaborated on the fact that audit structures have become central to the legitimisation of contemporary institutions, in our case institutions of research and higher education. It is a "symbol of acceptability, indicative of ideals of transparency, accountability and managerial willingness to learn" [Power 1997]. It is to be understood not so much as a type of society, place or people, but rather "as a condition: one shaped by the use

of modern techniques and principles of financial audit, but in contexts far removed from the world of financial accountancy” [Shore 2008: 279]. Michael Power’s diagnosis yet has to be seen as part of a broader diagnosis offered by Theodore Porter [1995] on society’s profound “trust in numbers”. “Numbers, graphs and formulas (are) first of all strategies of communication. They are intimately bound up with forms of community, and hence also with the social identity of the researchers. ... Reliance on numbers and quantitative manipulation minimises the need for intimate knowledge and personal trust.” [Porter 1995: viii] Thus authority is exercised via numbers and the accompanying techniques of producing them. They seem abstract and impersonal and thus gain quasi ‘natural’ authority without needing much legitimisation effort. Numbers somehow hide the traces of human production, they convey the feeling of being an objective, value-free description of reality. “While any sentence always contains a potential for negation, numbers tend to be read as affirmative: ‘What is counted usually counts.’” [Heintz 2007: 80] Yet it is of relevance to underline that – historically speaking – the system of science itself was an important – if not *the* central – actor in producing the ‘numbers’ to describe both the natural and then also the social world. However, for quite a while science has managed to avoid the self-application of this logic.² Hence these scientific practices of using numbers, graphs and formulas and the corresponding societies have to be understood as standing in a clear relation of co-production. The same holds for quantification and scientific enterprise.

In the academic domain, these “new kinds of relationships, habits and practices” [Power 1997: 279] created by the trust in numbers and the implemented audit structures are of particular interest to our study. In her reflections on audit cultures in universities, Strathern [2000] points out that acting within these newly-framed contexts means embracing both the ideology of economic efficiency as well as what is regarded as “good practice” within the academic field. This means that the best possible performance following the agreed set of indicators becomes the aim. The State then can retreat to the position of simply controlling the indicators (e.g. citations and impact factor papers) and enforcing self-control on researchers. The accompanying rhetoric gravitates around the idea of “helping people help themselves, including helping people get used to this new culture” [Strathern 2000: 4].

2 De Solla Price’s [1963] “science of science”, thus the introduction of indicators such as the number of institutes, publications, researchers etc. to assess the development of the system of science, could be seen as a kind of starting point for a now widespread, quantitatively-oriented crafting of an understanding of the dynamic growth processes of science.

Indeed such external audit structures always produce internal self-auditing processes, and thus enforce standards of “good behaviour” in a bottom-up manner. The power of such processes consists in the fact that they are largely indirect and build on the idea of “control of control”, thus on “the observation of arrangements of self-observation”, which means that they aim at stimulating the organisation and its members to exert self-control. It is astonishing to see to which degree auditing is embraced by research institutions, and thus the “ideological momentum that auditing has acquired” [Power 1997] without questioning the underlying assumption and without reflecting what it might produce beyond the intended effects.

Auditing also means to “make things auditable” [Power 1997] or, to take it further, to direct our attention only to those things that can be made auditable. Thus both institutions and individuals have to render what they do visible, or to act in ways which may be made accountable and to thus conform to the logic of the auditing process. The development of an ever-growing number of indicators for the ‘quality of research’ or the creation of information systems and the accompanying belief that they ‘represent’ the work performed might be taken as the most visible outcomes of this paradigm. This in turn – as we will show throughout our analysis – renders some parts of academic work more visible while others remain hidden away. This creates new and largely unreflected inequalities. How this more invisible work is shared among the actors involved in research and what this often quite uneven distribution means for them will be an essential question to address.

7. Structure of the study and methodological considerations

When putting together the KNOWING project, which is at the core of this book, the aim was to involve European countries that significantly differ in terms of R&D and higher education reforms and their implementation, with regard to gender equality policy implementation, and in terms of their closeness to or distance from what are dominantly regarded as centres of scientific knowledge production. With regard to the Czech Republic and Slovakia, the project had a specific case of ‘Eastern’ presence – formerly one country (until 1993), now two – again with different patterns of policy changes and gender relations. Austria was of interest because of its location in the ‘Central European region’ but outside the geopolitical ‘Eastern Block’ and with a strong affinity to the German-speaking scientific milieu, at least in the social sciences, and a high devotion to developing scientific leadership in the biosciences. Finland represented a country with a well-developed gender

equality agenda and ongoing R&D reforms, which were regarded as quite advanced by many of the other European countries. And finally the United Kingdom was picked as a country at the vanguard of R&D transformations, with a well-developed body of literature and research stemming from science and technology studies, with gender equality also implemented at State level with a history of grass roots organisations and, finally, as being regarded as closest to the US research culture.

Further we assumed that these differences would allow us to examine the implications of the drive at forming a single European research agenda – the European Research Area – and what this may mean in the different contexts. The title of the project tried to capture this comparison through the label of ‘East-West comparison’. In the course of carrying out the project, however, it became increasingly obvious that applying this distinction in a rigid manner to our comparative analysis would simply reinforce and essentialise imagined and lived differences while not delivering any deeper understanding of the more fine-grained mechanisms and changes at work. Thus the ‘East-West’ distinction will not be treated as a well-delimited and stable pair of categories, but rather as a distinction that is performed and gains power in certain constellations, that manages to create at moments certain orders, while at the same time remaining vague and indefinite.

In each of the five countries involved – Austria, the Czech Republic, Finland, Slovakia and the United Kingdom³ – we were studying two epistemic cultures: one from the social sciences and the other from the biosciences. We will use these generic terms, yet acknowledge the broad variations within these fields, without claiming that each of them is by any means to be regarded as homogeneous. We will thus use the comparative gaze on these two different knowledge cultures to get a deeper understanding of the specificities of each culture. Our comparison is between these sets of practices and less focused on differences within each of the fields. We were concerned with issues such as explicit and implicit epistemic hierarchies between fields and how they play out, on modes of working, on the structural aspects of these fields and also on the social and academic positioning of individuals. In addition,

3 Researchers from the following institutions participated in the project: Institute of Sociology of the Academy of Sciences of the Czech Republic; Faculty of Humanities, Charles University, Czech Republic; Gender Studies Centre, Faculty of Philosophy, Comenius University in Bratislava, Slovakia; University of Leeds, United Kingdom; Department of Social Studies of Sciences, University of Vienna, Austria; Department of Sociology, University of Turku, Finland; Turku School of Economics and Business Administration, Finland; Department of Sociological Studies, University of Sheffield, United Kingdom.

however, the concrete institutional contexts in which the knowledge cultures were studied differed. Some of the teams carried out the research in university and research-only institutions, some teams only in universities. This brought to the fore crucial differences, for example, in terms of the position in the research and/or teaching system, of career possibilities, as well as of organisational specificities and histories.

We also had the ambition to look into the gendering of organisational practices. From the outset we did not want to concentrate on the position of women in research institutions only, but rather to examine how persons occupying certain locations with certain value systems and responsible for certain types of work are gendered and gender themselves, how the research organisation is itself gendered, and how the fact that research organisations are gendered helps or hinders variously posited individuals in the system in progressing. The gendering process always cut across other concerns – such as the position of a particular research discipline or the seniority or juniority of a particular person. Yet, at the same time, gendering reinforces and helps to create distinctions and hierarchies. Not only individuals, but also their activities are gendered, thus enabling transfer of values and expectations independently of individuals' anatomical sex.

Finally, across the countries and different disciplines our interest was also directed at the so-called science-society communication or engagements: how researchers view their relationship to “society”, how they keep society at bay or become involved in it, whether they perceive research as something entangled with society or not, and how these attitudes and opinions may impact on the researchers' visions of accountability, responsibility and views of society and their own role in society.

To assemble relevant material and indicators for these different research interests, we employed a range of methods from short standardised life course questionnaires with a broader range of researchers in our research sites, over qualitative interviews of selected individual researchers, document analysis, focus group discussions for feedback and discussion on some of our findings, to shorter periods of ethnographic field-work. In doing so, we hoped to grasp the often-blurred pictures emerging from the entanglement of social, institutional, symbolic and epistemic dimensions. Even though the multi-method approach envisaged in this project should allow us to succeed precisely in both a deconstruction effort and careful reassembling, John Law reminds us that any constellation of social science methods is to be understood as emerging “enactments of relations that make some things (representations, objects, apprehensions) present ‘in-here’, whilst making others absent ‘out-there’. The ‘out-there’ comes in two forms: as manifest absence (for

instance as what is not represented); or, and more problematically, as a hinterland of indefinite, necessary, but hidden Otherness.” [Law 2004] Thus, throughout our analysis we will attempt to reflect on the ways in which we struggled to gain access to the complex reconfigurations of knowledge, institutions and researchers. We are aware, even if we offer rich and well-reflected observation and analysis that, at the same time, what we offer is only one possible assemblage.

However, the strength of our analysis rests on the multiple levels of comparisons at work: along dimensions such as national contexts and their different histories, institutions, epistemic cultures, generations of researchers and gender. The core level of comparison in this book will investigate the multiple articulations of these different levels and take place along three distinct analytical foci – boundaries and modes of ordering, forms of being together apart, as well as temporal dimensions. They were extracted from the experiences collected in the five different national settings. In bringing them together into one larger reflection of contemporary articulations of knowledge, institutions and researchers, we aim at going beyond many of the analyses offered in science and technology studies and other academic fields dealing with these questions, which tend to focus on one of these issues only. We see the three perspectives we will elaborate as crucial for understanding how researchers manage to imagine, delimit and structure an epistemic living space which they find sufficiently attractive to inhabit, develop and protect. In talking about boundaries, we will first cover the activities of researchers to order and delimit the territory they live in, and confront the orders which are imposed from policy and institutional sides. We will then address different forms of socialities and in particular, being together as well as individuated in research. The third analytical focus, which helps the understanding of contemporary epistemic living spaces, deals with the multiple time-regimes structuring researchers’ lives.

In choosing this approach, we will deconstruct the idea of a homogeneous national research system, and show that even similar discourses and policy measures might lead to a broad variety of realisations in the different contexts. This will allow us a deeper understanding of the multiple, partly contradictory forces at work, which continuously restructure research systems and give shape to working epistemic living spaces. Embracing as many as five different European countries, varying in size, regional situation, as well as socio-political and scientific history, and studying different epistemic cultures in each of these settings thus delivers a unique case of comparative research on epistemic communities and their living spaces.

Being a collaborative research project with researchers coming from many different language backgrounds and because comparison always needs local knowledge, we decided to reflect this in our analytical work as well as in the writing. The three chapters which follow thus have two main authors, yet there is always a group of researchers mentioned who have contributed with input from their countries in producing reports, re-analysing some of the data produced from specific analytical angles, giving feedback on the structure of the chapters and close reading at the different stages of text production. In so doing, we have aimed at ensuring the capturing of important similarities and differences among the countries participating in the study.

8. The structure of the book⁴

The core of the book has three major chapters addressing the relations of knowledge production, institutions and researchers from different angles.

Chapter II will focus on the broader dynamics at work when structuring research. Using the whole spectrum of materials from policy documents, over focus groups and participant observation to interviews, we investigate the ways in which policy-makers, institutions, research collectives, as well as individual researchers similarly develop and are subject to modes of ordering of their research practices, trajectories and environments. This means looking at how distinctions are created, contested and enacted, boundaries erected, fortified or torn down and different kinds of maps drawn to assure some orientation. More concretely, we will offer an analysis of how researchers (re)produce and position themselves in different kinds of ‘geographies’, looking in particular at the role of regions and nation states, at distinctions such as ‘East/West’ or ‘centre/periphery’, as well as at disciplines and disciplinarity. We thus question the meanings of such maps, who participates in the cartographic work and what effects they produce. Our attention will then move to two specific distinctions which have come to play a rather prominent role in researchers’ framing of their work and thus matter in quite important ways: one will be ‘excellent research’/‘the other research’, the second ‘applied (societally relevant)’/

⁴ In the three chapters which follow, we will use numerous quotations from the interview material as well as from the field observations. They will be referenced as follows: Country code_type of material_epistemic field_sex of the person. More concretely, this means the following abbreviations: Country Code: AT, CZ, FI, SK, UK; type of material: Int (Interview), FG (Focus Group), FN (Field Notes); epistemic field: SS (social sciences), BS (biosciences); sex: f or m. Only in rare cases the country code is not mentioned, to protect the anonymity of the informant.

‘basic research’. A closer look at the multiplicities of organising and enacting research in academic institutions will then be the next step in using the four distinct “modes of ordering” – administration, enterprise, vocation and vision – proposed by Law [1994]. Finally, the analysis will be rounded off by an in-depth reflection of the multi-layered and quite ambivalent relations between research(ers) and society at large.

Chapter III turns our attention to the tensions between individual and collective aspects of knowledge work in research, which is captured in the title, “Working Together Apart”. Starting with the exploration of some of the main logics of togetherness and apartness in science policy imaginaries, this chapter aims at comparing these policy logics with patterns of the practice of togetherness and apartness we found in our study. The point of departure is the predominant imaginary of working together in science – the network – which then is contrasted with what we found in the main places where science gets done: the lab and beyond. This will be followed by an exploration of KNOWING participants’ reactions to and experiences of institutional efforts to relocate togetherness to make science more responsive to neoliberal agendas of economic growth and social improvement. We focus here upon two main logics – mobility and interdisciplinarity – exploring for each of them epistemic, organisational and personal aspects of working together and apart in science. In doing so, the chapter draws upon observation of researchers’ interaction with materials, people and technologies in a range of research settings, as well as on narrations of career, belonging and identity, mainly from interviews and focus groups.

In the following Chapter IV, we explore the multiple ways in which researchers and their knowledge production practices are situated in and (re)produce a range of different temporal dimensions that must be managed and reconciled. We thus explore the multiple temporalities which structure epistemic living spaces, which allows us to shed new light on some persistently problematic issues relating to gender and science. These include the shape and speed of the scientific career, the reconciliation between work and more private dimensions of life, as well as the pressures and possibilities created by the sense of acceleration of knowledge work in academia. The chapter elaborates these issues along two lines: trajectories and everyday time. The notion of *trajectories* draws attention to narrated time – the stories that collective and individual actors in research tell about their linear movements through time, involving the ongoing production and reproduction of pasts, presents and futures. *Everyday time* then draws attention not only to the ways in which time is spent and saved, used and produced, managed and accounted for, day by day and week by week in concrete settings, but also to the plural ways it is experienced and made meaningful.

In Chapter 5 the book offers a short reflection on the relation between academic research and policy advice. The book concludes by drawing together some of the more pressing concerns which have become visible throughout the analysis. We reflect on how policies, seemingly universal in their discourse, play out in rather different ways not only in national and institutional contexts, but also in the epistemic fields we investigate. Rather diverse expressions thus became visible throughout the chapters that are reflected not only for their deeper impact on scientific knowledge production more generally, but above all also on the researchers as central epistemic agents. This will also lead us to understand the multiplicity of moments, constellations and articulations in which gender perspectives play out in contemporary research. We thus conclude with an invitation to more profound reflections on the consequences of the contemporary reordering of research systems, on the impact this has on epistemic living spaces, which in turn might be opening up or closing down possibilities, might become an environment felt as attractive or hostile. But, above all, it is also an invitation to rethink science policy in terms of broader societal responsibilities, reminding us that the way we describe and represent the world through science is not only deeply intertwined with the ways we live in it, but also with our imagination of potential futures.

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Modes of Ordering and Boundaries that Matter in Academic Knowledge Production

Ulrike Felt & Tereza Stöckelová

With contributions from: Lisa Garforth, Magdalena Górska, Lubica Kobová, Marcela Linková, Morgan Meyer, Seppo Poutanen, Veronika Řepíková, Mariana Szapuová, Veronika Wöhrer

1. Introduction: the making of research environments and their moral orders

An anecdote to start with:

Every year generally around mid-August the so-called Shanghai Academic Ranking of World Universities⁵ is published. From its very beginning, critics have tried hard to deconstruct this exercise and have stressed that the indicators this ranking uses do not reflect the quality of the ranked universities in a satisfactory way. Nevertheless the Shanghai Ranking continues to attract attention. Consider the following quote from its Wikipedia entry, which consists of an explanation of the Ranking's rationale as well as a list of the ranked universities: "The table below contains the rankings from 2003 to 2008 for all universities which ranked at least 100 in one of the years. The ranking is omitted for years in which the school did not land within the top 100. Note, the full ranking contains over 500 universities. If a university is not listed in this table, it fell below 100 in all six years." So, to the average readers' eyes, only those 'that made it', that have at least once over the past years been among "the best", are presented. One of the universities participating in this project, the University of Vienna, disappeared from the Top 100 after 2006. It suddenly dropped from rank 85 to the group of universities between ranks 152-200, where differentiation does not seem to make sense any more.

What happened? Did the quality of academic work done at Vienna University drop so dramatically?

The solution to this riddle is as easy as this: A new university law has separated the former medical faculty from the main university and turned it into an independent Medical University. Thus now the two universities that had originally been one could no longer share the Nobel Prizes, the high-level journal papers, the citations, and many of the other indicators. They had to carefully split them, moving both universities into the lower ranks.

Does it matter?

No, one could say. But why then continue ranking, reporting it and feeling pleasure when one moves up the ranks?

Maybe it does matter...

“In one way or another, we are attached to the idea that if our lives, our organisations, our social theories or our societies, were ‘properly ordered’ then all would be well.” [Law 1994: 4-5] With this statement, Law attracts our attention to the fact that modern societies seem to believe strongly not only in their capacities to order the world, but also that this ordering ultimately leads to better ways of living in this world. This seems to hold as much for science as for any other societal sector, perhaps even more so, as science has managed to become *the* central player in producing both knowledge and order for the natural and social worlds that surround us [Bowker, Star 1999; Shapin, Schaffer 1985]. Science understands itself and is largely understood as a superior way of performing classifications, identifying suitable categories, in short, of creating an order. The introductory anecdote points at how well entrenched some ordering mechanisms are in our society, how normal they have become and in how many subtle ways they change how we see and understand the world and thus also live in it. It is a story about how things which have been seen as chronically difficult to compare – such as the academic quality of hundreds of universities – are miraculously transformed into a ranking, and about the consequences this might have, such as how this ranking might change universities’ self-perception, how it potentially produces feelings of satisfaction, pride and competitiveness, but also of discontent, bitterness or simply marginality.

Following Law’s reflection on modernist efforts of ordering society, it seems attractive to direct our gaze at science through this lens: what are the contemporary understandings of a ‘properly ordered’ science,

in particular under the auspices of the high expectations placed on science and technology by policy makers with regard to its driving force for economic development?⁶ While ordering plays a central role in the ways in which researchers deal with their objects, observations and knowledge entities, we will not deal with those aspects here. We are much more interested in understanding the very environment in which researchers spend their working life and how they see it being ordered by institutional structures, policy measures and the underlying imaginaries and expectations addressed at them and their research. Looking at different major principles or logics of ordering could thus contribute in important ways, we argue, to understanding how institutions, the knowledge produced and people who generate this knowledge are intertwined in inseparable ways. More concretely, these logics of ordering materialise in at least four different forms:

(1) classifications and the building of essential categories such as basic and applied, excellent and normal research, East and West or different types of contracts; (2) rankings of institutions, journals, or countries as a logic of linearisation of complexity; (3) (re)production of maps and the accompanying boundary work such as epistemic (e.g. disciplinary) or institutional maps (e.g. network maps); and (4) the creation and implementation of value systems such as internationalisation, mobility or interdisciplinarity. Needless to say, these four types are not mutually exclusive, but overlap and co-perform order in the research system.

In their book, *Sorting things out. Classification and Its Consequences*, Bowker and Star [1999] have offered an in-depth analysis of *classifications* as a central part of modern lives. Understanding classification not as a rigid system but as a practice that is “situated, collective and historically specific” [ibid.: 288], they have drawn our attention to the dual nature: classifications are simultaneously material and symbolic, highly visible and invisible. The authors make us reflect the many classifications routinely deployed to govern everyday life also in research: researchers file their data, keep track records, select the ‘right reading’, pick suitable collaboration partners, define what is innovative and what is not, what is ‘excellent science’ and what is to be considered as ‘normal’. These are but a few elements of academic classification work which we routinely perform without posing too many questions. Researchers’ classification schemes are multiple, more or less mundane and partly even contradictory, as researchers always participate in different communities of practice. Simultaneously, research institutions and the research system more globally impose certain classifications and

6 See for example the Lisbon Declaration [EC 2000].

categorisations on the ‘elements’ – researchers, output, methods, data, technical equipment etc. – they are supposed to take care of. They form an important basis for the exertion of power and control in science. Yet Bowker and Star also remind us of the multiplicity and malleability, as well as of the tacit dimensions of these framing forces exerted by organisations on knowledge, people and their biographies.

Often we only realise how classifications matter at moments when, for example, individual biographies get twisted into predefined normative shapes because powerful classification schemes do not line up with the logic of everyday life, e.g. when researchers’ working conditions or life circumstances do not allow the production of the expected output and thus careers are endangered. Awareness of the performative power of classifications is then often linked to realising/experiencing that “the advantaged are those whose place in a set of classification systems is a powerful one and for whom powerful sets of classifications of knowledge appear natural” [ibid.: 225]. In that sense, the architecture of classification schemes has always to be read as “simultaneously a moral and an informatic one” [ibid.: 324]. And it deploys its force precisely through becoming naturalised – i.e. being seen as the obviously best/only way of making order – and thus unquestioned as well as unquestionable.

It is all the more important to stress that we often stand in ignorance not only of what the orders created through classifications actually look like, why they do so, who the architects are, but above all, what social and moral orders are embedded in and performed by them. The creation of categories of description and classifications, such as research indicators or different kinds of working contracts, always exercises material force, even though it might admittedly do so more or less instantly, directly or visibly. This means that our attention should not only be directed to the classification as such, but above all to the often unstated implicit orders. In that sense, we want to raise awareness of the fact that each standard and each category valorises some point of view and silences others [Bowker, Star 1999]. Putting in place a machinery creating certain sets of categories and classification schemes is hence not a technical or managerial task but a political enterprise which performs certain values as well as futures of the world.

In particular, much of feminist literature [Rossiter 1982; Longino 1989; Haraway 1988; Rose 1994; Etkowitz, Kemelgor, Uzzi 2000] has pointed to the fact of how implicit norms, values and categories have managed successfully to create multiple, largely invisible forms of exclusions – the “glass ceiling” with regard to gender effects on careers is but one of the metaphors trying to capture such phenomena. Thus using certain indicators and labels, describing research in one

way and not in another, accounting for achievements along certain lines has to be seen as a normative choice and therefore as an issue of responsibility in academic leadership. But these practices are today largely uprooted from localised accountability. Classifications in research are becoming increasingly translocal through the machineries of multilevel international comparative assessment. They often result in quite universalistic claims concerning the knowledge and innovation production process, thus making it increasingly difficult to question them. The recent reform movement spreading through European universities is but one example where a particular vision of autonomy and the accompanying accounting, career and value systems are being implemented [Felt 2003a, 2005].

Closely linked to classification activities, *rankings* are the second dominant logic of ordering, which aims at defining for a given entity – a university, department, researcher, journal – a position on a scale in relation to others belonging to the same set of elements. Building on the classifications and categorisations mentioned above, rankings define those categories that matter as well as the relations between them. Rankings thus rest on an idealised model of what constitutes quality in a certain set of elements. They perfectly fit the ideology of the audit society and go along with our deep trust in numbers as more objective ways of representing our world [Power 1997; Porter 1995]. Over the past decades, rankings have proliferated in astonishing ways. As our introductory anecdote to the chapter nicely shows, they aim at creating a seemingly clear linear order out of complex relationships and at attributing value to those taking part or being made a part (one does not necessarily explicitly choose to participate) in this exercise. Ranking of universities, journals, papers, students and many other elements thus structure research systems in important ways. Whether a region, country or institution is classified as top in a certain research field does matter in many ways and will show – sometimes planned, but often unintended – consequences. Researchers might feel they are on the periphery, might want to leave, aim for positions in institutions with a higher reputation; knowledge might get less attention when published in journals with a lower ranking; or researchers might decide to pick research topics which fall into those areas which are high on the agenda.

The third logic of ordering gravitates around *boundary drawing* and the often-linked production of more or less symbolic *maps* that facilitate orientation of researchers as well as policy makers and society at large. Addressing these issues underlines our starting assumption that research does not take place in a clearly delimited and structured territory with stable boundaries in and around it on which researchers simply need to position

themselves. Rather we focus our attention on how researchers in their accounts, in different types of print materials – from policy papers, to the media and to scientific output – but also in everyday practice (re)produce and negotiate these boundaries. Thus we are interested in the “texts and talk that constitute boundary-work” produced by researchers and policy makers alike. Much of this writing then – often implicitly – elaborates on epistemic, social and institutional “borders, territories, labelled landmarks, scales, and coordinates” which are all “the familiar features of geographic maps” [Gieryn 1999: 29]. This explains why Gieryn speaks of cultural maps of science, not only to point to the production process of this kind of order, but also to underline the orientation role these maps play. Our concrete interest will thus be not only in the kinds of maps that are being drawn, the boundaries erected, the authority attributed, the relations pictured, but also in how they are lived, questioned, stabilised, perceived as constraining or protecting by researchers. Yet it is also essential to understand that any emphasis of certain features and distinctions on a map hides some others, which might turn out essential for the creation and maintenance of epistemic living spaces.

Starting from this frame of analysis, it seems promising to take a closer look at when, where, how and by whom boundaries are drawn or torn down and distinctions that matter introduced. In doing so, we are focusing much less on practices in the lab, field or library, or on the concrete academic output, but more on the ways in which researchers describe and envision their epistemic living space, not only how it is formed and performed through policy making, institutionally, nationally and internationally, but also by the researcher inhabiting the field. Pragmatic demarcations within science are driven by social interest on different levels. They can aim at delimiting a whole research field in order to claim, monopolise, deny, or restrict the cognitive authority of a research issue or one method over another. Thus boundary work is about the “attribution of selected characteristics to the institution of science (i.e. to its practitioners, methods, stock of knowledge, values and work organisation) for purposes of constructing a social boundary” [Gieryn 1983: 782], allowing for distinctions which matter within the scientific field and beyond. Thus we observe “cartographic contests ... among diverse people, practices, and knowledge claims” [Gieryn 1995: 406]. However, these demarcations always remain context-bound and have to be read as a “local and episodic accomplishment, a consequence of rhetorical games of inclusion and exclusion in which agonistic parties do their best to justify their cultural map for audiences whose support, power, or influence they seek to enrol” [Gieryn 1995: 406]. Boundaries are thus never stable, and need continuous work which generally involves a variety of actors.

This dynamic will be captured in our analysis of the ways in which policy making is involved in the dynamics of boundary work through implementing a whole spectrum of central categories (and their embedded value systems) – third-party funding, (S)SCI⁷ publications, mobility, competitiveness, societal relevance, and many more. Be policies local, national or European, they all try to embrace imaginations in what direction science should develop, what material measures should lead there, as well as through which monitoring mechanisms change should be followed and success or failure ‘measured’. This allows the drawing of boundaries between those institutions and researchers who should be at the core or on the periphery of a research territory or should even be expelled. At the same time, researchers, both collectively and individually, also try to produce and impose their own maps, with boundaries and territories better suited to their projects.

It is the processes of border drawing, rather than the borders per se, that will be the centre of interest in the following analysis, with attention being paid to both the demarcation as well as the management of borders. Demarcation is relevant as it fixes the criteria and categories of in- and exclusion. Management then draws our attention not only to who is allowed to cross borders and in which direction, but also to those who determine to what degree this can happen.⁸ Thus we will, in what follows, be able to observe three different kinds of boundary drawing activities: the *creation and implementation of new boundaries*, the *undrawing of boundaries* and finally the *shifting of boundaries*. Yet these processes are often ambivalent and not unidirectional. Thus it will be essential also to grasp those moments where an undrawing activity happens on one level while this gives birth to new boundaries on another. The discourse on the social science and humanities would be an excellent example. In policy discourse, equal treatment of all research fields is underlined and thus an often-palpable boundary between natural sciences and “the others” gets explicitly undrawn. Yet at the same time, the categories and classification schemes imposed on all fields stem from the experiential background of the natural science and thus lead to an important unquestioned and seemingly unquestionable redrawing of boundaries.

Value systems are partly overlapping with and emerging from the above-mentioned logics of ordering and thus constitute a fourth dimension of order to consider. Robert Merton’s [(1942) 1973] seminal work on the ethos of science with the four basic norms – universalism, communalism, disinterestedness and organised scepticism – which he assumed as

7 (S)SCI = (Social) Science Citation Index.

8 Newman [2006] offers an interesting reflection on the dynamics of “borders in our ‘borderless’ world“, though not directly related to research.

guiding researchers' actions, constitutes a first explicit reflection on the way values are present in science. For Merton, these are non-codified, handed over from generation to generation through the process of socialisation, and he expressed the strong belief that not living up to them will lead to the exclusion from the system. While these norms are still present as ideals in researchers' narratives, many other values have been added explicitly, but also implicitly through the introduction of certain categories. Entrepreneurship, mobility, internationality, productivity, speed, linearity of careers – to mention but a few – have all become important sets of values imposed more or less explicitly by contemporary research systems [e.g. Felt et al. 2009b]. Instead of being seen as indicators characterising processes of knowledge production, they have been on many occasions set as equal with quality itself, and become goals in themselves. In part they have become codified, in part they are omnipresent yet not clearly operationalised (see the excellence discourse), yet others are tacitly embedded in the system. It will be thus essential to reflect these ordering forces along our analysis.

These orders, maps, boundaries and value systems as specific arrangements of science are deeply gendered.⁹ Throughout the history of scientific inquiry, these arrangements have become stabilised and made increasingly “obdurate through their institutionalization in credentialing and training systems, and through the shift of scientific work from private settings (like the home, where women have always been present) to public settings at universities and national academies (where, for a long time, women were not)”. Thus “traditions of inquiry, discovery, and practical applications” [Gieryn 1995: 421] became chopped up along gender lines. Therefore what counts as avant-garde in science and thus receives rewards and special financial support can be regarded as gendered. We have also learned from the history of science that fields where women participated in predominant ways were often shifted to the margins. And these arrangements also produce “effects on individual choices and institutional arrangements that thinned the flow of women into science, and steered it toward contributions that came to be seen as ancillary” [Gieryn 1995: 421-422]. This genderedness of academia already becomes visible in the early phase of science students' choices. As Seymour and Hewitt [1997] have shown in a study investigating the reasons for students in US universities to move out of science and engineering curricula and switch to other fields, the educational phase is already experienced as deeply gendered and academia or specific segments of it are felt as being a place not suitable

9 For a short reflection on this issue, bringing together much of feminist literature, can be found in [Gieryn 1995: 420-424].

for women. Yet it is also important to understand that such gender dimensions often overlap and are intertwined with other distinctions that matter and are thus not easy and obvious to trace and carve out. Gendering as a dimension of orderings in research will thus come forth in different places of our account; it will surface at specific points, catching the observer's eye, just to move backstage again.

If we want to understand these dynamic processes of reordering within research, but also between research and different levels of policy making, we could make use of an enlarged understanding of Galison's [1997] concept of "trading zones". Galison conceptualises such trading zones as places where members of different academic subcultures (in his case, in the field of particle physics) meet and interact. People enter such trading zones with specific pre-histories, interests and interpretations of their own, looking out for something that suits their visions. In our case, we would not look so much for interactive spaces where different sub-communities meet and engage in the epistemic sense, but much rather for trading zones where researchers and actors such as policy makers or university managers interact and struggle for coherence. Galison underlined that what is specific about such trading zones is that "groups can agree on rules of exchange even if they ascribe utterly different significance to the objects being exchanged; they may even disagree on the meaning of the exchange process itself" [Galison 1997: 783]. They struggle over specific languages to fit the purpose of interacting and often inadvertently these interactions in trading zones lead to stabilising certain practices. In the end, coordinated action, Galison stresses, might also lead to coordinated belief. In our case, we might ask how the interaction between researchers and policy makers and managers in such multiple trading zones in the end leads to a common set of practices and beliefs and maybe also to new myths about what it means to be doing research and being a researcher.

These reflections should be linked with Nowotny and co-authors' [2001] analysis stressing that, with the move from mode 1 to a mode 2 environment, the number of trading zones has proliferated. Trading with policy makers and their visions over how science should work with industry, other research partners or civil society seems to have become quite normal in science. While in mode 2 environments, boundaries appear less obvious, at the same time it seems important for the research community to uphold at least some of them, i.e. create awareness of the importance of certain norms and values in the research community. Researchers' positioning work with regard to the different orderings present in their respective research environments will become visible as partly contradictory and full of tensions. We will observe in many

cases a struggle for coherence, for living up to the immediate demands addressed at them, yet always trying to inscribe these present choices into a line that seems to flow from the past to an expected future, following the dominant rationale of the science system. The complexity of the phenomenon under observation, but also its fluidity in time and space – i.e. the fact that orders themselves change(d) with time and may vary between different places – guided our choice not to pick just one or two instantiations of orderings and boundary drawing at work and study them in a fine-grained manner. Rather, we decided to go for the dynamic in its breadth and address a whole repertoire of practices, mobilities¹⁰ and relations being ordered. We will simultaneously point to convergences and the relatedness of orders across different research practices and contexts, as well as convey a feeling for the messiness and contentiousness of the multiple meanings of being a researcher. Thus we aim at creating an understanding of the dynamics at work in research and at grasping how seemingly similar policies or research structures assume different meanings in respective epistemic, institutional and national contexts. We will follow how researchers construct differences that matter, how they draw and undo, accept and refuse different sorts of boundaries, such as those ‘tentatively imposed’ by policy measures or institutional settings. This leads to questioning how researchers attribute meaning to them and enact these boundaries; how they embrace, comply with, suffer from or reject different orders that are prevalent in their research field; and finally: how they position themselves within geographies they co-develop, be they epistemic, social, institutional or indeed nation-based. In a nutshell, we will write about how researchers manage to carve out their epistemic living spaces and how they inhabit them, offering an analysis of how the epistemic, the social and the structural become intertwined, as well as where and how opening-up or closing-down of possibilities happens.

In Section 2, we will start by addressing how researchers develop and/or perceive geographies, i.e. do and undo maps, to position themselves. They do so at least on three levels: the first is in terms of nation states and regions; the second level will address the more symbolic geographies and look at narratives around East/West as well as at centre/periphery accounts; finally we will move to the epistemic level and investigate how researchers use disciplinary maps for positioning themselves.

Two kinds of rather prominent distinctions used to describe research activities will be the focus of the following Section 3: firstly, the

¹⁰ Mobilities means here the capacity and necessity of movement of people, things, information and ideas central to contemporary science.

omnipresent divide between excellent – or any other label used to mark outstanding research – and other kinds of research, and secondly, the ways in which researchers deploy labels for their research such as basic, applied, relevant, etc. in their struggle to attribute a deeper societal value to their work. Issues of institutional boundaries and investigating the orderings within academic institutions will be at the core of Section 4. This will deliver important frames for understanding researchers' perceptions of their epistemic living spaces, their degrees of freedom and the constraints characterising them. Section 5 is devoted to addressing different entanglements of science and the social, as well as the efforts made to disentangle them again. We will show that researchers relate to society in terms of accountability and potential usefulness but mostly try to keep the social at a safe distance in epistemic terms. In the conclusion, we will revisit some of the master narratives on research and confront them with the findings of our analysis.

2. Geographies of reference: researchers' positioning work

When asking researchers to position themselves, as we implicitly and explicitly did by interviewing, discussing with and observing them, a broad and complex repertoire of narratives comes to light, which allows us to understand researchers' geographies of reference. By geographies of reference we mean the ways and categories along which they perceive research and science as ordered, as well as the imaginary maps produced for purposes of orientation. In the following three sections we will investigate three different kinds of maps, each produced along a specific dimension. The first map (Section 2.1) is organised along geopolitical dimensions, addressing the relation between European and more national aspects of research. Section 2.2. focuses on epistemicopolitical orders such as centres and peripheries, 'East' and 'West', obligatory passage points, networks etc. and how they are mapped out in researchers' and policy-makers' narratives. Finally in Section 2.3, epistemic orders and in particular researchers' relation to disciplines and disciplinarity will be addressed. All three geographies of reference contribute in important ways to giving shape to epistemic living spaces, they create possibilities but also limitations, participate in the development of epistemic self-esteem and thus are important in terms of fostering innovative potentials.

Researchers, in their accounts of living in research, use different systems of reference, jump between them, judge that in certain situations some are more suitable than others and thus employ a patchwork of different ways of locating themselves. Needless to say, the maps and reference

systems they use in their communication with us are not produced in an *ad hoc* way, but somehow reflect dominant orderings which pre-existed our encounter and which researchers imagine to make sense in this context. These orderings of epistemic worlds are to be understood as historically grown, they have different institutional roots, are often hybrids of diverse policy efforts, reflect established practices, yet currently all seem to focus at creating an environment to keep the steady flow of innovations going.

2.1. Regions and nation states

As outlined above, the project at the core of this book is European in multiple ways: it focused on five countries which occupy different places in the European geography, was carried out by researchers from different cultural contexts and was financed from European research budgets. Three of the countries are located next to each other in a region often labelled Central Europe: the Czech Republic, Slovakia and Austria. Two other countries represent quite different regions of the geopolitical entity called Europe, namely the UK and Finland. All of the countries have different cultural and, more specifically, scientific histories and relational networks to other countries. Does this matter for the researchers we talked to, for the ways they perceived themselves, their institutions and with it their possibilities to develop? How did their understanding of Europe and of national contexts frame what they saw as their epistemic living spaces?

While we will, in the next section, more explicitly look at the moral orders embedded in certain geographies, how notions like ‘East’ and ‘West’ – even if not expressed explicitly – become filled with meaning, here we want to direct our attention to the relation between nation states and the ‘larger entity’ they are part of, namely Europe. How does place matter from this perspective? This seems attractive to do, as European research policy discourse clearly aims at creating what has, since 2000, been labelled the “European Research Area” [EC 2000] and which is in a recent Green Paper – its cover carrying the slogan “Inventing our future together” – staged as “more than ever a cornerstone for a European knowledge society” [EC 2007: 5]. In such a scenario, Europe is conceptualised with a clear external boundary and without visible internal boundaries. The creation of such a unified techno-scientific Europe is perceived as the only way of assuring that in the “accelerating globalisation of research and technology” emerging “new scientific and technological powers – notably China and India” [EC 2007: 5] will not outperform Europe. The focus on external borders thus strongly creates

a set of binary distinctions between us and them, here and there, inside and outside on a variety of scales from the European to the national and down to the more personal spaces.

We might follow Andrew Barry when stressing that one way of understanding the European Union would not be as another even bigger state, “but as a series of imperfect zones of circulation” with “harmonisation play(ing) a key part in their formation” [Barry 2001]. Through these efforts of harmonisation and creation of common categories of thinking and developing research, Europe is supposed to emerge and be able to play its role in the global competition. More concretely, this should ideally lead to a quasi barrier-free circulation of researchers, knowledge and technologies as well as to assembling the best into the so-called ‘centres of excellence’. Thus we could argue that the European Union, to a certain degree, aims at dissolving the techno-scientific boundaries of nation states with the aim of merging them into one common research area. And this common scientific Europe to be created seems – at least in the mirror of national policies – to have certain contours that are taken as the leading imaginaries behind national policies.¹¹ The Austrian Minister for Science and Research, to take but one example, expressed this as follows:

“Nations are in global competition: for best brains, for best ideas, for scientific breakthroughs and technological innovations.

Europe has identified these challenges – and has taken them on in the so-called Lisbon Strategy. This strategy aims for the European Union to become the most competitive and dynamic knowledge-based economy in the world, capable of sustainable growth with more and better jobs and greater social cohesion.

The Austrian government is fully dedicated to the aims of the Lisbon strategy ...”

[bm:wf 2007: 3]

This policy expression, however, hints at the tensions inherent in these kinds of conceptualisations. Indeed, while in the first place he speaks of nations in competition for “best ideas, for scientific breakthroughs and technological innovations”, and thus implicitly also points to inner-

¹¹ For a discussion of how for example biotechnology is shaping and being shaped by European politics see chapter 3 in Jasanoff [2005].

European competition, he continues to refer to Europe as an entity which has to compete with other major players, generally the US being construed as the leading nation and China or India – again – being construed as the ones being hard on European heels.

Yet it is interesting to remark that when researchers themselves narrate their geopolitical position, Europe as a more or less coherent entity or a geopolitical reference point is largely absent. Nation states, research institutions and/or research groups at the top of the ranks are the categories that researchers refer to much more often when they speak about their epistemic work and their networks. This could be explained in terms of funding expenditures and programme structures that are still predominantly national. But it is also important to consider that not only institutional traditions, research infrastructures, but also memory practices – every country/institution stressing the past and present importance of ‘their researchers’ – are tied into more local histories and ways of doing things. Finally, we should not omit that European policy making through its working practices continuously reconstructs national entities, thus concurrently dissolving and redrawing national boundaries. One setting in which this happens is the large-scale monitoring systems generated out of the audit society logic, which fuel the competition and race metaphors present in policy talk. Thus nation states track expenditures, programmes, output, networks etc. and so do researchers – steadily comparing the systems to each other in a quest for assessment of their own position. The most recent example would be the starting and advanced researchers’ grants distributed by the European Research Council (ERC). While the mission statement of the ERC stresses that grants “are awarded through open competition to projects headed by ... researchers, irrespective of their origins, who are working or moving to work in Europe – the sole criterion for selection (being) scientific excellence”¹², the results of the first rounds are all presented along the category of nation states – either where researchers come from or where the host institution is located (for more details see Section 4.1 on excellence).

Through such a comparative gaze, the boundaries around nation states are continuously reconstructed, leading in turn to strong national ‘we’s which that structure accounts for. This is particularly visible in smaller and non-Anglophone nation states in our sample that measure up to the numbers set by the US and the UK. Whether the Austrian system is faring better or worse than others in this or that domain is, for example, a narrative strand in both epistemic communities – in the social sciences and biosciences – when speaking about their local epistemic living

12 <http://erc.europa.eu/index.cfm?fuseaction=page.display&topicID=12>.

spaces. One researcher captured this idea of locality, stressing his belief that “the sciences are not necessarily highly valued” in Austria¹³. And he continued to historically contextualise this statement by stressing that “this was also true in the past. The sentence from a Habsburg would still hold: we need no scholars, civil servants will suffice. This has been somehow passed on.” [AT_Int_BS_m] Similar references to the local can obviously be found in other national contexts. Bioscience researchers in the UK, for example, would compare ‘their’ grant initiatives with those in other countries like the US or France, stating that they have longer-term perspectives. Indeed careers are often comparatively valued as well as features of the research environment, such as excellent equipment or access to specific resources.

While this reflects more structural components and holds for both fields investigated, it seems important to reflect the Europe vs. nation states issue also against the background of epistemic differences. Indeed, whereas in the biosciences national boundaries are generally strongly and very explicitly deconstructed and national traditions might only gain meaning on the level of a highly aggregated and historically dense description, for the social sciences the nation state matters obviously in a much more direct way. Languages, cultures, traditions, legal systems and much more are seen not only as framing both epistemic objects but also as impinging on the ways of handling them. An Austrian social scientist quite openly expressed her vision of the difference and what that means in terms of publishing:

“... there are major differences between the German and the English traditions: how you elaborate something, pose a question; German speakers are definitely more critical and elaborate things in a much too long-winded manner, with too many ifs and buts ... and the British and Americans – they are into publishing everything possible, even the smallest parts ... and they evidently have the language advantage ... and they are less critical. Somehow one must trade-off our tradition and think in their tradition.”

[AT_Int_BS_f]

13 This is also a recurrent theme in Austrian media often using comparative Eurobarometer survey results as the ‘factual basis’ for such statements.

This quotation offers at least two valuable insights. Firstly, ways of addressing issues are seen as emerging from national, regional or even local settings and by using the term “tradition” they are conceptualised as more or less stable starting points. Secondly, the reference point to compare with is the English-speaking world and the researcher clearly underlines the pressure to adapt to this “other culture” of doing research [see also Meriläinen et al. 2008], which is in contradiction to the awareness that research objects can often not be understood in terms of global categories, but much rather need to be crafted and carved out of dense local networks and understandings and that they might have some difficulties in travelling easily. These epistemic differences might also explain the often unclear position researchers have towards constructs like the European Research Area (ERA). While some would think that it is only on its way to be realised (mainly expressed by bioscientists), others would not necessarily see this as a desirable development (a position held mainly by social scientists).

Further evidence of the role locality plays in social science research can be taken from the UK material. The following exchange with a social scientist gives us a glimpse of what research being tied into local – in this case policy – structures might mean:

“Interviewer: Do you feel that you’re part of the British research community? British policy? Is it international or is it more regional or more issue-based?”

Participant: I wouldn’t say it was regional. I’m very conscious of the nations within the UK and the differences and belonging to different parts of the policy community in that respect. So I wouldn’t say I belong to British social policy environment but I do belong to, say, an English ... policy environment or a Scottish ... policy environment.”

[UK_FG_SS_f]

From this short quotation, the multilayeredness of the reconstructions becomes visible. Depending on the kinds of research questions addressed and the level at which such “objects of inquiry” get framed and show effects, nation states, their regions or constructions like Europe do or don’t matter in the case of the social sciences.

However, we also encountered inverse cases, in which national, social and research issues were reconstructed as European ones. This could

be illustrated by looking at material from the social sciences in the Czech Republic. A research plan of a Faculty which aims at studying the most pressing social problems Czech society will have to deal with, is formulated in constant reference to the EU and the problems are to a significant extent construed as problems common to Europe. For example, the concept of “post-communism” widely used in the 1990s as a descriptive/explanatory perspective on the societal condition in the country is never mentioned in the document: rather the Czech society is labelled as “(post)modern” [Gorska et al. 2007: 27-30]. The localisation of the research object in Europe rather than within national boundaries can have multiple consequences: some issues are tackled while other, more locally specific ones, may not receive attention. Yet for the researchers this might also mean that results become more easily publishable in international arenas due to shared problem identification and conceptual framing.

Thus using national or transnational reference frames to describe and position one’s research is often a strategic choice, attributing or denying value and importance to the research questions posed, comparatively assessing the validity of research practices and constructing the epistemic object as more or less free of local context. Undoubtedly, we observed important differences between the two epistemic fields – the social sciences and the biosciences – but it would be too simplifying to order them in a bipolar manner: the biosciences being translocal/the social sciences being local. We have seen much more that, at different moments, different reference systems are at work and that these maps are more or less closely intertwined with the epistemic core activities, i.e. the creation and mobility of research objects as well as the ways of doing research. At the same time, the pressure of science policy systems in both cases is clearly on globalising research, while remaining national is often being staged as avoiding competitiveness and thus as lacking quality.

Yet implicitly much of what we have discussed in this sub-chapter also poses the question of ‘scientific language geographies’ which are at work when trying to elaborate regional or national maps. Thus it is not only about nation states and their historical and cultural frames, but also about the language spoken and how important it is perceived in an overall ‘ranking of languages’ in the scientific realm. Writing in English is generally tacitly equalled with ‘publishing internationally’ while, for example, writing in the German language is perceived as much more local, even though it would address a language community inhabiting three countries (Germany, Austria and Switzerland) and counting some 95 million people. Similar traces can be found in mobility accounts and the way places to move to are valued and ranked. This might then

form part of the explanation – as we will discuss in Section 2.2 below – why certain tacit geographies work so well and why, for example, British researchers feel much less pressured by internationalisation than those from the other countries represented in this study. Furthermore, the importance of language in framing and expressing epistemic work is fundamentally different in the biosciences as compared to the social sciences. While the former have agreed to a highly codified and extremely condensed way of communicating outcomes of research, the social sciences understand language as an integral part of the knowledge produced. The latter situation creates considerable complexities for researchers both in asking questions, creating objects, as well as in communicating outcomes which can travel across language boundaries. Some of these difficulties were visible in the KNOWING project itself. The common language of communication being English and only a few members speaking some languages represented in the consortium (Finish, German, Slovak and Czech) produced considerable hurdles in making the comparative work happen.

2.2. Tacit geographies: centres/peripheries & East/West

Besides using more or less nationally structured maps in order to do their positioning work, researchers also deploy other more implicit geographies. Given the focus and design of the KNOWING project, two closely intertwined sets of values seem of particular interest: centre/periphery and East/West. Obviously, using these notions does by no means suggest that these are stable categories or even that they are fixed reference points explicitly addressed by researchers. Rather, both are more or less invisible, tacit modes of ordering being performed in contemporary research systems, materialising only rarely explicitly as clearly delimited categories.

A step back and the reflection of yet another dominant idea of ordering research – the network idea – is needed if one wants to understand the performative character of these two pairs of notions. Both in science policy papers, as well as in the way researchers narrate their being part of a community, the network metaphor seems to be omnipresent. Cooperation networks, exchange networks, networks of excellence, citation networks, networks created through infrastructural measures are but a few examples of how places and researchers are tied together. Both sets of narratives on networks – by policy makers and researchers – reflect pecking orders with regard to places to which one should link, to descriptions of who is at the centre or on the periphery, to venues where one should publish, and much more. These networks have, over

the past years, increasingly turned into visual maps, being densely used in science policy to do positioning and ordering work. This fits in quite well with what Castells [2000] offered as a much broader diagnosis for contemporary societies – we live in a network society, with networks constituting the new social morphology of contemporary societies, with a “predominance of social morphology over social action” [Castells 2000: 500]. Networks organise, dis- and reorganise the rhythms in which we live, the actions we (can) take, the collective memories we construct and much more. They assure relationships across boundaries, be they epistemic, institutional or national. Networks thus become important multi-layered trading zones [Galison 1997]. The notion of the network, however, is also made up of imaginations of a physical structure, with intertwined paths, hubs, nodes, some of which manage to become obligatory passage points [Callon 1986], while others remain hardly visible and on the periphery. And after all, networks allow implicit orientation while trying to translate the complexity of multiple relationships.

Networks and their orderings, however, are multiple and not necessarily stable over time. They demand continuous work from their participants to uphold or dissolve a specific version of them or to reconfigure them when change is aimed at. Every node in such a larger network is in turn a smaller network (e.g. the effort made by institutions to be recognised locally and obtain institutional or policy support) that manages to stabilise the node and to open up potential ways of tying into larger networks. Being a central institution in the context of national networks might allow, in specific ways, tying into bigger international networks. Centres and peripheries, as much as East and West, can thus not simply be seen as existing stable entities, but they are produced by, are an effect of particular strategies of surveillance and control [Law 1992].

Indeed our observations give some clues as to where modes of ordering along the value sets of centre/periphery or East/West become important. Having asked researchers in our first round of investigations to name the most important collaboration partners, the central research institutions in their fields, as well as key journals could indeed be seen as a map-drawing exercise. Asking them for the most important collaboration partners, institutions and journals does not only make visible their ties, but also the implicit orders that make them judge certain links as more important or worth mentioning than others. Thus the answers should not merely be interpreted on a factual level, but much more as an exercise performing certain pecking orders.

Taking an exemplary look at the research collaborations researchers mentioned, it does not come by complete surprise that what emerges is a clear ‘westward trend’ in collaborative ties. This indeed holds for

both epistemic communities, although it is played out very differently for the biosciences than for the social sciences. Taking the Czech and Austrian data as examples, interesting variations become visible. They show the process character of network imaginations held by researchers and the lived differences between the epistemic cultures [Cervinkova et al. 2006: 20-21, 27-29; Felt, Sigl, Wöhrer 2006: 31-32]. Half of the network ties Austrian bioscientists mentioned were located on the North American continent, followed in order of frequency by Germany, France and England. Cooperative links with colleagues or institutions from countries geographically further East were far less – if at all – visible in researchers’ narratives. International collaboration is thus geared foremost to building or strengthening networks to what is seen as ‘the central cloud’ in this global network, namely the US and Canada. This has been a development over the past decades in the Austrian biosciences, as some of the senior researchers remind us. A male bioscientist remembers when he was a post-doc,

“doing a post-doc (abroad) was the exception. This means that at that time internationalisation was not as self-evident in Austria ... as it is now in (my institution). It is self-evident to everybody that our research is international and that we have international co-operations. This was unusual at that time. Like that you had no possibility to rank or assess yourself. This means there was nothing to compare with.”

[AT_Int_BS_m]

From the Czech data, we could see slightly different patterns in the biosciences [Cervinkova et al. 2006: 27-29]. While we definitely also find the trend of going West confirmed, as well as a strong narrative on the US connections, particularly in the biosciences, collaborative ties are also clearly visible with Western European countries. This is particularly triggered not only by geographic proximity, but also due to the strong emphasis on EU research possibilities (see remarks below on ‘westernisation’). As the analysis of policy discourse [ibid.: 4-11] has suggested, changes are clearly driven by the incentives of EU policies in the Czech case, creating a felt need for compliance and compatibility with the European Research Area and its frameworks. This is in accordance with policy documents where non-EU countries like Russia, China, and countries from the Middle East or Africa are mentioned in passing and referred to – as is the convention in EU co-operation policies – as ‘third

countries'. Prospective or formerly established networks of co-operation that might differ from the dominant focus are further eclipsed [Gorska et al. 2007: 14]. This does not mean that there is no collaboration with 'third countries', but that there seems to be the implicit assumption that 'going West' – both in terms of collaborative ties as well of physical mobility – is an important value, assuming that other researchers further East would move in and fill gaps emerging through the triggered brain drain. Indeed similar patterns could also be observed in the Slovak data, however here linking up with Czech researchers is further mentioned as an essential way to gradually tie into international collaborative networks. In accounts of Slovakian researchers, the Czech Republic was regarded as the environment where in a first step innovations would come from. To a certain extent, it became 'the West' from the Slovak researchers' perspective and clearly shows how deeply context-bound the imaginations and realisations of 'the West' are.

For the social sciences things look quite different. Local ties seem to play a much bigger role both on the social as well as on the epistemic level. Yet this finds little reflection in policy discourse. Common histories are narrated and densely grown, more local networks referred to. Scientific knowledge is perceived as being generated out of, about and for these more local contexts. And social science knowledge is conceptualised as deeply intertwined with language, an issue absent in the reflections of bioscientists. Thus epistemic locality is both rooted in the objects of study as well as in the analysis and expression of outcomes and is partly tied to the language in which it finds expression. Taking the Austrian data as an example, more than 60% of the collaboration labelled as international was in fact with German partners [Felt, Sigl Wöhrer 2006: 32]. Virtually no overseas collaboration is mentioned, the rest of the nodes being located in Western Europe. So also here the 'going West'-paradigm still seems important, yet the language affinities play a fairly important role and could be understood as a different form of remaining more local. This does not necessarily mean that researchers have no connections going geographically East, but that these do not make it on the short list of ties they regard as important enough to communicate to the interviewers. Similar observations could be made from the analysis of the Slovak data. Here Austria and Germany are dominantly pointed at for the collaborative ties (which could also be a hint towards language preferences), again the Czech connection receiving an important place in the positioning work. Finally, we see a slightly different pattern for the Czech social scientists, the connections mentioned being dominantly with North America and the UK, followed by western non-Anglophone EU countries, only 20% remaining in central and eastern EU countries.

What holds true for all social science groups studied in the KNOWING research, except for the UK group, is the fact that internationalisation is an issue, partly expressed in concerns about being (regarded as) too local or of lagging behind. Yet it often remains unclear what internationalisation concretely means and it is crucial to differentiate between its many possible meanings. Does internationalisation refer to the level on which research problems are defined and framed, to the collaboration in which knowledge production takes place, to the wider networks one is part of, to the language of publication, as well as the international visibility of the publication venue or to the mobility of researchers? The concrete understanding of the notion internationality/internationalisation often remains underspecified in the social sciences. It thus manages to become a kind of ‘boundary ideal’¹⁴ everybody would recognise as desirable to follow, while at the same time attributing rather different meanings to it. This explains why, in some countries, we find a diversity of narratives on whether or not a field is sufficiently international or if and what concretely should be done to reach the ideal.

In the Austrian context, this issue was approached explicitly on rather different levels. To increasingly publish in international peer-reviewed journals was quite a big issue, as university indicators start to focus on this type of output. Yet simultaneously ambivalence was expressed whether this would not narrow down and mainstream research. Mobility also was seen as becoming an evermore important topic, in terms of both going abroad and attracting international scholars to Vienna – something which social scientists saw much more as characteristic of the biosciences. Finally, international networking was still understood to be lacking. In the Report of the Finnish Academy of Sciences [2003] we find similar argumentations, stressing the need to further develop internationalisation both on the level of research collaborations and publications. European research programmes were seen in particular by Finnish policy makers as a means of improving the situation. Many of the assessments produced on the degree of internationalisation, however, stem from indicators focusing only on specific kinds of publications, citations and impact factor counts. These limitations to a narrow audit approach are openly addressed in a recent Finnish Ministry of Education publication, stressing that “in social sciences these indicators have little reference point as they are highly problematic in multiparadigmatic disciplines” [Puuska, Miettinen 2008]. In the case of the UK, it is interesting to remark that the life course interviews with social scientists revealed probably even fewer international and specifically European collaborative connections than the other countries under study

14 Named in analogy to Star and Griesemer’s [1989] “boundary object”.

[Garforth, Kerr 2006]. Collaboration was more often with researchers outside the home department but within the same university or in other national universities. Yet we found no evidence of a narrative of ‘lack’ or ‘deficit’ in this respect, which could be interpreted in terms of a self-understanding of being among the leading nations in the social sciences and/or as related to the epistemic objects researchers were dealing with: national legislature/welfare systems.

Both the need for internationalisation, together with the catching up rhetoric, create tensions clearly visible in the Czech case. Here we encounter a strong rhetoric of needing to catch up with ‘Western European standards’ (= higher standards). Simultaneously, in the social sciences, the regional role is emphasised by stressing that excellence should be reached in the framework of what is labelled as Central Europe. Therefore the establishment and development of the institutional level seems to have a form of a ‘modernisation’ story in a strong sense of an area being underdeveloped and rather abandoned during the communist regime and which will grow under the new conditions [Gorska et al. 2007: 21-22]. This rhetoric figure of constructing a historical account starting from a problematic and difficult past and narrating a much better future is a quite common feature of many catching up accounts. This kind of story-telling is well illustrated in a recent *Nature* article with the telling title, “Westernizing Eastern-block Science” [Schiermeier 2008: 558]. Here we find extensive explanations why ‘Eastern science’ was/is lagging behind: the separation of education and research, the lack of a need to compete for funding or aging staff are but a few elements put forward. However, then the tone becomes more optimistic, stressing a few outstanding fields, and taking the Czech Republic as an example, which had managed to recruit Czech- and Slovak-born researchers from abroad, put at their disposition the best infrastructure and thus had created quite a flourishing niche. Furthermore it is stressed that in a small country in the phase of building up research, outstanding individual researchers could gain considerable influence, thus making these jobs attractive. Similar stories then followed for countries like Estonia and Hungary. What is woven into this account is a clear understanding that there is a ‘western way of doing things in research’ – which is framed by using a vocabulary of competitiveness and aggressiveness – which is the only model to follow if one wants to be successful.

East/West as well as centre/periphery distinctions and the moral orders accompanying them also become visible through migration flows. A recent study on elite migration [Showkat et al. 2007] has shown “a remarkable funnelling of talent from a large number of donor countries into a small number of receiving countries” [ibid.: 27] and to a small number of

receiving institutions. There thus seem to be clustering phenomena that create highly visible epistemic orders. Yet the same study also underlined that they did not find what is often implicitly assumed in the mobility discourse, namely that there is “clear productivity difference (some years later) between the elite movers and the elite stayers” [ibid.: 28]. What a German study of ‘brain drain’ phenomena has further shown is that it is not predominantly the material conditions that make people move, but importantly the gradient in the symbolic value between the places where they are and where they aim at being. Both German researchers in foreign countries as well as immigrant researchers in Germany highlighted “the possibility to work in a prestigious institution” as an important criterion for mobility. Major losses of researchers are reported for “Eastern European countries, which are losing their staff both to the United States and to the ‘Old’ Europe. Furthermore, these countries hardly attract any ‘brain gain’” [Böhmer 2006]. Policies in countries like the Czech Republic or Austria stress the need to stop the brain drain and to attract skilled work and talents. As this is inscribed into the logic of competition (see quote by the Austrian Minister of Science and Research), the fact that such measures would need draining talent from other regions remains unaddressed [see also Gorska et al. 2007: 14]. Thus being situated in the core or on the periphery matters enormously for an institution also in terms of being attractive. But also specific ways of categorising staff and offering work contracts matter, as they affect both researchers’ self-perception but also the ways in which they are considered by their environment. In the Austrian context, these issues are receiving considerable attention, as recent reforms of the employment system have shown [Felt, Sigl, Wöhler 2006]. This quite nicely fits in with Schiermeier’s [2008] account of the capacity to make researchers who had left Eastern European countries return, if the visibility and attractiveness of newly created jobs are high.

While we have now focused on collaboration and the order of places, similar observations could be made when it comes to publications. While in the natural sciences the central nodes seem to be ‘universally’ agreed upon, at least in a specific epistemic sub-field [see Garforth, Kerr 2006: 31-33 on “epistemic landmarks”], the social sciences appear much less focused. Language differences come into play, locally read and distributed journals exist and agreement on one common order does not seem to be something to aim at for researchers. Rather, as we could see from the Austrian discussions, too much ordering activity was perceived as too normative for their understanding of the openness of the field. However, more or less implicit hierarchies also become visible when regarding the ‘who translates whom’. While most Czech, Slovak

and Austrian social science researchers mentioned textbooks being translated into their respective languages, there are hardly any accounts of textbooks or other materials being translated into languages such as English from the initiative of the UK or US researchers or publishing houses (evidently researchers with non-English languages may themselves want to publish in English). This again shows a tacit centre/periphery divide that is played out through language and reconstructed continuously through the idea of who is teaching whom and who has to learn from whom. In that sense, even if explicit efforts aim at deconstructing the East/West divide, they often participate in reconstructing it.

Drawing all these observations together, one might ask the question of how these orders matter. At a time when internationalisation, race, competition and the need to catch up with the best has become the gold standard in talking about research, one might say that these implicit orders matter enormously. This is further reinforced by the fact that much attention is paid to publishing in high impact journals virtually all edited in 'the West'. And even if there are more 'local' journals listed in the corresponding databases, their impact factors are generally marginal. The fact that apparently moving West is tacitly equated with higher standards in research, 'better science' and access to central nodes, matters with regard to researchers' self-understanding, as well as to their understanding of what it means to occupy a specific epistemic territory – i.e., the social sciences or the biosciences. While policy makers use quite a universalised language when talking about science and stressing that the social sciences are treated on an equal footing with the natural sciences, the value systems performed appear strongly 'inspired' by the latter. Thinking in terms of internationalisation and 'moving West' as values in themselves thus creates orders not only between the different epistemic fields but also for the researchers themselves, who 'have to accept' being conceptualised as backward and not up to the quality of top research. In that sense, it is also important to see more local/national decisions such as where to create centres of excellence – as we have learned for example from our Finish data or from the Czech case – as largely framed by the capacity to demonstrate international orientation and competitiveness.

2.3. Disciplinary maps and other epistemic orders

One might start by asking: why look at disciplines or disciplinarity when wanting to understand different epistemic communities at work? Is the idea of the epistemic community not trying precisely to overcome the quite rigid imaginations linked to disciplines, replacing them by a more

complex mixture of social, cognitive and practice oriented orders? Has the difficulty of capturing what actually happens in research in terms of disciplines not frequently been stressed? Indeed, as already mentioned earlier, authors like Stump and Galison [1996] have clearly hinted at the disunity of science, and have shown that knowledge production often emerges from diverse assemblages of disciplines and practices. There were also many other approaches trying to capture epistemic order in ways alternative to disciplinarity. Tony Becher [1989], for example, tried to draw our attention to what he called “people-to-problem ratio”, arguing that the way research is organised has much more to do with the relation of the number of identified key research questions to the number of researchers ‘living’ in a field than with anything like discipline. This in turn led him to postulate the existence of *urban* and *rural* research scenarios, a spatial metaphor meant to translate the fact that certain cognitive territories are densely populated, develop with high speed, have tight communication networks and demand from researchers cohabitation/cooperation, while others would consist of wide ‘knowledge plains’, where people could wander and do not necessarily either go for competition or cooperation. Richard Whitley [2000], to take a further example, would argue that scientific fields could best be characterised by their respective degrees and types of *mutual dependence* and *task uncertainty*. With the notion of mutual dependence, he tries to capture “the need to adhere to particular standards of competence and criteria of significance in order to reward important reputations for contributions” to research results [ibid.: 120]. Task uncertainty relates to the degree to which a field is paradigm-bound and can thus produce predictable, visible and replicable research results [ibid.: 120]. Both factors bring along certain work practices, e.g. of a more or less collective or individuated kind, which would not necessarily reproduce the natural/social science divide. Taking the example of the British research field of economy, he convincingly shows that even within one field different combinations of task uncertainty and mutual dependence coexist and thus lead to different research practices.

Also Knorr Cetina’s [1999] work on epistemic cultures showed how different epistemic objects and technical equipment needed correspond to fundamentally different social organisations and practices in two fields within what is generally labelled the natural sciences: high-energy physics and molecular biology. Hence she made visible how these distinct framings brought about rather different ways of relating individual researchers to more collective entities. Extending the argument further, academic analysis as well as policy discourse have started to argue for moving beyond the tight classical boundaries of disciplines, deeply

rooted in our system of academic reproduction and to become more open and transgressive. The ample literature produced around the mode 1 / mode 2 debate [Nowotny et al. 2001; Gibbons et al. 1994] addressed in the introductory chapter of this book, the analysis building on the triple helix model of changing university-industry-state relations [e.g. Leydesdorff, Etzkowitz 1998], or Weingart and Stehr's [2000] discussion of interdisciplinarity, all argue that the imagined boundaries around disciplines might turn out to be more of a hindrance than a support. Disciplines stand for a certain conservatism, protection of shared territories that are often associated with specialisation and are assumed about to become less and less important. Notions such as inter- or transdisciplinarity have taken an omnipresent place, particularly in policy debates and institutional discourse, being staged as the answer to more complex sets of problems needing to be addressed.

The answer to the question, "why look at the role of discipline?" is simple and complex at the same time: simple because researchers have learned – mostly through socialisation in science – to use what could be labelled disciplinary maps as a reference system, a navigation card, an imaginary map against which they describe their epistemic travel itinerary. Complex because actual work practices are indeed structured through multiple forces such as the ones addressed by Becher [1989] or Whitley [2000] and are always imbued by both the ideals of disciplinarity as well as the idea that innovation happens because people break with these ideals [Weingart, Stehr 2000]. Thus we are not so much interested in offering yet another way of capturing and classifying research work, but will reflect what both disciplinarity and its other forms (be it inter-, multi-, transdisciplinarity) actually mean for researchers, their careers and the institutional orders in which research is supposed to take place. How do the researchers in the institutions we investigated deal with these issues, what does it mean in terms of self-understanding and identity and do certain research environments make people feel that disciplinarity is an attractive form of epistemic cohabitation or not?

Investigating material from five national contexts and different epistemic environments, it quickly becomes evident that there is no clear line emerging of how researchers and institutions dealt with disciplines as a basic explanatory tool, how they assessed the role of disciplines and where, why and how they imagined disciplines as closing down/opening up possibilities for research. Doing so, we have to keep in mind that interdisciplinarity does only become visible and explicit in places and constellations where disciplinary boundaries are strong and guarded while, where boundaries are weak, it is simply "permissively assumed to be natural excursions" [Reese 1995].

These complex relationships to disciplinarity can be traced in the institutions participating in KNOWING on several levels. Organisational *identity as shown to the outside world* is one place to look for disciplinarity. Taking the Austrian material as example, important differences of how epistemic communities relate to disciplinarity can already be captured by simply taking a look at the way the institutions present themselves on the web, as well as by the way the relation between research and teaching is performed. Indeed the image of a traditional discipline is staged quite strongly in sociology, describing research as being situated “in the spirit of the tradition of Viennese sociology”.¹⁵ Thus there is a clear reference to historical tradition, yet research foci listed remain without any further detailing. More effort is spent on teaching issues with regard to the sociology curriculum. This creates a certain tension that could be interpreted against the background of the observations made by Heintz and co-authors [2004]. They argue that while, in research, disciplinary boundaries have been challenged, crossed and partly rewritten, university education – and in part reproduction of the field e.g. through habilitation – is still organised very much along classical disciplines. Indeed we could also use the distinction between science and research introduced by Latour [1998]. Thus teaching could be understood as being much more oriented towards reproducing science, while the rest of the tasks members of universities had to undertake were much more linked to research and thus were more messy – less ‘disciplined’ – by their very nature. And indeed, when moving beyond the higher education sector into extra-university research institutions in sociology, the self-understanding seems to emphasise much less the disciplinary nature of their work. Similar observations came from the British and Slovak contexts. Thus, in the university-bound sociology setting, we could argue that teaching, education as well as publishing still remain measured against disciplinary understanding.

That there is identity work taking place can be seen from the Czech social science case. The representations towards the outside world through web pages clearly mirror certain changes in the value system. Similarly to the Viennese case, history strongly mattered in the self-understanding, as an institutional web site put ‘the founding father’ at the core of identity work towards the outside world. Yet a recent reconfiguration of the web space has aimed at creating quite a different image. The web appearance of the Czech social science department as well as the whole faculty turned more PR-oriented: more visual, pointing to the international dimension of the study and connected lifestyles, and to future employment opportunities of graduates. The previous

15 The page has been changed in the course of the project.

history-oriented front page of the departmental site was included yet hidden on the website – under the label “history”.

In the biosciences, on the contrary, once again using the Austrian example, the organisation presents itself as structured through research areas and labs rather than by disciplinary considerations (e.g. home page of the life science institution). What is more, there is virtually no reference to the historical dimension, the presentation clearly buying into the rhetoric of achievements of the research, presenting the most recent publications. The home page clearly states that “the majority work on basic research topics but a significant number are also active in more applied fields of research”. And the text continues by stressing that “research in our groups is highly interdisciplinary and the topics of research transgress departmental borders”. Remarkable here is that the reference to teaching is structured very differently. With regard to undergraduate studies, there is no mention that part of the staff is lecturing there. Much more weight is placed on post-graduate studies, where particularly the PhD programme is advertised as “offering students from all over the world the opportunity to participate”. Thus training the next generation of researchers is at the centre and, in this context, the border drawing evidently appears quite differently than in the case of undergraduate teaching.

The relation to disciplines is, however, by no means unambiguous. On the one hand, disciplinarity is staged as limiting the innovative capacity and this rhetoric is rather dense in the policy discourse on inter/transdisciplinarity as a better way of answering complex societal questions and as the capacity to frame a problem in more innovative terms through the combination of know-how, methods and knowledge (see Chapter III of this volume for a more detailed discussion). On the other hand, researchers seem to perceive *disciplines as a historically-grown, somehow stable and protected space* in which developments can happen, which can grant authority [Bourdieu 1975], where agreements are reached, methods stabilised and knowledge shared. A discipline is a place where one can retreat to and where others can be “legitimately” denied access. Disciplinary boundary drawing can thus be used as a protective and valuing mechanism: it is used by researchers to flag out their competences as compared to others dealing with similar questions; it allows to deny to “outsiders” the capacity to question ideas and scientific claims. The following extract from a focus group discussion among bioscientists in the Austrian context nicely illustrates the meaning of disciplinarity:

“(m)olecular biologists lack basic knowledge. They know everything that is modern, that can be read in Nature, but they do not know how and why these ideas were developed, why researchers began to do such experiments. And this is a pity ...”

[AT_FG_BS_m]

Disciplinary is thus used to argue for the better foundation of the actions taken in research, a higher capacity to reorganise knowledge and observations in terms of theoretical interpretations and, in the social sciences, also a better knowledge of research methodologies.

The reference to discipline, however, also appears under different guise. In one of the gender studies units in the KNOWING countries, identification with sociology as a core discipline is seen as important and also explicitly expressed, for example, in grant proposals. Yet this also has clear drawbacks. Gender studies, being rather interdisciplinarily organised, bring in people with a non-sociological background, which in turn means that they cannot be easily integrated into the teaching of sociology. This makes researchers who are at the core of gender studies suddenly move to the periphery when the issue arises of being integrated into the formal structure of the department. A related, yet different example could be traced in the UK data. A researcher accounts for her interdisciplinary experiences in a then newly-founded gender studies department as follows:

“I think I called myself a sociologist and was sort of responsible for the sociological input on the teaching. But on the other hand ... We always had two members of staff present in a session ... which meant that I would teach in a session with ... a literary person, or with ... a history person. And I think that experience very much changed me into a much more kind of multidisciplinary, which I still think I have to some degree.”

[UK_Int_SS_f]

She continued to explain that after this experience she felt unable to go back to sociology, feeling that “she was a different sort of animal to the person who had left sociology” [UK_Int_SS_f].

Yet, while the talk of dissolving boundaries and understanding work as more interdisciplinary is common to both social and bio-scientists, the

reasons for doing so tend to be different. Here we could take on board Marilyn Strathern's reflections on interdisciplinarity in the natural and social sciences. While the former sees interdisciplinarity much more as an instrumental interchange of know-how or a conceptual transfer of knowledge, the social sciences become much more intertwined at the conceptual level [Strathern 2007]. In the Czech case, this distinction becomes visible on the level of focus group discussions. Bioscientists would talk about interdisciplinarity "either as obtaining a service (instrumental use) or entering a new field and getting expertise there (conceptual transfer of knowledge)" [Gorska et al. 2007: 12-13]. Some of the Slovak bioscientists also reflect their interdisciplinarity in terms of "combining their thought styles". In the Austrian context, however, some of the researchers coming from more traditional backgrounds, like chemistry, deplored a certain lack of clear theoretical integration of results when people are only used to working in "undisciplined contexts". This idea could also partly be found among PhD students, who feared that they would not have any specialised, more profound knowledge if they moved into interdisciplinary contexts too early on. In the focus group of junior researchers this was clearly brought to the fore by a PhD student.

"No, I have to laugh, because I am a molecular biologist and I precisely know if I have biological questions, I go to see a biologist and if I have a chemical question I go to a chemist. And I always say: I am in between, I am nothing (laughs). I know a bit of everything and not a lot of anything."

[AT_FG_BS_f]

Social scientists, on the contrary, conceive the borders of the disciplines as quite blurred, with a lot of boundary work needed to purify their discipline, sociology. This means that, while during the phase of knowledge production different disciplines get a voice, the analysis in the end must be "grasped and explained in a sociological way" [AT_FG_SS_f]. Thus in the discourse, sociology might not necessarily be addressed under the guise of a discipline, but as an identity, a way of thinking, a perspective from which to look at social phenomena, a conceptual repertoire which allows an issue to be grasped [Gorska et al. 2007: 11-13].

In that sense, it is also interesting to see that bioscientists and social scientists constructed disciplinary boundaries in a similar way. Both

distinguished themselves from rather close, but more successful fields: biochemists for example from molecular biology, or, sociologists from economics, political sciences or other fields, depending on the tradition and the local organisation of the 'cognitive map'. In both fields, boundary work [Gieryn 1999] was done via claiming 'better knowledge' with regard to methods and theories: molecular biology and economy being used as reference fields by some interviewees in the Austrian context were considered by some as being geared too much towards application and lacking basic scientific methodological skills, while biochemistry and sociology were described as having more tradition and broader, more deeply developed scientific/methodological competence [Felt, Sigl, Wöhrer 2008: 20]. Thus, according to the position from which one would make the comparison, sociologists would feel better equipped than economists to react to the challenges they judged as central. The same would hold for biochemists in relation to molecular biology, a field which was judged as not (yet) sufficiently 'developed'.

However, this does not mean that there were clear definitions and understandings at work when reference was made to a discipline. We could observe in the data efforts to define sociology: it was defined not by its substance, but rather by its difference from other social science disciplines ('sociological imagination' was often referred to but couldn't be defined except in this relational mode) and at the same time sociology is said to 'embrace' all other social science. And we could also observe the struggle over defining what social science means through doing boundary work towards 'epistemic neighbours'. Regular discussions emerged around the issue whether a particular research of a thematic field (represented in a paper) should in fact be regarded as 'social science' or as something else, for instance philosophy, psychology or history – i.e. something other than 'real' social science. Here one clearly sees boundary work taking place along the distinction of what social science is not, rather than defining what it actually is [Ahlbeck-Rehn 2007b].

Complexity is added in the case of the biosciences, as we also find reflections that the field is in continuous movement and that researchers have to shift their focus along with the issues at stake at the core of the field. They often talk about having to learn something different and new, to cross some boundary within the field. Thus for some it becomes quite unclear what the core for them is. This was nicely pointed out by a Finnish bioscientist:

“what I’m doing now is molecular biology work but I have the background and I’m still doing biophysical method-oriented work that is needed in my molecular work. So that’s what I’m mostly doing, my specialty here, based on that ... So it’s hard to say, probably I’m a bit of both, or more likely half of both, I hope (laughs), in time you forget it but yeah. And I’m trying to learn proteomics, so I’ll be a biochemist, so a little bit of that also.”

[FI_Int_BS_m]

This brings us to the last issue here, namely how disciplinary/interdisciplinarity is reflected in institutional structures in terms of boundary work and different modes of ordering (for more on interdisciplinarity as a form of togetherness: see Chapter III of this volume). Indeed we quite often find reference to institutional framings when researchers talk about the possibilities or impossibilities of working across disciplinary boundaries. In the Austrian case, we find social scientists complaining about the fact that the university context tends to develop a rather closed self-understanding, hindering the crossing of disciplinary boundaries in research. Inter- or transdisciplinary research thus mainly takes place in non-university contexts. As a consequence, one would have to move between these institutional contexts in order to be able to work in less disciplined ways. In that sense, we could argue with Nowotny and others [2001] that the social sciences have not managed to adapt institutional structures to allow for this kind of research. Mode 1 research remained ideally located in universities, while mode 2 knowledge production would mainly have to migrate to extra-university research structures. We also have some indication from the Czech case that, even if research centres include researchers from different backgrounds, the division between them remains quite robust. Finally in the UK context, mode 2 knowledge production mainly occurs in contract research, while in teaching departments disciplinarity is still predominantly performed (see Chapter III of this volume).

Institutional structures in molecular biology, however, might be better suited to accommodate these new kinds of requests towards research. Bioscientists were, in the Austrian context, situated in a hybrid organisation which allowed both for thinking and working in classical university structures while, at the same time, granting certain freedoms which would be hard to have in classical academic structures and which allow for the crossing of borders. Thus research was much more

oriented towards the concrete research focus, which was then inhabited by researchers from different disciplinary backgrounds (chemists, molecular biologists, medical researchers ... working on one topic). The Austrian social sciences studied, located in the university context, still seem to pay much more attention to the meaning of disciplines.

3. The politics of labelling research

When we were analysing researchers' narratives on knowledge production activities, their struggle with two reference frames became visible: one around the quality of their academic work, the other around the notion of the relevance of research broadly speaking. Around both notions – quality and relevance – major trading zones over values of and in research emerged, showing the deep struggles between policy, societal and scientific imaginaries of where contemporary research should go and what it should contribute to broader societal goals. We thus read these negotiations over labelling research as of 'outstanding quality' or of 'high relevance' as an important, often strategic, part of researchers' boundary work trying to carve out their epistemic living space.

Policy debates around the quality of research have been around in academia for several decades and, in all countries participating in the KNOWING study, quality assessment exercises have gradually and with differing intensity become an important element in defining the possibilities and limits researchers encounter. However, we do not want to discuss quality assessment in general¹⁶ here, and the diverse forms of impact it has (had) in different national and institutional contexts, but rather turn our attention to a relatively new phenomenon, namely policies concerning 'excellence in research'. It seems interesting to question what imaginaries are created through this discourse and the accompanying measures concerning research, researchers and their relation to broader societal development. How might the introduction of this distinction between 'normal research' and 'research at its best' and the moral order that comes along, matter to researchers in their self-conceptualisation, to the space in which they get to develop their own ideas, and to their readiness in risk taking and radical innovation? These are some of the questions we would like to address.

A second kind of boundary drawing activity important to analyse takes place around the notion of societal/practical relevance of research activities. Applicability is often staged as being in sharp contrast to the

16 There is a huge corpus of literature analysing the more problematic aspects of quality assessment. Yet this analysis remains mainly on the system level and much less attention is attributed to researchers' perception of these mechanisms.

ideal of basic research, which is frequently referred to as being under threat. It seems promising to carve out the importance attributed to such distinctions, question why they have become so important and how they play out differently in different epistemic contexts. As with ‘excellence in research’ also ‘societal relevance of research’ has moved high up on the policy agenda both at European level as well as in the member states, thus creating an implicit and explicit benchmark towards which researchers have to position their work.

3.1. ‘Excellent science’ and its others

Across all the research institutions we investigated in the different countries, the larger narrative of a global competition in techno-scientific innovations, of a grim race for leadership in the world’s knowledge economy, together with the idiom of “there is no time to lose” seems to have borne fruit in many different ways. These accounts and the underlying implicit orders build on a belief that research is the key asset for making Europe “the most dynamic competitive *knowledge-based economy* in the world” and that respective structural reforms will help to render the processes of knowledge production more efficient and will attract the necessary (wo)manpower [EC 2000, 2007]. Here it seems essential to stress the tacit equation of knowledge and research and thus the clear commitment to only a specific kind of knowledge that should have its central place in the imagined economic order. Recent reforms in the university systems and partly also in national funding structures are geared towards realising these visions [Felt 2005]. Indeed these strong master narratives and the accompanying imaginaries do not only inscribe themselves into institutions’ functioning logic (such as governance systems, quality assessment structures and personnel structures), but simultaneously render potential ways of opting out virtually impossible. The imaginary of lagging behind and of needing to catch up is palpable in many institutional or national science policy narratives, and is also partly performed in researchers’ accounts. However, while the rhetoric of race and competition and its material performance through the increasing number of rankings produced, are omnipresent, the perception of one’s own place in this race shows quite important variations both among and within countries and epistemic communities. While some aim at reaching the top, others simply strive to gradually move forward in the ranks, or are more preoccupied with not being overtaken by others. Researchers also are not insensitive vis-à-vis these changes and their narratives reflect both the ways in which they comply with these imaginations and buy into certain of these frames of reference, but also the ways in which they feel the disempowering effect of needing to comply.

Indeed, there would be many ways, moments and places to investigate the categories and their accompanying orders which structure such discourses and the traces left by them in researchers' imaginations about themselves and their work. However we decided to focus only on one main boundary drawing activity: the 'excellence in research' discourses and measures and partly how they relate to practices of research assessment more generally. Both assessment in general and excellence measures in particular are powerful mechanisms for making distinctions between those who live up to the system's expectations and 'the others'; they allow for building rankings, imagining more or less bright futures, establishing pecking orders.

Already a first analysis of our material shows that the excellence discourse has reached all the countries investigated and that national and European discourses are closely intertwined (although in the UK, national excellence discourses are perhaps less explicitly entangled with European ones). Yet the way this concept becomes operationalised, filled with meaning and transformed into practice differs in interesting ways. These variations might be seen as linked to the different histories of national research systems, to the imagined place a country/institution holds on a more global research map (see Section 3 of this chapter) and in particular to when and how research assessment exercises have started to be integrated. Countries such as the UK have been gathering their experiences with research assessment exercises (RAE) for more than two decades and have actually entered a phase that could be seen as routinisation. These exercises have become an integral part of science policy makers' understanding of how outstanding research performances are made visible and rewarded. Beyond the RAE, on a broader level, policy documents on academic careers also stress the need to increase opportunities for 'excellent' and 'the best' researchers, in order to counterbalance the rather fragmented academic careers in the face of 'soft' funding. In such cases 'excellence' is strongly tied up with supporting early career researchers on to an academic career track and building or enhancing research capacity [Roberts 2002]. Much less attention (at least explicitly) is paid to 'normal' research/ers and 'ordinary' career options. At the time of writing this book, debates were going on concerning significant changes to the mechanisms for carrying out these assessment exercises. This embraced exploring possibilities of moving away from more peer-review centred, large-scale assessments to a more indicator-driven system (citation, impact factors, ...).¹⁷

17 In the UK, where the research system has started much earlier to change into a more entrepreneurial structure with dense assessment activities, we find an interesting analysis of the impact these changes have on the research institutions. See for example

Other national research systems, like the Czech or the Slovakian and partly also Austrian, are only on their way to implementation, with Finland finding itself in the middle-field. The more specific debates around excellence in research take rather different forms in these national contexts. In some countries the discussions result in the creation of so-called centres of excellence, thus institutional settings that are conceptualised as places of outstanding quality. One example would be the recently founded Austrian Institute for Science and Technology, which conceptualises and positions itself as “a scientific flagship organisation for research and post-graduate studies at the highest level.”¹⁸ Teaching only in the post-graduate area, it is supposed to “perform basic research at a world-leading standard and shall open up and develop new areas of research”. In the Czech Republic, the idea of centres of excellence dates back to the late 1990s when it was introduced as a measure supported by the EU for accession countries to improve their integration into (European) quality science. Thus the idea was that, through singling out a small number of centres and offering them better conditions, a differentiation process within the national system could be launched, which would in turn create a more competitive dynamic. The Finnish research system also knows centres of excellence, typically associated with a high research profile and being well funded. However, achieving this status did not necessarily mean getting rid of the more mundane obstacles to research work. For example, the studied Finnish centre of excellence in the biosciences was by no means exempted from budgetary cuts in the 1990s [Kovalainen, Poutanen 2007].

Similar expectations are expressed for funding programmes focusing on supporting ‘excellent researchers’. The implicit assumption: researchers identified as excellent will – given good working conditions – create more innovative research output than others. The creation of special prizes more than 10 years ago in the Austrian context, such as the Wittgenstein Prize (for internationally outstanding Austrian senior scientists)¹⁹ and the Start Prize (for internationally outstanding Austrian junior scientists) are precisely aimed at offering ideal working conditions for those labelled ‘world-class researchers’ for a period of five or six years respectively. Since 2007, the Czech Academy of Sciences has awarded “academic premium” to selected researchers to support “financially and morally scientific excellence” by granting an annual budget for six years to be used for research.²⁰

[Morley 2003; Lucas 2006; Henkel 2000].

18 See <http://www.ist-austria.ac.at/>.

19 See <http://www.fwf.ac.at/> and <http://www.wittgenstein-club.at/>.

20 See <http://www.cas.cz/ostatni.php?m=4-10&ID=346>.

Such prizes go hand in hand with the logic of the European Research Council's starting (two to nine years after PhD) and advanced investigator grants, which are also meant to give generous financial support for a period of up to five years.²¹ The main aim of these grants is

*“to stimulate scientific excellence by supporting and encouraging the very best, truly creative scientists, scholars and engineers to be adventurous and take risks in their research. The scientists are encouraged to go beyond established frontiers of knowledge and the boundaries of disciplines.”*²²

At the time of writing this book, the first wave of grants was attributed, and the second round of applications is underway.²³ A first look makes clearly visible the implicit order of countries from centre to periphery and also the East/West order debated in this chapter. The UK as host country for ERC grant winners was particularly successful; Finland and Austria are by quite a large distance in the middle of the rankings and the Czech Republic is in the last segment of the statistics. Slovakia does not have any successful candidates. It is further interesting to remark that while, in the UK, Finland and Austria, grant receivers stay at least partially in their countries, all grant receivers of Czech nationality seem to leave the country to do their research in another place.²⁴

Here it seems worth making a couple of further observations. What Austria and the Czech Republic have in common with regard to the distribution of the prizes are two remarkable elements. Firstly, there is a clear gender bias in their distribution. In the case of Austria, out of the 21 scientists who won the Wittgenstein Prize, only two were women. In the Czech Republic the first six prize-winners (in 2007 and 2008) of the “academic premium” award were all male. A gender bias is also clearly visible in the ERC grant distributions, in particular when it comes to the advanced grants.²⁵ Here there are no female award-winners in the

21 See <http://erc.europa.eu/index.cfm>.

22 See <http://erc.europa.eu/index.cfm?fuseaction=page.display&topicID=12>.

23 See <http://europa.eu/rapid/pressReleasesAction.do?reference=MEMO/07/586&>.

24 See http://erc.europa.eu/pdf/AdG1-Statistics_06_11_08.pdf; There are a few counter-examples in the Czech case: Recently a Czech physicist receiving the EURYI award decided after his post-doc at Princeton to return to Prague. This was strongly reflected in the media as a success story for brain return.

25 See http://erc.europa.eu/pdf/AdG1-Statistics_06_11_08.pdf; for the young investigators, the gender distribution is 25% on average, so substantially higher than for the advanced investigators.

Czech Republic (prizes are all in the domain of physical science and engineering), below 10% in Austria and the UK, only Finland having a strong representation of female advanced researchers. Needless to say, if one looks not only at the number of women who could have technically been able to apply and receive such a prize, but also compared to the number who actually handed in proposals, the number of female award-winners is low. Presenting its first conclusions, the ERC explicitly referred to the low number of women winning advanced grants.²⁶ This reminds of the study by Wenneras and Wold [Wenneras, Wold 1997] and of role of tacit assumptions about quality of a candidate. Thus it seems essential to question whether or not such excellence structures reinforce exactly those differences which they explicitly claim not to support (e.g. gender segregation in research).

The second bias lies in the scientific fields from which researchers come. In the case of Austria, out of the 21 prizes, only three went to the social sciences and humanities and the majority of the remaining ones went to the life sciences; in the Czech Republic so far all have remained within the natural sciences. Similar distributions can also be seen from the results of the ERC awards. Further, first clear hints as to the onset of the Matthew Effect [Merton 1968] triggered through such prize structures can be observed. For example, the 2008 Wittgenstein Prize-winner had already been awarded the Start Prize in 2001. Other Start Prize-winners have, either in the year after or the year before, also won the ERC Young Investigator Award. Thus we find concentration phenomena – in terms of financial means, gender and research fields – which affect the research system as a whole. The Matthew Effect is also visible on the level of nation states. The UK can definitely take important advantage of its international position, both by having won about 20% of these new funding opportunities as well as by being able to attract researchers from other countries with their proposals either to stay in the UK or to move there.

The third essential observation seems to be the reproduction of tacit geographies and the distinctions addressed above through the European excellence programmes. A statement summarising the outcomes of the first round of grants allocated clearly stated that although “grants were awarded in 21 countries, the geographic distribution is quite uneven, reflecting the research environment in each of the host nations”. Thus there is awareness about the situation, yet at the same time it is downplayed by simply stating that “as countries increase their research and development budgets, the geographic distribution will likely be more level”²⁷.

26 See <http://www.earto.eu/nc/service/news/details/article/european-research-council-presents-its-first-conclusions/>.

27 See <http://www.earto.eu/nc/service/news/details/article/european-research->

Although these are but a few hints, they definitely should be read as traces of multiple important orderings at work introducing certain values, with effects not only on researchers' imaginations about themselves and the qualities needed to be regarded as excellent, but also on the place their field holds in the ranks of scientific disciplines. Drawing together these observations, a number of interesting reflections concerning the mechanisms at work as well as the underlying assumptions, values and expectations when achieving excellence may be made.

Firstly, looking at the way 'excellence' was dealt with, both on different national levels, as well as on the European level, we could argue that its apparent success is linked to its capacity to become yet another 'boundary ideal' next to internationalisation. Indeed 'excellence' is on the one hand rigid enough to be recognisable to all involved, while at the same time it is sufficiently malleable to be understood and implemented in rather different ways. As we have seen, different countries have started at different moments in time, from different starting positions and based on different rationales to implement some major distinctions between research/ers they label as outstanding and 'the others'. The trading zones, where the very meaning of this notion is negotiated, however, remain largely invisible to the community. Who gets access to such spaces of negotiation, whose values and aspirations are represented and for whom they open up or close down possibilities are thus essential issues. Thus linking this to our observations made in Section 3.2 regarding the important divides between centres and peripheries or between East and West, it is obvious that the notion of excellence as performed is a 'Western concept', with a strong implicit assumption of following 'the US model'.²⁸

Secondly, it is highly relevant to understand the ambivalences with which researchers position themselves not only towards the growing intensity of assessment exercises, but also towards the unclear relation between 'ordinary quality criteria' and the identification of excellence. Scepticism was expressed towards the ever-growing normative framing of scientific work through evaluation criteria used by academic and funding institutions. Researchers in both disciplines doubted that impact factors of journals actually measure anything close to quality and they accounted from their experiences that some topics (*à la mode*) definitely have better chances of being published in highly ranked journals than others. A bioscientist from Austria showed his ambivalence through

council-presents-its-first-conclusions/.

28 It is interesting to note that the US is often taken as an example, without considering the often fundamental differences in historical framing, institutional cultures and funding structures.

relating the anecdote of a crystallographer who found

“the correlation between the impact factor of the journal and the quality of the crystal structure to be reciprocal: the higher the impact factor, the worse the crystal structure.”

[AT_FG_BS_m]

Yet the appearance of debates to look for alternative indicators (like the so-called H-index)²⁹ shows that, while researchers feel that indicators hardly adequately represent the complex work procedures in research, the auditing ideal is already so deeply rooted in the research system that there is a felt obligation to look for an alternative indicator.

Indeed our interactions with researchers clearly made visible the quite strong impact quality assessments had on the way people conceptualise themselves and develop imaginations of what good research might be. Indeed when talking about ‘good science’ or ‘quality research’, researchers often make their judgements against the backdrop of scientometric evaluations and quantifiable criteria. Thus from our Slovak interviews, to take but an example, one could see that ‘good science’ is articulated as science that delivers results publishable “in good scientific journals recognised in the wide scientific community” [SK_Int_BS_m]. Journals are the gatekeepers, with international journals being central in order not to “remain buried in any of these scientific graveyards, by which I mean most of domestic scientific journals” [SK_Int_BS_m]. Quality and internationality (English-speaking) are here more or less explicitly equated and this is by no means a unique case. Also, looking at ratings of the UK 2008 Research Assessment Exercise (RAE), one can clearly see that the top scores are attributed to those who are “world-leading in terms of originality, significance and rigour”, while those who are ‘only nationally recognised’ score very low. As a consequence, citations in refereed international scientific journals with high impact-factors are taken as public proof that other scientists build on the locally produced knowledge, citations thus becoming a tool to technically represent not only ‘the quality’ of individuals, but also of workplaces (i.e. labs,

29 The H-index is named after the physicist Hirsch. It aims at quantifying both the actual scientific productivity and the apparent scientific impact of a scientist. The index is based on the set of the scientist’s most cited papers and the number of citations that he/she has received in other people’s publications. The index can also be applied to the productivity and impact of a group of scientists, such as a department or university or country.

institutions) and entire epistemic communities. Indicators thus allow one to draw maps highlighting the 'rich territories' and to develop rankings to establish seemingly clear orders – in short: audit society at its best.

Although there are critical debates, e.g. on issues of anonymity of reviewing in small fields, in general bioscientists seemed to live much more easily with such ordering principles and some even naturalised these measures, stressing that this was a reasonably good way to monitor what was happening. Needless to say, those who agreed with such indicators were publishing internationally quite frequently, well before the introduction of such quantitative indicators. Thus they refer to them in quite an unreflexive way when positioning themselves in relation to others. Only a few rather critical voices expressed serious doubts that these measures would actually represent anything close to quality. Sociologists, on the contrary, were much more distanced from these practices and even quite strongly expressed their concern as to what such 'automatisms' would create as self-understandings. This difference also reflects the fact that the established structures of assessment, the implicit routines that come along with them, as well as the underlying value-systems, are often perceived as being based on experiences and models from the natural science. This was then perceived as inappropriate or even as counterproductive for the knowledge production culture in the social sciences. However, social scientists find themselves in a dilemma. Rejecting these quality indicators is often interpreted as a refusal to talk about quality and as a consequence social scientists – as could be witnessed in a recent event on quality measurements in the social sciences at Vienna University – also struggle for developing quantifiable measures that could better fit their practices.³⁰

Thirdly, in some cases excellence is – in particular when it comes along with excellence prizes awarded to individuals – partly understood as a possibility of at least temporarily buying out of the system on two levels. Firstly, the issue of being able to take risks is highlighted. One female winner of such an excellence prize made this quite explicit:

30 Workshop "Quality and Impact of the Social Sciences", 16 September 2008; University of Vienna; <http://www.qs.univie.ac.at/index.php?id=36814>. The announcement of the programme states that "University and non-university research institutions are increasingly responsible for assessing the 'quality' of academic achievements." For the social sciences, classical quantitative measures pose the challenge. "social sciences are not very representatively covered in databases like Web of Science or Scopus and they embrace a broad and rather heterogeneous collection of disciplines, where the 'national publication model' plays an important role." The declared aim was thus to think of how one could capture the output and quality in quantitative ways.

“(If I have this money) I do not need to reflect in advance if it will work out; I obviously can do more risky stuff and that’s what I did. We have started three projects, which I could not have done for reasons of competition. It would have taken too long, to write a proposal, get the money and find someone to do it. There was the idea and it had to be realised immediately otherwise someone else would have done it.”

[AT_Int_BS_f]

Risk is used in a double sense here: risk of investing time and funds and not obtaining interesting results; but also risk in the sense of losing the race against competitors in the same field. This nicely fits in with the discourse developed at the European level and the mission statement of the ERC quoted earlier.³¹ The idea of being able to do risky research is high on the agenda, as quite a number of researchers believe that the current research system and the accompanying quality assurance mechanisms mainly foster conservative research. They stress that competition and time pressure does by no means support radically innovative and unusual projects. Funding agencies rely on reviewers who often already demand some preliminary results when applying for funds to lower risk investment. Simultaneously both writing grant proposals and reviewing have become highly time-consuming activities, making researchers comply to the normative imaginations embedded in the funding structures. Overall, researchers underlined that the more competitive and insecure funding structures became, the more research projects would tend to be ‘conservative’. Financial and, to a certain degree, also career stabilities accompanied by fairly longterm timeframes for working on a research subject, were thus seen as a prerequisite to carry out innovative research in a sustained manner.

However, buying to a certain degree out of the system also happens on another level. Indeed behind these excellence initiatives is the idea that, while most segments of the research system should speed up their pace of producing innovations in order to live up to the knowledge economy expectations, smaller segments of particularly outstanding researchers/institutions should obtain the privilege of partially escaping from this ‘production machinery’. Thus, while the ideology of the knowledge economy paired with a full-fledged audit society should drive the average

31 At the time of writing this book, there were debates going on whether the first round of ERC proposals has indeed been as radically innovative as the discourse which accompanied the programme.

researcher, i.e. high publication rates, high third-party funding, doing the teaching and supervision work, etc., this outstanding group should be able to retreat from this logic, both intellectually and institutionally, at least for a while. But this privilege is not for all. As Nowotny [2006], Vice-President of the newly created European Research Council (ERC), expressed quite clearly: “(t)he pursuit of excellence needs an autonomous space, where curiosity is the driving force, pursued by individual creative minds. But this autonomous space is not there as a free-for-all. It needs to be built and nourished. It needs to be cultivated, and cultivation depends, among other, on competition and selection.” Thus competition and selection are imagined to be the basic mechanisms for being allowed to gain a place to work under better conditions. Yet this ‘unconditioned support’ is only a temporary feature and it remains to be seen how far this buying out discourse will work out in the long run.³²

This brings us to a key question in all these drawings of distinctions: “how is excellence recognisable?” Here an interesting contradiction arose in how excellence is defined or recognised. While the paradigm of quantifiable indicators and rankings was rather widespread in the formulation of research policy, the excellence discourse does not seem to fully subscribe to this logic. In fact, when selecting outstanding researchers for prizes or in other selection procedures of that kind, qualitative judgements of peers are perceived as the central element. “Excellence is something you recognise when you encounter it” [Nowotny 2005: 3], has probably become the most cited sentence in this context. While this assertion has been challenged in multiple ways, it is nevertheless remarkable that it is also more or less explicitly reproduced by members of the scientific community. A Slovak bioscientist even extended this statement to research and its output more generally, by stressing that “quality criteria are not definable precisely, but the quality of an article or a particular research is always recognisable” [SK_Int_BS_m] – and this is the occasion for scientific communities to enter into the quality assessment process playing the role of gate-keepers or warrantors of quality.

Having said all this, we need to reflect on its consequences for researchers. Indeed it is not only the materiality of such initiatives singling out excellence that demands scrutinising, but we need to question what this means for the others who are implicitly labelled as ‘not excellent’. Some researchers openly ridiculed the alleged excellence of the exceptional few, visible in their publications count and impact. They disputed the

32 It will indeed be interesting to observe how far auditing and regular reporting will intrude into the excellence programmes and thus minimise the effects.

radical innovativeness of the research performed, stressing that this type of research often was simply more of the same and that all that was needed was the capacity to 'sell one's research'. Others expressed regret that such a strong normative frame of singling out 'the best' would leave little space for alternative concepts of being a 'good researcher' and would not pose the question of knowledge sustainability. Diverse types of knowledge in a broad range of fields are needed in order to assure future viability to knowledge and innovation systems.

Many of those who see the emphasis on excellence as problematic and 'overboard' are female researchers. They criticise the quite reductive image of being a 'good researcher' leaving out more reproductive functions like teaching and supervising. Indeed female researchers often invoke other values such as cooperation, research exchanges, know-how transfer and freedom, which they all perceive as restricted by the current stress on one-dimensional performance. This could be taken as a perfect example of the gendering of occupations rather than merely of individuals. Since researchers are subject to the individual research assessment, the vocational values are seen as slipping away and excellence as embodied in the exceptional few.

Finally in different disciplinary contexts this discourse, the accompanying measures and the underlying values are felt to have different impacts. Indeed, from the interviews, it could be argued that the biosciences have largely incorporated the new value system, even though they did not seem to endorse it right away and sometimes express regret for past ways of valuing research. Reference to key journals, patenting structures, international positioning, competitiveness, however, have already become quite common in these fields. However, these developments are described as considerably hindering those fields that are not at the core, as well as those that would like to follow more risky paths. At the same time, the social sciences have less structured and clear ordering principles and thus offer more freedom to develop. Yet this comes at the price of not being valued so highly on the policy ranks, being regarded as often not sufficiently rigorous and 'academic', a reflection explicitly made for example in the Austrian context.

3.2. Applied vs. basic research: an uneasy relationship

Along and closely intertwined with the excellence and quality discourse, another boundary drawing and ordering activity seems essential to reflect: the distinctions and classifications researchers use, such as basic and applied research or how they speak about a broader relevance of the knowledge they generate. In which ways do these dimensions capture

the fundamental dimensions of their work? Why is this classification such an important point of reference? And what does it tell us about researchers' epistemic models and the accompanying value systems?

It seems essential to shed some light on this mode of ordering, as it is strongly reflected in much of contemporary policy debates. We find quite a strong rhetoric around the need to open research to society, most of the time meaning to industry and other economically relevant actors, as well as a call for cross-disciplinary cooperation and for keeping in mind potential applications. This goes in line with what Gibbons, Nowotny and others [1994, 2001] have labelled mode 2 knowledge production (see chapter 1, this volume). Much of this has led to institutional transformation, such as the creation of transfer institutions or legal offices in universities, which are supposed to assure the knowledge flow towards the economic sphere. Yet our interest is not directed at such concrete measures, but rather at grasping how this boundary matters for researchers' positioning. We will do so by looking at three issues: innovation models forming the basis of the basic/applied distinction, roles and identities accompanying this divide and, finally, issues of researchers' responsibility. Further issues touching on how relations between science and society are mirrored in this distinction will be touched upon in Section 5 of this chapter.

Indeed, in all the accounts we collected from researchers in the five countries, the basic/applied research distinction marked its appearance in more or less explicit ways. All of them hint at the fact that these distinctions and the value structures that come with them create mixed feelings when researchers try to position themselves. Indeed, a closer look opens up a rather nuanced picture of differences in age, discipline, even research field or national context. To start with, it seems important to reflect the innovation model implicit in most of the narratives and to point at a marked difference between the social and the biosciences. Bioscientists in their narratives seemed to buy very strongly into the linear innovation model [Godin 2006], which starts from basic research – perceived as the foundation or the basement of any further step – then moves on to application-oriented research, from there to development and ultimately to marketable products. This corresponds to much of the policy discourse which also bought into this logic. The linear model serves, we want to hypothesise, both to justify basic research as being an essential foundation without necessarily having to prove direct knowledge transfer, as well as to allow the drawing of a line as to what researchers in universities should do and what they could be held responsible for and measured against. More senior bioscientists in the Austrian context, for example, strongly positioned their work

on the basic end of the innovation chain and experienced the demand for application as interference in their work. On an argumentative level, they worked with a clear dichotomy of an 'old' model of research they associate with value-free, curiosity-driven research versus a 'new' model of accountability and responding to societal needs. This narrative captured both their feeling that there was a clear transition going on from the 'old' to the 'new' and that the latter could become a potential danger to freedom of research. A similar argumentation can be found in Slovak researchers' accounts. While agreeing on a criterion of usefulness of research for society at large, they opposed giving preference to applied research, which was seen as being attributed too much importance in contemporary Slovak research policy. Yet it is important to keep in mind that subscribing to such a model often made researchers develop diverse narratives of promise that their basic findings would lead to valuable applications later in the innovation chain. Playing with the future potential of basic knowledge has thus become an integral element in researchers' positioning work.

While part of the social scientists also bought into such a linear logic, quite a substantial part of them deployed a much more 'messy' model of the translation of knowledge. Indeed many of them talked about experiencing societal problems as their starting point for becoming social scientists and as triggering their research [see Felt, Sigl, Wöhrer 2008]. And even if they did not place themselves in any direct context of application, they frequently argued the profound entanglement of social science with society. This close relation to their objects of research was, however, simultaneously seen as a profound credibility problem for the social sciences, and researchers implicitly and partly explicitly addressed the need for purifying [Latour 1993] these entanglements between science and society. At the same time, we encountered explicit complaints that the applied research dimension of social sciences is not valued enough in the current research system. In fact, social scientists encounter many more problems than bioscientists with the machineries of audit cultures: in particular, applied social science knowledge proved notoriously difficult to account for in quantifiable units suitable for auditing. Thus the messiness of the innovation model at work had clear repercussions on social scientists' positioning work along the basic/applied boundary.

This opens up two further interesting differentiations along age groups and professional roles, closely linked to the notion of expertise. In the Austrian interview material, we find quite a strong indication of the fact that both junior sociologists as well as junior molecular biologists express a certain appreciation for applied research and the explicit consideration of societal implication. Similar traces can also be found in the interviews

with Slovak bioscientists. “It is the dream of every scientist to get results which can be somehow applied, which are helpful” [SK_FG_BS_m], as one young biochemist formulated the goal of scientific inquiry. Early stage researchers in the Czech Republic also made similar arguments about the importance of producing useful knowledge, even if useful would be deferred into the far distant future [Gorska et al. 2007: 20-21]. Two different hypotheses might explain these observations. Firstly, we could argue that the general discourse on the societal role of science and the need for good knowledge to be somehow applicable has borne fruit with the younger generation as they have already been socialised in this discourse and the accompanying value system. However, if we take our observations from the UK context and focus on the difference of expressing their relation to application between contract researchers and university employed staff in the social sciences and connect it to the Austrian accounts, we could also argue that the project orientation, together with the fact that the number of contractual researchers is rising, might also bring about the change in both the self-valuing systems of researchers and in the way the boundary between basic and applied research is imagined.

Along with this boundary between basic and applied research came reflections on the different *roles and identities of researchers*. In fact, bioscientists mainly saw themselves as researchers and did not perform any other role or identity which could be seen as linked to their dominant situatedness at the basic research end of the innovation chain (those thinking more in terms of application were often relatively young). Social scientists, on the contrary, performed a variety of potential roles and identities. Besides being a researcher, being an expert or a professional was an important way of expressing the distinction between basic and applied research in the social sciences. Thus the boundary is not only constructed in terms of knowledge, but also in terms of persons who hold the knowledge and their roles as essential transfer agents of this knowledge. Yet these roles are not conceptualised in a homogeneous way throughout the different contexts. The British material gives us quite interesting insights into the nuances of the concept of expertise and how researchers struggle with it [Garforth, Kerr 2007]. It is seen sometimes merely as a performance for external audiences, a “presentational device” or “myth” that needs to be “kept alive” for students (this comment elicited a lot of laughter) [UK_FG_SS_f]; or as a “role” in which one is positioned by others, for example by being “invited to speak” [UK_FG_SS_m]. Simultaneously this role created quite some uneasiness and was seen as problematic. This was expressed as a more general problem: “I may not feel like an expert” [UK_FG_

SS_m] or in a more personal way: “I think maybe I would personally feel it (discomfort with being an expert) perhaps, but I think that’s more to do with my personality” [UK_FG_SS_f]. Slovak sociologists expressed their understanding of their roles still differently, as providing expertise for real life problems, connecting their work on the applied side of the boundary with their identification with ‘professionalism’ as opposed to ‘pure ivory tower science’ (see also Section 5 in this chapter).

In the Finnish context, the role of the expert was treated much more straightforwardly as something flowing out of the institutional vocation. Being employed in a specialised governmental sector research institute, the Finnish social researchers we studied considered it to be part of their role to influence to emerging societal problems and needs and thought of closely affecting and even formulating social and health care policies and actions. “It is very interesting because you’re so close to decision making and policy formulation ... sometimes there may emerge problems of bias if you want to study the same policies scientifically” [FI_FG_SS_m]. From the focus group discussion, the closeness of researchers to the Ministry became transparent, through involvement in preparation of legislation concerning health and social care matters. Yet this does not mean that social science knowledge is directly ‘translated’. Indeed it often does not materialise necessarily in policies, but strongly frames the self-understanding of researchers sometimes creating tensions when research and policy move strongly apart. A female interviewee working in a governmental sector research institute even implied that she has “true freedom of expression” only when she participates in international conferences [Kovalainen, Poutanen 2007].

Finally, along with such a distinction between basic and applied research, there the question of responsibility for the research also arises. In our material we could say that this is an interesting “absent presence” in the sense of John Law [2004]. Indeed, while many researchers throughout the five national contexts talk about the role of their knowledge and the potential or even direct impact this might have, they never explicitly take up the issue of responsibility they could have for their knowledge. Quite the contrary, if responsibility is addressed at all in the context of science and society issues, it is done so by arguing “we do not carry responsibility for politicians’ decisions” [FI_FG_SS_m]. Deploying the linear model of innovation allowed them to circumvent the responsibility issue perfectly: on the one hand, basic researchers fulfil their responsibility towards society in breaking ground for innovations to come while, on the other hand, they could decline taking responsibility for any consequences of the knowledge they produced [Felt et al. 2009a]. Therefore, boundary drawing between basic and applied research and

positioning oneself on the ‘right’ side was crucial not to be obliged to engage in any more profound reflection concerning the linkage between societal impact and the accompanying responsibility.

4. Assembling and ordering academic institutions

Our study was carried out in academic institutions, the research design being based on the assumption that institutions are significant sites in science. Academic researchers are mostly associated to an academic institution even though not necessarily on a regular and stable basis. This is absolutely crucial for the biosciences where institutions provide material tools and environments – the lab. But it is essential for social scientists as well, because institutions are significant reference points in science in many respects: they mark the quality of educational degrees; they provide credibility to researchers and their publications; they are objects of assessment in national evaluation exercises.

Institutions are usually taken for granted: as enduring, stable entities. They may be moving in rankings yet remain internally stable unless they split or undergo major reorganisation. We want to problematise this picture in this section and argue that the identity of institutions is not as straightforward. They do not have stable and sharply defined boundaries. They do not have a clear-cut exterior and also their interior is in a constant process of more or less visible reordering. We can rather imagine them as a field of attachments of varying intensities between people, buildings, materialities or projects, from which an institution is (re)assembled in different circumstances and for different purposes. While there were stabilities and permanencies in the institutions under study – namely in terms of buildings, symbols, a limited number of permanent academic employees, some research topics and sources of funding – there were also many things, people, images, locations in flux. We want to argue that the mobility and partial attachments of researchers and projects are as important for the academic research institutions as the stable ones. The controlled flux and partiality are constitutive features and managerial mechanisms of the institutions. We want to expose the shortcomings of (implicit) policy considerations and imaginaries of the institutions that are most often taken as stable and unproblematic points of reference for policy actions and assessments, e.g. as in bibliometric research assessments when the performance of researchers is assigned to institutions in a straightforward manner, without taking into account researchers’ mobility between institutions, multi-institutional affiliations and different intensities in which they are attached to academic research institutions.

To capture the multiplicities of organising and enacting research in academic institutions, we make use of the four distinct “modes of ordering” – administration, enterprise, vocation and vision – which Law [1994] developed to capture patterns of ‘research realities’ in his ethnographic research of a UK physics laboratory that he undertook at the beginning of the 1990s. Administration is, according to him, a mode enacting hierarchical structures of offices, people, files and machines, managed through planning and control. It relies on the ideal of rational division of labour. Enterprise values and rewards performance, pragmatism, and opportunism. The agency in this case is driven by self-interest. Vision is very close to the Weberian rule of charisma; it values grace, talents and transgressions. The agency is elitist and “generated by the denial of the structure; or better, by a juxtaposition of structure and non-structure” [ibid.: 79]. Finally, vocation speaks of the social basis of researchers’ skills. It appreciates that skills are learnt not only by means of formal education or in textbooks, but through apprenticeship and practice. In this way it also tends to create insiders and outsiders, and informal social difference (for summary exposition of the modes see Law 1994: 75-82). Science is differently assembled, bounded and ordered in these modes; different features, relations, instances and measures become relevant.

Law discusses these modes of ordering in different dimensions; we limit ourselves here to the ways in which different modes enact internal ordering and boundary drawing of academic research institutions and on the repercussions these orderings have for researchers’ epistemic living space. We will then conclude by considering the effects of coexistence of the four modes of ordering. Let us now look at the institutions under study.

4.1. Administration: institutional cores and peripheries

Bureaucratic organisations have been characterised by a clear structure of positions with defined rights and responsibilities, and by a career ladder. Here we want to look at how academic institutions are enacted as bureaucratic organisations today. Formal positions as defined by the type and duration of contracts seem to have a key impact on the epistemic living space of researchers. Even though differences across countries exist, there are three main types of posts:

Type 1: an institutionally stable position (in terms of structural embeddedness and durability) with permanent contracts (typically these will be professorial posts in departments, and in the UK

departments, also lecturer and senior lecturer posts);

Type 2: an institutionally stable position with fixed-term contracts (typically assistant professorial posts in departments; fixed-term contracts are most likely to be renewed in this case);

Type 3: project-related temporary positions with fixed-term contracts (contract researchers, PhD students; the contracts will not be renewed and possibly the signing of a contract for another project with the same person is uncertain).

In all institutions under study, the number of Type 1 posts is rather limited and the conditions for obtaining them are tough, and getting tougher. At the same time, Type 3 positions which are rather recent, are becoming increasingly numerous and important for the knowledge production in research organisations. This may be a legitimate career phase in the doctoral and post-doctoral period but, once a researcher gets stuck in it, he/she is marked as a failure. Novel and important, they are the focus of our attention. In Austria, permanent positions under the new contract scheme are restricted mainly to professors. At the same time, chaining of fixed-term contracts is only allowed for six years at a university which means that junior people are in effect forced to leave the university after this period (to go abroad or to another institution for at least a year), so that the counting of contract-years can start anew after their eventual return. The percentage of Type 3 positions in the life sciences is significantly higher (43 %) than in the social sciences (6.5 %), and they involve a significantly higher ratio of women to men [Felt, Sigl, Wöhler 2006: 15]. In the Czech Republic, academic workers have an exemption from the labour code banning the chaining of fixed-term contracts, so most of them repeatedly have fixed-term contracts renewed (or not). Only a very limited number of professors and researchers have permanent positions, a great majority of which were occupied by men in the institutions under study. Also in Slovakia, no research participant had a permanent position while we were conducting initial interviews (life course questionnaires – LCQ). In the UK, 59% of academics in the country held permanent contracts by 2005-2006. These involved a majority of teaching (or teaching and research) positions at universities, while a great majority of fixed-term contracts were research only, and women academics were more likely than men to be employed on fixed-terms (48% of women as opposed to 38% of men on fixed-term contracts in 2002-03) [AUT 2004: 19 cited in Garforth, Kerr 2008].

Universities are obliged to offer a permanent contract to a researcher after four years of employment. This, however, does not necessarily happen due to 'objective justification'. Running out of project money is often a reason for not giving a permanent contract. In Finland, the proportion of employees with fixed-term contracts in universities was over 60% during the period 2003 – 2008 (compared to roughly 30% in the State administration as a whole). The number of people with fixed-term contracts has increased in recent years, which was explained by the Ministry of Finance by the fact that research done in universities is increasingly dependent on external funding. This external money is practically always project-money, and people are thus hired for relatively short terms. There is no general difference concerning the proportion of women and men with fixed-term contracts [see e.g. Kuusisto 2005].

The Type 3 posts seem to be much more precarious in social sciences. In the UK, for example, contract researchers at the KNOWING research sites were mainly concentrated in applied social science research units which were spatially, structurally and culturally separate from the core teaching and research department. This has the benefits of good teamwork and a very strong support structure, but is felt to perpetuate exclusion from mainstream academic activities and career structures [Garforth, Kerr 2008]. On the contrary, Type 3 positions are more organically integrated into bioscience research. They form a legitimate career step in the post-doctoral phase, and only become 'suspicious' if not transformed into more stable Type 2 or Type 1 posts in later career stages. Due to pressure on inter-institutional mobility, integration of individual researchers in the biosciences is, however, weakened by relatively short periods of employment in these Type 3 posts. The looseness of attachment is thus constituted in terms of its duration. In both cases, the biosciences and the social sciences, we thus can observe an increasing size of institutional 'peripheries' of research organisations.

The status of Type 3 positions also differs across countries. While in the UK, where these positions have been established for the longest time among the countries under study and have become an explicit object of policy attention [cf. Roberts 2002: 143-188], researchers in Type 3 positions have legal rights to the same terms and conditions of work as permanent employees, in the Czech Republic, Type 3 positions do not necessarily mean a full-fledged employee contract, but can have the form of a contract for an amount of work or working hours which does not cover social and health insurance and employee benefits (e.g. luncheon vouchers; borrowing IT equipment from an institution). In Austria, these positions may also be characterised, especially in the social sciences, as offering less attractive employment situations and career

benefits compared to Type 2 positions [Felt, Sigl, Wöhrer 2006: 15], as there is no clear institutional standard prescribed for these contracts (compared to Type 2 contracts) and they depend on the personal decisions of project leaders.³³ This difference in legal status of the positions produces other differences. In Austria, even if Type 2 posts are also for shorter periods, they will be valued higher from the perspective of an academic career than Type 3 posts. For example, a junior researcher in sociology who had both a teaching contract as “external lecturer” and a research contract on a project affirmed that, even though the former contract ends each semester and therefore has to be renewed every six months, she perceives her part-time teaching activity as more stable and contributing more to shaping her academic identity than her contracts for research projects, albeit they are for longer periods. As she had been teaching already for years and in a subject area to be regarded as part of the core curriculum, she pictures this work as more stable than research projects which “come and go” and sometimes simply run out before the next one starts [AT_Int_SS_f]. The teaching position constitutes academic status and an attachment to the academic institution, which project work apparently does less. On the other hand, in the UK, where fixed-term positions have set legal standards, teaching-*only* posts, on whatever form of contract, are generally of low status and are not seen as being on an academic career trajectory. Type 3 research-only roles are valued more highly than any other, except permanent teaching-and-research posts – provided they are seen and performed in relation to the linear career trajectory, i.e., as a phase on the way to a Type 1 post. Aiming at a particular version of durability, stable future perspectives as well as current working conditions, an institution assembles people and projects with different strength of attachment regarding their durability and working conditions, and with significant difference in formal obligations to them. The number of people and projects orbiting in outer spheres of the institution is substantial. This is not provisional but an ordering permanently renewed through the mobility of people and the continuous renewal of projects. This mode of assembling does not, however, suffice for the management of the institution. We will now look at other orderings and levels of boundary drawings.

4.2. Enterprise: competition outside and inside

As the name of the mode suggests, enterprise in research institutions emulates business and economic actors. Burton Clark, who coined

33 This situation has changed with the signature of a wage agreement in May 2009. Any new contract will have to comply to these new financial arrangements.

the expression “entrepreneurial university” [Clark 1998], stressed (the ability to) change as a key feature; we want to focus here on another dimension emerging as most relevant from our data, on valuing measurable performance and on competition. In the previous section we discussed how measuring mechanisms are enacted on the national and international levels in the name of excellence. The imaginary of ordering trickles down to the levels below – to institutions and research teams; measuring and ranking of institutions on a national level is translated into the ranking of individuals and research teams.

The ordering principle in this case is their measurable performance in terms of publication, technological outcomes and third-party funding. In some institutions (CZ, SK), the measured performance of individuals translates into immediate or annual financial bonuses for those with higher records and, in the Czech research institution in the biosciences, it is also supposed to be the main basis for renewing or dissolving research teams. In Austria, the measured performance impacts on the internal distribution of resources between teams inside institutions. The measured performance thus contributes to constituting continuity and enhancing material wellbeing (in one case on the personal in the other on the research team’s level) and potentially further strengthens the link of an individual to the institution. In the UK, where research output does not directly influence salary, winning competitive external grant funds plays the same role in terms of strengthening the lab leader/principal investigator’s link to the institution, as it not only enables the building and continuity of research teams (in the biosciences) and contract research units (in the social sciences), but is also by definition precarious in relation to renewing and dissolving research teams where employment is contingent upon external grant funding.

In any case, the national research assessment translates into internal orderings and boundaries within each institution. We will now discuss the three most relevant issues: 1) increasing internal competition; 2) repercussions of rising the pressure to publish for the quality of epistemic work; and 3) emerging hierarchies of research programmes and orientations within institutions.

Firstly, teams and individual researchers are not measured by an agreed absolute standard, but against each other. Some of the researchers reported changes in the working atmosphere in directions of more competition and barriers in communication between teams and individuals. “I think that we overstepped a certain boundary to the detriment of collegiality and ability to cooperate. One weighs up with whom to collaborate in order not to fail in the competition” [CZ_Int_BS_f], complains a Czech female bioscience researcher, for example, (interestingly, it was

women who mostly commented on this changing atmosphere and were unhappy about it). Similarly, in Finland, there were many complaints about the ‘brutality’ and intense competitiveness of the academic world both by bio- and social scientists, female as well as male [Ahlbeck-Rehn 2007b]. Even in cases where scientists stressed that they had a good and even “cosy” atmosphere in the lab, they did not take this for granted, appreciated it explicitly and compared it to the situation in other labs that built much more on internal competition [e.g. AT_Int_BS_f]. A UK social science researcher, describing herself as not competitive, portrays the research environment in terms of academic horror: “(i) f you had to stab them in the back to get the next research contract I think most people would ...” [UK_Int_SS_f]. Competition seems omnipresent – at least potentially. The only exception of explicitly acknowledged relaxed conditions was in Slovak social sciences: several participants expressed that, although the university policy requires more accountability than previously, they don’t feel any pressure to perform better. “Everyone in the department has his/her own room ... there is no need for pressure (to perform better), everyone works on issues of his or her own interest, and by chance all of that is in a relatively good balance” [SK_FG_SS_m]. Even this researcher, however, says that this is “by chance”, and thus does not see it as a general condition of the system.

As the US is often presented in Europe as an ideal of scientific productivity for ‘us’ to catch up with, it is interesting to look in this context at the way in which the academic environment is described in the account of an American scientist. Rabinow provides an account of super-competitive academic environments in US universities and their repercussions for collaboration. One of his informants tells him: “(i)t is very difficult, as a scientist, to do interactionist, collaborative science. The acculturation process is one that is keyed to individual, personal achievement. You first learn that as a graduate student. To get into the best lab as a first- or second-year graduate student, you’d better excel on the individual achievement scale to get through your orals and qualifying exam. ... Thus one finds himself at age of forty being promoted to associate professor with tenure and twenty years of experience of how *not* to collaborate” [Rabinow 1997: 44-45; original emphasis]. It is remarkable that the quoted researcher compares his experience of the situation in a university and in a private research company and stresses that the competition is *higher* and more harmful at universities. We thus might reason that catching up with other contexts – universities emulating the private sector or the ‘East’ emulating the ‘West’ – might not lead to the desired results, but potentially to the contrary.

According to Shore, these changes in the ordering of academic institutions do not only concern working environments, but impact on researchers' subjectivities and constitute a new form of exerting power. "A key characteristic of neoliberal governance is that it relies on more indirect forms of intervention and control. In particular, it seeks to act *on* and *through* the agency, interests, desires and motivations of individuals, encouraging them to see themselves as *active* subjects responsible for improving their own conduct. By internalising the external norms of management, 'flexibilized workers' transform themselves into governable subjects of managerial power and control" [Shore 2008: 284; original emphasis]. In this mode, competitive self-managing individuals constitute the texture of a research institution.

Secondly, the competitive entrepreneurial logic and pressure on scientists to perform in internal assessments seem, however, to have a repercussion not only for the social fabric of research collaborations, but also for the very epistemic practices in the lab and on the publishing process. We gather comments on this specifically from junior bioscientists: "(b)ecause quality maybe suffers when people have the feeling that they have to publish, publish quickly; then I think lousy things happen. Then research is not done properly anymore ..." [AT_Int_BS_f]; "(the pressure on measurable performance) leads to publishing partial, not quality, unfinished results. Experiments don't get repeated as there is no time ... you can't rely on publications in these conditions" [CZ_Int_BS_f].

Thirdly, different disciplines, research topics and methods differ in their potential of scoring in measurable indicators (namely impact factor publications for 'basic' production and patents for 'applied' production); even though the national evaluation systems take into account, to a lesser or greater extent, differences between the sciences (natural, medical and technical), social sciences and humanities, often the model of the sciences is applied to social science and humanities in only a slightly modified form. Also, more subtle differences within the field of sciences, social sciences and humanities are not taken into account.³⁴ Epistemic differences present in the research institutions

34 The only exception seems to be the UK. Up until the most recent Research Assessment Exercise (2008) there has been a strong tradition of (discipline-specific) *peer review* in the RAE alongside citation, impact and other metric indicators. Peer review is argued to take into account different disciplinary production models – although see Harley and Lee [1997; cited in Lucas 2007] for an argument that in fact the RAE has co-opted the peer review process for institutional and instrumental ends. The UK RAE is currently under review and proposals endorse the introduction of a new system, the Research Excellence Framework, involving ongoing assessment based solely on 'pure' statistical indicators. These debates have very explicitly raised the issue once again of the relationship of the social sciences and humanities to research assessment.

(as e.g. computational and lab chemistry; qualitative and quantitative social research) are translated into hierarchical assessments of one-dimensional quality (of teams and individuals) and relevant institutional orderings. This creates tensions in the institutions as some researchers feel unjustly handicapped because of the nature of the research they are engaged in, for example, qualitative social science research developed in minor languages (CZ, FI), which is less often carried out within the frame of international comparative consortia, is more dependent on sophisticated language and is disadvantaged in relation to quantitative research (more easily publishable in international, i.e. English-language, impact factor journals). Lab-based chemistry, the experimental cycles of which take more time and have a high ratio of unsuccessful experiments, is disadvantaged in relation to computational chemistry, as both the lab and computational scientists admit. More interestingly, while the national assessments count a score constituted by the relevant research output divided by invested public money, the institutional evaluations only work with research output (while the price of producing it is not taken into account – even though it can substantially differ between research fields).

Science as enterprise epitomised in the research assessment thus creates institutional orderings and boundaries with possibly significant unintended epistemic consequences: firstly regarding the propensity of collaboration between individuals and teams, and secondly, for different strands and types of the research. This, however, remains without reflection at the policy level – a major failure given the ambition for creating a ‘knowledge-based economy’.

4.3. Vision: making visible and invisible individuals

The mode of vision is embodied in inspiring, visible individuals and expressed in talk about talents, gifts and originality. It assembles the institution around exclusive individuals who represent or symbolise it in the research field, in the public space (as public intellectuals or excellent scientists), as well as in relation to other institutions and disciplinary fields. These individuals can be embedded or cut across administration hierarchies. They also may, but do not need to, resonate with the mode of enterprise; they need not, especially in the social sciences where the measurable annual research output does not necessarily map on to actual influence in the field and the public space. The institution is shaped into the form of a comet in this case: the visionary at the front draws the institution forward and makes it visible – but at the expense of other possible directions and other people participating in knowledge making.

To some extent, s/he always includes and summarises collective efforts.

We found the visionary mode in two main forms. We could call the first one vision as a memory practice. Here the reassembling of institutions is enacted through naming them after famous scientists, recounting stories of their foundation, potential and growth, and through the figures of founding fathers (placed e.g. on the web pages). We found this kind of reference especially in social science institutions: their web pages often took the form of a historical introduction, building relation to a 'school' epitomised by concrete persons. But it is noteworthy that two of the social science institutions changed their web pages in the course of our research project in favour of a more 'contemporary' presentation. The other form of vision makes living scientists visible. It was present in some of the researchers' narratives in the form of an inspiring teacher or a lab leader. What is, however, especially interesting is how this visionary mode is taken up by public relation strategies of research institutions and policies. One of the Austrian researchers in biosciences highlighted this point nicely:

“Now you have these glossy brochures, where researchers are depicted. They are represented in completely different ways, they are partly pop-stars, who have a completely different character, a different nature, than the role models I had when I was young. Thus assessments and expectations what research can be change completely, as we link it inevitably to these persons ... Thus change happens.”

[AT_Int_BS_m]

Most often these visionary figures are men. As soon as women find themselves in such positions, they are dominantly expected to play a gender role model, e.g. by revealing their personal issues (family management etc.) along with their professional performance. We can compare, as an example, media appearances of two leading Austrian bioscientists [Wöhler, Sigl 2007: 6-7]. The male scientist, the director of a major research institution, is solely by counting the most prominent figure (743 entries between January 2000 and October 2006 in the Austrian media database), being portrayed as a kind of Austrian 'hero' – even as a possible candidate for a Nobel Prize. He is often asked about science policy and institutional issues of contemporary research, while his research and scientific achievements are by no means omitted. Another very prominent person in this field (240 entries) is a female

professor in molecular biology at another major research institution. She has won the most prestigious excellence prize in Austria (Wittgenstein Prize) and several other science prizes, was “Scientist of the Year”, is a Member of the bio-ethic commission of the Prime Minister, Member of the Austrian Academy of Sciences, etc. She is also active for equal opportunities of women and men in science and is interviewed in this regard and writes articles about this issue.³⁵ Yet, in contrast to the male scientist, her concrete research is only rarely explained. Articles about her are mostly about her prizes, duties and (political) engagements. So what is well known about her in broader public spheres is especially her engagement in regard to the promotion of women in the sciences, but much less her research. Even though gender is a relatively rare issue in media articles on science, it forms a very important implicit part of her media appearance. This way she gets somewhat reduced to being ‘the exception’, being the ‘the outstanding /exceptional female scientist’. These portrayals of a male scientist and his research, without broaching the issue of his gender on the one hand, and the strong focus on her broader engagement and gender of the female scientist on the other, reproduce the narrative of a normally male science which allows for a few exceptional women only. The UK team have explored similar dynamics in relation to national “women in science” policies, whereby foregrounding women in the ‘visionary’ mode makes their femininity hyper-visible and insists on revealing their domestic and parental selves, while at the same time reproducing invisible-masculine norms of the gendered academic institution [Garforth, Kerr 2007].

4.4. Vocation: the sociality of academic existence

John Law conceptualises the mode of vocation in terms of the social basis of expertise and skills, of tacit knowledge and also of the relation to work. The social basis, we observe, has its formalised as well as informal dimensions in the institution. The formal part takes the form of teamwork, seminars and institutionalised apprenticeship. It seems to be more intense and materialised in the biosciences than in the social sciences (see Chapter III, Section: Working together apart of this volume). This is definitely linked to the role which place plays in these two epistemic cultures. The biosciences are closely intertwined with the lab as place [Gieryn 2000] where togetherness and vocational orders can be performed. Sharing a common place means making vocational orderings palpable to each other. On the other hand, social scientists somehow seem to be a “place-less tribe” [Felt, Sigl, Wöhrer 2008]

35 See ORF science at <http://science.orf.at/science/schroeder>.

and the vocational ordering has to be played out differently. Indeed the vocational mode remains largely invisible in the everyday context of social scientists and thus needs to be made explicit. This might explain the efforts described in particular by younger and less clearly institutionally attached researchers to make themselves visible within the formal research establishment. The informal part of 'vocation' consists of social ties – among researchers but also between researchers and their institutions – of belonging or not to insiders' circles or personal apprenticeship.

The vocational mode of ordering became visible in our material – at least on two levels which we want to discuss in an exemplary way. The first ordering can be traced in comments on the relation between the social basis of knowledge and the administrative structure of posts. Here accounts could cover a rather broad range. On one side of the spectrum, social ties were seen as capable of turning the scales. Thus the tender procedure for lab leaders in the Czech bioscience institution, to take but one example, was denounced by some of the unsuccessful candidates as being biased towards personal networks and preferences of the director of the institution. On the other side, researchers with formally more peripheral administrative positions invested vocational work as a matter of compensation. In that sense we could understand Finnish social science contract researchers who strived to be present and highly visible in departmental meetings to compensate for their weak attachment to the institution in administrative terms [Ahlbeck-Rehn 2007b]. Yet, boundaries created through social ties can be crossed with very different levels of difficulty. In some cases, it can simply depend on starting to participate in the activities in question, being present; in other cases, there may be a strong resistance from insiders to integrate newcomers. Time, as well, seems to play quite an essential role in the building and making implicit of such social orders. In the Czech social science institution, a difference in position and influence between insiders (people who have been attached to the faculty for a long time as teachers or students) and outsiders (who joined the institutions without previous attachments) was quite significant. Many issues seem to be discussed and even decided in informal settings (informalised, unwritten). Importantly, the outsiders or newcomers mostly do not live in the city and therefore commute. The lack of a shared history is thus further reinforced by the low extent of physical presence at the department.

The second way in which the vocational mode of ordering became visible in our study was in researchers' stories about their personal engagement with research. This embraces narratives on the readiness to work for much longer hours than stated in the contract, to organise

time schedules along the need of the ‘lives’ of the research objects, simply to be there when the ‘research demands it’. Expressions such as: ‘research is not a nine-to-five job’, addressing the problematic non-commitment of certain colleagues or interspersing the conversation with mentioning long daily working hours, no week-ends etc. capture the grand narrative of the vocational relationship needed to be a successful researcher. As Law [1994: 119-120] notes, the modes of vocation, vision and enterprise demand workaholism, long hours and indifference to time – in the name of performance, vision or vocation. However what is specific about vocation, we would argue, is that it achieves the long hours discipline through merging work and the personal to some extent. “Private life gets intertwined with the group and the group with private life, they all do a lot of things together privately ... as those who come from abroad have no social networks. ... That means, it is a kind of family” [AT_Int_BS_f]. And if there still is any personal life and passion beyond the lab or library, it turns out to be problematic, as causing major tensions “between being father and husband and having a family on the one hand, and being exposed to the continuous expectations of the university, which derived from my role as assistant professor, on the other hand” [AT_Int_SS_m].³⁶

The two vocational modes of ordering now become relevant to our research interest in at least two ways. First, we have witnessed the importance of being part of a place structured by face-to-face, informal and socially embedded communication. While this is not really novel and has been stressed in much of the STS literature, it needs careful reconsideration when dominant policy discourse and its institutional manifestations gravitate around the rather delocalised concept of the ‘research area’ coupled with a strong ideology of ‘international mobility’. In particular, we might ask what this means in the long run for the flexible, mobile and younger researchers and how they experience vocational orderings in a particular place and the attachments they create.

Secondly, the enactment of informal social networks seems to have a gendered aspect. For example, almost all Austrian female researchers who have children – life scientists and social scientists alike – mentioned their children as reasons not only why they cannot spend long working hours at the lab/department, but also why they often cannot participate in informal social activities. Children and other social obligations associated historically with women can thus hinder their capacities for

36 These negotiations between the work and personal life are discussed in detail in the Chapter IV on Times and Trajectories in Academic Knowledge Production, Section 3.3, this volume.

developing the social basis of their academic existence and above all create the imagination of women not following the implicit ideal of the vocationally fully dedicated researcher.

4.5. Amoebic institutions

The four modes enact distinct boundaries and orderings of research institutions. Administration makes up a hierarchy of posts with differing intensities of attachment to an institution; enterprise, a measurable performance-based ranking; vocation, a set of more or less privileged physical presences and informal networks connected to personal histories and ties; and vision, a comet-like configuration characterised by an exclusive visibility of individuals and mirror images or invisibilities of all the others. These different versions of an institution co-exist but sometimes also clash; one can also be strategically mobilised at the expense of another. One's positioning and 'living' conditions in the institution are co-determined by the four orderings.

Given this complexity and fluid nature of institutional orderings and boundaries, the unproblematic reference to academic institutions is striking in the policy documents and procedures that imagine them rather as stable and durable entities – not only in legal terms but also in terms of people. This, however, does not hold. Let us discuss in this respect one of the most relevant policy operations towards academic institutions, that is research assessment. In this exercise, the institution is taken as a large organism of equals who contribute to its overall performance. As we have seen, this move is in tension with most of the orderings of the institution in the four modes. Firstly, we have seen that in the administrative mode, the institution is constituted by attachments of very different intensities (in terms of formal guarantees, working conditions etc.); but all the orbiting people and projects are neatly reclaimed by the institution and their research outcomes assigned to it as a credit. The only exception in this case is the UK where most fixed-term research-only staff are *not* eligible to be entered as “research active“ staff in the research assessment exercise (RAE). In this case, on the other hand, productive researchers are denied the possibility of contributing to the counted performance of their institution and thus making themselves visible career-wise [for argumentation, see e.g. Madden 2008]. While, in the first case, non-permanent/contract researchers are made invisible by being automatically fully reclaimed by an institution, in the second case they are made invisible by not being claimed at all. Their partial status is not recognised – as a specific institutional position or as a possible career option.

In this sense, DiGiacomo argues specifically for social sciences that

“many fully qualified professionals with competitive CVs are forced to spend their entire professional lives in this twilight zone where they are never even offered the opportunity to go through the tenure review process. However, as a condition for the possibility of continued employment, they are required by those who hire them to demonstrate a consistently high level of scholarly productivity, as are their securely employed colleagues, whose own scholarly activity is regularly monitored and reviewed, and who have, evidently, internalised fully the need for such surveillance even on a playing field which is manifestly not level. Of course, without the stability of uninterrupted access to university libraries, a living wage, and space in which to order one’s books, files and thoughts, that kind of intellectual production is immeasurably harder to achieve ... unless you are the perfect flexible producer, a person who thrives on that kind of instability and insecurity.”

[DiGiacomo 2003: 5]

What is also overshadowed by this move is the fact that researchers especially in the social sciences are often attached – for subsistence or other reasons – to several institutions, which can be academic, non-academic but research-related (research agencies, consultancies) or other (journalism, NGOs). The research-related or expert ‘outside’ positions were reflected by some researchers as enriching their academic work and professional identity. However, the academic institution takes sole credit for research performance without recognising the contribution of the partial connections to other (non-academic) institutions (this also has serious consequences for the science-society relation which we discuss in the next chapter). Secondly, in the enterprise mode, the external bibliometric assessment creates internal orderings out of epistemic differences, translating them into a one-dimensional quality scale. The recognition of epistemic diversity within research institutions may end up unintentionally prioritising some research strands and handicapping others. Thirdly, even quantitative bibliometric measurements have certain degrees of freedom and we have observed (in the Czech social science institution) that they are

differently translated into institutional recognition and internal politics. These translations can closely depend upon the formal and informal position of a researcher in an institution. For example, while a “remake” of a book by a senior professor was presented as a clever operation towards the national evaluation system, the collective nine-person authorship of a book published abroad, whose co-author was a junior female researcher from the department, was presented as “suspicious”, if not immoral. While the research assessment is meant to be a tool of steering research, it can hardly avoid being used by institutional actors to achieve local ends.³⁷ Rather than destabilising the institution, the amoebic institutional boundaries and continuous reorderings are a mode of living of institutions. It would be naïve to try firmly fixing them, but it is equally naïve, and can be to the detriment of individual researchers as well as political agendas, to overlook their amoebic nature and not to take this fact into account.

5. Purifying and merging the scientific, the social and the personal

Science in modern societies has been constructed as ‘the other’ in the realms of the social and political; modern thought defines it by its delimitation from the sphere of society. Many science studies scholars (e.g. Latour [1987; 1988], Haraway [1997]) have been challenging this view since the 1970s, arguing and showing how scientific practices are implicated in social and political practices. As Latour argued, modern science needs both for its proliferation: practices of purification which keep and represent science and society as separate and practices of hybridisation which entangle them; it is only the unguarded space secured by purification that allows for hybridisation on a massive scale. And he would continue arguing that we, as a polity, need to find ways of bringing to light and representing the entanglements so that they can be regulated and controlled [Latour 1993]. In recent years, the relation between science and society and their interconnectedness also became (with contributions by science studies scholars) a political issue on the European and some of the member states’ agendas. Among the countries in the project, attention is most significant in the UK, and most feeble and dominantly derived from the EU requirements in Austria, the Czech Republic and Slovakia (in these three countries, the relation between science and society is strongly tied to the progress of science discourse). It is increasingly acknowledged that the entanglements of science and society need to be articulated as public issues and represented in political terms.

37 For elaboration on the power potential of switches between the modes of ordering see [Stöckelová 2009].

In this sub-chapter we will focus on the ways in which researchers relate to this new political agenda and how they interconnect and delimit the scientific and the social – in the form of society and political agenda – and also in the form of personal experience, interests and politics.

5.1. Engaging with society?

We begin with a short reminder of the ways in which the science and society relationship is dealt with in European policies. Firstly, these entanglements are addressed and highlighted in policy documents in terms of the societal (mainly economic) usefulness of science, and the social and ethical concerns of the public over techno-scientific developments in areas like biotechnology and medical technologies. The science-society relation is supposed to be asymmetric: the flow is epistemic in the direction from science to society (knowledge and technology flow to be consumed) and it is social in the direction from society to science (social needs and concerns to be accommodated). Secondly, and relatedly, society is mostly seen as lagging behind science with which it needs to catch up. The change in techno-science is inherent and self-driven; society, on the contrary, needs to change in reaction to it [Levidow, Marris 2001; see also Felt, Wynne 2007: 53–61]. The approach sketched above resonates in some points with the ways in which researchers perceive their relationship to society. There are three main points to be highlighted about researchers' approaches to the science and society relationship.

Firstly, as we argue in Section 3 of this chapter, even though no one would directly reject the principle of public accountability of science, researchers in the biosciences mostly express concerns about protecting a space to do basic research in an environment of increasing demands for responsiveness to societal needs and orders; for social researchers, on the other hand, the differentiation between basic and applied research is not that relevant. It has to be stressed, however, that most of the bioscientists express the need for a strong vision of societal progress in specific areas to which they are supposedly contributing their bit. It is this vision that helps them to overcome frustrations about everyday laboratory work which often does not yield tangible outcomes or simply fails. According to the Czech team findings, this is described by women as especially painful; other teams, however, did not find similar gender differences. However, this vision of contribution to a societal good does not equal application in the narrow sense of producing immediately usable results and producing them 'on order' of society. At the same time, the vision is also quite easily compatible with experiencing science

as driven by personal curiosity and competition within the disciplinary community. The public good should be (potentially) present in the game of legitimising the efforts (not only before sponsors and different publics, but also before researchers themselves), but distant enough not to intervene in the epistemic process. This stance – and its hegemony (note the shared laughter in the quotation below) – is perfectly illustrated by a focus group exchange between lab leaders in the Czech bioscience institution.

LLF1: ... we have to try, at least, to explain the research and the goals simply ... what we are actually doing. When we prepare the report for the Academy of Sciences every year and we select the so-called most important results, we have one form for the Institute which is more, let's say scientific, and then another form for the public. Just to be written clearly so that other people can understand it. Yes, so I think it is ... I know that for us it's work but, on the other hand, I think it's important to do it because at least just now, as far as I know, there are not so many scientists on these committees (governmental committees steering research) and at the Ministry or in the Government and therefore it's important to do such things.

LLM1: Yeah, I fully agree, but this is what she said, that we should go out and communicate, whereas the other thing would be that they come in ... and tell us ...

LLF2: Yeah, whether we should listen to people and do what they want us to do ...

LLF1: ... or would ...

LLM1:listening is OK ... (laughter)

(Laughter all round)

Interviewer: So, it would be your opinion that more information would dispel fears that the public may have about aspects of research that they consider dangerous, for example, again I mean genetically modified organisms and also other environmental issues, you know now, for example, with the REACH programme of the European Commission to control and register all chemicals, so that just simply giving more information is the answer.

LLF1: Yeah ...

LLF2: Well, that's valid. I think, in general, why are people against Temelín (a nuclear power plant in Southern Bohemia) or the American radar (a planned US missile radar to be placed in the Czech Republic)? Because they don't know anything about it. So more information is always useful.

This quote illustrates in a dense way how the boundary between science and society is maintained and reinforced through the very process of communication with society. Firstly, scientists speak differently to different audiences and, in doing so, mark as well as reinforce the boundary between those inside and outside science. Secondly, communication is meant to be unidirectional, thus again marking and performing the asymmetry: scientists communicate to society and want to be taken seriously but they “listen” to society with laughter and will not take into account what they have “listened” to. We also encountered such a positioning strategy in British interviews and observations. Despite the transformation to ‘outreach’ and ‘public understanding of science’, the framing of research by biologists was explicitly meant to remain ‘internalist’. The UK bioscience staff focus group articulated a strong sense of their work being primarily for their (disciplinary) peers and thus driven by ‘internal interests’. Wider publics and communities, the idea of science as part of the wider ‘culture’, and the notion of accountability to sponsors were all mentioned, but seemed to be annexed to a primary rhetoric of ‘intellectual curiosity’ [Garforth, Kerr 2007]. Thirdly, researchers perform a clear deficit model of science communication [Wynne 1995]: ‘people’ are characterised as lacking information – while science is implicitly assumed to possess (certain) information ready to be diffused, which constitutes the ultimate epistemic asymmetry. This can also be traced at other moments, such as when Finnish researchers reflect on their image of society including politicians, stressing that the latter “are just like common people – they do not understand anything about science” [FI_FG_BS_f]. Both in the accounts of social scientists as well as of bioscientists, epistemological inferiority is referred to as a clear distinction criterion between members of science and society. Thus one could argue that communication is paradoxically undertaken for both reasons, to inscribe science into society and to create a feeling of public commitment to science and, at the same time, in order to reinforce science’s difference and distance from society [Felt 2003b]. In that sense, we observe a similar mechanism at work as we observed for

excellence and its others. By ascribing certain features and capacities to one part, the remaining 'other' will be characterised through a deficit.

For social research, the conception of 'societal usefulness' was more varied. Some of the social scientists interviewed (mainly CZ, UK) took a very similar approach as the bioscientists and defended 'basic', academic research as the most important public good in the long run, contrasting it with straightforward, 'applicable' expertise to those in power. UK social scientists partly very explicitly expressed resistance to user-led research as "stifling" and pleaded for a strong defence of the more "profound" contribution to knowledge that comes from orienting research towards "a more academic, intellectually demanding audience" [UK_FG_SS_f]. This response was developed by an applied social science lecturer, who valued 'academic' research in terms of how it may ultimately be used by practitioners, even – especially – if not produced explicitly *for* them. Yet, by contrast, the contract researchers mobilised accounts of the value of their research much more, in terms of its practical relevance for 'service users'. These researchers seemed to see themselves as invested primarily in a topical area of research in relation to commitments to vulnerable and excluded social groups who might benefit from it [Garforth, Kerr 2007].

On the other hand, Austrian social scientists often stressed their interest in applied research and in contributing to societal changes. Working with administrative bodies, international organisations, etc. and feeding back research to those who are in control of or affected by certain political or social measures seemed to be a desired and fulfilling task. They expressed regret that this is not recognised by research assessments as relevant output. Most of the Slovak sociologists see their mission in providing expertise in real life problems (the users being mainly public administration and private agencies, but also cultural institutes, citizens' associations, NGOs). They strengthen this orientation by underlining their identification with 'professionalism' as opposed to 'pure ivory tower science'; professionalism is associated with the capacity to apply (scientific) methods and approaches to societal problems. And also some of the UK applied social science researchers stressed the value of practical relevance of research for 'service users' – meaning, however, not those in power, but rather the vulnerable and excluded social groups [UK_FG_SS].

To capture researchers' attitudes to the science and society linkage, it is useful to refer to three models of relation and influence: the enlightenment model that works with the idea of long-term and often indirect cultivation of society by science; the engineering model that is oriented towards direct applicability of scientific outcomes in society; and the reflexive model that takes into account two-directional epistemic influences between science and society as knowledge travels between

different social spheres.³⁸ While researchers rely on the enlightenment and engineering models when imagining the relation of their knowledge to society, the reflexive model is rarely invoked; the exception seems to be some juniors in the social sciences (especially CZ junior researchers and UK applied social science contract researchers) and most Austrian sociologists. Even in sociology, ‘lay’ peoples’ knowledge is debunked by researchers as feigned and endangering the status of social science knowledge. Even though in Slovakia social researchers did not unequivocally identify with “being scientists“, they nevertheless asserted a “professional“ or “expert“ identity [SK_FG_SS]: the epistemological divide was guarded in this case between ‘the professional’ and ‘the lay’. Researchers are also rarely explicit about their personal experience and politics as elements of the knowledge production process. Even if researchers were addressed directly and in principle willing to talk about it, they seem to perceive it only as a potential threat to the research work, but not as a resource.

5.2. Keeping society at a safe distance

Even though the reflexive model is mostly rejected by researchers, entanglements on practical levels do exist – especially among social scientists. Beyond their position in academia, they often simultaneously have attachments to other institutions – NGOs (gender and feminist in the case of gender researchers; social and cultural, in the case of migration and minority researchers), think tanks, counselling bodies, the media. Some of them described these attachments as closely integrated with their academic activities. However, in more formal contexts (publications, public presentations), researchers are quite careful not to mix up their academic and other activities. It seems that admitting or claiming the connection and mutual influence is perceived as potentially endangering the academic/epistemic status of researchers as well as of the knowledge produced. The attachments might now, however, paradoxically be weakening in some countries with the rising emphasis on applied research and its prioritising in science policies and research evaluations. As the ‘applicable’ or usable outcomes of social sciences are not technological in nature or patentable, they are not at all

38 Engineering and enlightenment models are used by Mesny [1998] for discussing the two major approaches for the use of social science knowledge in society, to which she develops an alternative, emphasising appropriation of social science knowledge by lay actors in society and its reflexive move to social sciences. We label this alternative, ‘reflexive model’, and use the models for understanding of the approaches to both social science and natural knowledge. For the engineering and enlightenment models see also Weiss [1979].

valued in assessment systems of these countries (AT, CZ). Consequently the only viable strategy for the disciplines and individual researchers is to focus on valued academic outcomes (impact factor articles, books) and to limit their activities towards society and the public sphere (which can be accounted for in administrative terms). We can see that, paradoxically, what is asked from the natural sciences in the name of their increased societal accountability – the applicability of their results – is not at all recognised on the policy level as valorising research in the social sciences.

The strongest boundary between science and society is drawn by researchers in epistemic terms. While they mostly accommodated the notions of public accountability of science in terms of contributing to a societal good (some of them even declared this as the intrinsic motivation of scientists), society should be kept at a safe distance from science with regard to the knowledge production itself. For most of the researchers, society should not interfere with the inner dynamic of science regarding the choice of topics and priorities (the space for ‘basic research’), and the epistemic privilege of science should be guarded towards ‘lay’ actors and researchers’ subjective investments. Epistemic entanglements of researchers with other social actors and with their personal experiences which breach and irritate the modern epistemological contract, tend to remain unreflected or externalised from the realm of science itself and highlighted as a failure of a researcher by others. This stance is more firmly enacted by bioscientists, but social researchers generally do not take a significantly different stance. Even if social researchers are aware of the social embeddedness of research and themselves as knowledge producers, they tend to belittle and put aside its epistemic consequences and implications.

This is not that surprising, given the fact that modern science has been constituted and legitimised as an endeavour striving to separate the social, subjective and idiosyncratic from the ‘objective truth’. These efforts have been embodied since the 17th century when modern science was constituted in the figure of a socially transparent, self-invisible gentleman who was supposed to participate in truth-telling [Shapin, Schaffer 1985]. This move has been strongly gendered and gendering for both women and men, and for science. Not accidentally, at the same time when the social was normatively excluded from science, women were also excluded as possible disruptors of the processes of establishing truth [Haraway 1997: 26-39; Nobel 1992]. Simultaneously a new mode of masculinity, the modest and disinterested scientific masculinity, was incorporated in the role of witness to the truth. This needed a new understanding of modesty, which was no longer a female attribute and would thus not stand in fundamental contradiction to the ideals of

masculinity. The shift in the meaning of modesty created the need to reorder gender relations in science and resulted in women being strongly associated with epistemic unreliability (dependency, spontaneity, emotionality).³⁹ Thus science was at once attached to objectivist epistemology and to men. Consequently, women later became one of the primary targets of science communication *as members of society*, with an important reproductive and caring function; even in the early 20th century, the education of women was still quite explicitly understood in many places as mainly having a societal support function [for the Austrian context, see Felt 2000].

The epistemological dissociation of science from the social, personal and subjective, which was observable in the institutions under study, resonates with this foundation of modern science. Women have been gradually let into science, yet the gendered objectivist foundations of science are questioned and opened up only on the margins of science – in disciplines and research orientations that, on the contrary, often have their scientific status questioned (e.g. feminist studies, cultural studies) and which are not seen as “opening institutional doors” [AT_Int_SS_f]. Simultaneously, women are often conceptualised on the policy level as an ‘under-exploited resource’ in science and engineering, and only clearly moved up the European policy agenda in collusion with the growing concern about a lack of researchers to realise the imagined knowledge economy.⁴⁰ The idea of women in science seems to have mostly acceded to both the objectivist deal (inside science) and the capitalist conceptualisation of knowledge as a motor of economy (in policy).

6. Concluding remarks

Our observations and reflections have taken us through a number of places, institutions, moments, countries, disciplines, actor constellations and trading zones which all had something in common: they participated in or were subject to the making and breaking of orders and the creation, reinforcement or deconstruction of boundaries. Through this multi-sited and multidimensional account, we wanted to make palpable the complexities of the changes at work in academic research, as well as the diverse ways in which researchers perceive to be touched by this. We aimed at conveying a feeling for the many processes which are simultaneously

39 For example, the 17th-century scientist, Robert Boyle, a key figure in establishing experimental science, excluded women from experiments with reference to the case when a high-born woman attending the scientific demonstration of the air-pump demanded that air be let into the evacuated chamber of the pump for the suffocating birds inside who were to demonstrate the vacuum [Haraway 1997: 31].

40 See numerous policy declarations on the EU level, as well as in nation states.

taking place and through which an epistemic living space could emerge or actively be carved out by researchers. This allowed us to question some of the dominant contemporary master narratives on research, not only to reflect critically on all too simplistic models of how change happens or could be steered in academic knowledge production, but also to draw attention to the manifold impacts of policy imaginaries and interventions. This obviously means that major struggles, tensions and ambivalences are at work when researchers are dealing with both imaginaries and realities of their epistemic living spaces. To highlight some of these tensions between more globally formulated claims and their more local realisations, the multiple different solutions emerging as well as the proliferation of hybrid situatednesses thus seems essential as a conclusion to this chapter. Our reflections will be organised along six of the European master narratives already mentioned in the beginning of this chapter and their enactments in researchers' epistemic living spaces. The first and maybe most pervasive policy narrative is the one on efficiency, transparency and objectivity which should become characteristic for the new research systems to be put in place to meet the challenges of contemporary knowledge societies/economies. Monitoring the output of these systems should then allow a steady (self-)observation with continuous adjustments. As we showed throughout the chapter, on multiple levels and occasions, the broader idea of an audit society became embedded, 'trickling down' through all layers of research organisations to the very self-conceptualisation of researchers. The problematic aspect of this ideology is not so much the simple counting and monitoring, but rather making only those things count that can be counted. Thus particular value structures, a moral order, came along with such exercises, palpable on the individual level, but also creating hierarchies between epistemic activities, between geographic location, nation states, institutions and much more. These moral orders can be found from the most global level of rehearsing the order of the US, Europe, and the catching-up players like China and India, to the very way in which early-stage researchers conceptualise the quality of their work and imagine their careers.

We have also shown that these orders create numerous (un)intended consequences: the rehearsal of 'East/West' difference while declaring the effort to dissolve it, the hierarchical ordering of epistemic fields with the biosciences being the model which the social sciences should emulate, the values attached to certain types of contract, or the often tacit gender orders which are de- and reconstructed. We have also hinted at the fact that often contradictory forces are at work, such as the shift of many research institutions from a more visionary and vocational mode of ordering to a

quite dominantly entrepreneurial one, which in turn demands the re-imagination of researchers' role models to fit with what one could call the glossy brochure research-manager – often quite a gendered representation in itself. And we have pointed out the fact that such audit systems never work in an epistemic, institutional and social vacuum, but are overlapped and create frictions with other orders, historical, cultural or personal ones that are performed in research. Thus neither efficiency, nor transparency or objectivity are simply well defined entities 'out there', but what they mean emerges through complex negotiations in multiple trading-zones. In that sense, monitoring is not simply following predefined entities in their development, but much rather a process that needs careful and more inclusive modes of governance.

The second master narrative on research and its change addresses changing modes of knowledge production which are partly encapsulated by notions such as Mode 2 research (or similar analysis). Among other features, which will not be touched upon here, the growing importance of inter- and transdisciplinarity is highlighted, symbolically representing the complexities of research questions at stake. Thus there is a strong feeling that we are moving away from disciplinary modes of ordering to more problem-oriented modes. Yet while we can see traces that there are changes going on in the way knowledge is conceptualised, produced, distributed and performed, our analysis leads us to being much more reluctant to interpret them as a clearly directed gradual move away from more disciplinary to increasingly inter/transdisciplinary problem-oriented work. Rather, we perceive a less straightforward development, often moving back and forth between classical modes of ordering research in disciplines, classical key journals, well-organised theoretical bodies and entrenched social organisations, to which one could also count the publication rituals on the one hand, and new ways of conceptualising and ordering research along criteria of relevance, efficiency, problem orientation and speed on the other hand. But looking at disciplinary boundaries also highlights the ambivalence of such delimitations. While they seem, on the one hand, to create barriers which are difficult to cross – a theme often brought to the fore in the policy discourse – disciplinary boundaries are also perceived as a protection which creates the feeling of being on safe ground, of having the necessary epistemic coherence and of being better protected from direct external pressures. Thus disciplines and their boundaries play important social and epistemic roles for researchers in an environment that has become fluid and perceived as undergoing rapid change.

Excellence is the second buzzword in this master narrative on the new mode of knowledge production. As we have shown, the imagination strongly persists

that through specifically fostering only the most outstanding brains, research will quasi-automatically become highly innovative and advance the powerful idea of a leading knowledge-economy. However, we have tried to show that this label, while fostering competition, also creates counterproductive differences. It introduces the moral order of a two-class society into the research system without clearly knowing how to deal with 'the others' that do not fall into these elite conceptualisations. We showed the different ways in which these excellence discourses were used in the national contexts and how they sometimes produced gender differences (e.g. in the sheer number of prizes awarded to outstanding researchers), which at least should pose questions to those implementing such procedures. And it also made us question whether the simultaneous implementation of audit society criteria and discourses of excellence would not simply lead to an implicit reproduction of those who know how to play the audit game best – a fact that can by no means be automatically equated with innovative research.

Over the past decade, the need to better integrate science into society, the call for more dialogue, engagement and participation linked to the new modes of governing science, as well as the need for more context-sensitive innovations, have merged into the third master narrative we want to reflect here. However, our observations clearly revealed that many of the efforts made to 'bridge the gap' between science and society have contributed their share to continuously recreating this very boundary. Thus, while science is intruding into society in a much more sustained manner, simultaneously new mechanisms have been developed to keep society at arms' length and to continue using scientific communication to produce what Whitley [2000] has called "icons of truth".

Depending on broader techno-political cultures at work and the respective histories of the science and/in society debates, such efforts have developed quite different forms in the respective national contexts and have become integrated in researchers' 'normal' modes of action to differing degrees. While it seems no longer possible to refuse communication with society, this new duty often gets framed in terms of accountability and by no means in terms of engagement or dialogue, as the master narrative might suggest. Moreover, researchers expended considerable effort in arguing that all this would by no means touch the epistemic core of their work and would at best make a difference on the 'periphery'. Thus they create a clear vision of a linear chain from knowledge production, diffusion to public acceptance with little to no feedback loops. Public rejection of techno-scientific innovations was still staged in terms of a lack of knowledge.

Narrating the immense applications of scientific knowledge and how this will change society for the better, evidently works best in those

countries which could at least imagine validating such an imaginary, thus only countries with an already more developed economy. Further, it is interesting to remark that different epistemic communities manage, in quite different ways, to become visible through their applications within society. While the biosciences are often clearly linked to tangible outputs one could refer to, the knowledge social sciences produces often remains invisible – and is made invisible through the separation social scientists enact between their academic and engagement/more practice-oriented work. Yet it is precisely this capacity to merge in an inseparable way with the context of application that could be seen as the “success criterion” for social science take-up by society [Beck, Bonß 1989]. But in an academic world where things only count if one can count them (e.g. potential bioscientific applications can be measured in patents, while social sciences have no equivalent), this might turn out as a clear disadvantage and reinforce the hierarchical order between the two epistemic communities.

The fourth master narrative is around Europe as a common space for research, encapsulated in the acronym ERA. Yet we could argue that science as a cultural activity has to be understood as no different from others with regard to its bounding, sorting and ordering activities. In that sense, we could follow Newman’s argument that “we live in a world of lines and compartments. We may not necessarily see the lines, but they order our daily practices, strengthening our belonging to, and identity with, places and groups, while – at one and the same time – perpetuating and re-perpetuating notions of difference and othering. ... it is not possible to imagine a world which is borderless and deterritorialised. Even the globalisation purists would accept that the basic ordering of society requires categories and compartments, and that borders create order.” [Newman 2006: 143] Indeed our analysis has shown how a seemingly borderless research Europe is constructed through discourses on and concrete incentives for mobility and cross-border collaboration. Yet at the same time the use of dichotomies like East/West, old/new member states or Europe/nation states deconstructs the entity Europe in powerful ways. In particular, monitoring activities under the guise of guaranteeing transparency and objectivity strongly support the reconstruction of those lines that were wiped out discursively. Indeed we realised that in many ways ‘scientific Europe’, as a construct without borders, was greeted with a lot of mixed feelings by researchers. It either was unclear to them what Europe meant as a concrete reference frame and they thus did not use it in their positioning work, or they were much too aware that differences were so fundamental in research and its organisation in the different countries, that speaking of one research area seemed awkward. However, retreating

to the national territory cannot solely be understood as a limitation caused by not grasping or not following the European idea(l), but also as a form of protection and belonging. It represented different histories and ways of doing things in research, an imagination where change might happen as well as a feel for an implicit pecking order, felt much more strongly when moving within a shared European territory.

The fifth master narrative is one of global change in R&D, about the speed of change and competition imagined as taking place in a similar way in all countries, a narrative that somehow closes the option of thinking and wishing things to be different in the personal research context. This global narrative develops a strong normative drive towards an imagined ideal, which is often implicitly assumed to be existing somewhere across the ocean in the US. It assumes that change has to happen ever faster, that everyone has to participate in the race and that everything has to be done in order not to lag behind or to be overtaken. Yet this strong ideology – which one could label the tyranny of speed – can only be performed in an elaborate way when developing corresponding self-observation systems by European bodies, nation states, institutions and researchers alike. Thus the audit society becomes the basis for performing this narrative efficiently.

Throughout the analysis offered in this chapter, we have tried to show that, even in the seemingly very homogeneous discourse across national systems and the underlying ideals to aim at, the way in which actual change occurs takes fundamentally different forms, depending not only on local contingencies, actor constellations, but also on the epistemic communities we were looking at. While the dominant audit narrative tries to implement rather strict orders, competing myths persist and try to nourish and keep alive other orderings. But overall, this global narrative had a strong impact on the moral orders performed along with the speed narrative. Researchers coming from different national contexts participating in this study conceptualised their research system/country as lagging behind and as having to catch up, they felt quite pressured both on institutional and individual levels to buy into this logic and struggled to construct a strong epistemic self-esteem. They felt pressured to take up trendy and global topics, approaches and rhetorics, similarly to the way in which national science policies take up trendy and global research priorities. All this can gradually lead to reducing plurality in research and simultaneously to ‘de-localisation’ of research from national or regional contexts and needs.

The last master narrative is on gender and equal opportunities for women in research. Although it is definitely less prominent than the five others, it seems challenging to reflect on how gender is imagined

on a rhetorical level and how it gets performed in the different contexts. Throughout the chapter we have seen many places where the master narratives (re)produced existing gender divides in quite clear ways. Two such examples for gender divides are the disproportionate number of male scientists winning excellence prizes and the fact that women lose out in institutional reorderings. Yet we could also argue with a number of researchers in feminist epistemology that a male gendered ideal of research stemming from 17th-century science is rehearsed in the very way in which the boundary between science and society is upheld and the epistemic core staged as completely protected from societal influence. The homogeneous masculinity of the ideal model scientist is in fact implicitly reconstructed through the multiple venues and mechanisms constituting epistemic living spaces discussed in the chapter. Gender is not merely inscribed in the female body, but is lived through and interacts with other value systems to preserve and transform this domain of life. Even if we succeed in getting more women into research and allow them to move up the ranks, we might ask, in such a highly morally ordered environment, what the price to pay would be for them. What we have tried to argue in this conclusion is the obvious tension between the grand narratives and the actual ways in which bounding and ordering work in concrete contexts. We saw that seemingly homogeneous discourses were locally transformed into fundamentally quite different configurations, had to deal with local histories and contingencies and thus led to rather different realisations. We could observe that, although boundaries are often implicitly and explicitly staged as restrictions or as deranging the production of innovation in European policy discourse, they might play the role of creating spaces in which change can occur in different ways and on ground perceived as safe. But we could also see how some of the new policy orders destabilise whole parts of the science system – the social sciences or those regarded as ‘non-excellent’ researchers – by imposing one-dimensional measures of quality and legitimacy. In a nutshell, we would argue that things are more complicated and multidimensional – and for good reason. Not only from the point of view of diversity and inclusion, but also for the sake of the plural knowledge we will need in the uncertain world we live in. Our observations lead to the conclusion that any responsible European policy for science and technology would have to reflect in a much stronger way the boundaries that are created, reinforced and undone in the multiple processes of implementation of dominant discourses and measures, as well as the modes of ordering accompanying them. Thus European policy would have to consider – to go with Donna Haraway [1988] – “politics and epistemologies of location, positioning,

and situating” and to understand this partiality as its richness and as a/the “condition of being heard to make rational knowledge claims” [Haraway 1988: 589] (see also Preface to the book).

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Working Together Apart

Anne Kerr & Dagmar Lorenz-Meyer

With contributions from: Jutta Ahlbeck-Rehn, Alice Červinková, Ulrike Felt, Lisa Garforth, Marcela Linková, Susan Molyneux-Hodgson, Lisa Sigl, Tereza Stöckelová, Mariana Szapuová, Veronika Wöhrer

1. Introduction

Science has long been associated with individual vocation, especially in Universities, where the charting of new knowledge-territories is a highly personal quest. In this model, acquiring scientific knowledge is a form of discovery that is profoundly individualised [e.g. Merton 1957]. In today's neoliberal worlds, personal responsibility and individual fulfillment have been elevated yet further as organisations seek to turn these qualities of the self into institutional resources and benefits [e.g. Slaughter, Leslie 1997; Etzkowitz 2003]. The focus has shifted from revelation to vision, innovation and entrepreneurship, but the individual is still crucial, as systems of promotion and reward demonstrate.

Nowadays, researchers are increasingly expected to engage in large collaborative project-based research teams and units [Güddler 2003], and to generate financial value, not just in terms of patents and other 'spin out', but in terms of research grant income. These teams are not necessarily fixed in traditional 'lab' structures, but can be transient and fluid: inter/disciplinary collectives of experts held together by loyalty to shared projects, charismatic leadership [Amin, Roberts 2008] and more pragmatic needs to acquire training and create economies of scale to process large amounts of data. These arrangements are typically found in the biological and physical sciences, but are also becoming more common in the social sciences, alongside their increasing emphasis on flexible knowledge workers and user engagement [Connell 2006].

The tensions between the individual and collective aspects of knowledge work in the sciences are the main focus of this chapter on working together apart. We are especially interested in how individual and collective working arrangements are constitutive of and are in turn shaped by distinct epistemic living spaces in science. We explore some of the main logics of togetherness and apartness in science policy imaginaries and compare these with patterns of practice of togetherness and apartness we found in the social and bioscience institutions in the

five partner countries of the KNOWING research, drawing in particular upon observations of researchers' interactions with materials, people and technologies in a range of research settings – including the lab, the team meeting, the seminar – and on narrations of career, belonging and identity, mainly from interviews and focus groups. This follows in a long tradition of scholarship in science and technology studies (STS) in particular [Knorr Cetina 1999; Fleck (1935) 1979; Latour, Woolgar 1979], but also in other areas of sociology, of the academy and/or higher education [Lave, Wenger 1991; Becher, Trowler 2001; Etzkowitz 1992; Hackett 2005]. Yet our approach is marked by a particular interplay of reflections about policy and practice. This gives it a more institutional flavour than STS work with a more micro-level focus upon and a closer attention to practice than the more socio-political approaches of scholars in the sociology of the academy and/or higher education. We begin by looking at the predominant imaginary of working together in science – the network – and contrast this with what we found in the main places where science gets done: the laboratory, the office and beyond. Here we deliberately contrast the virtual and flexible characteristics of the network with the grounded nature of daily working to map some ways in which scientists work together and work apart in science and to consider how they relate. We also explore KNOWING participants' reactions to and experiences of some of the most prominent institutional projects to relocate togetherness to make science more responsive to neoliberal agendas of economic growth and social improvement. We focus here upon two main logics – mobility and interdisciplinarity – the first of which tends to relocate individuals to different countries and/or institutions, and the second relocates individuals in different teams and/or epistemic places which does not necessarily involve a physical move, but does not preclude one either. This involves them in new ways of working together and apart, which has epistemic, organisational and personal dimensions, creating new and sometimes fraught kinds of epistemic living spaces.

Exploring these various makings and breakings of togetherness across policy and practice also allows, and indeed requires us, to examine gender and East/West relations in science that were at the heart of the KNOWING project in three significant respects. First, we reject the masculinist logic of working alone as isolated objective knowers and explore the relational and co-operative aspects of making science work [Haraway 1991; Hill Collins 1991; Grasswick 2004]. Second, we also explore the distribution of labour within science and its gendered aspects. Drawing on the work of Morley [2001] and others, we treat Universities as gendered organisations where assumptions about

masculinity and femininity and a particular organisation of domestic life are constitutive elements of organisational practices and procedures, materialised in specific time regimes or work descriptions [Acker 1990]. As Star argues, it is important to understand the role of invisible work in science and how it is gendered through an exploration of the personal and the political, and/or the “subtle ways in which coalition becomes co-optation” [Star 1995: 24]. Third, we explore what the call to mobility means for people on the move and what this does to their sense of self, security and belonging in epistemic living spaces, as well as the relations between socio-economic centres and peripheries of knowledge production [Connell, Wood 2002]. Belonging is not only about where one stays in a physical sense, it is also about cultural values and connections, be that to an idea of a ‘lab’, a ‘team’, a ‘home’ or ‘family’, or even a canon of knowledge. The capacity to move around these places and the consequences of these moves differ, depending upon gender and nationality.

Before considering our findings in more depth, we turn to introduce the key policy imaginaries of working together and apart that we will interrogate in the sections that follow, and provide a short note justifying our treatment of these policy objects as produced and productive of particular socio-technical ensembles in their own right.

Neoliberal science policy imaginaries of the EU

EU documents cast science, technology and innovation policies in the service of building the ‘European knowledge society’; knowledge production is geared towards applicability and organised in a way to drive economic growth, social cohesion and sustainable environment or, in the language of the European Council [2000], “the most dynamic and competitive knowledge-based economy”. This involves the unification, streamlining and coordination of a seemingly bounded entity: the European Research Area (ERA). It also constitutes a high-level form of working together in the service of the economy: a transnational community of scientists/researchers, industry and citizens organised around nodal points of inter/national centres of excellence. Here the EU becomes a market for knowledge and research in which researchers, like knowledge and technology, circulate between institutions, sectors and countries. At the same time, it also reflects an individualised system of assessment, reward and promotion of scientific excellence and takes the natural science model of teamwork and excellence as its default: teams work to produce visible results that are translated into social and economic benefits through the entrepreneurial activities of scientific

leaders in the market – while the specifically collective aspects of the process of knowledge production often remain unacknowledged.

These framings are productive of a further set of more detailed imaginaries and innovation models about how scientists might work together and apart in the pursuit of excellence and economic growth – namely of the desirability of network building, researcher mobility and interdisciplinary research which we go on to explore in the sections to follow. We note here that the overarching visions of growth and achievement are produced by a range of actors, both human and non-human, as the discourses of other EU policy domains in areas such as work and economic growth shape the scientific domain and vice versa. There is no doubt that policies are not simply a ‘view from nowhere’ but are actively crafted by bureaucrats, politicians and experts themselves. They are also shaped and influenced by policies at a national and international level, and by policies and practices in competitor and allied nations, singularly and collectively. For example, while discourses of the knowledge-based economy were first promoted in the northern and western nations within the EU, they may have more public support in countries such as the Czech Republic. We are not able to trace these connections and their influence on policy making here, but we note that scientists themselves, at least certain elite groups of scientists (including some of our research participants), are involved in, rather than divorced from, these processes. This is to offset any sense that we argue that the researchers in our study are somehow the hapless victims of policies imposed from above. At the same time, we remain committed to exploring the ways in which practices and experiences on the ground are not only shaped by policy but also articulated beyond and despite of policy so that at times it can be facilitative, at times it can be a barrier or a hindrance, and at other times it is simply irrelevant or incommensurate with what happens in practice.

We now move on to discuss the networks of policy imaginaries, draw further on scholarship on scientific networks and communities from the academic STS and feminist epistemology literatures and contrast these with the main types of working together (and apart) that we found in the KNOWING research. Here we contrast forms of togetherness with discourses of alienation and isolation and show their inherent tensions with the drivers of individual academic excellence, and the solitude of particular forms of scholarship. We note that this is not a clear dichotomy. Working together and working apart are mutually constitutive, but also involve a series of tensions and ambivalences that have to be managed in each epistemic living space as they unfold.

2. Locating working together and working apart: Networks of excellence clusters and work-nets in institutions

2.1. Networks of excellence clusters

The European Research Area is, first and foremost, envisioned as “a powerful web of (specialised) research and innovation clusters” in Europe whose “reach should be amplified through ‘virtual research communities’” [Commission of the European Communities 2007: 8]. Networks have nodes of specialisation, located in universities and public research institutions, that are given sufficient autonomy to ensure their success and competitiveness on a global scale [CEC 2007: 14]. Networks then are a form of control with a range of national and European funding schemes having been devised to ensure that excellent individuals match excellent institutions. However, there is little flesh to the research teams of these imagined networks beyond their role as participants in collective research infrastructures, or hosts to excellent individuals. There are no standards or norms for how labs or research units should be organised [Commission of the European Communities 2005]; it is research institutions as a whole rather than successful individuals and teams that are defined as centres of excellence.

Running along parallel policy tracks, the same disjuncture between women in science networks and women working in science can also be found. Policy statements such as “Mobilising women ...” from 1999 and “Excellence and Innovation – Gender equality in science” from 2005, place great emphasis on the idea of women’s networking and experience sharing [Commission of the European Communities 1999: 10]. The 2008 report “Getting more women to the top in research” reiterates “the importance of formal and informal networks as tools for integration in science and research as well as for the promotion of scientific careers”, calling especially for the “promotion, strengthening and funding of institutions and formal and informal women networks which question and combat exclusion and lack of transparency (embodied in old-boys’ networks)” [European Commission 2008: 10]. However, these imaginaries give scant regard to the day-to-day relationships that constitute women’s work in science and research, and the importance of everyday contentment and support in developing a career and knowledge in its own right. The tendency to ‘hive off’ issues of gender to a specialist network also takes attention away from practices in labs and beyond where science gets done.

2.2. Work-nets in institutions

Scholars of science and technology have also focused upon networks of science as ‘work-nets’ [Latour 2004], by which they mean the dense fluid sets of connections between actors, human and non-human, which come together to constitute science and technology. This is exemplified in the work of Latour [ibid.] and characterised by a strong rejection of the idea that social factors or forces determine or drive particular outcomes. Power is articulated and distributed, rather than applied or easily ascribed, to dominant parties. It is internalised and enacted through notions of self and responsibility, rather than a blunt force. Thinking of science in this way, as a series of connections and confluences, helps us to understand it as productive of social arrangements, where individuals are eternally connected together. This avoids the abstract logic of the policy networks discussed above, emphasising the importance of how networks work in institutions. It allows us to consider how relationships are constituted within networks, in particular groupings of labs or teams, and how this can jar with the project of individual excellence and career building within institutional settings. It also means that we must turn our attention to the ways in which some actors can marshal resources or strategic advantage in order to move more freely within and between groupings, or to choose the types of labour in which they engage in such a way that they maximise the value of their labour, sometimes to the detriment of others. This is sometimes a matter of strategic engagement with institutional logics, but at other times it can be a form of resistance or complete lack of regard for what policies say or can do. Drawing on feminist work in organisational studies and science and technology studies, we can also consider how these relationships and practices are gendered [Morley 2001]. Here we are especially interested in invisible or feminised labour in science [Star 1995], but also women’s responses to the idea of networking with other women and what this means in practice [Kerr 1998; Garforth, Kerr 2009].

We now go on to explore the ways in which the researchers we studied constituted themselves in particular groups-that-mattered in the course of our research and will contrast this with their efforts to assert individual autonomy or to avoid or dismiss other kinds of groups. Although we do not want to replicate a simplistic division between the bio- and the social sciences in our study and we are conscious of the immense variability in the groups that people inhabit, we have chosen to begin this section by exploring the dominant grouping we found in the biosciences: the laboratory. Focusing upon the lab allows us to outline its peculiarities in contrast to other ways of working, and the tensions inherent in any lab between the project of the group and the project of individuals.

We move on to contrast the lab with the more diffuse and less certain collectives of the social sciences, specifically the research team, and the lone scholar within the department and the institution more generally. Across the variable forms of working together apart that we explore, we are especially interested in how these researchers articulated, resisted or took little account of the formalised versions of networks or groups that we found in policy. Can we identify particularly strong logics of commercialisation, for example? Were other networking imaginaries less relevant, particularly for women in science? We are also interested in what type of work coming into these groups involved and how it produced certain types of work in return. What work was visible and what work was invisible? Did some individuals or groups benefit more from particular forms of working together and apart?

2.3. Tensions in working together apart in the laboratory

It may seem an obvious statement to make but, first and foremost, bioscientists work in labs. Although more established researchers are networked across and beyond their institutions, their everyday work is very much lab-based: it is their main mode of working together. For social scientists with their own ways of working, the lab comes as something of a surprise: put simply, *people are there*, and it is impossible to work without paying attention to those other people. In part, the need to be there comes from experimental routines but it is also linked to the predominance of project work: colleagues are needed to help with interpreting data, preparing samples, sharing material. This does not just take place within the walls of one laboratory, but is often across laboratories. For example, in the Slovak bioscience site there was an intense traffic between labs, sharing materials and knowledge on a routine basis. The following quote from the Austrian study reveals that this is a matter of relational autonomy:

“... it is teamwork, and then again it's not. Because you're doing your experiments on your own. But you are discussing them with others. And when you don't know how you should proceed then ... there is somebody to talk to and you will get some feedback. Everyone is invited to think along the projects of the others as well.”

[AT_Int_BS_f]

While social scientists may find this everyday togetherness somewhat disconcerting, it appeared to be taken for granted by the bioscientists themselves. But this did not mean that the work of the group took precedence over the individual project, as the quote above, and the other findings, attest. Indeed, without their own project and their own direction of travel, the future of members of the lab and indeed the lab itself would be compromised.

For the senior lab leaders in our studies it was important to coordinate, to find “one problem in common that needs to be solved” [FI_Int_BS_f]. A successful lab had a clear vision, articulated most strongly by the team leader, of a problem with sufficient dimensions to support a range of interconnected projects as well as what one researcher called ‘stealth’ projects on the side [also Hackett 2005]. Each individual benefits from the development of their sphere of expertise, but the lab as a whole benefits from the work as a whole and the careful articulation work of the lab leader. As the Finnish team observed, seeing the whole picture and collecting together all the different individual pieces of expertise/knowledge was a key characteristic of the team leader, but difficult for other less senior actors to grasp because they tended to work on specific aspects of a larger research question [Ahlbeck-Rehn 2007a, b]. The timelines of these projects was also an issue here, for individual researchers working to advance their career, and the collective profile of the lab as a whole. Working together accelerated research work, as in the words of one Slovak bioscientist: “If I were to start from scratch, get back to the basics and study everything on my own, order everything by myself, do the work by myself, then it would take several fold time.” [SK_Int_BS_f] The lab leader was also able to co-ordinate this work by distributing projects which matched researchers’ need for publication, as well as competing demands around commercial secrecy. One of the lab leaders participating in the Czech research told us that he had PhD students working on more basic research because they need publications (about four co-authored articles in refereed journals were expected for a PhD). The three post-docs in the lab worked on industry-related research where experiments and trials can extend in time and publications were therefore delayed. This aligned with the requirement for confidentiality related to patenting which delays publication for one year (although it could also delay their career advancement). This kind of working together apart also gave legitimacy to the work of individuals – the lab acted as a badge of credibility or quality through association with the lab leader in particular. This was important in credentialising laboratory members, a necessary part in building their individual careers, as they moved through the national and international communities of science.

However, lest we get too carried away by the warm collegiality of the lab, it is important to remember that not everyone felt that they belonged and there was considerable work involved in negotiating the tensions between individual projects and the project of the lab as a whole. This seemed to be a particular problem for some post-docs who pointed to difficulties and discrepancies in teamwork, in contrast to some of the doctoral students who seemed to regard collaboration as rather uncomplicated. For these post-docs there was a contradiction between dependency on others (including juniors) and the need to compete internally, to being able to do things on their own, a profile required for advancement. For example, one male post-doc at the Finnish site stated that “everyone has their own segment of things, their own projects, and you’ll get merits when you do your own stuff and not just think about the other members of the group” [FI_Int_BS_m]. From the study as a whole, we found some post-docs experienced teamwork as a failing: a prop for inexperience or lack of drive. Others found the need to be visible problematic – to be seen to be present meant that they might be asked to perform a particular task by the lab leader and have to put their own activities to one side to do so. Although there was clearly a lot of value in being part of a working group, it also involved tensions with their own projects and priorities [see also Knorr Cetina 1999: Ch. 9]. Other researchers in the labs found subtle ways of resisting some of the controls and limits placed on their work by virtue of their membership in the lab. For example, some junior post-docs were known for their haphazard attendance at lab meetings, others came in early before the lab leader, to work uninterruptedly.

Writing papers for peer-reviewed journals in the biosciences as a prerequisite for building individual and laboratory careers also involved working together apart. In some cases, we found individual researchers writing in the first draft the empirical parts they conducted and the lab leader being responsible for drafting the introduction, the discussion and results. In other cases, the team leader pieced together written work from the team to pull the argument together. In the words of one PhD student in the Czech study:

“(in the lab meetings) we talk together and we decide what’s good and what’s new. What’s interesting ... And (the lab leader) says: ‘we should really publish it soon before anyone else will’. So he says who will do this and who will do that. If you do one part of the project – like enzyme analysis, some kinetics – then you write this down for methods. And (the team leader) somehow puts it together from all of us

and makes a draft. He does usually results and discussion. ... And then it goes around all the authors. We check it, put in our ideas and (the team leader) puts it together and then he puts another version out."

[CZ_Int_BS_f]

Practices with listing authorship also varied. In the Czech study, the lab leader (and occasionally a senior scientist) was usually the corresponding author, while the PhD student or post-doc, who was assigned the project and did most of the work, was the first author. In the UK study, sometimes the post-doc who had conducted the research was first and/or corresponding author, with the lab leader being the last author. When more than one lab participated in the research, these roles were negotiated between the lab leaders. Writing together in the biosciences demanded continuous balancing and orchestration of writing tasks and authorship so as to benefit both the research team as a whole and its individual members. This created tensions particularly for senior post-docs and scientists, tensions which were intensified by the trend towards attributing research to the lab leader in some cases. As the following quote from one Czech lab leader demonstrates, the focus on output could also mean a corresponding lack of institutional attention to good leadership within laboratories:

"In essence this evaluation, assessment and this being competitive, when you make it, simply you can say that is capitalism, that's how it works (laughs) and if the result is fine and it counts then how you achieve the output is basically not so important. It's important for your group members if they feel well, but when you are a terrible group leader for your people but the output is great you will be supported (institutionally). You will get more people to terrorise." (laughs)

[CZ_FG_BS_m]

This output orientation can be linked to a curious kind of blind spot around some aspects of managing togetherness effectively in the laboratory. Although participants could readily describe their routines and tensions around working together and apart in detail, some were less aware of a set of more hidden informal work that supported good working relations in the labs and was therefore essential to their smooth

running, but was not readily quantifiable as a measure of success. As we might expect, the work was typically performed by technicians, mostly women, but it was also conducted by post-docs (e.g. in the UK research), (PhD) students and senior scientists, especially when there was little money to hire technicians (e.g. in the Slovak research). This did not just involve cleaning, stocking up, cataloguing or caring for materials and machinery, it also involved supervision of less senior colleagues, help with interpreting data and writing papers and publications, guidance on CVs and presentations. Some of this work was delegated to the more advanced post-docs by the group leaders, but at other times it seemed to go unnoticed by the lab leader, especially when they had been away from 'the bench' for some time.

We would argue that, although these practices were gendered, this was far from a principle organiser of practices of working together apart, and where it was apparent it was within specific and complex contexts which we could nearly always contrast with other observations where roles were reversed. For example, in the UK laboratories we found instances of men and women taking on the role of housekeeper and social organiser and of these roles being associated with feminine qualities of empathy and relationality in some instances. However, in other instances they were not associated with femininity but with good management and deputy leadership which we could read as a more masculine narrative. We also found a strong discourse of gender blindness within the biosciences in particular; where participants were at pains to stress that their sex did not make a difference to the ways they worked together and/or apart. Male and female post-docs and PhD students learned to understand the working of the apparatus and acquire what one scientist called "a feeling for the machine" in order to tune it and to produce relevant phenomena [Lorenz-Meyer 2008] but this did not appear to be gendered or gendering, in the sense of being associated with inequalities of power along the lines of sex.

We also found some interesting reflection on the part of some of these post-docs on how other more single-minded career-focused post-docs moved through the laboratory without engaging in this kind of facilitative work, but this was not gendered in any simple way. Instead it shows that levels and types of autonomy were distributed unevenly across the group, partly depending upon seniority, but also on the ability and capacity to move through rather than stay on in the laboratory. The UK material suggests that people who stayed, or in one case returned, tended to be 'articulation workers', but their status remained precarious: either they took on a lower status technician job, or acquired an ambiguous position as putative lab manager but without much job security, given their dependency on

external grant income. (We will further explore the role of articulation workers in the context of mobility requirements in Section 3.5.)

These inherent tensions in working together and apart were shaped by traditions of inquiry, education/training and institutional structures and structures of career advancement as well as the funding and quality assurance/assessment mechanisms for research. We did not find much evidence that they were shaped by a profound commercialisation of the biosciences, rather we found instances when commercialisation mapped onto existing practices of a division of labour. Projects for commercial sponsors were 'fitted into' work as it evolved: space was found to accommodate specific requests or requirements with regard to intellectual property. We found little evidence of commercialisation driving secrecy about research findings either between or within the labs we studied. Instead, we found evidence of a more general concern about the ownership of data and competition/collaboration between lab-groups working in similar fields.

To sum up, the epistemic living spaces of the biosciences were based on routines of working together, shot through with practices of working alone, alongside or between different groupings and hierarchies therein. The lab was an archetypal space which framed these forms of togetherness, but there was much movement between and beyond its physical walls, as ideas, materials and relationships were patched together to produce scientific knowledge. This work-net contrasts with the network of policy imaginaries in two important ways: it involves incremental, sometimes hidden, articulation work to sustain and realise, and it involves particular tensions for some individuals at a crux in their career, often in its early stages, which also seems to go unnoticed by policy makers and institutions, intent upon forging research excellence.

2.4. Departmental anomie and networks of personal peers: working together apart in the social sciences

We now turn to contrast working together and apart in the biosciences, with what we tended to find in the social sciences, with the usual caveats about nuanced differences and similarities within and between the bio- and social sciences in mind. Let us begin with two extracts from the UK ethnography of a social science department, taken in early visits to the department:

... a maze of identical corridors, yellowy cream, with rows of closed doors. After you go through reception (where there are three administrators, looking very

busy) ... you step into a kind of hush and I didn't see anyone until I went into S's office. Waited for 10 minutes outside while she was on the phone. One or two doors of nearby offices were ajar, faint sound of keyboards tapping, but didn't see anyone at all.

[Fieldnote 10 05 07]

... the department almost eerily silent and non-social. I didn't see anyone all day except F. and when her PI popped in for about five minutes to ask about a meeting.

[Fieldnote 20 07 07]

This story of empty corridors was apparent in most of our social science field sites. In Austria, the researchers noted that there was no culture of talking informally about research within the department of sociology. Many knew only very little about what their office-neighbours were doing: there was little talk on the corridor, sharing of resources and methods or spontaneous helping out, as was the case in the laboratory [see Felt, Sigl, Wöhrer 2008]. In the social science department in Finland, the story was similar: presence at the department was not seen as necessary for professional discourse and exchange and longer periods of absence were not unusual. When researchers did come together, it was often to do administration or teaching rather than research. A regular workshop series where researchers met to discuss selected topics had only been established relatively recently, but there was also a sense for some of internal competition for posts and grants [Ahlbeck-Rehn, Kovalainen, Poutanen 2008]. We also found a sense of anomie and lack of collegiality amongst the UK social scientists. One lecturer articulated this in an interview when she told of her involvement in an unstable and competitive departmental 'team':

"I am kind of networked outside the institution but I haven't got anyone that I would sit down with and say, right, we need to get together and put in a research project. Because they are my competitors."

[UK_Int_SS_f]

Intra-departmental competition seemed to be a concern articulated more strongly by women. It also seemed to be a particular problem

when it came to thinking up ideas for grant applications, and it became increasingly problematic as researchers advanced in their careers and faced responsibility for setting up a research team, as this female lecturer from the UK acknowledged:

“Participant: So you’re very alone and you’re very individualistic ...

Interviewer: You would see yourself as being a lone researcher?

P: I do and that’s very inhibiting.

I: So what is it about it that’s the core of that kind of sense?

P: First of all, I would have to generate the ideas myself and respond to research paper calls or research bids. If I want to do the research myself, the field work, the analysis, the writing up, then that’s fine but therefore it has to be small scale. Otherwise I have to turn into an employer and put someone else on the contract and look after that person and create space for that person in the department. Employ, advertise. Huge. So a huge job.”

[UK_Int_SS_f]

While this kind of progression into a team leader and research manager seemed to be the norm in the biosciences, it was more uncomfortable for the social scientists in our study because it involved responsibilities for employee management in the context of alienation from the department, including a lack of spatial and administrative resources to manage junior research staff. While some of the team leaders in the natural sciences had also bemoaned their gradual retreat from work at the bench, the division of data generation and analysis can be seen as especially problematic in the qualitative social sciences because of the epistemic importance of contextual and embodied knowledge [Mauther, Doucet 2008].

However, we do not mean to imply that social scientists were completely atomised. Their sense of isolation co-existed with other ways of being connected. Overall, social scientists seemed to be networked throughout and beyond their institution in more or less diffuse, fluid and rather invisible, but nonetheless epistemically essential arrangements of togetherness. In all social science departments we found that groups

of researchers were working together in relatively informal ways, e.g. for PhD writing groups, or across institutions within their social peer group for specific smaller (and often applied) research projects, sometimes also around pedagogical projects. The Austrian team found that, in research, personal peer groups constituted essential and stable resources beyond the concrete institutional context [Felt, Sigl, Wöhrer 2008]. These peer networks were sometimes, but not always, single sex. For some social scientists from smaller countries, the specificity of their research topics necessitated collaborations with researchers abroad, although the need for international recognition differed across countries and specialisations. Others had developed interpersonal/epistemic networks within the department. The UK lecturer quoted was in one such network via her PhD supervisor (also head of department), and also strongly oriented towards the values of education and community engagement (although she saw this as to the detriment of her research career because her teaching and policy contributions were not valued to the same degree as research outputs and income).

We also found clear pressures to move towards a more team-based approach to working on projects in the social sciences, as expressed in the following quote from an Austrian academic:

“I think that sociology has very much focused on individual research. That has to change, it will not be possible otherwise and it changes slowly. Policies on the faculty and university level are pushing in this direction.”

[AT_Int_SS_m]

In the UK social science setting, this move towards teamwork was more advanced in the (contract) research unit that was linked to the department. We found that team-working and identity as a ‘team-player’ were very strong and unproblematic among staff in this setting, despite the constant making and breaking of teams in response to external funding environment. This group also saw themselves as doing valuable work both socially and in terms of the institution’s benefit (grant getting). However, there were clear tensions between the unit and the department. Some of the staff in the unit felt undervalued and insecure in relation to the lecturing staff, and considered this to be an unfair situation, given their important role in the generation of external funding. Some of the staff in the department felt that the unit staff had the luxury of not engaging in teaching or administration, so did not show sufficient ‘departmental allegiance’. It was also difficult for

staff to bridge this divide and to work together on projects. Only the most powerful individuals in the department seemed to be able to work across these tensions by virtue of particular institutional and personal networks and histories. In the Czech social science site, members of a gender studies unit maintained close collaborations with parts of the non-governmental sector. Similar to the contract unit in the UK, they found that more application-oriented forms of working together were not valued and supported by the core department. A particular political or moral goal seemed to serve as important glue for these forms of working together in the social sciences, but at the expense of traditional measures of research success in terms of publications in particular.

In contrast to the lab, the invisible work in the social sciences seemed to be more of the core business of research: thinking and writing alone, away from the department, as well as discussing one's ideas or commenting on those of others in virtual communication or international meetings. This was less true for the research unit in the UK, and it did not preclude other kinds of more mundane articulation work that social scientists conducted in smoothing relations between peers, organising and engaging in student support, informal workshops and lunchtime discussions. But, the togetherness of social scientists was notably different from what we found in the biosciences, where research and writing was much more of an open and shared activity, albeit with various layers and hierarchies of working together and apart therein. In the social sciences, working together seemed to be more organised around teaching and administration than around research. Put crudely, academics in the biosciences seemed to *come to do research work in the lab* – their core form of togetherness, whereas academics in the social sciences *went away from* the department to conduct research, be that fieldwork, thinking or writing.

With respect to gender, there is some evidence, particularly from the Finnish and UK sites, that some women social scientists were more prone to note or articulate gender differences in the distribution of work than bioscientists, particularly with respect to articulation work such as advising students, and what one woman aptly described as 'invisible caretaking' (see also Chapter IV of this volume). However, as was the case in the biosciences, gender differences in doing social science was typically denied or treated as largely irrelevant, or but one small part of the complex range of personal and institutional contexts of social science. There was considerable diversity in the levels of investment that social scientists placed on working together and apart. Some social scientists, like the research unit in the UK, were very much constituted as 'outward-facing' teams. Others were keen to preserve

the lone scholar model and to interpret the call to work together as a form of managerialism to be resisted. These constitute two poles of the epistemic living spaces of the social sciences, with a patchwork of working together and apart for many social scientists in between. This patchwork could be liberating when it allowed people to create quiet writing spaces and/or fulfilling partnerships with other scholars and/or key stakeholders beyond academia, but at other times it caused tension and unhappiness, particularly a sense of anomie and isolation from colleagues in the department.

2.5. Togetherness in politics and practice

When we contrast the policy imaginaries of science networks with togetherness in practice, we find that there is a need to flesh out what ideal forms of working together and apart could be, to account for and support the variety of ways of working together and apart in the bio- and social sciences in concrete organisational settings. There is a profound tension around collective and individual benefits of working in science, but also a need to balance belonging and feeling welcome in a group and stepping out of collective working to grapple with the difficult work of thinking and writing alone. However, not all researchers have the same levels of control over where and when they work together and apart: only certain actors were able to play the balance between working together apart to their advantage, usually those in more senior and institutionally safe positions, with a track record of funding and publishing success. In many instances those individuals were men, particularly in the biosciences, but this was not always the case. We also found that this capacity to choose when to work together and apart did not necessarily map onto dominant notions of research excellence. This is especially true for those on the margins, in the social and the biosciences, who were engaging in valuable but often invisible and unacknowledged forms of articulation work which supported knowledge production. Clearly the bioscience model cannot be imposed on the social sciences as a route to better knowledge production, but may have to be rethought with respect to inherent hierarchies and the role of invisible articulation work. On the other hand, it seems that the social sciences have something to learn from the biosciences in tackling anomie at the departmental level. Although they are far from the individualised intellectuals of traditional imaginaries, social scientists seemed to feel that they lacked an everyday kind of togetherness and belonging that employees in other jobs take for granted. At the same time, there is a need to institutionally acknowledge and support the invisible but epistemically pertinent networks of personal peers as well

as teaching and policy commitments that constitute epistemic living spaces for researchers in the social sciences.

The epistemic living spaces of the bio- and social sciences also were subtly gendered in ways that are more complex and contradictory than the policy imagery of women in science networks allows. Women in our study pointed to their hidden articulation work, and the less visible networks through which men advanced their academic careers. Some were part of personal peer networks with other women and we found some instances of feminist collaboration. The Austrian team observed vivid networking activities among younger women in the social sciences that were based on previous and current co-operations. On various occasions these women were supporting each other, socially as well as professionally. Professional support consisted in inviting each other to new collaborative projects. In group situations they used opportunities to refer to each others' work, to ascribe each others' skills and expertise or to give the floor to other women. This may suggest that women in the social sciences have appropriated strategies of support that have been described for men ("boys' clubs") [Felt et al. 2007]. In contrast, women's organising did not have much saliency for researchers in the biosciences, particularly in the Czech Republic and Slovakia. In the KNOWING study as a whole, women were ambivalent about joining together in a bid for career advancement. In the post-communist countries, this was shaped by a historical absence of feminist activism and the administrative/bureaucratic versions of feminism associated with the State in their countries' political history; and in all participating countries, it was reinforced by a strong discourse of the meritocracy in the biosciences in particular. The Austrian data, for example, shows that the success of one of the most renowned bioscientists who had won several prestigious awards, had an outstanding publication record and also was highly visible in the media for her engagements in communicating science to the public and supporting the advancement of women in science, was regarded with considerable suspicion. Some thought she had received support because she was a woman or because of her public visibility [Felt, Sigl, Woehrer 2007]. Such examples recall an observation that Keller made 30 years ago: 'It may be difficult for those removed from the mores of the scientific community to understand the enormous reticence with which anyone, especially a woman, would make public his or her personal impressions and experiences, particularly if they reflect negatively on the community. To do so is not only considered unprofessional, it jeopardises one's professional image of disinterest and objectivity. Women, who must work so hard to establish that image, are not likely to take such risks.' [Keller 1977/ 2001: 16].

We now turn to consider how togetherness is being reshaped by current key institutional drivers to relocate togetherness in the interests of better science: mobility and inter/disciplinarity.

3. Relocating togetherness: the call for mobility and flexible working

3.1. Celebrating mobility: official discourses on the move

The unfettered endorsement of a high level of mobility of ‘competent researchers’ and knowledge is one of the most visible and striking aspects of contemporary science policy imaginations and a principle characteristic of the vision of the ERA. Researchers’ movement “between institutions, disciplines, sectors and countries” [Commission of the European Communities 2007: 2] is closely associated with innovation and excellence. “Mobility is becoming a standard feature of a successful research career.” [CEC 2007: 8] It is seamless, and extended in time: “Researchers ... increasingly need to be mobile for large parts of their career” [CEC 2007: 11].

In these discourses, the alignment of human capital and research infrastructures is expected to create European and global centres of excellence. Mobility is also deemed essential to the sharing and transfer of knowledge between countries and particularly between research institutions and industry. International (as opposed to national or intra-institutional) hiring is therefore actively encouraged. The normative and practical endorsement of an “adequate flow of competent researchers” [CEC 2007: 2] through a range of mobility schemes that target individuals (not groups) thus evokes Mertonian ideals of an unrestricted flow of knowledge *and* embraces the increasing commercialisation of academic research. Yet mobility should not mean personal insecurity and isolation/lack of family for researchers. Here the EU is concerned with questions of “(h)ow ‘flexicurity’ principles (e.g. combining labour market flexibility with employment security) (could) be applied to the researcher labour market” [CEC 2007: 12] and considers mobility for “researchers and their families” [European Research Council 2007: 14]. “Lubricating the flow of highly skilled labour” [Iredale 2001] involves removing legal, administrative and practical obstacles to mobility at national, institutional and EU level.

These policies constitute a deliberate effort to relocate togetherness to make science and scientists more productive. Researchers are expected to move through institutions, building their careers and transmitting the benefits of their experience to new groups or teams. The most successful

researchers are assumed to be the most mobile, moving between post-doctoral positions in their early careers, to set up research groups of their own, in different institutions. This natural science model assumes that researchers are highly autonomous, and largely ignores what it takes to foster and maintain the everyday forms of working together in labs and departments that we identified in the previous section. As we now go on to explore, mobility, or relocating togetherness, also has distinct disadvantages for some researchers, if not for science itself.

3.2. Unequal flows: gender and geopolitical stratification

A range of studies has explored why scientists move, identifying “a continuum of choices and constraints shifting over time and space and the life course” [King 2002 cited in Ackers 2005: 104]. In contrast to the uniform picture painted by science policies, where researchers move to centres of excellence to advance their careers, this work also points to important differences in terms of gender and ethnicity, and the complex flows of people, knowledge and finance that form a backdrop to individual biographies and people’s capacities to move and/or stay in particular places (see also Chapter II of this volume). With respect to geopolitical location, research in the US has shown that those situated in the metropolitan centres of scientific production show little interest in internationalisation and international mobility: they travel abroad for research, conferences or study much less frequently than researchers from other countries [Altbach 1997]. Research conducted in Australia likewise shows that academic mobility commonly goes from the periphery to the science centre where most marketable science is produced (and then marketed to peripheries) [Connell, Wood 2002]. We also know that these dynamics of mobility are highly gendered, or more precisely marked by the intersection of gender, age and geopolitical location. Eastern European studies cited by Ackers [2005] show, for example, that mobility among early stage researchers is highly feminised, dropping to one third amongst the 30-49 year age groups. Connell and Wood [2002] similarly find for Australia that at this time of life it is expected that husbands’ careers determine the movement of families. These studies thus point to the need to explore how gender and East/West dimensions may shape responses to the call for mobility for the participants in our study that we will address in the next section. Although critical in some respects, most studies on academic mobility focus on mobility in the natural sciences and seem to take the universal benefits of mobility for granted. There are implicit assumptions about the role of mobility in advancing knowledge in official policies and some of

the academic studies detailed above, particularly the assumption that the act of moving around somehow makes knowledge better, for individuals and the ‘community’ as a whole: moving opens up the potential for new approaches to knowledge production, the transmission of new skills and stimulating learning and comparative thinking. This assumption about moving making better knowledge is also tied to assumptions about moving making people better – giving them new language and cultural skills, and developing their personal qualities such as resourcefulness and independence. Whether or not mobility may mean the same things for social and bioscientists will be a further focus of our analysis: How is mobility experienced by researchers on the ground, how does this shape epistemic living spaces and what does it do to knowledge in the bio- and social sciences? Who benefits and who loses from these relocations of togetherness?

3.3. Why mobility?

We have considerable evidence from across the national teams that mobility was seen as an integral part of research training in the biosciences. International mobility was a normal expectation or even obligation for researchers and lab leaders in most of the KNOWING labs, although this is a relatively recent development in some cases. Mobility was important to the development of an individual’s career, as well as the ‘career’ of the lab. This was not only a matter of building networks and liaisons between individuals and teams, but also part and parcel of the process of making knowledge and having it attributed. For example, in the Czech bioscience institution, a lab leader, who previously had a permanent position in his home country, encapsulated the individualistic impetus of mobility when emphasising how *not* moving had meant that he “was always in the shadow of the big boss”:

“Because you are somehow the lieutenant of the big boss and as such, everything you produce is seen as his research or his result. ... This I also experienced later several times in research commissions, I was accepted as a substitute of Professor X when he sent me, but never as myself.”

[CZ_Int_BS_m]

Similarly, in the Austrian biosciences it was striking how mobility was presented by some interviewees (often men) as vital to the scientific

life course because it meant that researchers had to be independent and prove themselves, thereby enhancing their scientific and personal development and creativity. A male bioscientist answered a question about what was important to his career thus:

“Yes, going abroad. ... I mean, really going away. Not only Bratislava but really away. You need to have the feeling that you are really left to your own resources.”

[AT_Int_BS_m]

This emphasis upon mobility also seemed to have become codified in employment practices. We noted a relative (in some cases, dramatic) decline of permanent positions over the past years and a concomitant increase in fixed-term contracts in countries such as the Czech Republic and Austria. Where countries have tried to limit successive fixed-term contracts, the impetus in the biosciences seemed to be to enforce (international) mobility rather than provide permanency. For example, in the Austrian bioscience research institution the new scientific director more or less explicitly excluded ‘local post-docs’ from applying for junior lab leader positions in the first instance, even when they had worked abroad with the stated aim to further the institution’s international competitiveness (the positions were opened up in a second round, albeit at a lower grade).

An emphasis on *westward* mobility (see also Chapter II) was also a strong theme in our findings from the biosciences in Slovakia and the Czech Republic, where researchers looked to move to other European countries or the US, where conditions were deemed better for creativity and individual development. Access to new/advanced instruments, programmes and techniques, not merely prestigious teams, was a key motivation for mobility in the biosciences. As one Czech woman lab leader said about her stay in the US, “you can think of experiments that you cannot afford here because of money and instruments” [CZ_Int_BS_f]. In the communist era, some scientists had the opportunity to move abroad but they told of how they were compelled to return by the State who threatened to block the advancement of others in their family if they did not. Nowadays movement is routine. In the Czech bioscience institution that we studied, PhD students were expected to move abroad on completion of their studies to gain experience. This was not just a matter of gaining professional experience, but also cultural and social experience and obtaining (English) language skills. There was also pride in being able to host international fellows at these institutes.

Here, the epistemic and the personal pleasure/benefit of mobility were emphasised together. Similarly, at the Slovakian bioscience site, mobility was presented as a ‘natural’ step in the career path. This was not just a benefit to individuals but to the institution, as new people brought new contacts and techniques with them. The ‘trade’ in post-docs formed part of their strategic alliances with other labs in the global scientific community [also reported by Traweek 1988]. This mobility was highly prized as part of what it meant to do international science – there was agreement that biosciences could not be national sciences. As one of the Czech lab leaders put it, “we think global, otherwise it makes no sense” [CZ_Int_BS_m].

In contrast to international mobility requirements in Austria, Finland, the Czech Republic and Slovakia, there was more evidence of national mobility in the UK, which was aspiring to be and sometimes perceived to be a kind of science centre within the EU. British researchers in one of the UK labs tended to look more exclusively to the US rather than Europe in strategic alliances to specific institutions with which the lab leader already had dense connections. The senior post-doc in one UK lab described these as an almost familial relationship, with the US lab leader being “kind of the academic grandfather”. However, a culture of mobility was well entrenched in other respects, not least in the large presence of European (and other international) researchers in some of the most successful labs. For some researchers, being mobile meant finding a temporary home for their work, as in the words of a French researcher:

“here (at the university) I don’t really have a real contract because I have a fellowship. So I’m not hired by (the lab leader). I have my own money for my salary and I just have a contract with the University that depends on me staying (in the lab) and getting that fund ... ”

[UK_Int_BS_f]

For the lab groups as a whole and the lab leaders in particular, the lab needed this flow of researchers ‘passing through’ to sustain its vitality.

Mobility in the social sciences was also present, but in a more restrained manner. Czech social scientists moved westwards for fellowships in English- and/or German-speaking countries, and forged intellectual links with academics abroad. Students also spent short periods abroad as part of their training. However, mobility was far from a career requirement. In fact, in all of the social science sites under study there

was a strong emphasis upon researching in national contexts. This was linked to an epistemic obligation to remain close to the local context in understanding social relations and cultures, as well as the need for language proficiency. For example, Slovak sociologists stressed the small number of social scientists in contrast to the large number of social problems that needed to be investigated. As one lecturer put it “who should care for local problems if not us? ... and these themes may not be interesting for (others) anyway” [SK_FG_SS_m]. While junior sociologists in Austria expected that the experience of international mobility would become more important in the future, there was a notable absence of arguments about the benefits of mobility overall.

3.4. Anxieties, insecurities, resistance

Our research also generated a range of stories about the substantial costs of mobility, to individuals in particular, and about the restricted access to it, particularly in the biosciences where mobility was increasingly normative. In the Austrian bioscience site there was considerable ambivalence about what it meant to be mobile: researchers felt that they could not plan to stay in an institution, but neither could they plan to come back to a specific place after a period spent abroad, with one commenting, “it makes career planning extremely difficult” and another noting that “there are simply too few jobs, and there is no career track in that sense” [AT_Int_BS_m]. We also found instances where local post-docs felt threatened by visitors without family obligations which enabled them to “get good results and get them fast” [FI_FG_BS_f], in the words of one Finnish scientist. Rather than a smooth flow of researchers between institutions there was considerable awareness of international competition and what one Finnish social scientist aptly called the “testosterone features in the whole system” [FI_Int_SS_m].

Many researchers also spoke of family obligations or future family planning which called for their more stable location and made them less willing or able to be mobile. Unsurprisingly, this affected women more profoundly. We found evidence of the high personal costs of being mobile and people’s experiences of feeling rootless, sometimes isolated and unable to form long-term relationships, to get mortgages or to have children, especially from women scientists. There was also considerable commentary on the hypocrisy of established researchers who expected mobility of their junior staff while they themselves did not have to meet this mobility norm in their early years:

“Yes, I’m considering going abroad. But it is, I think, a difficult decision. I don’t want to be separated from my girlfriend for a long time. I see it a bit, this force for being mobile, it’s not as easy as it is presented. It’s easy for a generation of professors to say that now. Mostly they are men, they had wives that joined them ... for me, it’s not so easy to decide”

[AT_Int_SS_m]

This quote demonstrates the gendered dimensions of these decisions, which are not only a matter for women with children, but for people with more equal relationships with their partners than those of the traditional ‘male breadwinner’ variety.

Mid-career scientists also sometimes reported overload and ‘excess’ of travelling, especially men, as demonstrated in the following quote: “I go perhaps 20 times a year abroad travelling for lectures or conferences or meetings. ... I was enjoying it earlier and I don’t hate it now but if I do not need to go, I don’t go” [CZ_Int_BS_m]. Bioscientists at this stage appeared to be strongly invested in establishing positions and building epistemic living spaces where seniors’ mobility was no longer required, but they could choose the timing and form of travel and short-term mobility they preferred. At the same time, they were applying for or were members of European mobility schemes that brought in junior researchers to their own labs, evidence of the importance of a constant flow of researchers that we noted above. Mobility was a multiply stratified endeavour, with those in more secure (team leader) positions most globally involved and often acting as mediators between science centres and peripheries, but in ways that did not require their own mobility [see also Connell, Crawford 2007].

Since we found much less emphasis upon mobility in the social sciences across our national sites, it is not surprising that we also found less explicit resistance to it. As discussed above, we did not find that it was a career requirement in the social sciences, although we found that a significant number of social scientists had worked abroad (except in the UK case, where this was less apparent). Although staying in one institution rather than moving to build a career was a problem for some, many social scientists actively sought stability and support. This was especially true for some of the academics in the UK social sciences, who spoke positively about the benefits that they saw in being ‘home-grown’ (although they also saw that this had slowed their rate of promotion). Since the often very local and/or national topics that sociology is

concerned with were also emphasised by participants in Finland and Slovakia, this embedding of the social sciences in national as opposed to international contexts can be seen as a key reason for researchers staying in national or regional environments that we found therein.

3.5. Mobility in politics and practice

The permanent circulation of researchers that is normatively and practically promoted in European policies and funding schemes, involves a continual relocation of togetherness and rebuilding of epistemic living spaces as researchers enter and leave research groups and departments in the ‘churn’ of knowledge. On first impression, social scientists would seem like the ideal mobile workers, given the more individualised forms of working that persist in these disciplines. However, we found that the mobility associated with working together apart in the social sciences was of a more low level and intermittent form than its routinised and larger scale in the biosciences. The disciplinary commitment to locale keeps social scientists in particular national and/or institutional contexts more readily. ‘Life changing’ events of moving jobs and countries every few years are nevertheless becoming more common in the early careers of researchers in both disciplines. On the other hand, we might also have expected to find a profound disjuncture between the call to mobility and flexible commitments and more obvious ways of working *together* in the laboratory that we found in the biosciences. But there was no such disjuncture, precisely because the ways in which researchers work together apart in the biosciences, particularly the tendency of juniors to work on fairly delimited projects allocated by seniors, seemed to bridge or manage this potential gap. Overall, we found that bioscientists demonstrated a greater willingness to relocate togetherness, perhaps because they were party to more routinised and embedded forms of everyday togetherness, in contrast to the more polarised forms of working together and working apart in the social sciences.

There were tensions in managing mobility nonetheless. Relocating togetherness through mobility does appear to intensify the divide between situated ‘articulation workers’ and mobile career-builders that we identified in the previous section, including its gendered dimensions. It also stratifies knowledge workers into an elite who occupy secure positions and are, to play on Morokvasic’s [2004] phrase, “mobile in settlement“, and an increasing class of less secure knowledge workers who cannot all make the transition into more privileged and stable positions after ‘proving’ their excellence through mobility. Mobility can bring opportunities for personal and professional

growth, even adventure, but it can also involve considerable personal cost to some individuals who find it disruptive and unsettling for their epistemic living space. Perpetual mobility was rejected by most, if not all bioscientists in our study. This was particularly true for researchers with families – be they men or women – although women were more likely to resist the call to mobility than men in these situations. There was an East/West dimension to mobility in our study that must also be recognised (see Chapter II of this volume). The pull north-westwards was most keenly experienced by young career-building scientists in the biosciences who were establishing their careers. But this could involve a mismatch between the needs of researchers and institutions. We found instances of researchers unable to ‘go home’ because there were no available jobs; we also found instances of institutions finding it hard to compensate for the loss of their post-docs to the West, most obviously perhaps when there were no such posts available for international researchers as at the Slovak bioscience institution. There is also a danger that this westward gaze will mean that knowledge could become homogenised to align with the ‘grand challenges’ of the affluent West at the expense of local solutions to local problems. The importance of epistemic living spaces which embed knowledge and relationships in locally defined communities should not be underestimated.

4. Locating inter/disciplinarity in knowledge work

4.1. Multi/inter/trans-disciplinarity in European science policies: an absent presence

The second dominant logic of relocating togetherness in science in policy imaginaries is interdisciplinarity. Explicit statements of the need for multi/inter/transdisciplinarity⁴¹ are not particularly prominent in EU economic or science policy, precisely because they have become entrenched. The Commission’s Green Paper on the European Research Area [CEC 2007] makes no direct reference to interdisciplinarity (or its alternative contenders), although “easy movement between disciplines and between public and private sectors” [CEC 2007: 8] is envisioned as

41 In line with authors such as Strathern [2007], we use interdisciplinarity as the generic term for a continuum of multi-, inter- and transdisciplinarity. Multidisciplinarity is often associated with an additive approach that brings together different perspectives but does not question the borders of disciplines; interdisciplinarity denotes synchronising and integrating methodologies and epistemologies across different fields; transdisciplinarity, similarly to interdisciplinarity, refers to “various forms of intellectual transculturation” [Steinmetz 2007], but also denotes the involvement and participation of non-scientists in problem formulation and knowledge production practices.

a characteristic of researcher mobility and “effective knowledge sharing notably between public research and industry” [ibid.: 2] is flagged up. There is little emphasis on the co-production of knowledge, and “involving society at large” [ibid.: 9] is restricted to a one-way model of “communicating and discussing science, research and technology” [ibid.: 17]. The implicit association of interdisciplinarity with excellence and innovation is more apparent in research funding where it is used synonymously with multidisciplinary. In the Seventh Framework Programme of the European Commission (FP7) multidisciplinary is ‘encouraged’ in the ‘Cooperation’ programme through research funding which cuts across the defined research priorities. The European Research Council (established in FP7 under the ‘Ideas’ programme) extends its funding to “ground-breaking, high-risk research in all scientific domains, including research of a multi- and interdisciplinary nature” [ERC 2007: 3], which more directly suggests that interdisciplinarity is associated with excellence. The Council also recommends that “the traditional departmental barriers need to be reconsidered, and a strong focus on interdisciplinarity promoted” [ERC 2007: 9]. Unsurprisingly, the main focus here is upon the natural sciences, and excellence is strongly tied to economic benefit and applicability. For example, the life sciences thematic priority in FP6 bundles interdisciplinarity and knowledge transfer, emphasising “(i)ntegrated multidisciplinary research, which enables a strong interaction between technology and biology ... to underpin applications to human health”⁴².

These various versions of interdisciplinarity all seek to relocate togetherness, to make researchers work in hybrid epistemic living spaces, across disciplinary and institutional divides, but with the common goal of better knowledge production, applicability and communication. Barry and colleagues draw attention to the “the multiplicity of interdisciplinary forms” [Barry, Brown, Weszkalyns 2008: 24] and note the three prominent logics that guide interdisciplinary practice: accountability, innovation and ontological change. Interdisciplinarity is synonymous with novelty and risk as well as excellence: a matter of breaking down barriers between subject areas, or silos as they are sometimes called, to forge new forms of more fluid, responsive and often marketable arrangements of togetherness.

42 See http://cordis.europa.eu/search/index.cfm?fuseaction=prog.document&PG_RCN=5465100.

4.2. Interdisciplinarity as relocating togetherness

The vague and programmatic concepts of multidisciplinary in European science policy have been met with lively debate within the academy and beyond [e.g. Nowotny et al. 2003; Strathern 2007; Maasen, Lieven 2006]⁴³. Researchers have noted that interdisciplinarity is not simply the synthesis of disciplines that necessarily guarantees epistemic change; it can also involve hierarchies of knowledge, with one discipline in the service of another (for example studies of the social impact of science to improve its uptake). Interdisciplinarity can also be driven by antagonisms to existing features of disciplines: feminist scholars who reject disciplinary canons and are committed to continuously questioning their own epistemological foundations and exclusions are one example [Dölling, Hark 2001]. However, as Barry and co-authors note:

“Disciplines should not ... be regarded as homogeneous, but as multiplicities or heterogeneous unities marked by differences which are themselves enacted in multiple ways” [Barry et al. 2008: 27]⁴⁴.

Interdisciplinarity has also been criticised on the grounds that the blurring of boundaries between research and industry undermine intellectual inquiry (because commercial considerations impact on project choice and limit academic freedom) [Kleinman, Vallas 2001]. These debates suggest that the policy imaginary of relocating togetherness through interdisciplinarity does not capture the challenges and possibilities for relocating togetherness, as researchers negotiate disciplinary and sectoral divides in the course of working together and apart. In the following sections, we explore this further, focusing in particular upon how the call to interdisciplinarity plays out in relation to the epistemic living spaces of researchers in the KNOWING study according to their social position, including their disciplinary histories. Interdisciplinarity also takes work: who does the building and rebuilding of interdisciplinary teams? When is it part of their normal work, and when is it something unusual?

43 See also <http://www.interdisciplines.org/interdisciplinarity>; <http://www.transdisciplinarity.ch>.

44 Similarly Dölling and Hark have rejected the common territorial conception of disciplines as clearly demarcated domains (Steinmetz e.g. compares disciplines to states) arguing that “disciplines are characterised by multiple interconnections and shot through with cross-disciplinary pathways” [Dölling, Hark 2001: 1196]; see also Strathern [2007].

4.3. *The biosciences: interdisciplinary by default?*

In the UK, interdisciplinarity is explicitly on the agenda of all of the Research Councils and interdisciplinary efforts have to be made explicit in most grant applications because of their dominant association with accountability and innovation [Strathern 2007]. This contrasts with Slovakia and the Czech Republic, where there is no focus on interdisciplinary research in funding bodies, although there is considerable emphasis on innovation-driven research and knowledge transfer.⁴⁵ Finland and Austria range somewhere between these, with Austria having established special grant schemes that promote interdisciplinary research, particularly in the life and nanosciences, but also between the social and the natural sciences.

These different policy contexts notwithstanding, interdisciplinary work was a self-evident and unremarkable part of science for bioscience researchers in the KNOWING study. In the Austrian institution, biochemists and molecular biologists worked together as a matter of routine. In the Czech, Finnish and Slovak departments, interdisciplinary work was often carried out in peer groups. As one Czech bioscience PhD student involved in interdisciplinary cooperation with a fellow student with a different specialisation put it:

P: I think today to be mono-disciplinary – in some cases, it's not possible. If you are working in pharmacology, this is not possible because you use many methods every day that are very complicated, very special. And you need some people who understand this and who are able to interpret the data. For example Y prepares some protease and we need to know how inhibitors bind in this enzyme so we need the X-ray structure. But to prepare the crystal and measure it and calculate the structure can be only done by a specialist.

I: Yes, but has it not always been like this? I mean is this then interdisciplinary? You just go to your crystallographer and say, 'here is my molecule'.

P: But we are working together (laughs). We discuss it, yeah? Without this method I am not able to improve my molecules. So it's a necessary part of my work this collaboration. It is not only service for me."

[CZ_Int_BS_m]

45 Notions of accountability in the Czech Republic remain restricted to neo-liberal commercial values and links to industry, while neglecting engagements with civil society organisations and society in general [Stöckelová, Linková 2006].

In this extract, interdisciplinarity is a form of collaboration of specialists, solving each other's problems in the interests of improving knowledge.

In other cases, we found more profound kinds of interdisciplinarity which involved what one Slovak scientist described as a practice of “crossing borders” – working across disciplines, or in interdisciplines, a process where researchers engaged with new knowledge and methodologies, building new approaches and experiments. As one of the Czech scientists put it “good projects really just follow trends in their field ... but to get excellent results, you also have to follow the trends in other fields” [CZ_FG_BS_f]. This illustrates bioscientists' tendency to frame interdisciplinarity as a form of problem solving and innovation. Practically however, this work was often performed by lone post-docs, employed to conduct an interdisciplinary project, liaising between two labs. Several scientists said that these post-docs were hired because they embodied particular disciplinary skills, or they told us about cases where PhD students were temporarily moved to other laboratories to acquire new skills that they then passed on to the team leader. According to Nowotny et al. [2003: 186], transdisciplinary knowledge tends to be embodied in the expertise of individual researchers or research teams more than in research products such as journal articles or patents.

This suggests that the epistemic living spaces of the biosciences are also characterised by fluid, cross-cutting methodologies, canons and techniques, formed around particular shared problems to be solved at the level of the experiment, the project and the field as a whole.

Making interdisciplinarity work

However, we also found considerable evidence of the difficulties of working in interdisciplinary ways in the biosciences. Typically, this was expressed in terms of a ‘clash of cultures’ and different conventions, speeds and views on what constitutes a publishable result (see Chapter II of this volume on disciplines). Others spoke of a lack of resources, particularly time to build a common language and expertise for interdisciplinary work to occur in practice. In the Czech focus group, after a discussion about the difficulties of finding a common language to interact with people from other fields in the short timeframes of a common project, one participant commented:

“What is very important I think in these co-operations is that none of the partners has the feeling that he’s just doing some service for the other partner. So, this is sometimes very, very delicate and very sensitive to keep

the balance so that each of the partners has a feeling that we are doing it together.”

[CZ_FG_BS_f]

The comment highlights the (invisible) articulation work of managing emotions and creating a sense of equality in the epistemic living spaces of the biosciences. A Czech scientist noted that it could be difficult to decline interdisciplinary collaboration without causing offence when you are still building your career. This suggests that interdisciplinarity does not easily collapse the boundaries and distinctions between disciplines, but involves considerable work to (re)negotiate their meaning and significance as the collaboration unfolds.

This also involved working within the confines of particular institutional agendas and cultures. As one of the Slovakian scientists pointed out, disciplinary boundaries reappeared with respect to research funding, administration and organisational divisions at Slovak universities and academies of sciences:

“Today it is almost impossible to say where the precise boundaries are between microbiology, genetics, biochemistry, molecular biology and some other fields, so in general the term life sciences is used or biomedical sciences. But what is paradoxical, if you have a look at the website of the Ministry of Education or the Accreditation Commission, you see that they put biochemistry among the sciences of inanimate objects ... this seems to me so stupid, really small-minded. Institutions abroad have names which are more historical ... while here it is about guarding, patrolling the sandpits ...”

[SK_Int_BS_m]

We also found that bioscientists identified themselves with particular sub/disciplines to narrate their educational and career trajectory in a convincing manner. For example, Austrian biochemists emphasised their grounding in the traditional discipline of chemistry and the importance of understanding phenomena on a very fundamental and theoretical basis, whereas molecular biologists spoke of more problem-oriented working and trying to answer questions within living systems. It was particularly important for bioscientists to present their specialist credentials to gain entry into interdisciplinary collaborations.

These difficulties with culture clashes, institutional and career conventions were experienced most acutely by post-docs who were expected to do interdisciplinarity (sometimes single-handedly) for the benefit of a particular project or team, but who were also not in a position to challenge the tendency of their seniors to expect them to perform and transmit work from other disciplines successfully. When the supervisory arrangements were split between institutions or sites within institutions, these post-docs found it especially difficult to find a home – an everyday kind of togetherness that lubricates the mundane flow of materials and tacit knowledge sharing essential to the success of experimental practice. Even when they were in one place, the strangeness of their project in relation to the rest of the team, and their different ways of working based on different training, even with mundane processes like health and safety or glass washing, marked them out as doubly different, and this made it difficult for them to belong. We know of at least one instance where an “interdisciplinary post-doc” relocated to her previous institution because of a failure to find a fit within the epistemologies, working practices and informal cultures of a lab. In practice then, disciplinary work coincided with interdisciplinarity in the biosciences that was much harder to achieve than current science policy discourse leads us to believe.

These tensions and clashes add another layer of complexity to our analysis of togetherness in the epistemic living spaces of the biosciences, suggesting that interdisciplinarity requires considerable, often hidden, work to sustain and achieve, and that certain individuals take on the bulk of this work, sometimes at considerable risk to their own careers.

4.4. Disciplining the social sciences

In contrast to the biosciences, the social scientists tended to emphasise disciplinary work as a core part of their epistemic living spaces. Working together with social scientists in one’s disciplinary tradition was more important than working together in an interdisciplinary way, even though a number of senior researchers had received training in different disciplines (in the UK or Czech Republic, for example). This emphasis upon disciplinarity occurred in official presentations of the work of the various departments and institutes we visited as well as in the everyday practice of the researchers therein. We found that, in contrast to the forward-looking ‘post-disciplinary’ self-presentations of some of the bioscience institutions, social science research institutions tended to emphasise national disciplinary histories and traditions (with the exception of the UK institution where this was less apparent) in

their narratives of togetherness. For example, in the Austrian case the common focus on both basic and applied social science research was characterised as a continuation of ‘the traditions of Viennese sociology’ (see Chapter II of this volume). Even when social science departments offered specialisations in different fields, these units were often headed by researchers trained in the ‘mother discipline’ – there was less emphasis than in the biosciences on hiring and collaborating with researchers with different disciplinary expertise.

Being *disciplined* in the social sciences was enacted in spaces of training in particular. Amongst the social sciences, sociology was often depicted by its practitioners as superior to other kinds of social sciences due to an established canon and methodology. In seminar discussions, boundaries of the field were re-drawn, sometimes with considerable emotional investment, through defining who and what counts as a ‘good theory’ or ‘adequate methodology’. PhD students and junior researchers noted pressures to establish a clear disciplinary identity and practice for a career in this field, having to translate, for example, concepts from other fields into a distinctly specialist language. This was aptly expressed by one of the Austrian social scientists in a focus group:

“For the academic field I would oppose this thesis of the disintegration of disciplines ... On the contrary, disciplines are perpetuated. For PhD theses you have to emphasise at a very early stage the original sociological character of the thesis.”

[AT_FG_SS_m]

The need to maintain disciplinary cohesion and methodological rigour also came to the fore in research seminars. For example, some Czech social scientists pointed to the historical dominance and normativity of quantitative approaches at the department where qualitative work was tolerated on the margins, although this was slowly changing with the arrival of new specialisations headed by more junior (female) researchers. Software programmes for qualitative text analysis played a role in legitimating these approaches although their proliferation was regarded with suspicion by those working in the quantitative tradition [Cervinkova et al. 2006]. The seminar was an important vehicle for making and delimiting epistemic living spaces, and the discipline was a core aspect of togetherness therein.

Distributing interdisciplinary work

We did find other instances of interdisciplinary work in the social sciences, particularly amongst researchers ‘on the margins’ of departments with strong disciplinary identities. For example, junior researchers at the Austrian universities spoke positively about interdisciplinary research environments in smaller, non-academic, sociological research institutions where they often gained their first research experience by working on externally funded research projects. These more interdisciplinary settings and practices required ‘negotiation, communication, moderation’ but were a way of working together that some researchers and students preferred and found more appropriate for analysing societal problems. An alternative epistemic living space was also forged by one of the Slovak social scientists who published his interdisciplinary work in a non-academic journal as a ‘hobby,’ making it invisible within academic contexts. These ways of working together beyond the department also occurred in circumstances where the lone scholar model of working was dominant. As one of the Slovak PhD students put it “each of them is a ‘lone runner’, everyone runs his/her own show ... (and this is) absolutely the key problem” [SK_Int_BS_m] for establishing more interdisciplinary collectives that many junior researchers desired. In the Finnish study we also found evidence of hidden interdisciplinarity because of worries that it would undermine credibility.

At the UK social science site, interdisciplinary epistemic living spaces were forged in yet another way. For some of the UK social scientists, especially those working in a dedicated research unit, interdisciplinarity was a core part of their *raison d’être*. They saw their work as problem- or topic-focused, and they brought a range of disciplinary tools to bear on it, as one researcher explained, “I mean housing crosses a number of disciplines. It isn’t a discipline in its own right so you’re crossing a number of different boundaries” [UK_Int_SS_m]. This commitment to interdisciplinarity was sometimes used to assert their role in making a difference to society, in contrast to the more academic, and by implication, narrow disciplinary focus of the core department. Even although participants from the department in the focus group commented upon their desire to “write a critique of interdisciplinarity”, challenging its location on the moral high ground of research and reasserting the importance of disciplinary traditions and methods, they also talked about how they traversed disciplinary boundaries in teaching, research and institutional contexts.

4.5. Interdisciplinarity in policy and practice

Interdisciplinarity was a multiple and hybrid practice on the ground in the bio- and social sciences we encountered in the KNOWING study. A range of researchers from both fields laid claim to interdisciplinary work, usually in the interests of problem solving and social change. They sought to relocate togetherness and build epistemic spaces beyond the laboratory or the department in some cases; and in other cases they hired particular individuals as the embodiment of interdisciplinarity. Bioscientists appeared to be more open to communication and working together with experts from different science disciplines and they tended to be more explicitly confident about interdisciplinary practice that was associated with excellence. In contrast, social scientists were more prone to do interdisciplinary work alone or apart from their departmental colleagues. However, interdisciplinarity coincided with mono-disciplinary practices and identifications in both the bio- and social science sites, especially the latter. We found instances of active disciplinary boundary drawing in both areas, especially in research training sites in sociology. Sometimes sociology functioned as a kind of ‘master discipline’. In other situations, its openness and even interdisciplinarity was foregrounded, for example by embracing or claiming new specialisations such as gender studies (often popular with students) within a department. We also found marked divisions of labour within interdisciplinary collectives in the biosciences, where different experts assumed responsibilities for different parts of a research process, or work was simply delegated to other experts and then pulled together in a multi-disciplinary fashion. In both fields, it seemed that junior academics bore the brunt of interdisciplinary working, and this could involve tensions with their career-building within disciplines.

These findings from the KNOWING study contrast with the interdisciplinarity of policy imaginaries, where togetherness is seamlessly expanded in the interests of better knowledge, economic growth and social progress. Instead we found that the epistemic living spaces of the bio- and the social sciences involved subtle ongoing relocations of togetherness on a micro scale, to accommodate multiple and sometimes contradictory demands of belonging to an academic community, a department or an institution, maintaining a sense of purpose and self-worth, taking up challenges and engaging with different experts and stakeholders.

5. Conclusions

This chapter has explored the ways in which researchers work together and apart and how this shapes the epistemic living spaces of science. We have shown that the policy logics of networks, mobility and interdisciplinarity have shaped practice, but not always in the ways in which policy makers have intended. In particular, we have pointed to the ongoing negotiation of tensions around teamwork and individual autonomy, mobility and stability, interdisciplinarity and disciplinarity, in the routine (re)construction of epistemic living spaces in both the bio- and the social sciences. The KNOWING project's multiple foci on policies, practices and organisational and geopolitical positions of researchers, as well as comparisons across select social and bioscience sites, have also enabled us to draw some important distinctions between the bio- and social sciences, without caricaturing or over-emphasising their differences. We found that teamwork, networking, mobility and interdisciplinarity were aligned to existing practices particularly in the biosciences – although those were less driven by commercialisation or engagements with society than policy rhetoric may have us believe. We found that social scientists were somehow less connected to the everyday workplace and therefore closer to the dislocated knowledge worker of policy imaginaries than first impressions might suggest, although this was offset by other distinctive commitments to local or regional epistemic contexts on their part.

More specifically, we identified a range of tensions and different visibilities and acknowledgements around these forms of working together and apart in both the bio- and social sciences that point to pervasive stratifications of epistemic living spaces. Teamwork and individual working were mutually constitutive and occurred within and beyond academic workplaces, but some individuals balanced them in such a way that their careers could advance, whilst others were more involved with less visible articulation work, often to the detriment of their careers. Mobility came at personal and institutional costs. At the organisational level, it was often promoted by those who had reached relatively secure and (more) permanent positions and profited most from new recruitments of knowledge workers. Disciplinarity was often reasserted in the face of interdisciplinarity as a necessary step to secure career advancement and a sense of belonging and identity with a body of knowledge; we also found a lack of recognised outlets for interdisciplinary work in the social sciences and incidences of dominance of particular fields and servicing of others in the biosciences. On the other hand, in circumstances where logics of individuality, immobility and disciplinarity were more pronounced, often in the social sciences, we

found instances of resistance, particularly amongst junior and contract research staff, sometimes drawing on policy imaginaries of teamwork and interdisciplinarity in the process.

We have also been able to bring gender back into an analysis of working together and apart in science. Articulation work, opportunities and investments in mobility and the individual burdens of interdisciplinarity in practice are interlinked and entwined with gender inequalities. This is particularly evident when we consider the roles and positions of post-docs and contract researchers in our findings, in both the bio- and social sciences. We found that the burden of balancing teamwork and autonomy, mobility and interdisciplinarity in practice fell disproportionately on post-docs, just at the time when women are entering this segment of the workforce in large numbers. Although this offers women in science and research more opportunities, staying put, facilitating the work of the team, or conducting pioneering interdisciplinary projects all presented particular career risks to post-docs in particular. Even in a lab or a department where the leaders were not prone to 'terrorise' or 'ignore' their junior colleagues, we found that striking a balance between too much and too little togetherness was a tricky and highly personalised business. More generally applying a gender lens to the neoliberal policy visions of moving centres of excellence, we argue that the eternally mobile knowledge worker who makes and breaks research groups is itself a masculine fiction which is built and valued in contrast to the feminised position of staying put and attending to 'local problems'.

This suggests that policy makers ought to turn their attention to the core forms of togetherness and apartness in the bio- and social sciences and consider how they shape and are shaped by the epistemic living spaces of science. This involves thinking about ways to foster better job security and, dare we suggest, contentment – two key but often overlooked aspects of productive knowledge work. It also requires that we do not simply relegate the balance between cooperation and competition to individual researchers or research institutions as a whole, but consider at an intermediate level what a good lab and a good social science department would look like. Epistemic living spaces cut across personal, institutional and knowledge practices, which involve a range of hidden and unacknowledged aspects of supportive, integrative knowledge work, which are also worthy of more attention and support. In so doing we can aim to build new epistemic living spaces that support and are in turn supported by relations of mutual trust and respect across gender, disciplinary and national divides.

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Times and Trajectories in Academic Knowledge Production

Lisa Garforth & Alice Červinková

*With contributions from:*⁴⁶ Ulrike Felt, Ismo Kantola, Zuzana Kiczková, Anne Kovalainen, Marcela Linková, Dagmar Lorenz-Meyer, Susan Molyneux-Hodgson, Seppo Poutanen, Mariana Szapuová, Lisa Sigl, Veronika Wöhrer

1. Introduction

In this chapter we look at the times and timings at work in epistemic life-spaces. Like all social action, knowledge production is a process that both takes time and makes time [Wyatt 2007]. On the one hand, time can be seen as a quantifiable resource that can be standardised, packaged, divided and organised – and hence used, spent or wasted, as well as supervised and audited. On the other hand, there is phenomenological time, time as experienced, embodied, contextual and qualitative. In this latter sense, the time of knowledge production can be understood as produced through the specificity of practice, as well as through the institutional and social discourses and structures within which researchers operate. We follow theorists such as Urry [2000] and Adam [1994] in being sceptical of the binary distinction between an instrumental, disciplining clock-time that threatens or can be resisted by recourse to unalienated, amodern experiential time. Both are important, but they do not exhaust the ways in which time matters; nor do they form a simple binary. We focus here on the plurality of time regimes at work and at play in and across epistemic living spaces, and on the ways in which heterogeneous forms of practice, discourse and ordering constitute different timescapes for researchers [Nowotny 1992]. We draw on Adam's [1994] analysis of time as multidimensional, and her warning that it can only ever be understood from a situated perspective – that is, through partial timeframes.

Two aspects of time noted by Adam have been central to our analysis of the temporalities of knowledge production. Firstly, we are interested in *timing* – the means by which we fit everything together, coordinate actions and synchronise agendas. From the conflicting demands of work

46 We would especially like to thank Marcela Linková and Dagmar Lorenz-Meyer for their invaluable conceptual contributions to this chapter.

and life in the day-to-day, to the complex planning of academic careers over the life course, how everything fits together, or not, is crucial to understanding why gender still matters in knowledge production. Secondly, we are interested in *tempo*, in questions of the speed, pace and intensity of practices and institutional change. The acceleration of science – from nation-states hurtling forward towards the anticipated benefits of the knowledge economy, to researchers on the ground endlessly trying to stay on top of deadlines and manage increasing workloads – captures vital aspects of lived experience in academic institutions. This chapter explores the ways in which researchers and their knowledge production practices are situated in relation to and are involved in the (re)production of a range of different timescapes that must be managed and reconciled. As Wyatt [2007] notes, the temporal dimension is often overlooked in sociological accounts of science and knowledge making. The epistemic world is frequently envisaged as a series of overlapping territories [see for example Becher, Trowler 2001], conceived of in terms of boundaries, spaces and places. Despite its focus on science as practice, science and technology studies are dominated by spatial imaginaries and metaphors, such as networks and boundaries. In this chapter, we build on the foregrounding of practice and the dynamics of doing in science studies to make the temporal dimension of knowledge production explicit. We explore the multiple temporalities of epistemic living spaces. This focus on the timescapes as well as the landscapes of knowledge work allows us to shed new light on some persistently problematic issues relating to gender and science, and in particular to re-frame their dominant construction in science policy imaginaries. These include the shape and speed of the science career; the reconciliation between working life and personal life; and the pressures and possibilities created by the sense of acceleration of knowledge work in the academy.

In this chapter we make an analytical distinction between two timescapes. We divide our attention between *trajectories* and *everyday time* in order to explore different aspects of our empirical data. The notion of *trajectories* draws attention to narrated time – the stories that collective and individual actors tell themselves about their linear movements through time, involving the ongoing production and reproduction of pasts, presents and futures. What do these say about the values, aspirations and logics of epistemic subjects and communities? What is brought into and left out of such narratives, with what consequences? Which are dominant stories and what (and whose) alternative narratives are left out of the account? Trajectory narratives can be found, for example, in national and European policy-making,

and in the ways that an institution presents itself to the wider world and indeed to itself. Powerful stories of past, present and future also circulate within smaller scale and face-to-face settings – in departments and laboratories, institutes and research groups. Such stories can be deeply inscribed in the identities and performances of researchers' academic selves, even before they arrive in specific working contexts. They are therefore crucial in shaping science contexts and careers. We also focus on narratives and experiences of individual career trajectories. We identify some discursive, institutional and policy processes that construct an abstracted ideal career – linear, standardised and mobile – and explore the consequences of this standardisation for researchers, as well as identifying and analysing different career trajectories.

By *everyday time* we mean to draw attention to the ways in which time is spent and saved, used and produced, managed and accounted for, day by day and week by week, in concrete settings – and to the plural ways it is experienced and made meaningful. Recent literatures on the experience of time in research contexts suggest that external pressures (including those brought into being by various policy trajectory narratives) are leading to an acceleration of knowledge work and speeding up of epistemic life. Our analysis of the multiplicity of everyday time regimes for researchers suggests that this is a very partial story, and we look in detail at how knowledge workers construct and negotiate 'time to think' in the everyday. We pay particular attention to the time(s) of practice and the specific rhythms and routines generated by different cultures and materials of knowledge work. We also look at the temporal dimensions of epistemic identities, and explore how knowledge workers account for and manage the different temporal demands at play in epistemic life worlds, particularly in relation to what in policy terms is referred to as 'the reconciliation of work and family life' (in EU policy) or 'work-life balance' (in the UK). Our findings suggest that for researchers the relationship between work time and private time is not understood in the policy terms of instrumental clock time (time as resource) to be rationally allocated between distinct spheres of experience. Rather, 'work-life' issues must be understood as operating across a range of time regimes, including the timescapes of identity and vocation, experiences and values related to autonomy and flexibility, and the processes involved in making time to think.

Rather than seeing gender as a monolithic category of experience or a structure that must be 'explained' (see also Chapter 1 of this volume), we explore how multiple and conflicting time-regimes are gendered and gendering. In particular, we look at the times and trajectories of the normative career in the context of national and European narratives

about research excellence and the knowledge economy. We show how academic careers and identities are negotiated in relation to everyday time, both 'at' work and across epistemic living spaces. In both cases, we identify some dominant or preferred temporal modes that are gendered, that is, rooted in masculine experiences of knowledge production and work organisation. At the same time, we try to show how the ordering and experiential work that goes into reconstructing and reproducing trajectories and tacit norms around everyday time are gendering – implicated in the making of gendered subjectivities. By focusing on the range of different times that matter in epistemic life, we draw attention to relationships between policy discourses and imaginaries and the messy times of everyday praxis. We conclude by asking *whose* times matter. Whose everyday times conform or add up to trajectory narratives? Whose everyday times and personal trajectories are exploited or made invisible through the operation of dominant institutional narratives? In both these respects, we find that masculine trajectories dominate and women are all too often *timed out* of academic excellence.

2. Trajectories

2.1. Introduction: imagining excellence

We begin our exploration of the times of knowledge production by focusing on trajectories, that is, the ways in which individuals, institutions and policy discourses narrate linear movements through time. In Adam's terms [1994], we are looking here at timescapes that concern the long scope of lifetimes rather than the days and weeks of everyday times. We pay special attention to the ways in which trajectories relate the present to (narrated) pasts and (imagined) futures. We begin by examining the temporal dimensions of contemporary science policies, in particular their relentless orientation to a better future built on more productive knowledge and innovation economies. Science policies increasingly construct the path to future economic success as a single track road along which all interested players must run, albeit at varying speeds. The imagined futures of national science policies have consequences for individual research careers, which we explore in section 2.2 below, showing how in the name of excellence a rigid, narrow and increasingly formalised career path is becoming embedded, at least in the biosciences. The policy imaginary of the linear career trajectory means that often the experiences of researchers on the ground, whose science careers have been ruptured or which take what we call a patchwork form, are overlooked. Finally, in section 2.3, we examine individual careers in context, by considering

them in relation to the forward-moving and backward-looking trajectories of laboratories, institutes and national politics.

As is discussed in Chapter II of this volume, in recent years science has become a high priority on the European policy agenda [see section 3.1]. Scientific excellence is supposed to promote globally competitive knowledge-driven economies built on innovation, research and development. Scientific progress is held to drive economic growth, which can be guaranteed or supported through the rational organisation of research and active mobilisation of a range of resources [Schott 1993]. Here we are particularly interested in how European, national and institutional policy discourses construct particular *trajectories* of knowledge production predicated on moving faster into the future. There is a perceived need to close the infamous gap between Europe and the US and Japan [Hellström, Jacob 2005: 446], and also not to be overtaken by China, India and other emerging economies [EC 2007].⁴⁷ Europe is seen as lagging behind in terms of innovation, and one of the aims of the Lisbon Treaty is to reconstitute Europe as the most competitive knowledge economy in the world. There are clear and important intra-European dynamics, however. International comparisons are important in framing science policy concerns that are driven by the evaluation and anticipation of different speeds. In the UK there is a quest to ‘stay ahead’ and maintain a position as third or fourth only to the US and Japan [Garforth, Kerr 2006]. In Slovak science policy, however, the discourse is significantly one of catching-up with what are defined as established European democracies. A similar rhetoric framed much Czech science policy from the late 1990s onwards [see for example Ministry of Education 1999]. Whilst the straightforward language of “lagging behind” [Analysis... 1999] has now disappeared, in a recent national science policy document [National Innovation Policy 2005] attention was focused on the problem of “falling behind”, and on what was called in several places the “backwardness” of Czech research and development.⁴⁸ Austrian science policy mobilises similar discourses in different contexts: on the one hand, there is a catching-up rhetoric in operation in relation to the USA; on the other, there is a related discourse of staying ahead compared to other European countries of similar size (at least in the biosciences). Recently, Finnish science policy

47 “... globalisation of research and technology is accelerating and new scientific and technological powers – China, India and other emerging economies – are attracting considerable and increasing amounts of R&D investments” [EC 2007: 6].

48 “(i)n the long term the Czech Republic finds itself on the frontier between the developed and developing countries” [Analysis 2004: 133].

has developed the image of a small nation in globalisation with its limited resources and stresses the goal of forward-looking ‘internationalisation’ of Finnish scientific research [Ahlbeck-Rehn 2007a].

We see in all these examples the range of ways in which excellence is spatialised by being equated with internationalisation. Excellence also has a temporal dimension, however, in relation to unquestionable futures that must be guaranteed. At the same time as all countries must move towards this future goal, they must also speed up in order to stay ahead or catch up with competitors. A key element of European science policies, then, is their desire to move faster towards the objective of knowledge and innovation economies, producing what Pels has called an “intense political prioritisation of research” [Pels 2003: 2]. We consider its outcomes in relation to organisational academic cultures and everyday time in the next section of this chapter. Here, we examine the ways in which this new political prioritisation of scientific research has been involved in constructing what we call ‘trajectories of excellence’ across the research landscape which have particular consequences for the construction of individual careers in academic research.

2.2. Individual careers: trajectories and patchworks

We begin this section by setting out the hypothetical case of an individual bioscience career trajectory in terms rooted in dominant science policy and institutional ideals. It reflects the normalisation and, in some contexts, the formalisation of a single model of the career path in the name of research excellence. We focus on the early science career, sketching in the generic lineaments of a trajectory that is coming to dominate institutional imaginaries and national policies across the KNOWING partner countries. We use this to establish a core set of norms and standards that shape the critical discussion which follows. In this discussion we explore the uneven ways in which science careers play out on the ground; the ways in which they are conditioned by different contexts (national, institutional and disciplinary); and the consequences of failing to live up to the normative, linear career for individual researchers. We go on to argue that women are positioned in particularly problematic ways by the creation and reproduction of linear science careers. This is Jan’s story.

Jan’s interest in molecular biology began during his undergraduate degree, and really took off in his third year when he got involved with a research project run by one of his lecturers. He spent time in a research

lab, surrounded by experienced post-docs and friendly post-grads, and got a taste for the bench work and the ideas. He wrote his final year dissertation on his small contribution to the research project, and after graduation got a place on an MSc course in the department of biology at the same university. After identifying the most successful national laboratories in the field, he identified Prof X as a research leader and applied for a PhD studentship in his lab. After a slow start, Jan's PhD project eventually came together (with the help of the lab technician, a couple of experienced post-doctoral researchers and the guidance of his supervisor). He completed his PhD in 4 years and even got his name on a joint publication in a leading journal in the field. With his supervisor's support, Jan started looking for a post-doctoral research position. His supervisor recommended a prestigious lab in the US with which he had good contacts. There was an opening there for a molecular biologist using new methods to work on a problem related to Jan's PhD work, and he moved to the US to take up this 2-year post-doctoral research position. The work went well and he co-authored two journal articles with the lab leader that were well-received. His personal life went well too – he met Maria, a post-doc researcher in the same university but a different lab, who had grown up near to Jan's hometown. Within a year they got married and started to think about moving home and having a family. Jan found a second post-doctoral position at a research institute in his hometown.

We will break the story here for the time being. We do so at a crucial moment when Jan, a male early-stage researcher, is on his way to becoming successfully settled in academic research. His wife, however, will face a range of important decisions and challenges to which we will return throughout this section.

Linearity can be prospective, tied to envisioned futures, or constructed retrospectively, as past events gain coherence and purpose through narrative. It can be framed as desirable, or rejected and resisted. Jan's story illustrates a dominant, desirable and largely anticipatory ideal of the early science career as it is envisaged in the biosciences. This normative career narrative begins with the PhD and progresses through

a series of well-demarcated stages, most notably post-doctoral research on a short-term basis. Each stage of the career depends formally on the completion of the previous stage. Much also depends on the publications that form the basis of the academic CV (although, in different contexts, expectations in this respect may be implicit or explicit), and on the making and maintaining of epistemic and interpersonal connections. Our story breaks off around the second post-doctoral position, demonstrating a divergence of paths in the science career. On the excellent academic path, the linear career will culminate in Jan finding a (relatively) permanent position in a university or research institute, establishing his own research projects and most likely a laboratory group. The other alternatives are heterogeneous, played out in relation to a range of contextual factors and personal experiences.

In the early stages, then, career structures in the biosciences are highly visible and well demarcated. In the social sciences, however, career structures are more ambiguous. In Austria, for example, new imaginaries of the good career built on linearity and a faster pace are in circulation, but not yet embedded in formal structures. In the UK there was some acknowledgement of a standard, linear career path in the social sciences, but it was often positioned as one among many possible trajectories. It was particularly notable in the UK and Austria that the tight linkage between biological age and career stage prevalent in the biosciences was absent in the social sciences. In the Czech context institutional rules were being introduced which were perceived as an opportunity to return the academic environment to the 'normal state', and the loose linkage between age and stage was also prevalent for social scientists who had not been allowed to practise at the university under normalisation.⁴⁹ The Austrian team found, for example, that in general, social scientists obtained their academic qualifications later in life (average age of gaining PhD 32.8 years) than bioscientists (average age of gaining PhD 28.8 years) [Felt, Sigl, Wöhler 2006: 27]. In the UK this looser connection between age and stage was particularly the case for female academics. Many researchers had been mature students

49 'Normalisation' indicates the period between 1969 and 1989. This period was preceded by the political, social and cultural liberalisation in the second half of the 1960s that culminated in the so-called Prague Spring. After the repression of the Prague Spring by Armies of the Warsaw Treaty, normalisation was introduced – through exclusions from the Communist party, censorship, dissolution of different interest groups and associations etc. In the academic world, the beginning of this period (up until 1972) was characterised by the restructuring and dissolution of research teams and sometimes whole research institutions. Many people were forced to leave their research positions. "Generally speaking academic research and publishing suffered under even greater ideological yoke" [Oates-Indruchová 2004: 77].

who had begun their PhDs later in life, or had moved into academia after other careers. In Austria approximately half of the social scientists interviewed had begun their academic training after a period of non-academic professional work or vocational training [Felt, Sigl, Wöhrer 2006: 26]; none of the bioscientists had done so. In the social sciences, then, we found an emergent notion of a linear career co-existing with multiple other possibilities.

In the biosciences, the clear and rather singular character of the career trajectory meant that there was no shortage of advice about how to progress, whether from institutional policies and programmes of support or from laboratory leaders. In the social sciences, however, there was no clear vision of how to support and manage careers, which Austrian researchers experienced as the lack of a “prescribed path” or “clear forward direction” [AT_FG_SS_f]. Similar uncertainties were expressed by a UK social science lecturer who gave an account of career expectations in which emergent or implicit norms were framed as an absent presence:

“So there’s somehow in my head, nobody has ever told me this but somehow in my head there exists some sense in which at this stage in your career you should have this number of publications or your profile should look like something. But no one is actually telling me what that is ... here it’s all by osmosis”

[UK_Int_SS_f]

However, while social scientists expressed frustrations around career ambiguities, endorsement of institutional support for formal career building was cautious and ambivalent. The desire to operate in a more transparent context was offset to some extent by concerns around the normalising powers of bureaucracy. In an Austrian social science focus group, participants expressed the idea that, while more information on how to build an academic career was desirable, the imposition of a highly standardised linear career path would be “too strict” [AT_FG_SS]. A senior social scientist in the UK compared the current emphasis in universities on career management, performance review and enhancement and so on with the not-so-good old days, when it was simply left to individuals, but also expressed some doubts about the rigidity of new structures and mechanisms introduced to formalise careers.

A key period in which the linearity of the career is set up (or not) is the PhD, as we saw in Jan’s story. Here clear temporal differences were

visible between the biological and social sciences. For the bioscientists it was usual to finish the doctorate within three to four years, and indeed this was seen as an important career factor. Traditionally, the PhD was about the production of original knowledge no matter how long it took. Increasingly, however, the emphasis is on delimiting the time period of the PhD in relation to demands of the overall career trajectory, and managing the knowledge production to fit the time frame. UK post-doctoral and post-graduate researchers in the biosciences made comments to the effect that the definition of the PhD was changing in their department so that the emphasis fell on original research which made a significant contribution to knowledge, rather than on an original contribution to knowledge *per se*.⁵⁰ This new emphasis, alongside a more prominent role for so-called transferable skills training in the PhD, was seen as having been introduced in the department in order to better manage academic career pathways, and some post-graduate students and post-doctoral researchers implied that it risked sacrificing academic rigour. However, none of them questioned the idea that the PhD must be completed within a predetermined and relatively short time period. In the social sciences, finishing the doctorate ‘in time’ often becomes an object of tension between, on the one hand, policies and institutional expectations focused on managing the three- or four-year PhD and, on the other, traditional conceptions of doctoral research. McCormack [2004: 320] notes the emphasis in the arts and social science disciplines on the process of producing new knowledge, a solitary activity that is conducted in its own time. She points to tensions between institutional and student conceptions of doctoral research. While PhD students are concerned primarily with the process of the research, institutions are focused on outcomes: “Research in this context is operationalised around conceptions of time that are linear: clock and calendar time. Policies emphasise start times, completion times, finishing in the prescribed time and completing pre-set tasks at fixed times during enrolment.” [ibid.] Similar concerns with the rigid timing of excellence are visible in the recent introduction in Austria of a prize by the Federal Ministry for Research of 2,500 Euro for an excellent PhD thesis. The thesis must be outstanding – but must also be completed within the average time a PhD takes in a particular field; in sociology this was set at three to four years.

In the Czech context, the official time limits for completion of the PhD are the same in the bio and social sciences, but PhDs tend to take longer in the social sciences.⁵¹ Some social scientists opposed the idea that a

50 See for example Park [2007a] and [2007b] on the multiple and shifting definitions of the doctorate in the UK.

51 In the UK, by contrast, the time limit of the typical social science PhD in

doctoral dissertation can be completed in three years and advocated a less linear model of the thesis. In a backward-looking narrative of her PhD, a Czech assistant professor who had interrupted her studies twice, including to work for an NGO and to study abroad, asked: “In three years, what can you get a handle on? It is not only about sitting in the library, you have to live and do other things.” [CZ_Int_SS_f] In an interview, a British female bioscience post-doctoral researcher explained her choice to undertake an MSc prior to her PhD – a modest year’s study that nonetheless in the UK was highly atypical and disrupted the fast pace of the linear trajectory. She made appeals to the value of life experience and more specifically personal development and self-awareness in justifying her choice, saying that she had needed an extra year to become mature and self-directed enough to be confident that she would be able to manage doctoral study. Her account foregrounded the process of becoming a researcher over the PhD as product, and reflected the ways in which career time can be stretched out in order to accommodate the more amorphous necessities of biographical time. These accounts of the PhD stage illustrate tensions set up for researchers by the emphasis on clock time over phenomenological time in the normative linear career trajectory. In the following section we explore in more detail what can happen later in the research career when individuals either cannot keep on the linear track or choose to move off it.

Patchwork careers

Starting once again with Jan’s scenario in mind, in this section we contrast his exemplary linear career with the lived experiences of researchers on the ground. For many, the linear career is either unachievable or undesirable. Our findings suggest that multiple versions of a patchwork or horizontal career run alongside, or perhaps more accurately underneath, the dominant policy vision. A few researchers achieve the model of the linear bioscience trajectory. In doing so, they succeed according to the narrow criteria of individual excellence that relate to careers. They also reproduce the linear model of the science career – sometimes intentionally, as in the case of one Czech bioscience lab leader who drew on his own career history as a model for his staff. For many others, however, continuing to work in academic research settings involved negotiating the consequences and meanings of non-linear trajectories – in some cases for a constrained period of time; for others rather more open-endedly. We use the term *patchwork careers* and

many institutions has for some years been getting shorter as funding councils impose penalties on departments for ‘late’ completion of doctorates.

the concept of horizontality to indicate trajectories which do not add up temporally to the linear standard. They may be broken, disrupted, fragmented or dispersed across different institutions and roles. They are often precarious, marginalised and demoralising. The quotes below give a flavour of some of the troubling consequences of the new patchworks of insecure and non-progressing academic careers for both bioscience and social science researchers:

“... one potential frightful scenario is that I don't have the guts to leave this world, that I'll be here hanging on short temp jobs”

[FI_Int_SS_m]

“I am not going to stick around as some desperate university hang-around that you can see around here, that you have some grants here, some grants there, then you're unemployed, and then you have a project for three years. If I can't establish my own position (permanent position), then I'm quitting.”

[FI_Int_BS_f]⁵²

“You are not informed if the (teaching) position is available. You are about to teach but the position will not be there. Well, I will be teaching here a course as an external lecturer. What a treat!”

[CZ_FG_SS_f]

Patchworking and horizontality are structured and experienced differently in the two disciplinary areas, however, and here we deal firstly with the case of the biosciences. As we argued above, the linear and vertical trajectory is embedded strongly in the biosciences. This trajectory worked well for some bioscientists in the anticipatory mode, when planning a career in discrete, successive stages. Many junior researchers, particularly PhD students, envisaged their future careers in precisely these terms, supported by departmental, institutional and funding council programmes of advice and education on the science career or by the trajectory visions of laboratory leaders. For some, the linear career trajectory also worked well looking backwards from the

52 We include two quotes from Finnish researchers here to make a general point that was observed from across our data; we do not necessarily suggest here that patchwork careers are more prevalent in the Finnish context.

vantage point of a permanent post, narrating how each stage led to the next. In a UK bioscience focus group, a female lecturer described her ‘typical’ career in these terms:

“I did a degree. I did a PhD immediately afterwards. One short post-doc and then one longer one. Maybe that’s not typical. I got in a nice comfortable well-funded position. And then I got a lectureship position and I didn’t drop off the bandwagon.”

[UK_FG_BS_f]

But the linear career often ceased to make sense of post-doctoral bioscience positions in the present, for researchers who were ‘in the middle’ and had not yet converted, or risked never converting, their emergent trajectory into a more permanent and established post. This phase is when biological scientists may find their linear career converted into a horizontal one which lacks the possibilities for career progression and places staff in insecure and precarious positions in relation to their institution. The normative career as constructed by universities and funding bodies positions post-doctoral research as a transient phase leading either to a more permanent role or as a managed exit point out of academia. However, we found that large numbers of bioscience researchers continued as post-docs for some years. These researchers risked institutional invisibility in career terms in a culture in which “perennial post-docs” [UK_FG_BS_m] were not supposed to exist. The linear career trajectory depends on and works to reproduce a particular organisational model of the laboratory team, based on a laboratory leader supported by PhD students and post-doctoral researchers. The conjunction of the normative linear career and the standardisation of the laboratory team were in flux in some of the KNOWING national contexts. In the Czech Republic, for instance, the position of the independent researcher – researchers in senior positions but not laboratory leaders – was increasingly seen as obsolete, a hangover from former times. It did not fit in with current imaginaries of progression and excellence in relation to the three-stage trajectory from PhD student through post-doctoral researcher to laboratory leader. In the UK, a broadly analogous position of independent researcher was only meaningful if individuals had been able to attract external or prestigious individual grant funding. Interestingly, in the Austrian bioscience site there was a small group of researchers who had collectively self-organised their funding over some years in order to be able to occupy

this horizontal middle ground, actively using their shared experience to push against the constraints of having no lab leader position.

In the social sciences it was contract research that presented the greatest risk of an insecure or patchworked career path. Whereas in the biological sciences post-doctoral research was seen as a transient stage in a career trajectory and researchers took their places in clear laboratory hierarchies, in the social sciences a more embedded contract research culture might be said to be emerging. As discussed in Chapter II of this volume, there has been a shift away from norms of permanent academic posts in research-and-teaching contexts to structures of non-permanent contracts in various stages of emergence across the different national contexts. Such roles are often dependent on short-term, external grant funding. For some in the social sciences, contract researcher had become an ongoing academic role at the individual level and a problematic part of the organisational culture of research universities and institutes. This was particularly visible in applied social science units in the UK. Here we found many researchers working continually on short-term research projects which were directly funded by external grant agencies. Some researchers did not aspire to teaching-and-research roles (which usually go hand in hand with permanent contracts); others did not have the opportunity to pursue them; and for some a complex mix of the two was at play. These researchers brought benefits to the institution in terms of research funding and outputs, but our findings suggest that these roles remained insecure and were inadequately built into frameworks for career progression. One experienced male contract researcher remarked in a British social science focus group on his frustration at repeatedly and unproductively trying to engage with university infrastructures to develop a career structure for contract researchers.

According to existing norms, time spent on fixed term projects must be transformed into academic capital – publications, teaching and administrative responsibilities, and ultimately permanent roles – in order for progression through the stages of the linear career to take place. When this did not happen, clock time and calendar time continued to run, but career time was disrupted or stopped. It was noticeably women in our study who were ‘left behind’ and ‘hanging on’, on successive fixed-term or precarious ‘open’ contracts. These researchers had no place in the ideal vision of the academic bioscience community, and in the social sciences they often found little organisational recognition for their roles. In both cases, valuable everyday work in laboratories and research teams could become invisible to the institution [Garforth, Kerr 2008]. The issue of patchworked careers had biographical as well as organisational consequences. Many researchers gave interview

accounts in which they struggled to narrate their careers in a satisfactory way – the past/present/future dynamic did not fit together plausibly, according to the normative ideal, and yet they were still working in academic research. For many, their experience was horizontal – moving around and across institutions, academic fields and different positions – rather than conventionally advancing. This was particularly the case for women and some men whose commitments and choices meant that they could not fulfil the full-time unbroken career model. Research was a particularly risky career path for women who worked part-time, whose careers had been disrupted by maternity or parental leave, or who had followed an academic partner taking up a post-doctoral position abroad. As one bioscience professor commented: “(t)he apprenticeship is quite long and pretty intense. And if you drop out it’s so difficult to get back in again.” [UK_FG_BS_m]

For some researchers, the linear career model had important implications in relation to planning a family, and we found a number of accounts of how biographical and career trajectories could or could not be stitched together over the life course. Although many policy discourses have begun to use the terminology of ‘early career’ rather than ‘young’ researchers, in the bioscience linear career the stages are often normatively closely linked with biological ages. Indeed this linkage is often simply taken for granted, as in this comment from a UK bioscience focus group:

“... we have some new (lecturer) appointments in the department and they’re, what, 31, 32. So if you leave school at 18 I reckon it will take you 10, 12, 13 years before you’re in a position to I guess write a grant to fund yourself.”

[UK_FG_BS_m]

The Slovak research found that for many the PhD phase was perceived as a discrete period, and family must be pursued after early career building. As one researcher in a Slovak focus group remarked: “In the current situation, I think that until thirty, nobody who is doing PhD study can manage to have something like a family.” [SK_FG_BS_f] Bioscience PhD students with children were seen as exceptions. This notion could be subverted, as in the UK case of one powerful lab leader who had her own children as a post-doc and encouraged staff in her laboratory group to make their own choices. However, most PhD students and early post-docs (male and female) appeared to take it for granted that it was simply a biological fact that there would ultimately

be a trade-off in choices for women if the end of the post-doc period coincided with their plans to start a family (or not). In the social sciences, the relationship was somewhat different. In the UK research we found much looser linkages between career stage and biological age in the social sciences. Perhaps unsurprisingly, researchers' narratives did not construct career and family as incompatible. Similarly, Slovak social science PhD students felt that family and early career could be pursued concurrently. The Austrian case was perhaps the exception here; researchers repeatedly remarked in participant observation and in an early career focus group that children hindered careers in sociology. It is notably women who still take account of these two potentially divergent trajectories in relation to their own careers, as was most visible in the Czech findings. Here, women's narratives were strongly related to the (prospective) family; that is, they took current *and future* relationships and parenthood into account (even if individual researchers were not in relationships and/or did not have children). Male career narratives included personal relationships to account for career decisions, but only in retrospect. For them, relating work to family life was a retrospective matter, compared to women's attempts to anticipate how conflicts between the two might shape their future choices.

It should be clear from this discussion, then, why we abandoned Jan and Maria's story towards the end of the post-doctoral stages of their respective research careers. In their early thirties, beginning to plan a family, and with research posts on different continents, there is no guarantee that the timelines of their partnership will be commensurable with both pursuing the linear, excellent, academic trajectory. We have seen that it does not take much disruption or fragmentation to push a career on to the horizontal plane, or for someone to get stuck. These stories need not play out in essentialist ways whereby women's careers move into the patchwork mode, while those of male partners remain linear. In the UK we found examples of couples where female careers took off on the linear trajectory while men negotiated career horizontality or patchworks. We also found plenty of evidence of academic couples attempting to find compromise timings for both career and family life that would enable both to progress. The much more heterogeneous temporal patterns of career in the social sciences meant that the conflicts and difficulties of the linear career were less likely to become concentrated into one or two career stages; rather, there was a constant juggling of narratives and trajectories.

It is clear, however, that linearity is gendered. It originates in the apparently neutral but actually masculine tradition of the full-time, unbroken career with someone else responsible for children and

domestic arrangements. This version of the ideal career path sits in tension with changing social norms and gender roles at work and in the private and family sphere. The demands and consequences of framing excellence solely in relation to the individual and linear career form part of the ongoing gendering of work in research organisations. The coherent, unbroken, concentrated career trajectory forms a masculine norm against which other alternatives are simultaneously feminised and undervalued. The consequences of such norms are significant not only for women's career prospects, but also for men who do not follow the linear path. Men too (and perhaps especially younger men) must negotiate the gendered career, and this is not without its own conflicts. The problem of gender and the linear masculine career has not gone unnoticed at the policy level, although here efforts are largely addressed at narrowly framed issues of 'women in science'. In Finland the main national research council (the Academy of Finland) and the Ministry of Education as well as universities have been committed to promote research equality in relation to the provision of maternity, paternity and parental leave through legislation [Ahlbeck-Rehn 2007a]. Over the last ten years, significant policy attention has been paid to women and science careers in the UK, including policy initiatives and schemes to support women returning to science (and academic) careers after maternity leave [Garforth, Kerr 2009]. Staff in a UK bioscience focus group talked enthusiastically about supporting women returners and helping mothers to stay in science – but at the same time they were adamant that the discipline itself moved too fast to make career breaks possible after the PhD stage. These initiatives, then, are best seen as adjuncts or supplements to the taken-for-granted norms of the linear career model. They do not fundamentally challenge the assumption that it should remain unbroken or offer alternative ways of valuing patchworked or fragmented careers. They are compensatory devices, focused on repairing women's 'broken' career trajectories. The masculine norm of linearity remains invisibly connected to excellence.

2.3. Articulations: individual and collective trajectories

In this section we turn to the ways in which individual careers are conditioned by institutional, national and disciplinary factors, played out through very specific local circumstances. Collective narratives play a significant role in shaping and reshaping epistemic life-spaces as clock time and historical time progress. Recall Jan's story, above. It is typical of the ways in which the linear, vertical shape of the career in the biosciences is deeply entangled with norms and expectations around mobility in the post-doctoral phase (see Chapter III of this

volume). For researchers from smaller countries (and it was the case of all consortium countries except perhaps the UK), this demands not only institutional but also international mobility. In many cases a post-doctoral fellowship abroad constituted an obligatory passage point in establishing a research career. Mobility, however, is not only a spatial issue, or one of community and togetherness, as explored in Chapter III of this volume. It is also a temporal matter – particularly in relation to how questions of moving away from institutions are caught up in anticipatory trajectories of returning, as in one of the labs in the Slovak study. PhD holders were expected to go abroad but, crucially, to come back – unlike in the UK and Czech contexts. In Slovak bioscience, the mobility and exchange of post-doctoral researchers was seen as crucial to the department's own forward trajectory, which involved forming strategic alliances with others in the international scientific community. In the Austrian context, return trajectories were more problematic. Conditions had recently changed, leaving few permanent lab leader posts available for post-docs returning to their home country. In the past, researchers had returned after one or two post-doctoral contracts; now they needed to stay inter/nationally mobile for longer. In the Slovak example, individual and institutional trajectories were tightly knitted together; in the Austrian case, they were beginning to diverge. Relatively stable narratives connecting the past and the future were in flux, with potentially significant consequences for researchers negotiating epistemic living spaces in the present.

As the discussion above suggests, establishing a career trajectory does not take place in a vacuum. Policy documents and discourses tend to construct imaginaries of individual career trajectories in reified and abstracted terms, paying little attention to the contextual and relational dynamics of knowledge work. Our findings show how making a career depends upon becoming part of a specific knowledge community – growing into a field or discipline, and negotiating epistemic life in particular institutional conditions. It means managing and narrating the forward-looking lines of one's own career in relation to other, collective, trajectories. At the macro level, time regimes in knowledge production differ fundamentally between disciplines. The Austrian findings highlight some key differences in this respect. Bioscientists mainly gave trajectory accounts that stressed their early ambition or even predestination to become scientists; thereafter things were thought to flow naturally [Felt, Sigl, Wöhrer 2008b]. Social scientists, on the other hand, almost exclusively told stories about slowly growing into a field, and their struggles to become researchers. In the bioscientists' anticipatory accounts, the future was explicable with respect to the present. In the

social scientists' retrospective accounts, the present was explained with reference to the past. These contrasting trajectory narratives suggest the very different conditions of careers in the two fields, including the spatial and organisational structures of research in the two disciplines. The UK findings have explored how biologists became practically and physically embedded in research teams from a very early stage of their studies (even from undergraduate final year project research), and at each stage usually worked closely with a researcher at the 'next level' up. This exposed them to a high degree of awareness of the shape of career paths in the field [Garforth, Kerr 2008]. Social science PhD students, by comparison, typically lacked strong professional and interactional (but not epistemic) connections with more senior staff and with those at the next career stage, an observation also made in the Finnish study [Ahlbeck-Rehn 2007a; Ahlbeck-Rehn et al. 2008].

These organisational elements of the varied epistemic living spaces we studied are crucial to understanding the social factors that condition individual career trajectories. The relational elements of the career trajectory condition contrasting experiences of becoming a researcher in the two disciplinary areas. These informal, experiential elements of trajectory narratives often contrast with the formal stages of the linear career imagined in policy and institutional discourses. The Czech data shows how in the biological sciences, the processual aspect of becoming a legitimate and competent epistemic subject was often framed in terms of learning craft and technical skills, rather than in relation to intellectual development. This meant that ownership of projects and research independence was deferred until later in the career – lab leaders set projects for junior researchers, who were not allowed to publish independently. By contrast, we found that from a very early stage social scientists were framed as independent epistemic subjects, not least because in the main they chose their own research projects. The Finnish team observed that a social scientist with a Masters thesis is already considered an expert and a qualified teacher, whereas a fresh post-doc in the biosciences is not considered a really competent professional yet [Ahlbeck-Rehn 2007a; Kovalainen, Poutanen 2007]. At the same time, the ambiguous and often patchworked nature of careers in the social sciences meant that some female researchers expressed anxiety over never truly feeling like legitimate experts or epistemic subjects; a British social science lecturer still felt inexperienced despite being established in her career trajectory, for example, and UK contract researchers expressed anxiety at the constant “deskilling and reskilling” involved in keeping on top of their work [Garforth, Kerr 2008: 38].

Disciplines and their trajectories, however, are not generic virtual communities, but ideas and ideals that are performed and reproduced in specific local conditions – in departments and institutes that are also shaped by distinctive national cultures. The complex entanglement of individual careers, institutional locations, disciplinary imaginaries and national politics can be seen particularly clearly by exploring an example taken from the Slovak KNOWING study, which also illustrates the importance of (imagined) futures and (narrated) pasts in shaping epistemic living spaces in the present. The laboratory under study was part of a faculty of biosciences, and the narratives of each, as depicted on their websites, differed substantially. The faculty website told a story of gradual development and the differentiation of disciplines since the 1940s. This is a conventional story of uninterrupted epistemic and institutional continuity. Historical changes were rarely mentioned. The narrative of the laboratory, in stark contrast, told a story of disruption and discontinuity shaped by historical and political events. The laboratory narrative also emphasised the centrality of an individual figure in its trajectory – Professor D., an outstanding scientist and the founder of a scientific school in the 1960s. In the 1970s, in the times of so-called ‘normalisation’ (see footnote 5 of this chapter), the community of scientists who formed this school was politically dissolved and its members were forced to leave the university. They were dispersed into various research institutions, and Professor D. worked during the 1970s and 1980s in medical and later in research settings. He later obtained a research post at the Slovak Academy and, after the political change in 1989, he went back to the university. He brought with him some of his fellows from the 1960s, as well as descendents who had cooperated with him after he had left the university. The contrasting narratives illustrate starkly how epistemic communities construct continuing trajectories in the face of organisational and political disruption, *and* how individual careers and group trajectories are powerfully shaped by local events.

What both narratives have in common, however, is the importance of *tradition*. In the laboratory narrative, what mattered was the restoration of tradition in recent times following the period of rupture, dissolution and dispersion in the 1970s. As one of the lab members recalls, “(w)e were striving to bring back to life this good tradition ... But to restore this interrupted tradition is not so easy nowadays” [SK_Int_BS_m]. Tradition was presented as the key that creates the successful present and

53 The material below regarding the Slovak bioscience institutions was prepared in collaboration with Mariana Szapuová for the Slovak team.

orients towards a stable future, as this Slovakian researcher explained:

“In Slovakia the main handicap is that we have no strong traditions in science. We have, let’s say, the school of professor D., but in comparison to other countries there is a lack of traditions. Our chance now is to overcome this big temporal gap.”

[SK_Int_BS_m]

The notion of tradition was established retrospectively, by looking back at the past. As Czarniawska [2004: 774] reminds us, “(i)t is the ending that chooses its beginning, not the other way around. Different endings require different beginnings – this is how the construction of a story proceeds”. In this Slovak case, the construction of tradition mobilised two key elements: excellence and continuity. Tradition was linked to excellent science in the past that was internationally recognised. This success was held to stem from the creation of a specific approach, from the ability “to decide on a way of approaching a problem that is not taken by others”, as one lab member puts it [SK_Int_BS_m]. The story of the past, of the good tradition, was evoked and mobilised in the establishment of a new laboratory in the early 90s, establishing a ‘tradition’ of success. But the stories told by these Slovakian researchers also emphasised a different kind of continuity, embodied and mobilised through direct interpersonal relationships between generations of researchers. Tradition here was framed as a way of thinking that is passed down from more experienced colleagues to less experienced ones. It depends on the apprenticeship of younger colleagues, on a collective trajectory that is continuously reproduced. Although Professor D. was regarded as a uniquely important figure in this scientific school, without the colleagues and students that shared the collective identity, the tradition would not be constituted (and would be interrupted):

“This heredity in science is operating in working groups where there are people who are creating and maintaining a tradition, who let’s say had good teachers and these had also good teachers, good scientists of course ... and this had a decisive role in my case, and I think not only for me but for my younger colleagues here too, to science”

[SK_Int_BS_m]

The rhetoric of the decisive role of teachers as role models was vividly present in the bioscientists' narratives – the motivating factors for making the affirmative decision on whether to embark on a scientific or academic career or not are teachers who are “zealous scientists” – “that decision always depends on who you get to, to what teacher” [SK_Int_BS_m].

We can see from this story how successful individual careers are not simply a matter of following a prospective linear trajectory, but can be related to established research programmes and to visionary lab leaders via the establishment of inter-generational ties. The context of the Slovakian story is highly specific: in the period of normalisation, scientists were under more scrutiny at the university (where they were training students) than at the Academy of Sciences, and subject to explicit political constraints upon scientists in terms of ideological loyalty and national publication outlets dominating over international journals. But this very particular example is important as it highlights the interweaving of institutional, disciplinary and historical/political time in a way that is common to all epistemic living spaces. These dynamics, however, may be less visible in Western labs where confident forward-looking narratives seem to dominate.

The Slovakian story is also valuable as it illustrates tensions between looking forward to dynamic (but uncertain) futures and finding strengths and certainties in narratives of tradition. On the whole, the dominant trajectory narratives of institutions in the biosciences tended to look forward to the promising and exciting future opened up by new and innovative areas of research, as the Austrian study found. Backward-looking stories that valued tradition and established fields of research were more prominent in the social sciences. The Austrian research found that social science departments based claims for more prestige, influence and funding, and for less competition, on disciplinary traditions rather than on future promises. In the Czech social science research site, strong notions of the tradition of a particular sociological school, established in the inter-war period, were in circulation. The department's website prominently featured a picture of the founding father of this school – although later this was complemented by new images of societies in flux – and photographs of former department heads and canonical sociologists (all male) lined the corridors. But this binary contrast between the biosciences and social sciences tells a partial story. At most sites, tensions between tradition/retrospective narratives and future/anticipatory narratives were at play. This was particularly evident in the Finnish social science research institute. Here, the department was officially presented on its

website through a discourse of history and tradition. But the shared collective narrative of its members and students was based on rhetorics of the dynamic future orientation of the institute, constituted out of a number of elements. Firstly, the unit saw itself as having established a strong international focus in its research, before this was commonly adopted in Finnish social science; it was ahead of the game in terms of global excellence trajectories. Secondly, the members of the Institute portrayed themselves as possessing particular expertise in social sciences which, they argued, would be much in demand by the new EU member states, which represented new scientific markets. Finally, however, the continuing relevance of this expertise was predicated on the persistence of social problems, which in the end would guarantee the continuity of the research unit [Ahlbeck-Rehn 2007a; Kovalainen, Poutanen 2007].

At the Czech KNOWNG research sites, tensions between tradition and anticipatory narratives of the future had consequences for researchers' life-worlds and career prospects. In both sites – bio and social sciences – there had been a recent period of institutional change. In the social sciences, these were connected to massification, increases in staff numbers and the establishment of a research centre; in the biosciences they related to a restructuring of the institution and the development of new evaluation criteria. In both cases, these disruptive changes stimulated new narratives told in terms of before and after. In the bioscience institution, anticipatory narratives looked forward to a new level of international excellence that might be achieved through supporting the independence of team leaders, rigorous programmes of internal assessment, and a newly competitive regime. But alternative narratives circulated in both sites. In the biosciences, these emerged particularly strongly in a focus group with female staff expressing criticism and anxiety over the loss of collegiality and the introduction of new administrative responsibilities. Stories in the social sciences about the time before the department's enlargement and transformation provided a source of identification with institution. An intimate and cooperative atmosphere was invoked as the school's founding father spoke of trying to preserve that space and scale in the face of new pressures. These backward-looking narratives recall Ylijoki's [2005] analysis of nostalgic narratives in academic settings. She argues that Golden Age retrospective stories should be read as a sign of "current tensions and dilemmas in work" [ibid.: 555] as researchers try to find a balance between emerging entrepreneurial values and traditional norms and morals associated with academia. For example in the Austrian case, social scientists mainly located the Golden Age in the 1970s and early 1980s. They referred to this period as a time when more jobs were

available in the field, with more stability and security for academic posts, when the social sciences were more socially valued, and when the discipline had a better position in the university. In the Czech bioscience context, however, these nostalgic stories were complicated by the perception of some junior staff that before and after stories could also function to maintain invisible ties that privileged continuities for those who had lived through the change, and undercut the possibilities of ongoing transformations, including new career opportunities.

The change in the Czech social scientific institute had produced large changes in staff composition and numbers. Graduate students had taken on lecturing roles and research projects, and even unit head positions, while continuing their PhD studies. This had complex consequences for individual careers, expressed in numerous narratives of multiple and contested identities. In conclusion, we include this example to illustrate how different career trajectories can appear on the ground, compared to the policy imaginary of the linear, standardised path that we introduced at the beginning of this section. In the Czech social science setting, career stages that are supposed to be successive were being enacted as simultaneous, even patchworked. The straight line of the excellent career path appears very differently when it is (re)situated in the context of retrospective biographical narratives, the breaks, ruptures and reinventions of departments and institutes, in relation to the rising and falling of particular research fields, and the tensions between futures and pasts played out in collective trajectory narratives.

3. Everyday time

3.1. Introduction

“Trying to fit everything into your day ... it’s like a parcel that you need to pack.”

[UK_Int_BS_f]

“When I’m lucky, I am somewhat in time ... But rather it is typical for my work that I always have too much and that I’m never done, and that always something new turns up ... Mostly I come home with a bunch of work that necessarily needs to be done the same day and it is never finished. That’s typical for science.”

[AT_Int_SS_f]

In this part of the chapter we take up a different temporal frame: time as it is lived and organised in the everyday. Our emphasis shifts from the linear, narrated movements through past, present and future that cohere into individual and collective trajectories to the messiness and multiplicity of everyday temporal orders. In our concluding discussion, we will examine more closely how trajectories and everyday times are related, asking in particular how and whose everyday times add up to recognisable science careers. Here, however, we focus on how researchers make and manage quotidian time in their work of knowledge production – not only in laboratories and offices but also at home and beyond. We consider how they ‘pack the parcel’ of their everyday work, and how that work carries over after the end of the working day. This section foregrounds the ways in which national teams’ data speak to forms of *practice*, both epistemic and organisational, drawing extensively on findings from our participant observational research alongside interview and focus group accounts.

One historically dominant image of scientific knowledge has been that of timeless and universal reason. “Science ... has been historically and traditionally identified as a special time- and space-transcending enterprise” and endowed with “the special privileges and powers of universal truth” [Pels 2003: 9]. Another is that the building of knowledge proceeds progressively in a smooth, cumulative line, or is only occasionally disrupted and re-routed by paradigm shifts [Kuhn 1996] – another linear trajectory. Contemporary STS approaches to science as practice, however, undermine claims to the disinterested accumulation of knowledge. By introducing power, politics and the social into the processes of knowledge production, the multiplicity of its temporal and dynamic aspects are addressed. As such, time is present in science studies, but it is often implicit [Wyatt 2007]. An important exception is in Pickering’s [1995] account of the “mangle of practice”, in which time is a key element of how knowledge claims are shaped out of plural, routine and contingent sequences of activities. Knorr Cetina’s work [2001] draws attention to the distinctive constraints and possibilities of different epistemic cultures, and opens up the possibility of exploring how varied temporalities come into play via the particular assemblages of machines, materials, methods and scripts which produce knowledge claims in different disciplines and sub-fields. Simultaneously, everyday knowledge time is shaped, negotiated and accounted for within organisational contexts. Menzies and Newson [2007] argue that higher education institutions, research institutes, funding bodies and policy-makers are all involved in the production and maintenance of knowledge landscapes and timescapes. But such timescapes are not themselves static

and stable. Within institutions, ordering itself is a temporal process, one that is heterogeneous and dynamic, as Law argues in *Organizing Modernity* [1994; see also Chapter II of this volume]. Rather than seeing the organisation of knowledge production in terms of frozen structures of power operating on a single principle, Law's framework allows us to recognise a range of modes through which the micro-ordering of epistemic communities and practices takes place. Universities and research communities produce and reproduce order around different dynamics which have heterogeneous timings. These modes of ordering are relevant to understanding how both managers and researchers organise and account for their own time structures and how, in doing so, they can both reproduce and destabilise temporal regimes.

Knowledge making, then, takes place within embedded and embodied time cultures. These time cultures appear to be undergoing important shifts at the present (historical) time, and we focus on arguments relating to the new time-regimes of academic life in section 3.2 below. From the late 20th century onwards, it is argued, academic research organisations have been intensively restructured in the pursuit of individual and institutional excellence and internationally competitive knowledge economies. This points to new timescapes in research which are argued to bring new and often punishing disciplines to bear on doing knowledge work in the day-to-day, overloading academics with excessive workloads and leaving 'no time to think.' We begin this section on everyday time by exploring arguments around *acceleration* and *overload* in relation to our findings. However, we go on to show that making and finding 'time to think' in research settings is more complex and multiple than these arguments suggest. Negotiating time to think is not simply a matter of arranging and rearranging the hours of the working day, but is deeply implicated in the experience and performance of researchers' life-worlds and identity practices. As such, 'time to think' (and time *not* to think) cuts across conventional boundaries between work and life as they are imagined in policy. In section 3.3 we go on to explore this problematic boundary from a distinctively temporal perspective. We show that reconciling work and family life cannot be achieved by enabling or entitling knowledge workers to allocate their everyday time quantitatively between these two 'separate' spheres, or by encouraging 'flexible' work practices. The detailed exploration of our findings in relation to everyday time reveals that lived time is both gendered and *gendering*. Time making and time ordering (re)produce gendered subjects in relation to invisible masculine norms of epistemic life and academic institutions.

3.2. (No) time to think?

Acceleration and overload in academic cultures

“I think it’s fair to say that the women in the department do a whole lot of invisible caretaking which frees up the time of these men in the department ... (There’s) something about (women) not allowing ourselves to do that. Not being ahead of the game and saying you want to protect time and absenting yourself to do that ...”

[UK_Int_SS_f]

This quote, from an interview with a lecturer working in a social science department, indicates some of our key arguments in this section. It suggests that time is seen as a scarce resource which can generate conflicts and inequalities; that everyday time is gendered and political; and that accelerating pressures to be, in the terminology of the UK Research Assessment Exercise, “research active” and build a successful career necessitate constantly staying ahead of the game. A wealth of recent arguments have suggested that academic cultures are increasingly being reshaped by work *overload* and by the *acceleration* of knowledge work. In Adam’s [1994] terms, these constitute analytically separate but experientially related problems of work *timing* (the co-ordination and fitting together of diverse tasks within limited timeframes) and *tempo* (the speed, pace and intensity of work activities and organisational change). Science and universities have come to be colonised by accelerating network times of connectivity and instant circulation of information [Hassan 2003]. The massification of higher education increases the teaching workloads of academic departments, often without the provision of extra resources to cope with higher student numbers. As funding sources for research across Europe shift from continuing block grants to responsive project modes, the work of producing knowledge must increasingly submit to contractual and managerial logics involving more work and multiple deadlines – Gant chart schedules, work-packages, outputs and deliverables [Nowotny, Scott, Gibbons 2001]. There are additional pressures from the introduction of new management cultures of audit and performativity [see Strathern 2000; Power 1999; Lucas 2006; Morley 2003]. On the one hand, administrative tasks related to the documenting of and accounting for time and activities proliferate, while on the other there are various arrangements in place for periodic or ongoing assessments of institutional and individual performance relating to output and publication (see Chapter II of this volume).

The consequences of these new conditions of overload and acceleration mean, it is argued, that there is ‘no time to think’ in cultures of knowledge production [Menzies, Newson 2007]. Similar conclusions were drawn by a major UK report on the health and value of the social sciences in Britain, where the emergence of an “academic treadmill” was noted, leading to “less and poorer research” and stifling the creation of “new ideas” [Commission on the Social Sciences 2003: 82]. The tempo of everyday time speeds up and there is too much to do in any given working day, week, semester or year. Hassan [2003: 239] decries the “abbreviated thinking” that has emerged in research contexts as a result; Ylijoki [2005] notes the proliferation of nostalgic narratives circulating in academia which seek to keep alive the possibility of lost forms of slow academic time and absence of pressures in the present. Pels [2003: 2] similarly emphasises the “stress and haste” that are increasingly imposed on science and research by “external” pressures and incentives such as user relevance, managerial efficiency, cost-effectiveness, employability, benchmarking, league-tabling, and audit accountability”. The cry of ‘no time to think’ has two sets of implications in these literatures. Firstly, it signifies a deep concern over the loss of slow times for reflection and immersion in epistemic work – what Urry [2000: 131], in a rather different context, calls “dwelling” – a “staying with things”. Menzies and Newson identify the loss of time for being present to oneself and for dialogue and collegial conversations, for deep connections with others and with ideas and texts [2007]. Pels [2003] argues that these fast times represent a threat to the distinctive chronopolitical register of science itself; that unhastening is precisely the mode of knowing that sets academic research apart from politics and business. Secondly, ‘no time to think’ signals anxieties over a loss of autonomy, compared to an ideal of professional independence and trust in cultures of collegiality [see for example Ylijoki 2005]. Concerns over the loss of autonomy seem to be in part related to the rise of new and intensifying cultures of managerialism and audit [see for example Morley 2003]. In short, in the new cultures of academic life, time efficiency and accountability have come to dominate over time reflexivity and autonomy [Menzies, Newson 2007; Nowotny 1992: 444-445]. This shift appears to threaten ideals and disrupt norms that have traditionally been seen as operating at the very heart of academic knowledge production.

A number of factors are at play in creating ‘no time to think,’ and the pressure to publish appeared prominently among them for participants in the KNOWING studies:

“If you are doing research within hot topics, it happens quite often that your results are being published by others. Ten other people in the world work with exactly the same topic, and somebody else can publish the results first. Then your paper is worth nothing. You do not have to be only smart and efficient, you have to be fast.”

[FI_FG_BS_f]

The theme of competition and the need to be fast and be first was particularly marked in the biosciences. In a focus group in the UK, these pressures on everyday time were explained in terms of the fast-moving pace of the field itself, which was characterised by some as a constant treadmill [Garforth, Kerr 2008]. For many researchers, being able to publish ‘in time’ and stay ‘ahead of time’ meant intensively managing plans for project work and grants in the present in order to be able to guarantee outputs in the future. A particularly striking example of this can be found in a Czech focus group discussion with women researchers in the biosciences:

“A: I would say that grants are awarded for what is half-ready, because otherwise it would not be possible at all, it would be against the sense of the philosophy of the grants ... It should be for something completely new but you can't do it that way.

B: I have exactly this experience because I submitted a grant and after one round of resubmission I got it, for something I started working on a long time ago and it's absolutely impossible to manage to do it in three years.”

[CZ_FG_BS_f]

The sense that everyday time was characterised by a constant sense of “lagging behind” in terms of publication output, articulated by one participant in an Austrian social science interview, was pervasive [AT_Int_SS_f]. Failing to keep up with the accelerating tempo of publication expectations, driven by the need for career-building and/or living up to audit and assessment, was thematised particularly intensely in the accounts of women and early career researchers. As we saw in relation to career trajectories, narratives of broken or disrupted careers disadvantaged women in relation to organisational processes and reinforced the invisibly masculine norms of the linear career. At the level of everyday time, this translated into accounts of an ongoing struggle to

keep up with producing papers and attracting grant funding:

“I have the feeling: OK, I cannot invest any more time than I actually do and I have two children ... I am at the outer limit and cannot publish more, I just can't manage to do more ... this wheel of publishing, this application writing, it is like the sword of Damocles hanging above young researchers”

[AT_Int_SS_f]

Researchers in both the bio- and social sciences also referred to different kinds of publication in ways that implicitly constructed ‘proper’ academic publishing as something in which slow daily time must be invested. In the biosciences, this might be by making a distinction between producing an output quickly, however small and limited, in order not to be outpublished, which was contrasted with fully scholarly articles. In the social sciences, contrasts were drawn between applied contract research outputs and academic publications. In an interview, a social science lecturer talked about the demanding tempo of producing a piece of government-commissioned research. She felt she was pressed to speed up and meet deadlines set by civil servants, who were themselves rushing to meet the targets of government ministers, who in turn were being driven by a government legislative agenda. This research project would result in her co-authoring an international report. However, this was a publication from which she felt alienated, “(b)ecause it was too intense, too fast”. She compares this to another publication that she had written from a previous research project when she was “given the time to think and absorb myself in the analysis and to come up with some useful and novel findings for people. That to me is what research is about” [UK_Int_SS_f]. This lecturer imagined writing a similar “good quality piece of work” from the government-commissioned research project, but during the observation period it became clear that this journal article had been endlessly deferred as routine teaching demands and administrative roles grew more pressing.⁵⁴ The lack of time for immersive reflection and analysis was felt both as a source of personal frustration and anxiety and as a threat to the quality of research findings. This sense of the impossibility of keeping up with desirable publication schedules, because of the inability to find or make time to think about research findings in the context of competing pressures on daily time

54 Ylioki [2005] discusses similar findings in the social sciences.

from teaching and administration, was a common theme in our study. It is often noted that academic roles involve a perhaps unusually wide range of disparate tasks, usually summarised in the triumvirate of teaching, research and administration. The conflicts between the different types of work were more strongly thematised in the social sciences, where accounts of fragmentation and multiple time pressures were more common. Czech social science researchers, for example, gave accounts in which ‘time to think’ was impossible during intense term-time teaching and exam periods, and was time-shifted into the summer, which – it was imagined – enabled long periods of immersion in research. In bioscience institutes, by comparison, there were no formal teaching responsibilities, and training for PhD students was integrated into the daily life of the laboratory. However, for bioscientists in university settings, teaching costing time at the expense of research could also be a problem, and there was “a permanent juggling”, as this Austrian bioscientist reflects: “(s)ometimes I think: again a day has passed and I haven’t managed to do a single experiment, ok? And at the same time I am my most efficient worker.” [AT_Int_BS_f] However, Austrian bioscientists were more likely to distinguish between administrative and scientific times and complain about finding administrative tasks overwhelming.

Particularly in the social sciences, the imbalance between teaching and research was emphasised. In some national contexts this may be closely related to the disproportionate impact of the massification of higher education in the non-natural sciences; i.e. the rise in student numbers without any necessary increase of the unit of resource for supporting them. Whereas teaching was necessary and time-consuming – and often enjoyable and rewarding – it was widely felt across national contexts that it counted little in relation to career building and institutional evaluations. These tensions between teaching and research in everyday time have been noted elsewhere as strongly gendered. Etzkowitz et al. [2000] argue that women are more likely to invest their energies in student-centred activities and enact relational identities in embodying the ‘good campus citizen’. Morley has found that evaluation and audit regimes in UK higher education are reproducing gendered divisions of academic labour, in which men more readily engage with the individualistic and competitive (even ‘macho’) demands of research productivity, while women are “responsibilized” for collective teaching obligations [Morley 2003: 68]. As a British social science lecturer commented in an interview:

“It is interesting how the women in the department operate so very differently. They tend to be the ones who are here through the summer vacation even though they’ve probably got kids. They’re the ones who are regularly around when students are worried or concerned. They’re less likely to avoid confrontational difficult department meetings.”

[UK_Int_SS_f]

Researchers also offered troubling narratives in which project times and grant deadlines were reducing the “free space of creative research,” as one Austrian bioscientist put it in an interview [AT_Int_BS_f]. This was particularly the case for contract researchers. In the UK, an experienced social science researcher talked of having won government research council funding for a one-year project. For him this represented a crucial time in which he could undertake speculative and qualitative research, compared to the types of work possible within the short deadlines and pressures of commissioned contract research. This was echoed in the Czech findings, which suggest that new time regimes of acceleration and accountability meant that some researchers felt unable to pursue or were dissuaded from pursuing slow and unpredictable qualitative research methodologies, such as ethnographies, which would not fit neatly into external funding and evaluation deadlines. Similar tensions were at work in the biosciences:

“When I have a fixed-term project position, it is very difficult to follow my individual interests and I am bound to project guidelines, and by the end of the project I am mercilessly evaluated by specific, very strict criteria ... When I am lucky and the project topic is in accordance with my interests – I have a certain freedom to explore things at the borders of the project topic. But my freedom is very limited.”

[AT_Int_BS_m]

These findings regarding constraints on pursuing some topics and methodologies present ironies and contradictions in relation to science and institutional policy, which are also indicated by our findings above regarding putting in grant bids for work that is already begun. It seems that the relentlessly future-oriented focus of excellence trajectories

restricted the epistemic horizons of some researchers in terms of everyday time, and in this way potentially hindered genuine innovation. This might be read in terms of the perversities and distortions of audit discussed in Chapter II of this volume.

Time to rethink?

Our findings, then, offer some powerful suggestions that everyday time is speeding up and becoming increasingly overloaded and constrained by new temporal regimes. The “micro-politics” [Morley 1999] of organisational time play out in conflictual ways: between gendered subjects and organisational positions; across generations; and in relation to the differential material constraints and opportunities experienced by contract researchers and permanent academic staff. Fixed-term project research in particular appears to complicate conventionally-defined academic freedoms to pursue one’s own research interests, in one’s own time, by binding researchers into time-delimited, output-oriented scientific work programmes. However, we also found intriguing accounts of researchers negotiating and managing these new time regimes. In the UK participant observation study, for example, we found a post-doctoral researcher in the biosciences who managed to make time in the margins of a working week dominated by official grant-funded projects. He explained that, in parallel to his project work, he was able to pursue “stealth projects” – new and independent lines of enquiry, generated by his previous experience and current projects which might both benefit his future career and the trajectory of the whole laboratory [UK_FN_SS_m].

This post-doc’s stealth projects direct us to the ways in which arguments about overload and acceleration in academic life represent only one part of the story of changing everyday times. Time to think can be made or recouped within changing organisational and epistemic contexts. Moreover, the idea of ‘time to think’ is not as self-evident as it may first appear. It is notable that claims that there is ‘no time to think’ in academic life tend to come from studies based on interviews or personal experience, and privilege retrospective accounts of changing time cultures over the past twenty or thirty years. As a consequence, these literatures suggest that a resource model of time emphasising instrumentalism and efficiency has replaced an earlier collegial immersive time of thought and study in academic settings. A distinctive aspect of the KNOWING findings is that they are rooted in participant observation studies. This gives us a different perspective on everyday time as *practised*. It allows us to try to capture aspects of daily time in the making as well

as something that participants narrated or described. Thinking about time and/as practice necessitates a reconfiguration of the problematics of ‘no time to think’ and the dominance of efficiency times. If time is, as Adam suggests, multi-directional and heterogeneous rather than linear and singular, then time to think or its absence must be explored across the range of timescapes that constitute epistemic living spaces, and as something that can be made or produced rather than found or lost. It also suggests that what might count as ‘time to think’ is not self-evident but must be explored in relation to the local context and meanings that are relevant for researchers. In the following section, then, we analyse the situated ways in which researchers produce ‘time to think’.

The materiality of epistemic times

In the biosciences, the resistances and rhythms of specific knowledge production practices in different epistemic cultures matter. The practices of everyday time are conditioned by networks involving non-human as well as human entities. Following Knorr Cetina [2001], we think here about how knowledge is not produced by individual minds meditating on a problem; rather, individual and collective human subjectivities form one element of heterogeneous epistemic ensembles which also include materials, machines and methods.

*“Tomorrow morning I’ll be in at 7:30 am to fertilise
(the eggs) whether I want to or not.”*

[Int_BS_m]⁵⁵

*“You set up an experiment, spend four hours here and
then you go to look at the results and the machine crashes.”*

[UK_FN_BS_f]

As these quotes suggest, the materials and machines at the heart of experimental sciences impose their own tempos and timings. These might be the routine demands they make on experimenters, as in the first example, where delicate and perishable animal tissues demand care and attention according to schedules that cut across the desires and preferences of researchers. Or machines and materials might introduce unpredictability and a slowing down into the everyday routines of researchers by breaking down or refusing to cooperate.⁵⁶

55 This quote has not been given a national identifier in order to preserve the anonymity of the research participants.

56 It was notable in one of the bioscience field sites that it was in these kinds

In one of the Czech bioscience research sites, researchers worked with mass spectrometers. When these machines were defective, researchers had to wait for replacement parts (which came from Western Europe), sometimes for weeks. There were instances too where human agents in researchers' extended networks delayed publication and the completion of projects, and occasioned the starting of new ones, by not sending the required revisions/additions for an article. Juniors reported being highly stressed by these events (particularly when they coincided), since they found it difficult to switch between completely different research projects. Researchers often commented with frustration on their lack of control over epistemic processes in these contexts. Tensions were felt especially sharply when the unhurried or resistant timescales of particular experimental materials were contrasted with the need for high speed publishing schedules. As one bioscience researcher commented, "(i)t takes a long time to get data from (the materials). You can't do an experiment in a day on embryos because you need seven days to make them in the first place." [Int_BS_m]⁵⁷ Researchers in the biosciences sometimes complained that they worked in "slow fields", citing examples such as 4-6 week plant growth cycles, or the demands of working with whole organisms. In one Czech site, distinctions were made between computational and experimental chemistry; in the former area, computation was supposed to be faster in getting publishable output. Our findings show both pressures towards acceleration around publication and performativity, but also the complex ways in which these are enmeshed with the timescapes of experimental ensembles.

But as well as being experienced as frustrations or constraints, the temporal demands and unpredictable refusals of materials and machines were normalised as part of the standard routines of laboratory science. Bioscience researchers' reciprocal interactions with the non-human elements of their networks sometimes had perhaps surprising consequences in terms of producing time to think. On the one hand, the sudden failures or complications of machines could introduce unexpected free time into a working day or working week, offering time for reading, planning and other types of analysis and writing, as when a key piece of equipment malfunctioned in one of the UK lab sites and the post-grads left the lab to "go get some reading done" [UK_FN_BS]. In the Czech bioscience site mentioned above, the operating

of circumstances – when seeds did not germinate or incubators became infected – that researchers were most likely to imbue non-human entities with stubborn or even malign subjectivities.

57 This quote has not been given a national identifier in order to preserve the anonymity of the research participant.

programmes of the mass spectrometers regularly crashed, albeit on different schedules. It took up to 30 minutes for the programme to come up again. Although researchers expressed their frustration with these delays, the time was used for writing time (e-mail, articles) – or to answer the KNOWING researcher's questions. On the other hand, experimental and preparation work itself could be seen as a site of certain kinds of thinking and reflection. Routine work, such as preparing in vitro solutions or separating seeds from plants, demanded lots of fine embodied skills but also allowed for relatively undirected reflection. Other types of practical work, such as inspecting changes in plant morphology, were valued as a distinctive kind of focused thinking that complemented formal analysis and writing. In the Austrian context, some of the bioscientists talked of routine bench work as “casual time”, a welcome alternative to complex thinking tasks [AT_FN_BS_f].

Here, then, we begin to get a sense of how the notion that research and knowledge production are inexorably speeding up is complicated by practices on the ground. Materials are resistant, and have their own time-cycles, as do machines, which also break down. The experimental ensembles of knowledge production have the capacity to slow down and multiply everyday epistemic times, and also offer quotidian times for immersive, reflexive thought, albeit embedded in practical, instrumental routines. Focusing on everyday time as *practice* then, in a very modest way, supports Adam's [1994] arguments about the multiplicity and multidirectionality of time; that is, researchers are rarely doing one thing at once. Rather, we might see them as engaged simultaneously in producing a range of different timescapes, running at different tempos and demanding different timings.

Time to think?

We have seen in the section above how *time* to think can be found and made in contingent and practical situations. The complexity of everyday times and their embedding in a range of epistemic activities also draws attention to some of the problematic assumptions underpinning arguments that there is no time to *think* in academic life. The activity of thinking encompasses a range of cognitive and practical processes and, crucially, these are shaped by different epistemic, disciplinary and organisational cultures. The existing literatures on ‘no time to think’ in academic life tend to suggest that what has been lost is a particular and self-evident style of cognitive practice – individual reflexive immersion in ideas and concepts. Drawing on findings from the KNOWING project, in conjunction with broader arguments relating to the turn to practice

and science in action in science studies, allows us to open up what time to *think* might mean. For example, bioscience researchers in the UK study did not always make hard and fast distinctions between ‘practical’ and intellectual or analytical work, and were sometimes puzzled by the KNOWING researchers’ repeated questions about the epistemic value of always being involved in routine work at the bench. We argue that thinking is not solely a matter of sustained, uninterrupted periods of freedom to reflect, isolated from other academic time pressures, but rather takes many forms produced across a range of different activities and contexts.

Two further examples from the UK participant observation data help flesh out this claim. Both involve tracing misunderstandings and consequences generated by the process of observational research itself to insist that ‘time to think’ is a problematic and contingent activity. The first example considers what it means to analyse data. During a week or two of observational studies, a bioscience post-doctoral researcher repeated several times that he needed to find time to analyse some data that he had taken from a set of machine-based tests, suggesting that the KNOWING researcher observed while he did so. When he finally began analysing the figures, he explained and showed on his computer screen how this was a matter of systematic interpretation or ‘cleaning up’ the data – checking measurements, removing outlying results, and relating the main statistics to his earlier hypothesis. The data was not analysed on this occasion; the task was once again deferred. From a practical point of view, the post-doc did not have time to actually carry out the analysis, because he used the time to explain what such a process would involve to a social science observer. However, from the point of view of the observer, this was a failed observation in a different way. Trained and socialised into different disciplinary norms, she could not recognise this technical manipulation of data as analytical thinking. This instance of multiple cross purposes reveals the important role that disciplinary norms and practices play in constructing apparently neutral objects like time to think. The second example highlights a dialogical and teaching-driven mode of thinking that is rarely referred to in the literatures on ‘no time to think’ (although it is well rehearsed elsewhere in literatures on the value of pedagogy in higher education). Late in the participant observation period, a social science lecturer shared with the British field observer that she had come up with a “new idea for a journal article” [UK_FN_SS_f]. This was in part an outcome of work she had done producing a new undergraduate course in her department, and in part had arisen as she had reflected on what she had told the KNOWING observer early in the observation period about how, after

taking up a lectureship shortly after a period of maternity leave, she had begun to reconstruct her academic identity in relation to teaching rather than research. The new idea, then, had been constructed out of the materials at hand in a day-to-day temporal flux of epistemic activity – organising concepts for teaching; discussions with a disciplinary peer about teaching and research in academic cultures – rather than out of solitary withdrawal and immersion.

Lab meetings and empty calendars

These examples demonstrate the need to widen and question definitions of ‘time to think’, challenging the idea that thinking is a bounded, discrete and individuated activity, and considering its dispersed, hybrid and collective dimensions. We are interested here in how researchers make time in and out of their everyday rhythms of work for collective routines of thinking, building, and communicating knowledge. This offers a different model of ‘time to think’ than the usual ones of withdrawal and immersion in ideas, books or writing. Across the range of KNOWING research sites, we found that collective routines for thinking together were more unproblematically and unreflexively embedded in everyday practice in the biosciences than in the social sciences. The emblem of this embeddedness is the *lab meeting*, those routine occasions where researchers come together to share their most recent findings, discuss the work of other teams in the same field, and raise shared lab business. The taken-for-granted centrality of such meetings to laboratory life perhaps disguises the array of functions they perform. They not only circulate but build and test new knowledge claims; they provide an occasion for reflection on techniques and methodologies; they build lab group identities and (re)produce relationships within the wider field; and they play a key role in the socialisation and enculturation of junior researchers. As such, they are crucial to building collective epistemic and organisational structures out of the fabric of everyday time. Moreover, they are highly valued by individual scientists, as can be seen in Chapter III of this volume.

The ordinary ways in which everyday time builds into epistemic and organisational processes in the biosciences is perhaps best shown by exploring contrasts with the social sciences. We discuss this below, drawing extensively on the Finnish analysis of a case study of research seminars in the social sciences.⁵⁸ In the Finnish social science department,

58 The material in italics below was authored by Seppo Poutanen and Ismo Kantola, based on original analytical work in Ahlbeck-Rehn 2007b.

PhD students were expected to participate in regular research seminars, both as presenters and as discussants. Graduate students presented their developing PhD work at two sessions – firstly, their research plan, and later outlining their progress. *A single session of the research seminar as well as a series of sessions can be considered nodal points in academic time management. Differing conceptions of time, both ‘objective’ (official or shared) and subjectively experienced, are dynamically and variably brought together in them. There is, however, a structural feature which may significantly determine how a typical seminar session proceeds. Each PhD student can freely choose their research subject and, more importantly, choose a scientific approach from a plethora of social scientific theories, methods, and conceptual framings. Thus very diverse papers are offered for discussion in any term, and a presenter can consider herself lucky if there is someone there who can discuss her paper with deep expertise and real interest. Perhaps this is the reason for the PhD students’ typically quite negative judgements on the research seminar. Participation is more often considered “an obligatory ritual”, “futile”, or a “waste of time” than anything positive.* [Ahlbeck-Rehn 2007b]

In bioscience lab meetings there is an expectation that researchers will share scientific questions, objects and/or methods, and the hierarchical range of researchers from the most junior PhD student to the laboratory leader/professor will be present. These social science research seminars, by contrast, brought together junior researchers across diverse research interests and for many graduate students and staff were experienced as an imposition. *PhD students affiliated with the department as well as other members of staff express their time priorities by stressing that they would rather write than participate in any research seminar session. Accordingly, a potential participant’s absence from a session is understood to mean that the person has something more important to do, i.e. research or teaching.* For others, however, the research seminar was a crucial element of their research routines. *To PhD students who work outside the department, the question of time use often looks the other way round. They must more actively arrange their calendars to be able to participate in sessions that always take place in the early afternoon. These students’ regular contact with the department may extend to nothing much else than attending the research seminar, and they understandably hope that their participation will be time well used.*

In the social sciences, then, research seminars are often seen as added on to the normal practice of the epistemic culture in a much more individuated model of thinking work.⁵⁹ Everyday time is more likely to

59 Interestingly, when a bioscience research seminar, as distinct from a lab meeting, is perceived as not directly relevant to researchers, similar issues of withdrawal and attempts to present such occasions as mandatory follow. This is evident in an

be spent alone, in offices or at home. The ‘empty corridors’ explored in Chapter III of this volume have their temporal analogy in the “empty calendars” mentioned by junior social science researchers, especially PhD students, in the Austrian study. In the social sciences, it seems, unstructured and individualised time regimes are the dominant experience. When social scientists come together to think at research seminars, they are more likely to perform their disciplinary identities in ephemeral and occasioned ways [Garforth, Kerr 2008; Ahlbeck-Rehn 2007b], and retrospectively present knowledge claims rather than enacting shared everyday working relationships and developing work in progress. In this sense, both the UK and Finnish findings suggest that in relation to the routines made of everyday time, the necessity of multi-faceted work teams in bioscience departments and social science contract research institutes make them more alike than either is to a mainstream social science teaching-and-research department.

Exploring collective routines for thinking together, then, reveals yet more dimensions of the problem of ‘no time to think’. As we have seen, in the biosciences regular occasions for the cumulative building of thinking together and working together in the everyday are taken-for-granted and built into epistemic living spaces. Making time to think together is normal. In the social sciences, choosing when and how to think is to a much greater extent at the discretion of individuals. The default setting is solitary thinking work – often undertaken in places other than researchers’ main institutional bases for teaching and administration. Building routines in such spatially decentred conditions means that their logics are more individual than collective. The more individualised nature of their projects means that social science researchers have more academic freedom to command their everyday time. At the same time, as the Finnish team observe, this freedom often means strict self-discipline over one’s time and projects [Ahlbeck-Rehn 2007b, 2007c]. We go on to explore this theme in the following section.

excerpt from an invitation to researchers at a Czech bioscience institute to attend a conference taking place at a related institute: “I assume that all scientists and students (of the institute) will attend the conference; it would be a shame if the room is half-empty. Please do not plan any experiments for Friday, reserve all the day for attending the conference.”

3.3. *Everyday time across epistemic living spaces*

The reconciliation of work and family life

Since the 1990s, 'reconciliation of work and family life' has become a priority in EU policy in relation to changing patterns and demands of work and shifts in family structures and the 'gender contract' (see for example the Resolution of the Council of Ministers for Employment and Social Policy on Balanced Participation in Work and Family Life 2000). A recent European Foundation report [Riedmann et al. 2006] found that a number of factors were driving the need for increased flexibility in relation to paid work, among them economic globalisation, ageing populations, increases in female employment, and Lisbon targets for increased labour market participation. Policies on work-life balance are gendered in their origin, both in terms of their particular focus on how women might combine family and career, and because feminist research and politics have driven commitments to such issues on the European agenda. Framing time solely as a quantitative resource is crucial to the construction of work-life balance issues and policies. Examples of employment policies that highlight work-life times include, for example, the provision of *time out* – career breaks and parental and other forms of family leave; *part-time* working; and recognition and support for various forms of *time-flexible* working: flexitime, compressed working week, job-share, home-working, term-time working and annual hours schemes. In recent years, flexible working times have been increasingly framed as solutions for both employers and employees [ibid.]. Temporal flexibility is held to enable employers to respond more effectively to changing market demands, at the same time as it allows employees to manage work and family life.

On the ground, there are wide variations in national policies on work-life reconciliation due to differences in national policies (taxation, family-friendly social policies, etc) and cultures of employment and care arrangements, especially in relation to childcare [see for example Wattis 2005]. Contemporary work-life balance policies also operate in national and sectoral contexts with very different histories of employment norms and gender contracts. Work-life balance policies were well embedded in some national contexts and were seen as an important way to ensure gender equality in career opportunities. In Finland there are extensive legal entitlements to maternity leave, maternal and paternal rights, norms of relatively short working hours, high levels of State childcare provision. Work-life balance here is a visible and naturalised part of national gender politics, although the invisible and time-consuming work of managing

parenting alongside paid work appears to fall mainly to women. In the UK and Austria, the work-life balance model approximates what Wattis [2005] describes as “supportive rhetoric, limited structure”. In Austria and in Slovakia there are no explicit policies on work-life balance in academia, either nationally or institutionally. In the Czech Republic, limited and expensive childcare was noted as a major issue for women in getting on and staying on the full-time linear career trajectory.

The KNOWING research found changing perceptions and expectations of the gender contract and work-life balance among the younger generation of researchers. The Slovak findings showed that some young researchers, male and female, argued for the possibility of reconciling work and family life as opposed to the dichotomous models invoked by some of their colleagues. Traditional fathering roles were being reconsidered and new combinations of everyday time regimes were emerging in dual career families, with both partners working part-time and taking on caring responsibilities. In Section 2.2 above we discussed some aspects of the tensions between work and family life as they played out over the biography, in terms of trajectories and careers. In this section we focus on how issues of work and family life are worked out in everyday time and across epistemic living spaces. Indeed, the concept of epistemic living spaces is crucial here to challenge the problematic binary implied by policy imaginaries, which tend to operate by constructing exclusive spheres of ‘work’ and ‘private life’ (this is particularly marked in the UK policy terminology of ‘work-life balance’). Our findings strongly suggest that this boundary is routinely unsettled by researchers in their epistemic practices and performances of identity, both in relation to the forms of flexibility and autonomy offered by academic research work, and in relation to the strongly vocational identities and modes of ordering that are hallmarks of academic cultures. We explore these issues below.

Discussions of acceleration and overload, as we have seen, frequently involve the assertion that time in academic institutions has become pressured to the extent that academics have lost a significant degree of professional and intellectual autonomy. However, our findings suggest that in terms of the capacity to organise everyday working times, academic researchers enjoy a perhaps unusual degree of autonomy. This was related to the distinctive spatio-temporal organisation of knowledge work in many of our settings. In the natural sciences, it is true that much depends on particular spaces and proximity to specific objects, machines and materials. As Knorr Cetina argues, “(t)he bench laboratory is *always activated*; it is an actual space in which research tasks are performed continuously and simultaneously” [1999: 37,

emphasis added]. For some participants, long hours at the bench were crucial to achieving anything in their field. By contrast, Knorr Cetina characterises the laboratory of the social sciences as *occasional*, “a virtual space and, in most respects, co-extensive with the experiment ...” [ibid.: 35]. Social science departments, as we have seen, can be spaces of closed office doors and multiple uncoordinated schedules. In both cases, however, and despite the particular demands of bench science, researchers routinely have what the Slovak findings characterise as “time latitude” [Szapuova, Kiczkova 2007]. Although, as we explore below, long-hours’ cultures could be problematic, researchers rarely worked conventional office hours. In the social sciences especially, physical presence in institutional spaces was not required except for specific occasions – lectures, meetings etc – and sometimes at particular times, for example the Wednesday academic afternoons in the Czech Republic. As a tactic for reconciling paid work with childcare, time flexibility was universally valued. Female biologists in the Austrian study argued that scientific work and having children were compatible only because of flexible working times, even if they worked long hours. In the UK context, we found that flexibilities in everyday time were creating (limited) possibilities for renegotiating gender contracts, especially in dual-career couples, as in the following quote from a British social science contract researcher:

“Perhaps the biggest advantage of (this) job is that you just need to do the work, just get it done. Doesn’t matter when you do it ... So that means, for example, if my daughter needs picking up from nursery a bit early I can do that knowing that I’m going to be disciplined and I’ve got to pick up the books that evening, or just work longer another day.”

[UK_Int_SS_m]

However, time flexibility in itself was not enough to tackle deeply entrenched gendered time inequalities in relation to work and caring responsibilities. The long hours culture of academic research was highlighted. Many researchers gave accounts which stressed that women undertook more domestic and childcare tasks than their male partners and that the demands of a full-time job in addition simply meant that there were not enough hours in the day or week; as this Finnish researcher observed, “(w)omen still carry the largest responsibility ... whilst men are much more able to sort of float around in an undefined

swamp” [FI_Int_SS_m]. A female social science lecturer in the UK explained that starting an academic career with young children was unusually easy, because “you don’t have to be present in the environment to do the job ... not nine to five, Monday to Friday”. Difficulties arose, however, in relation to “the need to make up the time in other ways by working evenings” [UK_Int_SS_f]. At some sites, a long hours culture became a kind of presentism that was endorsed by laboratory leaders. In one Czech laboratory, a particular researcher was favoured by the leader because he spent most of his waking hours in the laboratory, producing a lot of results despite not being seen as especially clever. Other members of the lab group attributed this to his personal circumstances – living at home and being looked after by his mother – and felt tensions in being expected to live up to this ideal. If long hours are necessary to scientific excellence, women (or more precisely mothers) can be perceived as unable to enter the excellence zone, as in accounts from biological scientists in the Czech study. The Austrian findings show that women academics felt that the expected number of publications demanded time inputs that were difficult to manage with children. Female researchers in Slovakia discussed the asymmetric divisions of time between work and family, giving accounts in which men had better opportunities to realise their potential in science as they had more time available to them. For some Slovak researchers, gendered time inequalities seemed particularly stark. In order to negotiate time pressures, it was argued, one must choose between children and career. Although both choices were presented by participants as normal and positive, the logic of sacrifice in these accounts was presented as inevitable.

Within the timeframe of the everyday, flexibility and autonomy must also be understood in the context of the increasing importance of audit and performativity in academic life. Or, as a social science lecturer in the UK study commented, “I think one of the things you do have as an academic is flexibility but that means ... it’s output orientated” [UK_FG_SS_f]. There may be a relative lack of external structures in epistemic work, but internalised disciplines exert their own pressures which have the capacity to produce what Shore and Wright [1999] call self-surveilling subjects [see also Strathern 2000]. Many researchers are willing to internalise and comply with accelerating time pressures in order to pursue their careers and live up to expectations of success in competitive environments. The assessment of academic work is increasingly related to quantifiable performance, most conspicuously the number and quality of publications. This logic is reinforced by the cultures of some laboratories. In contrast to the Czech example above, in one UK lab the group felt that the lab leader wasn’t interested when

particular staff were present at the bench or the desk, but made it clear to researchers that “it’s your balance”: “(i)f you don’t work you get nothing and it’s your career. And if you do work you’ll get stuff” [UK_Int_BS_f]. In this particular lab, the group appreciated the “relaxed but hard-working atmosphere” [UK_Int_BS_f]. However, conflicts between the organisational times of ‘work’ and the performative times of success and excellence were felt by many researchers, which played out in terms of acute tensions around everyday time. These impacted particularly badly on people with ruptured careers or who worked part-time, and of course these impacts are still largely borne by women. Participants felt that their outputs were assessed by the standards of the unbroken, male, full-time, vocational career mode. In the UK, recent rounds of the long-established research assessment exercise (RAE) (see Chapter III of this volume) have been redesigned to formally accommodate maternal career breaks and periods of part-time working, but informally and in career terms, women felt that they were nonetheless expected to perform to a different standard:

“I have to produce exactly the same number of publications. And yet I don’t have any more time made available to me within my contract to produce the same level of work to go into the RAE ... there’s also the issue that if you work part-time then you’re assessed for promotion in the same way as somebody that is working full time”

[UK_FG_SS_f]

Academic freedom in the form of time flexibility and time autonomy, then, is not separate from but *part of* forms of self-discipline and responsibility which could be seen, as in this comment from a Finnish researcher, as “very oppressive” – “you’re your own employer and ruler of your own work time” [FI_Int_SS_m]. Time flexibility cannot be understood simply as a solution in the promotion of work-life balance. Whilst flexibility and personal autonomy might enable practical time management across multiple or even competing spheres, they also work to reinforce conceptions and cultures of academic work as temporally unlimited (Finnish bioscientists, for example, discussed norms of working night and day; the doing of research was seen as never-ending). Moving time around (or moving tasks around in time) does not in itself address or explain the ways in which long hours and personal commitments and investments of time in knowledge production can

add up to time pressures and anxieties, and how they play out in relation to the academic career. In order to look more closely at these aspects of knowledge production, we need to move away from quantitative and allocative time models and think instead about how academic identities are lived in everyday time.

Everyday time in the vocational mode

“I think the people who are happiest among us are the people who really love their job and don’t see it as a strain to work on Sundays. They basically love it so much that they don’t think it’s work.”

[UK_Int_BS_f]

“I’m a researcher, it doesn’t mean I’m a researcher at work, doing research, it means that it’s a part of me.”

[FI_Int_BS_f]

Across all the national contexts and the different disciplines in our study, a significant set of accounts of epistemic life stressed that science was not a job but, in the words of one Slovak interviewee, “a vocation with a certain mission” [SK_Int_BS_f]. This vocational construction of identity frames academic knowledge work as a set of personal commitments, a way of life, or a “way of being” [FI_Int_SS_m]. A British bioscience PhD student talked in an interview of the need to “surrender” to science, and many other bioscience participants agreed that love of and commitment to the work itself was crucial to success. Social scientists often constructed vocation in terms of distinctive epistemic lenses or a perspective that could not be removed – research was a way of thinking; sociology was a way of looking at the world. These vocational accounts challenge the easy separation of work and life that frame work-life reconciliation policies. Knowledge production is construed as part of the self, inherent and internalised. For some, therefore, everything is work including, for one Austrian sociologist, “reading the newspaper”. For others, nothing is work: “(w)atch out, I live sociology, which means I don’t work!” [SK_SS_Int_m]

It is unsurprising that as academics negotiate time to think, their efforts and practices are not neatly compartmentalised by clock divisions between the working day and home and family time. They are, however, gendered. In interviews, Czech male bioscientists talked about working

on problems in their private or leisure time – while riding a bike or going for a hike or at home. Women’s narratives, however, tended to stress the interweaving of thinking time with domestic and caring work, for example when bathing the kids. This produced double binds for some female researchers. One social science professor discussed the benefits of working at home. On a good day, she said, you could write some of a paper and do the laundry. But on a bad day, you might be unproductive at both and lose twice. Women’s accounts also thematised the need to find time *not to think*. A Czech bioscientist talked about the need to “switch off for some time and relax”, but added wistfully that “[u]nfortunately, that will probably not be before Christmas” [CZ_Email_BS_f]. Here, the de-localised flexibility of knowledge production work could be experienced as invasive or overwhelming, producing feelings of guilt and anxiety.

Vocation as ordering

Most participants framed their accounts of knowledge work as vocation as a matter of personal identity and individual disposition. However, enactments and discourses of vocation could be considered as a mode of ordering [Law, 1994; see also Chapter II of this volume]. In Law’s terms, vocation is one of a number of orientations to epistemic practice (others include vision, administration and enterprise) [1994: 75-86] which contribute to the ongoing production and reproduction of organisational entities. He argues that apparently stable and singular institutions are in fact effects generated by the dynamic interaction of heterogeneous forms of ordering. A key point of Law’s work is that ordering is always incomplete, partial, in motion – something that individuals are implicated in reproducing, rather than something imposed from above, and something that is temporal in nature. Thus acting and narrating vocationally both derive from and contribute to the maintenance of wider organisational cultures, even as they enable and condition forms of individual agency. The vocational mode sets up norms and expectations around things like long working hours, illustrated in the comment from a Slovak researcher that “doing science implies staying in the workplace from dawn till dusk” [SK_Int_BS_f]. But, as in the following quote from a female Finnish bioscientist, this is often accounted for as an obligation to the self and one’s vocational commitments:

*“It’s not about the amount of time you spend at work
but rather how you feel that this is a vocational job, so*

that the ones who feel that vocation naturally act in a way that meets their own norms ... And again, if you don't feel it as your vocation then you're simply in the wrong field"

[FI_Int_BS_f]

The quote above starkly illustrates the naturalisation of vocational time norms in epistemic life spaces. However, the vocational mode and the long hours culture were questioned by a significant number of researchers. Challenging these taken-for-granted norms highlights the ways in which everyday time can become a politicised object. It is difficult to read this politicisation of time straightforwardly in terms of resistances to organisational cultures. Because of the processes of internalisation and the double-edged nature of autonomy we have observed above, researchers are often fighting themselves. But our findings do suggest that there are spaces for challenging the consequences of acceleration and overload in academic life, and considering alternative ways of managing time. For some, the idea of 'nine to five' office hours was important. Here, an imaginary of strict boundaries between work and other aspects of life seemed to be deployed strategically to manage work demands and pressures on the vocational self. Some Slovak researchers hoped for the possibility of separating work and private life at least occasionally, for example by not bringing work home or not working at weekends. In the UK observation study, one experienced male post-doc, struggling to decide whether to stay in academia, often referred to a "proper job" as he explored whether he would be prepared to trade off his academic identity and enjoyment of bench science for (a hypothetical ideal of) better pay and strict office hours [UK_FN_BS_m]. One Finnish bioscientist rejected outright "the idea that if you're doing research it's demanded of you that it should be your whole life. I think that it can be quite good research even if you work eight hours a day" [FI_Int_BS_f]. Invocations of limited hours and work-life balance are used in researchers' accounts to demarcate other ways of being "very committed to their work ... to high quality output" while resisting the vocation mode: research here is "just work and it's a job. I like to do it very, very well, and the best I can, but it's a job" [UK_Int_SS_m].

Accounts like these implicitly contest the heroic moralising built into some performances of vocational academic identities, but remain on the terrain of vocation or its rejection as a personal commitment. Wider questions need to be asked about whose interests these vocational norms operate in. A related question concerns who has the power to impose

their agenda on everyday time. Two examples from our findings raise this issue particularly starkly. The first concerns an occasion in the Czech observation of a social science department where new labour legislation was introduced regarding extra pay for overtime and weekend work. Lecturers working in distance learning programmes became annoyed because their managers took for granted that they would not ask to be paid properly; they would see such work as part of their professional commitment. The second comes from an interview with a social science researcher who discusses “the traditional academic view that it’s not a job, it’s a vocation”:

“You know, being told (by senior academics) that if you are an academic you’ll be working 60 or 80 hours a week. You do it because you’ve got fire in your belly. All these arguments being deployed to not work nine to five.”

[UK_Int_SS_m]

He goes on to argue that, for the moment the flexibility of research work suited him and he had “the chance to spend more time with my kids”. But he expressed the concern that this situation might not continue because he was not performing a “genuine” – vocational – academic identity [UK_Int_SS_m]. The vocational mode is read as a problematic organisational norm embedded in the practices and performances of senior academics and managers who are able to take advantage of professional commitments and vocational subjectivities. Simultaneously, researchers feel that they carry all the responsibility for their own time management problems and career success. As one Finnish researcher put it: “(y)ou do (research) for yourself. If it all goes to hell then you can only blame yourself.” [FI_Int_SS_m]

If we ask who benefits from the vocational mode, it seems clear that gendered organisations benefit and, in doing so, masculine models of career success are reproduced, based on immersion, the epistemic self, and autonomy without responsibility. Women’s accounts often implied that it was not simply that they could not *practically* commit their time in the ways that men do, but rather that they *experienced* their everyday epistemic lifeworlds in terms of deficit [see Garforth, Kerr 2007]. This sense comes through strongly in the following account from a Czech bioscientist, who discussed how she felt about her partner (also a scientist) regularly working at home:

“This is most often that simply somehow he is working all the time and I am all the time only lazy, either reading something or watching TV or doing something completely not productive. So I have the feeling that I should work as well. And this irritates me, because I don’t want to work ...”

[CZ_Int_BS_f]

This suggests how everyday time and evaluations of commitment are tied together in problematic ways in the ordering processes of the vocational mode. As we noted in the introduction to this chapter, everyday time regimes are not only gendered (in terms of the gender contract and caring/domestic responsibilities in relation to paid work) but gendering. Current arrangements for doing epistemic work and building an academic career in the everyday create female subjects who are in deficit to an apparently neutral but *de facto* male ideal academic worker who embodies and performs an ideal of science as all-consuming and the vocational self as normal and natural. Although we also found evidence of changing gender contracts in relation to work and domestic life, these appeared to be increasingly subjecting (younger) men to the pressures of the double-time-bind of care and work, and putting them in feminised positions of deficit or lack compared to the masculine model of vocation, long hours, and visibly high performance. In the following quote, a female lecturer articulated these conflicting pressures:

“I think part of the problem is ... there is this sort of academic culture, which I suspect men do more than women, of working every hour God sends. And I have absolutely no desire to work every hour God sends. I want to do a 37- hour week ... and if I need to do more than that to get on, well tough. I’m not bothered. I don’t want to get on ... I want to have a life as well. If you look at academics who have got on they’re completely all-consumed by it. And they love it. I find it interesting but I want to have a life beyond it.” [UK_FG_SS_f]

In a similar comment from a Finnish researcher, she insists that “if I had to choose, I would without question leave this and live a life, not bury myself in some science” [FI_Int_BS_f]. In these accounts, the only alternative to unmanageable vocational immersion in work is to leave (or imagine leaving) academia. Work-life is framed as a dichotomy in which life can only ‘win’ at the expense of work. At the level of everyday clock time management, the two can be reconciled. In the vocational

mode, the two are inseparable. In our interpretation of these extracts, a rather bleak and gendered dichotomy is maintained.

4. Conclusion: how do times add up?

We have shown throughout this chapter that the times of knowledge-making are complex and multiple; that they are actively negotiated and managed by researchers; and that they have implications for the ordering of academic institutions. In this conclusion we focus on the ways in which the times of epistemic living spaces are both *gendered* and *gendering*. Time is made as well as taken and spent, and thus time is a crucial aspect of processes of social construction. We develop this argument by looking at how everyday times add up – and whose everyday times add up – to the linear careers of policy imaginaries. We conclude by outlining some ways in which the KNOWING findings on times and trajectories can speak to policies related to gender and science.

In relation to both the linear career trajectory and everyday time, it seems clear that temporal regimes are gendered. The immersive long hours culture of knowledge production in the everyday and the straight, single, unencumbered trajectory of the successful career are deeply marked by histories of male dominance in the academy. But they are also gendering. Time regimes play a part in the *reproduction* of gendered organisations. One outcome of this is that certain career trajectories (or non-trajectories) and ways of finding time to think (and not to think) take on masculine or feminine characteristics. The linear career reproduces masculine epistemic subject positions that are best characterised in terms of their propensity for an intense and narrow focus and an atomistic rather than a relational orientation. This is reinforced by the dominance of vocational modes of living and ordering time in the everyday, which privilege immersive and single – rather than distracted and multiple – orientations to epistemic work and identities. The emphasis in science policy on careers that add up to a coherent and singular trajectory and the emphasis in academic cultures on the indivisible and all-pervading qualities of the academic self present an overwhelmingly masculine dominant ideal and subordinate feminine alternatives are represented and often experienced in terms of lack or deficit. These are cultural, not essentialist logics, and we found that they were operating on sexed bodies in the academy in new ways and against a backdrop of changing familial and gender contracts. Older and more senior female academics often enacted masculine epistemic subject positions, while younger men struggled with or resisted masculine

vocational norms. This points to the importance of multiple and relational divisions beyond gender – generation, age, class and ethnicity – in shaping the unequal academy, although we do not have the space to explore them here. However, our research clearly revealed that the gendered timescapes of academic research were not simply hangovers from the past, but were being dynamically reproduced in changing conditions.

In this context, patchworked and horizontal careers are relentlessly evaluated as distorted or deficient versions of the linear ideal. For some, particularly bioscience post-docs, horizontality is a transient period of the career trajectory (albeit for growing numbers of post-docs, an increasingly long one). It is a period of waiting to see whether anticipatory career planning will work out in practice, and can only be made sense of in retrospect from the frame of a more permanent position (or a position outside academia). But for a significant number of researchers (including contract researchers in the social sciences), the patchworked career is not simply a temporary or inadequate version of linear excellence but rather constitutes an alternative way of doing academic work. In order for everyday time to ‘add up’ to the successful career trajectory, its outcomes must be made tangible, or at least visible, in the form of publications and individual reputation. This logic of academic life – emphasising product over process – has been reinforced and modified in recent years by audit and performance regimes, particularly in relation to quantifying research outputs. Research time that cannot be translated into publication capital, or remains invisible to audit and promotion mechanisms, might be valuable to oneself, one’s peers and one’s students, but it does not count in formal career terms. Women are more prone to invest their time in such invisible work – but increasingly men who also do so find themselves in feminised and undervalued occupational roles. Over and above the implications for individuals’ career progression, cultures of self-discipline and self-surveillance in academic life have damaging consequences. It is not simply a matter of working conditions, but an issue relating to the quality of academic research and of the rational resource management of skilled epistemic subjects.

There are two clear ways in which our analysis of the multiple, complex times of knowledge work speaks to policy. The first concerns *the science career*. We do not offer suggestions as to how the linear career can be made more inclusive and equitable; nor do we follow policy directions in most of the KNOWING countries in asking how women can be helped on to, or helped back into, the linear career trajectory. Instead, we want to draw attention to the plethora of ways in which researchers construct

and manage careers of a horizontal or patchworked nature. There are too many of these researchers, whether passing through a transient career stage or stuck or hanging on in precarious positions, to overlook them in the name of the excellent linear career trajectory. In this respect, we believe that policy-makers and institutions need to consider ways of recognising and supporting non-vertical careers. The second way in which our analysis speaks to policy concerns *the reconciliation of work and family life*. As we have shown, quantitative and allocative models do not add up to an effective mechanism for supporting gender equality in science. This is because the time of science work in the present, and the time that adds up to science career trajectories, is above all *vocational*. The vocational mode involves the investment of everyday time across conventional work-life boundaries and represents a key trope through which biographical and career trajectories are not just articulated but thoroughly entwined. Researchers negotiate vocational time in a range of ways – from welcoming it as part of their performance of identity to rejecting it with recourse to an ideal of limited office hours. But the vocational mode has an organisational and cultural reality too, and here it all too easily becomes caught up with mechanisms for auditing research and disciplining researchers. These impacts fall most heavily on women, and also risk damaging emergent and more equal work/family life contracts for both sexes. It seems that there is rarely enough time in any relationship, especially with children, to support more than one full-time career on the excellent/linear model. In order for work-life balance to be meaningful, then, there needs to be cultural change in the academy in relation to the linear career and the ideal epistemic subject.

In relation to both these issues, one possible answer might be to think about time cultures of knowledge-making in their collective as well as their individual dimensions. We have seen in the biosciences the creative and multiple ways in which researchers find time to think in different kinds of relationalities – with colleagues in lab meetings, and with materials and machines. Our findings from the social sciences imply that there may be parallel potential to think about relational time in teaching and course-making. The main question is: how can these time investments be made to count in valuing diverse contributions to academic organisations, and to resist the gendering of academic knowledge production and scientific careers?

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Susan Molyneux-Hodgson

Science and expert systems are obvious candidates for cultural division; they are pursued by groupings of specialists who are separated from other experts by institutional boundaries deeply entrenched in all levels of education, in most research organizations, in career choices, in our general levels of classification.

[Knorr-Cetina 1999: 2]

That there is a link between the outputs of academic research and inputs to policy processes has, over recent times, become a more pronounced assumption. Upon inspection, the assumed link takes a variety of forms. This brief note sits between the 'findings' and the 'conclusions' of the research work, providing the opportunity to reflect on both the matter of outputs (of research) and the issue of inputs (to the policy arena).

The relationship between research evidence and the policy arena, and indeed our frames of reference for understanding the relationship, remain contested. Social scientific understanding of the policy process has undergone some radical changes. The rise of 'interpretive policy studies' is at the forefront of promoting new ways of understanding processes of policy formation and of policy accomplishment. In this vein, some authors have aimed to move away from policy as a *given*, into which research and other kinds of evidence may simply feed. Rather, a more dynamic and dialogic process can be used to provide better understandings of the complex web of relations between research arenas and policy contexts [see e.g. Hodgson, Irving 2007].

So, where does policy come from? It is clearly no longer just a matter for the nation state. Arguably, the 'State' is no longer the prime actor or necessarily the dominant voice in policy making. Policy is generated, and is accomplished, at many different scales. Our conceptualisation of policy processes, at multiple scales, has advanced hugely in recent times. We no longer accept policy as a straightforward, linear and simple hierarchical process. Policy should be seen rather as a complex of interactions between different levels and amongst a plurality of interested actors. KNOWING research was built within a particular set of policy contexts. As a 'completed project', it now endeavours to speak to many policies and to a range of policy actors.

As stated previously, the character of policy is changing, because the world is changing. Rather than asking whether any given policy is sensible, or works, – as is usually the case in policy studies – we need instead to understand what sense gets made of policy. How is policy *accomplished* at whatever levels it is seen to be operating. This does not mean further elaborations of the gap between policy and practice. Rather we need to aim for fully textured accounts of the dis-assembling and re-making of policy.

Throughout this final account of the KNOWING project, we have seen evidence of these *translations* of policy. From policies that are espoused at European level, to national and to institutional policies – and at various junctures in between – we see how policy statements, intents and imaginations become translated into actions, practices and discourses in different places and at different times. Making links between the situated practices of knowledge work in the laboratories and offices of universities and research institutes, and the policy ‘imaginings’ of those within and outside those institutions, is a challenge enough. This challenge has been met here, analytically, through the weaving of stories of boundaries, time and forms of collectivity.

However, a further challenge for researchers is the disjunction between the language of policy and the language of research. Now that the stories have been woven, how do we display the cloth to others? How does one successfully knit together the micro-level detailed stories of knowing and the institutional infrastructures, Europeanising programme and global policy worlds in which these stories emerge? In particular, how does one accomplish this when these levels are operating with differing logics of storytelling?

Knowledge contexts and cultures will interact with policies in different ways. Elaborating these interactions has been one task accomplished in the KNOWING work. Now that we have read and understood something of the translation and accomplishments of policy, how do we read the stories of interaction back to the policy world? In terms of the analytical categories used here, we need to be mindful that:

- there is little sensitivity or nuance in policy: it tends to disregard existing boundaries and build new ones of its own. Policy makes categories [Britton 2007], which then define what needs to be known and in what forms it can be known;
- time takes on differing meanings in policy and research worlds: demands for knowledge often imply particular routes to knowledge acquisition with implications for the timescales of knowledge production;

- the forms of collective that research shows are important, and are not the same as the ones upon which policy pronounces.

What this points to is the difficulty for research to challenge dominant (policy) discourses. Thus, the KNOWING work needs to stand not merely as a critique of policy but to create new spaces for policy action. It aims to do this by expanding on the texture of the lived enactment of the policies, how they impact and influence scientific work and lives. However, now there is a further translation job to be done. Having resolved how contexts and cultures inform knowledge production (in other words, the enactment of policies-in-practice), what remains is to re-translate the enactment back into policy. Specifically, to work toward policy that centres on what counts, not only on that which can easily be counted.

There is a need for policy to recognise the variety of kinds of life that are contained within the endeavour of science. There is no one kind of scientific career, no one best way to be mobile; no singular prescription for the production of knowledge. With this in mind, we can move to conclude.

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**Creating and Inhabiting Epistemic Living Spaces:
Concluding Reflections**

Ulrike Felt

This book started out to explore the multiple articulations of knowledge, institutions and researchers in changing academic research environments. The analysis took us through different national contexts, research fields, places, institutional structures, social interactions, time regimes, and value systems. It made us aware of the omnipresent, in part contradictory imaginaries about research and the role it should play in contemporary (knowledge) societies. We witnessed the many transformation processes of these imaginaries and accompanying policies into research realities on multiple levels. How researchers encounter, transform and oppose these changes and how they create *epistemic living spaces* sufficiently attractive and spacious for the researchers to feel like inhabiting them was the focus of our analysis. Developing and using this concept made us alert to the social, material and symbolic dimensions of living in research. As outlined in the introduction, it encapsulates dimensions such as feeling intellectually and socially ‘at home’, having an understanding of the often non-codified sets of values which matter, feeling subjected to and performing certain temporal regimes, tacitly sharing a repertoire of practices to tackle knowledge questions and many more. It addresses the intertwinedness of the personal, the institutional, the intellectual, the symbolic and the political. Our concern for epistemic living spaces stemmed from encounters with the transformations of research systems and the profound worries many researchers expressed in the face of them; it was reinforced by the observation that researchers could and did no longer count on simply finding a research environment hospitable to their work, but realised that they needed to create it. They thus need to become entrepreneurial architects managing uncertainties intrinsic to the epistemic spaces they inhabit. To look at their visions, commitments, strategies but also their readiness of personal investment is thus crucial to understand contemporary academic research [see e.g. Shapin 2008].

Having chosen to look at the multiple reordering processes researchers participated in and were subject to, at the ways in which working together and apart structured their lives and how temporal regimes organised both their everyday practices as well as their trajectories through the research system, what can we take away from combining

these three perspectives? Where do we feel that urgent reflection is needed from those making policies on diverse levels and in different institutional settings and who carry the responsibility for the research system that is seen so central to society's future development? Building on the specific conclusions of the prior chapters, we now address more broadly the concept of epistemic living spaces.

For a start, it seems essential to briefly reflect on the approach we have chosen in doing the research and writing this book and the possibilities and limits this brought about. Readers who expected a more systematic comparison of the research systems along a set of predefined indicators in the five national contexts and across the two epistemic cultures might feel disappointed. Indeed, we have deliberately chosen to employ our specific kind of comparative gaze to identify some of the key issues that mattered across countries, as well as to create a feeling for how the concrete implementations of seemingly same policies largely differed. This approach made us aware that, even though efforts for harmonisation in Europe are rather strong and the dominant, quite standardised policy discourses are supportive of the idea of harmonisation, historically grown, culturally rooted and socially entrenched differences persist and matter widely. We could thus link to the introduction and argue with Gieryn [2000] how strongly place matters in shaping research and that these more local, contextual elements have to be treated as an integral part of the contemporary fabric of research. While one could sometimes read them as resisting the change prescribed by policy makers, we could also understand these local forces as assuring diversity and maybe as a contribution to creating a more diverse and sustainable knowledge environment across different national contexts. This also supports the claim that what is needed is not so much a mere focus on the macro-level of research policies, but rather a deeper understanding of their micro-political realisations and articulations.

We had opted for a qualitative social science approach in our research, building on interviews, discourse analysis of central documents, focus groups and ethnographic fieldwork. This entailed making choices concerning concrete institutional settings, epistemic orientations and researchers to inform us. While this meant being limited in scope on some levels, it simultaneously allowed us to enter deeply into epistemic lives and work environments, understanding how they were structured on the micro level and what effects this produced. The focus of our attention was directed at the researchers and how they experienced all this, what constrained, irritated and upset them and what, at the same time, allowed them to feel supported and to live the fascination and pleasure of doing research. Maybe we have not spent sufficient

time in our analysis to stress these moments of engagement, pleasure and fascination which are so profoundly essential as to why and how researchers work hard to carve out their living spaces even in seemingly unsupportive environments, how they structure and 'furnish' them in a way that makes them attractive to remain against all odds and allows for creativity to unfold.

Selecting the three perspectives which addressed the issue of implicit and explicit orders at work in research (Chapter II of this volume), of the multiple ways in which researchers were together while always having to be apart simultaneously (Chapter III of this volume) and of the multilayered time logics which governed researchers' lives (Chapter IV of this volume) allowed us to tackle many of the burning issues researchers shared with us. These three strands largely captured our observations, bringing them together and showing how deeply they were interwoven with each other. Interdisciplinarity, to take but one example, thus was discussed as an effort to dissolve disciplinary boundaries and thereby to break with fairly strong ordering forces in science, while simultaneously it opened up issues of new forms of socio-epistemic togetherness and of time needed to do the articulation work necessary in order to think and communicate across boundaries. This and other cases made obvious how multiple most issues we touched upon are. Our aim thus is to sensitise the reader to the normative forces at work in both contemporary research policies as well as research systems.

1. Normative machineries at work

Throughout all three chapters, the issue of a growing normativity in the research systems runs like a red thread. Be it topics like excellence and application orientation, the growing need to address society, the imaginations of collaborative and individual dimensions of research, the issue of mobility, the projectification of research, as well as time management and career trajectories: all of them display the strong normative dimensions of current ideals of science. They constitute no longer a value system that gently guide researchers in building their own scientific lives. These framings of contemporary research much rather take the form of a mangle which researchers are put through and whereby they have to show their malleability. One could argue against the often encountered narrative of fundamental change by stressing that in fact many of the issues raised are not entirely new and had been around in science for quite a while. Yet, we would respond that what is at stake are not the issues as such, but the ways in which they are addressed and materialise in contemporary research systems.

While mobility, to take but one example, belonged to the self-conceptualisation of research as a non-local activity, it has been transformed into a rigid system of ‘having to leave’ in contemporary biosciences (see Chapter III of this volume). It has become rather an enforced migration that has little in common with the initial ideal of choosing to learn specific things in specific places. Mobility has turned into a matter of strategic choice, into a way of accumulating capital and researchers feel little possibility to escape. This in turn has made life in academia quite complex and ambivalent for some inhabitants. Within the social sciences, for example, remaining local and gathering competences on local issues hardly seems an accepted or acceptable value in research, thus devaluing both this kind of knowledge and its producers. The same holds for career issues, to take a second example. While careers have always been imbued with more or less normative issues, under the new regimes of science, in many countries making a career is equalled with running through a complex machinery whose logic is not necessarily transparent, a feeling in particular expressed by young researchers. As shown in Chapter IV, the ideal of the linear career has a strong influence on who wants to remain/remains in science. Thus, as outlined in Chapter II of this volume, the reorganisation of research also entails new, or reinforces existing, moral orders and thus creates in- and exclusions. Debating excellence in research, we tried to illustrate how normativity on the one hand builds on rather classical value systems (such as highly ranked publications) while on the other hand is accompanied by not very transparent selection mechanisms (i.e. , excellence is recognisable when one sees it; Chapter II of this volume). We thus wanted to make visible the more obvious contradiction that European as well as national policy actors stress the need for more scientists while simultaneously the system expels many – and, we would argue, also very gifted ones – from it, for reasons of their non-conformity to norms. Should policy makers thus not much rather question why scientific careers have become less attractive to brilliant young people, instead of simply investing considerable amounts of money and researchers’ time into selling science as attractive and funny to the younger generation? Shouldn’t we question the idea that creativity is fostered through making researchers yield to the normative imaginaries of policy makers? And shouldn’t we denounce that, while speaking of giving more autonomy to academic institutions and thus more capacity to react and freedom to act, the accompanying audit logic, which is mainly driven by the hope to gain control over knowledge production, in fact profoundly does the contrary.

2. Myths on/in contemporary research

A second line of broader reflection triggered through our analysis is on new and old myths concerning academic research. Indeed, at many moments of our fieldwork and interviewing, we encountered strong nostalgia for the loss of essential elements that for researchers characterised good science and made it attractive for them. Simultaneously they found it difficult to embrace the newly emerging narratives about research and science. We thus witnessed a struggle between new and old myths of science, as well as between the actors (re)producing, upholding or refuting them.

Myths – narratives or related mechanisms that function as sense-making practices – played a prominent role in many ways in our account on epistemic living spaces. As every myth does, they embrace some elements of historical truth and refer to visible reference points, yet with numerous other elements being added [Veyen 1983]. Reflecting the power structure of the system they are part of, their force lies in their capacity for making the beliefs they promote and the values congenial to them seem natural and innocent and thus making also certain actions appear as reasonable [Barthes 2000]. Myths aim at becoming self-evident and inevitable, they exclude other beliefs that might challenge them through their very functioning logic and they conceal certain (social) realities for their convenience. They generally perform a number of more concrete functions: they explain issues and values, making everyday experiences seem rational; they function as a justification of group norms and behaviour, thus strengthening the cohesion and integrity; they provide a causal framework creating coherence between the individual and the collective dimension; and finally, myths function as a means of overcoming apparent contradictions. In that sense they “are much more substantial than mere ‘stories’ – they intersect dynamically with the material institutional, economic, technical and cultural forms of society” [Felt, Wynne 2007: 73].

Science policy narratives can be seen as creating specific kinds of myths. In these conclusions, it thus seems essential to underline that we do not think that they simply represent the world, but that they enable actors to intervene and perform in this world in specific ways. Myths in that sense simultaneously serve many purposes: they are frames of reference, points of departure, justifications and sense-making devices. They become inscribed in and are made durable through institutional practices, routines and taken-for-granted value sets and, in the end, shape social identities of researchers and policy makers alike.

In our research we witnessed a struggle between two larger sets of myths about research and science. One set gravitated around the idea of a 'golden past' and was performed quite densely by researchers across all countries and epistemic fields. There seemed to be longing for this imagined past where there was still much more time to think, careers were smoother, competition less, freedom existed to pick the research that seemed attractive, needs for complex technical infrastructure were fewer, accountability was virtually a non-issue, curiosity was the driving force and not the economy and expectations of applications. There were no rules for the pressured people to be mobile, teaching and research had a more intimate relationship and institutions seemed happy to employ people for a whole career. However, this past appeared to vanish gradually, being replaced by a new set of myths that was judged by most as not a quarter as attractive as the old ones.

The new set of myths that was being put in place mainly by policy makers of diverse kinds embraced elements such as that competition would quasi-automatically lead to more innovation, that mobility would assure quality, that settling down too early would hinder creativity, that careers had to be linear, that time needed dense accounting and management, that knowledge production should be projectified and subject to strict selection processes, that research should be prioritised along societal needs, and much more. Most of our interviewees referred to these new myths as being created and performed in the context of neoliberal imaginations of the European knowledge economy. Yet it is important not to overlook that the attractiveness of such new myths also resides in the strong techno-scientific promises that are embedded in them. A better world for everybody would emerge, economically stable and assuring employment, carried by techno-scientific innovations. Excellent research would be the key to this future.

Thus researchers saw them as mainly imposed from the outside, as new sense-making narratives that should guide their actions and feed their value systems and, in doing so, allow a successful implementation of science policy measures. Thus the 'rhetoric reform' of the science system through the creation and implementation of these new myths was seen as a necessary precursor to a deep structural transformation. As a consequence, many of them encountered the new myths with distance and ambivalence, trying to redefine or adapt them in terms that would allow them to make sense and live with them. Moreover, an important difference could be observed between the social sciences and the biosciences. While the former quite explicitly rejected the use of these new myths for creating a self-understanding and often narrated themselves as being forced into a new logic, the latter would

at least partly adopt the value systems underlying the new myths (e.g. accounting systems for publications). This creates a deeply unequal situation between the two epistemic fields, although policy narratives pretend that their imaginaries of research and the accompanying measures would treat the social sciences and the natural sciences equally. Taking Jasanoff's [2004] concept of co-production of science and society seriously, we actually have to understand these value systems framing the knowledge we produce as intertwined with fundamental choices concerning the ways we want to live in the world. As a consequence, it seems essential to refuse to obscure the choices underlying the contemporary myths of science, and consequently to explicitly pose the question of responsibility for their production, to reassess them and to develop alternatives.

3. Managing innovation and excellence: creativity lost in accounting?

Throughout all three chapters we witnessed that with "managerialism" [Deem, Hillyard, Reed 2008] entering academic research and challenging established values and procedures, accounting has become a key element, not only as a concrete activity, but also as a way of thinking and conceptualising what is done or what should be done. As outlined in Chapter I, it is essential to understand that the power of audit societies lies precisely in representing "ideals of transparency, accountability and managerial willingness to learn" [Power 1997]. It transformed, as we saw, relationships, routines and practices, reducing things that can reasonably count in decision-making to those that can be counted. Quantification was staged as a precondition for good management, as a way to assure just return for high quality work and as a guarantor of efficiency.

Although many researchers make explicit their reluctance to buy into this logic, they simultaneously (have to) comply with it, thinking that any other type of behaviour would be classified as unacceptable and unprofessional and could thus harm them and their work. This would definitely be more the case in the biosciences than in the social sciences, reflecting the hierarchy between these epistemic fields. These changes in the research environment and the moral orders that came along with them were thus starting to be incorporated by researchers, were gradually normalised and more widely accepted as reference frames.

However, throughout our analysis we showed in numerous moments where and how the audit logic created ruptures and caused fragmentation in the research process. Indeed, translation work was needed in order to turn one's work into auditable and thus visible entities and in that

way to be able to survive in the system. Epistemic risk-taking strategies, personnel decisions, career tracks, but also self-esteem became subject to the auditing logic. Without a track record of highly ranked publications, it would indeed be impossible that a person or project could enter the pool of those considered as excellent. To obtain a good post-doc, special scholarships, or an ERC grant, formal prerequisites were required. The temporalising of research employment contracts and their link to ever more concretely defined output – often also summed up under a heading such as ‘human resource management’ – was another way of realising the audit logic. Even the capacity for being creative was mainly attributed to those who had first cleared the hurdle of the accounting system. And here other ordering logics also came into play. It proved not only to be important to publish, but the question was where to publish; it was also important not only to go abroad, but the question was where to go abroad; and it was not only central to be part of a network, but the question was which centres it would contain. In that sense, other orders like East and West started to play out in more or less subtle ways, as did the hierarchies between language cultures (Chapter II of this volume).

As a consequence, anything that was not smooth, linear and corresponding to the accounting ideal, was perceived as distorted and deficient and was much less valued. Thus the “patchworked career” (Chapter IV of this volume), to take but one example, increasingly observed in the social sciences and also in the biosciences, was judged as inadequate and as a temporary possibility at best, instead of perceiving it as an alternative way of living in research. Furthermore, there was no place for people not wanting to make a career in the classical sense and to move up the ladder, assuming leadership functions as soon as possible. Wanting to stay at the bench or simply to do one’s research was not perceived as an adequate behaviour in the system.

Instead of impoverishing our look on research as an activity through the lens of an audit logic, we rather plead for opening up the possibilities of living in research. Even if publishing internationally, mobility or networking are central elements for high quality research, it seems essential to allow for diversity and creativity within these categories and thus also in the way academic living spaces are developed and inhabited.

4. Reassembling gender dimensions

Gender and science has been now on the R&D policy agenda for quite a while. Gender mainstreaming has been a European policy concern, countries and institutions have developed more local measures to

increase the number of women in research, yet we would – on the basis of our research experience – argue that they have profoundly failed to capture and address the gender dimensions at work, not only in the everyday contexts of research, but partly also within the new myths co-produced by policy discourse.

Gender dimensions were indeed omnipresent in our analysis of epistemic living spaces, though often in rather tacit ways, woven into the social, institutional and cognitive fabric academic life is made of. We did thus – as stressed in the introductory chapter – not treat gender as a separate, clear-cut and well-delimited category to be elaborated on each level of analysis. Gender was instead treated as something more vague and indefinite, that gleamed through many of our observations, that was de- and reconstructed simultaneously in different places and at different moments, was imposed, performed or refused as an explicit category, thus creating effects which matter. Hence it needed careful reassembling work in order to make it visible.

Throughout our analysis, we looked at the ways in which contemporary institutions of research imagine academic work, how value structures get institutionalised and become part of researchers' imaginaries, how careers are conceptualised and implemented and how time regimes and accompanying imaginaries or race and competition are performed and what all this means in terms of gender. Through subsequently reassembling a small selection of the accounts which showed the gendered and gendering nature of contemporary research, we want to draw attention to the many moments and places where gender differences simultaneously appear in and impact on choices.

Not only the strong normative forces omnipresent in contemporary research and the accountability logics being put in place, but also the massive rewriting exercises of the mythologies give large room for re/de-gendering science in subtle ways. We have shown in Chapter II of this volume how gender is performed through the excellence discourse and structures, in which is apparently little living space left for those not complying with the audit logic. The ideal of continuous competition and constant struggle for survival as a way of becoming a good researcher and being creative was described as alien by quite a number of female researchers and thus was judged as unattractive. Some of them would opt out, others would try to remain 'in the second row', patchwork their careers and thus survive on the margins of the system.

But also the division of labour within science showed clear gender dimensions. In Chapter III we showed, for example, how deeply gendered the distribution of work was, some kinds of work being less valued and

often rendered invisible – also through accounting mechanisms put in place – in particular articulation and support work often taken over by female researchers. This is tightly linked to the time logic inherent in research: time needs to be accounted for and this happens through the output that is produced as counterpart to its investment (see Chapter IV of this volume). Therefore, doing such articulation and support work is not valued and thus was not performed by those who wanted above all to succeed in the system. Also the roles which researchers adopted for themselves were gendered: support and articulation work was feminised in that it was articulated as a form of caring and supporting the ‘lab family’. Being closely related to other value structures and aspects of social life, mobility was another issue showing important gender dimensions. Belonging was not only about where one stays in the physical sense, but was also linked to the idea of feeling at home as part of a team or even at ease with how knowledge was produced and what questions were being seen as important. As a consequence, the capacity and, above all, the readiness to move around during large parts of early research lives differed in important ways and showed differences according to/in terms of gender and also nationality.

Finally, as already outlined in Section 3 of this chapter, only smooth and linear careers were valued, others being described as distorted and deficient. But it was not only the patchworked or horizontal nature of the career that might cause difficulties for women to stay in research. And although some countries have put in place programmes for women to re-enter science after having taken a break for family reasons, the fact that speed of knowledge production has become an intrinsic quality of research through the dense policy narratives of being in a race and arriving first, has rendered making a break nearly impossible. In the end, the dense myth of competition for successful careers has left virtually no space for anyone not wanting to climb the ladder of success.

In summary, one could say that most of these tacit complexities of everyday work practice remained unreflected by policy making. Stressing the need for women’s networks as kind of counterpart to the well-functioning old-boys’ networks, producing statistics on women in science as well as special career plans for women – as proposed in some of the European policy documents – does not by any means capture the complexities of living in research. Performed as measures on the macro level, they often distract our attention from the micro politics embedded in practices in labs and other constellations where research is undertaken. And such policy measures neglect the fact that women do not necessarily want to buy into simply mimicking male practices in order to survive – an issue we find in numerous accounts by female researchers.

Through this project and the analysis we offer, we want not only to contribute to a deeper understanding of how contemporary research works, how epistemic differences matter, but above all of how gender is at work in science. We have shown that many more complexities need to be taken into account, moving away from more naïve models of counting heads or of imitating/reproducing male structures. Despite the growing discourse on gender inclusiveness and the policy measures which are simultaneously being adopted, many of the more invisible orders installed through changing imaginaries and narratives, as well as accounting structures and the growing normativity are deeply gendered. Indeed, as we have shown throughout our analysis (see for example Chapter IV of this volume), a considerable number of research environments still reflect outdated gender norms, which seem to repulse both young men and women alike. Thus it is time to stop being ‘astonished’ either because women, and more generally young people, do not move into science in sufficient numbers or drop out – part of them seemingly ‘by choice’.

Across many of the issues addressed, we want to show whether research, will be an attractive place to live in and allowing for creativity to happen, will largely depend on the research systems’ capacity to convey the feeling that there is sufficient freedom to carve out epistemic living spaces that researchers also want to inhabit. This cannot be reached merely by policy directives and accompanying control systems, but would need many more refined incentive and support structures. Simply implementing a highly temporalised system – rapid production of innovation at any price combined with a flexible, short-term workforce – accompanied by dense normative frames thus seems a quite risky business with little to no guarantee of contemporary knowledge systems functioning in sustainable ways. Thus, if we believe in knowledge as the driver of contemporary economies and as a central force in shaping contemporary societies, and if we hold the understanding that the ways in which we know and represent the world are always linked to our choices of inhabiting it [Jasanoff 2004; see Chapter 1 of this volume], then policy concepts need to accommodate more complex and open arrangements. This implies to acknowledge that creativity and work environments are interdependent. This acknowledgement cannot only be accomplished through paying lip service to general ideas such as knowledge society but must happen in adequate policy framings. In particular it will be crucial to develop a much more fine grained understanding that knowing and living in research are inseparably intertwined.

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email: prodej@soc.cas.cz

