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Embodied Anxiety

On Experiences of Living, Working and Coping with Conditions of

Precarity in Research Cultures of the Academic Life Sciences

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0. Acknowledgements

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1. Introduction

>> I have been in my lab for about three years now. I basically like working here. I am getting along well with my colleagues and I like the process of developing something new together. After all, I have always been a curious person – from childhood on – and working in academia is a way of making this curiosity into my profession. That is really a luxury when you think about it, not many people are able to do that. I also like the freedom of being able to plan my time on my own. I come and go whenever I want. Of course we work more than other people. In fact, we never stop thinking about what we do – not at home, not in bed, not even when we're on vacation. But nobody controls your time. However, right now I'm in a bit of a difficult situation. I am following a risky research question, it is very exciting and promising but also very unpredictable. And right now my experiment just isn't working out and I don't have much time left before my contract ends. I wish that my postdoc was still here. She used to supervise me and there is a good chance that she would know what to do now! But the university wouldn't let her stay; they don't want us to stay for too long. It may be good for science if people move from lab to lab, but for me personally it was quite difficult to lose her as a colleague and mentor. I would still really like to finish this experiment. I could really make a contribution to science. And by the way, I could publish it in a top-rated journal. Man, this would look great on my CV! Any lab leader would be glad to take me in afterwards! Personally, I don't find it particularly rewarding to work for publications; it's more the process of trying out something new that motivates me. But publications are the currency with which we are paid! Anyways, I should really finish my thesis as soon as possible. A PhD really shouldn't take longer than three years and if I want to stay in academia, I should start applying for grants and jobs soon. In fact I should already have started. My lab leader once mentioned that he might be able to employ me part-time for a little longer. He sometimes saves some grant money for such cases. I should ask him again. <<

This ideal-typical statement of a life scientist at the end of her PhD studies illustrates in condensed form the complex network of uncertainties, ambiguities and tensions that young researchers experience in their everyday academic research environments. It is an eclectically compiled narration that builds on interviews and focus group discussions with young scientists, and gives a sense of the multifacetedness of uncertainty-experiences – from job uncertainty to contingencies in the life science research process (epistemic uncertainties) – that can emerge in the scientific work cultures that I will investigate in this thesis. What comes across in this statement is that epistemic uncertainties are entangled with other uncertainties such as job uncertainty in flexible working conditions, as well as an ambivalence about academic performance measurements or a tension between implicit academic career requirements and societal expectations about academic research. In this context, epistemic uncertainties seem to contribute to a new experience of social uncertainty.

In a broader societal perspective, uncertainty-experiences – and the challenge of coping with them – seem to have become such an omnipresent phenomenon that they deserve a day of celebration. November 17th is official "Coping With Uncertainty Day", according to a recent article in the *New York Times* (Tugend 2009). When talking to academic life scientists about their working experiences, one quickly gets the impression that every day is a day of coping with uncertainties. Asked whether an academic career was a safe choice, a PhD student answered:

> *Safe? No, certainly not. Anyway, science is in not safe in any way. You can qualify for the profession, but then you need to perform extraordinarily to have a salary. That's why I'd rather not call it a safe… job or something. Because there is still too much competition… as soon as you are in… you need to fight to stay in.* (f8: 604-10[i])

When researchers speak about uncertainty-experiences as "enervating" (m2: 1001[ii]) and "stressful" (f7: 1491, m4.1: 361, f1.1: 610[iii]) or in terms of "panic" (f4.1: 389[iv]) and "angst" (FGk_jun: 2185[v]) this supports the *New York Times* journalist when she asserts "uncertainty can sometimes take a greater toll than bad news" (2009).

However, researchers have a way of describing their living and working conditions as an ambivalent enterprise. Particularly the younger generation (PhD students, postdocs) seems to experience academic work as a career choice that entails a complex interplay of uncertainties, while at the same time being an exceptionally interesting and fulfilling activity that allows for many freedoms. The narration at the beginning makes no clear opposition between the "interesting, fulfilling and free" part of a career in science and the "uncertainty" part. Rather, the free and joyful pursuit of new knowledge is described as an inherently uncertain and unpredictable activity. Researchers even characterize as "risky research" the most exciting research where the outcome is completely unknown. It seems to be a core skill of life science researchers to (learn to) be able to expose themselves to such uncertainties, to creatively cope with them and to tolerate the frustrating moments that they entail. In exchange for the uncertainty, many researchers would contend that a certain degree of freedom is assured. It seems fair to say that experiences of living and working in academia are structured by the complex interrelation between experiences of uncertainties and freedoms to act, an interrelation where both rest on each other and create synergies but also contradict each other and create tensions. An understanding of uncertainty-experiences in this context thus seems to require careful analysis.

But why is it interesting to explore uncertainty-experiences of young academic life science researchers? Even if these feelings of uncertainty are intense, researchers are, after all, highly qualified people with comparably low probability of ending up unemployed and penniless. I will address this concern from two different angles to

show the broader political relevance of analysing uncertainty-experiences in contemporary research cultures.

1.1. Tacit Governance of Research Cultures

The pervasiveness that adhered to researchers' ways of narrating uncertainty-experiences suggested that they might contribute to how their research cultures were organized – i.e. by prompting certain ways of reasoning and by guiding decisions on what research trajectories they followed. Such narrations were often accompanied by narrations about where they located desirable or unwanted uncertainties, how they worked around or along uncertainties or how they used and tried to avoid them. The statement above was for instance very likely to continue as follows:

>> *Well, maybe it wasn't too wise to risk this – others have warned me! I could have picked a safer question. If I continue in academia, I should probably choose a safer project – or even better – a safe project alongside a risky one. This way I will be able to publish quicker and be better equipped for getting myself the next job or grant. It is a bit paradox if you think about it. It's almost an incentive for not taking risks. But if we stopped doing risky research, we might just as well do any other job.* <<

At moments, they started to wonder how much "risky research" they could still afford to do. Most therefore talked about learning to distinguish risky from safe research trajectories in order to tame epistemic uncertainty and prevent it from turning into a social risk or career risk, like in the example given above when an experiment required more time than expected and the contract was running out. In this case, some researchers even considered leaving academia altogether, as these two PhD students very vividly described in a focus group discussion:

PhD1: *…the reason for why I don't want to make an academic career is that I have angst. I'm talking about safety here. Because I'm missing safety in this kind of career. …*

PhD2: *…what I can't live with is that the rest of your life* [is] *unsafe and mobile, simply live an uncertain life and have to be mobile. Up until old age.*

(FGk_jun: 2185-202[vi])

As in this quote and the statement above, researchers often discursively linked their uncertainty-experiences to reflections on how they (re-)arranged their practices in order to make their working conditions liveable, to accommodate meaningful research, and to make plans for their professional and personal futures. Exploring the rationales that are implicit in such (re)arrangements hence opens up a new perspective on how research cultures are currently being transformed – and particularly on how researchers contribute to changing research cultures from

below. Even if ways of coping with uncertainties often appear as individualised at first sight, these rationales contribute to creating a new repertoire from which researchers draw and shape the way that they learn to act and to make decisions within broader currents of transformation.

Alongside debates about how to build knowledge-based societies and economies, the question of how research cultures are, should be and can be transformed and governed in order to meet societal expectations has become a controversial topic. In this context, research policies are seen as a key site for further developing contemporary societies according to the rationales of a knowledge-based economy. In the past decades, Austrian academic research institutions have been subject to several waves of reorganisation. They have seen transformations of university organisation, of funding rationales, of employment patterns and of overall national and international research policies. How these measures find expression in everyday research cultures has however remained largely unexplored. When I was interviewing researchers for this study, they usually insisted that academia (still) was a place where research could be done quite freely. New governmental measures seemed to leave relatively large spaces for autonomous decision-making and for negotiating in the everyday how research is actually planned carried out. At the same time however, researchers' narrations suggested that their everyday research practices were guided by subtle – but nevertheless powerful – rationales that determine which research was too risky or safe enough within contemporary conditions of the academic life sciences.

Especially within the young generation of researchers, experiences of pervasive uncertainty seemed to thrive and in some ways guide how they learned to set priorities, organise and carry out their research. Or put differently: These experiences seemed to introduce new dynamics in the governance of research cultures. This thesis explores empirically what might be happening when living and coping with pervasive uncertainty-experiences becomes part of the academic self. It presents an in-depth exploration of how two different kinds of change interrelate in the everyday research cultures of the academic life sciences: The introduction of newly emerging research policies in academia on the one hand, and increasing experiences of uncertainty on the other hand. The analyses will amongst others make graspable the complex interrelation of structural contexts within which young researchers are situated. In the above statements we can, for example, find traces of research funding patterns, of employment policies of academic institutions, of performance criteria and of academic career cultures. Even if it may be possible for young researchers to ignore them most of the time and enjoy the freedoms they provide, there are certain biographical moments – such as at the end of a contract – in which these structural contexts seem to be actualised and affect researchers' ways of acting and deciding. However, young scientists' accounts of how they (re-)order

everyday research life suggest that they are not determined by structural conditions, but rather that structural conditions constitute spaces – however filled with uncertainties, ambiguities and tensions – within which young researchers (learn to) manoeuvre. The way they contribute to change is thus not to be understood as a passive one that follows policy-measures but an active practice of adapting to or reordering the structural conditions of their own research practices, however tacit this may seem at first sight. For my purposes here, I will speak of this dimension of change in terms of tacit governance, a concept proposed by Felt/Fochler. In their understanding, change happens, in part, through

> …mediated, often not directly visible relation effects… tacit governance is meant to express that steering effects of governance measures on the macro-level cannot be related to effects of these measures in a linear and definite way… Rather, distortions and fractions in academic life emerge… as a result of coexisting, partly uncoordinated logics of governance… and open up… new spaces of negotiation and possibility. (Felt/Fochler 2010: 298, German original)

In this thesis, I explore the spaces of experience and manoeuvring that the current set of conditions for doing life science research establish for those who carry it out in the everyday. Building on the accounts of researchers, I am interested in how they (decide to) fill such spaces of negotiation and potential and thereby contribute to the way that research cultures are currently being transformed. This perspective follows Steven Shapin who – in his recent book *The Scientific Life* – has suggested that a better understanding of scientific work cultures requires a detailed look at „concrete realities of individuals' lives and choices" rather than treating their acts as determined by institutional structures (Shapin 2008: 231).

1.2. Academic Researchers as (Knowledge) Workers

This thesis aims to better understand how young researchers learn to act and decide, by looking at them not only from an academic career perspective but to look at researchers as (knowledge) workers. The challenging and ambivalent aspect about this perspective is that only in the fewest of cases does it actually coincide with the perspective that researchers themselves took. Many implicitly objected the notion of their activity as (wage) work and preferred a notion of self-determined, free and highly personalised activity. Even when, in the light of the high level of competition, they were aware of the difficult prospect of making an academic career, their accounts suggested that they organised their professional life in the pursuit of it. Nevertheless, I see at least three reasons for approaching their research activities in terms of (wage) work. The first and very simple one is that they did research (amongst other factors) for their living – that is, they depended on their jobs to pay for simple living costs such as their rent, their food, their clothes or

provisions for their children. The rationales by which decisions are made in their everyday research contexts were thus always (also) guided by the question of how to secure their future employment. A second reason concerns the (probable) occupational biography of young researchers. When researchers pondered their professional futures, they often estimated the likelihood of actually making an academic career as very small. Even if most of them seemed to organise their work according to the requirements of an academic career, statistical data suggest that it is most likely that they will leave academia for another workplace, sooner rather than later. In the UK context only 3,5% end up with a permanent job in academia and only 0,45% will be professors. For most people that are active in research in academia, this is only a brief period in their life and about 80% quit doing research entirely (The Royal Society 2010: 14). At the same time, the number of PhD students and postdocs is steadily increasing and they cover a growing share of the workload in academic research. For the US, the "Committee on Science Engineering and Public Policy (COSEPUP)" indicated that since "the 1960s the performance of research, especially in universities, has relied more and more on a growing population of postdoctoral scholars" and that in the meantime the "size of the postdoctoral population has increased without a parallel increase in the number of academic faculty positions" (COSEPUP 2000: 20). For the Austrian context we lack such detailed data but – as I will discuss in a later chapter – it is very likely that we are witnessing a similar development. In retrospect, for the majority of young researchers in academia, their work experiences in academia will not have been the first step into an academic career but their last academic job. Instead it is likely to be their first job experience in a series of jobs that may or may not be related to their research field or to knowledge production at all. Most likely they will become part of the highly qualified knowledge workers who have become recognised as crucial workforce in the emerging knowledge economies (chapter 2). In that they will have had their first work experiences in academic research, the academic workplace is however becoming an important space of socialisation and subjectification not only for academic but for non-academic work cultures as well. Whether or not young researchers will be able to stay, academia is the place where they first learn to cope with uncertainty-experiences, to self-organise, to shape, relate to and appropriate their work(ing) contexts. From this perspective, acting upon uncertainty-experiences in academic contexts is not only interesting with regard to transformations of academic work cultures but might prove relevant for the broader question of what kinds of work cultures are emerging in knowledge-based societies. A final reason for looking at academic activity in terms of work is that – to a certain extent – it seems to have lost its semi-privileged status as free and self-organised activity. We know of course from historical studies that academic activity has never been absolutely free and self-organised. However, with the

attempt to transform Western industrial societies into knowledge-based ones, academic activity has showed up on the radar for regimes of productivity, profitability and instrumental reasoning at a scale that might be triggering qualitative changes in the character of contemporary academic activity. In recent years, some authors have suggested that this change is happening tacitly – i.e. not only as a visible convergence of academic and non-academic industrial institutions but also as a subtle convergence of academic and capitalist codes and norms (Kleinman/Vallas 2005, 2007). The organizing values of non-academic work cultures seem to be less inclined to stop at the doorstep to the university, or in the words of Clark Kerr: „the university becomes tied into the world of work, the professor – at least in the natural and some of the social sciences – takes on the characteristics of an entrepreneur… The two worlds are merging physically and psychologically" (Shapin 2008: 231). Such observations tend to call into question the qualitative difference between work in academia and elsewhere.

In this context, my work will make use of insights from the field of labour studies in order to better understand newly emerging dynamics in academic work cultures. I will consider in particular more recent studies and theories about subjectified (or immaterial) labour – that is, labour that builds on a high investment of subjective factors such as self-motivation, affectivity, creativity and communicative skills (Lazzarato 1998, Moldaschl/Voß 2003, Beynon/Nichols 2006, Moosbrugger 2008, Lohr/Nickel 2009). Reflections from these fields of study will help me to better characterize why emerging uncertainties, ambiguities and tensions are often narrated as very personal experiences. They will also shed light on newly developing forms of social (or labour) conflicts in the contexts of such subjectified work. Researchers' accounts suggest that in academic work cultures, employment standards that have come to be formally accepted in Western industrialised countries – such as in the Austrian case the 8-hour-day, 40-hour week, free weekends, five weeks of vacation per year, employment protection during maternity leave – are subverted on a regular basis. In researchers' words, this situation would typically resemble the following:

>> Boy, sometimes I envy people with ordinary jobs: permanent contracts, fixed working times, weekends off, a clear set of rules on what and what not to do and no worries about the future. Then again, doing research is just so great, because it's so deeply connected with who I am and what I personally like to do. I feel free because the motivation comes predominantly from myself. However, my spouse and I are planning to have a child soon and honestly I am a bit afraid about it, because in academia you don't earn too much – especially when you count in the overtime – and there are only short contracts available. Lately, I have also started thinking about alternatives. Even if I still want to continue, I find that having other options relieves some of the pressure. Who knows how it all will work out? Some things are just not that predictable, right? (laughing) <<

As in this paragraph, it often seemed that the way that researchers experienced uncertainties, ambiguities and tensions in their working contexts had to do with the personal relationship they had (developed) with seeking new knowledge. This suggest that if we want to understand latent conflicts within newly emerging work cultures we need to take seriously such accounts of highly personal – i.e. subjectified – activity. A new source of tension in this context is the high level of voluntary unpaid overtime in such work cultures that ever again has gained some media attention in recent years. The German newspaper *taz* has for example approached several researchers with the question: "Why do PhD students, research assistants and researchers without professorships… let themselves be exploited in that way?" (Friedmann 2010, German original). The most common answer was that people do it in the hopes of better working conditions in the future and that academic work is a dream job for which people are ready to cope with discomforts. This suggests that – at least as an interim arrangement – researchers are willing to trade interesting, personally challenging work for compromised employment rights (such as unpaid overtime) and job uncertainties. The interesting point about subjectified work contexts, however, is that employment standards are often not (primarily and directly) subverted by the employer. In the empirical sites of this PhD study for instance the academic institution or the project leaders rarely intervened directly or demanded long working-hours. Rather it often seemed that there are anonymous forces at work that favoured (so-called) voluntary self-exploitation.

This particular set of interests, ambiguities and moments of conflict is often discussed as being incongruent with established procedures of acquiring more certainty in work cultures in that it tends to discourage a recourse to established forms of coping with uncertainties such as collective bargaining and unionising. This suggests that we might be seeing the emergence of work cultures with newly constituted uncertainty-experiences that require new infrastructures for social security and assertion of workers' rights. By developing a better understanding of such work cultures and by exploring uncertainty-experiences within them, this thesis also aims to shed light on how appropriate infrastructures for social security might be formed.

1.3. Research Gap and Research Questions

Ever more diagnoses of the present – whether they derive from social scientific studies, philosophical works or social movements – describe intense uncertainty as a defining experience for our age: "Age of uncertainty" (Nowotny et al. 2001), "risk society" (Beck 1986), "boundless angst" (Virno 2005) or "social precarity" (Chainworkers 2005) are just a few examples. Since it can sometimes be confusing to orient oneself in the terminological thicket of innumerable works about the

topic, and since in recent years the terminology of precariousness seems to have stabilised in both public and academic debates, I will use the term precarity[1] when I address pervasive uncertainty-experiences (cf. Castel/Dörre 2009, Freudenschuss 2009). However, even when depicted as an omnipresent phenomenon, the majority of works about precarity emphasises that uncertainty-experiences vary widely and that they always have to be seen as situated in specific contexts. It is all the more surprising that the academic debate about experiences of precarity still largely lacks grounding in empirical in-depth explorations of the contexts that condition uncertainty-experiences and of how they feature and express themselves in everyday living conditions. Indeed, with regard to academic living spaces, social science research has recently started to explore the implications that flexible employment conditions and uncertain career prospects can have for academic research cultures (cf. Bultmann 2008, Crang 2007, Dörre/Neis 2008, Hecht et al. 2009, Salonius 2010). Andrew Ross for instance has described young academic researchers as a typical example of precarious living and working conditions in his book *Life and Labour in Precarious Times*:

> Once they are in this game, some of the players thrive, but most subsist, neither as employers nor traditional employees, in a limbo of uncertainty, juggling their options, massaging their contracts, managing their overcommitted time, and developing coping strategies for handling the uncertainty of never knowing where their next project, or source of income, is coming from… (Ross 2010: 5)

While the geographic focus for his observation is the US, empirical accounts of researchers suggest similar conditions in the Austrian context. Sociological studies like this have, however, primarily investigated the ways in which people deal with social uncertainties – such as employment uncertainty (e.g. Greco 2000). As suggested above, however, it seems that for the particular case of life science research cultures we need to go beyond such a limited notion of uncertainty. Instead what seems to shape the specific sort of uncertainty-experience in academic life sciences is an unstable and malleable assemblage of uncertainties, ambiguities and tensions that results from researchers' high personal involvement in a highly contingent research process under newly evolving societal ways of framing their activity. It is surprising that while knowledge production is said to have become so crucial for supporting, sustaining and organising our societies, there are few studies that have investigated how the abundance of uncertainties in the knowledge production process (epistemic uncertainties) impinges on the lives of those who are involved in its everyday practices.

The aim of this thesis is to trace uncertainty-experiences in this broader sense and to read researchers' ways of living and working in academia as a way of

[1] For a thorough reflection on why I chose to do so and for my particular understanding of the term in this context, see chapter three.

manoeuvring within, with and around them. The particular research questions are, on the one hand, concerned with how young researchers interpret uncertainty-experiences in their epistemic living spaces (Felt/Fochler 2011[2]) and their ways of being and acting within them and, on the other hand, with how their uncertainty-experiences might relate to broader societal phenomena:

(1) What kinds of uncertainties do young life science researchers experience in academia? And how do they give these uncertainties meaning?

(2) What framework conditions of (life science) research do early stage researchers refer to in their narrations of uncertainty-experiences?

(3) What explicit and implicit accounts do they give about coping with uncertainties?

(4) How can the way in which researchers experience uncertainties be conceptualised?

(5) How do their experiences relate to more general societal diagnoses of increased uncertainty-experiences?

Throughout the exploration of these questions, this thesis is interested in the ways that policy measures on the macro-level relate to researchers' tacit micropolitics in everyday research environments. In using the academic life sciences as an example, it aims at shedding light on the role that young researchers play in articulating policies into the contemporary transformation of research cultures.

In order to explore these questions, this study builds on qualitative empirical materials (interviews, group discussions and observations), which were largely compiled in the context of two collaborative projects at the Department of Social Sciences/University of Vienna that investigated work cultures in academia (GOLD-II, KNOWING). These materials were further supplemented by more focused interviews on uncertainty-experiences within my subsequent PhD project "Uncertain Research Landscapes".

1.4. Outline of the Thesis

This thesis is subdivided into four parts that each discusses uncertainty-experiences on a different level. I will start by exploring the wider societal currents of transformations within which they emerge (part 1) before I examine in more detail the structural preconditions that seem to inform the particular uncertainty-experiences in academic life science research cultures (part 2). Then I look more closely at how the convergence of these conditions establishes a set of conditions in

[2] For a further exploration of the concept of epistemic living spaces see the introduction of part two of this thesis.

the everyday that support a pervasive experience of uncertainties (part 3) and will finally explore researchers' ways of manoeuvring within, with and around them (part 4).

The first part lays out the wider frame of debates about societal transformation within which uncertainty-experiences of young academic life scientists gain relevance. Chapter two explores the societal, economical and political boundary conditions within which knowledge production in general – and academic institutions and researchers in particular – are discursively (re-)positioned in the context of the political aim of constituting the European Union (EU) as a knowledge-based society or economy. In chapter three I review debates about broader societal diagnoses of increasing uncertainty-experiences and position my approach to exploring uncertainty-experiences in academic research cultures at the intersection of these debates on transformative dynamics. Chapter four then builds the bridge between the wider societal background and the empirical grounding of my particular study. I discuss the empirical settings and material of the thesis as well as the methodological and analytical concepts that will be employed throughout.

The second part analyses the systemic preconditions of uncertainty-experiences in the particular field of the academic life sciences. The selection and discussion of conditions (epistemic uncertainties, subjectified work, academic career norms, casualisation and commodification) builds on the empirical analysis of researchers' accounts but is largely located on the level of theoretical reflection. The purpose is to sharpen my gaze on this particular empirical context by using experiences of existing strands of academic debate.

Part three provides an empirically grounded in-depth analysis of how these preconditions are experienced as converging and expressing themselves in a set of conditions in everyday academic work cultures. In doing so, it explores how spaces for doing life science research are established in which pervasive experiences of uncertainty emerge for young life scientists. This analysis is then brought together into a chapter that reflects on how researchers' overall experience of uncertainties, ambiguities and tension might be inscribed in their subjectivities during – what I describe as – an ongoing cycle of anticipation, guilt and restlessness.

In the last part I build on the previous explorations and discuss researchers' ways of acting upon conditions of precarity. I analyse in-depth how they talk about their ways of coping with experienced uncertainties and identify four modes of coping that seem to complement each other in the everyday but at moments also come in conflict and push each other aside. These ways of coping are then reflected from three different angles along the question of how young researchers might be contributing to the tacit transformation of their work cultures from below.

Based on these explorations I will finally conclude that the way that researchers are socialised and subjectified within pervasive uncertainty-experienced indeed seems to tacitly – but possibly very sustainably – take part in the governance and transformation of research cultures. In that uncertainties, ambiguities and tensions seem to generalise in an overall *anxiety* and in that they researchers continuously learn to cope with it they might internalise and accommodate – i.e. *embody* – certain rationales of thinking and acting and thereby in the long run introduce new dynamics in the social and epistemic workings of academic life science research. I will therefore suggest labelling the particular form of precarity in the academic life sciences "embodied anxiety".

PART 1: Setting the Wider Frame of Transforming Research Cultures

In this first part I discuss two strands of transformation within which young life science researchers in academia can be pictured: first, the changing role that knowledge production is ascribed in our societies and the implications this had for the organisation of academic institutions; and second, a general increase of uncertainty-experiences, that is often referred to as precarisation. In doing so, I will on the one hand set the wider societal, economical and political background before which uncertainty-experiences of young life science researchers are discussed in this thesis. On the other hand it will hint at the broader social and political debates that this thesis aims at contributing to. In doing so this part will also clarify terminologies and concepts that will frame the analyses in later parts. Chapter two explores how knowledge production in general – and academic institutions in particular – are (re-)positioned and reorganised along the building of a knowledge-based society or economy and the changing expectations towards academic institutions and academic researchers in this context. Within the broader developments, it takes a particular focus on the emergence of new work cultures. The third chapter discusses debates that have suggested increased experiences of uncertainty as defining for our age. I will consider both, approaches that focus on social uncertainties and such that build on uncertainties that arise along techno-scientific progress. In doing so, the chapter gives an overview over a range of understandings of uncertainties and notions of the precarious that can be found in the academic debate. I conclude the chapter by positioning my study within a relational and context-sensitive understanding of precarity that hypothesises that uncertainty-experiences can have governmental character. Chapter four then leads over to the empirical settings in which I have explored my research questions and discusses the empirical material and methodological approach that my analyses build on. It also aims at providing the reader with transparency about how the particular research questions and trajectories developed and about why I choose the concepts of the "everyday" and "experience" for my analysis.

2. The Changing Role of Knowledge Production

>> Mud flows and volcano eruptions have blocked out some paths while others are easier to follow. Huge new cathedrals have been built and have become centres of attention while the fundaments of others have lost loading capacity. Everywhere, there are new circumstances that are barely visible and sometimes change in a blink of an eye. Manoeuvring these earthquaky landscapes is an uncertain and potentially even dangerous enterprise: a path that is entered might turn out as a dead end or lead into an abyss while the word is out that new, beautifully shaped trails might lead to the promised lands of academic career and freedom. However, there is a forest of fingerposts in which seeing the big picture has become impossible. It is unsure whether to look for the promised land in a valley or on the top of a mountain since contradicting rumours have been planted – some insist that the boundaries of science are disintegrating and the pleasures of free knowledge production are soon to be found around every corner while others insist that the real joy and liberation is still enclosed at the top of the highest mountain where excellence lives and that only the fittest and most adventurous will reach – provided they are lucky and willing to make respective sacrifices. <<

If young researchers in the life sciences were to put the landscapes within which they do their work and pursue their academic career in metaphorical speech, it might turn out like such a narrative. While the picture drawn here insinuates several dangers, ambiguities and uncertainties, it also harbours utopian moments of the transforming research landscapes such as more freedom in knowledge production or liberated activity. What, however, may be experienced as haphazard or accidental changes in work cultures on the micro-level of everyday research is accompanied by a policy discourse and broad academic debate of the changing role of knowledge production in society and the economy over the past decades. In this chapter I will discuss the macro-political considerations and discursive patterns before which these changes are taking place and trace implicit dystopian and utopian moments in them. I will do so on different levels. First I revisit academic theory-building about the changing role of knowledge and knowledge production in society that date back to the 1960s, before I turn to the more recent policy discourse that has accompanied this transformation process. In doing so, I focus on the development of science policies within the European Union (EU) that have intensified with the Bologna Declaration in 1999 and the Lisbon Process from 2000 onwards. I will then explore the changing expectations towards and new ways of governing academic institutions and reflect on what this implicates for academic work cultures. Finally, I will tackle the question of how the quality of those changes has been conceptualised in the academic debate.

2.1. Toward a Post-Fordist Utopia?

> That's the way it is. It could as well be otherwise. (Nowotny 1999, German original)

Theorists from the 1960s on have predicted, observed and discussed the growing importance of knowledge production for societies. They have ascribed these changes particular relevance since they seemed to significantly be changing modes of production, modes of labour and therewith the workings of the economy. Different terminologies have been found to describe such as a shift from a manufacturing society to an information society/knowledge industry (Machlup 1962), economy based on knowledge (Drucker 1969), post-industrial society (Bell [1973] 1999), information economy (Porat 1977), knowledge society (Stehr 1994, Knorr-Cetina 2000) or network society (Castells 1996). Discussions of these new states of knowledge have always carried a range of utopian moments. Promises about greater control over social destinies, greater individual freedom in informed decision-making, potential for more democratic organisation, fewer social hierarchies, or global communication were discussed, alongside worries about information overload or lack of relevant knowledge. Theorists of labour studies anticipated an improvement in working conditions by a supposed shift from Fordist work organisation that had entailed alienated labour to a different character of work that has been discussed under the headings of post-Fordist, subjectified, immaterial or knowledge work. In particular, they expected less hierarchical social organisation, more flexible and self-determined forms of work; i.e. a substantial liberation from estranged labour and restraints to further capitalist development. These optimistic accounts have within the past decades become empirically challenged by studies on post-Fordist work organisation. They have indeed observed a shift away from Fordist production processes, but have also pointed out potential new downsides of post-Fordist modes of production such as self-exploitation and burnout (Beynon/Nichols 2006). André Gorz has addressed the multi-potential character of this transformation of work cultures as follows:

> Post-Fordist production presents itself both as the heralding of a possible transformation of work by the workers and as the regression towards a total subjugation and quasi-vassaldom of the very person of the worker. Both aspects are always present. (Gorz 1999: 32)

When investigating such work cultures it thus seems pivotal to pay attention to the particular ways in which are socially framed and organised. Daniel Bell has emphasised this very early on in saying it will depend on the way in which societies and institutions will learn to organise knowledge-intensive activities:

> Like many advances in human history, post-industrial developments promise men and women greater control of their social destinies. But this is only possible under conditions of intellectual freedom and open political institutions, the freedom to

> pursue truth against those who wish to restrict it. This is the alpha and omega of the alphabet of knowledge. (Bell [1973] 1999: lxxxiv)

On such grounds, critiques on the econcentric character of emerging knowledge societies have emerged. Manuel Castells for instance has formulated the controversial hypothesis that "capitalism has used the informational revolution to renew itself following the 1970's crisis of industrial capitalism" (Castells 2001, cit. Jessop 2007: 118).

2.2. At the Crossroads: Knowledge Society vs. Knowledge Economy

In the meantime, statistical data such as from the Organisation of Economic Co-operation and Development (OECD 2002) seem to have established beyond controversy that the share of total value added by the knowledge/information sector has been increasing and that ever more people are "not involved in the production of tangible goods" as Daniel Bell had anticipated ([1973] 1999: 348) but in the production and management of knowledge and information.

This shift has repositioned scientific activity in society and has been transforming the relationship between science and its societal contexts. It did so especially with regard to the growing importance of knowledge (production) in the increasingly inherent role that (knowledge) production is regarded to have in the economic growth of Western de-industrialising societies. With the perspective of industries and material production being more and more moved off-site from European countries, national states and supra-national unions such as the EU lay their hopes for continual economic growth and societal wealth on their potential for (economically valuable) knowledge production.

Within the EU, the most visible policy signs of the transforming relationship between science and society are the Bologna Declaration 1999, the Lisbon Strategy 2000 and – most recently – the EU framework programme for research and innovation "Horizon 2020". In the use of terminologies surrounding them, an interesting shift is tangible that marks the development of a focus on the economic value of knowledge within the broader discourse about building knowledge societies. Earlier policy documents spoke about a "Europe of Knowledge" in which the relevance of knowledge production was formulated in a relatively broad way as necessary for societal development. It was seen as an

> …irreplaceable factor for social and human growth and as an indispensable component to consolidate and enrich the European citizenship, capable of giving its citizens the necessary competencies to face the challenges of the new millennium society. (Bologna Declaration 1999)

Within the past decade we could observe a shift away from the terminology of "Europe of Knowledge and "knowledge society" towards a terminology of "knowledge-based economy" (KBE) (Hunsiger 2010). Policy documents surrounding the ongoing Lisbon Process may illustrate this. In "Extracts from Presidency Conclusions on the Lisbon Strategy", the European Commission declares that the Lisbon Strategy commits the EU and its member states to the aim of becoming "the most dynamic and competitive knowledge-based economy in the world" (European Commission 2004: 5, cf. European Council 2000), building on the belief that the

> …European Union is confronted with a quantum shift resulting from globalisation and the challenges of a new knowledge-driven economy. These changes are affecting every aspect of people's lives and require a radical transformation of the European economy. (Ibid. 2004: 5)

This economic shift is considered to be "of crucial importance for competitiveness and growth and for building a more inclusive society" (Ibid. 2004: 22). This shift towards an emphasis on the economic relevance of knowledge production was informed by statistical analyses and theory-building about the economic potential of knowledge in strategic policy papers by expert groups and think tanks around the OECD and the EU. The EU agency named Eurofound (European Foundation for the Improvement of Living and Working Conditions), for example, provides "expertise", "data and analysis for informing and supporting the formulation of EU policy on working and living conditions" (Eurofound 2010). Eurofound's research manager, Timo Kauppinen, gave a conference talk in 2010 in which he discursively linked knowledge competitiveness to economic success of a company, region or country. His core concept of knowledge capital is exemplary for how the economic potential of knowledge production is conceptualised on a policy level. He sees in it

> …the driving force of the knowledge economy, referring to the society or region's capacity for creating new ideas, and converting knowledge into commercial values. (Kauppinen 2005: 12)

Further, knowledge capital is often described as requiring sophisticated infrastructure for its production and accumulation. For exploring the production cycle of a knowledge-based economy, the author defines a

> …strategic triangle of knowledge society… in which human and social capital lay the foundation for increased accumulation of knowledge capital [that] provides the raw material for knowledge competitiveness required to enhance the welfare of citizens. (Ibid)

The meaning of knowledge society in this context is mainly defined in economic terms. There are indeed also expert groups that highlight non-economic aspects of the knowledge society. The "Expert Group on Science and Governance to the Science" for example has paid attention to more democratic procedures in systems

of knowledge production in an elaborate report. While the terms "knowledge society" or "Europe of Knowledge" seem to have embraced understandings of "social and human growth" in a very broad sense, the notion of the KBE, as it is currently predominantly applied in policy discourse, seems to have focused on economic growth and to prioritise the aim of using scientific knowledge for competitive economic advantage. Science in this notion is seen as both the key factor of new production and as traded commodity-product in itself (cf. Felt/Wynne 2007: 14).

In policy papers it is formulated as a key trajectory of research policies to reinforce concepts of market economics such as competitiveness, efficiency and private property rights within research cultures. "Innovation" seems to be increasingly understood as knowledge production that is marketable, as the following quote may exemplify:

> Research activities at national and Union level must be better integrated and coordinated to make them as efficient and innovative as possible… innovation and ideas must be adequately rewarded within the new knowledge-based economy, particularly through patent protection. (European Commission 2004: 27)

Newly evolving research policies have also embraced the academic scientific realm. The periodic evaluation of the Lisbon Process by EC-expert groups includes the performance of universities. The "quality of research in European universities" is to be strengthened in "its impact, including the economic impact". The economic and merit-based criteria that are applied to trace their progress include "patent documents, number of spin-offs, number of high-growth firms created" (European Commission 2010: 141).

There is however a further noteworthy aspect to Kauppinen's theory: rather than financial and physical capital or raw materials he defines human and social capital as the basis for knowledge production and knowledge competitiveness and thus the European economies. Which resources and skills people who produce knowledge will need to help build a KBE has been a widely discussed matter along the Lisbon Process and there is still an open debate about how knowledge workers need to be educated and socialised to acquire those skills. Education, in particular academic education, has moved up in the priority list of policy makers internationally. In the 2001 Education Policy Analysis of the OECD the particular understanding of education in this context is formulated:

> A group of 'knowledge workers' can be identified as those performing knowledge-rich jobs. Such workers are typically but not universally well educated. Some knowledge workers have high levels of literacy and lower levels of education, implying that basic skills obtained beyond education are recognised in the knowledge economy.

> There are additional 'workplace competencies' needed in the knowledge economy. Communication skills, problem-solving skills, the ability to work in teams and ICT skills, among others, are becoming important and complementary to basic core or foundation skills. … However, further research is needed to inform education policy makers about how to develop the right skills for a knowledge economy, rather than assuming that high levels of education alone, as conventionally defined, will be enough. (OECD 2001: 99)

What is interesting to note here is that the quality of human resources is defined beyond traditional skill sets of formal qualification. People's social and communicative capacities are defined as equally crucial in building up adequate human capital for the knowledge economy. Policy documents that have accompanied the Lisbon Process follow this line of thought. What can be observed throughout these documents is that this new understanding of eduction is expected to have an enormous impact on the way in which people need to be educated and plan their professional careers because "every citizen" needs to be equipped "with the skills needed to live and work in this new information society" (European Commission 2004: 20). An additional noteworthy skill that is emphasised in EC-documents is the skill to make knowledge marketable. In "an ambitious, comprehensive and mutually reinforcing ten-year programme of reforms" the Lisbon Strategy particularly aims at initiatives to "harness research, finance and business talent to ensure that European ideas reach the European market place first" (European Commission 2004: 12, 28). In a later policy document, academic institutions are ascribed a core function within the "fully-functioning knowledge triangle" in two respects:

> - a much enhanced and constantly evolving knowledge base in universities[3] and research centres that could be quickly translated into innovative products, services, approaches and methods in the wider economy and society at large,
>
> - promoting a creative, innovative and entrepreneurial mindset among pupils, trainees, students, teachers and researchers which would underpin the progressive development of a greater culture of enterprise through education and training together with a more dynamic European labour market and a higher skilled workforce. (Council 2009: 3)

In two respects, universities are mentioned as potentially contributing to the knowledge economy: first, in immediately producing marketable knowledge and second, in producing human resources that have a mindset that aims at making knowledge marketable. What is required from the human resources of tomorrow seems not only to be the right and higher education, but also communication, networking and entrepreneurial skills.

[3] In this policy text, the term „universities" is used to denote all types of higher education institutions.

In this process – and building on OECD-data that projected an ongoing and strong increase in the need for suitably qualified scientific researchers – an expert group from the European Commission has diagnosed a "crisis in the production of human resources for science and technology" (Gago 2004: 7). In this contest, academic institutions have been identified as key players in the production of the "human resources" needed in a competitive knowledge-based economy. Alongside "competitiveness" and "marketability", "employability" of graduates has become a guiding concept in how to restructure scientific education and science policies in the European Education and Research Area. Recent expert opinions highlighted the need for the organisation of universities to "become more competitive, either for competitive funding, for students, or for industry/services" (European Commission 2010: 140).

The trend that I have sketched here is supported and followed up by strategic financial instruments such the EU framework programme for research and innovation "Horizon 2020" that emphasises the future relevance of a science-based industry (European Commission 2012). Along such reasoning, a reorganisation and new governance of educational accompanied the re-conceptualisation of the economic basis of the EU within the past decades – including academic – systems.

2.3. New Ways of Governing

With respect to the production of suitably skilled human resources, the workings of academic institutions, and in particular their role in the education of the knowledge economy's work force, have become subject to a range of new governmental measures and to organisational restructurings. The reorganisation of universities has come to be debated as gradually giving universities autonomy. Universities that used to be public sector universities have come to be "autonomous" in the sense that parts of the decision-making have been shifted from governments and ministries to the universities and that universities have become responsible for acquiring and managing their own funds (cf. Felt/Glanz 2003, 2005). However, discussing this transformation process in terms of autonomy can be misleading. Contrary to what the term "autonomous" suggests, it can be argued that recent changes did not leave academic institutions with less governance but has introduced different way of governing.

In the Austrian context, the process of "releasing universities to autonomy" began relatively late in the mid 1990s. In 1993 the University Organisation Act (UOG 93) aimed at giving universities more sovereignty in decision-making in order to make universities faster and more flexible in adapting to societal demands. The UOG 93 was also the first measure to institutionalise more business-type management into universities. The University Act 2002 (UG 02) and the "Civil Servants Law-

Amendment" 2001 further enforced the "autonomy" of universities and reorganised decision-making structures along the organisation of modern companies. External funding has become an inherent and strongly increasing portion of academic research funding. Since the UG02 came into force in 2004, researchers who are employed using project funds are formally also employees of the universities. This has gradually but fundamentally altered employment patters of academic researchers. Taken together, these organisational changes basically ended the tenure track system and changed the legal status of university employees from being civil servants to being private employees. In 2009 then, a new collective labour contract came into force that reintroduced a different form of tenure track system and regulated wages. Overall it has provided universities with a wide range of employment possibilities for researchers so that universities can individually decide how to handle them. Experiences in recent years have proven for example that universities implement the collective contract in very heterogeneous ways. Statistically, there is a clear overall trend towards more part-time contracts and a shift from employment on basic funds to project funding for research personnel.

These changes in personnel policies are part of a broader transformation. Under the pretence of releasing universities to autonomy, the state has confined universities to strict business management (Knobloch 2010) and its core elements such as accountability, definition of goals, expansion of formal structures and professional management. They have resulted in a "managerial revolution of the higher education system". In the wake of these restructurings, universities have come to be regarded as entrepreneurial actors and university management as in need for more entrepreneurial rationality (Maasen/Weingart 2006: 19ff, 20, German original). In the Austrian context, the company-character of universities has particularly been emphasised with the University Act 2002. The rector has much more power than before and has autonomy over budget and personnel decisions. Amongst others, a new body called the university board that is meant to be functionally equal to the "board of directors" in private companies has been introduced. These boards mainly include members elected by the university senate and members appointed by the federal government – a measure by which the federal government secures some influence over seemingly autonomous universities. It has been criticised that even if they were installed to ascertain that universities are responsive to societal needs, university boards are often dominated by stakeholders from industry and the needs of industry rather than society as a whole (Gulas 2006). In Austria, about 32% of the members have a background as businessmen/entrepreneurs, 30% are scientists, 20% hold high positions in art and culture, the rest is subdivided into medicine, management and law. Less than one third of the members are female (Ibid: 85f).

In this context universities have been described as undergoing a corporatisation – a transformation in which they increasingly assume a corporate character and that is

informed by neoliberal policies that refer "to the doctrine and campaign for internationalisation of market economy, for intensive society-wide privatization as well as extensive globalised market deregulation". Its main implications are "deregulation, public disinvestment, and 'market-first' ideology" (Canaan/Shumar 2008: xiiif). The theory of New Public Management has become a main vector in this change as "a movement of thought and philosophy of public sector reform" that "managed the introduction of instruments and a logic of private organization into the university" (Bousquet 1998, Edu-factory Collective 2009: 8).

In summary it can be said that due to changing societal demands – particularly the demands of a knowledge-based economy – policies of autonomisation have reshaped the organisational structure and funding basis of academic education and research (cf. Felt/Glanz 2004). In doing so, they have become redefined as more service-oriented institutions and have introduced new modes of coordinating and steering academic research.

2.4. Ways of Conceptualising Change

The meaning of these transformations has been subject to heavy debate in which strong opponents, optimistic proponents and critical observers have met. The general trend that they describe is dissolution of boundaries between science and society and – more generally – a postmodern trend of transcendence of the institutional and cultural boundaries created by modernity (e.g. Gibbons et al. 1994, Latour 1998). Authors have claimed that in this process, science and society no longer evolve in different spheres and apart from each other with their distinct sets of norms but rather co-evolve (Gibbons et al. 1994), converge (Kleinman/Vallas 2001, 2007) and are co-produced (Jasanoff 2004). In their concept of mode-2 science, Nowotny et al. (2001) postulate a completely new constellation of knowledge production processes in which many more actors than before are involved. Many of these theories surprisingly agree in a couple of arguments, amongst which are: (1) that academic institutions have lost the monopoly – or predominance – over knowledge production; (2) that knowledge production is no longer organised along the search for the basic laws of nature; (3) that quality criteria of research are defined beyond academic criteria along additional social, political and economic criteria; and that (4) knowledge production has become accountable (Weingart 1997: 2f; cf. Gibbons et al. 1994: 4f, Funtowicz/Ravetz 1993: 121). The underlying argument is that academia – and the social fields that surround it – cease to be clearly defined by clear-cut norms, codes and practices. Rather, they say, qualities of different social worlds are becoming transversal and contested in complex political processes.

Authors have found several metaphors for the transforming relationship between science and society. A very popular one is the contract-metaphor that is used not only in policy documents and debates (Slaughter/Rhoades 2005: 537) but also in scientific reflection of the relationship between science and society. Hessels/van Lente for instance argue along Guston/Kenniston (1994: 5) that

> …the contract is a useful metaphor because:
>
> - it 'implies two distinct parties, each with different interests, who come together to reach a formal agreement on some common goal',
> - a contract is negotiated, 'arrived at through a series of exchanges in which each party tries to secure the most advantageous terms',
> - a contract 'suggests the possibility of conflict – or at least disparity of interests'; and
> - 'contracts can be renegotiated if conditions change for either party'. (Hessels/van Lente 2009: 389)

Besides other reactions, a global wave of protests and occupations of academic institutions since 2009 has vividly shown that the trajectories of this change – and particularly the commercialisation of academic institutions – are highly contested. As societal actors, students and academic workers raised their voices, demanded democratisation and de-commercialisation of universities as well as space and time for collective debate about the role that academic institutions (should) have. Amongst others, they reclaimed definitional power over their own role in an emerging knowledge society. Other societal actors – such as the federal government – clearly had different visions of how to deal with changing conditions in academic institutions and also made use of different means in exercising them. Contrary to what the contract-metaphor suggests, there are not two monolithic actors (science and society) involved in what is called a re-negotiation of the role of science in society and in shaping the policies linked to it. As Slaughter/Rhoades convincingly lay out, the idea of social contracts is

> …at once a theory and a policy strategy. As a theory, it explains federal support for science as based on a tacit, trust-based social contract between society and science. As a strategy, it is… a rhetorical strategy to obligate and direct the power of the state, and a (depoliticized) discourse, which encourages public support for professional and scientific expertise. (Slaughter/Rhoades 2005: 537)

As they continue, it seems that the social contract metaphor "is underspecified to the point where it is difficult to determine how the contract works, to whom the contract applies, for what duration, and with what resources" (Slaughter/Rhoades 2005: 537). I would add that the term "contract" implies a negotiation between all actors and also implies that involved parties have in some procedure signed this contract in freewill or have at least agreed under conditions of informed consent.

The notion of a science-society contract seems to mask that the players involved in this process are not equal players, that some are better equipped in asserting their interests, their norms, codes and practices. It tends to implicitly cover (im)balances of power and hierarchies in decision-making procedures. In doing so, they obscure our view on how changes in research cultures are actually taking place and what role knowledge producers re taking on in this process. It also blinds us to more subtle dynamics of change that the dissolution of boundaries between science and other societal spheres brings about. Social empirical studies have suggested that, for an understanding of how policy-changes on the macro-level articulate into transformations in academic research cultures on the micro-level, we might have to consider more subcutaneous dynamics. When boundaries between science and other societal fields dissolve, territories open up that become highly contested and objects of colonisation. Particularly on the level of the everyday, these processes are beyond formal negotiation. Rather, it has been suggested that they operate on the more subtle levels of informal codes and practices. For the question of how the relationship between science and economy is changing Kleinman/Vallas contend

> …that cultural traffic between university and commercial science has increased, blurring the boundary between them and generating a new and often contradictory knowledge regime, the product of a growing confluence of organizational logics that had previously been distinct. The emergence of this regime… holds important implications for prevailing theories of university–industry relations and of organizational change as well. (Kleinman/Vallas 2007: 1)

On the grounds of empirical studies, they conclude that certain trends in who succeeds in this colonisation process seem to be showing. They speak of an "asymmetrical convergence" of the corporate and the academic domain, saying that

> …although the emerging hybrid regimes are constructed of codes and practices from both sides of the divide between industry and academia, 'in the last instance' it is the logic of profit that is shaping this process. (Kleinman/Vallas 2005: 37)

Similarly, Marc Bousquet has suggested for the US that the university starts functioning along parameter of the corporate world and that the "knowledge business" has started importing "conditions, mentalities, and habits from academic labour" (Bousquet 1998, cf. The Edu-factory Collective 2009: 8). What I point to here are two crucial aspects of recent changes: the reciprocity of influence between the spheres of science and society/economy and the level on which the above authors have located changes. Rather than direct links between universities and corporate organisations, they identify "subtle, systematic influences" by which "academic organizations increasingly come to adopt practices… that were formerly specific to the corporate domain" (Kleinman/Vallas 2001: 453) or situate changes on the level of tacit conditions such as subjectivity and conventions of doing things (The Edu-factory Collective 2009).

In an attempt to trace the variety of factors that need to be considered when we want to understand the transformation of research cultures on the micro-level, Felt/Fochler/Sigl (2007a) have identified different social fields and rationalities that might be impinging on how research cultures in the life sciences are working today: it includes the knowledge economy but also considers new temporal regimes, new work cultures and intensified internationalisation of academic research cultures. In demonstrating the multitude of rationalities, codes and forms of practice that might be involved in shaping actual research practices, they used the metaphor of a "puzzle" (see Figure 1): in taking the field of the life sciences as an example, the picture shows institutions, practices of knowledge production, features of the academic career, output criteria, forms of social organisation, innovation models as well as ethical considerations that all shape research cultures today. In doing so it is very clear in showing that there is no clear-cut boundary between science and other societal actors.

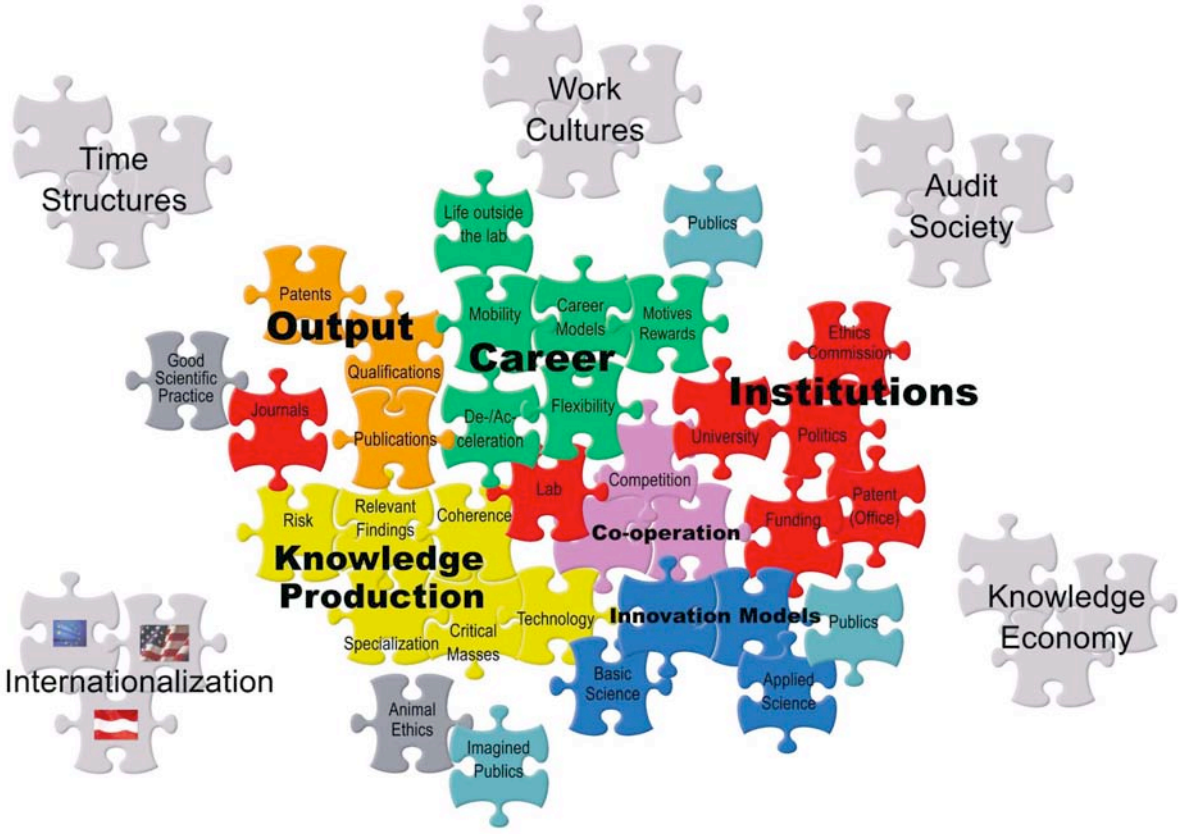

Figure 1: Living Changes in the Life Sciences. A Puzzling Experience (Felt et al. 2007a)

In reading the puzzle-metaphor upside down, the visualisation intends to convey that the different elements of the puzzle do not necessarily finally fit together as an actual puzzle. They are overlapping, cross each other or seem to push each other aside. Different interests and rationalities do not smoothly complement each other but create frictions. Deliberately, the figure also shows free spaces between the puzzle tiles: on the one hand they leave room for hitherto unidentified or just

emerging rationalities, codes or practices and on the other hand allow for conceptualising spaces of negotiation where researchers themselves juggle the puzzle tiles, push them aside, make them fit or create new ones.

In this line of thinking, science and society are better described as heterogeneous fields. Transformations are not thought of as one-way influences from one field to the other and the implications of a changing role of knowledge production for academic research cultures are thought of as far from determined by single policy measures or societal actors. For understanding the workings of academic research it pictures them in the cultural, political and economic landscapes in which they are embedded and whose subtle systematic influences impinge on the „tacit governance" of academic research on the micro-level (cf. Felt/Fochler 2010). Such tacit changes seem to be at least partly outside the realm of macro-political decision-making procedures. Its dynamics still remain underexplored, as does the question how researchers contribute to this tacit governance in their everyday decision-making. As the metaphorical abstract at the beginning of this chapter tried to illustrate, the newly emerging landscapes in which traditional parameters of doing an academic career seem to have become partly destabilised, while other potentially promising paths are on the rise. Academic researchers still seem to be struggling to find points of orientation and ways of manoeuvring within them.

3. Governmental Precarisation

A second line of transformation that is interesting to look at when we picture young life science researchers' experiences of uncertainty is an increase of uncertainty experiences on a broader societal level. Within social and political sciences, but also in philosophical theory and theory-building of social movements, diagnoses of the present have tended to be clustered around the observation that experiences of uncertainty are becoming more pervasive in society than they had been in previous decades. In the following I will refer to such generalised uncertainty-experiences in terms of precarity. Recently, authors have drawn attention to a possibly broader relevance of this phenomenon. In a nutshell, their hypothesis is that uncertainty-experiences suggest certain liabilities in acting and deciding and thereby can change societal dynamics from below. Within these debates there seems to be almost unanimous agreement that in contemporary societies an intense experience of uncertainty is thriving. However, academic authors, media or political discourse have found very different terminologies for talking about the observed phenomenon, making communication about it often difficult and confusing (cf. Freudenschuß 2009: 2). Since terminologies always carry certain meanings and hidden hypotheses, I will in the following clarify the specific meaning of the terminology that I chose to operate with in this thesis: in particular, I will distinguish between the concepts of precariousness, precarity and governmental precarisation.

To begin with it needs to be said that an experience of uncertainty – particularly its existential form in the face of hunger, illness or mortality – is of course not a new phenomenon and that communities and individuals have always been developing ways of living with and protecting themselves and each other from negative effects of uncertainties. In historical review, we can observe that the dominant mode of coping with uncertainties has changed along societal transformations and in particular also along the relations of (re)production. During feudalism, paternalistic care such as in vassalage or in domestic clan structures became a predominant form of protection. Alongside however, other forms of care have existed that built on a generic societal solidarity and a shared use of commons. With the emergence of capitalism then, property-based security systems came to the fore.

It is only after the Second World War (WWII) that institutions of social security as we know them today (such as social insurance) were institutionalised in many, particularly Western, industrialised countries. The latter have a work-based character – meaning that those who are engaged in wage labour (and have the right

passport[4]) are included in a community of protection. These largely build on standard employment patterns that have developed along Fordist relations of production and work (cf. Boltanski/Chiapello [1999] 2006: 28). The establishment of collective structures of social security and protection – such as wage agreements, labour legislation, public pension schemes or unemployment insurance – has at some point been so extensive that European societies of the 1960/70s were even labelled "societies of insurance" (Castel 2009: 24, German original: "Versicherungsgesellschaften"). The increasing disintegration of standard employment patterns and flexibilisation of work cultures has however started challenging such work-based systems of protection and have vehemently put the question of societal protection on the political agenda again.

Before we start exploring this crisis of social protection, we need to clarify some terminology on a conceptual level. What do I mean when we talk about "the precarious"? Is there a difference between "precariousness" and "precarity"? And how is it different from "governmental precarisation"? This obviously depends on how those terms are defined. As an overall classification of experiences of the precarious – as a kind of umbrella term for uncertainty experiences – Isabel Lorey's distinction between precariousness, precarity and governmental precarisation is useful for singling out the different levels on which the debate about uncertainty-experiences is carried out. In her understanding, the basic experience of uncertainties as discussed above has always been a permanent "condition of every life". Even if "in historically and geographically very different variations", it is an "existential common of all living beings". For this very basic experience of uncertainty, Lorey reserves the term precariousness. Besides it, Lorey distinguishes two other dimensions of the precarious: Precarity and governmental precarisation. Following her understanding, precarity "denotes the effects of different political, social and legal compensations for general precariousness". It is a notion that acknowledges the unequal ways of distributing social protection from uncertainties – i.e. an unequal distribution of precariousness. Her notion of governmental precarisation makes the claim that such unequal distribution of uncertainties can be and is operating as a governing technique that might entail wider social and societal implications (Lorey 2011: 1, 4). All three dimensions are understood as relational – i.e. they do not exist *per se* but are the result of societal ways of identifying and coping with uncertainties, of ways in which people have learned to mutually protect each other from precariousness and have found ways of excluding certain (groups of) people from their communities of protection.

[4] In fact, historically speaking, welfare was systematically organised along permanent residence since the development of cities (Castel [2000] 2008: 47ff).

3.1. The New Social Question

Using this classification, we can say that most lines of the academic debate about experiences of uncertainty are located within Lorey's dimension of "precarity". Sociological work within the past two decades has widely diagnosed a situation in which former economic certainties via wage labour are disintegrating. Standard employment patterns, such as full-time and long-term jobs are becoming fewer and fewer. Anthony Giddens' book *Sociology* – the probably most widespread handbook of sociology – might serve as an example here. He writes that

> …the phenomenon of job insecurity has become an important topic of debate within the sociology of work. Many commentators and media have suggested that there has been a steady increase in job insecurity for some thirty or more years and that this insecurity has now reached unprecedented heights in industrialised countries. Young people can no longer count on a secure career with one employer, they claim… Despite the benefits of flexibility at the workplace, the argument continues, we now live in a 'hire-and-fire' culture where the idea of a 'job for life' no longer applies. (Giddens 2006: 777)

This hire-and-fire culture is empirically grounded in an increase of "irregular or atypical" forms of employment or "contingent work" – and referred to in terms of a process of flexibilisation or casualisation. On a policy level, such changes are often encouraged as a need to adapt to a globalisation of competition and technological progress, such as in a recent document of the European Commission:

> Companies are under increasing pressure to adapt and develop their products and services more quickly. If they want to stay in the market, they have to continuously adapt their production methods and their workforce. This is placing greater demands on business to help their workers acquire new skills. It is also placing greater demands on workers with regards to their ability and readiness for change. (Neacsu/Baldan 2008: 794)

The line of argumentation is neoliberally accentuated in that the driving force of such changes is the competitive capitalist market economy to which the social world – "business" and "workers" themselves – have to adapt. Within the European Union (EU), the trend towards flexibilisation within the past decade has been shown empirically: flexibilisation is said to have increased job numbers, but this increase is to be found in subcontracted labour, temporary work or the low-pay sector to significant extents (Kok 2004: 12, 37). Within academic, though also in media or public debate, concerns have risen that the embracing of "flexibility" as a guiding concept for economic growth has tended to make invisible its exploitive downsides – a condition that Pierre Bourdieu termed "flexploitation" almost 15 years ago (1997, German original).

Within the past decade the academic debate about uncertain working conditions has widened and differentiated. Flexible working conditions are not only seen as bearing uncertainties because of uncertain economic maintenance, but because they seem to be de-coupled from established and institutionalised forms of social protection and solidarity. Rather than adapting to flexible working conditions, they are still oriented on standard full-time and permanent contracts. In his book *The New Social Question*, Robert Castel has suggested that contemporary European societies experience a weakening and dissolving of structures of protection and insurance – a process that has been discussed in terms of a disaffiliation between the individual and such structures (cf. Castel 2009: 29). In this context it needs to be mentioned that disaffiliation has always existed in work-based social protection systems: They have been disadvantageous for people who work part-time or rely on reproductive – and mostly unpaid – labour (such as housework or care for others). Recent changes in work cultures, however, have started to leave ever more working people with less social protection and people therefore lose legitimacy; e.g. freelancers have to take care of social insurance themselves, have no (proper) union support and are not included in wage agreements. Said more abstractly, socially established and institutionalised ways of coping with uncertainties partly cease to suit newly evolving work cultures and render formerly well-protected social groups with less social protection. Castel und Dörre therefore argue that with a welfare state and a union movement that are a crisis, we face a qualitatively new social question (cf. Castel/Dörre 2009: 11, Castel [2000] 2008).

Since these concerns have been raised, there have been attempts on a policy-level to conciliate flexible working relations with social security. Flexicurity-programmes have aimed at considering both

> …the advantages of flexibility for employers (and some employees), making it easier to hire and fire [and to increase] the pay and welfare entitlements of flexi workers over time… and… include(s) strong provisions for those who are temporarily unemployed in flexible labor markets. (Ross 2008: 40)

As a concept, flexicurity was pioneered in Denmark and the Netherlands in the 1990s and has even been adapted by the EU as part of the Lisbon Process. It remains to be seen, however, whether such programmes will keep pace with the changes ahead (cf. Wilthagen et al. 2003, cf. Ross 2008: 40). In the meantime the have already met substantial criticism saying that most seem to have focused on the flexibility-dimension while disregarding the security dimension (cf. Leschke et al. 2006: 19). More substantial criticism has argued that flexicurity – as a conceptual framework for relieving contemporary uncertainty-experiences – disregards the very quality of changes in the organisation of labour. They situate casualisation and disaffiliation within wider societal implications of neoliberal policies of the "slender state", forcing the disintegration of important buffers between the individual and

the contingencies of market economy. Without providing a structural alternative, neoliberalism has sidelined a solidary mode of coping with uncertainties (c.f. Beck 1986: 159). The result is a privatisation of dealing with precariousness, i.e. an increase of individualised, social risks (cf. Bröckling 2007: 196). Zygmunt Bauman's exploration of this situation is exemplary for the academic discussion:

> The postmodern society has proven to be an almost perfect machinery of articulation – one that interprets every present or future social question as individual worry… the most fruitful of all privatisations was the privatisation of human problems and of responsibility for solving them… Privatised ambitions define frustration as a private matter from the very beginning… uniquely incompatible with being transformed into a collective grievance. (Bauman 2005: 411ff)

In this context, institutionalised forms of solidarity – like unionism – have started losing affiliation with everyday working realities. As Finn Bowring has put it, the new regime of flexibility seems to have swept "away the remaining institutions and bonds of social solidarity" (2002: 160). Like other forms of social protection, unions have become specialised in representing interests of workers under conditions of standard employment. Despite efforts in adapting their activity to flexible working conditions (such as the GPA-djp in the Austrian context) and the reorientation of trade unionist strategies (towards concepts of organising, cf. Schmalstieg/Choi 2009), unions still seem to be unable to stop their membership declines – particularly in work sectors where precarious working conditions are becoming predominant. It is these new relations of work that the terminology of precarity refers to.

In summary we can say that precarity is the result of a process of redistribution of social responsibility for dealing with uncertainties from institutions to the individual; or in other words, institutions have (partly) withdrawn from being a buffer between societal crises and the individual. This de-collectivisation of dealing with uncertainties creates a new immediacy of uncertainties: societal crises come to be experienced as individual crises (cf. Castel 2009: 25, Beck 1986: 159). Risks, problems but also forms of social acceptance are shifted to individuals while state institutions for minimising risks and for solving social problems are disintegrated and replaced by private organisations and the individual itself. It has been hypothesised that this neoliberal idea of individual responsibility promotes an experience of life as exposedness, vulnerability and threat. For Paolo Virno it is one of the core problems of precarisation that it becomes impossible to distinguish between a limited fear and a delimited anxiety. In the absence of intermediate institutions that deal with uncertainties on a societal level thus, uncertainties appear to generalise in a culture of angst (Virno 2005: 35ff, cf. Neundlinger/Raunig 2005: 15ff).

Genealogically, the term precarity was adopted to the academic debate only after it was fabricated as a political concept by self-organised social movements against the developments described above. They aimed to address the downsides of the flexibility-regime and to reflect its wider societal implications. Around 2000, it first emerged in the region of Milan/Italy – a region with strongly increasing precarious working conditions – to make visible a situation in which flexibilised and destabilised working conditions had not only come to mismatch institutions of the welfare state but also established institutions that represent the interests of workers. The notion of precarity has since then been substantiated and actualised with political interventions such as the organisation of MayDay-parades on the 1st of May (the International Worker's Day) or the creation of San Precario and Santa Precaria as the patron saint of precarious work and life. Social movements have therewith managed to rupture the symbolic order of neoliberal labour policies and to start a line of thought for inventing new collective ways of coping with and resisting a "steady march of contingency" into work and life (Ross: 2008: 34). As a political concept, precarity was thus meant to re-create a collective consciousness of a shared experience of uncertainty – however multiple and individualised the precarious are in shape and constitution. In doing so the Mayday-movement understands pervasive uncertainty-experiences not as quasi-natural and unavoidable but as structurally produced. One line of argument in their debate is that since wage labour increasingly loses its function as source of a plannable, secure and dignified life, social security/protection needs to be provided unconditionally and delinked from the employment history of a person (cf. Altvater/Mahnkopf 2002: 36). Similarly, this line of thought argues that the advocation of interests needs to be delinked from wage labour and claim a „truly life-oriented syndicalism (biosyndicalism)" that „operate(s) on the immediate level of common life experiences" (Tsianos/Papadopoulos 2006: 7).

In academic, public and media discourse, precarity is often equated with what we have discussed above as flexibilisation or casualisation. Since there is no appropriate translation for casualisation in the German language, the English-speaking debate is often translated in terms of precarisation ("Prekarisierung"). However, my understanding of precarisation is rather oriented on the broader societal implications that the concept of precarity originally entailed. It addresses a "shared concern… about the insecurity of all aspects of… lives" as Andrew Ross has put it (2008: 34f). The precarious experience is thus not reducible to a concrete working situation but includes a range of phenomena along the mismatch of contemporary forms of social security with contemporary forms of organising societal labour. The public and policy debates around "unemployment" may serve as an example here for the broader implications that precarity has for individuals' relations to their lives and the demands it places on their subjectivities. On the policy-level, the "question

of unemployment" is mainly answered by the idea of "employability" – i.e. the fitness of the individual for being employed and competing for a job. Working on this fitness by being prepared for re-education in a lifelong learning process is presented as the only way of confronting this risk. The concept of employability thus frames the "question of unemployment" in a specific way: namely as a quasi-natural risk that best prevented by individually working on one's skills and staying employable. At the same time the concept of "employability" disregards other explanations – for example one that sees unemployment as socially produced, as resulting from certain ways of organising and distributing work and societal wealth. From this other perspective, unemployment does not appear as quasi-natural risk but as a risk that is constructed by a capitalist employment system and is thus avoidable by different ways of organising and distributing work and societal wealth (cf. Wolf 2009). The concept of employability is thus implicitly promoting an individualisation of responsibility: it shifts social responsibility of institutions to self-control, to working on one's capabilities and productive potentials. In doing so, it embodies a key dimension for understanding the modern notion of social risk, a "negative individualisation" that is characterised by high responsibility, little certainty and stable relations (cf. Castel [2000] 2008: 401f). Recent debates thus suggest a re-framing of the meaning of social (in)security insofar as it reaches *beyond* concrete work relations.

Existing studies of precarity however suggest that it depends on the respective cases and living spaces, what aspects of this *beyond* to come to the fore and gain relevance in the experience of uncertainties. Castel/Dörre suggest that it is in particular the kind of activity that people are engaged in and the way that people learn to make sense of them (in terms of social relevance, a personal relation to the task, the involved social relations and social status) that steer the way in which researchers integrate uncertainty-experiences into their lives (Castel/Dörre 2009: 17). For the case of young academic life science researchers, it is the activity of doing research – of seeking and finding new knowledge – including the uncertainties of the process – that need consideration. While uncertainties of the scientific enterprise have been given attention with regard to it's implications for societal development, little attention has yet been paid to how the people that are engaged in the actual knowledge production process experience and cope with its inherent uncertainties in the everyday.

3.2. Uncertainties along the Science-Society Nexus

While in the modern condition, science has been a tool for achieving certainty, and the established boundaries and mutually ascribed roles between science and society used to serve as coordinates for decision-making procedures (cf. Krücken 2006: 7),

the situation seems to have become reversed in the postmodern condition. Bruno Latour has suggested that, while we were once accommodated to – what he calls – a "culture of 'science'" in which knowledge production was given authority over defining truth and was thus a source of certainty, we have now been entering a "culture of 'research'" in which new knowledge production tends to question certainties rather than creating certainty (1998). A team of authors around Helga Nowotny have picked up on this thought in their theory about an "age of uncertainty". They see new uncertainties as related to a new mode or "more open systems" of knowledge production that question science's role as an institution of insurance and protection (2001: 47).

This postmodern experience of uncertainty can be understood as related to the historically previous experience of certainty. It seems as if modernity was only a short historical trip to certainty: "the zenith of modern certainty about the future (was) quite short" (Böschen/Weis 2007: 13). The radical questioning of the foundations of modern societies loosens the – supposedly fixed and stable – coordinates for decision-making and every-day life. When we are talking about uncertainty-experiences today, we are talking about "social uncertainty after certainty" and protection (Castel 2009: 27), an after that many see as closely related to the before, in fact an after that was already laid out in the before – in particular in the emergence of a new quality of uncertainties alongside the techno-scientific progress of modernity (cf. Beck 1986: 25ff). In his thinking, the postmodern obsession with unpredictability is not an inversion of the obsession with security in the modern condition, but must be understood as a necessary development that originates in the latter. The book *Reflexive Modernisation* hypothesises, that we live in a stage of modernity that questions its foundations from within. As an internal side effect of modernity, it is argued, that certainties are re-negotiated and conquered (Beck et al. 1996: 10). Karin Knorr-Cetina concedes with the authors when she writes that in „the risk society"

> …we are increasingly confronted with risks and uncertainties emanating from the very technological, scientific and other achievements of modernity… (Knorr-Cetina 2007: 370).

In other words, the perception of uncertainty is not only high because futures are increasingly uncertain but because it is no longer clear how certainty can possibly be achieved (cf. Böschen/Weis 2007: 169).

What modern cultures had supposedly brought under control – a certainty about the future – is subject of negotiation and societal struggles once more. This observation rests in a paradox of our postmodern relation to the future: It is argued that in the postmodern condition we actively seek change (and instability) and that the extent of change and acceleration seems to have reached a point where modern

options for dealing with the future have become impossible and problematic (cf. Rosa 2005). While in the modern condition we have become used to the certainty that we can actively change, affect and control our futures we have at the same time assured ourselves of the many options that the future provides. The paradox lies in the incommensurability of these positions. While we actively plan and permanently take action in controlling our futures, planning has changed its character; instead of actually creating futures, it has become a way of permanently sorting out uncertain opportunities. Future has thus become more indeterminate and evacuated of certainty. Brown et al. have put this in the following words: "Saturated, as it is with competition, risk and knowledge intensity, planning has become more, not less, indeterminate" (Brown et al. 2000: 6). The belief that we are able to change our destiny has resulted in a stronger orientation toward the future - a future, however, that "is fundamentally uncertain and unknowable" (Adam/Groves 2007: 12). We have become obsessed with planning and unpredictability, and at the same time, as Brown/Michael have summed this up precisely:

> Those instruments devised to create knowledge about the future and to facilitate its better management (scenarios, foresight initiatives, statistical probabilities, future assessments in financial services, etc.) have a tendency to confront us in the present with glaring uncertainties, and not least when outcomes routinely deviate from what has been predicted. (Brown/Michael 2002: 4f)

It seems as if the chance to colonise and shape the future with our plans tends to lead to a growing preoccupation with the future and a growing awareness of future risk (Adam/Groves 2007: 11ff), a "shared escalation of uncertainty" (Brown/Michael 2002: 4f). A changing relationship between science and society and the focus on knowledge production and new technologies in the development of our societies thus seems to have introduced novel sources for uncertainties (cf. Boltanski/Chiapello [1999] 2006). As Sheila Jasanoff has stated exemplarily, science and technology are now seen as accounting

> …for many of the signature characteristics of contemporary societies; the uncertainty, unaccountability and speed that contribute, at the level of personal experience, to feelings of being perpetually off balance. (Jasanoff 2004: 13)

Barbara Adam and Chris Groves have identified five techno-scientifically-mediated reasons for this increasing experience of uncertainty: a logic of progress and the acceleration of change that comes with it; the decomposition of structural security; the increase in mobility (of people, objects and information); information and communication technologies that reduce periods of time to almost nothing and the decoupling of future from its concrete socio-economic, political and socio-ecological processes and events (2007: 12). Within Science and Technology Studies there is a long tradition of assessing and analysing these uncertainties and possible ways of dealing with them on a societal level. Departments for technology

assessment have been established and the reflection on the unpredictable effects of new knowledge and new technologies has been put on the agenda of policy makers.

What is, however, a stunning gap in reflecting the implications of necessary unpredictability of techno-scientific progress is the question of what they mean for those who engage in knowledge production at an everyday level. What is important to hold on to is that a broad range of STS literatures is concerned with reflecting a societal constitution that accepts and welcomes techno-scientific progress but still has not learned to understand the implications of welcoming the unpredictability that they introduce to the workings of societies, institutions, work cultures and living spaces. In situating knowledge production and its inherent uncertainties at the core of our knowledge societies/economies in the making, however, these uncertainties have come to play an increasing role in the everyday of ever more people who inhabit our societies and who undertake the uncertain activities of seeking new knowledge. On this particular aspect of techno-scientific progress and of creating knowledge societies/economies and on its implications for experiences of the everyday, there is still little debate, empirical investigation and academic reflection. The particular case study of the experience of precarity in academic life science research contexts is situated within exactly this gap and reflects the implications of working and living with epistemic uncertainties in the everyday.

3.3. Governance by Precarity

A second gap within which this study is situated is empirical research on possible governmental implications of uncertainty-experiences. What we have already discussed above is that precarity in contemporary societies/economies seems to have a dimension *beyond* social security. Various authors have hypothesised that the uncertainty-experience has a governance dimension: in particular regarding the governance of the everyday and – through the everyday – of the workings of contemporary societies: Pierre Bourdieu for example has defined precarity as paradigm of governance that not only produces a collective mindset of generalised uncertainty but unfolds performative power dynamics (cf. Bourdieu 1997). Pat O'Malley has proposed to understand "(a)ngst as a Neoliberal concept", a "politics of fear" that does not coincidentally emerge but is produced by an "uncertainty industry" (O'Malley 2004: 3ff). For Isabel Lorey, this constitutes a third dimension of the precarious: governmental precarisation. In her understanding, it is performative not only through a "destabilization through wage labor, but also a destabilization of ways of living and hence of bodies" (Lorey 2011: 1). She does, however, not limit precarisation to a mere instrument of power from above. Along a reflection of Foucault's art of governing, her notion of precarisation considers both the social and political production of conditions of precarity and the "active participation of each

individual in the reproduction of governing techniques" that serves subjugation but also enables social struggles and resistance potentials (Lorey 2011: 4). This concept also allows for rejecting a notion of precarity that refers to people as victims of precarity. Rather "individuals, who move in power relations, who are conducted and governed in them, are always 'acting subjects': subjects with agency. In acting they participate in the way they are governed" (Lorey 2011: 3). She argues that this self-governing operates along a special mode of subjectification, one that emerges along a condition where "fewer and fewer people are able to distinguish between the anxious worry about existential vulnerability and the fear that arises through precarization" (Lorey 2011: 5).

How far it is appropriate to describe and analyse dynamics of uncertainty-experiences in the everyday of life science researchers in the framework of governmental precarisation will be discussed in detail in part four of this thesis. Here I only want to give a few empirically informed examples that suggest such a governmental character. It is safe to say that there are abundant accounts of researchers that hint at newly emerging social and epistemic dynamics. One frequently brought example for that is that the tight temporality of employment is related to ways of conducting research and speeds of publishing:

> *…in the worst cases you have no other choice than saying: … whatever, we need this publication now. … let's knock it out! …* [that] *you… say: Ok, these are our data, they hint at the fact that this and that could be interesting to follow further…* [or] *that's a bigger project and we should employ a PhD student… that's how it should be. But you are forced into a pragmatism that is probably not always best for research.* (m4.2: 1274-85[vii])

Other accounts link flexible working cultures to dynamics in the availability of local expertise. Since there was no longer any core personnel, this researcher states that tacit knowledge is lost and communities of expertise can be ruptured:

> *…as a result of these short contracts that… on the one hand fresh wind is guaranteed. But the problem is that partly the contracts* [of successive personnel] *don't even overlap and much know-how is lost… sometimes you have a complete switch. A lot of knowledge is lost and you have to start from zero. I find that rather idiotic. Because… when you have invested a lot in someone and he has knowledge and he can't even hand it over before he has to go (.) it is also money that is wasted… And research* [quality] *obviously suffers from that.* (f7: 312-21[viii])

Another narrative strand in our interviews regarded the social dynamics that they saw emerging along individualisation and normative systems of reward in academia that indicated a focus on individual careers and a structural disregard of caring for the collaborative activities and reproductive work in the immediate lab environment:

> *…in the scientific world, the impact is all that counts unfortunately. That is very hard at the university… because of the lack of jobs… with the competition, and everyone must look after himself… But when someone runs the place – and he makes high-quality work and takes supervision very seriously… his name will never in the first or last* [most prestigious] *position in the authorship-list but he is just as important… But the impact factor is the only thing that counts for the people and not the human being… but when you work with so many people you also need managerial skills… The one has skills in being a researcher who is creative and has many ideas… And the other… looks after supervision and the whole environment, the infrastructure, the technical equipment… And that doesn't count in terms of impact.* (m3: 1148-69[ix])

In this context some researchers raised the concern that these dynamics might induce changes in the quality of published findings:

> *Because probably the research quality suffers when people have the feeling that they must publish, publish fast. I think that many bad things happen then. People don't do proper research any more and overall, there is not so much space for pure curiosity. This idealistic image of research; it might have been that way 50 years ago. It's gone now… And that's a bit unfortunate, right? Not only a bit, this has obstructed our future a bit. (.) At least my future.* (f1.1: 159-70[x])

These few quotes may suffice for indicating that certain conditions – such as high levels of competition, normative career standards or short employment periods are experienced as introducing certain dynamics into the everyday workings of life science labs. An in-depth analysis and a more detailed reflection on researchers as acting subjects within precarity – or governmental precarisation – will take place in part four of this thesis.

4. Empirical Material and Methodological Approach

For exploring my research questions I am working with a mix of different qualitative methods: mainly narrative, biographical interviews and group discussions with young life science researchers but, to a smaller extent, also with observations of everyday lab practices and analyses of the socio-political and institutional environments of life science research in Austria. Overall, my study builds on two research projects I collaborated on at the Department of Social Studies of Science at the University of Vienna. One of these projects also included (largely quantitative) questionnaires about professional life courses in the life sciences. Their analyses gave background information on researchers' educational backgrounds, professional biographies and some indications on their work place satisfaction and work-life balance (Felt et al. 2007b) that also informed the analysis for this study. In this chapter, I will first explore the socio-political embeddings of the empirical settings of my research before I discuss my empirical material and methodological approach in detail.

4.1. Situating the Empirical Settings

Along new policies, the Austrian science and research landscape has significantly changed over the past 15 years on the levels of decision-making structures, funding structures and steering of research activities. The field of the life sciences has gained particular attention in this transformation process. The "life sciences" as a field of research have in fact only slowly been established as a field of research in its own right in this process. Indeed there is still no clear-cut definition of what the life sciences actually are and researchers in the sample of this study often rather identified themselves with their educational – disciplinary – background than with a "field of the life sciences". The latter was rather understood as a collective label for a field within the biosciences and has emerged out of an interdisciplinary orientation of classical disciplines such as biology, chemistry and medicine. In a public and policy understanding, the life sciences are not only a field of research that is defined by epistemic affinity but are strongly defined by criteria that are external to the academic realm – particularly to the potentially marketable value of knowledge they produce and their potential of creating a job market in the field of biosciences, thus contributing to the evolving knowledge economy. As such, the life sciences were an important subject to research policies and newly evolving forms of governing science. The Austrian Council for Research and Technology Development (RFTE) – an advisory body for the governance of science in Austria that was founded in the year 2000 – had particular influence on their reorganisation. A large majority of its members are from industry, technology and the natural sciences and their

recommendations strongly focused on the respective research fields. Social sciences and the humanities only gradually entered their considerations (cf. Felt et al. 2007b: 6). Amongst others, the RFTE has identified the life sciences as field of high economic potential within the national research landscape (RFTE 2002). As a result, the Austrian government declared top priority for funding life sciences in 2000 and targeted research and educational programs have been set in place. Since then the life sciences have experienced rapid growth. The largest of the following funding initiatives was the GEN-AU programme with a total funding of 100m€ – subdivided into three subsequent three-year calls (Rechnungshof 2006). Altogether, GEN-AU was a large-scale, nine-year-funding instrument for the life sciences in Austria. In the programme's own description, the political and economical demarcation is openly addressed:

> *Genomics is nowadays regarded as a key area to the development of science but also of economy and society. The Ministry for Science and Research (BMWF[5]) developed an independent national genomic research programme that should help Austria gain international acknowledgement in the field of genomics.* (GEN-AU 2011)

With regard to the reorganisation of Austria's governance of knowledge production, the GEN-AU programme was part of a redistribution of the state's funding for research. In comparison to other funding instruments, GEN-AU had heavy monetary support: annually, it had at its disposal an amount that corresponded to one-tenth of the state's support for the most important basic-research fund, the Austrian Science Fund (FWF). In the same period, the FWF regularly had to struggle with cuts in funding by the RFTE[6]. Besides direct research funding, the field of genome research has also gained support on the levels of educating young life scientists, increasing support infrastructure (core facilities such as for the raising of laboratory animals) and support for the commercial development of a life science industry. The Ministry of Economics, Family and Youth (BMWFJ) for instance has – together with the Austria Wirtschaftsservice (aws; a development bank for small-companies) – developed a programme called Life Science Austria (LISA) that aims at contributing

> *…to the success of life science enterprises* [by offering infrastructure and assistance] *in the search for funding and finance… helping bring scientific discoveries to the market* [and] *providing general business consultancy, education and support to ensure healthy commercial development…* (LISA 2009)

Besides such efforts on the federal level, city governments have for the past 20-30 years helped to build local clusters of life science research. One of them is the Campus Vienna Biocenter (VBC) that has been growing since 1988, when a contract

[5] When the programme was initiated in 2000 the ministry responsible was still called "Ministry for Education, Science and Culture (BMBWK)".
[6] The cutback at one point reached 20% in total funding for the FWF (FWF 2003).

between the minister of science and Boehringer Ingelheim was signed. It included agreements on the joint use of infrastructure such as a library, core facilities and teaching programmes. Today it is a site at which a joint venture of academic research institutions, Max F. Perutz Laboratories (MFPL), works in close proximity to the (semi-) public Life Science Center of the Austrian Academy of Sciences (ÖAW) (such as the Institute for Molecular Biotechnology – IMBA, an initiative of the ÖAW and Boehringer Ingelheim, and the GMI – Gregor Mendel Institute of Molecular Plant Biology) as well as to a private research institution (Research Institute for Molecular Pathology – IMP, Boehringer Ingelheim). The overall aim of the VBC was to join academic and private research efforts to increase the marketability of biotechnological knowledge production and to serve as an "incubator for spin-off companies" (VBC 2003). In summary it seems safe to say that in the Austrian context the life sciences have an outstanding role in the reorganisation of academic activity. This makes this field of research an interesting field for investigating the new quality of entanglements between academic research cultures and new ways of governing, funding, employing and managing research(ers).

The empirical work for this PhD study was done within two of the above described settings: (1) a network project that was funded within the GEN-AU programme: "Genomics of Lipid-associated Disorders – GOLD II" and (2) several labs at the MFPL (VBC). Both settings have grown historically to embody transitional lines within the governance of academic research – such as increased external and project funding, a focus on international competitiveness, public-private partnerships and marketability.

GOLD II was a large interdisciplinary consortium of genome researchers and one in a row of three successive GOLD-projects that were funded within the Genome Research Programme GEN-AU. It came to be known as one of the most – if not the most – successful projects within the programme. Recently it has gained public attention through substantial international publication activities and national rewards (GEN-AU 2011).[7] GOLD was funded over the whole programme period, managed to repeatedly publish in the prestigious journal "Science" and its project leader was awarded the "Wittgenstein-Preis", a distinguished and highly remunerated prize for excellent scientists in Austria. GOLD II combined more than ten life science research projects at eight academic institutions, mainly operating in the cities of Graz and Vienna, where about 50 life science researchers were working on the project (GOLD 2012). The research area, genomics of lipid-associated disorders, was investigated from a variety of disciplinary and experimental

[7] GOLD II also included research projects on ethical, legal and social aspects (ELSA); one of them being GOLD II/C14, that I participated in for the work on this PhD thesis (see below).

approaches, ranging from microbiology over animal experimentation to genetic epidemiology. Along the project's duration, researchers within the GOLD consortium were able to enlarge the research field by acquiring additional funds such as a special research area fund (SFB –Sonderforschungsbereich) and a PhD programme (DK – Doktoratskolleg), both awarded by the FWF. The labs involved were small- to medium-scale labs whose personnel was only partly funded by the GOLD-project. The actual research was largely oriented at basic research but operated with a clearly circumscribed social and medical relevance (namely clarifying possible genomic causes for obesity and lipid-associated disorders and therefore contributing to medical treatment) in its grant applications and public appearances. Therefore overall, the GEN-AU programme and the research area it represents are good examples of target-oriented research funding and new governmental measures such as special research programmes and PhD programmes.

The second empirical setting, the MFPL, was an institutional brace for labs of the University of Vienna and the Medical University of Vienna at the VBC. It is one of the biggest, most important and expanding sites for Life Science research in Austria. Building on these observations of everyday research practices in two labs for two weeks respectively, I will, in the following, describe the composition of two labs at the MFPL in more detail to give the reader a sense of the social structures of research and work in academic life sciences: The MFPL consists of about 40 labs from the Centre for Molecular Biology/University of Vienna and about 20 labs from the Department of Medical Biochemistry/Medical University of Vienna. Most of those labs are relatively small-scale labs of between two and 15 members. In 2011, about 470 researchers were working there. As a research-oriented institution, it is only marginally involved in regular undergraduate teaching. Educating master and PhD students, however, is seen as an important task. Statistically, PhD students constitute a major part of the MFPLs work force. Researchers at the MFPL are mainly employed in the framework of externally funded research projects or PhD programmes, only about 25% have university-funded positions. The research areas are situated across a range of disciplines from immunology over neuroscience to computational biology (MFPL 2012). The two labs that we observed defined themselves as a molecular biology lab and a biochemistry lab.

The lab leader (f) of the first was a professor and head of a department at the age of 54. When we first visited the lab in 2006 the lab had 12 lab members: five postdocs (2m, 3f), four PhD students (4f), one master student (f), one lab manager (m) and one technical assistant (f). Most of them worked full-time, only a female master student (who had a small child) and the technical assistant worked part-time. All but two researchers were working on third-party funded projects; the lab leader and one postdoc were university-funded. In comparison to other academic labs, the lab members considered their lab as relatively well resourced with regard to project

money and equipment, but poor compared to private research institutions. The lab space consisted of four rooms: two lab rooms and two offices. The larger office was shared by the lab leader and a man in charge of administrative tasks, and the smaller one for the postdoc with the university position. The other staff used lab benches as offices and writing desks. Most lab members were trained biologists, who had then specialised in molecular biology. The epistemic working unit of the lab can be described as secluded and translucent at the same time: it was frequented not only by regular lab personnel but also by students who did support work or researchers from other labs when they collaborated. During the two-year period of the empirical work, the fluctuation of lab personnel was very high: at the time of the first observation it was clear that four of the postdocs and PhD students planned to leave and that two new PhD students would come soon. After four years there was – except for the lab leader – only one of the original lab members still in the lab.

The lab leader (m) of the second lab was, at the age of 45, also the head of a department. At the time of the observation it consisted of 15 members: five postdocs (3f, 2m), seven PhD-students (4f, 3m), two master students (1f, 1m) and one technical assistant (f). The lab leader and one and a half postdoc positions were university-funded, all other lab members were employed on externally funded projects (most under the lab leaders' leadership) or individual scholarships; except for two part-time postdocs, all of them were employed full-time. Most lab members had studied chemistry and some had been trained at interfaces of chemistry, biology and medicine. They primarily situated their work as being within the field of biochemistry. Their core method required highly qualified personnel to operate the research machineries. The lab (and its members) subtly subdivided in two parts – a "wet lab" and a "theory lab". Their operating rooms and offices were also roughly divided along this dividing line. The "theory-lab" designed and conducted experiments and operated the larger technical equipment. The "wet lab" purified doses of substrate required by the experiments of the dry lab. On a regular basis, members of the lab collaborated with other labs that asked for their experimental expertise or their expensive technical equipment. Similarly to the first lab, the fluctuation of personnel was quite high: only five researchers (and the technical assistant) of the original 15 had stayed four years after the beginning of this study.

Despite the relatively strong links to policies of making knowledge production marketable, it was surprisingly characteristic for the lab groups under study that their research efforts were not directly aimed at collaborations with private companies; not one of the young life science researchers that I worked with was openly collaborating with Boehringer Ingelheim or followed a spin-off strategy. Rather, their research efforts were organised along academic performance indicators (in particular the amount and value of publications) and the requirements of academic career making (including stays abroad and international collaboration).

The empirical material for this PhD study includes qualitative interviews and group discussions with young researchers within both settings.

4.2. Material and Methods

Large parts of the empirical material that this thesis builds on were gathered during two collaborative research projects on research cultures in academia at the Department of Social Studies of Science in Vienna within the research focus "Traces of a Knowledge Society in Transition. Diagnoses, Analyses, Interventions" (Science Studies 2011). The empirical work was done together with project leader Ulrike Felt and collaborators Maximilian Fochler and Veronika Wöhrer.

The first project, GOLD II/C14 (Rethinking Biosciences as Culture and Practice: Tracing Ethics and Society in Genome Research – a Pilot Study), was part of the ELSA programme of GEN-AU[8] and its network project GOLD (cf. above). It was particularly designed for developing methods that would allow the biosciences to be studied as a culture and practice. The second project was the Austrian sub-project of the comparative research project KNOWING (Knowledge, Institutions and Gender: An East-West Comparative Study[9]). Its aim was to work out differences and similarities in cultures of knowledge production in different national or socio-political contexts. One of its empirical settings was life science research at the MFPL (cf. Felt ed. 2009). The two projects shared the basic approach to investigating academic (life science) research: Even if done from different angles, both investigated interrelations between science and society and traced transformations in academic research cultures along this interrelation; for instance we tried to develop a better understanding of how governmental measures, but also more subtle societal conditions such as public discourse, related to decision-making in everyday research practices. Their basic assumption was that science is not a self-contained system but that in scientific practices different societal fields – and their codes and practices – impinge on locally specific academic research cultures. A particular aim was to develop methodological tools for making this convergence more tangible in everyday experiences of academic life science research.

The research interest for this thesis developed along my collaboration in these projects. Since the project material was very rich in accounts of uncertainty-

[8] The project GOLD II/14 took place from 03/2006-06/2007 and was funded by the Ministry for Education, Science and Culture (BMBWK, as it was called at the time; currently, it is called the Ministry for Science and Research – BMWF) in the framework of the GEN-AU programme (www.gen-au.at/); Project leader: Ulrike Felt, Project collaborators: Maximilian Fochler, Lisa Sigl; http://sciencestudies.univieac.at/forschung/abgeschlossene-projekte/gold-ii/

[9] The project KNOWING took place from 01/2006-12/2008 and was funded within the „Science and Society"-domain of the Sixth Framework Programme of the European Union and took place in five European countries; Project coordinator: Marcela Linkova; Project leader of the Austrian sub-project: Ulrike Felt, Project Collaborators: Lisa Sigl, Veronika Wöhrer; www.knowing.soc.cas.cz/

experiences amongst the younger generation of researchers and a first analysis suggested that such experiences might contribute to transformative dynamics in everyday research practices, I chose to investigate this phenomenon in-depth. In doing so, I focused on the empirical work that we had done with young life science researchers and later complemented the empirical material with follow-up interviews with five of those researchers. This last stage of my research was funded by the Austrian Academy of Sciences as a PhD project (DOC – Manouvering Uncertain Research Landscapes) (see the table below for an overview of the involved projects and materials).

The core sample I selected for this thesis consists of 14 young researchers and includes three master students, four PhD students and seven postdocs. All of them had or were having their first work experiences in the academic life sciences at this time. As is usual for young researchers in academia, they held temporary positions of one to four years. Master students either held a small scholarship or – like most PhD students – were employed on the research projects of their lab leader or of a senior postdoc. One PhD was funded by a PhD programme and one held an individual fellowship. The employment conditions of postdocs were also diverse: most were still employed on their lab leaders' projects, some had a (part-time) university position and some had acquired fellowships or their own research projects. Several were employed part-time or were living on mixed incomes; i.e. had for instance two part-time contracts that added up to full-time employment. I was in contact with most researchers several times between 2006 and 2009: they were either interviewed twice, participated in both, interviews and group discussions, or were members of the observed labs. During this three-year period, most of them have advanced from being master students to PhD students or from being PhD students to postdocs, some postdocs had left for industry or non-research jobs and almost all had different contracts and funding in 2006 than in 2009. Even though their employment biographies were diverse, two characteristics seemed to unify the sample: almost all of them aspired to an ongoing academic career, although they suspected that this was a rather unlikely and uncertain path and, unlike researchers that would already be planning to leave academia for a job in industry or elsewhere, they tended to refer closely to the framework of expectations and demands within the academic system in their decision-making and in the experiences of themselves.

Along a "systematic triangulation of perspectives" the study combined different methods of qualitative research (cf. Flick 2005: 315, 2008: 55, German original). Narrative, biographical interviews, group discussions and observations complemented each other and opened up distinct perspectives on researchers' experiences of uncertainty. In the following I will explore how this multi-method-approach allowed additional dimensions of the uncertainty-experience to be grasped; particularly with regard to investigating the relationship between

individual and collective experiences, to exploring researchers' experience along changing positionings within an academic career and to linking everyday-experiences to more global and systematic changes.

Table 1: Overview of Empirical Material that was used for this PhD study[10]

Projects	**Knowledge, Institutions and Gender (KNOWING) 01/2006-12/2008** Project leader: Ulrike Felt Collaborators: Lisa Sigl, Veronika Wöhrer *funded by: EC/FP6*	**Re-Thinking biosciences as culture and practice (GOLD II/C14) 03/2006-06/2007** Project leader: Ulrike Felt Collaborators: Maximilian Fochler, Lisa Sigl *funded by: BMBWK*	**Manouvering Uncertain Research Landscapes (DOC) 04/2009-03/2011** Project leader: Ulrike Felt Fellowship holder: Lisa Sigl *funded by: ÖAW*
2006	Life Course Questionnaires (LCQ, largely quantitative)	6 narrative, biographical interviews (qualitative)	
2007	8 narrative, biographical interviews (qualitative) 2 weeks of observation in 2 labs 2 group discussions	2 group discussions	
2009			5 follow-up, qualitative in-depth interviews
Empirical Settings	Vienna Biocenter/ MFPL (Max F. Perutz Laboratories); small-scale labs	Genome Research Programme GEN-AU; small-scale labs within the network project GOLD II (Genomics of Lipid-Associated Disorders)	MFPL & GOLD II 5 interviewees from the samples of KNOWING & GOLD II/C14

The most important material I worked with were transcripts of 14 narrative, biographical interviews.[11] They start with an elaborate biographical part in which interviewees were asked to discuss their motivation for engaging in research, their ways of becoming part of the scientific community and of manoeuvring through the institutional landscape. They were also asked about opportunities they grasped and difficulties and uncertainties they experienced along the course of their professional life. This biographical part was followed by questions on how researchers developed ideas for new research projects, how they designed and carried them out, how they published their work and how it was recognised and assessed. The interviews thus

[10] Additionally – but only selectively and as contextualising material – an analysis of interviews and LCQs with more senior researchers (professors, lab leaders) entered my reflections.

[11] Six of them were taken within the project GOLD II/C14 and had a rather extensive narrative, biographical character (about three hours). The eight interviews that were taken from the project KNOWING were somewhat shorter (about 1½-2 hours).

included broad retrospective and prospective reflection about careers and research plans, narrations on how researchers sounded out research and employment opportunities and on how they dealt with obstacles and uncertainties in their professional careers. Within all this, particular emphasis was placed on elucidating links between individual agencies and the societal and infrastructural contexts the researchers and their labs were embedded in; such as societal expectations, reward structures within academia or university policies. In that sense, the interviews also had reflexive character: researchers were asked to discuss their environment, their roles within it and the reasonings behind their decision-making and actions (cf. Rosenthal 2005: 50). This allowed for a better grasp of how researchers conceptualised their spaces of manoeuvring (their confinements but also their degrees of freedom to decide and act) and of contributing to the transformation of their environments. The overall aim was not to reveal how their lives actually were but to shed light on researchers' "biographical work" – i.e. the ways in which they made sense of how they were moving within their societal contexts (cf. Fischer-Rosenthal/Rosenthal 1997: 135). Put differently, the biographical method was not chosen to reconstruct past biographies but rather as a tool for understanding present experience.

In doing so, this study followed a reflexive approach towards biographical research. To explain this, I will draw on the methodological debate about biographical research that dates back to the 1970s about how the relation between lived and narrated biography can be understood. The biographical method has been accused of equating actual biographies with the ones that people narrate – of being prone to falling into a trap of "retrospective illusion" (Osterland 1983, German original) or "biographical illusion" (Bourdieu 1990: 76, German original). Most prominently, but like others before him, Pierre Bourdieu has raised the concern that in autobiographical narrations, people tend to retrospectively create very straight biographical trajectories and become "ideologists of their own life" (Ibid: 76). In the meantime the debate has reached broad agreement that it is methodologically unsound to equate lived and narrated lives. The question, however, remains whether it is reasonable to think along the divide lived/narrated. Any such divide implicitly assumes that there is one actually existing biography that can be revealed with a careful methodological approach, i.e. that it is possible to access the objectively and actually lived lives. This is an assumption that I do not share. Rather than revealing an objective truth, I regard different methods as tools for differently enacting biographies. From this perspective it is all the more important to consciously reflect on and understand how the methods used enact biographies.
With regard to this study, I am therefore concerned with a methodological discussion beyond the lived/narrated-divide and the representation of biographies. In a reflexive approach I acknowledge that narrated biographies necessarily

reconstruct lives retrospectively, i.e. that they display biographies only through the glasses of memory, filtered, reordered and rewritten along present relevancies and hopes for the future. This, however, suggests, that in listening to narrated pasts, we can actually learn a lot more about the present than the past. We might learn about people's filters of remembering, modes of ordering and reference points of writing their biographies. In this way, the approach somehow inverts the above argument: rather than regarding biographical narrations as harbouring an objective truth about past lives, it says that it is particularly the retrospective and "ideology"-informed character of biographical narrations that make them in fact very interesting for understanding the experience of the present. How people make sense of and frame their pasts carries implicit value systems, present interests and aspired futures and can show how people appropriate their living spaces, and frame their actions and decision-making. Biographical narrations are always deeply entangled with with local and more global societal contexts and thus allow the present conditions that constitute specific situatednesses to be illuminated. For my particular research questions, this means that we can learn about the positionings from which uncertainty-experiences are had and about what researchers experience as the structural preconditions for uncertainties from their biographical narrations.
Authors like Bourdieu were further concerned that the biographical method would perform societal conventions – such as linearity and homogeneity – and that ruptures or ambivalences in life courses tended to be systematically blanked out (cf. 1990). To avoid such linear readings of biographical narrations as much as possible – and to be able to identify ambivalent or conflicting lines of narration – the interview guidelines of our interviews included several interventionist questions: we explicitly asked for ruptures and turning points in researchers' life courses and, most importantly, we approached researchers' biographies from different angles (such as their research, employment and funding histories and their academic careers) as to make tangible the moments in which different strata of their biographies might have created tensions. This allowed controlled reflections in researcher's biographical narrations to be obtained; in the sense that the interventionist questions made the extent to which our particular methodological approach was enacting their past lives open to scrutiny.

What biographical interviews, however, necessarily do is focus on individual experience and provide limited perspective on the social dynamics within the respective (lab) collectives. Combining interviews with group discussions and observations in labs therefore helped us to grasp collective experiences within academic life science research and relate them to individual experiences of being a young life scientist in academia. The four group discussions we carried out varied in size (between three and over 20 participants) and lasted about 1½ hours. The individual groups were made up of researchers in similar positions within the

academic career hierarchy: two were carried out with PhD students and two with postdocs. They gave the opportunity to reflect in detail some problem areas from their respective perspectives, or in other words, they helped to gain some access to the views of researchers in different positions. This approach builds on the understanding that meanings, sense-making stories and experiences of individuals are always produced in relation to others. From this perspective, group discussions are a site of constant negotiation. Looking at the interactions between people in the group can shed light not only on commonly shared understandings, but the relational construction of beliefs (cf. Waterton/Wynne 1999: 133). Interview data had, for instance, indicated that the particular positioning in a research process and in the social order of the lab could make some experiences more intense and others come to the fore. Some uncertainties seemed to be shared amongst PhD students, while they differed from postdoc's experiences. Since PhD students, for example, were often closer to hands-on research in the lab, they were more directly confronted with epistemic uncertainties. They also had little tacit experience in dealing with them, which seemed to make them more dependent on experienced colleagues. postdocs, on the other hand, typically carried more responsibility in handing on their tacit knowledge, the supervision of younger colleagues and with this, the success of the lab. Since they were already higher up on the career ladder, they were more immediately exposed to performance expectations under conditions of high competition. Discussion amongst these groups and along discursive processes of solidarity and mutual reconfirmation of experiences allowed for these differences to be investigated in-depth and for generalisations regarding shared codes and norms across them. However, the observed negotiation processes also brought to the fore the fluidity of meanings that are always only stabilised in a specific social context and at a certain time. As Waterton/Wynne have put it, we thus might have to look at such interactions

> …not just [as] a neutral medium through which intrinsic preferences and values are expressed, but [they] are themselves a substantive part of the *formation* of values and attitudes; they themselves have moral and social 'weight', as ends and not just means. (Waterton/Wynne 1999: 136)

As a methodological tool, the focus groups in this study helped explore the diversity of meanings and the ways in which they collectively made sense of these meanings in creating shared understandings and in collectively referring those meanings back to their social and institutional embeddings.

Finally, the observations in two labs provided an opportunity to examine tensions between individual and collective experiences in everyday research practices. They were carried out along a rough observation-guide that included questions about the epistemic and social structures of labs and on their institutional embeddings. The idea was to accompany individual lab members for one full day each to cover

experiences of lab practices from different individual perspectives. This plan was flexibly adapted when it proved reasonable: e.g. when it seemed more important to observe collaborative activities and talks without the attachment to one individual researcher. These observations helped to demystify the partly smooth and idealistic narrations about lab life that we had obtained from the biographical interviews. For instance, it put into perspective the workings of academic career standards: while from analysing the interviews it could have been assumed that researchers had a very homogeneous way of doing a career, observations suggested that researchers were constructing such an ideal career from very heterogeneous positions. In the second lab that I described above for example there was an informal, yet openly acknowledged, arrangement of distributing labour between two parts of the lab. As I was told, being positioned in either one had different implications for supervision, for recognition within the academic community and – most importantly – for academic career opportunities. In such respects, observations of the lab practices were crucial for working out otherwise hidden tensions and ambiguities in researchers' everyday research practices and career making.

Five follow-up interviews finally allowed for an in-depth questioning on uncertainty-experiences. Since these interviews were done about two years after the first interviews, they allowed us to trace shifts in researchers' uncertainty-experiences and their preferred ways of coping with uncertainties along changing employment conditions, career perspectives and positions in the lab.

4.3. Grounded Theory OR The Development of my Research Questions

The course of my research and analysis followed a Grounded Theory approach. The particular research interest was not pre-cast from the beginning but developed along a broader investigation of research cultures in the life sciences along a circular mode of working with the empirical material and theory-building about academic research as culture and practice: alternately, empirical material was gathered and analysed along an open coding procedure that helped to identify relevant topics and suggested more focused questions (cf. Glaser/Strauss 2005, Böhm 2005: 475ff). In the following, I will reconstruct the course and certain turning points of the process of developing my research questions and analytical approaches.

I first developed a slightly more focused question about cultures of work in academic life sciences and asked: What kind of work is it that life scientists in academia are engaged in? Since this still was a quite large area of interest, I then decided to focus on one phenomenon that seemed particularly dominant within the groups of young researchers and that they also pointed to when they were reasoning about the differences of "their" work from "other" kinds of work: intense experiences

of uncertainties and a necessity for developing ways of coping with them. The set of research questions that I followed from then on were:

1. **What kinds of uncertainties do early stage researchers experience and how do they give these uncertainties meaning?**

This question was meant to trace and reflect experiences of uncertainties in researchers' narrations: Do they mention institutional, career-wise, social, etc. uncertainties? What role does uncertainty within the knowledge production process play? Are their experiences unequivocal or do they inhabit ambivalences?

2. **To what framework conditions of research do early stage researchers refer to in their narrations on uncertainties?**

As a question on societal and institutional reference points aimed at shedding light on the broader lines of development that the uncertainties mentioned derived from: Under which circumstances are uncertainties perceived? What kinds of contexts create experiences of desirable or avoidable uncertainty?

3. **What explicit and implicit accounts do they give of ways of coping with uncertainties?**

This question was meant to trace contexts that researchers find, use and/or create themselves in order to deal with uncertainties. What are these ways of coping supposed to accomplish: limit or extend spaces for (what kinds of) uncertainties? How do their coping strategies guide researchers' ways of making decisions and acting in research environments and of contributing to the tacit governance of research cultures?

These research questions have been revised and improved on in collective discussions at summer schools and seminars as well as during first rounds of analyses. They aided me in developing an understanding of the broader political implications of my research questions and in carving out analytical dimensions that guided the ongoing analyses. Most importantly for my ongoing analyses, debates about preliminary findings made me aware that my set of research questions had tacitly implied individualistic ways of coping or put differently that I had structurally underperceived collective experiences of uncertainty and the collective dimension of coping with uncertainties. After a couple of months I thus revisited my original set of questions and added three (sub-) questions. The social dimension thus became a specific focus of further empirical work and the subsequent analyses along the question:

How far are academic norms and ideals – such as collaboration – involved in researchers' ways of coping with uncertainties? How do social cooperative and individual ways of coping with and acting on uncertainties complement each other in the everyday of life science research?

During the subsequent steps of analysis, I increasingly tried to build a theory around these specific uncertainty-experiences and asked:

How can the specific constitution of their overall uncertainty-experience be conceptualised? How does it feature in the everyday and on what grounds and what characteristics can the experience be specified?

Finally, I started asking under what premises I could relate uncertainty-experiences in academic life sciences to broader diagnoses of the present that build their core-argument around increased uncertainty-experiences. I therefore started reviewing such diagnoses from various academic backgrounds and asked:

(How) can researchers' experiences be related to more global conceptualisations of intense experiences of uncertainty in (knowledge) societies? To what theoretical lines of academic debate about work cultures can the specific case of young, academic life science researchers be linked?

Analyses along these questions followed an open coding process and aimed at identifying uncertainty-experiences and rationales of coping with them, theorise them in an empirically grounded way and finally to relate them to conditions in their societal contexts. I did this by working out researchers' latent structures of making sense of their environments and of themselves within them. Throughout my research process, the scientific software programs "Atlas.TI" and "TAMSAnalyzer" have assisted a comprehensible management of ample amounts of transcripts and facilitated analyses across transcripts of interviews, group discussions and observational notes with flexible coding scripts supported the analysis.

4.4. Analysing Everyday Experience

Choosing the terminology for writing about living spaces and people's ways of perceiving them and themselves within them seems crucial. Interpreting qualitative – and particularly biographical material – bears the danger of overly psychologising their accounts and of expecting coherent explanations. The terminology thus needs to acknowledge that what we can learn from them is always necessarily fragmentary: It is essentially their situated reality and their fragile, flexible and inconsistent ways of perceiving their living spaces and themselves within them. To express this – in itself precarious – condition of qualitative social science research, I use two conceptual terms to speak of researchers' ways of making sense of themselves within their living spaces: the "everyday" and "experience". Both are undoubtedly heavily intertwined and act upon each other but the question of how (far) they relate to each other is still widely debated. Using them methodologically therefore requires reflecting on the specific notions of everyday and experience that I follow: In my understanding, the everyday does not determine experience nor is it the other way around. Their relation is neither constant, nor is it predictable. Rather, it is often indecisive and often contradictory:

> The everyday is the site in which experience circulates and transforms. … the everyday as a force which keeps events and experiences together, letting them interact without determining their meaning and without demanding that all elements of the story be compatible with each other. … The everyday is the medium through which experience gets under the skin and materializes, affecting selves, others and situations. (Stephenson/Papadopoulos 2006: xif)

Experiences of the everyday are – even though they are produced collectively within a social context – necessarily heterogeneous and depend on individual situatedness and histories. Because of this indeterminate, even contradictory and incommensurable character of experiences, I do not regard my research as a means of representing everyday experience in life science research contexts. The knowledge that can be gained from analysing everyday experience rather lies in how people actively appropriate and transform the social structures and spaces around them. This approach is informed by the methodology of "memory work" as it was developed within the feminist movement of the 1980s and is put in a nutshell by the following quote:

> Rather then passively determined by our social contexts – we actively take them on, make sense of them, weave ourselves into them and in so doing become who we are. So the constraints of the social realm cannot be thought independently of our experience with them. (Stephenson/Papadopoulos 2006: 54)

Experience is thought of as an active practice of appropriating social contexts, a practice that is mediated by the kinds of subjectivities involved and that is a creative force that co-produces the everyday. A notion of experience must therefore avoid determined, unitary or fixed notions of the individual subject. Rather, it is the fluidity and malleability of experience that needs consideration when we want to understand what lies behind people's motivation for acting.

PART 2: Preconditions of Uncertainty-Experiences

Researchers offered different explanatory models for why they experienced uncertainties and, in doing so, identified certain aspects of their academic environment that they felt were responsible for their experience. Each of the five chapters of this second part explores one of the dominant structural preconditions in the fabric of living and working in the academic life sciences that researchers referred to: uncertainties within the research process itself (chapter 5: epistemic uncertainties), their high personal involvement in their research activity (chapter 6: subjectified work), the requirements for pursuing an academic career (chapter 7: academic career trajectory), the flexible and temporary employment conditions (chapter 8: casualisation of work) and the changes they perceived in the performance measurement and social organisation of their work (chapter 9: commodification of academic practice). The overall argument developed in this part is that the convergence of these five conditions establishes a space within which pervasive uncertainty-experiences seem likely to emerge.

In this discussion of the entanglement of different conditions in the everyday of the academic life sciences I will use the conceptual tool of *epistemic living spaces* (Felt 2009). It provides an approach for understanding research cultures that gives consideration 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. … It addresses the intertwinedness of the personal, the institutional, the epistemic, the symbolic and the political. (Felt 2009: 19)

Within this theoretical framework, I will show how the above-mentioned conditions are the segments of researchers' epistemic living spaces that support their uncertainty-experiences. However, the picture I draw about living and working in academic life sciences must be seen as "both, opening up and closing down possible degrees of agency", as subject to a "constant reordering" that is being "performed simultaneously by researchers, institutions, policy makers as well as by society at large" (Ibid). As Ulrike Felt points out in her conceptualisation of "epistemic living spaces" they are to be thought of as messy and embattled places in which little is as clear-cut as I will depict it in the following chapters. Untangling the conditions of uncertainty-experiences however is a necessary analytical step in the better understanding of how different influences entangle in making up the fabric of living and working in the academic life sciences today. It will clear up our view and enable us to see what facilitates and delimits researchers' capacity to think, act and decide within them is a matter of a constant transformation and contestation.

In the following I will therefore analytically parse and discuss in detail the individual conditions that researchers refer to in their narrations about uncertainty-experiences. Doing so will, in later chapters, allow for a better understanding of the multiple articulations of how broader societal conditions get entangled and are articulated in live science researchers' everyday. Since the meaning and management of epistemic features is often so central in researchers' narratives about uncertainty-experiences, the concept of epistemic living spaces is a particularly good framework for reflecting on how the epistemic aspects of life science research cultures are entangled with their social contexts (e.g. funding, lab structures, the fabric of academic institutions), or as Felt/Fochler have put it, the concept allows me

> …to address the inextricable interdependence of epistemic practices, institutional rationales, individual biographical decisions, and political frameworks, which characterises the lived experiential realities of researchers today. (Felt/Fochler 2010: 137, German original)

The analysis in this thesis will make it possible to think different perspectives together such as

> …individual and collective perceptions,… changes, heterogeneities and fluidities in today's research landscape, and to link individual and collective experiences to more global systemic changes… (Ibid)

Before I start exploring the structural preconditions for uncertainty-experiences, I need to note that these were quite differently framed within life scientists' narratives. While researchers tended to discuss two of these features (epistemic uncertainties and subjectified work) as part of the inherent nature of knowledge production processes, the other three were described as transformable – and in fact currently transforming – boundary conditions of academic life science research (casualisation, academic career scripts, commodification). As the reader will notice, the chapters vary in the way they combine theoretical debate and empirical discussion. This variation is due to the degree to which it was possible to relate existing theoretical debates about work cultures (within science and technology studies, labour studies and economic theory) to what researchers in the life sciences experienced. Therefore, the chapter on subjectified work and commodification of academic practice elaborate quite extensively on academic debates about the transformation of work relations; the chapters on epistemic uncertainties and academic career trajectories both mainly rely on empirical analyses; and the chapter on casualisation largely builds on secondary literature and existing statistical data. What all five chapters have in common is the aim of relating concrete experiences of the everyday in the academic life sciences with ongoing theoretical debates about the character and transformation of work cultures.

5. Epistemic Uncertainties

> *There is a lot of luck involved… we work certain things on proteins, that's (sic!) not exactly predictable whether or not it will work or not… when you consider a few things beforehand, you will be successful in about 10% of the cases – simply because there are a range of things that might not work out. So, you have to be lucky to catch these 10%. Or you measure all proteins one by one. And that's very time-intensive. That's what I mean when I speak of luck…* (m1.2: 356-64[xi])

When life science researchers were asked about characteristics of their activity and of their living spaces they would most frequently mention the uncertainty and unpredictability of the research process. Like in the quote above they would narrate their practices of planning and carrying out their research and of routines of lab work as organised around such epistemic uncertainties. In that sense researchers conceived of their living spaces as inherently epistemic – as framed by characteristics of the knowledge production process. How these epistemic uncertainties are best handled is a frequently discussed matter in research communities themselves as well as in the research policy discourse. In this chapter I will recapitulate the ways in which uncertainties in the knowledge production process are addressed by policy makers as well as by life scientists themselves. While doing so I locate moments of ambivalence and tension within both discourses.

Within contemporary – increasingly merit-based – ways of organising research cultures, epistemic uncertainties are typically narrated as both a necessary precondition for innovative knowledge production but also as a bit inconvenient since the outcome of research always also appears to rely on a certain amount of luck and not on well thought through and smart ways of planning and carrying out research alone. The salient question for the researchers is whether and how epistemic uncertainties can be put to work in order to optimise outcomes and how far it is reasonable to try to tame and organise them. This tension is palpable in the report of an expert group on behalf of the European Commission:

> *A particular and inherent uncertainty in research arises from the fact that basic research itself is not goal-directed in a strict sense. The outcomes are not known in advance, since what is looked for are new properties, mechanisms and phenomena. Basic research therefore is an open and an open-ended process in which serendipity, the accidental finding of interesting and relevant phenomena that one was not looking for, is often decisive.*
>
> *This inherent uncertainty is a characteristic feature that basic research activities share with the uncertainties that are inherent in the process of innovation. Certain preconditions, like adequate funding, institutional and organizational structures, scientific networks and technological configurations, scientific and entrepreneurial*

> *leadership, team size and composition, can play a crucial role. With the benefit of hindsight from contemporary and historical case studies, favourable and disadvantaging configurations can be identified, but no prescriptive guidelines can be deduced that will predict when breakthroughs will be achieved or what form these will take. Human creativity as expressed in basic, curiosity-driven research – as distinguished from applied research and development where already available knowledge is further used or developed towards specific ends – avoids such prediction and planning.* (European Commission 2010: 47f)

The interesting aspect of this text is not only that it frames epistemic uncertainties as necessary precondition for what is defined as innovative research, but that it suggests that whether and how the innovative potentials of epistemic uncertainties can be harnessed depends on the social and economic infrastructures – such as funding or organizational structures. The European Commission claims that the environmental framework of research (such as funding or social organisation) is crucial for promoting or hindering desirable outcomes. However, the text also states that a necessary uncertainty remains. Despite historical experience, it says, that it is impossible to identify forms of organisation that will guarantee the desired outcomes. The ambivalence of the statement lies in the performance-oriented expectations that the European Commission nevertheless places upon research when emphasising the need for productivity and goal-directedness of research and in proposing a strongly "merit-based and grant-based competition for funding" (European Commission 2010: 187). Both, the merit-based and the grant-based way of organising research however seem to strikingly diverge from the above assumption about the uncertainty, serendipity and unpredictability of research processes. The great majority of research grants are held for a fixed period, which leave little room for surprising turns in the research processes and merit-based competition acts on the assumption that hard work is proportionate to achievement.

This contradiction is traceable in researchers' narratives and is always interwoven with accounts of uncertainty-experiences. This is demonstrated in the quote at the beginning, in which a postdoc insists that luck is a necessary precondition to being successful in research. Even if one is smart and works hard researchers would typically contend that the reality of research – defined as the inquiry into the unknown – is that "in 90% of all cases it doesn't work out at first go" (m2: 630-3).[xii] Especially in the field of the life sciences – a field that is "working with living material" – they insisted that unexpected things like "an infection of the cell culture" can happen and "three months can be wasted" easily (m4.2: 944-6[xiii]). An Austrian life science magazine spoke to this point while addressing a taboo of talking about the fact that "science is indeed an occupation that builds on failure" (Steindorfer 2009: 18, German original) in the sense that it relies on the chance that, because of epistemic uncertainties, an experiment may not work out or may turn out

differently than planned thereby opening up new and interesting trajectories for seeking knowledge.

How far and in what ways this inherent uncertainty is shaping the research cultures at hand and what this means for those who are working in these cultures deserves closer attention. Within the academic debate – particularly in the field of social studies of science – there is a long-standing tradition of investigating how epistemic features guide the ways in which research is socially organised (cf. Shinn 1982, Whitley 1984, Knorr-Cetina 1999: 88, Shapin 2008, Roth 2009). Terry Shinn has, for example, explored how intellectual and social structures are interrelated in different research fields. In his comparative study he has drawn attention to "the unique character of research practices in different fields of scientific investigation" that he sees as depending on the epistemic structures of a research field (Shinn 1982: 239). One of the epistemic aspects these studies have discussed is how epistemic uncertainties define specific research cultures. It seems to be beyond dispute that uncertainties are what defines proper research. Since, as Steven Shapin argued, scientific research is an "inquiry into the relatively unknown", it must be assumed that

> …neither the exact shape of the eventual results, nor the methods which will be successful in securing those results, not the time and resources required for success, nor the likelihood of success, nor finally, the consequences of findings can be exactly specified in advance of undertaking research… One of the most mundane, yet characteristic, features of any research properly called so is uncertainty – uncertainty in its outcomes and uncertainty in the procedures employed to secure outcomes. If one defines research as an inquiry into the relatively unknown, then neither the exact shape of the eventual results, nor the methods which will be successful in securing those results, not the time and resources required for success, nor the likelihood of success, nor finally, the consequences of findings can be exactly specified in advance of undertaking research. (Shapin 2008: 132)

However, others have suggested that considering that the particularities of epistemic uncertainties differ between research fields, it stands to reason, that whether and how epistemic uncertainties gain significance within disciplinary contexts may differ as well.

In her influential book on epistemic cultures, Karin Knorr-Cetina has pointed out that while some epistemic cultures live with epistemic uncertainties and manoeuvre around them, others see it as their core task to eliminate them and thereby gain as much control as possible over the research process. In her comparative study she identifies the way in which researchers cope with epistemic uncertainties as a distinguishing characteristic of the field of high-energy physics (HEP) and molecular biology (i.e. a field that is today regarded as one discipline contributing to the life sciences). Her analysis suggests that while in HEP a major part of research

activities relies on identifying and erasing uncertainties, in molecular biology researchers tend to leave a good share of epistemic contingencies unexplored:

> In a molecular biology laboratory, little is fully controlled. Lines of inquiry are continually set back because of unexplained problems, procedures that used to work in the past suddenly stop working, and approaches that looked promising lead nowhere. Most of these difficulties cannot easily be explained, and, in participants' reckoning of how to use one's time, they are not worth trying to explain. (Knorr-Cetina 1999: 229)

She describes the management of epistemic uncertainty as characteristic of the respective work cultures. Whereas researchers in the field of HEP act on the assumption that they are able to explain everything and eliminate inconsistencies, molecular biologists do not claim to fully control the knowledge production process. In research cultures that work with living material it rather seems that the uncertainty lies in not knowing where the uncertainty lies and in not knowing whether and how certainty can be achieved (cf. Krohn/Krücken 1993, Evers/Nowotny 1987).

These considerations suggest that knowledge producing work cultures develop characteristic ways of dealing with the often very particular epistemic uncertainties that their research entails. What meaning uncertainties take on in a respective research culture seems to depend, among other things, on the meaning that a research community gives to them. An understanding of the significance that epistemic uncertainties have in a research culture must therefore ask how researchers and their communities let uncertainties guide their everyday work routines, whether and how they build social infrastructures around uncertainties or – in short – how people learn to deal with them within their given environments. Epistemic uncertainties and the question of how epistemic cultures learn to cope with them is one angle from which we learn to better understand the particularities of a research field.

Sara Delamont and Paul Atkinson have chosen this question as their point of departure for investigating research cultures in laboratory and field studies as they look at the way in which doctoral students learn to deal with uncertainties. They explore how young students first get accustomed to controlled learning environments and are later released into the uncertainty-intensive research environment. A considerable share of training in the doctoral phase is thus made up of acquiring tacit knowledge and craft skills for dealing with uncertainties and contingencies (Delamont/Atkinson 2001: 88, 101). A postdoc in the empirical sample of this thesis once described this process of slowly becoming accustomed to the unpredictable workings of research as follows:

> *At the beginning you think: OK, I'm doing an experiment and then I will know something about it. And that's when you don't know yet, that you will eventually have to do the experiment ten times until it works, and that it is hard hands-on work, and that you have to repeat things and that it is in fact a very slow process… And that's frustrating at the beginning. And then when you develop a certain tolerance to this frustration, it is reasonably ok.* (f1.1: 115-23[xiv])

What this quote insinuates is that learning to deal with epistemic uncertainties is a crucial part of making the epistemic living spaces liveable for the individual researchers that inhabit them. Once acquired however, a set of tacit knowledge and skills seems to turn uncertainties into a normal – and almost forgotten – part of their working procedures. Delamont/Atkinson further argue that as a result of this late learning phase PhD students and postdocs even "learn to remove all mention of those tacit, indeterminate aspects from public accounts of their research" (Delamont/Atkinson 2001: 87f). Although life scientists describe epistemic uncertainty as a core characteristic of their research that they must permanently deal with, these experiences tend to be made invisible in formal representations of research activities (such as publications or conference talks).

While uncertainties seem almost forgotten on the surface, ways of handling uncertainties appear to be an omnipresent force and a key factor for acting and making decisions during research. In informal shop-talk, epistemic uncertainties (and the need to adjust to unforeseen developments of an experiment) are discussed by researchers as a major challenge – a challenge that, on the one hand, bears risks but, on the other hand, provides the exceptional joy of letting oneself into such uncertainty-intensive processes and every once in a while experiencing surprising turns. Taking the term "research" literally, one interviewee explained that if it were different and less uncertainty-intensive, it would not have to be called research any more:

> *That's why it is called research; re as in 'search again'… it is time and again a cycle until you can show something…* (m2: 633-5[xv])

This progression of uncertainties was narrated as a characteristic that made academic life science research interesting and exceptional compared to other professions. In fact, many researchers mentioned the freedom to follow highly uncertain research questions as a main reason for not leaving academia for other jobs.

However, when researchers talk about how they deal with those uncertainties – and what meaning they are given in their actual work process – they often refer to the environmental conditions within which they perform their research. Exploring the ways in which researchers talked about and handled epistemic uncertainties makes visible a complex net of interrelations between academic research practices and the

social infrastructures that are built around them. In the following paragraph a postdoc talks about how the funding situation can determine to what degree researchers are likely to welcome uncertainties:

> *It's about research funding, that is, if there is little money for research… it is important to have good output, and it is also important to have a certain amount of pressure.* [But]*… when the pressure is too high, that you cannot even start high-risk projects, then the pressure is clearly… completely misguided, then there's a clear malfunction in the system. It can also be that you can only do high-risk projects to make it, and then you have an enormous share of downfalls. I think, it is important that you have a certain economic freedom. If you have more money to work with, then you can try out projects that might not work out and your lab does not go bankrupt because of this. But then you have at least tried it, but it didn't work. You have worked very hard, and that shouldn't push you in the doom, right. And when money is scarce from the beginning and you can only work very conservatively, then you will only have very conservative findings,* [I mean] *to a large extent… I don't want to waste resources, but the freedom to do experiments is much bigger. If the economic pressure is not so big, and when you can just risk things… I think, pressure has a lot to do with money and with the number of positions…* (f6.1: 713-41[xvi])

In this quote, the postdoc explicitly links her funding situation to the meaning that epistemic uncertainties have for her. She suggests that an economically tense situation is linked to an avoidance of uncertainties while she claims that economic freedom would favour a process that involves more epistemic uncertainties. It is not the epistemic uncertainties nor the uncertainty of funding in and of themselves that create a problem, but their specific interrelation. The fact that contracts are temporary makes epistemic uncertainty an individual problem; and conversely it is the need to risk epistemic uncertainty (and achieve positive results) that reinforces the perception that temporary contracts bear uncertainties. In this quote she highlights how competitive funding affects her estimation of the "riskiness" of a research project along with her wellbeing and level of stress. In doing so, the meaning of epistemic uncertainties takes an interesting turn. What the quote hints at is that in a temporary employment situation, epistemic uncertainties can be experienced as personal social risks. Overall it can be said that the experience of epistemic uncertainty oscillates between these two poles: as being the precondition for exciting activity and of outstanding research results and as being a quality of their work that can easily turn into an individual social risk.

The social risk notion of epistemic uncertainties was however always discursively situated within particular social and environmental contexts – for instance the length of employment or certain performance expectations. Or, in other words, when they subtly interpret epistemic uncertainties as individual social risk they do so in terms of socially constructed risk: as produced by certain conditions in their epistemic living spaces. In the following chapters I will explore four conditions in

detail that they most frequently referred to when their notions of epistemic uncertainty took on an overtone of social risk – i.e. those conditions that from the researchers' perspective contributed to making epistemic uncertainties part of an overall pervasive uncertainty-experience.

6. Subjectified Activity

> *…it is very personal because everything in your scientific work builds on yourself, and not because some customer wants it or because your boss tells you or the like. You know what I mean? It really builds on yourself. Your motivation depends very much on yourself. How you are doing… Yes, how you are feeling. So, I can really observe this with regard to myself. (laughs)* (f4.1: 989-94[xvii])

Besides the prevalence of epistemic uncertainties, researchers experienced the personal character of research activities as inherent to their work. They saw it closely linked to what they were interested in, who they were as a person, and to how they designed their lives. The quote above is a powerful account of this "subjectified" character that they often ascribed to their activity. The researcher emphasises that her research was very much entangled with her personal motivation and with her emotions. In such reflections, researchers mostly found it unintuitive to conceive of their activity in terms of waged work – mostly because they associated it with detached and alienated activity. In this chapter I will use concepts from the field of labour studies to develop a better understanding of such personalised notions of work. While doing so, I focus on theoretical debates about the high investment of subjectivity in work cultures – i.e. the investment of personal qualities such as motivation, affectivity, creativity and communication skills. I will recapitulate this debate with reference to the empirical sample of this PhD study.

6.1. Research is not (Only) a Job!?

As the quote above suggests, for life science researchers the meaning of their activity goes beyond what is usually understood as waged labour. As Hugh Willmott has put it, perceiving knowledge-production as a job tends to contradict the way they see themselves:

> For many academics, the notion of 'selling labor" is directly at odds with the self-image as professionals, for whom payment of their work is, or has been, regarded more as a necessary condition for providing a (vocational) service to society than a means of providing them with income. (Willmott 1995: 995)

In labour studies literature, such kinds of work have been discussed under different labels. Authors who are interested in the products of such work (which is common in the policy debate and some strands of the academic debate) refer to it as "knowledge work", since knowledge is the product of the labour (Kleinman/Vallas 2001, Sewell 2005). In other contexts (particularly in labour studies or theory developed in social movements) it is rather the social organisation of the production

process, the meaning that work has for the workers and the ways it is governed that are discussed (cf. Baethge 1991, Kleemann 1999, Papadopoulos 2008, Lohr/Nickel 2009). Exemplary for the latter perspective are notions of post-Fordist (as opposed to Fordist) labour relations, of subjectified and of immaterial work. Since I am in this thesis mostly concerned with personal experiences of such work within a particular setting, I will focus here on this second perspective. Using exemplary accounts of life science researchers, I will discuss to what degree it can open up a new angle for exploring experiences of uncertainty in the knowledge producing work cultures of the academic life sciences.

The notion of post-Fordist labour dates back to the 1970s and starts from the observation that – in so-called industrialised countries and from a historical perspective – increasingly fewer sectors are organised according to Fordist, industrial labour relations. While industrial labour tends to be organised according to Taylorist forms of rationalisation such as division of work, clear instructions and mass production, post-Fordist labour relations are characterised by service-orientation, flexible work organisation and less rigid division of work. This development can be characterised partly in the abandonment of Fordist work relations in sectors where it was widely spread such as in the (eponymous) car factories, and also to a growth of work sectors that seem to be incompatible with such work principles. It builds on the assumption that both productivity and worker satisfaction can be maximised by more self-directed aspects of the work process. Post-Fordist work relations therefore tend to be characterised by self-determination and self-responsibility in structuring working procedures. The notion of post-Fordist labour relations is linked to broader historical transformations of work cultures and refers to the investment of subjective factors such as self-motivation, affectivity, creativity and communicative skills by the workers.

In the past decades an ever broader academic debate has developed that explores what implications these global trends have for changing the qualities and meanings of work on the micro-level of everyday experience. In recent years, the concept of "subjectified" (or sometimes "immaterial") work has also become a tool for better understanding the characteristics and practices of newly emerging work cultures in knowledge societies/economies. Since the life science researchers we interviewed described their work as including a range of what is usually subsumed under "high investment of subjectivity" (cf. Lazzarato 1998: 2, German original) it seems worth exploring to what degree this concept might be helpful for life science research cultures.

This subjective dimension of work has become intensely discussed with regard to new expectations and demands in cultures of labour (cf. Baethge 1991, Sennett 1998, Hardt/Negri [2000] 2003, Boltanski/Chiapello [1999] 2006). When Maurizio

Lazzarato speaks of a "high investment of subjectivity" he primarily addresses work that is knowledge-based. As one characteristic of these kinds of labour he discusses the non-existence or shifting of boundaries that characterises work in Taylorist organisation by saying that in knowledge-intensive work the

> …old dichotomy between ‚mental and manual labor', or between ‚material labor and immaterial labor', risks failing to grasp the new nature of productive activity, which takes this separation on board and transforms it. The split between conception and execution, between labor and creativity, between author and audience, is… transcended within the, labor process'… (Lazzatato 1998: 1, German original)

He writes that these workers need to combine different skills in one person: s/he is mental and manual labourer, designer and executer of tasks and author and audience at the same time. An impossibility of dividing work tasks between workers seems to foreclose it from Taylorist rationalisation procedures. The workforce as he calls it

> …combines various different types of work skill: intellectual skills, as regards the cultural-informational content; manual skills for the ability to combine creativity, imagination, and technical and manual labor; and entrepreneurial skills in the management of social relations and the structuring of that social cooperation of which they are part. (Lazzarato 1998: 4, German original)

In doing so, the concept denotes work that requires very personal competences – i.e. subjectivities that are rich in knowledge, highly motivated, capable of self-organisation and of working collectively, or in Lazzarato's words:

> …(w)ork can also be defined as the capacity to activate and manage productive cooperation. In this phase, workers are expected to become ‚active subjects' in the coordination of the various functions of production, instead of being subjected to it as simple command. We arrive at a point where a collective learning process becomes the heart of productivity, because it is no longer a matter of finding different ways of composing or organizing already existing job functions, but of looking for new ones. (Lazzarato 1998: 2, German original)

Another boundary that authors describe as dissolving in the context of subjectified work cultures is the boundary between work time and private time. When embodied capacities such as motivation and creativity are directly put to work, it is often impossible to stop working when working time is over. Work and life become undistinguishable, which is a very frequent account within academic work cultures. Massimo De Angelis and David Harvie have therefore concluded:

> Work in academia seems to capture the basic features of (such) labour: a form of directly social work, in which the form of social cooperation is crucial in defining the 'output', a form of doing that is necessarily grounded on relational awareness, and that produces affects… It goes without saying that academic work is also a context for the production of ideas, research papers and books; moreover that this

> production is ‚biopolitical' and can occur at any moment of the 24/7 span: we both have experienced waking up in the middle of the night with the solution of a problem insoluble during 9 to 5, or have reached an insight that will find its way into a paper while playing with a child. (De Angelis/Harvie 2006: 4f)

Such observations and theoretical accounts correspond well with what life science researchers told us in the interviews. As mentioned above, many researchers did not first and foremost experience doing research in academia as a job. A dominant framing narrative of being a researcher was that research is a very special kind of activity that is qualitatively distinct from other jobs and that the challenges that researchers face are different from the ones that people with "entirely common jobs" (f4.1: 976f[xviii]) have to deal with. Rather, being active in academic research was described as a kind of hobby that they were happy to have the opportunity to do for a living. That researchers insist on the distinct quality of research work might on a surface be interpreted as an expression of feeling socially distinct. And indeed an interviewee casually mentioned, "of course it comes with prestige" (f4.2: 1157[xix]). However, social distinction was secondary and far outweighed by the distinctive personal aspects that the concept of subjectified work addresses.

In line with such notions of academic activity was the fact that with almost no exception, the life science researchers cultivated ontological stories about being a life scientist. In other words, they regarded themselves as having a natural, embodied predisposition for this kind of activity. Asked how they got into academic research, they tended to frame their biographies not as one of conscious decision-making but as a process determined by their curiosity for how nature works. One typical account of this quasi-natural disposition for a particular kind of thinking and thus a special gift for doing life science research is that of one female life scientist:

> *I have this predisposition. It was already visible in primary school that I favoured mathematics and also biology; I collected foliage that would preoccupy me for eternities. Yes, it was in me.* (f4.1: 17-21)[xx]

Narratives about a "deeply rooted curiosity" and an omnipresence of the question "why?" accompanied such descriptions in their lives (f3: 12-4)[xxi]. This was then described as the cause of a relatively straight trajectory into the field, leaving little space for moments of hesitation or doubt. They gave accounts of always having felt part of a specially gifted group – an explanation that insists that they never had to become but in fact always had been life science researchers (cf. Felt et al. forthcoming). Such accounts supported notions of being very closely connected to their research activity and of having a highly personal motivation for their research. They conceptualised themselves in a holistic way, as not experiencing boundaries between work and private life:

> *…this feeling that you have in many companies that you close the door behind you and then it's private life, you have this much less in science… I work with cells, I have to look after them on the weekends, because when they get overgrown over the weekend, then I cannot use them any more…* (m4.2: 760-4[xxii])

Narrations like this suggested that they saw their living and working in academic research as a lifestyle in which the boundary between work life and private life is quite permeable. The lab was often described as the place where they had found their best friends and life partners. As a consequence, most have explicitly refused a "work-life balance" rhetoric since it implied a clear boundary between work and private life.[12] Such narrations suggest that doing research was considered part of who they were as a person, as an embodied capacity they were lucky to be able to express in academic activity. Even if they sometimes envied people who had clear boundaries and could "switch off" at home, they tended to prefer this form of living and working because it appeared to allow for the development of their embodied capacities without being externally controlled. Even if they often argued that spaces for non-instructed research were becoming narrow – for instance due to preferences of funding institutions, they would perceive enough free space to follow their own interests, or as a postdoc put it:

> *…still I have the impression, that, despite the fact that conditions at university are becoming harder… that you can still do interest-driven research, even if you do very low-risk projects in your field, it still is rather driven by your interest than driven by economic aims such as in pharmaceutical industry or so.* (m4.2: 359-64[xxiii])

Researchers were happy to trade leisure time or a bigger salary for the ability to continue doing interest-driven work. One interviewee directly balanced economic and intellectual benefits by saying that „it's not as if we are drawing a huge salary… but instead we have the benefit of an interesting job, right?" (m1.1: 304-6[xxiv]). Research in industry – which was often figured as their prime alternative career option – often served as negative inversion of this image in that it was seen as work that was directed by economic interest and in which others decided whether or not they could follow certain research questions.

Another freedom that they typically saw as inherent to doing research in academia was the social and collective aspect. Despite and besides the hierarchical organisation of their labs that were stratified along steps of qualification and supervision relationships, researchers mostly seemed to experience an open and socially equal working atmosphere including free, mutually supportive relationships:

[12] In one of the research projects (KNOWING) that the empirical material for this thesis derives from, we had introduced the notion of "work-life balance" by the interview guideline in a question about the relationship between their work and private life.

> *…I still have people that actually work with it, with whom I can utterly exchange – methodologically as well as with regard to information. And it is often the case that I go to a colleague and say: 'It doesn't work for me. How are you doing that?' Or: 'Can you do that for me? I am desperate and give up, because I just don't get it right.' … Or the other way round that you can offer something. … just because I have my own project doesn't mean that I don't really, actively work together with people… And I think that's crucial.* (FGk_jun: 810-22[xxv])

Especially young researchers tended to get advice on using the lab equipment and relied on the feedback of more senior researchers about how to develop a reasonable research question or in deciding on the trajectory of their work. However more experienced staff also insisted that research was an inherently social activity that builds upon open cooperation and exchange relationships.

All this contributed to how young researchers conceptualised their academic work as a special form of earning their living and also as a special way of conceptualising themselves in terms of the devotion of their capacities to research activities. This highly personal quality of interest-driven research was also described as the reason for why they chose it over other ways of earning their living, even if they were conscious about relatively uncertain future employment conditions. What prevented them from leaving for other jobs was that other job perspectives were simply experienced as incoherent with their self-perception. The subjectified character that was ascribed to research work can thus help us to understand why for so many life scientists there is such a strong commitment to staying in academia.

6.2. The Governance of Motivation

On a systemic level however the question remains: how are work cultures that build on such self-determined and relatively autonomous subjects governed? In their narratives, researchers tended to insist that it was not external incentives that were driving them but that the motivation came from within themselves:

> *It is… a firm of your own, but without, without the force of money behind it that tells me that I need to work, or otherwise I would not get paid… It's not even that, right? You have to work because you like to work, because it interests you, ok? And that makes an ultimate difference.* (f4.1: 1000-5)

When narrated in these terms, the power of external governance appears quite limited at first sight. Rather than a punch clock and a foreman it seemed to be researchers' personal motivation that disciplined them. However, at the same time researchers' narrations implied that there were tacit forces that guided the way they developed a preference for (not) following certain practices and research questions. Thus, it might be argued that despite notions of high levels of freedom and self-

determination, their choices were to some extent guided by complexly entangled and often hardly recognisable forces.

In labour studies literature this kind of control is described as an immaterial form of governing. It is suggested that subjectified work is governed by the building of subjectivities, by socialisation and by appropriating and embodying certain liabilities to act – i.e. by immaterial rather than material forces. Maurizio Lazzarato has addressed this in his concept of "immaterial work":

> The worker's personality and subjectivity have to be made susceptible to organization and command. It is around immateriality that the quality and quantity of labor are organized. (Lazzarato 1998: 2, German original)

Thus, in subjectification processes within work cultures there seems to lie a twofold potential: on the one hand, for an experience of work as free and self-determined and on the other hand, for a potentially new form of control that reaches into the personal motivation of the working subjects. Over the past decade, a controversial debate over whether and how these potentials currently unfold has developed in academic circles. For a long time, personally motivated work was emphatically welcomed and strongly defended, for example by social movements that opposed alienated forms of work in Fordist labour relations. Many authors still see the personal quality of such work as a source of liberation from external forces that determine how people live and work. Notions of subjectified (or immaterial) work are thus often linked to political hopes that build on the expectation that post-Fordist labour relations will overrule and suspend the downsides of estranged Fordist factory work. The line of argument is that personalised work is inherently autonomous in a way that transcends alienation and incorporation into a capitalist and rationalised production cycle. From such a perspective, post-Fordist transformations of work cultures are immanently progressive. Prominent advocates of this view are for instance Michael Hardt and Antonio Negri who have argued that immaterial, subjectified labour would liberate workers from alienation in an almost deterministic way ([2000] 2003).

Other authors however have challenged this optimistic view (Lazzarato 1998, Gorz 2005, Bowring 1996). They have emphasised that the quality of subjectified and knowledge-intensive work significantly depends on the social infrastructures that it is embedded in. Like the accounts above, they assume that control mechanisms such as fixed working times or fixed work procedures (as are typical for Fordist labour relations) might not be compatible with subjectified work cultures. However, they suggest that we might be witnessing the emergence of different forms of regulation and control located in the inner motivation and embodied capacities of people. They argue that while on the one hand a high investment of subjectivity can mean less estrangement, the subjective factor – affectivity, creativity,

communication, motivation – in work (organisation) has been recognised as an important resource in the working process (cf. Moldaschl/Voß 2003: 15):

> New 'objective' forces (intensified competition in conditions of globalisation, structural economic transformation,… requirements of flexibility…), but also changing expectations and needs of working subjects build the grounds for a 'disenclosure' of work, organisation and life… and enables access to the subjective potentials of human workforce. (Lohr/Nickel 2009: 7f, German original)

In this context, authors have raised concerns about the rise of labour that sculpts the subjectivities of workers – i.e. that shapes and uses not only people's skills but the whole working person, including personal abilities and deep strata of the self. These concerns have recently been supported by empirical studies that have suggested that the promise of self-determination and freedom has not – or not to the extent that some had expected – been kept. Analyses of such work cultures have indicated that the ambivalent potentials of subjectified work can very easily be directed towards quasi-voluntary forms of over-performance, high levels of stress and new forms of exploitation of the self (cf. Beynon/Nichols 2006). A particular study on knowledge workers for instance suggests that they "have higher attitudinal commitment and lower intention to quit than routine-task workers" (Benson/Brown 2007).

These observations suggest that discipline, rationalisation and control are not absent in knowledge producing work cultures, but that such forces are rather located within the subject. It seems to be a certain attitude towards work – eagerness, work enthusiasm and basic readiness – that might become the crucial locus for regulating work performance. In this light, professional qualification is certainly a prerequisite for good performance. However, it appears that it must also be linked to a specific mindset (or subjectivity) that is willing to perform. When academic institutions thus start out to act like companies and set out to regulate and direct researchers' work, they appear to be in kind of a dilemma. In that they deeply rely on the willingness to perform and cannot – or only to a limited extent – draw on traditional, Taylorist forms of rationalisation and optimisation, they must draw on people's willingness to perform. The means of governance must address the whole person(ality), make him or her excited about a topic and able to be self-reliant in finding creative problem solving strategies. Integrating academics with the imperative of productivity therefore is not limited to enforcing performance standards but also requires that they internalise mechanisms of self-adjustment that make them personally favour those standards (cf. Bröckling 2005: 375ff). Overall it seems to be crucial for this post-Taylorist (self-)rationalisation and (self-)governance that people buy into the idea of authenticity, coherency and personal motivation as opposed to external direction.

6.3. Subjectification as a Political Project

From this perspective the question how academics are subjectified in academic institutions appears as an issue of (tacit) governance. In that subjectification encourages the individual to build and maintain a certain relation to his or herself within their environment, people are driven to align with certain rationales of acting and thereby contribute to shaping the inner workings of work cultures. In doing so people are both, made into subjects that act freely and subjected to policies. Blackman et al. suggest that unlike identity formation, the building of subjectivities is never definite or absolute but quite to the contrary, subjectification is never fully finished adjusting the way that subjects perceive themselves, their environments and their ways of sounding out limits and potentials of thinking and acting. It thus allows for understanding the (tacit) governance of work cultures as an ongoing process that is interfered with by (changing) environmental conditions as well as by (changing) individual orientation. In subjectified cultures of labour thus, subjectification seems to have become an eminently political project; the individual life course becomes a series of strategic decisions and tactic calculi – turning into „life politics/politics of life“ (Blackman et al. 2008: 6).

Efforts in understanding such processes that encourage the individual to conceive of his- or herself in a specific way have gained interest within the social sciences (and studies of labour relations in particular; cf. Bröckling 2007: 22f). Most recent debates about such governance through subjectivity increasingly take into account its ambivalent potentials. While earlier works have defined subjectivity as something that must be made to fit into existing societal relations and that must contribute to the subjects' reproduction, debates in the newly constituted journal *Subjectivity* have set out to explore "subjectivity/subjectivities as a locus of social change and a means of political intervention" (Subjectivity 2011). They acknowledge that a particularly new "neoliberal power establishes a social order… through the multiplication and assimilation of subjectivities that are created by one's own reflexivity and one's own positionality…" (Blackman et al. 2008: 14) while at the same time suggest that subjectivities are not only passively shaped by and therefore reproduce neoliberal power but are also "active agent(s)". They hypothesise that subjectivity "shapes and is shaped by prevailing social, cultural and political spaces." However, it can also challenge them and be a source of self-determined resistance from below (Ibid: 14, 9). Processes of subjectification can therefore be thought of as part of a tacit governance that "involves both acting upon others and acting upon the self" and thereby remaking our social worlds (Papadopoulos 2008: 150f, cf. Blackman et al. 2008: 2, Rose 1996).

What we can thus learn from labour studies debates in the context of academic work cultures is, first, that academic institutions might to some extent be tacitly governed by the subjectivities that it helps create. And second, that the meaning that the experience of subjectified work has for people depends on their entanglement with social conditions. We thus need to step back from the idea that there is a particular *nature* to academic work and look at the ambivalent potentials of research activity within particular social contexts and power dynamics. The organisation of work processes is and has always been contested by different interest groups: on the one hand social forces who tried to rationalise work processes for productivity and capitalise on them for profit and on the other hand individual actors and social movements that tend to claim autonomy in their work contexts. Work in academia therefore is – just as any other work – never just natural but is a politically, socially and economically embedded and contested form of human activity. Rather than speak of a *nature* of academic work the chapters in the first part of this thesis locate the *character* of life science research activity in its particular epistemic, political, social and economic surroundings (cf. Kleinman/Vallas 2005: 41). While the last chapter has already hinted at implications that epistemic uncertainties might have for processes of subjectification in this contexts, I will in the following chapters work out three more preconditions in the research environment that seem to interfere with such processes in the academic life sciences: academic career scripts, casualisation of employment and commodification of their activity.

7. Academic Career Scripts

> *…all researchers are put in the same box somehow… it is so normative because… what counts is the amount of publications that you have and if you have been abroad or not. Or… how many postdocs positions you had and where.* (FGk_pd: 236-43)

Quotes like the above suggest that life science researchers have a shared understanding of what makes up a successful academic career – in particular in regards to the steps that need to be taken and the kind of output they need to produce. This model appears to be an influential reference for researchers when they make biographical decisions, plan their everyday research activities and envision themselves within their academic environment. They thus provide a sense of certainty and transparency in terms of what a life science researcher needs to achieve when s/he wants to pursue an academic career. Young life scientists would however also discuss the complexities of actually meeting these requirements within the conditions that they are provided with. They would for instance emphasise that they had to comply with a variety of institutional regulations (such as those set by their university or funding institution) that made it difficult to keep track of where to set priorities. Sometimes these regulations appeared to conflict with one another or it appeared difficult to adjust their private lives to what was asked of them (for instance international mobility). The fabric of academic careers thus seems to be more complex than it seems in narrations like the above. While researchers seem to share an imaginary of an ideal-type career, they are at the same time also positioned quite heterogeneously within institutional contexts that have their own ways of conceiving academic research and careers. For investigating lived academic careers it thus seems reasonable to consider the particular contexts within which researchers conceived of and enacted career ideals and opportunities (cf. Hermanovicz 2007). This chapter discusses the structural framework within which academic careers in the life sciences unfold and investigates to what degree researchers' own rationales about how they wish to live their (academic) lives can play a role in their ways of enacting them. For doing so, I first discuss empirical accounts of careers in the academic life sciences. After that I draw on career theory – most importantly Stephen Barley's concept of career scripts (1989) – for tracing different influences on careers and argue that life science careers are characterised by a double-bind between an entrepreneurial and an employee model of career.

7.1. Career Norms

The overall ideal-type career narration is that a life in academia is subdivided into different periods. In the first period, a life scientist completes a Masters thesis in a close supervision relationship. The subsequent PhD period is regarded as a phase of

less supervision, which usually lasts three years. While there is still supervision by a lab leader or a postdoc it is expected that PhD students become more independent in developing and processing on a research question. This period is also often considered to be a necessary stage of professional training:

> *As a pure chemist it is difficult, without a dissertation, to even get a job, only with the* [academic] *degree. And so the dissertation is naturally a logical point to stop, that you say: now I think about what I want to make and what direction I take… when you say, I am finished with my Masters and now I add-on a dissertation, that's not that big of a decision, because mostly it is not obstructive for anything*. (f6.2: 606-12)[xxvi]

After the PhD period, several postdoctoral years follow in which researchers are supposed to spend time (usually periods of two to four years) in different labs, go abroad, collaborate on projects with their lab leaders, start teaching and supervising. In later postdoctoral time it is regarded as wise to launch an independent project and thereby establish a small quasi-lab within a bigger lab context, as this postdoc describes:

> *I'm becoming relatively independent now. That has changed in the last years. Because earlier I have simply done my projects and that was it. And now it's rather that I delegate … and supervise.* (f1.1: 249-52[xxvii])

At the same time the postdoctoral period is considered a time of high international mobility, of professional networking and of an ongoing educational development. For transitioning to the next and final career period, which consists of establishing a lab group on their own (and possibly becoming a professor), researchers are asked to learn how to obtain grant money, acquire a repertoire of (new) research technologies and to develop an individual epistemic and career profile (cf. Müller 2012). Even the postdoctoral phase is now mostly considered a training period, especially for learning how to formulate research objectives independently, supervise students and design research projects. It has been suggested that the training periods in academia are naturally becoming longer with research questions becoming more complex and that lab work will require an increasing number of postdocs and graduate students who will need even longer periods to qualify and complete the training for it (cf. Russo 2003: 354). After several postdoctoral positions, a life science researcher is usually regarded as ready to apply for a lab leader position and/or professorship.

Besides explicit institutional regulations, there are tacit but strong assumptions about how long the career stages should take, maximum age one should be during each stage, what position one should have gained by then, what needs to be achieved when, how long one should stay in a lab and where the career path must lead. A narration by a postdoc who already was in a position to choose personnel

herself illuminates how such tacit assumptions get actualised and normalised in concrete decision-making procedures:

> *…right now I've got 180 PhD applications on my desk. And (.) with about 7% you get a very clear picture that they don't fit. Decision seeking* [is to] *sort them out, rather quickly. Let's say 60%. And for the other I look how old these people are, what have they had, especially for people who want the first job… You do age, you do! Someone who is 28, just finished their diploma thesis, seeking for PhD or you try find clues in the CV what this person did all the time. Some worked and then it's clear or some did… something else. Some just stayed there… And then you think automatically. Because the people who have three Nature papers are very rare, they're not very many… So I think it does matter… people in average, I would say, are around 30, or close to 30 when they finish their PhD theses. And then it's kind of expected of them to go abroad.* (FGg_pd: 175-90, engl. orig.)

What this quote interestingly suggests is that environmental conditions such as high competition and time pressure might contribute to reinforcing the normalisation of academic careers in that having a large pool of qualified applications necessitates a tick box approach to choosing candidates.

Not surprisingly, career steps were seen as directly sequential. Researchers have often stressed that there is no time for hesitation, no way back and that to keep moving is the only way to remain in academia. It was thus considered important to avoid breaks in between contracts and career periods. At any point in time they seemed to envision only two options: climbing up the academic career hierarchy in a quick and linear way or dropping out. Leaving academia for some time and then coming back was not considered an option. It was also not considered an option to stay in one of the advanced stages for a longer period of time. This distinguishes an academic career from most non-academic professions in which the majority of people engage in a phase of apprenticeship that is followed by becoming and staying (!) some kind of a journeymen. Further qualification for becoming a master craftsman is an option but not a mandatory for being able to stay in the profession. Pursuing an academic career in the life sciences by contrast requires a constant advancement from one step to the next up until a relatively late age. In their narrations, the assumption was clear that those who need too much time to finish their PhD thesis will have a hard time finding a postdoctoral position and researchers who take too long to qualify for and be able to attain a lab leader position will sooner or later reach an age where they can no longer continue. Being too slow is thus experienced as a dropout scenario. The postdoctoral period was described as a particularly decisive time. Researchers said that this period was *"not meant to go on forever"* (m2: 720-1) or that there was an expiration date on being a postdoc: *„Eventually it's over, you cannot be a postdoc any more when you're 40"* (m5: 344-5). Felt et al. have described this idea of a teleological career as follows:

> Becoming an independent group leader is seen as *the* goal, the other… phases are conceptualised as stages one needs to pass to arrive there. Neither being a PhD student, nor being a postdoc is seen as an institutionally accepted mode of existence beyond a certain point in time in an academic life. (Felt et al. 2008: 9)

All three stages, Masters, PhD and even the highly qualified postdoctoral period are considered transitory positions. Until the first postdoctoral years, it was considered quite easy for young researchers to keep moving forward. Especially if they were willing to go abroad and accept short-term and part-time positions researchers described it as relatively easy to find a lab that would employ them until they got too old. As a result of the well-defined career models, each step appeared almost self-evident to them and many experienced their life course as having "*ended up in this track*" (f1.1: 56[xxviii]), during which time they had made only "*semiconscious decisions"* (f1.1: 46f[xxix]). Emília Rodrigues Araújo has once said that the PhD seems to be conceptualised "as a beginning, as a stepping stone to an academic career" (Araújo 2005: 192).

7.2. Career Entanglements and Career Construction

How such normative ideas about academic careers develop is not self-evident. Researchers' narrations often suggested that different governing measures that impinged on their epistemic living spaces carried partly contradictory ideas about how they should plan their professional paths. How the convergence of these different imaginaries yielded their relatively normative ideas about what their career should look like therefore requires giving attention to both the ideas of career that were inscribed in their environmental contexts and their ways of acting upon them. Stephen R. Barley's "structuration" model of career is a useful thinking tool for this exploration. Drawing on his analysis we could say that social contexts carry different career scripts and that individual researchers are positioned „between career scripts and patterns of interaction as well as the relation between such scripts and the institutions they encode" (Barley 1989: 54). He suggests that both structure and agency – and the relation between them – need to be considered when we want to understand how hegemonic models of careers are stabilised and transformed. As Elizabeth Craig has put it, such scripts simultaneously open up and close down what is possible to imagine as appropriate career choices:

> Career scripts delimit options by encouraging pursuit of certain careers and also by discouraging pursuit of others. These are institutional rather than relational influences – they are the implicit understandings and expectations, as well as explicit rules, of a society or community. In general, career scripts make clear the available and appropriate career possibilities. (Craig 2009: 126)

Following up on these accounts, Joanne Duberley et al. have argued that we need to consider the „diverse (and frequently intersecting) institutional contexts in which scientists seek to develop their careers, and their characteristic modes of engagement with such contexts". They seem to have ways in which they negotiate with their social context and sound out room to manoeuvre (Duberley et al. 2006: 1131+5f). In doing so, people seem to invoke institutional scripts and/or personal preferences in structuring their options as they engage in an active process of career construction in which they make sense of and make choices from the imagined career possibilities (Craig 2009: 115). In the following I will trace the ideal-type imaginations of life science careers in academia back to career models inscribed in their environment and therewith provide a framework for understanding the career construction processes and the challenges that different career scripts carry for individual career-making.

Career policies of academic institutions in the Austrian context are guided by the notion that there is a need to discourage researchers from pursuing institutionalised careers (so-called „Hausberufungen") in favour of flexible and more international academic careers. This flexibility is commonly linked to ideas of making universities more responsive to the changing needs of society, of assuring the quality of academic research via periodic performance assessment and overall, of promoting higher levels of innovation and productivity. In the past decades, a variety of governmental measures have been designed to further these ideas and increasingly more actors have come to influence academic career cultures. Besides formal qualification requirements, particularly strong influences on researchers' ideas were the length and performance requirements of employment policies in academic and funding institutions as well as perceived norms within the international scientific community. I will in the following make an attempt to unravel this entanglement during the PhD and postdoctoral phase.

In the Austrian context, PhD positions used to be legally defined as 4-year pre-doctoral university positions. However for the case of academic life science research it is interesting to note that – in contradistinction to the social sciences – there were almost no such pre-doctoral university positions available for PhD researchers. Rather, they were funded by either externally funded PhD programmes or by the research projects of their more senior lab members and were usually three years in duration. What was considered an appropriate timeframe for finishing a PhD (i.e. three years) therefore seems to be the result of an entanglement of employment policies of academic institutions and of policies of funding institutions. By largely leaving the funding of PhDs to external sources, universities increasingly allowed norms of research projects to define the temporalities of academic qualification.

Similarly, the idea of having to be flexible and go abroad in order to pursue a career seemed to be the result of a variety of measures that made mobility a prerequisite

for getting an academic position or funding. Most Austrian universities for instance today restrict the stay of researchers to six years[13] in order to motivate researchers to go abroad and to rejuvenate the universities' work forces. The mobility of researchers is also increasingly regarded as a condition for getting new funds. This mobility-imperative was narrated as introducing new dynamics for the individual construction of academic careers. It is somewhat paradoxical that while academic institutions now have less power to define what the overall careers of their scientists will look like since they have decided to only employ them only temporarily, such institutional policies have proven to be quite powerful in enacting the globalisation of academic careers. In doing so, they have invited international career scripts to influence researchers' imaginations an ideal academic career. In other words: in that international mobility is increasingly inscribed in local employment policies, it seems that the career scripts of the international research community have become more important than they were before.[14]

Overall, within life science research cultures a standard hegemonic career script seems to have established that generates more certainty in the process of career construction but at the same time also creates moments of tension and unease. For instance they suggested that career scripts caused problems because they tended to not only standardise academic careers but also their private life and personal relationships, as this quote indicates:

> *I don't want to sit alone somewhere right out in the sticks at a university and without my social environment. I personally need my social environment to somehow function, I would say. It did it once and I would not do it alone again. And that's why an academic career is not right for me, with that… decision, right?* (f4.2: 814-8)[xxx]

Others worried that the mobility imperative of contemporary academic careers limited their chances of having a relationship and children. In particular the postdoctoral period was seen as a decisive time. Because of the de-facto abolishment of permanent positions and a generally high degree of competition, they were seen as in increased performance pressure since it was assumed that at some point it would become difficult to switch to a job in industry, as a senior postdoc describes it:

> *Because then it is really difficult to go into industry, when you are 35 or so already, right?… between 35 and 40… that's when your whole life is decided upon again. And that's frustrating a bit, when you work towards achieving something, and then it doesn't work out, right?* (f5: 772-6)[xxxi]

[13] For part-time employment they can stay up to eight years and in exceptional cases up to 12 years.

[14] In doing so they have also invited ever more normative and internationally accepted modes of measuring quality and evaluating performance. For more detailed discussion of possible follow-up dynamics of such ever more standardised criteria see chapter nine.

Younger colleagues seemed to learn from observing more senior colleagues that this one-way trajectory of the academic career can create a sense of anxiety. As a result, PhDs often wondered how big a personal risk it was to pursue an academic career:

> *I don't want* to [be in the situation that]… *I decide for this* [academic career] *path and from 38, from 40 on there is no funding, no perspective. What would I do then, right? Then I am… so specialised that no one can use me anymore. Really, what can I do then?* (f4.1: 829-33[xxxii])

Because of these risks, academic career norms were already in early career stages often narrated as a source of anxiety.

7.3. The Double Bind of an Academic Life

There is one more specificity of academic career construction that requires special attention with regard to experiences of uncertainty, ambiguity and tension; namely that living and working in science is characterised by two different orientations: individual researchers on the one hand need to build their individual, increasingly mobile and flexible careers while on the other hand they (are asked to) contribute to the sustainable development of the local academic institutions that host them. Researchers are thus located in between two different trajectories: a model of an entrepreneur and a model of an employee of a company. The interviewed life science researchers – particularly from the postdoctoral time onwards – frequently described their work using entrepreneurial metaphors, such as in the following quote:

> *I have the sole responsibility, but then I also get all the credits. …You always have the feeling – and that is a very important attitude – that you are first and foremost working for yourself. On the bottom line, you are like a self-employed person… you know that you will own the credits for your work afterwards, your name is on the paper, in the best case you can personify your research findings… That is a very important thing, that you don't have as an employee in industry… And that's why the personification with the own activity is very big. And that is the motivation. That is the best description of what we are doing… working as a self-employed person… to be able to apply for new projects, it is as if I would compete for customers. That's directly comparable…* (m2: 236-771)

What these lines exemplify are core dimensions of entrepreneurism: independent responsibility, individual accountability and individual ownership of the products of one's own work. In recent years, many scholars have suggested that the scientific vocation is increasingly turning into real entrepreneurial work; it is not only scientific excellence or intelligence that are required for an academic career but rather an entrepreneurial spirit and a willingness to take personal risks (Torka/Borcherding 2008, Shapin 2008). To denote this entrepreneurial orientation

in a profession, Ulrich Bröckling has coined the term "the entrepreneurial self" (2007: 275, German original). He argues that this specific mode of subjectification requires conscious personal development, career planning, self-monitoring and the ability to take on personal risk. It is a mode of subjectification that requires workers to regard themselves as their own primary resource:

> The entrepreneurial self is a capitalist of the human with regard to its own life: it treats its knowledge, its capacities, its health but also its outer appearance, social contacts and personal habits as scarce resources, that require investment to build them up, maintain them and increase them. Even if it does not own material goods, it has at its disposal its life time and can utilise it economically. (Bröckling 2007: 275, German original)

In other words, an assignment to act in an entrepreneurial way becomes the vanishing point of individual efforts of optimisation. The self becomes a reflexive project that is subject to self-monitoring for the purpose of making constant life course adjustments along these lines. This also is a fitting description of the way that life science researchers conceived of themselves in the context of their academic career biographies. When Bröckling speaks of an "obligation to become an individual" and self-modelling with which individualised subjects are generated (Bröckling 2005: 24f, 275, German original) he describes forces similar to those that create the ideal of an academic career.

Alongside these entrepreneurial metaphors however, researchers also speak of institutionalised limitations to actual entrepreneurial independence. In contradistinction to actual entrepreneurs, a career in the life sciences is not thinkable without being tied to academic institutions since the merit of a researcher always relies on the reputation of his/her institution and colleagues. Particularly in the life sciences, the reputation of young researchers is closely linked to the prestige of the lab leader. And secondly, in order to carry out their research, life science researchers depend on the means of production that only a large institution can provide: expensive technical equipment of a lab and the social collaboration required for using it. In that regard, their situation seems to be independent and dependent at the same time, an ambivalence that is expressed in the following quote in which a postdoc speaks about being very independent with regard to the course of her research, but also dependent on securing an institution for actually doing it:

> *…particularly in those areas in which there are rather short-term contracts and in which you depend on the advocacy of heads of department or things like that. Because in a young scientific career you cannot speak of independence. In fact we have a lot of it, not in the way we work but we have it, right?* (f6.2: 353-7)[xxxiii]

The "entrepreneurial self" as a label for life science researchers therefore seems to be only partly accurate. It applies to the epistemic sphere but not to the institutional context. Indeed like an entrepreneur, researchers symbolically possess the goods that s/he produces. Research results are published in his/her name and become parts of his/her Curriculum Vitae (CV) that in turn – metaphorically speaking – represents the researcher's institutional capital. The latter determinates their value as a human resource but at the same time universities are increasingly – factually and symbolically – taking possession of it as well.[15] However, the researcher will at least in most cases still be able to write his/her publications on his/her CV and take it with him/her when s/he leaves for another institution. In this reading, the CV of an academic researcher can be seen as a form of employee profit sharing ("Mitarbeiterbeteiligung") that goes both ways: the university formally owns the products of research but the better the overall performance of researchers within an academic institution, the more value will be ascribed to the individual researchers his/herself.

Overall thus, researchers can be conceptualised as being in an ambivalent position, allowing us to get a better grasp on the partly tense and ambivalent processes of subjectification that I have hinted at in the previous chapter. Their academic environment requires them to see themselves as entrepreneurial careerists that work for their own profit while at the same time they are also bound to see themselves as employees who work for the profit of their university. This often tense double-bind of contemporary academic careers appears to play a role in researchers' ways of contemplating how to live and work in their institutional contexts and is a factor in understanding the specific way in which processes of subjectification are taking place in academic research cultures (cf. Lohr/Nickel 2009: 8).

In summary we can say that several moments of tension appear to be linked to the perceived normalisation of academic careers. As I will work out in the following chapter on flexible working conditions, young researchers in the life sciences start from a quite different position – some from PhD programmes, some from project contracts – which carry different possibilities for fitting into the standard career scripts – particularly when expectations of private life are considered as well. It could be argued that we are facing the paradoxical situation that while increasingly powerful and more normative ideas about academic careers are created, researchers are increasingly less able to actually reconcile their professional biographies with these ideas. Felt et al. speak in this context of an new kind of managerial work that is emerging in which researchers "reshape their epistemic living space to fit better with their own expectations, or to resist shifts and changes that are perceived as negative" (Felt et al. 2008: 7). In the everyday workings of life science research it

[15] This point will be further discussed in chapter nine on the commodification of academic activity.

seems that researchers develop practices of consolidating such academic career scripts with perceived realities and their own expectations about life and work.

8. Casualisation in Academic Employment Cultures

> *I was continuously funded and I am still funded by* [this large-scale programme]*, but it was again and again interim funding. In fact I have the whole time been working on the same topic, but in the meantime I have signed the sixth or so supplement to my employment contract… Everything is changing completely right now, there are a lot of factors involved… so that my contract… ends at the end of the year… in the meantime there is a solution but it was more than half a year quite uncertain whether it will go on at all… there would have been enough money to employ me. The situation was so that I could have continued my research in peace, but* [because of new legal regulations] *the* [university] *said: No… you will not get a contract for longer than a total of six years… and your contract ends at the end of the year…* (m4.2: 36-71[xxxiv])

What this life science researcher hints at here is a complex web of environmental conditions that play a role in academic employment cultures today. He describes the interrelation of legal frameworks, project funding and the universities' employment policies as shaping a contract culture that for him resulted in subsequent short-term contracts. As mentioned above, the idea behind such flexible academic careers (and working conditions) on the policy level is that academic life must be prevented from remaining at a standstill. Mandating frequent changes in personnel is meant to enliven academic research and teaching and to thereby make it more innovative and adaptable to changing societal needs. However, while from the systemic perspective this flexibilisation is figured as a means of quality control, it becomes immediately apparent from listening to life scientists that it means much greater employment uncertainty for the new generation of researchers. Virtually all of the young life scientists interviewed had temporary contracts, mostly for two or three years but sometimes only for a couple of months. While statistical data does show that a greater focus on external project funding has created more jobs in research, it has opened up more possibilities to employ researchers for shorter durations and on a part-time basis. In English-speaking contexts such changes in contract cultures are often referred to as casualisation (Bunting 2004) – i.e. as a process of reducing the commitment of institutions to employing its personnel on a longer-term basis and thus making its personnel more easily exchangeable (casual). In this chapter I will untangle the relational web of employment in academic life science research in order to get a better understanding of the policy background against which the flexibilisation of employment in academia is happening and to understand the implications that this might have for pervasive uncertainty-experiences in the everyday of life science research cultures.

8.1. Contemporary Working Conditions in Academia

Contemporary working conditions in academia are best understood in the context of the recent changes in its legal frameworks. The University Law 2002 was a landmark change that passed in 2004[16]. It has not only formally introduced the concept of university autonomy to Austrian universities and created in its wake new forms of public management, but has also changed the funding and employment patterns in academic institutions, particularly for the younger generation. It abolished a tenure-track system[17] and has changed the legal status of project-funded personnel by formally integrating the often rather young contract researchers into the pool of human resources of the academic institutions. This and other measures have set powerful incentives to increase project funds and the number of (temporary) contract researchers. As a result there has been a 20% increase in third-party funding at Austrian universities within the four years following 2004. By 2007/08 25% of scientific and artistic personnel were third-party funded (BMWF 2008: 17, 22). When the teaching staff is excluded from the calculation, roughly 30% of research personnel were employed as contract researchers (Betriebsrat 2009). Since for contract researchers there is a larger range of choice in terms of how many hours a researcher is employed, this development seems to have influenced the amount of part-time employment at Austrian universities. While the total of university employees increased by 13% in this period, the full-time equivalents only increased by 3,8%. This shows both that, indeed, more people work at universities today than ever before, while at the same time they are, to a larger extent, only employed part-time (BMWF: 2008: 22). Statistics however also show that the relation of traditional university-positions (like assistant positions) and externally funded positions varies widely between academic disciplines. While in sociology the number for contract researchers is very low[18], contract researchers can make up to 50% of the personnel in the field of the life sciences. In fact, at the Max F. Perutz Laboratories (MFPL) – one of the research sites where the empirical material for this PhD was collected – 80% of female and 60% of male scientific personnel were funded by third-parties in 2006 (Felt et al. 2007b: 23).

Since the number of researchers that are employed on external projects is so significantly high, I will explore in more detail what it can actually mean to be a contract researcher in the academic life sciences. On a surface, project-based funding seems like an efficient way to structure academic research. Since funds are

[16] It must be mentioned here that a second landmark for contract cultures was the collective contract that came into force in 2009. Since within the empirical phase of this study (2006-2008) it did not play a role yet, I will discuss its implications only briefly in the footnotes.

[17] A new collective contract has partly introduced such positions again but so far it seems that shrinking the total budgets for universities will prevent universities from making use of these positions.

[18] This is partly due to fewer possibilities to acquire funds.

given on an internationally competitive basis, the quality of research seems guaranteed and as long as this quality can be maintained, a researcher will be allowed to stay and do research. However as the quote at the beginning of the chapter suggests, the situation can be a bit more complex on the ground. What can be learned from exploring researchers' biographies is that even if funding is guaranteed, legal frameworks and employment policies of universities can still interfere with individual working conditions and thereby with the prospects of making an academic career. Today, a frequently discussed matter within research communities is the way in which academic institutions decide to handle legal frameworks. In a statement to the employees of the University of Vienna, the works council has emphasised this:

> *The collective contract is a binding legal basis for all universities… the actual implementation however is yet to be complemented by university-specific company agreements between the rector and works councils.* (Betriebsrat 2009)

During the interviews it indeed appeared to be the case that institutional policies can be of higher relevance for individual researchers than the broader regulations as such. In the following I will explore this with respect to the handling of project funds and the employment of contract researchers.

While project funding has for some time before the University Law been an alternative possibility for academic research, research personnel were employed by the universities. Since 2004 then, external projects are much more integrated into the workings of academic institutions and the university is no longer the employer of contract researchers. This has had substantive implications for researchers' working conditions. Most importantly it implied that the way that universities handled the regulation of "consecutive (or chain) contracts" (Kettenverträge) which says that individuals cannot be employed by a university on temporary contracts for longer than six years (or eight years in case of part-time work); or put differently, the regulation says that temporary contracts by law turn into permanent contracts when a person is employed for more than six years on temporary contracts by the same employer. Individual universities have decided how to handle this regulation differently. While some smaller universities have decided to make permanent contracts an option, other universities – such as the University of Vienna and the Graz University of Technology – have used this regulation to argue for a maximum of six years (or 8 years part-time) of university employment. The popular argument for doing so is that the limitation is necessary to facilitate a flexibilisation and mobilisation of the universities' workforce (Betriebsrat 2009).[19] For the pursuit of an

[19] A similar situation can be described for the implementation and handling of the collective contract that came into force in 2009. While it was on the one hand widely welcomed by the union (Gewerkschaft Öffentlicher Dienst – GÖD) and some interest groups because it opens up options for tenure-track policies, higher standard salaries and provided a range of possibilities to employ

academic career in Austria this often means that researchers are allowed to stay for six years and then allowed to return after one year abroad. At the same time, universities do not seem to have learned how to develop a sustainable and reliable personnel plan, which leaves the next generation of researchers with relatively few fixed reference points for their career planning (cf. Plattform Drittmittel Personal 2009). Their employment status is best described as "permanently temporary", to use a term from Marc Bousquet (2008: 15). Employment is mostly unstable beyond the project duration and as statistical data show, contracts are rarely standardised with regard to length and working time, making employment details – to significant extents – a matter of individual negotiation. In the sample of interviewees for this PhD study there were, for instance, cases where contracts were given only for part of a project period and then prolonged yearly or only every few months by the lab leader. Since in temporary contract cultures mid- to long-term career planning seems impossible, Ylijoki and Mäntylä have suggested speaking of a "contracted time experience," where academic researchers experience

> …a sense of time as something that is terminating combined with an uncertainty about the future. The orientation is towards the end of the present contract (how much time do I have left?), and a worry about the future (how/when/where do I get the next contract?)… Being constantly alert to the terminative present and the unknown future makes long-term planning difficult. (Ylijoki/Mäntylä 2003: 65f)

For those researchers who had a patchworked income in which parts of their salary came from different sources, the situation was often more complex since with more than one contract involved, more than one contract temporality needed to be considered, as a postdoc explained:

> *…my part of the project… from which 50% of my salary derives from, will end… at the end of March or April next year. That is, if nothing else comes up, I will lose 50% of my income.*

I: And the other 50% are?

> *A temporary assistant position… it has a longer term but it is basically not interesting. I will not continue with only half a salary.* (m1.2: 21-33)[xxxv]

When we want to understand what makes researchers experience their working environment in a specific way, it thus is advisable to think about different levels of regulation together with researchers' ways of handling them. Neither the project

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contract researcher on permanent contracts, it was on the other hand criticised for its wide range or possibilities to make use of them or not. For instance, at the University of Klagenfurt many positions have been converted into the newly introduced personnel category of permanent „staff scientist" positions while the University of Vienna there are still very few of these positions. Experiences since 2009 show that the increase in salary has often led to a decrease in contracted working time and in turn to an increase of overcommitted time. Also, it did not change policies of handling chain-contracts so that temporary contracts are still the overall standard and universities' limited basic funds do not seem to allow them to announce tenure-track positions to an adequate extent.

mode of funding nor the regulation of consecutive (chain) contracts or institutional policies alone is sufficient for explaining the casualisation of employment in academia. To understand how researchers choose to live in these contract cultures, we must also consider the ways they experience their work (as subjectified) and the ways they experience academic career trajectories (as normative). Even if employed part-time for instance, researchers tended nevertheless to work full- time or overtime. The sample of life science researchers at the University of Vienna that we studied were on average employed 79% –approx. 32h/week. Our study however showed that 90% estimated their real working hours as being more than 40 hours and over 50% with more than 50 hours (Felt et al. 2007b: 30, 44). This liability to work overtime was often emphasised in our interviews as well. Asked whether her employment was full-time, an early PhD student answered:

> *Exactly, yes. 40 hours under quotation marks (laughing).*

I: Then I guess then it is a bit more?

> *Yes, mostly it's about between 50 and 60.* (f8: 204-8)[xxxvi]

In a focus group discussion, some researchers suspected a systemic logic was being played out in this compulsion to do overtime work and assumed that they were hired as cheap labour:

> *…young people breathe new life into it. And when you are the boss or when it is about money, the case is very clear: young people don't cost anything and they work twice as much as those who sooner or later, when they get older, start being more interested in private life.* (FGg_jun: 339-44)[xxxvii]

Following this statement, several other PhD students in the group discussion shared the opinion that they were easily exchangeable:

PhD1: Yes, I think it is becoming ever more difficult. In the past, it was easier…

I: How far?

PhD2: Yes, because the performance pressure was not that high.
PhD3: We have become so replaceable.
PhD2: Right. Right. We have become replace-, that's a good expression. We have become completely replaceable, yes.
PhD3: You can go wherever you want; someone else will come who can do the same things.
PhD2: Yes, but never mind, and especially, when a master student follows, he will cost me nothing and works twice as hard. Yes, that's rightly said, yes. (FGg_jun: 356-72)[xxxviii]

What is implicit in such narratives about being exchangeable and (casual) work and often also explicitly addressed was a subtle fear of what would happen to them when they droped out after having tried to make an academic career.

> *And that is the problem somehow, when you say: Yes you do a postdoc, 3 years, maybe then again 2 years, and again 3 years. And at some point, maybe there is no money any more. We have such a case. He is 40. And 40 is far too late for industry, no one will take him.* (FGg_jun: 585-9)[xxxix]

Indeed the international scientific community lacks awareness about this category of researchers who will sooner or later drop out of the academic system and might then have trouble starting a career outside academia. In his article in the prestigious journal *Nature* called "victims of success", Eugene Russo points out that „the system mainly recognizes those scientists who succeed in the well-trodden career pathway towards being a faculty member." And then argues that "(f)inding and promoting additional pathways may be necessary" (Russo 2003: 354). Still however these researchers are usually not considered by personnel policies of academic institutions who emphasise the imperative for academic career planning. Despite the fact that most will sooner or later (have to) leave academia because of harsh competition, all are encouraged to pursue an academic career instead of weighing their options inside and outside academia and proceeding accordingly. Somewhat paradoxically thus, despite an awareness about high competition and few opportunities for more permanent employment, the academic path seems to persist as the dominant career option for many young researchers. As I have explored in the chapter on subjectified work, it can be argued that this might also be the case because academic research is considered a job that is fulfilling, with quite a lot of freedom and hardly any estranged work.

8.2. The Academic Job System

This reflects a more global situation in which an increasing number of young researchers are attracted to an academic career and are accepted as candidates, while relatively few are likely to succeed in actually attaining one. In 1997 it was argued that in the U.S. the supply and demand of academic jobs had already become dramatically unbalanced. In the subsequent years, the share of researchers who had a tenure-track position four to six years after their PhD dropped from 25 to 15 percent between 1993 and 2003 (Benderly 2005). During this time, the number of PhD graduates in the field of the life sciences increased by as much as 70%. Ulrike Felt and Maximilian Fochler conclude that there is an increasingly large number of qualified researchers in temporary positions on the one hand and a relatively small amount of promising positions on the other (2010: 316). Even if on a systemic level there is more awareness that there is little chance of achieving an academic career, the pressure to be first and to compete globally drives already established researchers into employing "armies of graduate students and postdoctoral fellows" in order to "make their laboratory groups the smartest and fastest" (Anderson et al.

2007: 440). This competitive situation was recently described as "Darwinian-like system" where survival of the fittest was seen as a predominant value (Felt/Fochler 2010: 316, German original). Within the past ten years, this steep academic employment pyramid has begun to raise concerns on the policy level as well and even beyond academic employment possibilities. A report by a High Level Group of the European Commission on *Increasing Human Resources for Science and Technology in Europe* reflects this concern:

> *Today, the gap in research investment and employment is primarily on the demand side: the desired jobs simply do not exist and will not appear in the requisite numbers just because people are being trained for them. Industry is not demanding more researchers except in a few niche areas.* (Gago 2004: 82)

Here we see the academic job system as a place where chances are high that researchers who start an academic career will at a certain point have to start anew in a non-academic career trajectory or enter a different profession altogether. Considering this situation, some authors have argued that PhDs are produced "not because of a massive demand for new faculty" – nor as highly qualified workers for the knowledge economy but because of an institutional demand for cheap graduate student labour (Nelson 1998). Borrowing a notion from Chandra Mukerji, it could be argued that an „elite reserve labor force" (1998: 190) for the academic system as well as for the emerging knowledge economy is being maintained.[20] This reserve however seems to not primarily have the function of filling positions because of an increased demand for employees. First and foremost it creates a situation in which an increasing number of people compete for relatively few positions, as a group of authors recently concluded:

> In short, there are many people (the oversupply factor) competing for prestigious, desirable and scarce rewards and resources (the funding factor), in a struggle that bestows those rewards disproportionately on those of marginally greater achievement (the tournament factor). This situation is supported to the detriment of that 'legion of the discontented' and to the benefit of senior investigators, because it 'generates good research by employing idealistic young graduate students and postdoctoral fellows at low cost'... In other words, the benefits accrue to funding and employing institutions. (Anderson et al. 2007: 441)

As a result "many young biomedical Ph.D.s are locked into long 'holding patterns' at the postdoctoral level before they get a 'real' job", as was observed in 2003 in the prestigious journal *Science* (Juliano 2003: 763). The contemporary academic job system therefore appears to produce an artificially large number of highly qualified people on the supply-side, which is a source of anxiety for researchers because they

[20] Chandra Mukerji had argued that the US had maintained a skilled „elite reserve labour force of scientists after World War II to make use of their potential in the case of military conflict or war" (1998: 190).

will only find out quite late in life whether they will be able to have an academic career or not. Many – even very young researchers – believe that they might be too specialised for non-academic jobs when they decide to leave after a postdoc (Teitelbaum 2003: 49).

In this context, Marc Bousquet has argued that – as opposed to the notion of a job market – it might be more accurate to speak of a job system. He lays out his case historically by tracing the discourse about the situation of academic labour post 1945. While the first wave was concerned with "the analysis and commitments associated with the movement for unionisation of the tenure-stream faculty in the 1960s and 1970s" the second wave became dominant in the 1980s and came to see academic labour in terms of a job market. By considering a neoliberal market ideology behind this shift, Bousquet argues that this wave [o]riginated "as management's oppositional knowledge in response to the emergence of faculty and student power". In this phase he further reasons that a concept of an academic labour *market* developed in which supply (degree holders) and demand (tenure-track job advertisements) would meet. In this market-logic, an overproduction of PhD holders would quasi-naturally lead to a decrease in PhDs production. From this perspective, the market "provides an imaginary solution – the invisible hand – to a real problem" (Bousquet 2008: 21). From this perspective, graduate education appears as a waste product of the academic system:

> Yet at precisely the juncture that this 'preparation' should end and regular employment begin – the acquisition of the Ph.D. – the system embarrasses itself and discloses a systematic truth that every recent degree holder knows and few administrators wish to acknowledge: in many disciplines, for the majority of graduates, the Ph.D. indicates the logical conclusion of an academic career. (Ibid: 23)

Bousquet then closes by suggesting a third-wave of reasoning about academic labour that characterises the present situation as an academic labour *system* that is characterised by figuring postgraduates and graduates not apprentices nor primarily as students but as being workers. Using the notion of the academic labour system, "labour markets" appear as socially constructed:

> The intervening official knowledge [about a job market], informed by liberal economic determinism, works to conceal the operation of a policy universe (social, legal, institutional) that shapes academic working conditions. (Ibid: 41)

What is implicit in such writings is a latent labour dispute. In Bousquet's account young researchers appear as people who have the "right to bargain collectively" instead of having to accept uncertainties in their work conditions (Ibid: 41). The specificity of work cultures in academia – including a personal, subjectified relation to research activities, the individualistic career orientation and a strong willingness

to invest in an academic career – however seems to present challenges to collective bargaining. The particularities of how research activities are organised further raises the question of how collective bargaining can be organised in such contexts. Researchers told us for instance that the writing of a project must start months or even a year before actual funding can be expected. Indeed it seems that rights of labour were in that sense subverted at several points within academic work cultures – but often on a voluntary basis. Researchers mostly do not conceive of the overtime in which they are writing grants, managing projects or networking for their careers as unpaid work, even when those are unavoidable tasks for making an academic career. The moments of potential labour conflict thus seem to be somewhat different than in established unionised work cultures. It for example seems that conflicts are often not dealt with within the contracted time but in the private life in terms of stress management. Stefan Nowotny has put this for project work cultures more generally: How people perform in their work is supported by subjective coping capabilities:

> …part of the subjective performance, that the project form utilizes, that is externalised in that the costs are not covered and the ‚costs' as subjective are deferred in the realm of what we have learned to call the precarious form of life. (Nowotny/Raunig 2008: 112, German original)

This implies that these workers need their interests represented in a way that goes beyond the concrete work context. In regard to the Austrian situation, traditional interest groups such as unions and workers' councils seem to have a hard time adapting to these conditions. Workers' councils in academic institutions have only existed since the legal constitution of universities in 2004 and they still have not established appropriate practices of representing the interests of the universities' employees. Also, there is no union that explicitly dedicates its resources to the specific issues affecting young researchers. In academic work therefore, the traditional form of unionization is in crisis and internationally, young researchers seem to prefer "professional society models" such as the National Postdoc Association (NPA) over the labour union model because – as NPA leader Claudina Stevenson – once said, it is difficult to "bargain with your mentor for X number of networking opportunities and X amount of time" (Benderly 2003, cf. Bhattacharjee 2003). Indeed, beside such traditional models, self-organised interest groups experiment with different forms of representation. They make visible casualised conditions of work in academia and are beginning to be recognised by the media. However they are still small, not well institutionalised and their activity is mostly limited to consciousness-raising public relations work and public events. This situation leads to a vicious cycle in which conditions of precarity prevent researchers from unionisation and collective bargaining while the lack of interest in organising in turn contributes to the casualised conditions of academic workers.

On a systemic level the concern has been raised that casualised conditions can compromise research quality through the regular disruption of research activities and local collaborative processes. Furthermore there are fears that universities are turning into mere transit-institutions in researchers' biographical context. Having fewer attachments to the academic institution could then diminish their investment in institutional infrastructure and local collaborative activity. When listening to life science researchers it seems that such casualised working conditions are at odds with what they had expected from working in academia and with their optimal research conditions. They seemed to feel that a permanent uncertainty about their professional future was occupying their minds and time – thus making it not only a matter of personal unease and anxiety but also of taking their mental and bodily capacities away from research and putting them towards the management of their careers.

9. Commodification of Academic Activities

> *Yes, the funny thing is, basically it is quantified. Basically, in a way the life course is validated in these impact factors, right? … And of course it is the case that… molecular biologists… they can publish in journals that have a higher impact. This is the result of them* [molecular biologists] *being simply more numerous. There are more people who read it, and as a result there are more who cite it. And at the MFPL it seems to be the case that people are rated using the number or the average of impact factors, in the past three years or something.* (FGk_pd: 249-60)[xl]

Another angle from which experiences of uncertainty, ambiguity and tension in transforming research cultures can be explored is the rationales used to ascribe value to research. As in the above quote, researchers experience their activities as increasingly rated according to measures that are quantifiable and rather abstract – such as the number of publications or impact factors. This "growing trust in numbers" is described as a relatively recent development that aims at making academic research more productive and as facilitating competitive and fair distribution of funding resources (cf. Felt 2009: 29). New empirical studies and a recent strand of social theory have suggested that this reorganisation can be understood as a process of commodification – i.e. a process by which things are increasingly treated as if they were for sale on a capitalist market. In this chapter I will discuss how far and in what sense it is legitimate to say that for contemporary academic life sciences "university intellectuals are increasingly trapped by the relentless logic of commodification" (Shumar 1997: 9). In order to understand this process and explore to what degree such a notion might be useful with regard to academic knowledge production I will first discuss the theoretical concept of commodification as Karl Polanyi has described it in his work "The Great Transformation". Then I will review works that have addressed commodification with regard to knowledge production processes. I will continue by tracing how young life scientists speak about the way that value is ascribed to their research activities and the transitions taking place inside their research cultures.

9.1. On the Concept of (Fictitious) Commodification

What has come to be known as commodification is a process by which communities or societies learn to frame a specific human activity differently – namely by treating things and activities as commodities, as if they were produced to be sold on a (capitalist) market. While the term "commodification" only came into use in 1977, as a conceptual framework of thinking it has been central before to understand the emergence of capitalism in Marxist political economy. The particular notion of commodification that I am going to follow here is one that Karl Polanyi has

developed in his book *The Great Transformation*. In it he traces back the "political and economic origins of our time", the rise of the market economy and the great transformation that societies and economies underwent in the 19th century to a process of enclosure or, as it would later be referred to, (fictitious) commodification (Polanyi [1944] 2001). He does so by using the Industrial Revolution in England as an example and explains that the main factors of production – land, labour and money – began to be treated as commodities before they were integrated into the capitalist market economy. His central argument is that this was not a self-evident development but that societal efforts were necessary to establish a commodity approach towards certain aspects of life:

> …the postulate that anything that is bought and sold must have been produced for sale is emphatically untrue in regard to them. In other words, according to the empirical definition of a commodity they are not commodities. Labor is only another name for a human activity which goes with life itself, which in its turn is not produced for sale but for entirely different reasons, nor can that activity be detached from the rest of life, be stored or mobilized; land is only another name for nature, which is not produced by man; actual money, finally, is merely a token of purchasing power which, as a rule, is not produced at all, but comes into being through the mechanism of banking and finance. The commodity description of labor, land, and money is entirely fictitious. (Ibid: 75f)

He further argues that treating entities as commodities needs to be learned:

> The crucial step was that labor and land were made into commodities; that is, they were treated as if they had been produced for sale. Of course, they were not actually commodities, since they were either not produced at all (like land) or, if so, not for sale (like labor). (Ibid: 10)

What he means by learning to treat something as a commodity is to make it "subject to the supply-and-demand mechanism with price" (Ibid: 75). Treating an entity as a commodity thus means quantifying its value and inventing a rationale for comparing its value to the abstract value of other commodities. "Value" is therefore never given but the result of social negotiation or a struggle over what is regarded valuable and what its value is. Following Polanyi, it is the establishment of rules and routines that stabilises the perception of entities as commodities. Institutionalised rules provide a framework for calculating value and maintain the routines through which social practices adhere to the logic of these values. This is what Polanyi means when he states that a "market economy can only exist in a market society" (Ibid: 74). This rootedness in the social world has an important implication for him: things and activities are never completely integrated into the capitalist market system. Figuring them as commodities relies on a sophisticated process of organising the social according to the needs of the market. In this way, commodification is a complex enterprise of inventing and implementing social

institutions that support a commodity perspective – such as, for example, rationales of ascribing a quantified value (or price) to things. However, since they are often not originally produced to be commodities, this process always remains contested and embedded in other logics and practices that follow different codes and norms. This is why, in Polanyi's understanding, commodification always remains incomplete in that it is both supported and resisted by the social worlds that it is embedded in (Ibid). From this perspective, commodification cannot be understood as an absolute, historically determined, self-evident, smooth or irreversible process but as an active, fractious and self-contradicting social process and struggle that – as Karl Polanyi has insisted – must always be fictitious and incomplete in its character.

In recent theoretical debates, the concept of commodification has been taken up again to gain a better understanding of the neoliberal restructurings of society (e.g. Rifkin 2000, Jessop 2000, 2007). It is argued that while the commodification process of land, labour and money is far advanced, it is still a nascent process in regards to nature and many human activities – including academic cultures.

9.2. From Commercialisation to Commodification – A Shift in Perspective

That publications are the "*currency in which you are paid*" (f4.1: 909-11) is an often-repeated statement amongst researchers. It hints at certain logics with which academic researchers have come to interpret and value their activities: a product-orientation (publications), a specific way of quantifying their performance and an application of the codes of capitalist economy and wage work to academic practices. The first page of the genome research programme GEN-AU illustrates how prevalent this perspective is within life science research cultures. Under the title "The currency of science" it says:

> *The list of one's own scientific publications is the most important representation of every researcher and every research group, even of whole universities. Publications document what, how much and the quality of the research done. They also determine how much money is invested in the respective research area.* (GEN-AU 2012, German original)[xli]

For a long time now, analyses of scientific practices and ways in which academic career rationales are performed have suggested that academics cannot be regarded as hostile to or shut off from capitalist norms and practices. Bruno Latour has for instance portrayed a scientist as a "wild capitalist" in regards to the pursuit of his career (1996: 113ff). He argues that the way to make a successful career in the field of biology is by internalising capitalist market logics. When Sheila Slaughter and Larry L. Leslie coined the term "Academic Capitalism" in 2001 they hinted at a more systemic process that they saw as reinforcing market-like behaviour in academic research cultures. They saw it in

> …institutional and faculty competition for monies, whether these are from external grants and contracts, endowment funds, university-industry partnerships, institutional investment in professors' spin-off companies, student tuition and fees, or some other revenue-generating activity. (Slaughter/Leslie 2001: 154)

In these early writings they argue that a classic notion of commercialisation is at play and describe the direct links between academia and the market. In their later work they further develop this concept by including mechanisms beyond external market forces, mechanisms that are more internal to academia such as

> …organizational restructuring or interstitial emergence of new organizations; products, processes and services and their markets, public and private; managerial rewards and incentives, penalties and disincentives; and of course ideology. (Ibid: 155)

This expanded understanding of how capitalist norms and values interfere with academic cultures comes close to more recent empirical investigations of how the capitalist and academic worlds have become entangled.

Merle Jacob has outlined a recent shift in analytic perspective from a commercialisation-approach to a commodification-approach (Jacob 2009: 391f). A range of recent studies that explore the strengthening of capitalist norms and practices in academia can be subsumed under this approach even when they do not use the term. In the U.S. research context, for instance, Daniel L. Kleinman's and Steven P. Vallas do not posit any direct links between academia and the capitalist economy; however they do regard codes and norms as the crucial axis governing the contemporary transformation of academic work cultures (Kleinman/Vallas 2005). Using a study of academic work cultures in the field of biotechnology, they come to the conclusion that capitalist expansion does not primarily happen through "direct economic links to corporate organizations, but also and especially because of subtle, systematic influences" by which "academic organizations increasingly come to adapt practices… that were formerly specific to the corporate domain" such as "a proprietary view of knowledge" or "quantitative methods for performance appraisal" (Kleinman/Vallas 2001: 453). They conclude that on this level we are witnessing an "asymmetrical convergence" of academic and capitalist codes and norms that privileges capitalist values rather than established academic values. They suggest that in order to understand how the university works in the context of a capitalist knowledge economy it might be helpful to look at informal and indirect effects of the culture of commerce on the daily practices of academic science than to investigate the direct and open links to capitalist markets (Kleinman/Vallas 2005, 2007).

Such studies suggest that discussing the concept of commodification in a contemporary context requires moving beyond an analysis of the exchange of

knowledge for money. The concept of commodification as it was formulated by Polanyi allows a more nuanced approach by showing how social worlds are (re)organised in order for knowledge to be treated as a commodity. Following this line of argument, Bob Jessop makes use of Polanyi's concept of fictitious commodities to discuss the role of information, knowledge and intelligence in post-industrialist, knowledge-based societies and emphasises that we have to focus on the "social relations rather than naturalized factors of production." We must ask "under what conditions" does knowledge take "the form of a commodity" (Jessop 2007 122).

In the academic debate two aspects of contemporary research cultures employing commodifying rationales to reorder the way in which value is ascribed to research activities can be identified: structures of distributing funding resources and the criteria of quality control. In his work on university education in the UK, Hugh Willmott traces these aspects back to the introduction of managerialism into academic cultures:

> [T]he commodification of academic labor and the managerial control of academic work results from politico-economic pressures to demonstrate that funds are being directed in ways that are ostensibly congruent with the commodifying logic and priorities of capitalism. (Willmott 1995: 993)

Recent empirical studies have focused on the ways in which quality is measured within such systems of managerial control. As Braskamp/Ory have stated in regards to academic funding cultures, such quantitative measures like the number of publications or the Impact Factor have been introduced as a basis for decision-making in universities' internal re-distribution of resources and as an empirical basis for measuring "individual and institutional performance". They have suggested that the number of criteria for assessing research quality has changed the perception of what counts as valid knowledge and innovative knowledge production (1994). Similarly, Massimo De Angelis and David Harvie make the point that academic work tends to be measured in increasingly abstract ways:

> By implanting the criteria of quantification, surveillance and standardisation into the daily activity of academic labour, we argue, public managers seek to measure academic labour with criteria that are predicated on values other than the values of teaching, researching and the collective production of ideas and thinking… In thus doing, academic work increasingly becomes 'abstract' in the Marxian sense linked to alienation of 'human labour power abstracted from the form of its expenditure'… (De Angelis/Harvie 2006: 5)

On a broader societal level Michael Power has subsumed such tendencies under the label of "Audit Society" – the trend towards standardising performance criteria in order to be able to make informed decisions about the allocation of resources

(1997). Building on these concepts it could be argued that commodification processes are currently being introduced into the social world of academic research by forms of quantitative measuring and defining value through comparison.

9.3. Means of Commodification in Academic Research

In the Austrian context, the incorporation of commodifying rationales in research cultures can be traced back to the University Organisation Act in 1993 that was then followed by the University Law 2003, introducing consecutively "more business type management" and "service-orientation" (Felt et al. 2007b: 9). From 1993 on capitalist norms and values have become increasingly traceable in the organisational framing of academic research. The following standard phrase in employment contracts at the University of Vienna for instance exemplifies a stronger proprietary approach towards knowledge in institutional policies:

> *The employee acknowledges that the employer has full property- and immaterial goods-rights on the work achievements in the framework of official business and… grants him temporally, spatially and factually absolute, exclusive and irreversible usage rights*. (Standard Contract University of Vienna 2009, German original)[xlii]

Drawing from David Harvey and Bob Jessop it could be argued that this appropriation of the products of research is equivalent to a process of primitive accumulation or an accumulation by dispossession (Harvey 2003, cf. Jessop 2007: 121f). However, within the sample of labs and researchers that were studied, knowledge production was still mostly "unproductive" in the classical sense of producing marketable products. Even if there was increasing structural pressure to work towards patentable results and immaterial property[21], they still claimed to work towards knowledge that was publishable in academic journals instead of producing knowledge that might be useable for producing a marketable commodity. The phrase about property rights in employees' contracts therefore seemed to be largely unenforced.

Nevertheless it seems that in the context of this proprietary rhetoric and the growing structures that promote competitive funding allocation, quantitative evaluation and research quality assessments have gained much more influence in how researchers speak and think about their activities. In researchers' narrations they refer to a range of ways that these quantitative measures of value impinge on their epistemic living spaces. In the following I will explore two governmental measures within the empirical settings of this study that can be regarded as

[21] In the project GOLDII for instance every paper that researchers wanted to publish needed to be first screened for patentable content.

contributing to the treatment of academic labour and its products as commodities: first, research assessment and second, the project mode of funding.

Since academic institutions have been granted economic autonomy and competitive research funding has increased, efforts to assess academic performance have intensified. They now need to negotiate how much state-money they are allocated and in order to measure (and try to maximise) their productivity, universities have started to gather information on the performance and productivity of their staff in so-called "balances of knowledge" (Wissensbilanzen). The University of Vienna for example is legally bound to "submit the 'balance of knowledge' regarding the last calendar year every April 30th to the federal ministry" (Universität Wien 2010, German original[xliii]). Universities in Austria have thus begun to undertake "research activity documentation" and compile "balances of knowledge" that contain information about the performance of the university and its employees and are supposed to facilitate an informed allocation and re-distribution of resources:

> *This 'balance of knowledge'… serves as an integral illustration, evaluation and communication of immaterial asset values, performance processes/value chains and their impact and is to be used as qualitative and quantitative basis for the preparation and closure of performance agreements….* (Universität Wien 2010, German original[xliv])

They mostly include numbers that are meant to indicate the performance of the university and its sub-units. Researchers are regularly asked to fill in an online form that counts the number and value of their publications, conference talks or other academic activities as well as information about researchers' profile like memberships in scientific institutions. These quantified metrics have become not only an integral part of the internal organisation and the allocation of resources within academic institutions but also seem to have influenced researchers' ideas of what kind of knowledge is valuable within an academic career rationale. In the life sciences for example researchers usually identify the number and value of their publications – as it is calculated by the impact factor – as the most crucial measure of their performance. Asked what had changed over the past few years a postdoc said the following:

> *Everything now is accounted now according to impact factors. The allocation of money has been completely altered… here it is now divided according to… accumulated impact factors… everyone at the institute, his impact factors from the last five or three years, are summed-up and then the institutes are compared and the institute… will get the allotted amount.* (m1.2: 1189-202[xlv])

Researchers also talked about relatively strict rules about who owns the symbolic value of a publication. Very generally, a position at the front of the authorship list is more valuable than a position at the end. A publication is clearly most valuable for

the first one who is mentioned in the authorship list since this position indicates the person who put most work into the paper. The publication is similarly valuable to the person who is listed in the last position because it indicates the person whose intellectual guidance and ideas have led to the publication. Most frequently it is the lab leader who is chosen to be the last author. In a sense, the list of co-authors of a publication can be compared to the list of its shareholders. This rationale of ascribing value is reminiscent to a certain extent of the way that the value of a share is determined on the stock market. The publication gets its first rating when it is accepted to an academic journal with its respective impact factor – i.e. its expected demand (impact factor/frequency citation). Taking the metaphor further we could say that when a journal accepts the publication of a researcher this is similar to investing in an entrepreneur; the journal gives authors symbolic capital so that they will be able to carry on doing research and producing knowledge. The journal then expects revenue: When the paper is cited this will bring impact points to the journal and the value of the share that it has in the publication as well as the value of the journal itself increases. All this is calculated according to the defined metric "impact factor" that is retrieved from the Journal Citation Report (JCR)[22]. It measures the frequency with which an average article in a journal has been cited in a given period of time:

> *The impact factor for a journal is calculated based on a three-year period, and can be considered to be the average number of times published papers are cited up to two years after publication. For example, the impact factor 2011 for a journal would be calculated as follows:*
> *A = the number of times articles published in 2009-2010 were cited in indexed journals during 2011*
> *B = the number of articles, reviews, proceedings or notes published in 2009-2010*
> *impact factor 2011 = A/B* (Sciencegateway 2011)

What I want to show by discussing these ways of validating knowledge is that handling knowledge in terms of quantified metrics is not self-evident but the result of the invention of rationales and institutions that facilitate it (like the impact factor, RAD and knowledge balances). How a social world ascribes value to research activities and to knowledge, therefore, is the result of institutionalised calculation processes.

As will be discussed in the subsequent chapter ten, researchers sometimes question whether the measures described above are a reasonable way of defining what kinds of activities and what kinds of knowledge are to be given value. Nevertheless however such measures are described as becoming ever more powerful since the allocation of resources is increasingly organised according to them. In this context it

[22] It is a product of Thomson Institute for Scientific Information – that "provides quantitative tools for evaluating journals" (http://www.sciencegateway.org/impact/).

is particularly external project funding that is described as fundamentally changing the framework of academic activity – or more precisely it is considered a way to optimise and rationalise research using quantifiable and objectifiable performance metrics. The predominance that project funding has gained in academic research is reflected in the sample of this study. At one of the empirical research sites up to 80% of personnel were externally funded.

To understand what characterises these project-based work cultures and what tensions they involve, it is worth looking at critical management literature. A common definition of projects is that they are characterised by finiteness (projects are temporally fixed and have a defined beginning and end), uniqueness (they are singular and cannot be carried out twice) and collectivity (they mostly are conducted in collaboration with others) (Steinbuch 1998: 24ff). The concept of project-based management is that one project smoothly follows the other and that project teams are re-constituted depending on their priorities and needs. The knowledge sector has been described as particularly well suited for project-based organisation since it is regarded as a form of organisation that can bring together "expertise, information and skills from different disciplines" and perspectives (cf. Kalkowski/Mickler 2002: 120, German original). According to these rationales project-based organisation was expected to catalyse a transformation of work cultures in knowledge-based societies/economies (Ibid). Indeed "projectification" has even become identified as one of the main transformations happening in our society. Kalkowski/Mickler understand projects as an emergent mode of organisation (Ibid: 123); Bröckling describes projects as a "form of socialisation" (2005, German original); Boltanski/Chiapello identify the "project based Polis" as the main ideology of contemporary capitalism ([1999] 2006, German original); and other authors have even been speaking in broad terms about the "projectification of society" (cf. Hodgson/Cimil 2006: 5). Critical accounts of the project mode however have raised concerns about the tensions, ambivalences and homogenising aspects that it can bring with it. Marc Torka has argued that organising academic work in the project mode has become so commonsense that other modes of funding – such as temporally open and untargeted research – have become unthinkable. As researchers emphasised, a project is only likely to be funded when the proposer can give a plausible timeline for what s/he will be able to achieve within a given funding period. In the project mode, research appears as an activity in which valuable output can be calculated. Getting the next project funded depends on the amount and value of output/publications that the proposer has been able to obtain within the previous one:

> *…it is very difficult, without possessing publications, to say: Ok, I apply for another position. To say: I was here for two or three years and nothing came out of it.* (f6.2: 69f[xlvi])

In that sense, writing a project proposal is a mental exercise in projection, planning and predicting outcomes that – from researchers' perspective – can always only remain tentative and incomplete because of the epistemic uncertainties that are involved. In that context, concerns have been raised that the project mode creates inherent tensions within research activities. Georg Krücken has for instance argued that fixed and reproducible relations between means and ends contradict the inherently uncertain character of scientific research (2006: 16). Indeed the most prominent remark in critical analyses of the project mode is that it creates a necessary tension between the performance principle and these epistemic uncertainties (Thomas 2005; Kalkowski/Mickler 2002, 2009; Bröckling 2005; Torka 2006, 2009; Boltanski/Chiapello [1999] 2006, Boltanski 2009). Thus, while the idea of a project was originally to allow for the possibility of failure (Steinbuch 1998: 24, Krajewski 2004), projects are currently framed in a way that promises predictability and masks uncertainties. Marc Torka has in this context spoken of the paradox condition of the project mode. He argues that researchers are left to deal with both predictability as a crucial requirement of project funding and unpredictability within their everyday research. While the project mode aims at creating temporally, epistemically and socially contained units, research as a practice is characterised precisely by its uncontained character and its likelihood of failing initial expectations. At the same time Torka states that innovative research practice is expected to take unforeseeable turns; the more uncertain a research trajectory, the more innovative it is considered to be (Torka 2006: 72f). Researchers thus undertake predictive exercises and announce the results in advance, while at the same time knowing that "the stabilization of arguments usually takes place at the end of a research process – otherwise research would not be necessary"; or as Torka continues: "You have to take directive decisions in advance without enough… substantiation" (Torka 2006: 72f, German original). The project mode thus seems to discipline researchers' thinking in two ways: in that they are asked to make detailed plans about how to use their time and resources and in that they learn to do so according to a defined rationale for what counts as valuable. Overall the project mode guides them in arranging time and resources according to research trajectories that are, in this sense, productive.

In the sense that it is about increasing the efficiency and productivity of research activities, the project mode can be interpreted as a mode of rationalising research practices – even if the means of rationalisation are quite fundamentally different from what we historically know from Taylorist rationalisation processes. While the latter is based on detailed instructions, division of work, clear determination of working time and location, and transparent subordination of workers to the company's goals, these post-Taylorist forms of rationalisation are based on workers' self-determined ability to constantly adapt the uncertain production processes to

the needs of a quantifying assessment structure. In conditions of highly uncertain work processes – such as research work – the project mode allows for creative solutions and flexibility for continuous reconsideration of previous assumptions while still upholding the standards of predictability, efficiency and productivity.

In the theoretical debate however, using the notion of commodification in this context is not undisputed. In fact it has been debated whether or not knowledge (production) can be treated like a commodity at all. The most obvious argument against treating knowledge as a sellable commodity is that it cannot be consumed in the sense that its use value diminishes when used by others. Rather, knowledge multiplies when it is shared and used by others. A person appropriates and (re)produces it as soon as s/he learns it. In this sense, it appears that knowledge can never fully be abstracted from – or expropriated from – the person who has learned it.

According to Polanyi's concept of commodification however it can be argued that such interpretations miss one core characteristic of the commodification processes – namely that they are necessarily incomplete and fractious. As the examples that I have given above have implied, and as will be furthered explored in subsequent chapters, new measures of valuing and organising research are often experienced as being in tension with everyday research processes – and in particular with its inherent uncertainties. It thus seems that commodification is taking place, but at the cost of creating experiences of uncertainty, ambiguity and tension in the social worlds. Thus, while there are might indeed be several characteristics of academic research activities that resist commodification it is nevertheless possible to invent commodifying rationales and to build commodifying infrastructures around it that make it possible to treat knowledge as if it were a commodity (Radin 1996, cit. Jacob 2009: 399).

9.4. The Co-Production of Commodification

On a conceptual level therefore this suggests that knowledge and knowledge work – just like land, labour and capital – are neither factors of capitalist production nor commodities in themselves but are increasingly treated like commodities within and through their social and institutional environments. This process of commodification can however not be understood as an invasion of capitalist market rationales. Rather, it makes use of long-standing academic infrastructures such as publication systems, self-determined work, personal motivation, individual autonomy and academic career logics. In doing so it seems that commodification does not replace old forms of organising academic activity but subtly transforms and reorganises them along commodifying rationales. The particular kind of commodification process that we witness in academic work cultures thus might best

be thought of as co-produced by capitalist and academic codes and norms. Applying the concept of commodification as it was developed for the socio-political developments of the 19th century to what happens to academic knowledge production in the 21st century therefore requires careful consideration of the historically situated social and institutional environment as well as of the way that researchers experience their work. Some authors have suggested that commodification within subjectified work cultures does not only change rationales, practices and infrastructures but that it has implications for how researchers are subjectified. Joyce E. Canaan argues that it "reworks the identities of those working in academia" (2008: 273) and Wesley Shumar proposes: "university workers and students see themselves and their activities differently through… a 'commoditized apprehension of reality'" (Shumar 1997: 15). It might thus be that the commodification of academic activity encloses embodied capacities and the subjectivities of knowledge workers – or, as Maurizio Lazzarato has described it for immaterial, subjectified work more generally: „What modern management techniques are looking for is for 'the worker's soul to become part of the factory'" (Lazzarato 1998: 2, German original). This ties in with what we have discussed earlier in the chapter on subjectified (immaterial) work; namely that it is best governed when subjects appropriate and embody – i.e. subjectifiy – the governing rationales. In the context of academic activity thus, commodification might have to be thought of as taking place on three levels: the rationales for assessing the products (abstract and quantified performance metrics), the rationalisation of how research is carried out (the project mode) and the reworking of subjectivities (the embodiment of commodifying rationales).

The following chapters – in which I will explore experiences of uncertainty, ambiguity and tension in life science research cultures – will show how a commodity perspective creates contradictions and tensions within everyday academic activities. By tacitly changing the fabric of life science research cultures and by contradicting values researchers themselves ascribe to their activity they seem to contribute to a pervasive experience of uncertainties, ambiguities and tensions.

Part 3: Thinking Things Together

In this third part I will discuss the aspects that life science researchers discursively linked to notions of uncertainty in a broader sense of including ambiguities and tensions in-depth. In doing so, I will work out the conditions in their everyday epistemic living spaces that seemed to result in uncertainty-experiences. During the discussion I will trace back these everyday conditions to a convergence of the five broader preconditions that I have discussed in the previous part: epistemic uncertainties, subjectified activity, academic career scripts, casualisation, and commodification. As will become clear, all five carry different expectations about how life science research can and should be done as well as assumptions about the temporal rationales along which it can and should be organised. Based on the analysis of researchers' narrations I will suggest that, as structural preconditions, they do not just trickle down into or are directly mirrored in everyday work cultures but are woven together to create a new set of heterarchical conditions in the everyday. I will argue that as a result of different expectations and rationales, researchers experience and have to deal with contradictions and dissonances in the everyday.

In chapter nine I will show how these conditions create an environment that can nurture an overall sense of uncertainty. I will argue that it is in particular experiences of high competition, permanent lack of time and of their futures as always latent contribute to making this a characteristic of contemporary life science research cultures. In chapter ten I follow up on this and suggest that we can better understand the particular experience of uncertainty in picturing uncertainty-experiences within a cycle of experiences of anticipation, guilt and restlessness. This perspective will make the highl personal, subjectified or embodied character of uncertainty-experiences better graspable.

10. The Everyday Conditions of Uncertainty-Experiences

In the second part of this thesis I have discussed experiences of uncertainty as being framed by a range of environmental circumstances. Describing such structural preconditions alone however is too abstract for gaining a good grasp on what it means to live and work in academic life science research today. In researchers' narrations it seemed that epistemic uncertainties, subjectified activity, academic career scripts, casualisation of work and commodification of academic practice were not seen as directly responsible for their uncertainty-experiences. Rather they were always described in an entangled way and that in this entanglement they established a space within which uncertainty-experiences were likely to emerge. The aim of this chapter is therefore to explore the everyday experiences of uncertainty, ambiguity and tension in detail that these preconditions seem to engender in young life science researchers' epistemic living spaces.

10.1. Contradictions and Dissonances in Experiences of the Everyday

When life science researchers talked about changes in their immediate institutional environments there was often a sense of confusion about the unclear expectations that were linked to these changes. It seems that in the current state of transformation, there are different systems of coordinating research at work. While older ones have not (yet) been abandoned, infrastructures and conditions are emerging that are undergirded by new norms and values. Researchers thus appeared to see themselves as caught between old and new, still evolving, forms of governance, as this quote illustrates:

> *…as I said, that's a thing of constant change… the last development at…* [my university] *is, that they are trying to introduce general quality criteria… that everyone has to meet. And I have heard that they are doing it at* [the neighbour university] *as well. So… at least in the future it is supposed to be clearer for the individual.* (m4.2: 555-60[xlvii])

This and similar narrations suggest that in contemporary research environments researchers have the impression that different institutional contexts (e.g. academic institutions and funding institutions) are pulling in opposite directions. They saw themselves in a situation of being caught in between and in a growing sense of uncertainty about where research organisation was headed. In researchers' narratives it seems that the pace and diversity of change has reached a point where it is difficult for them to see the big picture, causing many people to experience themselves as not fitting in or in a state of ambiguity. As in the following quote, many conceptualised their own situation as exceptionally difficult:

> *In theory it is not possible at al. I will probably be one of these problematic cases that… are affected by the university law 02 that forbids researchers to be employed for more than six years – I think – by one university… Whereas, it is not quite clear if my employment in* [this project] *was formally a university employment… that's legally not resolved yet. So they might say no. … And so I am somewhere in this grey area.* (f6.2: 249-59)[xlviii]

Such accounts of being in a grey area were quite frequent and expressed little trust in the stability and reliability of institutional regulation nor in the validity and continuity of expectations placed upon them. What was often implicit in these notions was an experience of perplexity over having to conform to quite different definitions of research quality by quite different parties. These parties included their academic institution, funding agencies, the lab leader/supervisor, their own peers or, in a very general sense, "society". It seems that this dilemma was particularly strongly felt by the youngest researchers who had just entered the field of the life sciences. On the one hand because they seemed to feel that they had not yet learned to reconcile these expectations but on the other hand they regarded them as not actually being reconcilable at all. This in turn appeared to create a sense of underachievement or of regularly failing to meet the whole set of expectations. I will illustrate this with a few examples, starting with the particular constellation of tensions prevalent in the PhD and postdoctoral period.

In the life sciences, it is recommended and very common that students write a cumulative PhD thesis instead of writing a monograph. That is, most young researchers aim at publishing several journal articles and then summarise them for their thesis. A cumulative PhD thesis is therefore reviewed and evaluated by many more actors than a traditional PhD monograph – journal editors, peer reviewers and two PhD supervisors/evaluators. In this context it is described as a challenge to do justice to all of their expectations. One PhD student for instance told us that for him paper reviews were insufficient to rely on in the context of his PhD thesis. In his case, turning the journal papers into a thesis meant working one additional year to adapt his papers to the quality criteria of his supervisor:

> *…the dissertation was then finished only a year later… because I thought, I could hand in my PhD tomorrow and could close this chapter. But then… my boss asked from me certain additional experiments and that was a very unpleasant experience, the whole thing.* (m4.2: 22-31[xlix])

During the postdoctoral period researchers tended to describe an increasing number of parties who needed consideration. While basically they were often independent when doing their research, they actually listed a lot of actors that they depended on, such as a lab leader to hire them, journal editors and reviewers to publish, funding institutions to secure grants and criteria of academic institutions to continue to employ them. Postdocs' accounts also suggested that even policies that

tried to promote the same thing (e.g. a higher mobility of young researchers) sometimes contradicted one another. Paradoxically, in several cases mobility policies of institutions came into conflict with mobility policies of funding institutions. At one academic institution, for instance, researchers told us about a rule that excluded on-site postdocs from applying for more permanent junior lab leader positions. Funding institutions on the other hand have over the past few years aimed to bring researchers back to Austria with comeback-scholarships – thus making it easier to go abroad and then come back. Those scholarships implicitly promised them a good chance of pursuing an academic career in Austria. Those who had come back on such a scholarship (or had otherwise been abroad) now found themselves in a situation where they were formally not allowed to apply for suitable jobs. In the following quote, one postdoc expresses his perplexity about this situation:

> *…as university assistant… I'm a bit confused with the legal regulations… it should be the case that it's possible for me to work on projects until the end of my contract in 2012… but with the current administration here I'm not sure that it be very easily possible and anyways it wouldn't be interesting. Because I would have to write a grant for half a postdoc-position and I'm sure I wouldn't be allowed to have employees like I have now. That's certainly impossible… But no matter how, we need to leave anyways and somehow that's even better because now we at least know…*

I: …that is, there is no possibility for any of you to get a permanent position?

> *No.*

I: Because you are not external?…

> *Well, that is even much clearer now than it was two years ago… some have applied but as far as I know no one has been shortlisted…*

I: Okay, that means that indeed a lot of people will leave…

> *Just leave… there are several people in my position… administration has never cared about us… probably because we have temporary contracts and will be gone… And yes, definitely, in every meeting in which you are, they tell you that you should leave.* (m1.2: 72-134[1])

This quote illustrates a common problem created by conflicting rationales that are at play in an academic career path: the preference of funding institutions for funding postdocs half-time, the way that his university handles legal regulations (such as chain-contracts), and his academic institution's administration that he assumes will not allow him to establish a research group of his own. Another example of having to meet different rationales is how their research is evaluated in the context of project funding. While on the one hand, getting funds was described as crucially dependent on publications (and impact factors), researchers assumed that they needed to make an argument for the immediate societal relevance of their

research as well. For the post-project evaluation and for how their academic institutions evaluated their performance, the societal relevance seemed negligible.[23]

Overall, young researchers' narrations suggested that it was a challenge for them to stay on top of what, when and how they needed to perform. Despite very clearly defined performance criteria (such as impact factor), many described expectations as quite variable and opaque rather than as clear frameworks for making decisions:

> *Yes, I mean, the requirements are… very variable. That is, the requirements are clear, but how they are interpreted, that is variable. So, I have already seen this with several colleagues, that these target agreements for example more or less,* [are in] *a transitional period, where these target agreements are introduced; that means, they are then designed more or less according to the profile (laughing) of the person that is applying for the job.* (m4.2: 1248-52[li])[24]

According to such accounts of co-existing and partly contradicting frameworks of expectations, researchers often seemed to experience existing performance criteria as failing to promote what they had been intended for – that is, to be a reference point for coherent and transparent decision-making procedures for doing research and for constructing their careers:

> *And at…* [our institution] *there is a chief of the* [research] *centre, and he decides what positions are assigned. And there is not really… a scheme that one can rely on. And that's a major uncertainty. Because most people think: Yes, I have had a good scientific career… but there are no clear decision-making processes or clear career paths (.) at university or in research.* (m2: 837-43[lii])

As this quote suggests, well-defined criteria for evaluation were partly experienced as resulting in an opposite dynamic. Within different frameworks of expectation, these criteria did not appear to provide transparent and objective parameters for performance. Quite to the contrary, despite well-defined criteria, actual decision-making tended to fall back on single individuals and the priorities they set.

A further source of tension in their narratives was that researchers tended to differentiate between the value that was given to research findings within an academic culture and the quality they saw themselves in their own work. While they would for instance find it subjectively (and with regard to a progress in knowledge production) most interesting to follow highly uncertain research questions and not

[23] Using the concept of „credibility cycles", Laurens Hessels and Harro van Lente have described a similar phenomenon of evaluation cycles within Dutch Academic Chemistry. They argue that while in research fields that were more proximate to application, criteria of social relevance contributed more to researchers' identity-building than in basic research fields that were rather detached from any application of their research findings (Hessels/van Lente 2011).

[24] Regarding the multiplicity of quality criteria it is interesting to note that the criterion "excellence" was not at all referred to by researchers themselves despite its increasing prevalence in the public and policy discourse.

focus too much on short-term productivity, they experienced their environment as increasingly following a output-oriented productivity rationale:

> *We are really doing real basic research. And that's not appreciated any more. Everything must… be productive in this sense. And I think that can have negative consequences for research… Because maybe the quality is compromised when… people have the feeling they must publish, publish quickly; I think that then a lot of bad things happen.* (f1.1: 151-61[liii])

Performance-oriented expectations organised according to generalised and quantified metrics thus generated anxiety because they were in conflict with their own ideas about what constituted research quality:

> *The currency with which we are paid is completely weird… it's not about whether it was scientifically nice, whether it was something important in your field. It only counts how many publications you have, right? How many… points you have… We do so many other things too and… basically that's not considered at all. And honestly speaking it's not creating… personal satisfaction to have a paper. … So, I can't really find my way around that…* (f4.1: 909-22[liv])

As is illustrated by this quote, researchers tended to describe their personal perspective on research quality as partly incongruous with ways of valueing that they found in their academic environment. Such accounts suggest that quantitative metrics narrow the idea of what good research is. The above quoted researcher for instance felt that such measures failed in recognising activities and knowledge that were vital for research communities but not easily represented by numbers. Many also more fundamentally claimed that it was an extremely difficult, if not altogether impossible, task to define research quality and performance objectively and fairly – mainly because from their perspective, investment and output were experienced as having no clear correlation with uncertainty-intensive life science research:

> *The laziest PhD student with the best results will nevertheless be the best PhD student. Someone who does 5.000 analyses but unfortunately analyses the wrong sample will not achieve anything… that's certainly… more extreme in the biosciences, this factor of luck …* (f9: 455-8[lv])

Since "*nature sketches out the way*" as one PhD student contended, it was impossible to predict or fully plan the research process and it could happen that one has to "*redirect"* and publish it *"with a lower impact* [factor] *than… initially planned"* (m4.1: 1809-14)[lvi]. While researchers would usually also insist that despite procedures to make research more predictable an underlying epistemic uncertainty would be unavoidable because it is part of the nature of life science inquiry. Unforeseeable problems or opportunities that happen during the research process would make it likely that their research could take a different direction than expected. In that sense, researchers felt that they were periodically asked to create expectations, which they knew they would most likely not be able to meet.

In all those ways, researchers experienced tensions and incongruities in the expectations surrounding their activities and emphasised that their work consisted of activities that were beyond valuation by the number of publications or the impact factor.

What I have so far left out of the picture is how different assumptions about the temporalities of life science research are described as often conflicting. Researchers conceived of their temporal environment not as of one piece but rather as a patchwork of temporal rationales. When talking about their academic lives they referred to different temporal logics: the temporal logics of qualification steps, funding times, contracted times, institutional timelines, the time it takes to get findings published, experimentation times, temporalities of collaborations or private activities (such as child-care, family, friends). It seemed that the de facto abolishment of permanent contracts, project funding and new evaluation and employment rationales of academic institutions had intensified the challenge of dealing with such multiple temporalities. Besides the challenge of constantly being concerned about what contract will come next, researchers needed to cope with moments in which temporalities came into conflict.

A very frequently narrated example of conflicting temporal rationales was an asynchrony between employment periods (contracted times) and epistemic temporalities (temporal logics of experiments). Especially in the life sciences where researchers handled living (and thus often unpredictable) material, experiments tended to be open-ended as opposed to limited to the contract length. A particular sense of unease was generated when an experiment was not finished or had resulted in less conclusive output than expected by the time their contract ended. In such situations, researchers often feared that they might have to leave their labs before their experiments had finished, before the results were published and before they were ready to apply for their next job. One example of conflicting temporalities was when it took researchers longer to write a PhD thesis than their contracted time. While there were usually efforts to make these requirements match by choosing reasonably small research questions, it seemed that often they did not. When reflecting on whether or not she will be able to finish her thesis before the end of her contract, a PhD student said that „*considering the uncertainties*" this was difficult to say because "*you have to deal with problems that you cannot… ad hoc… solve according to the methods that you have studied*". And she continued by saying that it was "*A: uncertain, and B: you never know if something will come out of it*" (f4.2: 134-43)[lvii]. This and similar narratives suggest that PhD theses or publications are often only finished after the end of a contract, within the following contract or during jobless periods. In general it seemed that standard employment periods of three years were considered relatively constrictive for research processes in the life sciences, given that they need to take into account the longer life cycles of model

organisms and anticipate unforeseen complications. A PhD student for example who was working on mouse models and was thus bound to the timeframes of their life cycle doubted that time periods of only a few years were suitable for promoting good research:

> *I don't think that it will be possible in the long run to only have these few year, short-term contracts… really in molecular biology, three years are nothing. Really that's a puff of wind* [german original: "Lufthauch"]. *When you look at our experiments… that's extremely short.* (f7: 728-32[lviii])

A further case of conflicting temporalities happened when the employment rationales of academic institutions interfered with the runtime of projects. One postdoc for example explained that despite existing project funding, the university refused to renew his contract beyond the employment limit the university had set as an institutional rule:

> *…I didn't know then, that the six-year-time-limit applied to me as well… I only became aware of it after I got my new contract… which was only until the end of the year. Then… I asked at the personnel office… why do you not prolong it until the end of the project? And then they told me, that at…* [my university]*… I can only be employed until the end of the year.* (m4.2: 192-7[lix])

In quotes like this, the temporalities of both institutional employment policies and project duration were described as interfering with the continuity of collaboration and supervision relationships. Due to a temporary contract culture and the trend of moving from one lab to the next it seemed to be quite often the case that employment periods for fellow researchers were out of sync with theirs. This regularly disrupted the collaborative relationships that both younger and more experienced researchers relied on for supervision and exchange. In this context, particularly younger researchers often described difficulties in meeting the requirement of continuously building up their own research profile. When contracts of fellow researchers ended or they decided to go abroad, a postdoc explained, research processes can be interrupted, slowed down or in the worst case ended; in her own case a collaboration she had had with another postdoc ended when her collaborator went abroad in order to take another job:

> *For me it was for example not good that the postdoc quit at that time… she was here for one and a half years. (.) And that was the end of a project that was not finished yet, and that's not so… convenient. We try indeed to continue that, she is now in* [another city], *but… everything goes at a slower pace, now that another lab group is involved… we try to carry on with the old project but at the moment it's not working that well.* (f1.2: 476-94)[lx]

Researchers' narratives suggested that these interruptions created uncertainty with respect to their own future job perspectives but also with respect to how long they would be able to continue smooth collaborative relations and to whether and how

they themselves could hold on to their responsibility to other researchers. Another postdoc for instance who was facing the end of her contract was concerned that she might not be able to properly supervise her PhDs before they finish: "*It's also that I am establishing a group and I have PhD students for whom I am responsible and by no means I want to abandon them*" (f6.2: 74-7).[lxi] Accordingly it can be a quite serious setback to a lab when a lab leader needs to leave or decides to go abroad. Since lab groups – the institutional space and technical equipment for a certain research – mostly rest on the group leaders, young researchers worried that they might be left without the technical and social infrastructure or supervision to continue their work.

As is implicit in the quotes above, it seemed that the more flexible and unstable a research environment becomes the more work needs to be put into creating and maintaining coherency in academic research life. Since experimentation techniques were increasingly complex to learn, researchers were concerned that it might become increasingly disruptive when lab personnel were exchanged too often:

> *I mean… biochemistry alone is very complex and you can do a lot of things and you can focus on that… you only have 40, 50 hours to work. And then there is* [this technology] *that I have perceived as something that adds a lot of levels; namely on how to design an experiment, on how you can write new experiments, how you interpret it, what you make with it, how you handle the… programme… there are trillions of things. That's why I think, that it is impossible to be a specialist in both. I think that's almost impossible.* (FGk_pd: 501-13[lxii])

What is implicitly described here is a complex network of expertise that researchers experienced as allowing for the smooth investigation of research questions. Particularly in the bigger labs, researchers found the functioning of their networks of expertise compromised by a high turnover of young researchers. In the labs that we observed it was only the lab leaders and technical assistents who held permanent jobs. Since the lab leaders however were active in hands-on lab work any more (due to the need to write applications, teach and do managerial work) the balance between experienced staff and new staff in the actual lab seemed to easily get out of balance. Vor covering the gaps in local expertise that were created when other staff left, it seemed to be a continuous effort for all lab members to acquaint newly incoming staff with the local workings specifities of the lab. Thereby, the creation of continuity and coherency in research processes of labs ironically was largely left to non-permanent staff.

In summary it seems that the temporal fragmentation of life science research was experienced as increasing, which was resulting in a structural discontinuity of researchers' income, research processes, collaborative activities and finally, the social security of these working contexts. As a postdoc put it while talking about a dilemma in coordinating various collaboration and supervision obligations that all

ran on different timelines: "*there is not the big chaos every three years but the small chaos almost every year or so*" (f6.2: 89-94[lxiii]). In some respects such high flexibility in work cultures was seen as good for facilitating research quality and international cooperation and exchange. However, researchers also adressed possible downsides for everyday research practices – particularly in highly collaborative research fields like the life sciences where it seemed be a real effort for individuals to reconcile different expectations about how life science research should be carried out and different assumptions about its temporal rationales into a continuous and coherent career, employment and epistemic life. This seemed to be creating not only additional managerial tasks but also anxiety about whether and how they would succeed in doing so.

10.2. Experiences of Competition, Lack of Time and Latent Futures

The space that was established by this framework of contradictions and dissonances explored above appeared to privilege experiences of competition, lack of time and a particular way of thinking about their futures – and overall a pervasive experience of uncertainty.

Implicit notions of competition were omnipresent in researchers' narratives about how one must live and work in the academic life sciences. It appeared to be a shared assumption that requirements were steadily increasing:

> *I think that the requirements are increasing. That's probably the consequence of the fact that the available positions are decreasing.* (m1.1: 534-6)[lxiv]

As in this quote, researchers believed an increasingly competitive research environment with *"simply too few positions"* (m2: 197[lxv]) was causing an increase in performance standards. In the current situation, one researcher said, a career might not even be possible when all requirements are met or one's performance was in fact outstanding:

> *…if you see it in terms of career, this is the minimum requirement for taking the next step and to take up a permanent position. But with regard to maximum requirements everything is open. Projects, acquiring funds and leading research groups, publish – two times a month. (laughs)* (m4.2: 525-9)[lxvi]

Meeting minimum requirements was certainly not regarded to be enough for making a career. On the basis of observing careers of postdocs, young researchers seemed to become sceptical about whether it was wise to try and fulfil the formally expected career requirements. In the words of a postdocs the saw others

> *…come back now and find themselves in the situation that they have done all that – be it with advantages or disadvantages – and are not compensated for it now. (.) And that's why people are more sceptical now, if that's a way to go about it.* (m2: 172-6)[lxvii]

These quotes hint at an interesting characteristic of performance criteria in these competitive contexts: Instead of being experienced as a precondition for *succeeding* in career competition, these requirements were narrated as a precondition for being able to *enter* the competition. As in the quote above, the performance level of potential competitors was perceived as in principle open-ended. This is telling about the form of competition within which researchers pictured themselves. Interestingly, they would hardly ever say that they were directly affected by competition. When they spoke about direct competition they often narrated it as being delayed in time or happening in a different place:

> *Within our lab there is no competition (laughing). It is certainly not the case that the group leader puts two, three postdocs to working on one project and the one who finishes it first gets the publication out. But there are these kinds of labs.* (m2: 485-8)[lxviii]

Indeed they spoke of rumours about lab leaders who would put their lab members in direct competition, but these stories seemed to mostly serve as proxy-stories for an overall competitive situation within which they often had little knowledge about who they were actually competing with. Their stories evoked a general sense of competition that was not articulated by a physically present competitor but rather by a set of requirements that seemed rather abstract and always open-ended. They will only meet their competitors when they are anonymously „compared in lists", as an interviewee put it:

> *In effect it is counted, who has how many publications. And if you… have spent three years on paternity leave, you have accordingly fewer publications and (.) the one* [with a higher number] *will win.* (FGk_pd: 1347-50)[lxix]

They seemed to conceive of themselves not as competing within the same lab or with colleagues they personally knew but with an anonymous mass of other young life science researchers who also want to pursue a career. Other life scientists in the global scientific community were experienced as a sort of invisible followers that were always trying to catch up with and eventually outrun them.

The sense of competition in academic life sciences thus rather is a generalised competitive mindset that is fed by observing colleagues who are struggling to construct their careers and an overall experience of a growing set of expectations. What seemed to create a sense of anxiety in researchers was that this competitive mindset appeared to be actualised in increasingly shorter intervals. The end of every contract, every paper and every grant review process is a moment for evaluation. For instance – as was mentioned above – the evaluation of a PhD thesis required in-between evaluation in peer review processes of publications. Usually one to three publications – at least one of which to be first-authored – were regarded as necessary to proceed. I.e. within a three-year period of time, there are three

evaluation procedures to undergo and possibly also a kind of evaluation for the renewal of their contracts. Similarly, the grants they apply for and the articles they publish can be seen as points of evaluation, as a postdoc hints at:

> *…I really need my independent papers. That's clear. And also independent funding, so… I can qualify for* [the next career step]. (f3: 817-20[lxx])

While competition in academia has of course always been present in academic living spaces, it seems that with recent transformations (and particularly in tandem with the promotion of international careers and increasing external funding) it has taken on a more generalised form. Potential – often unknown – competitors have increased in number and thereby, the requirements for pursuing an academic career have expanded and tend to be perceived as open-ended (i.e. one can never do enough). Although there are mutually agreed on norms that govern the kinds of outputs necessary for each stage, these are only regarded as minimum requirement for entering the stage of international competition – as opposed to being a quite certain precondition for an academic career. In this maze researchers never know whether they are fast enough and have accomplished enough.

Closely linked to such accounts of generalised competition are accounts of never having enough time – both in a general sense of never having done enough in a highly competitive environment and with regard to concerns over the amount of time that they needed to spend for administration, project management, acquisition of funds or supervision duties. In this context, a postdoc pointed out that in the academic world "*(y)ou have to realise soon enough that a naïve science is not possible in the sense that it is enough to make good research*" (f3.2: 346-8).[lxxi] Besides the actual experimentation work, this researcher referred to time that she needed for the acquisition and management of projects, the establishment and maintenance of professional networks and for leading a mobile life. Particularly the grant application process was considered an increasingly time-consuming task. For lab leaders it was often said that the acquisition of funding can use up to 75% of their time (LCQprof_f1, LCQprof_m3) and a postdoc estimated that "*nowadays the writing of projects (proposals)… consumes 10-14%" of his time"* (m1.1: 584-5)[lxxii]. He also emphasised that on top of the writing a project proposal it is necessary to do some *"preparatory work"* – that is to have some preliminary findings to present in a proposal (f1.1: 602-4)[lxxiii]. Another task they described as time-consuming beyond core academic activities was the communication work and professional networking required to become part of peer groups and career networks. Maintaining good contacts with established researchers was considered crucial – for example with regard to support and advice in strategic career planning. Without the ability and willingness to network and "sell" oneself – as a postdoc put it – they considered it unlikely that they would survive in academia:

> *It's very much networking and there is this need to sell yourself – such things, and that is not my style… In my little niche here that's ok, but this niche will not exist forever.* (f1.2: 109-13)[lxxiv]

Similarly, the requirement of going abroad was discussed as being very time consuming, not only in terms of the actual moving time but in that the private life needed to be organised around it and that they needed time to familiarise themselves with the workings of another lab. As in the following quote, time in academia was always described as limited and even more limited during times abroad:

> *You can have one hobby, I would say. It allows for that, but there is a lot of weekend-work included, especially when you are abroad and you use your free time to keep contact to your home.* (m2: 853-6)[lxxv]

Since it consumed a lot of time, an academic career was regarded as requiring an appropriate configuration of private life. As a project manager told us in an interview it seemed that a career in the life sciences required a clear focus on academic activity. One interview said in this context that researchers *"do whatever they can"* (f9: 161-2[lxxvi]) – a phrase that is usually reserved for emergency situations in which everything possible must be done as quickly as possible.

It therefore appeared that their experience of not-having-enough-time had a significance beyond a mere lack of time. Most young researchers – from the PhD to later postdoctoral phases – tended to conceptualise their position on the academic career hierarchy as exceptional. They often narrated the phase that they were in as an extraordinarily demanding phase in which they had to and were willing to spend time – mostly in anticipation of a less demanding phase. In their narratives there was no time to lose in order to move forward, get findings published, get grants funded and secure the next contract. The above quoted PhD student for example continued by saying:

> *It was my personal experience that… in this time you really focus on your project. Because there simply have to be results. At this point in time you lay the cornerstones more or less.* (m2: 856-9[lxxvii])

However, postdocs as well described their position as exceptionally demanding. In the quest to establish themselves as independent researchers, acquire their own funding and find out whether a more permanent position or an academic career will be possible to achieve for them, they usually regarded themselves as being in a decisive phase of their lives:

> *And the* [postdoc] *has to see and try to get a position somewhere, right? He is working like mad, cooperates with everyone, he has to look after himself in order to move on…* (f4.2: 924-9[lxxviii])

According to this line of thought, postdocs tended to think that they could not afford to take a break. For „*keeping up and maintaining a scientific career*" an interviewee for example said that gaps of half a year or a year between contracts „*are a sheer lunacy*" (f6.2: 362-7)[lxxix]. The choice of words in such accounts was often striking. As phrases like "working like mad" or "a sheer lunacy" suggest, researchers experience themselves as being in a difficult starting position from which there are few possibilities for action beyond desperately trying to keep up with the set of academic requirements. Doing that at a pace that allowed for participating in an internationally competitive academic job system seemed to create high levels of time pressure and stress. In that context it thus seemed that researchers regarded the time that they had available to invest in their academic life as a crucial factor for a successful academic career – that is the willingness to set one's priorities on scientific activities and to mobilise as much time as possible for being able to compete at a high level.

The overall effect of these intense experiences of competition and a permanent lack of time seemed to be an anxiety about their options for future professional development. Not knowing if and where they would be able to secure their next position was often regarded as acceptable for the first few years of professional life but it seemed that when researchers took on private care relations or aimed at having a family they started seeking planning stability. Their accounts often suggested that in academia, a permanent need to plan research and careers created a situation where real planning was an almost impossible task, as one postdoc remarks:

> *In our situation it is very difficult to plan something. If I could I would really like the feeling of having more than three or five years to work on certain projects… then we could be concerned more about planning the scientific stuff… but the way it is right now, we're mostly planning our own futures.* (m1.2: 289-94[lxxx])

Another postdoc – who was struggling to get his contract prolonged – saw the time and effort that he put into planning his career future as beginning to compromise the time and effort that he could dedicate to his actual research:

> *…everything is in limbo right now. We* [my lab leader and I] *have a master plan…* [but] *the question is whether we will be able to realise it…* [the situation is that I] *… spent at least one third of my time to fight for survival, writing permanent (sic) applications and… applying for scholarships and so on – which is of course part of the job – but all these struggles with…* [the university]*, I should rather spend the time in the lab or read papers, but right now that's not my reality.* (m4.2: 166-70[lxxxi])

Making plans to pursue an academic career was thus always accompanied by the concern that their academic lives might end when their contract ended and even if

he was permanently planning his possible futures, this planning might easily becomes obsolete:

> *…basically I think it's a good idea to think in a long-term way… however often situations emerge that can change the fabric of the university, and then a completely different situation emerges that you didn't expect. And… the long-term-thinking might become obsolete*. (m4.2: 479-83[lxxxii])

In many cases it seemed not only that the permanent planning of futures tended to rule out the development of longer term future prospects for doing research but also was in serious competition with present trajectories. Young life science researchers thus seemed to be in the ambivalent situation of having and needing to plan but at the same time knowing that real "planning" was impossible and that they needed to be prepared to drop their plans for a new future at any time:

> *…So the plan is – however unplannable it is – really is to further strengthen and build my group here and, that I want to gain ground here. If that doesn't work out, than I have to go anyways!* (f6.2: 259-62[lxxxiii])

Every future that they envisioned seemed to always stay latent. In their latency however they were very present in the sense the spaces and opportunities to realise these futures were often changing and new futures had to be sketched on a regular basis.

Recent studies in the field of science and technology studies have observed before that the scientific world is increasingly being organised according to different rationales. Kleinman/Vallas for instance have spoken about a „confluence of organizational logics" leading to a heterarchy of different regimes (cf. Kleinman/Vallas 2007: 1). From a historical perspective, Hessels/van Lente have argued that the current transformation of academic practices does not leave traditional practices and values behind but rather takes on new ones, resulting in a co-existence of different modes of research (2008). Similarly, Edward Hackett speaks of diverse sciences and emphasises that their co-existence results in contradictions and tensions in the everyday:

> The various tensions experienced by research groups may be symptomatic of a fitful transition from one mode of research to another… or may indicate that the two modes exist together – and perhaps always existed together – as simultaneous potentials… Science may be in oscillation or ambivalence rather than in revolution or transition… Diverse sciences co-exist as simultaneous potentials. (Hackett 2005: 820f)

Building on such basic observations, authors have started to empirically study the sources and implications of such contradictions and tensions. Interestingly it is particularly the different frameworks of expectations and conflicting temporal

rationales in epistemic living spaces that are starting to be discussed more intensely. Georg Krücken for example has observed that a "multiplicity of partly conflicting evaluation criteria" renders a "homogenous and systematic overall view of research performance impossible" (Krücken 2006: 14, German original, cf. Maasen/Weingart 2006) and Garforth/Červinková have concluded that there is a "plurality of time regimes at work" that seems to require handling and integration on the part of the individual in order to create coherent biographies (2009: 169). In this context, Felt/Fochler have argued that researchers are positioned in the middle of different co-existing and partly uncoordinated logics of governance and that it seems that the contemporary state of change in academic work cultures creates a situation in which regulations that concern young researchers are poorly balanced and poorly coordinated (Felt/Fochler 2010). What I have tried to show in this chapter is that in academic life science research cultures these heterarchical conditions seem to have established and stabilised a space within which experiences of uncertainty, ambiguity and tension are likely to emerge. Particularly for young researchers this seems to make an experience of anxiety a pervasive feature of epistemic living spaces.

11. A Cycle of Experiences of Anticipation, Guilt and Restlessness

The conditions described in the previous chapter seem to place a set of personal challenges on young life science researchers that appear to affect the overall way that uncertainties are experienced in life science research contexts, namely in a very subjective and – as I will argue in the last concluding chapter – embodied way. In that researchers conceive of their research as a personal activity (as discussed in the chapter on subjectified work), the conditions under which they carry it out appear to interfere with how young researchers learn to conceive of themselves personally. In this chapter I will reflect on how the everyday conditions of uncertainty-experiences that life scientists describe might be guiding processes of subjectification in contemporary life science research cultures and suggest that the particular way in which uncertainties are experienced here tends to be strongly tied to experiences of anticipation, guilt and restlessness. In doing so I will approach an understanding of the broader meanings of uncertainty-experiences in academic work cultures that go beyond the fact that they are often unpleasant for researchers themselves.

Researchers did not experience their job in the academic life sciences as ending at the threshold of the laboratory or at the surface of their skin. The drive to read, think, learn and experiment was rather perceived as happening within and beyond the workplace and within and beyond their bodies. Asked what it takes to be a good life scientist, a postdoc described her experience of the upsides and downsides of research activity as very strongly linked to her personality and emotional life:

> *...personal traits, what do you need for science?... Yes, first of all, tolerance to frustration... there is nothing worse than working scientifically, regarding tolerance to frustration... I cannot imagine any other profession, that entails such ups and downs and so many personal crises... because it builds solely on your interest, on your... motivation for research.* (f4.1: 963-9[lxxxiv])

This implies that being a successful life scientist requires a particular set of personal capacities. What she characterises here as the capacity to tolerate frustration was a reoccurring motif in researchers' narrations that can be explored along the axes of anticipation, guilt and restlessness.

As illustrated in the above quote, research was perceived as an occupation that is sustained by a personal motivation for developing and nurturing ideas, for testing them and having them confirmed or disproven. However uncertain the research process and eventual output may be, it strongly builds on the capacity to imagine and anticipate fruitful paths or possible results. In this context, the notion of tolerance to frustration circumscribes a personal capacity for maintaining motivation and for continuously being able to anticipate new research trajectories

even when things do not work out smoothly or go in a different direction than intended. The latter would frequently happen as researchers across-the-board have argued. How long it will take to conduct an experiment cannot be foreseen, nor is there a guarantee that there will be useable results at all, even if you are very good at what you are doing, as a postdoc explains in this quote:

> *It's not a secure job in the sense of: okay, if I work 40 hours a week and certainly something will come out. (.) I don't know, like a craftsman or something, who knows: okay, I can tile something and something will come out of it. As a scientist you say: I… master my methods and the concept and at the end it can nevertheless be (.) that only some ambivalent findings or nothing concrete or nothing comes out…* (m2: 645-51[lxxxv])

A core requirement for being an academic life scientist thus seems to be the capacity to deal with experiences of (disappointed) anticipation. In such situations, they would say, you need to find the courage and creativity to rethink, move on and start a new research trajectory. Interestingly, this strong focus on anticipatory reasoning is palpable in the way they talked about their careers as well. They appeared to approach their professional futures similarly to how they approached a new research project: by sounding out the different trajectories and through a constant re-organisation of perspectives – i.e. in constantly reworking their visions of different futures. Even within a later postdoctoral phase, a 38-year old postdoc experienced being and staying in academia as being in a state of limbo:

> *…this is, what I would like to do.* [I am] *(c)ompletely aware that it will probably not be easy… and will probably not work out. … in principle I still have the feeling this would be what I want to do.* (m1.1: 36-51[lxxxvi])

What is interesting in this quote is that he uses the subjunctive tense, despite having done research for more than 15 years. What he had, in fact, already been doing was discursively relocated to some future and remained a possibility in his lived experience rather than an actuality. Researchers thus seemed to experience themselves as permanently being close to the finish line but never arriving. As a Masters student aims to get a PhD position, the PhD student is concerned about where s/he will be able to do his postdoctoral positions, the postdoc worries that s/he will not finally be able to stay in academia. Hoping that they will be able to stay in academia, they tend to live for the future. In this context it sometimes seemed as if they had somehow traded the (experience of the) present for a hoped-for future that was yet to come. Borrowing a metaphor that Böschen/Weis have used, it could be said that future projections (in our case an academic career) serve them as a secular form of salvation (2007: 159ff). Emília Rodrigues Araújo has described this phenomenon during the PhD time as a "sacrifice of the present in service of the future". She has observed this as a general attitude that young researchers have developed for their private lives as well as a kind of sacrifice that was "mixed with wishful thinking concerning individual and family life after completion of the

doctorate". She then continues by stating that such an "experience of time as a 'phase' implies the actions of anticipation and postponement, in a way that projects everything into a *time ahead*" (Araújo 2005: 197-200). By drafting different versions of the future and enacting these futures in the now, they live in and work for a permanently absent future (cf. Research and Destroy 2009).

Dealing with this situation appears to draw on subjective capacities such as affectivity, creativity and motivation to flexibly adapt to unforeseen changes. What researchers often described was a need for having self-confidence and a belief that it was not necessarily their fault when experiments failed but that uncertainties – quite literally – lie in the nature of things. Frustration tolerance in that context thus means that one needs to get used to coping with uncertain, unpredictable and uncontrollable situations. What is however also implicit in the above narrations is that such processes of anticipating and of dealing with disappointed anticipation are experienced in very personal ways.

What they also suggest is that within contemporary conditions for doing academic life science research the personal and existential character of uncertainty-experiences is even more enhanced. It seems that when the five structural preconditions that were described in part two of this thesis converge, a space is established within which epistemic uncertainties are easily experienced as a personal social and career risk. It creates everyday conditions in which a failing experiment gains a meaning way beyond itself; namely in that it is experienced as immediately putting the academic career path at stake. A lab leader once made this very transparent by saying:

> *One thing is certain: scientific success requires luck. And that factor is not calculable – neither for proposals nor for PhD theses… you have to tell the kids that. When you are lucky, you can do great things with your PhD. And when you're unlucky, you will have a PhD but not much more. Well, that's simply bad luck.* (prof_m2: 930-47[lxxxvii])

By "not much more" in this context he means that without publishing during the PhD phase, an academic career is essentially unthinkable. However, this pragmatic way of coping with the contingencies of research as it is suggested here by an established researcher is seldomly shared by the concerned young researchers themselves. Rather, in their ways of talking about epistemic contingencies it is palpable that what is at stake for them is not only their academic career but the promise they made to themselves to pursue a lifestyle that allows them to utilise their basic curiosity, creativity and personal motivation. In this promise they had dedicated themselves to a plan that they designed for themselves. Thus, they worked not only under conditions of high motivation and evaluated their work before expectations that they themselves have set. When they felt that they did not succeed in meeting these expectations, they tended to blame themselves, even if –

as I have tried to show in the previous chapter – it is to a considerable extent the structural preconditions that suggest to young researchers that they do not perfectly fit in and that they are hardly ever able to meet all expectations. In a sense, this renders them guilty before themselves.

This explains to a certain extent why – even though some situations are described as being frustrating – many young life scientists are at the same time highly motivated to pursue an academic career and to accept the responsibility of personally managing contradictions and dissonances in their working environment. Uneasiness and anxiety were tempered by researchers' ways of working around them; they rarely, if ever, erupted in conflicts in their environmental conditions. This is reminiscent of how Zygmunt Bauman has characterised postmodern societies in general. He writes that

> *…failure is mirrored in guilt and shame. Frustration creates embarrassment, not dissent. It might be that it releases all familiar symptoms of behaviour of the Nietzsche-Schelerist ressentiments, but it is politically disarming and creates apathy.* (Bauman 2005: 412)

With regard to research cultures in academic life sciences, it appears that the dealing with the conditions that create uncertainty-experiences is outsourced to individuals' subjectivities and their capacity for dealing with experiences of disappointed anticipation and guilt. The whole person of the researcher is recruited to soothe the systemic tensions in the everyday.

This need for fully investing the self into research and to dedicate more to the job than the requisite amount of time is omnipresent in researchers' narratives. In order to do the job many would repeat that "*you need to be interested in scientific research because it demands more than a 40h/week job"* (f6.2: 344-6[lxxxviii]). Even people who reported to work more than 60h/week contended that they would prefer having more time available for doing research – expressing a sense of restlessness that renders overspending not as extraordinary or optional but as given and required by the nature of the job. There was however some ambivalence about the promise of an activity where they are able to grow and unfold as a person. As mentioned above it can encourage researchers to sacrifice the present for a future that might never happen and it can tend to strip people of control over their lives, or as put slightly differently by Tsianos/ Papadopoulos: "The expression 'I don't have the time' is the paradigmatic figure for the subjective internalisation of non disposal over one's own labour power" (2006: 5).

This is where the circle of anticipation, guilt and restlessness is complete. The high motivation and enthusiasm for research (and for pursuing an academic career) was often accompanied by a subtle and subcutaneous anxiety over whether their dedication to academia would pay off. This can be seen in an interview segment

with a postdoc who discursively made a direct link between such career anticipations and his self-motivation:

> *But I say, it's these long-term prospects* [that] *are the bait we take, a bit similar to a law firm, where you become a permanent associate sometime. With this you are driven to maximum performance.* (m2: 748-52[lxxxix])

In this way, giving up latent futures appears as a defeat that seems difficult to accept. An estimation and evaluation of whether the research is going well enough and of failure and success in the everyday appears to be self-imposed. The postdoc then continued by describing how the long-term prospect feeds an impulse to monitor himself and his performance:

> *Particularly also when it comes to vacation. That's not a problem in our case. It is rather a pressure, in terms of that you say to yourself: Ok, how much time can I take off? And you do that yourself. Is it running well enough so that I can take off? Or should I better not? But you do that yourself, it's not external… And the same holds for the daily working hours, that certainly amount to much more than eight hours.* (m2: 875-83[xc])

As he follows up on this thought, there is a palpable sense of anxiety about his professional future:

> *On the other hand, the missing security is absolutely enervating. When you don't know, ok, where will I be in one, two years, if a project will come up or not… for that you really need confidence in yourself.* (m2: 1001-4[xci])

What this quote again demonstrates is the experience that he is personally responsible for keeping up with conditions of living and working in such work cultures and for dealing with the uncertainties these conditions entail.

What I want to highlight here is that uncertainty-experiences can take on a potentially high relevance for how young researchers experience living and working in academic environments. It seems that their subjectivities tend to become a buffer for uncertainties, ambituities and tensions. Thereby a sense of anxiety seems to be induced in the way they experience themselves within their environmental contexts that to a certain extent diminishes their control over an activity that they otherwise experience as self-motivated. What the prevalence of these experiences tell us is that uncertainty-experiences not only *frame* how researchers live and work in academic life sciences but can also *steer* the ways in which young researchers are socialised into the academic world.

However, the picture I have drawn by now is still incomplete. I have discussed the ways in which researchers tend to be subjugated under everyday conditions of uncertainties using a focus on (self) control. In the next part of this thesis I will widen the perspective and address the question of how researchers find and manage to create experiences of freedom in their epistemic living spaces. This does more

justice to researchers' accounts of freedom within their work cultures and also of narrations of their ways of coping with external conditions that allow them to enlarge spaces of freedom, to enjoy positive (epistemic) uncertainties and therefore avoid or find relief from generalised experiences of uncertainty.

PART 4: The Tacit Workings of Uncertainty-Experiences

In the fourth part of this thesis, I will explore the ways in which young life science researchers learn to deal with their uncertainty-experiences and how they actively position themselves and act within the environmental conditions that create them. In doing so I will invert the perspective that I have so far taken on everyday experiences in life scientists' epistemic living spaces. While I have concluded the previous part by suggesting that young researchers can be understood as subjugated within everyday conditions of uncertainty-experiences, the perspective that I take in this chapter makes young life science researchers' ways of actively shaping and transforming their epistemic living spaces from below. The central argument will be that researchers are not a passive, individualised pawns that are pushed around ("Manövriermasse") within their academic environment but that they develop ways of living and appropriating resources in their environments and therewith create certain degrees of freedom.

In doing so, my approach differs slightly from previous studies that have analysed how people cope with uncertainty-experiences. Stephan Klecha for example has laid out a typology of five different coping styles: managers of precarity, career-oriented idealists, the fragile middle, scientists by chance and crossover scientists (Klecha/Reimer 2008, Klecha 2008, Hecht et al. 2009; 17f). For him, managers of precarity are oriented around an ideal type academic career but begin to doubt their success without considering an alternative career; career-oriented idealists do not doubt their success and work closely with their superiors in order to profit from adaptation and legwork; the fragile middle accepts the loss of leisure time, the long qualification time and economic loss in order to actively participate in a scientific network. They however consider quitting academia because they fear long-term precarious conditions. Scientists by chance are goal-oriented and only consider realistic options; they do not experience their situation as precarious; and crossover-scientists are active and innovative within the academic system but are interested in practice-related topics; they are mainly oriented around a non-academic professional future; within academia they have clear demands with regard to their funding and employment security (Klecha 2008, German original). Typologies like this often focus on the ways in which people handle career-uncertainties. My analysis takes a broader perspective and regards the contradictions and dissonances described in part three as contributing to an overall sense of anxiety that young life science researchers need to deal with in their epistemic living spaces. I am therefore interested in the rationales and practices along which they narrate their ways of dealing with and relieving uncertainty-experiences in this wider sense and thereby contribute to co-producing and transforming life science research cultures. From

this perspective, ascribing researchers clear-cut coping identities has its analytical limitations. It tends not to be context-sensitive and disregards the particular and regularly changing positionings of young life science researchers as they proceed in their academic careers. Since the analysis of the interviews and group discussions suggested that researchers engaged in various ways of coping and different rationales of acting at the same time and that they seemed to be very creative in (re-)assembling different ways of coping with their changing positionings, I adopted a more flexible analytical approach. Instead of fleshing out coping types, my discussion will therefore be structured around different rationales – or modes – of coping. This approach follows an analytical suggestion made by John Law who defines "modes of ordering" within academic living realities. His modes are depersonalised – i.e. he does not try to ascribe individual persons to one mode. Rather, he describes rationales that people make use of and combine depending on their situatedness and on their ways of relating to others (Law 1994). One argument that I will bring forward is that these modes are aligned to particular ways in which researchers learn to understand themselves within their environmental conditions and social relations. This allows for discussing researchers' ways of coping as being arranged according to different individual and social forms of subjectification (cf. Subjectivity 2011). This form of abstraction provides a more flexible and multilayered perspective on coping in the sense that more than one and even contradictory rationales may co-exist and be entangled with and complement each other.

Chapter 12 in which I identify the four prevalent modes of coping with uncertainty-experiences will be structured in two parts: one that examines modes of coping that can be thought of as social (coping like a clan and coping like a collective) and one that discusses modes of coping that can be thought of as individual (coping like a manager and coping like a trickster).

In chapter 13 I will then discuss these modes of coping from three perspectives that will each open up a different angle for understanding the meaning that unceratinty-experiences can take on in academic life science research cultures. First, I conceptualise the role that young scientists currently assume in life science research cultures as they deal with uncertainty-experiences and argue that they become interfaces between different rationales of acting and different forms of subjectification. In the second discussion I take the argument further and show how picturing young researchers as interfaces allows for a new perspective on how they contribute to the ongoing transformation and tacit governance of academic research cultures. And thirdly, I explore the ambivalent role that experiences of freedom, resistance and subversion might play in this particular form of governance.

12. Modes of Coping with Conditions of Uncertainty-Experiences

For the analysis of the prevalent ways in which researchers narrated their coping with uncertainty-experiences, the following questions were explored: What people (and what kinds of resources) did young life scientists experience as important for reaching a level of (social) security that was acceptable for them? In what ways (and according to what rationales) were they seeking certainty and protection? The analysis of researches' narrations in interviews and group discussions as well as observations of everyday lab work suggested that they were dealing with uncertainty-experiences in their alignment with others – i.e. with their social environment –, but also by dealing with them individually and by dealing with themselves. This chapter is therefore structured in two parts: coping with the social environment and coping as an individual.

12.1. Coping with the Social Environment

When young life science researchers talked about coping with uncertainties, the most important social reference point was their "lab" – the working unit that they were part of. They described it as a tightly woven net of collaborative relationships with other researchers and lab managers/technicians and the (largely) locally defined place of working together on an everyday basis. Rather than the university or the department, young researchers' narrations suggested that their lab was the epicentre of their epistemic development as well as for securing their further career and job prospects. This also showed in how they talked about entering academic research; namely by narrating how they joined a lab and particularly how they built up a relationship with a lab leader. For their everyday research lives – and for coping with uncertainties, ambiguities and tensions within them – they tended to emphasise the way they worked together and related to other lab members.

In regard to coping with uncertainty-experiences it seems however important to distinguish between two different ways of relating to fellow researchers different trajectories and rationales of coping with uncertainty-experiences are assigned to them. I will sketch these by using the metaphors of the clan – a social structure that I understand here as defined by the leitmotif of paternalistic care and the collective, a form of relating to each other that I understand here as instead defined by a leitmotif of self-organised solidarity.

12.1.1. Coping like a Clan

I understand the clan mode as a form of coping that young researchers described as being aligned with a hierarchical social structure. In this mode they tended to picture themselves as having relatively few resources and as depending on someone with more resources (the lab leader or more experienced fellow researchers). These individuals with more resources were described as being in the position to administer, distribute and allocate resources. It is a mode of coping organised according to the leitmotif of paternalistic care that requires maintaining good social relations. Protection is – primarily – granted by a system of guardianship.

When young researchers were asked about lab organisation and about how they dealt with uncertainties, ambiguities and tensions, they often identified the lab leader[25] as being in a decisive position. As the following quote exemplifies, the lab leader was sometimes perceived as the last resort in situations of high uncertainty:

> *There is no protection at all. You can only hope that – should the experiments not work out or not as well as you were hoping for – that the supervisor – who is the head of department in our case – will allow for you* [*to stay*] *anyways.* (m4.2: 929-32[xcii])

As is illustrated by this quote, it was particularly in moments when someone was not able to finish his/her research in the given contracted time that the lab leaders' role in creating continuity – both in their employment and their research – was emphasised. His/her symbolic as well as economic resources, but also his/her character, sense of responsibility, feedback practices and managerial skills were not only highly valued scientifically but also with regard to how well they felt protected in an uncertainty-intensive environment. In that overall lab leaders were regarded as being (also) their employers, they were seen as being in the position to provide social security to the lab staff – provided of course that s/he had enough funding:

> [*My lab leader*] *had – because he wanted to keep me – had three… other options for funding me in case my scholarship wouldn't have worked out. That wouldn't have been difficult, I would say.* (f4.1: 338-41[xciii])

The support of lab leaders was however not only seen as limited to economic resources. I will follow through by quoting from the career narration of an advanced postdoc to illustrate some other dimensions of support that many interviewees saw as crucial for making a career in academia. Here she reflects on the role that her lab leader had in her career so far:

> *…it was easy for me to do my dissertation with her… everything fit perfectly and I knew this would last for the next three or four years. And another aspect was, that* [my lab leader] *was well known internationally… I had noticed that at congresses. She knows a*

[25] For PhD students in bigger labs senior postdocs partly took over supervision and the acquisition of grants they were employed under. In such cases, PhDs tended to depict the postdoc as functionally assuming the role of the lab leader.

lot of people and many people know her. Really, the top scientists in our field know her. And that's of course important for a career. Because when you are from a no-name-lab, then they question you more than when they know: hey, I know the lab leader, I know she delivers good, qualitatively valuable work. It's a bit easier then. Even though it is not your own merit. But it makes things much easier and so it was a huge advantage for me. (f3: 103-14[xciv])

What she hints at here is the symbolic capital that young researchers acquire by working in a particular lab. From the perspective of researchers, the reputation of their lab leader was crucial for opening – and closing down – opportunities for making an academic career and as potentially relieving anxiety over permanently having to prove themselves outside of the lab. However, she then continues by saying that one of the most important resources that her lab leader had provided her with was time. Even if she narrated it in terms of a prolongation of her contract, the actual point she seems to be making here is that time in the lab is the critical resource for being able to develop ideas further, publish and finally to pursue an academic career in a relatively smooth and safe way:

That's a thing that we all value very highly… after [my PhD thesis] *I stayed half a year in* [her]*… lab to finish things,… publish etc.. And she let me finish before and paid me as postdoc, right? Others would say: 'Yes, finish first and then you can do your Defence. And then you leave.' Right? And of course you are more expensive, right? And she said: 'No, do your Defence and then I will continue to pay for you and then you can search for a job et cetera.'… that's again a thing where* [she] *has supported me a lot. She offered me a job…* [and while] *most university… assistants really work for the professor, right? Not only 50% but really almost 100%… And that was very different with* [her]*… She said: You work entirely for yourself. … So, I mainly wrote grants that are prestigious and that don't have such a large chance of success. But I had the job that* [*she*] *gave me and so I could afford the 'luxury' – under quotation marks – to hand in these grants… I've agreed with…* [her] *to do it that way.* (f3: 116-268[xcv])

Here, the amount of support is only possible because the lab leader at the time had won a lot of grant money. Nevertheless this example shows a broad range of actions that lab leaders can take to support individual researchers – particularly by providing them with resources that are important for building an academic career such as employment time, access to lab equipment and strengthening their reputation.

Particularly PhD students often hint at a further resource on which they depend: tacit experience and knowledge of how to handle the challenges of living and working in academia. As in the following example most of these accounts refer to ways of dealing with the given epistemic uncertainties of life science research:

PhD1: *And he* [*the lab leader*] *said, I should continue this project of his, and additionally I can establish another project… in the old project I would have the prospect to work on*

> *highly ranked publications. And should the second project fail, I would still have the highly ranked publications from this one… I think a system like that is much better.* (FGg_jun: 866-72)[xcvi]

It was particularly the very young researchers like this one who experience their success as relying on the willingness of others – and particularly of their supervisors – to share tacit knowledge of life science research. Whenever a statement like the above came up in a group discussion, several other researchers quite vehemently affirmed that they have had similar experiences. After the above quote, for example, other PhD students in the interview continued as follows:

PhD2: *Yes, that's again a question of supervision… most people just don't make the effort to think about these things and to plan and to bother. …*

PhD3: *Yes, I know exactly what you are talking about…* (FGg_jun: 886-92)[xcvii]

In this regard, PhD students described their position as being quite precarious since they still lacked the tacit knowledge for estimating the degree of uncertainty and the riskiness of a project. As a result, they experienced their individual academic success and quality of their work as strongly dependent not only on themselves but on their supervisors' ways of acting in difficult situations during the research process. When for instance a project was failing, the lab leader was seen as being in the position to recognise it from experience and as being in the position to prevent a failed experiment from threatening a young researchers' academic career:

> *It is seldom the case that a person exclusively works on one topic and needs to follow it through until the end. That's never the case. There are always backup procedures. There are enough other topics and when you see that this one doesn't work, then you can do a topic that is more secure, where we have preliminary work, where you know: Ok, that should work out… There are definitely options to counteract* [problems with research trajectories]. *Whereas when you'd have a supervisor who doesn't care and who doesn't bother – as I know it is the case in other departments – then you really have a problem.* (f9: 512-21[xcviii])

Being assigned to the right research question was seen as particularly crucial also with regard to publishing opportunities. As in the following quote, knowing about temporalities and modes of good and safe publishing was narrated as being crucial for quickly progressing in academia:

> *It is like, you do your experiments and we exchange a lot and at some point the lab leader says: 'Ok, I think it's enough to publish it. Let's try it!'… I rely a bit on the lab leader in these questions, that he has a feeling for which journal would publish that. Because I don't have any experience in that. You can only develop that over time… I rely on my boss for these things.* (f7: 1353-415[xcix])

The role of the supervisor in helping with these questions was described as being indispensible. Particularly from the perspective of PhD students it seemed as if

there was no alternative to relying on his/her willingness to advise the students through the research process.

In these discursive contexts, young researchers often implied that the managerial effort of their lab leaders were crucial for making research environments less contradictory and dissonant. As was also illustrated above, lab leaders have found practices of creating continuity in a lab's personnel and its research efforts despite a discontinuous funding situation:

> *…it is a relatively frequent scenario that you need to have interim funding. At the department we have the lucky situation that we have several projects and then you can shift money to and fro. … When you have a small lab group that doesn't have additional projects and there are no resources for interim funding, then it becomes more difficult… I know situations… where people are suspended… One collaborator of mine has finished her PhD now and they carelessly forgot to send in the next grant… if you want to secure continuity in personnel, then you have an overhead-time of one year or one and a half years, I would guess. I talked to a colleague who writes a lot of grants. He said, you need to start thinking one and a half years before your project is finished, that is, not only think but also write.* (m4.2: 976-1019[c])

Such narrations suggest important managerial efforts by researchers – and particularly by lab leaders – for continuously securing a sufficient amount of funding and for being able to provide interim funding when necessary. In most quotes it was described as being particularly important that the lab leader had experience in and skills for dealing with the temporalities of project funding. Some lab leaders were described as putting aside a back-up funding pool that they saved from project funds and thereby creating economic security buffers that they could draw on when a project ended and the next project funds were not (yet) secured or experiments took longer than expected.

An interesting point in these narrations was that while most lab members were in fact largely project-funded, the project structures were to some extent made invisible or unnoticeable in the temporal, epistemic and social fabric of everyday research. Particularly very young researchers often did not even know which project they were formally assigned to and employed by. This suggests that creating continuity in personnel requires that the lab leaders be ready to navigate – and somehow deconstruct – the given funding structures and create from the given resources a different temporal, epistemic and social environment for their labs.[26] In doing so, they actively create an environment that allows for more continuous and coherent research than a largely project-funded environment structurally provides. From the perspective of young researchers however such practices of navigating – of

[26] In this context it is also interesting to note that in institutions' self-representation – e.g. on websites – project funding structures are seldom mentioned. Rather, the lab and the lab leader that it is named after and the publications that lab generated were the visible units.

deconstructing and reconfiguring were experienced as providing them larger spaces for freely pursuing their research and as relieving anxiety over uncertain and highly fragmented employment conditions. What their narrations suggest is that actual living and working conditions within life science labs are not determined by funding or other institutional structures but are – to significant extent – malleable, depending on managerial practices of lab leaders.

What can be seen in many of the exemplary quotes above is an anxiety over the lab leader's sense of responsibility for the individual research processes and for the individual careers of their lab members. An example of moments of personal dependence in academic life were the situations in which the lab leader's authorship policies decided who would be included in and assigned a particular position within an authorship list. Since these are very sensitive decisions about who gets the credit for research findings and who does not, the lab leaders' position was seen as being particularly pivotal for deciding the further career prospects of his lab members. While it was often contended in interviews that some lab leaders had a quite strict and exclusive authorship policy, other lab leaders were described as following a relatively open and inclusive policy. One postdoc for instance told us that a lab leader had even renounced the last – and most prestigious – position in the authorship (it is habitually the lab leader's right to be named there) and gave it to her.

In order to get a grasp on the multiple ways in which researchers related to each other in what I have at the beginning called a clan mode of coping, it is at this point necessary to explore how lab leaders and others who were in the position to allocate resources in life science labs experienced their roles in this context. The strong notion of young researchers' dependency on the lab leaders' sense of responsibility and care was also matter of reflection for the lab leaders themselves. The following example shows this in the context of authorship disputes that researchers described as quite frequently happening. One lab leader illustrated his difficult position by talking about his regrets regarding a decision he once made:

> *Co-authors… that's a very sensitive point, I think somehow… I had a project for example… where we had a triple-first-authorship. (.) Because I've thought: Okay, these three people have… almost contributed the same, right. And then you learn a year later that… one of these persons… hadn't earned a first authorship… and that another of the three people… she had really accomplished a lot… and then you think: 'Hell! That was the wrong decision'… or 'I owe this person somehow, because she has contributed a lot.' That's something that almost hurts… when someone has suffered… Hell, that decision, should I have to make it again, I would make it differently. That's always bad.* (prof_m1: 1744-58[ci])

Narrations like this hint at a mutually perceived (partly) hierarchical mode of relating to each other within life science labs and at the complex challenges of

taking on the responsibility for lab members' careers. One of the senior postdocs who was already in a position to have her own projects and supervise PhDs was very explicit about her sense of responsibility and care for the researchers she supervised:

> *…when I supervise people, I am aware of the responsibility… it's not as gross as in America… where you can see that… a bunch of people are put on one project and one of them will be successful and will have a top publication. The others are in a very bad position then… That is, whenever we have a PhD student, I try to make sure that he gets – within the two or two and a half years that he is here – one good, or two good publications. Because I know that this will be his stepping stone to a good postdoc position or for a position in industry or whatever… That is, you have a certain responsibility… you cannot say: ok, now I have – under quotation marks – a few slaves that work for me and some of them will do something really smart that I can profit from… you have, you certainly have a responsibility…* (m3: 278-94[cii])

What these quotes by lab leaders and supervisors suggest is not only that there may be some potential for anxiety in such situations but also that lab leaders and supervisors assign themselves a crucial role in providing less experienced lab members with their tacit experience and knowledge, attention and care.

However to return to young researchers' experience, I would like to argue that lab leaders (or supervisors) are understood as having a central role in shaping everyday work experiences in life science research cultures. Particularly in the ability to create a less anxiety-stimulating local environment they seem to have a crucial role with regard to the immediacy with which conditions that further uncertainty-experiences (cf. parts two and three of this thesis) will affect young researchers' epistemic living spaces. By deciding the length of contracts and the handling of interim funding, lab leaders can potentially relieve some anxiety about contract cultures, reconcile breaks and ruptures in employment biographies and make futures be experienced as more solid. Since every contract period also marks an evaluation period in which researchers need to prove the quality of their performance, lab leaders' ways of evaluating and of giving contracts are crucial to the frequency with which experiences of competition are actualised in the young generation of researchers. In summary thus, whether or not they will be able to move on academically is seen by young life science researchers – amongst other factors – as depending on the ability and willingness of the lab leader to allow for it.

With regard to coping with uncertainties, the managerial practices and the responsibility of lab leaders (supervisors) can be read as ways of regulating the degree to which epistemic uncertainties are experienced as individual risks. By shifting around economic resources, they can avoid negative consequences of failed experiments for the individual; e.g. by prolonging a contract when failure seems to be due to bad luck. Conceptually such practices by lab leaders can be seen as ways

of creating risk communities in which negative consequences are redistributed and risks are shared amongst the lab community.

These accounts of dependency on the lab leaders' managerial practices and sense of responsibility, however, do not yet offer too much insight into how young life scientists themselves deal with uncertainties. However, young researchers do not picture themselves as passive within such clan-like relational networks. Since lab leaders are seen as being in the position to structure the workings of a lab in ways that the risks are either shared and brought into the collective or individualised, the act of choosing the lab – and therefore also a lab leader – becomes important. For young researchers it was not only a question of whether or not they want to work in the lab's research area but also of evaluating the funding basis of the lab, the ways in which lab leaders choose to deal with them and the ways in which they choose to handle supervision. During participant observation, for instance, a PhD student told me that she had picked her lab over the others because she knew that the lab leader and lab members had acquired a comparably high level of funding. She had also heard from other lab members that contracts were often prolonged when necessary and that the lab leader was easy to get along with (o_f4). In a certain sense though, by choosing a lab she also chose the way in which uncertainties were handled.

The accounts of researchers however suggested that once they had entered their lab it was important to network and make sure to find people who supported their cause when necessary as the following quote by a postdoc illustrates:

> *And then you need to make sure that you find a certain clique or some patrons… behind you that can speak up for you. Especially in areas with short-term contracts and where you extremely depend on the pleading of department heads and similar things… because regarding young scientific careers you can't speak of independence. We do in fact have independence – not in the way we work but in terms of that we have it, right?…* [but] *most need the support of the professoriate. They say: No, it is important that this person gets her contract prolonged. When you lack that support,…* [you] *need to leave.* (f6.2: 351-62[ciii])

Whether or not a researcher is included in risk communities is described here implicitly as depending on the relationships that s/he develops and maintains by networking. The subtext of the quote is that only a good relation with their lab leaders and other patrons can ensure that they are responsibly provided for. In this context, networking and communicating can be regarded as practices with which researchers aim at becoming part of certain risk communities and thereby part of risk-sharing tactics of others (e.g. their lab leaders' and sometimes also of neighbouring or related lab leaders).

It was remarkable in many of researchers' accounts about this kind of guardianship that they often narratively linked them to an absence of institutionalised or legal

forms of protection. One example for this is how researchers talked about issues of parental leave and the right to return afterwards:

> *So this legal issue of being able to come back doesn't exist anymore. We cannot come back like… we are supposed to, because there is nothing to come back. And then it comes into politics – like social small nanopolitics in the institutes – in the lab and then it comes to the professor. If he wants to have a mother working or also probably a father who is very involved in any case or not. And what I see is that a lot of professors… are very reluctant to take mothers back… Out of very… subjective reasons, at least for me they seem very subjective. And then they give arguments like: '… I can't rely on them, because they have, they will be home when their kids are sick, or they have to leave on time. I cannot tell them at seven in the afternoon, I need this until tomorrow.' And this is the kind of… arguments they bring for themselves, why they don't want to foster* [parents]. (FGg_pd: 426-40)[civ]

What we can see in this researcher's narration is an experience of basic social rights being pushed out by a culture of temporary contracts. The hope for guardianship by the "nano-politics" or personal preference of lab leaders is, what researchers fall back on in the gap of protection, that emerges when leaving formerly established social rights like pregnancy leave, the right to come back after maternity leave and parental leave are levered out by flexible employment conditions. A second quote by a senior researcher who just had had her first child shows how strongly researchers emphasise the dimension of personal dependency in the relation they have to their superiors:

> *…it's difficult… there is a case… in a PhD programme where a PhD student became pregnant and… the official statement was: her contract will nevertheless end. And only the PI* [principle investigator, i.e. the lab leader]*… has struggled for months – even with the rector, that… she can have the one year funding – the time that she has to be on parental leave – prolonged after that. But that doesn't mean that there is any legal security.* (f6.2: 849-56[cv])

From these impressions it seems that networks – and particularly the relation to the lab leader – are seen as essential for surviving in academia and therefore young researchers put much effort into maintaining these relationships.

Based on the description of young researchers, it seems that consciously choosing a lab remains important throughout an academic career along with networking and relationship-building. What seems to change from PhD phase to the postdoctoral phase, however, is that the practice of creating career networks broadens from building a strong relationship with the lab leader to developing international networks. The following quote from a postdoc shows how closely they link their future career options to the social relations they create and maintain:

> *…I was in Spain recently, it wasn't a job interview in the strict sense, but it might be a long-term option. And yes, you have your networks… also in Austria there are certain options. But not at* [my current institution]… *There is the possibility… in* [another town], *that's possible because I know people there from previous times. They are searching for someone, but they cannot say for sure, that it will be possible to create that job. But they are relatively optimistic.* (m1.2: 176-89[cvi])

He then concludes that it is probably wise to

> *…grab the people… to have networks in some ways. … Partly, I cannot say so easily, but maybe it's about some kind of boldness to apply for anything that only fits somewhat, maybe that helps?* (m1.2: 330-3[cvii])

In summary, we can say that lab leaders (and later on also broader networks) are seen by young researchers as necessary for providing relative employment security and social security. In that they are seen as defining the epistemic, social and temporal fabric of life science labs, they also appear to be in a crucial position to increase or decrease the immediacy with which young researchers face the conditions that make uncertainty-experiences possible. Young researchers therefore work to be included in lab leaders' (or supervisors') risk communities by building networks and maintaining good working relationships. I will refer to this mode of coping with uncertainty-experiences as the clan mode since relating to each other is depicted as vertical: people with few resources personally depend on those with more resources to responsibly deal with them. Like in the clan type of kin relations this mode of coping with uncertainties is described as being organised according to rationales of paternalistic care and guardianship that rely on informal liabilities within nets of relationships as opposed to formalised rights and duties.

12.1.2. Coping like a Collective

I understand the collective mode of coping to be the often invisible and unrepresented practices of commonalising – i.e. sharing – resources amongst the lab collective according to the rationales of mutual support and reciprocity that is experienced as largely egalitarian. They build on social negotiation processes by providing individual skills and tacit knowledge to the lab collective. Protection is – primarily – granted by self-organised solidarity.

A second (social) mode of coping with uncertainties is coping like a collective. In this mode, researchers strongly rely on their relation to fellow researchers and the everyday practices of collaboration. Despite stratifications within the lab community, which are ordered by qualification steps and positioning in the labs, most researchers put narrative emphasis on equal collaboration structures in the lab collective. As in the following quote they argued that mutual support and collaboration are crucial for productive research:

> *So, most things only work through teamwork. You will almost never be able to achieve anything alone. You always need people… some that do whatever kind of analysis, while others specialise in the biochemical stuff and so on. That is only possible in teamwork. You just can't do it alone.* (f8: 253-7)[cviii]

Such narratives subtly imply that resources like skills and tacit knowledge that lab members have at their disposal are exchanged collegially. In fact, many quotes suggest that this is necessary for a smooth research process. According to such ideal-type depictions of lab life every member has equivalent rights and duties in the functioning of lab work and the lab collective. Resources of individual lab members appear not only to be shared in a self-evident and self-organised way but are actively being made common and seen as collectively owned.

Besides making research more productive, researchers tended to describe practices of commonalising resources as making their lives in research less risky. In particular, these practices were narrated as preventing epistemic uncertainties from becoming an individual risk for the lab members in that skills and tacit knowledge of other lab members provided orientation during their uncertainty-intensive research processes and practical advice for handling their career futures. For dealing with uncertain epistemic practices young researchers described it as core challenge to find the right balance between tolerating a certain degree of uncertainty as a necessary part of the discovery process and knowing when this degree has been exceeded and a failing experiment threatens someone's career. Knowing where this line lies is described as a skill that cannot be taught in a textbook but as a "feeling" that is "developed" over time, as this postdoc asserted in an interview:

> *Yes, you need to develop a feeling for realising when a project has really failed and if you should continue or not. … it is typical that not everything works straightaway. Mostly you need to do many, many detours to come to a result that you hadn't foreseen.* (f1.2: 549-53[cix])

The crucial role that this balance had for them is best explored through their context-sensitive notion of failure. Whether or not an experiment had failed was always defined in relation to the time available. In other words, it was defined as failing when it did not appear to lead to publishable results in a given (often the contracted) timeframe. Particularly the very young life science researchers relied on the tacit skill that more senior researchers had developed in their years of experience. For that they relied on creating mutually supportive relationships. It seemed that in everyday research at the lab benches, researchers were monitoring and correcting each other's research processes in a cybernetic manner – i.e. in self-regulatory feedback loops. These processes required having enough time, flexible work organisation and early diagnoses of mistakes in order to prevent epistemic uncertainties from becoming a risk to the productivity of the lab's research.

This way of coping with uncertainties however was described as requiring a particular way of relating to other lab members; i.e. a particular kind of collective subjectification: namely one that is rather horizontal and that relies on the perspective that every lab member is equally responsible for smooth activities in the lab. This mode of coping was discussed as being particularly important for the inexperienced researchers but nevertheless depicted as vital throughout a research career. Many labs therefore organised semi-institutionalised feedback meetings in which the individual projects were discussed collectively:

> *You present regularly in so-called lab meetings and there you get input from other people. It's important – and it stays important even when you become an established group leader – that you have this kind of discussion…* (f3: 655-8[cx])

The kind of skills and tacit knowledge that they spoke about in this context focused on how to plan and organise research and publishing processes in a way that corresponded to contracted time periods and career requirements. A postdoc for example emphasised the need to develop a concise personal career plan:

> *And say to yourself: OK, time has progressed. Again and again I say that to younger colleagues, who let themselves be exploited. And I tell them that they must go…* [to their supervisor] *and say…: 'It's not working.' And* [ask] *if there is the option for them to work on another project… however, let them make a time plan for themselves. Otherwise you bob up and down and six years pass without having worked within a well-defined area, because you have been assigned here once and then again somewhere else. That happens… in our area it is necessary to do stuff yourself. Nobody tells you that, but it's a good idea.* (m4.2: 883-91[cxi])

What this postdoc insinuates is that from his perspective it is important to avoid anxious situations by becoming acquainted not only with formal career requirements but also with more tacit career knowledge. Since career knowledge was best learned from more experienced fellow researchers who have recently been in a similar position, what he describes as crucial is a practice of making intergenerational tacit knowledge about the academic career system common. As he further explained in the interview, he had learned by the time of his graduation that working in a lab for a long time without developing an individual research profile can be problematic and far too time consuming – particularly when it comes to writing a concise PhD thesis. Most significantly it can leave young researchers with too little scientific capital to pursue an academic career. Making available hidden and tacit knowledge about how to move on in academia can thus prevent others from manoeuvring themselves into this anxiety-laden situation and is a voluntary way of helping younger colleagues that is organised according to a rationale of solidarity towards them. For the postdoctoral period, this kind of knowledge-sharing was described as crucial for minimising uncertainties in the publication process:

> *And when you want to be sure – or particularly when it is a high impact journal – then you send it to… other peers, colleagues on the same level as you are, mostly outside the campus. Let's say: ex-colleagues or people that you have met at conferences… or ex-collaborators and you ask them for confidential feedback, what their opinion would be.* (m2: 307-12[cxii])

As is illustrated by this quote, researchers communicate and socialise within a network of colleagues in order to achieve more certainty in publishing results. In some cases it seems that even time resources are made common in order to relieve each other's anxiety during intense research phases when the epistemic process requires their presence almost all the time:

> *…when you want to make a weekend trip, you need to look and see that someone can put in a weekend shift for you and such things and that's not like in a firm where you are paid for such things. Rather people do that, yes, for free and… you need to sponge the precious weekend time from your colleague that is unpaid… That is inconvenient… it mostly works on a personal basis, because those are favours that we mutually do each other.* (m4.2: 767-74[cxiii])

Building on an assumption of reciprocity, commonalising time resources is described as self-organised solidarity for coping with the restlessness and anxiety that can emerge in uncertainty-intensive work environments.

Such ways of working together in terms of commonalising skills and tacit knowledge appeared to be subtle ways of refusing competitive dynamics. Especially in smaller research communities researchers explained that it was common to develop informal social mechanisms to avoid two researchers or research groups working on the same questions at the same time and thereby prevent competition:

> *You always have competition with other labs. Maybe that is not intended from the beginning… It depends on the research field. When the field is big enough, there is certainly competition. Then you often have one very hot research question… and it's all about who will solve it, who will be top at the moment and has brought home the bacon. It will always be that way. In case the field is smaller, it is usually the case that the people know each other very well… and they amiably split* [the question] *– I would say. Or someone is in the position where he can solve the problem faster or better. And the other will say to himself: ok, I'm going to focus on something else.* (m2: 490-501[cxiv])

Narratives like this suggest that some – particularly small – research communities subtly develop negotiation practices to decide how to spend the available resources to solve the prevalent research questions in a particular research area. They manage resources through division of work and by complementing each other's research activities. Thereby they avoid the duplication of research efforts and prevent themselves from being drawn into competitive dynamics. The following quote from a PhD student illustrates how this exchange and mutual assistance is experienced as

crucial, such that to not engage in these practices would be viewed as an assault on academic culture:

> *Sometimes I have the feeling that people hold back with their competencies. They don't want to give them away because they want to establish a position within the lab in which they are indispensable. (.) That's my personal opinion – especially concerning* [this postdoc]. *I know it, for sure! That's indisputable! Because I have asked him 100.000 times if he can explain this thing to me, so that I know it myself and I can do it myself. And*: [the answer was:] *'No.'... I don't want to question his competencies. But he doesn't transfer his knowledge. And honestly speaking I think* [that] *happens consciously. Because when I seal myself off from all the others in one area, so that they always have to ask me, then I am indispensable... But that doesn't have anything to do with* [the idea of] *university, I have to tell you honestly.* (FGk_jun: 257off[cxv])

In this quote, we can see a tension that is characteristic in many narratives about academic work cultures: that contemporary career models in academia tend to favour a privatisation of skills and competencies while at the same time academic research cultures deeply rely on making them common within research communities.

Feedback-loops like the one described above appear to be embedded in life science research cultures in everyday practice but are of course also anchored in many (semi-) institutionalised ways (like collective publishing, PhD programmes or lab meetings). In contemporary life science research cultures however, the extent to which individual researchers are included in such practices varied to a large degree, as the following quote demonstrates:

> *It really depends on the PhD programme that people are in. In* [that specific] *programme... there are committee meetings in which findings are discussed and that should be a forum that helps, where suggestions are made – like: Should that project be stopped or not? This project already lasts too long and nothing comes out of it... in which direction should it be developed further? And also every person has a mentor... with whom she can speak to about problems. The first contact person is of course the supervising person who has an interest in the success of the project himself. But when for one reason or the other the interaction is not that smooth, then there is this... external mentor, who is also at the university so that he understands, where the problems lie.* (f6.2: 761-76[cxvi])

The involvement of individual researchers in such feedback communities thus seems to be context-dependent. Regular lab meetings for instance were indeed narrated as quite common in life science labs but in the particular cases that I investigated for this thesis often tended to be influenced by time and resource constraints. In such contexts, being included in feedback communities appeared to be dependent on environmental conditions such as the specific structure and size of

the lab, the funding structures behind them (third-party funds, PhD programmes) but, most importantly, on researchers' ability and their available time to self-organise social feedback structures. Particularly when more experienced lab members who could provide younger staff with tacit knowledge and embodied skills were under high pressure to produce valuable results for proceeding in their own academic career, they tended to describe it as taxing to provide regular feedback to lab members. In this context it seemed that being protected by a lab community largely depended on researchers' capacity to socialise within the lab and to become part of self-organised risk communities: "*It always depends*", a PhD student told us, on "*how you are positioned in the group*" (f4.2: 858[cxvii]). How important it is to remain in good standing with other lab members and to be able to adapt to a changing lab environment is illustrated by the example of a PhD student who used to work together with some colleagues very closely. A few years later she had lost this opportunity for exchange due to the time pressure that her collaborators were facing. In an early interview she said:

> *…the environment is perfect. Because I have the right people to work with… For my PhD-project it is necessary to be in a scientific environment, that I have XY and XX and also…* [the lab leader] *who does things for me. And without that – when I would be somewhere else with the same topic – I don't know if I could do it…* (f4.1: 393-763[cxviii])

This perfect environment then changed over the course of her PhD thesis. The postdoc XY who had supervised her for her Masters thesis was not the formal supervisor for her PhD any more. She was then assigned to the lab leader for supervision whom she described as lacking the time and up-to-date hands-on experience because he increasingly had to take on managerial tasks within the growing lab. The two postdocs (XY and XX) she mentions in the quote were much less available for feedback and guidance due to new projects, other students and their career and family planning. Thus, two years after the first interview in which she emphasises her good social exchange network she describes herself as "a solitary worker". Her efforts to re-institutionalise lab meetings and seek assistance from her former supervisor had mostly failed and her position within the lab had worsened to a point where she could not get any feedback for a paper she had written:

> *Then I took the critique* [of the journal] *and revised* [the paper] *and I sent it to my colleagues and no feedback came in return… the situation is: I am a solitary worker… I didn't speak to my boss in the last ¾ of the year.* (f4.2: 170-4[cxix])

When she realised her situation within the lab, she started reaching out to international colleagues abroad for feedback and exchange of experience:

> *…and that's a bit uncertain… considering that there are problems… that you get… and of which you don't know if you will be able… to solve them alone, right? I mean, I have… my Joker*[27] [a researcher from the US] *whom I always ask…* (f4.2: 137-41[cxx])

Like this PhD student, most researchers seemed to gather a manageable group of people for informal feedback-loops and advice. These peer groups were often long-term relationships and often had a personal or friendship-character. Early stage researchers who first entered a lab used to have peer groups within their lab but later on they were a mix of former and present lab colleagues. The labs they worked in or passed through during their scientific lives however appeared to be the epicentres of these relationships (cf. Felt et al. forthcoming).

What I have tried to show by discussing how researchers manoeuvre within everyday working lives according to a clan and the collective rationale is that the social fabric of academic life science research equips young researchers with a range of ways for coping with (conditions of) uncertainty-experiences. They can relieve anxiety over epistemic uncertainties that are experienced as individual career risks but also relieve anxiety about high levels of competition, time pressure or an always latent future. According to both the clan and the collective rationales, researchers appeared to sound out ways of shaping their research environments. Even if they followed quite different rationales of relating to others: coping like a clan builds on the rationales of paternalistic care and guardianship and coping like a community is based on making resources common, self-organised solidarity and feedback-loops. While the practices within these two social formations are partially the same – in that they build on care, communication, socialising and networking – these two modes can be thought of as representing two different (ideal-type) forms of collective subjectification: one that builds on a hierarchical social structure and personal dependency on provision and one that builds on egalitarian social dynamic and reciprocity to provide each other with skills and tacit knowledge. As has been shown in the narrations above (and will be discussed further in chapter 13), both modes of coping usually complement one another in the everyday but can also come in conflict and push each other aside. These dynamics seem to be governed by the particular environmental conditions as well as by the researchers themselves to prioritise one over the other form of subjectification. It seems that in loosely structured lab environments with few institutionalised feedback-mechanisms, the skill of socialising and self-organising feedback mechanisms becomes more vital than in more institutionalised environments. The amount of relief from uncertainty-experiences researchers can provide each other appears to depend on

[27] This is an expression that refers to a card game: a "joker" is the highest trump card.

conscious efforts by lab members to responsibly organise lab structures, distribute resources and maintain common spaces.

12.2. Coping as an Individual

In discussing the social modes of coping it seems that dealing with uncertainty-experiences and the conditions that make them possible requires certain skills and tacit knowledge as well as the capacity to utilise different forms of subjectification. The previous chapter discussed two forms of collective subjectification that appear relevant with regard to dealing with uncertainties (like a clan and like a collective). Researchers' narrations however show that while in earlier phases of a life science career they can and – due to their limited experience – must rely on social ways of coping, the higher up in the career hierarchy life science researchers get, the more they are expected to be able to deal with uncertainties, ambiguities and tensions alone. From the postdoctoral time onwards it is assumed that they have acquired skills and tacit knowledge for dealing with epistemic uncertainties themselves, that they have established peer groups and networks for self-orgainsed feedback-structures and that they have gotten a handle on how academic careers work. Felt et al. have described this process of becoming more independent during an academic career as a process of "reconfiguring the individual", meaning that it seems imperative for people pursuing an academic career to carve out an individual career project of the (to large extents) collaborative research processes in order to become recognisable as an individual researcher with a defined research profile (Felt et al. forthcoming). Life science researchers in fact often say that they are trained to perceive themselves as an individual career project within their academic environment. In this context being too much linked to others' work is perceived as threatening to an academic career,[28] as the following quote of a PhD student illustrates:

> *But essentially…* [my PhD project] *is closely connected to what* [this postdoc] *was doing. That's why in the meantime I kind of panic because I'm not doing something completely, terribly new and she has done it partly already, right? But… I try to widen this and look and test it. It's not that I will not do anything new in science but it is closely connected.* (f4.1: 387-93[cxxi])

Notions of carving out an individual research area appeared to become more important the farther along an academic career they got. As this postdoc emphasised, a good CV in the life sciences requires a concise and individual scientific profile:

[28] Ruth Müller shares this observation in her argument that enhanced career competition puts a strain on teamwork and collaboration" (2012: 38).

> *And it is quite important that you… distinguish yourself… And I have written the first grants so that they are a bit related to the things that I had done previously. Because of course these were things that I had published already. And when I change and say: I do something completely different, then people say: Hey, you don't have expertise, no preliminary work. … But in a long-term-perspective I have searched for a new model system… and have read about it… now I have also found it.* (f3: 237-46[cxxii])

In such quotes it appears that researchers need to find the right balance between maintaining some proximity to others' research in order to be noticed at all, while at the same time contrasting their work with the work of their colleagues in order for it to count as original and to be recognised as a researcher with an individual profile. This is described as fundamentally important for having the chance to finally establish oneself permanently in academia as a lab leader. Felt/Fochler/Müller have argued that researchers believe that in order to have a career in the life sciences there is no alternative to this teleological orientation towards becoming an independent group leader (cf. Felt et al. 2008: 9). I will further explore the tensions that result from the social collaborative aspects of research while at the same time carving out an individual career in chapter 13. In this chapter I first want to get a differentiated view on what it means to cope individually with uncertainties, ambiguities and tension. Within this partly tense relation between the social and the individual, life science researchers described two slightly different ways of conceiving of themselves within their academic environments. I will therefore identify two forms of individual subjectification that appeared to be relevant with regard to coping with uncertainty-experiences; namely coping like a manager and coping like a trickster.

12.2.1. Coping like a Manager

I understand the managerial mode of coping as relying on self-guided tactics of avoiding risks in order to make an academic career. It builds on tactics of articulating heterogeneous conditions into a continuous, homogenous and successful career project. Protection is guaranteed by the practice of re-ordering the environment and of optimising the individual's investments.

The way that researchers talked about their living and working in the academic life sciences often insinuated that in pursuing an academic career, they had developed a managerial attitude towards themselves and towards their environment. They for example described practices of configuring a continuous and coherent career project within fragmented contexts (cf. Felt et al. forthcoming & Garforth/Cervinková 2009 have discussed this for similar empirical samples). Such managerial efforts are often meant to patchwork the fragmented pieces of academic

life together in order to secure epistemic and temporal coherence and continuity as much as possible, as the following quote illustrates:

> *…that was… a project for two years… I knew I could do research for a year… or one-and-a-half years before I needed to look for new funding… It's indeed good to have a certain competition but there has to be a certain continuity, that you have something like a tenure-track position or something… that you say, I can do research, and I can carry on doing research, and I don't need to spend half a year within these two years writing a proposal to secure my position…* (f6.1: 335-48[cxxiii])

As discussed earlier, the fragmentation of employment biographies – e.g. by part-time employment – has become more common. While PhDs in the life sciences often held a full-time position during the time allotted for thesis writing, it was often the case that postdocs had a mixed income – i.e. that they were funded by more than one source. Only very rarely did postdocs hold the "traditional" postdoctoral assistant-positions with a six year contract. Many already had their own funds – either a scholarship or a research grant and it was likely that project funds did not support a full-time postdoctoral position. Rather – and since their salaries are higher than PhD's salaries – it was likely that postdocs' incomes were mixed. Their lab leaders – as well as they themselves – often did not apply for full-time postdoctoral position in grants. Not because postdocs preferred working part-time but because they did not think it was promising to hand in grants that they thought was too expensive to be funded. When we asked a postdoc who was in this situation whether it was possible to pay herself a full-time position instead of splitting her grant up this way, she answered:

> *Yes, that would maybe have been possible… but then I wouldn't have had enough money to pay a PhD. It was difficult enough to get this half-time position; because of course a postdoc has a higher salary than a PhD-student…* (f6.2: 49-53)[cxxiv]

As is illustrated by this quote postdocs would also assert that they depended on the cheaper labour power of PhD students (to do the empirical bench work) in order to stay competitive. However, for keeping up with performance standards in academia however, being a part-time researcher was not considered an option. Even though they often had part-time contracts, they tended to work full-time or, when they had the opportunity, they patchworked different funding sources together so that it added up to full-time employment. However, when these different contracts were not simultaneous or conflicted in their temporalities – as they often did – they had to find interim funding solutions or manage to live on half-time employment for a while. In one of the cases mentioned above, the university position lasted for two years while the project lasted for three years. One interviewee who had 50% funding through a university position and 50% through his own project explained that he was going to lose half of his income as soon as the project ended. Securing full-time employment for a longer period of time in such patchworked employment

situations was described as requiring extensive managerial efforts – and as harbouring a high potential for anxiety. In the latter case, the supposed follow-up project had been recently rejected, and he was now in the situation of not knowing where half of his salary would come from at the end of his current project. Considering that he had two children to care for, this was a situation that he would of course have liked to avoid. Even if he hoped for other opportunities to arise, he thought about quitting his job because he could not afford to do it any more (m1.2: 32-3[cxxv]).

Another aspect that researchers often described in managerial terms was how they learned to approach epistemic uncertainties. Implicit in the discussions of ways of coping according to the clan mode and the collective mode, was that researchers developed skills and tacit knowledge for evaluating epistemic uncertainties in order to prevent them from being experienced as an individual risk. Framing epistemic uncertainties in terms of risk however is not a given but a way of coping with uncertainties in its own right. In researchers' narrations about risky research, the institutional organisation of research always plays a crucial role, as this quote demonstrates:

> *The risk is very high… the…* [one] *project… is very risky, it is very risky. … I hope that something will come out but also that… with its little sub-projects, that I can define sub-targets that can lead to a publication anyways. I hope that I can accomplish a certain, a certain output this way, right; without being here* [in the lab] *day and night and needing to take the… shortcut from the sixth floor down, right? (laughing)* (f6.1: 469-77[cxxvi])

The exit-option "shortcut from the sixth floor" is used here as a vivid metaphor for the experience of existential personal threat involved in not being able to produce any valuable output. According to her line of reasoning, she needs both, calculable, "cash-cow" aspects and incalculable, personally "exciting" aspects in order to advance. The challenge seems to be to find the right balance. While in her earlier postdoctoral years (cf. the quote above) she had focused on risky research, in a later interview she put more emphasis on secure research and reflected on the process of acquiring the skills and tacit knowledge to balance risk within contemporary conditions of doing academic life science research:

> *I think my projects have been too risky. I would do that differently today. It was very difficult at that time… because there was no preliminary work and I think I am now able to balance that better. From the beginning on, with the little preliminary work that we had, it wasn't very smart to do that. But it was the only way. … Risky research is exciting, and secure research is cash cow. … With* [secure research] *I can publish in any case and when I have publications… my staff is not frustrated, I am not frustrated and I get new project-money, so that I can move on with my work.* (f6.2: 520-48[cxxvii])

The smart management of epistemic risks in her narration is a crucial skill for preventing the risky situation of ending up at the end of a contract without any output. A frequently mentioned way of managing risk was running on two tracks ("zweigleisig"; f1.2: 288[cxxviii]) – meaning that they had a tactic of combining different research trajectoreis. As is illustrated by the following quote, this strategy is a way of organising research where at least one branch is a secure project:

> *Especially when you are doing a dissertation or so, then… at least part of it should be a relatively secure project so that you have some findings for your dissertation.* (f1.2: 288-91[cxxix])

Many researchers thus ended up relying on mixed-risk tactics. In order to do so, researchers performed sophisticated risk evaluations in which they considered and balanced the epistemic characteristics of the research questions and the time frames of their contracts in order to decide which question was reasonable to pursue. As in the following quote, choosing the breadth of a research question seemed also to be regarded a form of risk management:

> *…when there is a certain funding structure, then you know, this would be an interesting project – but it is not doable with this kind of money. In* [my research area] *it is very cost-intensive and you can do interesting (.) combinations… and then you must consider, how much can I observe, how far can I still fund it, right? So, that's very flexibly adapted because otherwise you can't do it.* (f6.1: 988-1000[cxxx])

The interesting point here is that success in academia is defined as a question of risk management. Not scientific qualification nor the innovative potential of their work but the ability to estimate and balance – to manage – risk is foregrounded. Good performance appears to result from successfully coping with epistemic uncertainties:

> *It must not have negative consequences, but… I have the impression, I observe it in my own mental processes, that you don't have the freedom to say: Ok, that would be a project, that runs over a longer period of time, where you would have to bring in collaborators… I might be able to do that as soon as I have established myself – that I can announce the project myself – that would make sense. But right now when I don't know who will pay my salary… after 2011 this is* [*not possible*]… (m4.2: 1289-95[cxxxi])

Researchers tended to narratively link these risk management strategies to how they monitored their research processes and their own performance:

> *You cannot avoid an experiment failing, you can't!… the solution for that is to monitor yourself and ask: Can I trust my findings… or is the whole thing too difficult for me.* (f6.2: 740-8[cxxxii])

This way of treating uncertainties as risk is based on the distinction risky vs. secure as opposed to uncertain vs. certain. Risk is a specific idea of uncertainty and of a future that can be administered by a process of rational control. Managing risks

treats epistemic uncertainty as something potentially measurable, controllable and follows an objectified notion of uncertainty. Risk – as opposed to uncertainty – appears as a problem that is estimable and manageable with the right knowledge and skills. Because they see their balancing techniques as linked with their probability of making a career, it seems that risk management – including self-monitoring – is a way for them to soothe the anxiety over latent futures in that it makes them experience their uncertainty-intensive environments as less contingent and more controllable. Coping in terms of risk management is a form of self-monitoring, of permanently re-evaluating the quality of one's work and one's ability to balance risk and security.

Coping like a manager for researchers also tended to be about rationales of time investment. What the above quote points at is that one crucial resource that young researchers have to manage for themselves is the time they have at their disposal. Whether and to what extent they follow a risky or less risky research approach is primarily a decision about where to invest their limited time resources. Since they conceived of their occupation not primarily as a job but really as a very personal enterprise, many tended to extend the invested time resources into private time, as a PhD describes:

> *…it is not 100% a job in the sense that I would see any other job. Rather, it happens that in my private time – and it happens a lot lately because I'm writing my dissertation – that I just read in my private time (laughs) … when I'm at home… when you take that as working hours too, then the overtime-balance is really tough.* (m4.1: 420-5[cxxxiii])

Conditions of high epistemic uncertainty seem to make them mobilise as many personal resources as are available as a compensatory move to increase the probability of valuable output. A postdoc explains this by saying that it is "either luck or diligence" (m1.2: 367[cxxxiv]) that leads to a successful career.

Coping in such managerial terms seems to have a long-term orientation toward a future career project while its actual practices are often oriented at securing short-term prospects for two to three years. What characterises the managerial mode of coping is a form of subjectification that is liable to patchwork fragments of an academic life into a coherent and continuous career, to monitor one's own performance and to optimise time investments for being able to progress.

12.2.2. Coping like a Trickster

The trickster mode requires practices of evading requirements, of disregarding implicit and explicit expectations and of considering alternative career paths. By refusing to trade the present for an always latent academic career future, it represents a mindset

and subjectivity that allows researchers to reclaim a liveable present and reject anxiety over uncertain futures.

What characterises the managerial subjectivity is that it complies with expectations for optimising valuable output in order to meet the requirements for making an academic career. As discussed above, being oriented towards a future that is always about to arrive means that the present tends to slip out of sight. In researchers' narrations however there was another orientation traceable that is less committed to the academic career trajectory and less bound to formalised requirements and quality criteria. It is characterised instead by a mindset of deviation and a desire to resist certain environmental conditions.

Many researchers would for example emphasise that following their interests is sometimes only possible by hiding their original motivation from evaluation or funding institutions. Most of the interviewed researchers claimed for instance that even if they were using arguments of societal relevance to acquire funding, what they intended to do was, in fact, basic research:

> *…you just have to write some sexy things in your grant proposal. Something, any catchphrase, it must be something like cancer or HIV or something applicable; even if we are far away from any application. What we are in fact doing is purely basic research.* (f2.1: 148-52)[cxxxv]

In this context they would for example claim that they were unwilling to change their research focuses because of the requirements of a funding institution. As we can see in the following two quotes in which researchers answered the question of what they would do if a research idea did not get funded, many researchers said that they were likely to pretend that they were working on another project while they were in fact following their original plan:

> *I would apply for a different project, but continue doing my idea… That is a bit exaggerated… Formally you work on a project but really you work on… this actual project that has been previously rejected…* (m1.2: 891-6[cxxxvi])
>
> OR
>
> *You just try it once again somewhere else* [a different funding agency], *or you try to sell or package it better* [to the same funding agency]… *or you write something different and do your project anyways.* (f1.2: 724-6[cxxxvii])

Such practices are often narrated as quite sophisticated ways of creating opportunities to continue the research – that is to create a sense of certainty. At the same time they narrated them as creating spaces where epistemic uncertainties can play out without turning into individual risk. Researchers often experience their original interest as uncertainty-intensive and exciting but also as being incompatible with the narrow time frames that project funding offers. In some ways

though, their narratives of working around expectations can be read as a way of buying – or in fact stealing – time to follow their uncertain research interests. As in the following quote, one way of doing so was to stockpile reliable findings in order to have something *in petto* in case other more uncertain experiments fail:

> *I write something in the project proposal that is already done or so… that is very common, that – not only here but also in America – that you write in the proposal about what you have already done; or at least a part of it. Then you can say relatively early on: ok, here is a publication*. (f1.2: 314-9[cxxxviii])

Such practices of bypassing project rationales appears to relieve the immediate pressure to be productive and allow for more autonomous research. At the same time they reclaim spaces for epistemic uncertainties to happen – for "*side projects*" in research, in which the outcome is not foreseeable (m1.2: 682-3[cxxxix]). This suggests that with such tricky practices they seek the autonomy to allow for uncertainties to happen and to refuse to set conservative limits for tolerable uncertainty by staying on the relatively well known grounds of secure research. In such contexts creating spaces of autonomy is equivalent to carving out spaces for highly uncertain research questions – or as one researcher referred to it, "*free project[s]*" (m2: 455-7[cxl]).

In order to grasp the essence of this trickster mode of coping, it seems important to identify two interesting aspects in these narrations: first, researchers' practices of trickstery within their academic environment seem to allow them to shape their everyday research cultures. By doing so, they not only create an environment for themselves that is less contradictory and dissonant and therefore less liable to create experiences of anxiety, but it is also a way for them to actively participate in the governance of emerging research cultures. In that they create spaces for epistemic uncertainties to play out. Another central aspect in such accounts of trickery is that they suggest that from researchers' experience it seems that the academic environment and its framework of expectations becomes more liveable by creatively manoeuvring and by being deviant.

There are a couple of subtler accounts of such deviance. For instance they were traceable in accounts of considering alternative professional futures that often implied the refusal of explicit or implicit academic career norms or requirments. It seems that the degree to which researchers consider employment alternatives has a significant influence on the intensity with which they experienced anxiety within their academic environment. While most young life science researchers we interviewed aspired to a future in academia, some nevertheless thought about alternative forms of future employment. They started to consider other job opportunities where they could realise their desires and utilise their skills. One postdoc put it like this:

> *…right now I'm very much in doubt over what I should do in the future, because my… scholarship runs out in half a year. And probably I will apply for another scholarship… to continue for a few years…* [my job future. But then I had an interview for a non-academic job]*… And it was indeed interesting. … it went really well…* [Anyways] *this is something I could do anytime as a backup.* (f1.2: 27-87[cxli])

Even if it was not considered all that easy, having other ways of earning their living seemed to be relieving and to confer a sense of certainty to the present. A PhD student for example explained how her anxiety over possibly failing experiments was relieved by having another professional path in sight. Exit strategies thus allowed for more tolerance of uncertainty, for more autonomous work and for following their own ideas about what valuable research is. The same PhD studend for example explained:

> *I don't really care. Because I think, it is my dissertation, nothing needs to come out of it, right? Mainly because my life path – how can I say that – I have already decided to leave research. So, I completely don't care whether a paper* [comes out of it]*… I don't need it. Sure,… if you wanted to stay inside – right – you would already have a crisis* [in this situation] *like: Damn it! I've been working for three years now and nothing has come out of it, and maybe nothing – absolutely nothing – will come out of it. That's for sure… I think you would have a crisis already. I only want to finish my dissertation and that's why the crisis is not that bad.* (f4.2: 134-53[cxlii])

Thinking about alternatives also seemed to allow researchers to have faith that they would be able to build a life outside academia and that they would be able to learn new things quickly and adapt to new working situations:

> *That's a reorientation that I would have to do in my head. But I don't think, that I would have to learn a lot intellectually… except for the skills that are required there. It is the same as when I switch to a completely different project here, there would be new things to learn. So, I would certainly risk doing that…* (f6.2: 468-72[cxliii])

Quotes like this represent a line of thinking that encourages researchers to abandon the idea that only academia can guarantee a good and personally fulfilling professional future and trust in the ability to find alternatives and that one's skills will fit in easily somewhere else. For her this seemed to relieve anxiety over an uncertain professional future. Because it allows for a rejection of the academic career orientation, considering career alternatives tended to be narrated as defying the tacit obligation to meet all requirements for pursuing an academic career and to permanently having to engage in career planning. A broader range of options and a refusal to engage in career politics seemed to relieve the anxiety that leaving academia was going to be a huge loss in their lives. Some for example intensely refused career-networking opportunities, such as a postdoc, who explained why she chose not to attend career events that taught career-strategic decision-making:

> *…sometimes people come for a ‚career lunch' or the like. Someone comes and talks… about his life and what he has done… I wouldn't call that job consultancy in that sense… but I barely have the time to attend. I rather do my own thing. Maybe that… that's probably a mistake. When you want to advance in these kinds of structures you have to do these things: coaching and networking and mentoring and things like that. It would probably work out better like that.* (f1.2: 755-73[cxliv])

By deciding to ignore the requirements of networking and career planning she was reclaiming a different ways of experiencing time than the one she felt was expected of her – one that is not so much concerned with future opportunities but with present activities. In a sense, this can be understood as a way of refusing immediate responsibility for the future, as communicated by an early PhD student who was quite uncertain about her future employment situation:

> *I only think about it when it's happening. I'm not someone who plans a lot in advance, because mostly you are disappointed. That is, I wait and see, I don't know how long the PhD will take, how long this will be prolonged, whether in fact it will end with the end of the project or, one doesn't know yet.* (f8: 299-303[cxlv])

Detailed planning and strategy-building were also rejected by a postdoc who refused to manage his uncertain employment conditions by letting these practices control the decisions he made in his private life – such as having children:

> *Or you just think: I don't care, if I don't have children now, I will not have them at all. To hell with protection.* (m2: 996-7[cxlvi])

By not letting fear of missing protection interfere with his personal decisions, he can be understood as not allowing himself to be (too strongly) governed by – or subjected uncertainty-experiences by subverting the implicit expectation of fully devoting himself to academia and to its career requirements.

In that sense, coping in the trickster mode seems to be oriented around creating liveable presents. It is a way of creating spaces in which they can work with uncertainties instead of against them, a form of reclaiming riskless uncertainty. For this mode of coping researchers tend to describe it as an important lesson to learn how to pretend, improvise and be opportunistic in certain situations in order to make the academic environment more liveable. By carving out spaces for uncertain research questions they seemed to carve out spaces of autonomy and time for re-connecting with their personal motivations. In doing so they create and enlarge niches to work in a way that they tend to experience as less estranged from their desires, that is autonomous and self-determined.

It is a mode of coping that seems to be not so much grounded in certain practices but rather in a mode of subjectification that allows researchers to experience themselves within their academic environment in a certain way, namely as being able to live better through deviance. I use the figure of the trickster to describe this

mode of coping because of the way that researchers understand themselves in relation to the environment is very similar to the tricksters that Lewis Hyde describes as needing to be "masters of deceit if they are to proceed" (Hyde 1999: 7). In the trickster mode, researchers play tricks and otherwise disobey the rules of conventional behaviour and subjectification in order to pursue their needs or personal motivation and make life and work more liveable in order to survive when they experience their environment as hostile.

13. Reflections

13.1. A Remarkable Absence: The Institutionalised Mode of Coping

When we take a step back and consider that people have always been facing precarious situations, we make a remarkable observation: Amongst the range of ways that have historically been developed to cope with uncertainty-experiences, one that has been integral particularly in the second half of the 20th century in Western industrialised countries is largely missing in researchers' narrations: institutionalised ways of coping with uncertainties. For my purposes here I define as institutionalised mode of coping when every member of a defined social group is granted certain basic social security rights. Examples for how protection has been secured in such a way are social insurance and unionising. From a historical perspective, these social infrastructures have been built within the past century in order to collectively deal with the precarious aspects of life and to redistribute and share the burden of bad luck and individual risk such as with health insurance for the risk of illness or unemployment insurance for the risk of being jobless. While these institutionalised forms of social protection have never replaced other forms of coping with uncertainties (such as for instance in clans or collectives), they have come to accompany them and partly eclipse them. It would seem plausible therefore that institutionalised ways of coping would be more present in researchers' narrations. Why that is not the case is worth considering. In the rare moments when researchers mention for instance the right for paternity leave or unemployment benefits they tended to refer to them as unavailable to them or as being systematically hollowed out by the way academic research is currently organised. In this regard they particularly brought up the temporary contract culture and linked to it the personnel policies of their universities:

> [The] *university should be the apparatus that supports you… in doing your research… today, all…* [research] *is mostly conducted via projects and anyways most people… take care of their funding by themselves and it is difficult enough… But when – additionally – the university says goodbye after six years – regardless of whether you have funding or not – that is really very counterproductive… this is where I clearly criticise the university…* (m4.2: 1301-8[cxlvii])

For the researchers, their academic institutions represented the absence of certainty. In their experience, instead of encouraging longer-term employment and plannable career paths and therefore more certainty, they tended to promote flexibility, mobility and discontinuity of careers and of research processes, thereby further adding moments of uncertainty. In line with these notions of absent certainty, researchers seemed to experience academic institutions as being unfamiliar with regular social security rights. The availability of those rights for

them was rather described as coming from their self-organised efforts. However their struggles with bureaucracy in doing so appeared to actualise uncertainties. The following quote by a postdoc who recently had a baby illustrates this:

> *…I noticed that when I was pregnant myself, that you are… from the personnel office… you get the impression that you are the first woman who has ever become pregnant (laughs)… they just don't know how it is supposed to work; they don't know the legal situation. So, it is really complicated in the end. It is very bureaucracy-intensive for something that should, could be so easy, right?* (f6.2: 861-7[cxlviii])

It could be argued that in a Western industrialised country, institutionalised ways of coping may be taken for granted to such an extent that they are not considered worth mentioning anymore. In order to find out about the motivations behind the researchers' actions, the "actual" existence of these institutionalised ways of coping is not as important as their subjective experiences. In this context, I would like to point out one particular aspect in their narrations about the absence of institutional support. Namely that it was linked to the fact that problems were viewed as individual rather than as systemic. One interviewee expressed this by saying that if he and his wife (who was a researcher too) did not ultimately fit into the system, it was their bad luck rather than a lack of institutional responsibility and adaptation:

> *I think they're doing it just right… I think it's ok. I'm sure it is, for scientific progress it's good to always have… new people* [come] *and that no long-term contracts* [are given] and only some are prolonged… *they have their reasons… That it doesn't fit for us now, in this case, with family, that's basically our problem. They cannot consider that… I think, they will set things right, yes.* (m1.1: 958-68[cxlix])

Similarly, many other researchers doubted that having the institution take responsibility for dealing with uncertainties was even an option, as this postdoc said very explicitly:

> *I'm not sure if a university, a state, can solve that* [problem], *or if not everyone has to find a solution for himself.* (f2: 1034-6[cl])

Considering the perceived absence of institutionalised ways of coping, together with the view that they mostly needed to deal with uncertainty individually, allows us to put researchers' ways of coping with uncertainty-experiences into a broader perspective. What options for acting *are* taken and can be taken tells us just as much as the options that *are not* taken or have come to be perceived as impossible to take.

To consider modes of coping it seems necessary to hold on to the observation that within contemporary work cultures in the academic life sciences institutional ways of coping appear to slip out of sight or to be de-coupled from researchers' experience of the everyday. As a result, ways of coping that are not institutionalised appeared to gain more relevance and the responsibility for dealing with

uncertainties tended to be shifted onto the individual researchers and their ability to organise their own ways of coping.

13.1.1. Researchers as Interfaces in Heterarchical Conditions

When we reconsider the range of the practices that I have described in the previous chapters – such as deconstruction and reconfiguration, the distribution of tacit knowledge, social negotiation processes or patchworking there is one aspect that many of them have in common: in different ways researchers seem to reconcile contradictions and dissonances in their epistemic living spaces and create within them a more continuous and coherent epistemic and social fabric of academic research. In a sense they take on the function of interfaces in its heterarchical conditions (cf. part three of this thesis).

One example that illustrates their role as interface is how they reconcile funding agencies' expectations about the ability to planning epistemic processes with their everyday experience of contingencies in the research process. The writing of grant proposals was for instance described as including the writing of timetables that are almost never expected to work out:

> *You just give any dummy-figure that you estimate on the basis of your experience. In fact, then you need to work and see, what really works out. There are some experiments that might work out in one week or that might just as easily take you half a year.* (f6.1: 978-81[cli])

As they explained, in order to get a project funded, they needed to be able to present their research as secure and doable. They thus described certain practices like the writing of timetables that can be defined as a negotiation between different rationales and expectations. In this context, researchers often told us that they were unable to point out the uncertainties of a project if they wanted to get it funded. Rather uncertainties must be hidden or removed as much as possible as a postdoc explained:

> *…funding institutions say that they want the project to be doable. And therefore they want a lot of preliminary work… they only fund projects that are more or less secure… If you don't have preliminary work but the idea is just really great you need to try and get funding from somewhere else, or within another project, for doing preparatory work…* (f6.2: 933-9[cliii])

By engaging in this kind of interface work, researchers individually provide the resources for dealing with these tensions and for producing a veneer of control that does not actually correspond to their experienced reality. As the quote above suggests, timetables are seen as signs of certainty rather than as actually providing certainty. Accounts of making uncertainties invisible are also given with regard to

publications. According to researchers, acknowledging uncertainties in a written paper would seriously compromise the credibility of research findings – and particularly so in a highly competitive environment. In these situations, they individually buffer the tension between the need to engage in detailed forecasting – to represent expectations of certainty – and an overall experience of contingency – a largely unrepresented expectation of uncertainty).

Dealing with such tensions is however described by researchers as relying on a very personal capacity for shouldering contradictions and dissonances in their epistemic living spaces. While practices like those described above produce the illusion of being in control of uncertainty and tension, these experiences do not disappear but are left for the individual to deal with. Acting like an interface thus does not only seem to be a hands-on practice but is taking place on the level of embodied skills as well. Another capacity is the development of certain character traits. A frequently mentioned example of what it takes to be a life science researcher is being a person who can tolerate frustration about experiments that do not work out the way they had been planned. As a postdoc explains, the uncertainty of life science research needs

> *(r)ather stoic people…* [you] *need this high tolerance to frustration… The others – who are not stoic – don't make it very far. Because they throw in the towel too often.* (f9: 1597-9[cliii])

The observation that researchers are positioned as interfaces within academic work cultures has implications for how the academic profession might be redefined within contemporary conditions. Besides academic qualification and research skills, an embodied skill of coping with uncertainties and enervating conditions with emotional housekeeping and character building is perceived as a core capacity for pursuing a career in the academic life sciences.

What this shows is that it is often not concrete practices but a feeling, an approach or motivation that is understood as crucial for career-making: one that is able to reconcile contradicting or dissonant expectations. One capacity that researchers implicitly describe is the capacity to switch between different modes of coping accordingly as opportunities emerge. In order to deal with uncertainties, ambiguities and tensions in their everyday young life science researchers seem to switch between the different modes of coping that I have discussed in chapter 12. Thereby they also switch between different ways of relating to themselves and in doing so are able to act as an interface between different forms of subjectification. They need to develop the capacity to relate to others in terms of a clan and as a collective at the same time as they conceive of themselves and act within their epistemic living spaces like managers and tricksters.

In summary, young researchers need to be able to transform heterarchical conditions into liveable living spaces and to be ready to (re-)conceptualise themselves according to changing requirements. By acting as interfaces they hold together what tends to be ever more divided in academic life science research: increasing expectations of certainty and the everyday experience of the uncertainty of epistemic processes. They do so for the sake of their individual careers while doing so might become increasingly important for continuous and coherent research cultures on a systemic level. Seen in this light it is paradoxical that it is more and more the temporarily employed young life science researchers that create continuity and coherency in research and that it is those who are at the highest individual risk who soothe uncertainties, ambiguities and tensions that are systemically produced.

13.2. Coping as Tacit Governance

I have explored in the chapter how young life science researchers sound out their degrees of freedom by acting upon experiences of uncertainties, ambiguities and tensions. In the following section I will perform one particular reading of these ways of coping: namely as ways in which researchers make use of, enlarge and fill the spaces of negotiation that policy measures leave in the everyday of academic research. This perspective will allow for reflections on how young researchers are shaping their research cultures. In order to do so I explore their ways of coping more conceptually. Therewith I aim at contributing to understanding how an overall transformation of research cultures is mediated by a tacit governance from below (Felt/Fochler 2011).

When I conceptualise young researchers as interfaces, I point at the way researchers can be seen as contributing to the workings of academic research. They mediate between heterarchical environmental conditions and what they feel is necessary for the actual research process. In doing so, they also find ways of using policies differently than originally intended: they find ways of sidelining criteria, of working around or avoiding policies and of carving out or reclaiming spaces of freedom. Researchers thus go beyond maintaining the status quo and act upon and sometimes remain ignorant of these environmental conditions. In doing so they appear as actively taking part in shaping their research cultures. The way they do so can be described in terms of „articulation“ as Stephenson/Papadopoulos have defined it:

> In fact, articulation is exactly that: the rearrangement of an order of practices and signs from which new orders occur. (Stephenson/Papadopoulos 2006: 31)

These processes are often largely invisible to and unrecognised by formal, visible – or "represented" – forms of research organisation (such as the project, performance criteria). Rather they seem to rely on informal, imperceptible, "unrepresented" activities. Michel de Certeau has distinguished such unrepresented "tactics" from represented "strategy":

> I call 'strategy' the calculus of force-relationships which becomes possible when a subject of will and power (a proprietor, an enterprise, a city, a scientific institution) can be isolated from an 'environment'. A strategy assumes a place that can be circumscribed as proper (propre) and thus serve as the basis for generating relations with an exterior distinct from it (competitors, adversaries, 'clienteles', 'targets', or 'objects' of research). Political, economic, and scientific rationality has been constructed on this strategic model.
>
> I call a 'tactic', on the other hand, a calculus which cannot count on a 'proper' (a spatial or institutional localization), nor thus on a borderline distinguishing the other as a visible totality. The place of a tactic belongs to the other. A tactic insinuates itself to the other's place, fragmentarily, without taking it over in its entirety, without being able to keep it at a distance. It has at its disposal no base where it can capitalize on its advantages, prepare its expansions, and secure independence with respect to circumstances... Whatever it wins, it does not keep. It must constantly manipulate events in order to turn them into 'opportunities'. (de Certeau 1984: xviiif)

In these terms, young researchers' ways of coping with anxiety belong to something "other" – rather than to the "proper": to those spaces that are not properly circumscribed and represented within their institutional environment. As de Certeau further discusses, the "other", the unrepresented, is anything but passive. Tactics are an art of the weak, a form of practice that involves trickery and is often the only possibility, the last resort (de Certeau 1984: xxii, 37). Young researchers tend to describe themselves in such a weak position and mainly seem to draw on tactics that – although lacking "proper" power – appear as potentially powerful in creating a new, less contradictive and dissonant environment. In de Certeau's words, their "tactics introduce a Brownian movement into the system" – random action that is indeterminate, that cannot be foreseen or described in a proper way (de Certeau 1984: xx). What basically distinguishes strategies from tactics is that in carving out the interstitial spaces of tactical acting and by creating the "other", they indeed often refer to "the proper" but not all their actions can be traced back to it:

> Although they remain dependent upon the possibilities offered by circumstances, these traverse tactics do not obey the law of the place, for they are not defined or identified by it. (de Certeau 1984: 29)

This conceptualisation of researchers' ways of perceiving their practices does justice to interview and observation data; it allows for the perspective that researchers'

everyday practice and their participation in shaping research practices and subjectivities indeed refer to circumstance but at the same time reach beyond it. In that they seemed to conceive of their practices as partly following their *Eigensinn*, neither their practices nor the ways in which they were subjectified were seen as determined by the environmental conditions of academic life science research. Within these conditions, researchers saw gaps where they could decide whether and how far they could make use of spaces of freedom and negotiation and whether and how they could choose between ways of relating to themselves and to others. In doing so, their tactics seemed to be never fully foreseeable and detectable. In that, they pointed at something beyond the "proper", a surplus of sociability or *Eigensinn* that neither originates from circumstance, nor is it fully able to be integrated.

However, even if the trajectory of their practices seemed to remain indeterminate and often imperceptible, researchers' narrations suggested that we can identify certain levels on which their tactics might be introducing de Certeau's Brownian movement. In the following I will identify the moments in researchers' narrations that address the epistemic and the social aspects of research processes.

13.2.1. Epistemic Dynamics – Risky vs. Secure Research

Many of the ways in which researchers deal with uncertainties are motivated by the desire to prevent epistemic uncertainties from becoming an individual risk to their employment and career futures. Some researchers were very determined that the degree to which they allowed uncertainties to play out was partly in their hands. Asked about his ways of managing risk and whether he preferred secure projects at the moment, a postdoc answered in very definite terms:

> *Right now, definitely yes! Because the future is so insecure…* (m4.2: 290-3[cliv])

He then clearly linked his willingness to allow for uncertainties to structural preconditions – and in particular to his future employment opportunities:

> *…the vice-dean asked himself: why should I employ people permanently when their boss is thinking about going abroad. That was the problem and that's the reason why people, why I have inhibitions regarding longer-term, prospective projects at the department. Because it's not clear how it will go on.* (m4.2: 410-5[clv])

Quotes like these suggest that the contemporary environmental conditions of doing academic life science research introduce levels of uncertainty about the future that tend to prevent researchers from approaching the large-scale – but often more risky – research questions in their field. Many quotes insinuate that contemporary conditions limit the freedom to do the really innovative and groundbreaking research which young researchers had expected from an academic environment, as is traceable in the way that the same postdoc continues his statement:

> *In general I think that… working conditions – in particular at university where there should be room for basic research – it should be the case that you can have the courage to do risk-intensive projects. Because it is often these breakthroughs that are needed in research… and when you are forced by your working environment to pick the… low-hanging fruits (laughs) – those fruits that are easiest to pick… these are then the most evident and scientifically maybe not so interesting or relevant.* (m4.2: 275-83[clvi])

However, while most researchers would contend that they mostly did secure research, it was implicit in their narrations that they always tried to fit in uncertain and risky parts as well. Like the narrations above suggest it was often the most uncertain – and potentially risky parts of – research that were regarded as the real purpose of academic research and the basic motivation for engaging in it. This aspect appears to be of particular importance when it comes to why and how individual researchers actually choose their research questions. As a researcher told us, with uncertain research *"(y)ou try something new, and you find – in our case – new biological interrelations, explanations. That's why we do all that*" (f6.2: 557-8[clvii]). Researchers thus were able to overlook risk potential if there was a potential for doing interesting research. A PhD student told us that she

> *…mainly did theory… this was the most uncertain topic of my PhD but also the most interesting.* (f4.2: 117-21[clviii])

The observation of a lab leader supports this point. Despite her expectation to the contrary, she says that in her experience her lab members actually preferred risky projects:

> *Everyone has his/her own gambling strategy. Interestingly… recently I have noticed that my people tend to take the risk – so that they have a chance to get a top-rated publication… They don't want to do small things… you would think that he would rather… prefer to feel safe… (.) That's indeed astonishing, that has changed in my lab, that they take greater risks.* (LCQprof_m4: 969-76[clix])

It seemed though that despite frequent contentions that research conditions were inhibiting uncertain research questions many researchers nonetheless tried to partly pick the more uncertain higher hanging fruits. In doing so, they seemed to on the one hand renew their motivation and on the other hand also maintain the hope of producing outputs of high value and impact. These research questions were thus regarded not only as having high potential of failure but also as having the potential to make their careers. It seems that the epistemic strategy that researchers chose significantly depended on the individual's preferences, motivation and attitude towards his/her ambitions within academia and on whether or not s/he was willing to take the risk.

13.2.2. Social Dynamics – Working Together vs. Working Alone

Another level at which conditions of uncertainty-experiences seemed to introduce a Brownian movement into life science research cultures is the social dynamics of labs. It is common for young researchers in their labs to engage in both social and collaborative activities and try to create a meaningful epistemic biography as well as an individual career. A PhD student explains this by emphasising the importance that the "scientific environment" has for good research:

> *…the team is always important for me… it doesn't depend only on yourself but to large extents on your scientific environment, right? Really to large extents! You can be a brilliant mind, but one who can't do certain things. Then you stay a brilliant mind in your… area, but you will not have the opportunity to realise it. Second, it is also that things like discussions inspire you to do certain things… That's… tremendously important.* (f4.1: 729-33[clx])

To borrow a term that Heidi Grasswick has coined, we can say that epistemic subjects in life science research are "individuals-in-communities". This concept emphasises that epistemic processes can only be understood if we look at "the relations between knowers and their communities" (Grasswick 2004: 85ff). When Karin Knorr-Cetina thus speaks of an "impossibility" of collaboration in research fields like the life sciences, this is best understood as a metaphor for individualising career models and not as an epistemic feature of such lab sciences (see Knorr-Cetina 1999: 234ff). Quotes like the above suggest the contrary: that life research does not allow researchers to abandon or escape collaboration but requires acting within a complex net of exchange relationships. Inside and outside of these collaborations however – in order to be able to continue their research in the future – they need to develop and pursue an individual research question and thereby build an individual career. Especially considering the epistemic level, we can say that from researchers' perspective it is impossible *not* to collaborate.

For researchers' everyday that means that they not only need to learn how to oscillate between working together and working alone – but also how to be able to simultaneously work together and alone. As I have tried to show in the previous chapter, we can observe the simultaneity of togetherness and aloneness in researchers' ways of coping with uncertainty-experiences as well. In fact, in that young researchers live and work at the crossroads of working together and carving out an individual career, they appear to live and work at the crossroads of the four orientations of subjectivity that I have described: the socially embedded clan and collective subjectifications and the individual orientations of the managerial and trickster subjectifications. However, the position from which researchers juggle these subjectifications appears to differ strongly between the individual researchers. Their narrations suggest that the position of each individual in lab constellations

always remains specific within a progressing life science career. What these relations between researchers look like seems to mostly depend on the size and structure of the lab as well as on its funding. A postdoc described the complex structure of her relatively big lab as follows:

> *…in the wet lab… there is a technician… She is always there and always knows where you can find what. There is…* [this] *postdoc, she is in the lab a lot of time and knows a lot. She is organising a lot in the lab. So, she is the contact person if you want to do anything in the lab. The third is* [another postdoc], *who used to produce proteins for me a lot, but is working in the* [other part of the lab] *as well. So, she is a bit of a bridge… there are people who are doing more* [this one part of our research] *and… there are people who work more directly with…* [the lab leader], *and then there are a couple of people who are… rather with* [the senior postdoc]*… And then there's us, we are the group interested in* [the other part of our research]*… and then there's* [this PhD student], *that is, she is often with* [us]. *That is a bit shared. Then there's* [one]*… who does his Master thesis… Then there's that PhD who is also with* [us]. (f1.1: 328-49[clxi])

The quote could be extended; she subsequently names a few other Master students, PhDs and postdocs. What the quote shows is the unique position that young researchers tend to have in labs. While some seem to be specialised in one area, others are rather described as being in a bridging position, some are described as working in close relation to fellow researchers while others are described as working alone. Whether a social or an individual orientation comes to the fore seems to be open to individual negotiation and struggle in the everyday. Supervision conditions, opportunities for getting feedback and the constitution of uncertainties seem to depend on how researchers learn to manoeuvre within these complex structures and on how they decide to build up and maintain their working and coping relationships. Particularly in bigger labs the self-organisation of coping relationships is described as important. Because lab personnel are frequently changing, feedback structures and risk collectives are described as periodically ruptured and individually (re-)arranged.

Their narrations however also suggest that ways of working together can come into conflict with individual career orientations. In that individual ways of coping are eventually prioritised in order to build a career, collective arrangements can create tensions. One PhD student for instance told us that the two postdocs that she had previously collaborated with were focusing increasingly on their own projects and individual careers. This in turn made their tacit knowledge and skills more unavailable to her and other younger colleagues:

> *I also think, that* [these two postdocs] *really want to pass on their experiences. But at the moment… their kids are very demanding. Both have to look after their own projects, these*

people are 37 already and do have… [funds] *now. But what will they do afterwards?* (FGk_jun: 2670-4[clxii])

This quote suggests that moments of collectivity and of sharing and commonalising resources tend to get downgraded in priority when career pressure increases. When the more experienced lab staff is too deeply drawn into career work, they have less time and resources to spend on the collective forms of coping with uncertainties in everyday lab work. In the worst case, this dynamic seems to be able to rupture the working relations within labs to a point where "r*eal, actual, proper collaboration – that I am deeply convinced of – real collaboration, within our group, you will not find it*" (FGk_jun: 576-85[clxiii]). Felt et al. in a forthcoming paper use the phrase "together alone" to describe this situation. It expresses both the necessity of working together and of working alone as well as a tension that often develops between them. In their narratives, researchers engage in both forms of working and see them as always coexisting and in need of reconciliation. In order to advance in the field, researchers discuss both the necessity for engaging in different forms of working together and the capacity to finally be able to reconfigure themselves as individual researchers. The phrase "together alone" however at the same time points to a clear trend that is traceable the subtext of many narrations: namely the trend towards ever more individualisation to the detriment of the social aspects of life science research (Felt et al. forthcoming). As the following quote illustrates, rationales of individual career-making are perceived as getting the upper hand in life science research cultures:

> *Exactly. Because everyone* [wants to] *proceed in his field… the bottom line is, that everyone is first and foremost interested in what he individually does… Because you want to achieve something.* (LCQm6: 504-10[clxiv])

What this rather young PhD student displays here is clearly an individualistic and career-oriented orientation. In his interview he rarely referred to the collective aspects of his research but tended to highlight his personal achievements and his future career prospects. As it later turned out, this orientation had been the springboard to a successful career. Five years after this interview – and after a stay abroad – he already held a lab leader position.

What is implicit in such career narrations is a potential shift in the modes of coping that researchers engage in. Within the experience of a permanent lack of time, researchers have to learn to choose carefully where to invest their resources. Dynamics of a generally competitive environment might subtly prevent them from establishing continuous and stable ways of working together and lead to a privatisation of researchers' skills. The emphasis academic career might suggest to young researchers that it is wise to focus on other than collective ways of coping,

since they learn that the clan mode of coping is more immediately necessary for securing their next employment.

Both, the epistemic and the social dynamics that I have reflected on here do not appear to be (directly) caused by individual policy measures or intended by research policy. Rather they appear as the consequence of distortions and fractures that different – and partly uncoordinated – governing measures induce in academic life. In the sense that they are not directly relatable to those measures but are nonetheless a relevant force in shaping research cultures, these dynamics can be understood in terms of tacit governance. What I have tried to work out in this reflection is that tacit governance in life science research cultures is partly mediated by researchers' capacity to act as an interface and by their capacity to understand themselves in a particular way (to subjectify themselves).

13.3. Coping within the Predicament of Resistance

When researchers talked about coping with uncertainty-experiences there was often a sense of cheating regulations or of breaking the rules. In moments when they perceived their environment as contradicting the needs of their research they were proud to sideline expectations and requirements that they found unreasonable and to re-order their epistemic living spaces in order to make them more liveable. In the following I will trace these notions of resistance against the background of theoretical work on resistance and subversion. In my reflections on researchers' narrations I will refer to theoretical debates – for instance by Luc Boltanski and Éve Chiapello's "The New Spirit of Capitalism" ([1999] 2006) and Papadopoulos et al.'s (2008) "Escape Routes: Control and Subversion in the 21st Century" – that have claimed that neoliberal forms of governance and of transformation are in fact not actually subverted by deviant or resistance practices and mindsets but deeply rely on them. I will thus reflect on the ambivalences in researchers' narrations about cheating regulations and breaking the rules which will lead me to reflecting on whether and how far researchers manage to escape the "predicament of resistance" – i.e. whether and how researchers' ways of coping are being co-opted by neoliberal transformation processes of academic research cultures. Speaking with Foucault, the guiding question is: What does their "art of not being governed quite so much" look like (Foucault 1978: 45)? By asking this question, I aim at getting a better grasp on the ways in which researchers can contribute to the transformation of life science research cultures today. Since previous work on resistance in academic working places builds on slightly different initial questions, I will very briefly review some of their conclusions and then situate my reflections within the debate on the

predicament – that is the partly contradictive and ambivalent state – that resistance seems to assume within neoliberal forms of governance and transformation.

While in the 1990s it was repeatedly noted in literature on work organisation, that "the topic of worker resistance has been given a very limited role in our theoretical models of the workplace" (Hodson 1995: 79), recent academic debates have shown growing interest in how people resist changing organisational parameters – in the academic workplace amongst others. One line of this discussion can be followed within critical management studies work that has tried to understand resistance to – what they call – a dispositive of managerialism. In this context, managerialism is understood as the attempt to "measure, monitor and control" (Barry et al. 2001: 91) or "as a power/knowledge discourse enacted through a series of localised mechanisms, techniques, and practices within the workplace" (cf. Anderson 2008). What they describe as part of this dispositive partly coincides with what I have identified as being conditions of uncertainty-experiences. It has also been argued that such measures can generate certain reactions in the work cultures. In 2001, Barry et al. have noted that investigations about the „impact of managerialism and the reactions it has engendered in university life" have shown that „resistance to domination and control has been underplayed" (Barry et al. 2001: 87). Building on their study of „junior- to middle-ranking academics and administrators in two UK universities" they contend that academics „*resist* the imposition of control in various ways" and that therefore "matters are not settled":

> In reacting to processes of managerial change our interviewees have sometimes accommodated, for example to peer review, ignored or circumvented pressures to increase workload and act in autocratic ways, and (re)negotiated, mediated and moderated the harsher effects of the recent changes. (Ibid. 98f)

What Barry et al. describe here is that researchers are in an intermediary position in which they can negotiate, mediate and moderate – and thereby alter the effects – of certain measures. In the terminology that I have developed in this chapter we could say that in their position as an interface between different conditions and requirements they seem to articulate conditions that they perceive as unreasonable into a more liveable environment. More recent debates have attempted to „theorise the micro-politics of resistance" in academic institutions (Thomas/Davies 2005: 683). Robyn Thomas and Annette Davies have made the case for "widening… both the scope and the level of the definition of resistance to produce empirical accounts that focus on the more subtle and contextualised aspects of resistance". They suggest a new and broader understanding of resistance that goes beyond "resistance behaviours" and includes resistance on the level of "meanings and identities" (2005: 686f). Another relatively recent point that can broaden our perspective on resistance is that resistance is increasingly not understood as obstructing change but as actively shaping – and transforming – the living spaces of those who engage

in it.[29] Papadopoulos et al. have suggested picturing resistance as an active stimulus of transformation processes – one that can even end up reinforcing that which it was originally intended to resist. This is what they call the "predicament of resistance":

> …resistance becomes just another structural element contributing to the erection of postliberal aggregates. We already know that the very conditions for resistance are always directly entangled in power. But such entanglement… does not necessarily block the development of effective strategies of subversion. Of course, sovereignty digests resistance: active forms of resistance are continually co-opted. But this twin movement of flight and capture only appears catastrophic if we insist that there must be an ultimate solution to social conflicts. We do not. (Papadopoulos et al. 2008: 74f)

In their understanding, resistance appears as increasingly compromised – or co-opted – by its entanglement in neoliberal forms of governance (Ibid: 71). They contrast such forms of deviance with what they understand as "subversion" – a form of deviance that escapes the predicament of resistance and starts creating something new:

> Some may want to use the word resistance instead. But here we understand subversion (or resistance if you prefer) in a positive way: as the desire to depart from the plenitude, which organises control in a certain field. Or better, as the trust in something, which is absent and unrepresentable, and yet operative and constitutive of a specific field. This desire comes from the very heart of the situation, but leads directly and unconditionally beyond it. (Ibid: 2008: 81)

With this reflection they open up spaces to think deviance as contradictive and ambivalent and therefore, as a relevant motor for societal transformation. Since the life science researchers that have been interviewed for this study presented their ways of coping as very active forms of constructing their environments, the following discussion will make use of the terminological differentiation between resistance and subversion. I will understand *resistance* as taking part in a neoliberal transformation process while *subversion* as taking part in a transformation process that goes beyond it. This discussion will allow for picturing ways of coping on the one hand as ways of relieving uncertainty-experiences but will also open up a perspective on coping that sheds light on how certain ways of coping might contribute to dynamics that can in fact reinforce uncertainty-experiences.

I will start by discussing some of the coping practices that I have described in chapter 12 as ways of being deviant to the temporal, epistemic and social structures of the project mode of funding. Practices like not following formal timetables, of

[29] Previous studies have discussed academics' practices often as preventing change in that they adhere to "oppositional" academic norms (Anderson 2008), or "a scholarly craft-ethic" (Barry et al. 2001: 98).

deconstructing project teams and agendas as well as their reconfiguration into different structures can be interpreted as practices that resist a given project structure. In everyday research practices this becomes visible in the fact that, for example, timetables of projects seemed to be quite poor at actually structuring what researchers did. Rather, they were described as important for communicating with funding institutions. In researchers' narrations it sometimes seemed that as soon as they had acquired the funding, they felt free to continue any kind of research that they were eager to follow up on. They tended to narrate such practices in terms of creating a different environment that avoided and resisted undesirable conditions. By patchworking different structures together, and at times cheat them and break their rules, they narrated their practices as enlarging the spaces in which they could work autonomously. Borrowing a concept from André Gorz helps us understand how they considered these to be practices of resistance. He distinguishes autonomous activity from heteronomous activity that is

> …made up of socially predetermined and relatively impersonal tasks… which individuals have to accomplish as functions co-ordinated from outside by a pre-established organization. The nature of the tasks is determined as such that individuals function as cogs in a big machinery. (Gorz 1987: 102)

In his understanding, autonomous activities are also activities „in which the individual is the sovereign author of actions carried out without recourse to necessity, alibis or excuses" (Gorz 1987: 93). He defines them as

> *activities, which are themselves their own end… They are valued for and in themselves not because they have no other objective than the satisfaction and pleasure they procure, but because the action which achieves the goal is as much a source of satisfaction as the achievement of the goal itself: the end is reflected in the means and vice versa.* (Gorz 1989: 165, German original)

In short, he defines autonomous work as being personally significant and heteronomous work as not personally significant. This distinction corresponds to a distinction that researchers tended to draw themselves when they talked about two different parts of their work: the work they had to do in order to survive in academia (research that most probably will lead to valuable results, i.e. "secure research") and the kind of work that they did because they found it interesting and exciting (such as risky research with less potential to produce valuable results). The following quote expresses the motivation for doing such autonomous work even if – in order to do so – it seemed necessary to "wrap it up" artificially and to be deviant to the perceived expectations of a funding institution:

> *…still, I have the feeling that in academia you can much rather (.) do research on interesting stuff. You need to wrap it up in the grant, so that it sounds like it is directly relevant in clinical terms… Of course you need to sell it skilfully.* (m4.1: 644-53[clxv])

As this quote illustrates, one leitmotif of deviance lay in the personal motivation for doing their research – i.e. in the subjectified character of living and working in life science research. At the same time however, there are plenty of accounts that assert that in order to survive in academia it is necessary to deviate and to know when to break the rules. It is treated as an open secret that certain regulations *can* be and *are* avoided, bypassed or even *need* to be bypassed or broken in order to be able to continue smooth research processes. One senior researcher for example told us that in order to secure a necessary degree of collaboration, he needed to neglect certain requirements. In the following quote he speaks about ignoring certain co-authorship regulations:

> *…the determination of co-authors, that is certainly something that I do, of course in considering other opinions… I prefer to include more people. Because I say to myself: It is important for me that the cooperation works well. I mean, there are these criteria for co-authorship – right – and I have to say honestly, they are so strict that you'd have to exclude a lot of people. But, then I will certainly have no one in the future who wants to collaborate with me… So, I have to say honestly, we probably often break these rules, but otherwise we couldn't do our research any more…* (prof_m1: 1716-27[clxvi])

Besides such accounts from senior researchers it is particularly narrations of the younger generation of researchers that suggest that life science research within the project mode of funding is only made possible because of the ability of researchers to work around formal regulations in everyday research. The practice of deconstructing and reconfiguring project structures for example was quite widely accepted not only because it was regarded as increasing the degrees of freedom for their research. It was seen as "part of the game" (f6.1: 1037-9[clxvii]) and unavoidable. What they actually spent their time on was often not congruent with what the project outlines had circumscribed. As often contended in researchers' narrations, work on projects does not start when the project starts and does not end when the project ends. Within project-based environments, a postdoc says, preliminary work is always expected. When you start a project, he said:

> *You have to have gotten started already.* (m1.2: 925[clxviii])

Considering the time that a grant needs in order to get reviewed and accepted, this means that project funding introduces a temporal structure that goes way beyond the timeframe of the formal project itself. Researchers sometimes needed to start working on preliminary results for the next project a year prior to handing in the grant, which means that the time resources of a former project or private time must be invested. Researchers mentioned that a project of three years in fact often only leaves one and a half or two years to work on the actual question of the project. The remaining time is then used to prepare for the next: e.g. for producing preliminary results for the next project and write several grants to secure successive funding. In

that sense the project framework seems to somehow structurally resist itself in that it builds on researchers' ability to deviate from the formal timeframe of projects.

13.3.1. The Freedom to Deviate as a Regulatory Ideal

Seen in its narrative context thus, what researchers experience as resistance is often quite ambivalent: the predicament lies in that they made sense of such practices as necessary for the systemic workings of good and continuous research in their labs as well as for renewing or recreating their motivation for doing research. From this perspective creating new environments appears as a way of neoliberal change co-opting researchers' capacities of creating new environments that facilitate good research and high motivation even if conditions are often experienced as contradicting both. Notions of deviance in life science research cultures thus are not without ruptures. In that they are narrated as deeply inscribed in the workings of life science research today – like in this quote – deviance is unavoidable for survival in academia:

> *…you need to take the risk and hope that it will not be evaluated that badly… even if you have done something completely different. (laughing) What we usually do. At least in our case – because it is not predictable what you will be doing – you have to deviate from the very detailed project planning that you present to the committee.* (f1.2: 319-30[clxix])

The interesting aspect in this quote is that she deviates from project planning not because she *wants to* but because she *has to*. She does so because the assumption of certainty that is inscribed into detailed project planning contradicts the inherent uncertainties of the research process. The example of a PhD student who failed because she *refused to deviate* is another example of deviance being essential for success. Since she was convinced of her originally defined idea, she wanted to stick with what she had written in the grant proposal for her scholarship and did not take the advice of her colleagues to just do „something else":

> *…do something else – I heard all the time. But I thought… I have a defined project, I cannot do something else. And in the first place, that's what I am interested in – why should I do something else?* (f4.2: 217ff[clxx])

From that perspective deviance can be pictured as both a way of cheating the conditions of uncertainty-experiences in life science research cultures and at the same time reproducing them and optimising them. This paradox seems to be nourished by the researchers' self-image of being a disobedient subject, of being a subject that still has the freedom to deviate in academia – a self-image that is often linked to very high self-motivation and the willingness to personally invest in one's work.

There is however one aspect in researchers' narrations that suggests the governing character of this kind of freedom: namely that accounts of freedom were often discursively linked to the limits of their freedom:

> *We have a boss who, who is completely different, right?…* [our lab leader] *he has a lot, a lot of influence because… he gives complete freedom. Basically, right? Except for moments in which he gives you shit…* (FGk_jun: 2395-9)[clxxi]

While this PhD student emphasises the experience of freedom in her lab, she at the same time links the degree of freedom and self-organisation to the degree of tolerance that her lab leader has. In that this kind of freedom is not unconditional but granted under certain preconditions, it appears to be a tool that influences the dynamics of the lab. Such quotes subtly imply that freedom itself has a regulatory function. While it allows for self-organisation and self-motivation it can at the same time be withdrawn very quickly. Or in other words: in that deviation comes at the cost of an anxiety over whether they were deviating too much, it leaves them having to take the risk and hope.

The freedom to deviate thus appears in an ambivalent light. It is experienced as liberating, as necessary and as threatening at the same time. In this scenario researchers are, in effect, chasing their own tails as is for example illustrated in the way that researchers employ mixed-risk strategies. Because highly uncertain research eventually becomes too high a risk for the individual, they tend to do risky and safe research at the same time. Most accounts suggest that they do safe research as a bread-job – or cash-cow as one researcher put it – while they simultaneously do risky research as a luxury besides. They appear to do so in part in order to renew their motivation and to cultivate their creativity but also in the subtle hope that by risking a lot they will achieve the really innovative and outstanding results that will make their career. As a result it appears that most researchers put effort into both deviation and adaptation. This paradoxical situation has the potential to be a powerful mechanism for maintaining the vicious cycle of anxiety. The way researchers narrated it, the freedom to deviate and to do autonomous work tends to require ever more managerial work. The anxiety over possibly deviating too much thus tends to increase researchers' investment of time. As regulatory ideal, this kind of freedom to deviate seems to fuel a circle of acceleration, of self-intensifying workloads and of voluntary overexertion. In this light, freedom and self-responsibility appear to have the potential to flow into a self-governance that makes possible what is seen as being structurally prevented: risky, innovative and interesting research.

Such processes can place researchers in the position of self-subjugation in which they are governed instead of liberated by a regulatory ideal of freedom; an ideal that as "a fiction… operates within discourses and which, discursively and institutionally

sustained, wields enormous power" (Nicholson 1990: 335; cf. Bröckling 2007: 78ff, Greco 2000: 265). What I mean by that is that these subjects perceive of themselves as free, and indeed are free in the sense that an ideal of freedom guides them in manouvring their research landscapes – making their anticipations, their guilt and their restlessness appear as self-imposed.

Further reflections on subjectified work cultures in academia might thus have to consider that "sophisticated disciplinary technologies work to ensure that management discourses colonise worker subjectivities, such that they participate in their own subjugation, effectively removing worker opposition" (Thomas/Davies 2005: 686). Rather than dissolving contradictions and dissonances, increasing structural continuity or de-intensifying labour, they can appear to be reproducing and strengthening them. In that sense "[w]orker's autonomy", and researchers' efforts to create spaces of freedom might in part have lost their subversive character and been transformed into an instrument of rationalisation (cf. Bröckling 2000: 142). The kind of freedom that they enjoy seems to allow for accepting certain conditions that are in their own perception unfavourable. By developing subjectivities that conceive of themselves as free and self-responsible and that have the capacity to be an interface between contradicting and dissonant conditions researchers assume the function both of a buffer zone and of a glue within the heterarchical conditions of academic life science research.

This strengthens the argument of the previous subchapter 13.2 that for understanding transformation in these terms, the construction of subjectivities – and the modes of its constitution, creation and change – need to be taken into account. The formation of subjectivity is neither exclusively a process of subjugation nor subversion of neoliberal transformation. Rather it seems that

> …neoliberal power establishes a social order not primarily through liquidating otherness, inferiority or subjectivity, but by fabricating and regulating otherness and subalternity through the multiplication and assimilation of subjectivities that are created by one's own reflexivity of one's own positionality… subjectivity could be never understood for itself, rather it is always part of an assemblage in which it is used and in which it gains its craft to forge efficacious propositions. (Blackman et al. 2008: 14f)

Subjectivity therefore can be regarded as just another playground for the predicament of resistance and co-optation.

13.3.2. Escaping the Predicament?

Against this background we might have to reconsider the meaning that deviance and notions of resistance can have in the social worlds of life science research. When they become necessary for the machineries of knowledge production to

function smoothly, it appears as almost impossible to identify actual escape routes from uncertainty-experiences. This can at least be said for those young researchers who aspire to an academic career. Somewhat paradoxically it seems that for young researchers the least anxious and uncertainty-intensive way of living and working in academic life sciences is to consider career alternatives and refuse to take responsibility for a potential academic career future – i.e. when they consider or plan to leave the academic life sciences.

Against the background of theoretical debates on resistance and subversion in neoliberal governance, the coping tactics that researchers narrate as resistance appear as an ambivalent enterprise in that they on the one hand carve out more spaces for autonomous work but at the same time might functionally correspond to the framework that produces uncertainty-experiences; they are thus subversive and conservative at the same time. Even under temporal structures of funding and employment that are experienced as structurally closing down the opportunity for risky and innovative research, for example, researchers carve out spaces that allow for it to happen even when it is described as easily turning into individual career risk. From researchers' perspective it makes epistemic living spaces more liveable. This leaves us in the end with an impossibility to judge to what extent researchers' practices are co-opted. On the one hand we can read their ways of coping as a form of creatively contributing to shaping their epistemic living spaces and enlarge their spaces of autonomy. On the other hand however we might have to consider reading them as a way of allowing academic cultures to access their ability to assume and switch between different modes of coping and subjectification as well as their tacit managerial skills, trickster capacities and willingness to invest more and more time in order to guarantee what researchers themselves appear to increasingly experience as structurally compromised: the smooth working of academic life science research.

14. Discussion and Conclusions

In this thesis I have studied the experiences of uncertainty that young researchers face in contemporary academic life science research cultures in Austria. Building on qualitative empirical material such as interviews and group discussions, I have explored how they are expressed in the everyday epistemic living spaces of young academic life science researchers. In part two I discussed five conditions that researchers often identified as structural preconditions for their uncertainty-experiences (epistemic uncertainties, subjectified work, academic career scripts, casualisation and commodification). Then in part three I sketched the contradictions and dissonances that these structural preconditions create in the everyday and argued that they create a cycle of anticipation, guilt and restlessness that tends to make uncertainty-experiences a pervasive feature of their research cultures. The overall argument that I have made is that – when thought together – these conditions can create a generalised experience of uncertainty that cannot be traced to a single cause but that permeates all aspects of life. Amongst others this results in an experience of epistemic uncertainties as personal risk in the form of career risk. Since for many academic researchers, an academic career is linked to a certain conception of their life, this career risk is likely to create existential fear of losing not just their job, but their way of life. In part four I analysed researchers' ways of dealing with and relieving uncertainties and, in following up on this analysis, I reflected on the degrees of freedom that researchers find and create during this ongoing coping process.

In this concluding chapter I will develop a framework for understanding young researchers' ways of experiencing and coping with uncertainties: the concept of embodied anxiety. I will define is as a form of precarity that builds on and creates certain tacit knowledge and skills for coping with uncertainties. I will argue that in doing so, embodied anxiety might not only be understood as a state of experience but as a way in which young researchers learn to live and work in the academic life sciences that comes to dominate the way in which they learn to approach and appropriate their epistemic living spaces. Building on the previous analyses and reflections, I will then discuss, what implications embodied anxiety might have for academic life science research cultures in particular and on a broader societal level. In doing so, I link the analyses of this thesis back to the two currents of transformation within which I located young researchers' uncertainty-experiences in the first part of this thesis: first, the changing role of knowledge production in our societies – namely its growing importance as an economic sector; and second, a general trend of increased uncertainty-experiences and the challenges that it poses for how social security is and can be created. Each of these sub-conclusion thus builds on a particular way of reading my study. The first reads researchers' ways of living with uncertainty as coping with expanding capitalist dynamics and the second

reads it as a study about latent social conflicts within conditions of precarity. Both readings aim at contributing to a better understanding of the tacit governance of contemporary life science research cultures.

14.1. The Concept of Embodied Anxiety

As explored in part four, researchers develop ways of coping with uncertainty-experiences that build on tactics and personal capacities such as the ability to estimate and balance risks, to tolerate frustration, to be ready to communicate and network and – overall – to create a new epistemic and social environment out of a set of given circumstances. Living and working with generalised uncertainty-experiences thus gives rise to the kinds of tacit knowledge and tacit skills required to handle research processes and to live a life within research cultures. In the following I will make use of Michael Polanyi's concept of tacit knowledge in order to gain a better understanding of the meaning that uncertainties can assume in the current transformation of academic life science research cultures. For Polanyi, tacit knowledge emphasises the perception that for certain knowledge-making practices, "people know more than they can tell" (1985: 14, German original). He uses the concept particularly to discuss work-related practical knowledge, a kind of intuition about what and what not to do and when to do it. It is acquired not through verbal communication but by experience. Even though people might not be able to define it, speak about it and write it down, they carry it with them as a set of abilities that allows them to take action in certain ways. It is a kind of knowledge that is not shared explicitly but informally and implicitly conveyed through ways of working together and observing others (Ibid 1985: 16). Understanding young researchers' ways of coping with uncertainty-experiences allows us to better grasp the embodied character of tacit knowledge in contemporary academic life science research. In that certain practices and ways of handling things are internalised, they travel with the bodies of the researchers (1985: 24f). In this way, the knowledge and skills required for coping literally get under the skin. Tacit as they are, they are pushed into the margins of conscious practice, somehow forgotten as they become part of who life science researchers are. It is in that sense that I therefore conceptualise the particular way in which uncertainties are experienced in academic life science research as embodied anxiety – a new form of being in academia that entails a new set of knowledge and skills that the younger generation of life science researchers needs to master in order to make an academic career and that also introduces a new tacit dimension in the epistemic and social dynamics of life science research.

In doing so, embodied anxiety assumes a meaning beyond discomfort and unease; it takes on a performative role and becomes a shaping force in academic work cultures. Speaking of uncertainty-experiences in this context thus means speaking

about a form of precarity that has governance effects in epistemic living spaces. When they relieve experiences of uncertainty, researchers not only create a more liveable environment but catalyse certain dynamics within their epistemic living spaces. I have suggested that figuring researchers as interfaces who adapt to different conditions by deconstructing and reconfiguring their given environment, through risk management or by making resources common provides a site for analysing the degrees of freedom that researchers find and create in shaping their research cultures. How researchers decide to do so appears to play a crucial role in how they occupy the "spaces of negotiation and possibility" that coexisting and partly uncoordinated logics of governance create (Felt/Fochler 2010: 298, German original). Their ways of contributing to the transformation of the everyday workings of life science research thus seemed to be mediated by their ability and willingness to employ tacit knowledge and skills for coping with uncertainty-experiences and, in particular, their ways of preventing epistemic uncertainties from becoming a personal risk.

In chapter 13.2. I sketched the epistemic and social dynamics that embodied anxiety introduces to academic life science research according to the researchers themselves. In the following I will discuss their possible implications on a more systemic level.

The most frequently mentioned concern that researchers expressed was that epistemic uncertainties were increasingly experienced as personal career and social risk and that as a result the most uncertain research questions might get pushed aside. Since they tended to believe that really interesting and groundbreaking research had high levels of epistemic uncertainty, it is worth considering what dynamics embodied anxiety might engender in the long run in terms of the innovative capacity of academic research cultures. When researchers learn to perceive epistemic uncertainties as personal risk, their ways of imagining new research questions might be streamlined to such an extent that – from researchers' perspective – really innovative research trajectories are no longer entertained. A temporary and highly uncertain employment culture might thus unintentionally be creating a situation in which policy measures that aim at furthering research quality by increasing competition might actually have risk-adverse effects. A further epistemic implication that researchers hint at is that in temporary employment cultures it appears pointless for them to plan their research ahead beyond the next two or three years. As they have stressed in our interviews, writing a project proposal requires focusing on "doable" research interests (f6.2: 350f). This suggests that when researchers are socialised in contexts in which they are regularly instructed to imagine research questions that can be investigated in short periods of time, they do not learn to think long-term. As longer-term research trajectories decline, the coherency and continuity of academic life science research might be

compromised on a systemic level and result in a narrower field of vision of what counts as valid and valuable knowledge. Strong performance orientation and tight temporal restrictions not only focus researchers' time and commitment on strictly purpose-oriented activities but as a side-effect they transform research temporalities and modes of thinking as well. Such trends have been described in recent literature on academic work cultures. Robert Hassan has argued that conditions of increasing competition and self-imposed workload are responsible for a trend towards "abbreviated thinking to help cope with the potential overload and make life seem in some way manageable" (Hassan 2003: 239). When purposeless time, time to think or "timeless time" (Ylijoki/Mäntylä 2003) becomes increasingly described as a "luxury", one wider implication of embodied anxiety is that we are dealing with "a fundamental change compared to former images of scientific practice" (Felt/Fochler 2010: 314, German original).

An unintended side effect of conditions of embodied anxiety seems to be that a high fluctuation of personnel creates regular ruptures in the locally available networks of expertise. Young researchers who become highly specialised experts in locally situated epistemic practices and who build up localised networks of expertise take these skills with them when their contracts end, when they leave the lab, go abroad or leave academia. Their expertise must be passed on to the next young researchers or it will be lost (as is often the case). Reestablishing new networks of expertise in a different lab and acclimating new staff to local conditions requires large time investment. Paradoxically this makes the continuity and coherence of local research rely on the temporarily employed young researchers' capacities and their willingness to pass on acquired local tacit knowledge and skills. Building on researchers' accounts we should be concerned that the individualising dynamics of career structures can significantly limit the capacity of lab collectives to do so and this capacity might be further taxed by a chronic underfunding that structurally supports the employment of cheaper – but also less experienced – PhD students over more qualified and more experienced postdoctoral researchers. This increases the effort that the postdoctoral staff needs to put into supervision, instruction and guidance and can put even more time pressure on a career phase that is already described as suffering from a chronic lack of time due to their individual career efforts (cf. Müller 2012). In this context, a situation of high career competition and time pressure might put collaborative and supervision relationships at risk of becoming instrumentalised for the purpose of increasing the individual researchers' own productivity and therefore her/his value as a researcher.

A further possible implication of embodied anxiety might be a shift in tacit academic career requirements. When listening to researchers' accounts of coping with uncertainty-experiences it seems that their perception of what it takes to make an academic career is changing. The ability to cope with epistemic uncertainties and

to prevent them from becoming a career risk is often mentioned as a necessary precondition for moving forward. What should make us worry on a systemic level is that in these narrative contexts the ability to do good research tends to be downplayed as factor for being successful in academia in the favour of coping capacities or put differently: intellectual capacities are narrated as increasingly absorbed by efforts to deal with embodied anxiety.

These observations raise the question of whether the contemporary way of organising academic research contributes to building sustainable and high-quality research systems or whether we need to pay more attention to the tacit transformations in the everyday. The dynamics of tacit governance from below that I have described in this thesis suggest that the so-called withdrawal of the state from governing academia has not left academic research with less governance but has partially replaced it with a different form of governance – one that is more complex and heterogeneous and less tangible, but nevertheless fairly effective: embodied anxiety. Considering these trends it seems that by not providing those who perform everyday research with economic stability and planning security, research cultures are at risk of becoming less able to facilitate continuous and coherent research processes and – to adopt a term that researchers used – they are becoming more "conservative".

14.1.1. Embodied Anxiety in the Context of Expanding Capitalist Dynamics

One of the driving forces for experiences of uncertainty, ambiguity and tension in academic living spaces that has rarely been explored in existing debates is how a commodity perspective leads to the tacit enclosure of knowledge production processes. It therefore deserves a separate section in my conclusions. As many authors have previously argued, academia has never been free from capitalist rationales (cf. Latour 1996). However, recent studies have suggested that capitalist norms and values such as competitiveness and marketability are becoming more thoroughgoing and pervasive with recent developments (cf. Shumar 1997; Slaughter/Leslie 1997, 2001; Kleinman/Vallas 2001, 2005, 2007; Radder ed. 2010). Most of the existing studies of this process are based in English-speaking countries like the US, Canada or Australia. Building on the analysis of this study I argue that we are witnessing a similar development in the Austrian context and that in a certain sense researchers' accounts of coping with uncertainties, ambiguities and tensions can be read as the ways in which life science research cultures accommodate and also attempt to resist and subvert a commodity-perspective.

In chapter nine I discussed quantitative performance measures and the project mode of doing research as two trajectories along which I see a commodity perspective at work in the epistemic living spaces of young life scientists: on the

level of valuing knowledge and on the level of the social organisation of knowledge production. The first is furthered by rationales of New Public Management that aim at giving research findings a quantifiable and abstractly comparable value. In doing so, they create the fiction that knowledge was produced as a commodity – i.e. the fiction that knowledge can be disconnected "from its social roots" (Jessop 2007: 122). As a second level of commodification I discussed the project mode of organising research, since competitive grant procedures are a powerful means by which such abstract values find their way into how researchers plan and carry out actual research processes.

As I have laid out in part three of this thesis, these practices of valuing knowledge and of organising research along values of competition and expectations of plannability partially contradict the way that researchers themselves conceive of their research. In these narrative contexts, a commodity perspective is often described as creating experiences of contradiction and tension and thereby contributing to experiences of anxiety in researchers' epistemic living spaces. Researchers' ways of dealing with experiences of contradicting expectations and with the project mode of organising research – as discussed in part four of this thesis – can thus be read as ways of dealing with tacit processes of commodification. In that their ways of coping often entail ways of avoiding and subverting what I have described as means of commodification, coping with uncertainties, ambiguities and tensions can be seen as ways of resisting a trend towards abstract values increasingly steering academic activity. However at the same time what surfaces in their narrations is that for maintaining smooth and qualitatively good research, researchers cannot exclusively rely on abstract calculations of value and expectations of plannability. Drawing on Polanyi's concept of fictitious commodification that I have discussed in chapter nine it seems that – quite to the contrary – the commodity-perspective relies deeply on the social world of life science research cultures to organise around it and sustain smooth research processes. Coping with such a perspective however builds on alternatives to capitalist norms and values such as collaboration, engaging in activities that are not strictly purpose-oriented or a commonalisation of tacit knoweledge and skills. In that sense, knowledge is treated as a commodity but the commodity-perspective is never absolute and remains incomplete, rendering knowledge a fictitious commodity (cf. Jessop 2007). It is in that sense that researchers resist processes of commodification and reclaim other than commodity-values while at the same time facilitating the survival of commodity-values by soothing the tensions they create in their epistemic living spaces.

We should be worried that this is becoming the norm in research cultures. By being increasingly trained to perform according to commodity-values, researchers might learn to accommodate commodity-values to such an extent that they cannot

imagine alternatives to a commodity-perspective. Since in the particular case of research cultures in the academic life sciences, where coping reaches into very personal, subjective and embodied tacit capacities, we might then speak of a third level of commodification. Accommodating the process of fictitious commodification of the academic labour process might extend to the reorganisation of subjectivities, a self-commodification of the social and emotional bodies of academic labourers. Since in researchers' experience, their production – and the control of their production – critically builds on their very personal motivations and creativity, researchers must want to perform and must want to perform according to particular criteria and in particular ways. The criteria and modes according to which they produce knowledge need to be learned and internalised if they are to be followed, even if they are considered unfavourable.

Drawing on the conceptual framework that Karl Polanyi has developed for how commodity-perspectives are introduced into social practices ([1944] 2001), researchers' ways of coping with uncertainties, ambiguities and tensions can thus described on three different levels: the level of knowledge (i.e. the products of research), the level of labour organisation (i.e. the social organisation of research processes) and the level of the social body of researchers (i.e. embodied abilities to act).

In the sense that researchers accommodate a commodity-perspective, fictitious commodification cannot be seen as a hostile takeover. Rather, the active involvement of researchers and of academic codes and norms cannot be emphasised enough. Both, capitalist codes and norms such as quantifying performance and value, generalised competition but also academic codes and norms such as individual career patterns, ideas of being free to deviate and a high personal involvement in and motivation for the activity seem to form the groundwork for the particular process of commodification in academic life science research cultures.

Generalised uncertainty-experiences can in this context be interpreted as a set of experiences that furthers a third level of commodification. Since one of its features is the existential angst of losing a chosen way of life, and against the background of intensified structural efforts for promoting a commodity-perspective, it seems that researchers must commodify themselves if they want to survive in academia. In doing so, embodied anxiety supports new ways in which capitalist dynamics expand further into the spaces and the bodies of research cultures.

Such dynamics gain broader societal relevance when they are understood to be taking part in the wider societal development towards knowledge societies/economies – a trend in which post-Fordist, often subjectified work cultures are said to become more important. Concerns have been raised about the possibly troublesome implications of commodification processes. Finn Bowring for

instance has suggested that such commodificatin might entail a form of alienation that reaches "the social body – that is, the broader cultural resources, relations and conflicts which set the parameters of selfhood, authenticity and justice" (Bowring 2002: 165). Joseph E. Davis has critically discussed the implications that this commodification of the self might have for the social fabric of work cultures – particularly with regard to the instrumentalisation of social relations:

> Commodifying and marketing ourselves also necessarily implies a change in our social relations. Relentless self-promotion, even if carried off without appearing to be self-absorbed and self-aggrandizing… requires a carefully controlled and utilitarian way of relating to others. They too must be objectified in the interest of the bottom line. On another level, self-commodification also means that at least certain relationships must be more attenuated and even displaced as sources of meaning. (Davis 2003: 49)

In the broader context of an overall transformation of the complex relationship between academia and capitalism in emerging knowledge economies, we might have to consider that, as commodity-perspectives are learned and embodied in a particular way – namely as co-produced with academic codes and norms – in academic research contexts they travel with the bodies of researchers when they leave. Therefore, inasmuch as academic ideals and virtues are redefined by capitalist codes and norms, commodification processes outside academia might also be informed and shaped by the codes and norms of the academic profession. In a sense the university can be understood as an experimental ground for how a commodity-perspective can be introduced into subjectified work cultures – an experimental ground for how knowledge production and the subjectivities involved need to be re-organised in order to serve the needs of a capitalist knowledge economy. Paying close attention to the processes of commodification in academic cultures therefore is impotant in order to get a better understanding of contemporary transformations of academic research cultures within emerging knowledge economies.

14.1.2. Latent Social Conflicts within Embodied Forms of Precarity

In this last concluding chapter I want to return to the observation that initiated my engagement with uncertainty-experiences in academic life science research cultures: that particularly for young researchers, experiences of uncertainty have become so pervasive and existentially felt that they often resulted in self-exploitative dynamics. In that they were generalised into an existential experience of vulnerability – or form of precarity – they seem to considerably limit researchers' imaginations about what kind of life they can lead while pursuing an academic career (cf. Virno 2005: 35ff). Building on the analysis of empirical material, I have argued that this generalised way of experiencing uncertainties assumes a governing function – i.e.

that it is not only an unpleasant side-effect of new ways of governing academic research but an inherent part of the new formations of governing knowledge production that researchers are actively implicated in and that I have worked into the concept of embodied anxiety. During my explorations of the dynamics of embodied anxiety, I have on a few occasions pointed at moments in which researchers' ways of coping might in fact be reinforcing pervasive experiences of uncertainty. In that they seem to favour individual forms of subjectification they tend to suppress researchers' engagement in collective ways of coping and thereby limit the range of opportunities for relieving uncertainty-experiences.

What I have also briefly hinted at is that beyond their effects on young academic life science researchers, conditions of embodied anxiety might result in putting too much pressure on already established researchers. It appears that particularly in larger labs it is often the case that too many young researchers rely on their lab leaders' ways of building and sustaining risk communities. As a result, the lab leaders, in their double role as supervisors and quasi-employers, were made overly responsible for the social security of their staff. Contrary to legal protection or institutionalised social security rights where the provision of certainty allows basic protection to be decoupled from personal dependency. The prevalence of such ways of coping (that I have conceptualised as the clan mode of coping) might turn into an additional source of anxiety for both the young life science researchers who found themselves depending on the sense of responsibility of their lab leaders and the lab leaders who struggled to do justice to the social security expectations that they found resting on their shoulders.

The broader source of tension that underlies these dynamics is the lack of institutionalised protection, which tends to defer the responsibility of dealing with uncertainties to the individual and saddles personal relations with moments of dependency. Seeking certainty, soothing out moments of tension and relieving anxiety over contradictions, discontinuities and dissonances in their academic living spaces seems to increasingly become a tacit requirement for young life science researchers. However, engaging in sophisticated coping tactics can intensify and increase their workload and prevent them from engaging in collective forms of coping. Anderson has recently suggested "the increased workloads of academics can act to repress resistance as effectively as more overtly coercive practices" (Anderson 2008: 262). Beyond that high individual efforts in coping seem to absorb intellectual capacities and thus structurally limit the range of possible ways of coping. The question thus remains: What trajectories can these latent social struggles follow in order to escape the predicament of resistance – i.e. the self-reinforcing dynamics of embodied anxiety? In her text, Anderson suggests that we might have to think differently when we think of exit strategies and for instance consider avoiding managerial tasks to prevent "angst" (Anderson 2008: 263). In the context of this

thesis, I have described such avoidance actions in the trickster mode of coping that tends to refuse the tacit requirements that the academic career script places on young researchers. By refusing to be guided by the values of individual performance and by efforts to plan a permanently latent future, some researchers accepted the contingencies of research processes and of their professional futures. In a sense, by rejecting the imperative to take responsibility for their social security individually, they reclaimed a present that was less preoccupied with future latent possibilities. In doing so, they were able to break the cycle of anticipation, guilt and restlessness that so effectively supports embodied anxiety in the epistemic living spaces of the life sciences and claimed a more autonomous present.

However, while autonomy has often been described as being a typical trajectory of resistance for highly skilled workers (Hodson 1995: 94), as Finn Bowring notes in referring to André Gorz, boldly demanding autonomy might be too unspecific for latent social struggles to become effective:

> …the form which social conflict is likely to take in the 21st century will centre not on the demand for autonomy, but on the struggle over the status, scope and meaning of that autonomy. (cit. Bowring 2002: 166)

In this context it is worthwhile to consider whether researchers' ways of creating spaces of freedom that I have described along their ways of of coping are in fact effective in the sustainable achievement of social security. After all, the kind of autonomy that researchers mostly achieve is temporary and individual and therefore also only relieves embodied anxiety temporarily and individually.

More sustainable forms of relieving embodied anxiety however would have to structurally prevent generalised uncertainty-experiences. For the case of epistemic living spaces in the academic life sciences this would first and foremost mean preventing epistemic uncertainties from being experienced as personal, existential, social risk. Since, as I have discussed throughout this thesis, embodied anxiety is a multi-conditional phenomenon which are not only shaped by institutions but also by more tacit value systems such as the requirements of making an academic career, the list of places where measures against embodied anxiety could intervene is potentially quite long. For my purposes, however, I will deliberately restrict the discussion to possible institutional measures, since institutions can also provide the spaces of negotiation and potential within which other, more tacit forms of collective coping might develop and unfold. From my point of view, the social responsibility for implementing structural measures for relieving generalised uncertainty-experiences, lies on different levels. This thesis offers suggestions for what these structural measures should look like in order to be effective: Researchers employ their coping strategies in order to achieve continuity in income and social security, to create continuity and coherence of their research processes and to allow

for epistemic uncertainties to play out. I suggest that in order to structurally prevent embodied anxiety, these three dimensions must be collectively addressed. In order for them to be sustainable, the challenge is to find smart ways of institutionalising them within and around epistemic living spaces.

One way that academic institutions can help alleviate embodied anxiety is to build an environment that allows for doing continuous and coherent research that includes risk-free, but highly uncertain research. In order relieve young researchers of the burden of creating such an environment themselves, this must first and foremost include more continuous – and most importantly more plannable employment and funding. Ulrich Bröckling has suggested that for the sustainable and fair development of academic research organisation, project-based work organisation needs to be able to rely on non-project-based institutions that are capable of providing continuity in discontinuous temporal cycles of project acquisition and conduct (Bröckling 2007: 241). For academic institutions this means that they must adapt to the requirements of project-based research cultures that tend to externalise the need to cope with uncertainties to the individual and take on the responsibility of being a project institution (cf. Nowotny/Raunig 2008: 94ff). For doing so, some universities already made use of available measures in establishing interim funding for employment gaps, by allowing for permanent contracts, by providing clear reference points for making an academic career, by providing funds for writing grants and by creating a personnel plan that provides a better balance between permanent and temporary staff and thereby provides for more continuity in local networks of expertise.

In the context of chronically underfinanced academic institutions and work biographies that are highly flexible and mobile, the question of how continuous income and social security can be provided is probably the most difficult and challenging question to approach. In this context, scholars have begun to adopt a broader focus on new forms of social protection in conditions of precarity (cf. Tsianos/Papadopoulos 2006: 5). A particularly interesting point in the academic debate is that for conditions in which employment is increasingly discontinuous it might be wise for social movements and (national) governments to think about ways of de-coupling income and social security from employed work and to establish both as unconditional basic rights (cf. Vobruba 2006). In recent years, concrete ways of implementing these measures – in terms of the provision of unconditional basic income and social infrastructures – have been intensely discussed.

Against this background and the analysis of this study I want to finally reflect on what Helga Nowotny and co-authors stated ten years ago: namely that the "contemporary meaning of 'risk' has to some extent eclipsed the more fundamental

importance of 'uncertainty' as an inherent feature of both knowledge production and social change" (Nowotny et al. 2001: 33f). This suggests that, when we – as a society – really want to stake the wealth and wellbeing of our societies on knowledge production, we need to be aware that in doing so we impose the uncertainties involved in the enterprise of seeking new knowledge on our social worlds. This will require measures and institutions that prevent these epistemic uncertainties from being experienced as an existential risk for the workers that produce this knowledge. We find ourselves in urgent need of finding new collective and creative solutions for dealing with those emerging uncertainties if we do not want to witness those who deal with them firsthand collapsing in one way or another under the pressure of embodied anxiety. This might entail finding ways of building knowledge-based societies that escape the dynamics of capitalist economy. In this context, social scientists as well as policy makers have the mandate to invent new forms of organising knowledge production and forms of organising social security that institutionalise continuous economic security and prevent uncertainties from turning into personal and existential social risks.

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16. Annexes

16.1. List of Abbreviations

BMBWK	Bundesministerium für Bildung, Wissenschaft und Kultur (Federal Ministry for Education, Science and Culture)
BMWF	Bundesministerium für Wissenschaft und Forschung (Federal Ministry for Science and Research)
BMWFJ	Bundesministerium für Wirtschaft, Familie und Jugend (Federal Ministry of Economy, Family and Youth)
COSEPUP	Committee on Science Engineering and Public Policy
EC	European Commission
ESRC	Economic and Social Research Council (UK)
EU	European Union
Eurofound	European Foundation for the Improvement of Living and Working Conditions
FWF	Der Wissenschaftsfonds (Austrian Science Fund)
HEP	High Energy Physics
GEN-AU	GENome research in AUstria
GÖD	Gewerkschaft Öffentlicher Dienst (Union for the Public Service Sector)
GOLD	Genomics Of Lipid-associated Disorders
KBE	Knowledge Based Economy
MFPL	Max F. Perutz Laboratories
ÖAW	Österreichische Akademie der Wissenschaften (Austrian Academy of Sciences)
OECD	Organisation for Economic Co-operation and Development
PhD	Doctor of Philosophy (postgraduate academic degree)
RFTE	Rat für Forschung und Technologieentwicklung (Austrian Council for Research and Technology Development)
VBC	Vienna Bio Center

16.2. Speaker Codes in Quotations

Speaker Codes (f=female, m=male)	**Interview No 1** (2006/7)	**Interview No 2** (2009)	**Project**
Qualitative biographical interviews			
Postdoc	f1.1	f1.2	KNOWING
PhD student	f2		KNOWING
Postdoc	f3		KNOWING
PhD student	f4.1	f4.2	KNOWING
PhD student	f5		KNOWING
Postdoc	f6.1	f6.2	GOLD II
PhD student	f7		GOLD II
PhD student	f8		GOLD II
Postdoc	f9		GOLD II
Postdoc	m1.1	m1.2	KNOWING
Postdoc	m2		KNOWING
Postdoc	m3		GOLD II
Postdoc	m4.1	m4.2	GOLD II
PhD student	m5		KNOWING
Lab leader/Professor	prof_m1		GOLD II
Lab leader/Professor	prof_m2		GOLD II
Lab leader/Professor	prof_m3		GOLD II
Group discussions			
Group discussion with Master & PhD students	FGk_jun		KNOWING
Group discussion with Master & PhD students	FGg_jun		GOLD II
Group discussion with Postdocs	FGk_pd		KNOWING
Group discussion with Postdocs	FGg_pd		GOLD II
Life course questionnaires (LCQ)			
PhD student	LCQm6		KNOWING
Lab leader/Professor	LCQprof_f1		KNOWING
Lab leader/Professor	LCQprof_m4		KNOWING
Observational notes			
PhD student	o_f4		KNOWING

16.3. Original Quotations

[i] Sicher? (.) Nein, sicher nicht. Also Wissenschaft ist überhaupt in keiner Hinsicht sicher. Du kannst zwar deinen Beruf kriegen, aber du musst dann nachher meistens Außertourliches leisten, damit du dann ein Gehalt hast. Deswegen würd' ich's nicht unbedingt als einen sicheren… Job oder irgendwie bezeichnen. Weil es gibt da einfach zu viel Konkurrenz noch. … wenn du einmal irgendwo drin bist, dann musst du halt kämpfen, dass du da drin bleibst. (f8: 604-10)

[ii] …ennervierend… (m2: 1001)

[iii] …Stress… (f7: 1491), …recht stressig… (m4.1: 361), …in irgendwelchen stressigen Dingen… (f1.1: 610)

[iv] …hab ich eigentlich mittlerweile irgendwie die Panik… (f4.1: 389)

[v] …Angst… (FGk: 2185)

[vi] PhD1: …ich will Wissenschaftskarriere deswegen nicht machen, weil ich Angst habe. Also sprich Sicherheit. Weil wenn mir Sicherheit fehlt bei diesem Werdegang. … PhD2: …womit ich nicht leben kann ist, dass du eben den Rest deines Lebens unsicher und mobil, also unsicher leben wirst und mobil sein musst. Auch bis ins hohe Alter. (FGk_jun: 2185-202)

[vii] …in den schlimmsten Fällen bleibt dann nur übrig, dass man sagt…: Wurscht, wir brauchen jetzt die Publikation. … raus damit! … [dass] man sich… sagt: Okay, das sind jetzt die Daten, das weist drauf hin, dass dieses und dieses interessant sein könnte, lass uns doch das verfolgen und das ist jetzt ein großes Projekt, da sollte man einen Dissertanten anstellen oder eine Dissertantin, so sollte es eigentlich sein, aber man ist eigentlich in einen Pragmatismus reingezwungen, der wahrscheinlich nicht immer das Beste für die Forschung ist. (m4.2: 1274-85)

[viii] …dadurch dass wir immer nur so kurze Verträge kriegen, kommt einerseits zwar frischer Wind rein. Das Problem ist halt, dass es sich zum Teil gar nicht überschneidet und sehr viel Know-how wieder verloren geht. … zum Teil hat man ja einen vollständigen Wechsel. Da geht einfach so viel Wissen wieder verloren, dass man zum Teil dann wieder bei Null anfängt. Und das find ich ziemlich blödsinnig. Weil… wenn man in jemanden schon viel Geld reingesteckt hat, der dann schon Wissen hat, und er kann es dann noch nicht mal weitergeben bevor er dann gehen muss, (.) ist einfach sehr viel Geld auch weggeschmissen, mehr oder minder. Und woran auch die Forschung leidet natürlich. (f7: 312-21)

[ix] …der Impact… offiziell oder in der Wissenschaftswelt zählt nur das, leider Gottes. Was sehr schwierig… dort auf der Uni, weil's einfach die Jobs nicht gibt… mit der Konkurrenz, und ein jeder muss schauen für sich selber. Wenn jemand jetzt eine qualitativ Top-Arbeit macht, er betreut die Leute oder schaut, dass der Laden funktioniert… Und der steht jetzt nicht an erster oder an letzter [der prestigereichen] Stelle, dann, sagen wir, der, das ist genauso wichtig… Also, der Impact-Faktor, würd ich sagen… ist das eigentlich das Einzige, was zählt bei die Leute. Aber es zählt nicht der Mensch. … wenn man mit so viel Leut arbeitet, man muss auch Führungsqualitäten, man muss auch die Fähigkeiten haben, ja. Und beim einen liegen die Fähigkeiten da, dass der vielleicht eher ein Forscher ist, der sehr kreativ, viele Ideen hat… Und ein anderer… oder eine andere… schaut, dass die Betreuung, dass das ganze Umfeld, die ganze Infrastruktur, die Geräte und das alles passt, oder? Und das fließt natürlich nicht in den Impact rein. (m3: 1148-69)

[x] Weil die Qualität vielleicht leidet wenn, wenn die Leut das Gefühl haben sie müssen publizieren, schnell publizieren, dann glaub ich passiert sehr viel Schlechtes. Dann wird vielleicht nicht mehr so ordentlich geforscht, es ist überhaupt nicht mehr so viel Raum für die pure Neugier. Also dieses idealistische Bild der Forschung, das war vielleicht vor 50 Jahren so. Und das ist heut nicht mehr da… Ist ein bisschen bedauernswert, ja. Nicht nur ein bisschen, weil eigentlich ist es das was uns auch die Zukunft ein bisschen verbockt. (.) Also mir zumindest glaub ich [lacht]. (f1.1: 159-70)

[xi] Also Glück ist viel dabei. … wir arbeiten da bestimmte Sachen an Proteinen, und das ist nicht unbedingt vorhersagbar, ob das jetzt funktionieren wird oder nicht. … wenn man sich es vorher ein bisserl überlegt, dass man dann in zehn Prozent der Fälle wirklich zu einem positiven Ergebnis kommen kann, einfach weil es viele Möglichkeiten gibt, was nicht funktioniert. Also man muss irgendwie das Glück haben und diese zehn Prozent erwischen. Beziehungsweise man misst einfach so viele Proteine durch. Und dann ist es aber wieder sehr zeitaufwändig. Also das ist das, was ich mit Glück meine… (m1.2: 356-64)

[xii] Und grade wenn man Wissenschaft macht, will man etwas Neues machen, etwas Neues erforschen, das heißt, man macht immer etwas, das noch keiner vorher gemacht und man weiß nie ob's funktioniert. Und in 90% der Fälle funktioniert's nicht auf Anhieb. Das ist einfach so. (m2: 630-3)

[xiii] …Tatsache, dass wir mit lebendem Material arbeiten und dass, wenn man eine Infektion in der Zellkultur hat, drei Monate Arbeit dahin sind. (m4.2: 944-6)

[xiv] Am Anfang glaubt man, ja, jetzt mach ich ein Experiment und dann weiß ich was drüber. Und da weiß man auch noch nicht, dass man das Experiment vielleicht zehn Mal machen muss bis es funktioniert, und dass es mühsame Handarbeit ist, und dass man Sachen wiederholen muss und dass es irrsinnig langsam geht eigentlich… Und das ist dann frustrierend anfangs, und wenn man dann ein bisschen Frustrationstoleranz entwickelt, dann geht's halbwegs. (f1.1: 115-23)

[xv] Und es heißt ja auch Research, Re-, wieder suchen… das ist einfach immer wieder ein Zyklus bis man das dann wirklich zeigen kann, als erster zeigen kann. (m2: 633-5)

[xvi] Es geht um Finanzierung der Forschung, also, wenn wenig Geld für Forschung da ist… es ist wichtig, dass ein guter Output da ist, und es ist auch wichtig, dass es einen gewissen Druck gibt. [Aber]… wenn der Druck so groß ist, dass man High-Risk-Projekte gar nicht mehr beginnen kann, ist der Druck eindeutig… völlig fehlgeleitet, dann ist ein eindeutiger Fehler im System drinnen. Es kann auch sein, dass man nur mehr High-Risk-Projekte machen kann, damit man's schafft, und dann hat man eben eine Untergangsquote, die enorm hoch ist. Wichtig ist eben, glaub ich, dass einfach eine Forschung, dass du eine gewisse finanzielle Freiheit hast. Wenn du mehr Geld hast, mit dem du arbeiten kannst, dann kannst du auch andere Projekte probieren, die vielleicht nicht funktionieren und wenn dann dein Labor nicht bankrott ist, dann hast du's probiert, es hat nicht funktioniert, du hast eh viel Arbeit reingestellt, aber das soll dich nicht sozusagen in den Untergang treiben, ne. Und wenn da einfach das Geld von vornherein so knapp ist, dass man nur ganz konservativ arbeiten kann, dann kommen halt auch nur ganz konservative Ergebnisse raus, [ich meine], zum größten Teil. Also, es ist, (.) ohne dass ich verschwenderisch arbeiten möchte, aber einfach die Freiheit, was zu experimentieren ist wesentlich größer. Wenn's auch, wenn der finanzielle Druck nicht so groß ist, wenn man's dann einfach riskieren kann… Also, ich glaube, der Druck hat sehr viel einfach mit Geld und damit der Anzahl der Stellen zu tun… (f6.1: 713-41)

[xvii] Also wie ich schon gesagt hab, es ist sehr persönlich bedingt. Dadurch dass es deine ganze wissenschaftliche Arbeit von dir aus gründet, und nicht weil das ein Kunde so haben will, weil mein Chef das gesagt hat oder weil irgendwas. Weißt was ich mein, sondern es gründet wirklich aus dir, hängt deine Motivation stark von dir selber ab. Wie es dir geht. Oder (.) ja, wie du dich grad fühlst. Also ich kann's bei mir wirklich beobachten (lacht). (f4.1: 989-94)

[xviii] …mit Freunden oder so die ganz normale Berufe haben… (f4.1: 974-83)

[xix] Prestige hat das sicher schon. (f4.2: 1157)

[xx] Das ist mir schon gegeben. Also das, das hat man schon in der Volksschule gemerkt, dass ich für Mathematik sehr viel übrig hatte und auch, auch Biologie, irgendwelche Blätter gesammelt hab und mich da Ewigkeiten damit beschäftigt hab. Also das, das, das war schon in mir, ja? (f4.1: 17-21)

[xxi] Und meine Eltern würden bestätigen, dass ich schon immer sehr neugierig war, mich alles immer im Detail interessiert hat und immer wieder eine Warum-Frage gekommen ist. (f3: 12-4)

[xxii] …dieses Gefühl, was es in vielen Firmen gibt, dass man quasi um fünf die Tür hinter sich zu macht und dann ist Privatleben, das gibt's in der Wissenschaft viel weniger… ich arbeite jetzt mit Zellen, die muss ich mir am Wochenende auch anschauen, weil wenn die übers Wochenende zuwachsen, dann kann ich sie nicht mehr verwenden… (m4.2: 760-4)

[xxiii] …nach wie vor hab ich den Eindruck, dass, obwohl die, die Bedingungen auf der Uni immer härter werden… dass man prinzipiell immer noch eher interessensgetrieben forschen kann, auch wenn man innerhalb diesem Bereich wieder die low-risk Projekte aussucht, aber trotzdem ist das immer noch eher interessensgetrieben als rein wirtschaftlich angetrieben wie in der Pharma-Industrie oder so. (m4.2: 359-64)

[xxiv] …es ist nicht so, dass wir jetzt ein riesen Gehalt hätten und (.), aber dafür haben wir eben den Vorteil, dass wir eine interessante Arbeit haben, oder? (m1.1: 304-6)

[xxv] Sprich, ich hab immer noch die Leute, die jetzt tatsächlich damit arbeiten, mit denen ich einen absoluten Austausch führen kann. Sowohl methodisch, also auch informationstechnisch. Und es ist oft so, dass ich zu einer Kollegin geh und sag: ‚Bei mir hat's nicht funktioniert. Wie machst das du?' Oder: ‚Kannst du's einmal für mich mitmachen? Weil ich geb schon verzweifelt auf, weil ich schaff's nicht.' … Oder umgekehrt. Man bietet mal was an… nur weil ich jetzt ein eigenes Projekt habe, heißt das nicht, dass ich wirklich (.) aktiv mit anderen Leuten (.), also dass ich nicht mehr aktiv mit anderen Leuten zusammenarbeite… Und ich glaub, dass das ausschlaggebend ist. (FGk_jun: 810-22)

[xxvi] Also als reiner Chemiker ist es schwer, ohne Dissertation überhaupt einen Job zu bekommen, nur mit dem Titel. Und also ist natürlich die Dissertation ein logischer Stoppunkt, dass man sagt jetzt überleg ich mir, was ich mache und welche Richtung schlage ich ein… wenn man sagt man ist mit seinem Magister fertig und macht noch eine Dissertation kann man auch zusätzlich, diese große

Entscheidung braucht man auch noch nicht treffen, weil die meistens nicht hinderlich ist. (f6.2: 606-12)

[xxvii] Also ich, ich werd halbwegs selbstständig langsam. Das hat sich geändert in den letzten Jahren. Weil früher hab ich einfach mein Projekt gemacht und aus. Und jetzt bin ich so, dass ich weiterdelegier (.) und es eher beaufsichtige. (f1.1: 249-52)

[xxviii] …so bin ich in diese Schiene reingerutscht… (f1.1: 56)

[xxix] Das waren so diese halbbewussten Entscheidungen, oder? (f1.1: 46f)

[xxx] Ich will nicht mehr allein, ohne soziales Umfeld irgendwo eben in der Pampa auf einer Uni sitzen. Also ich persönlich brauche mein soziales Umfeld, damit ich irgendwie funktionier, sag ich einmal so. Das war nur mal, also allein mach ich das sicher nicht. Dementsprechend fällt eigentlich wissenschaftliche Karriere aus, mit dem, mit der Entscheidung, ja? (f4.2: 814-8)

[xxxi] Weil dann ist es urschwierig, noch mal in eine Firma zu gehen – oder mit 35 schon oder so, na?… zwischen 35 und 40… da entscheidet sich dann eigentlich wieder dein ganzes Leben. Und das ist schon irgendwie frustrierend, wenn du darauf hinarbeitest, dass du irgendwas schaffst, und dann geht's doch nicht, na? (f5: 772-6)

[xxxii] Nur möchte ich auch nicht… [in der Situation sein], dass… ich mich wirklich für den [akademischen Karriere-]Weg entscheid und… ab 38, ab 40 gibt's dann keine Finanzierung, keine Perspektive. Ja, was mach ich dann? Dann bin ich… so spezialisiert, dass mich eigentlich keiner mehr brauchen kann, ja? Und was mach ich dann wirklich, ja? (f4.1: 829-33)

[xxxiii] …gerade in den Bereichen wo es so kurzfristige Verträge gibt und wo man so von der Fürsprache von Institutsvorständen und Ähnlichem abhängig ist. Weil einfach in der jungen wissenschaftlichen Karriere kann man noch nicht von einer Unabhängigkeit sprechen, die wir tatsächlich haben, nicht wie wir arbeiten, sondern wie wir sie tatsächlich haben, ned? (f6.2: 353-7)

[xxxiv] Ich war weiter finanziert und bin nach wie vor von… [einem grossen Programm] finanziert… es waren allerdings immer wieder Zwischenfinanzierungen, ich hab zwar immer am gleichen Thema gearbeitet, hab aber inzwischen schon den sechsten oder so Nachtrag zu meinem Dienstvertrag unterschrieben… Das ist zur Zeit alles total im Umbruch, mehrere Komponenten spielen da mit… dass mein Vertrag Ende des Jahres… ausläuft… es gibt jetzt zwar inzwischen schon eine Lösung, aber das war jetzt oder mehr weniger ein halbes Jahr der Ungewissheit, wie geht's überhaupt weiter… das heißt, es wär eigentlich Geld für mich dagewesen. Es wär die Situation so, dass ich in Ruhe hätt' meine Forschungen weitermachen können, aber die… [Universität] hat [auf Basis eines neuen Gesetzes] gesagt: Nein… du wirst nicht länger als… sechs Jahre angestellt… und Ende des Jahres endet der Vertrag… (m4.2: 36-71)

[xxxv] …mein Teil des Projekts… aus dem 50% meines Gehalts stammt, endet… Ende März oder Ende April nächsten Jahres, d.h. wenn sich dann nicht irgendwie irgendwas ergibt, werde ich 50% meines Einkommens verlieren…

I: Und die anderen 50% sind…?

Das ist eine befristete Assistentenstelle… Die geht noch länger, ist aber im Prinzip uninteressant. Mit einem halben Gehalt mach ich nicht weiter. (m1.2: 21-33)

[xxxvi] Genau, ja. 40 Stunden ist unter Anführungszeichen (beide lachen). I: Ein bisschen mehr nehm ich an. Ja, so zwischen 50 und 60 sind's meistens. (f8: 204-8)

[xxxvii] …junge Leut bringen auch wieder viel neuen Wind. Und wenn man halt der Chef ist bzw. wenn's um die Finanzierung geht, ist es eine ganz klare Sache: junge Leut Kosten nix, arbeiten doppelt so viel wie die, die irgendwann einmal damit anfangen, je älter sie werden, sich auch für Privatleben sich mehr zu interessieren. (FGg_jun: 339-44)

[xxxviii] PhD1: Ja. Ich glaub, es wird immer schwieriger. Das war früher ja viel leichter… I: Inwiefern? PhD2: Ja, weil, weil man nicht so unter Leistungsdruck war. PhD3: Weil man ist so ersetzbar geworden. PhD2: Genau. Genau. Man ist ersetz-, das ist gut. Ja. Man ist total ersetzbar geworden, ja. PhD3: Kannst gehen, gehen wohin du willst, es kommt eh jemand nach der das Gleiche machen kann. PhD2: Aber das ist ja wurscht, und vor allem, kommt vor allem ein Diplomand nach, der kostet mich dann vielleicht nix und arbeitet dann doppelt so viel. Ja genau, das ist, ist gut gesagt, ja. (FGg_jun: 356-72)

[xxxix] Und das sind halt Probleme irgendwie, wenn du sagst, ja du machst jetzt den Postdoc, 3 Jahre, vielleicht noch mal 2 Jahre, noch mal 3 Jahre. Und irgendwann, vielleicht gibt's kein Geld mehr. Wir haben einen Fall bei uns. Der ist 40 und für 40 reichlich zu spät für die Privatwirtschaft, den nimmt keiner. (FGg_jun: 585-9)

[xl] Ja das Witzige ist, es wird im Prinzip quantifiziert dann auch. Im Prinzip wird so in gewisser Weise das, der Lebenslauf qualifiziert in diese Impact-Faktoren, oder?... Und da ist es natürlich schon so, dass die, der Robert sagt das immer, die, die Molekularbiologen oder, oder (.), die können in Journalen publizieren, die einen höheren Impactfaktor haben. Das resultiert einfach teilweise daraus, dass es mehr gibt. Es gibt mehr Menschen, die es lesen, dass es mehr gibt, die das zitieren. Und bei uns am, am MFPL ist es scheinbar eben so, dass die Leute gewertet werden nach der Zahl oder dem Durchschnitt der Impactfaktoren, in den vergangenen drei Jahren oder so was. (FGk_pd: 249-60)

[xli] Die Währung der Wissenschaft. Die Liste mit eigenen wissenschaftlichen Veröffentlichungen ist das wichtigste Aushängeschild jedes Forschenden und jeder Forschungsgruppe, ja von ganzen Universitäten. Publikationen dokumentieren woran, wie viel und in welcher Qualität geforscht wird. Sie bestimmen so auch, wie viel Geld in das jeweilige Forschungsgebiet investiert wird. (GEN-AU 2012)

[xlii] Die Arbeitnehmerin räumt hiermit dem Arbeitgeber an der von ihr im Rahmen der dienstlichen Obliegenheiten geleisteten Arbeit sämtliche Eigentums- und Immaterialgüterrechte ein, und erteilt ihm, falls eine Vollrechtsübertragung rechtlich nicht möglich ist, zeitlich, räumlich und sachlich unbeschränkte ausschließliche und unwiderrufbare Nutzungsrechte. (Standard-Arbeitsvertrag der Universität Wien)

[xliii] Die Universität Wien hat der/dem zuständigen BundesministerIn im Wege des Universitätsrats jeweils bis 30. April eine Wissensbilanz über das abgelaufene Kalenderjahr vorzulegen (§ 13 Abs. 6 des Universitätsgesetzes (UG) 2002, BGBl. I Nr. 120/2002 in der jeweils geltenden Fassung) und diese in ihrem Mitteilungsblatt zu veröffentlichen (§ 20 Abs. 6 Z. 3 UG 2002). (Universität Wien 2010)

[xliv] Diese Wissensbilanz dient gemäß Verordnung der Bundesministerin für Bildung, Wissenschaft und Kultur über die Wissensbilanz (Wissensbilanz-Verordnung – WBV, BGBl. II Nr. 63/2006) der ganzheitlichen Darstellung, Bewertung und Kommunikation von immateriellen Vermögenswerten, Leistungsprozessen und deren Wirkungen und ist als qualitative und quantitative Grundlage für die Erstellung und den Abschluss der Leistungsvereinbarung heranzuziehen (§ 2 WBV). (Universität Wien 2010)

[xlv] Es wird alles über Impact-Faktoren abgerechnet. Die Finanzierung ist komplett umgelagert worden, würd ich jetzt einmal sagen... bei uns wird das aufgeteilt nach... kumulierten Impact-Faktoren... jeder Einzelne in diesem Institut, von dem werden die Impact-Faktoren der letzten fünf... oder drei Jahre zusammengezählt und dann werden Institute miteinander verglichen und das Institut kriegt... den entsprechenden aliquoten Anteil. (m1.2: 1189-202)

[xlvi] ...es ist sehr schwer möglich, dass ich ohne hier Publikationen vorzuweisen, dass ich sage ich bewerbe mich für ein andere Stelle und ich war hier zwei, drei Jahre und da ist überhaupt nichts rausgekommen. (f6.2: 69f)

[xlvii] wie gesagt, das ist auch ein Ding der ständigen Veränderung. Die, die, die letzte Entwicklung hier an der... [Universität] ist eben, das man versucht, generelle Qualitätskriterien... die man erreichen muss, einzuführen. Und ich hab eben mitgekriegt, dass es auf der... [Nachbaruni] auch so ist... also, das dürfte jetzt in Zukunft... zumindest klarer sein für den Einzelnen. (m4.2: 555-60)

[xlviii] Theoretisch ist es gar nicht möglich. Also theoretisch kann es sein, dass ich wieder in den Problemfall komme, da es ja vom UG 2002... darf man nicht an der Universität... länger als 6 Jahre – glaube ich – bedienstet sein... wobei es noch immer nicht ganz klar ist, ob... mein offizieller Dienstgeber [bei diesem Projekt]... die Universität [war]... das ist noch nicht ganz ausjudiziert, also es kann durchaus sein, dass sie dann sagen, da geht es nicht... Und irgendwo in diesem Graubereich befinde ich mich. (6.2: 249-59)

[xlix] ...die Dissertation war dann fast ein Jahr später erst unter Dach und Fach...weil ich quasi gedacht hab, so, morgen geb ich meine Diss ab und damit ist ein Kapitel beendet, das war dann aber so, dass der Chef dann doch noch gewisse Experimente gefordert hat und war eher eine unangenehme Erfahrung, das Ganze... (m4.2: 22-31)

[l] Und ich als Assistent,... die gesetzliche Lage ist mir nicht ganz klar, es... müsste wahrscheinlich schon so sein, dass ich bis zum Ende meiner Assistentenstelle 2012 die Möglichkeit haben müsste, ein Projekt zu beantragen, aber ich bin mir nicht sicher, ob das sehr leicht möglich ist bei der momentanen Administration und vor allem, es ist eigentlich total uninteressant, also weil da müsst ich einen eigenen Projektantrag schreiben für eine halbe Postdoc-Stelle, ich dürfte mich Sicherheit keine Mitarbeiter beantragen so wie ich jetzt habe, also das ist sicher ausgeschlossen... Es ist eigentlich eh egal, weil wir müssen sowieso weg und das irgendwie vielleicht sogar besser, weil jetzt wissen wir zumindest... I: ...das heißt, es gibt hier keine Möglichkeit für irgendwen von euch, eine

permanente Stelle zu kriegen? T: Nein. I: Weil ihr eben nicht extern seid… T: Also das ist jetzt noch viel klarer als es vor zwei Jahren war… es gibt schon Leute, die sich intern bewerben, aber es ist, glaub ich, meines Wissens… noch nie jemand wirklich in die nähere Auswahl gekommen. I: Okay, das heißt, es müssen eigentlich wirklich sehr viele einfach gehen. T: Einfach gehen… und… die Administration [hat sich] um uns Assistenten noch nie gekümmert… wahrscheinlich deswegen, weil wir eh befristete Verträge haben und dann weg sind… Definitiv. Also es wird vermittelt, es wird also in jedem Meeting, wo man sitzt wird vermittelt, dass man gehen soll. (m1.2: 72-134)

[li] Ja, ich mein die Vorgaben sind ja, sind ja sehr variabel. Also die Vorgaben sind klar, aber wie die dann interpretiert werden, das ist sehr variabel. Also das hab ich jetzt schon bei mehreren Kollegen gesehen, dass diese Zielvereinbarungen zum Beispiel mehr oder weniger, ja, jetzt ist grad so eine Übergangsphase, wo diese Zielvereinbarungen eingeführt werden, das heißt, sie werden dann mehr oder weniger so formuliert, dass es eh schon auf das Profil passt [lacht] von der Person, die sich da bewirbt. Also es ist teilweise dann schon. (m4.2: 1248-52)

[lii] Aber bei uns gibt's halt wie gesagt einen Zentrumsleiter, und der bestimmt welche Positionen besetzt werden. Und da gibt's jetzt wirklich jetzt nicht… ein Schema, auf das man sich verlassen kann. Und das ist eine Riesenverunsicherung. Weil die meisten Leute denken sich: ja, ich hab eine gute wissenschaftliche Laufbahn hingelegt und so, aber es gibt keine klaren Entscheidungsprozesse oder klare Karrierewege (.) auf der Uni oder in der Forschung. (m3: 837-43)

[liii] Wir machen wirklich, wirkliche Grundlagenforschung. Und das ist aber einfach nicht mehr so sehr gern gesehen. Es muss alles wieder so irgendwie produktiv sein in dem Sinn. (.) Das kann sich ein bisschen negativ auf die Forschung auswirken glaub ich…Weil die Qualität vielleicht leidet wenn, wenn die Leut das Gefühl haben sie müssen publizieren, schnell publizieren, dann glaub ich passiert sehr viel Schlechtes. (f1.1: 151-61)

[liv] Die Währung mit der du bezahlt wirst ist auch komplett komisch, ja? Also es geht nicht darum, ob das wissenschaftlich nett war, ob das in deinem Bereich, Fachgebiet was Wichtiges ist. Es zählt einfach nur wieviele Publikationen du hast, ja? Wieviel, wieviel, wieviel Punkte du hast… Du machst total viel andere Sachen und es ist, das wird nicht, das wird im Prinzip nicht berücksichtigt. Und ganz ehrlich… mein persönliches Befriedigung ist es nicht, dass ich ein Paper hab… Also mit der kann ich mich nicht irgendwie zurecht finden, ja? (f4.1: 909-22)

[lv] Der faulste Dissertant mit den besten Ergebnissen wird trotzdem der beste Dissertant sein. Jemand der 5.000 Analysen macht, aber leider die falschen Proben analysiert hat, wird trotzdem nix reißen. Und (.) das ist halt (.) das ist bei, in der Biowissenschaft sicher noch extremer, dieser Glücksfaktor… (f9: 455-8)

[lvi] Es wird [gleichzeitig] kein so hochrangiges Paper werden, weil (.) mein Projekt jetzt, mein Unterprojekt, das ist eben ganz und gar nicht so gegangen, wie ich mir das vorgestellt hab. Spielt wieder in den Bereich rein, dass die Natur den Weg vorgibt. [schmunzelt] Und jetzt ist quasi, jetzt hab ich quasi umschwenken müssen und (.) jetzt ist das ganze wahrscheinlich mit einem niedrigeren Impact zu publizieren, als geplant war. (m4.1: 1809-14)

[lvii] Es ist halt von der Unsicherheitseite weil du teilweise mit Problemen immer wieder konfrontiert bist die du… nicht ad hoc… anhand von deinem Studiums… lösen kannst – ja?… und ich meine, ich versuche eh das irgendwie so… Aber das ist halt A: unsicher und B: weißt du auch nie, ob da überhaupt irgendwas heraus schaut. (f4.1: 134-3)

[lviii] Also ich kann mir nicht vorstellen, dass es auf Dauer wirklich so möglich ist wie es jetzt ist, dass man immer nur diese wenig Jahre, kurzzeitigen Verträge hat. Diese, und wirklich in der Molekularbiologie sind drei Jahre nichts. Wirklich, das ist ein Lufthauch. Wenn man unsere Experimente anschaut und so weiter, ist das vom Zeitrahmen her extremst wenig. (f7: 729-32)

[lix] …da war mir noch nicht bewusst, dass diese Sechs-Jahres-Frist auf mich auch zutrifft… das ist mir erst heuer… bewusst worden, nachdem ich dann wieder den neuen Vertrag gekriegt hab… und der ist dann aber nur bis Ende des Jahres gegangen. Dann… hab [ich]… beim Personalbüro… gefragt, wieso nicht gleich bis Ende des Projekts verlängern und dann haben sie mir eben gesagt, dass auf… [meiner Universität] nur bis Ende des Jahres die Möglichkeit besteht. (m4.2: 192-7)

[lx] Ich meine, für mich das zum Beispiel nicht gut, dass die Postdoc aufgehört hat damals… die war eineinhalb Jahre da im Endeffekt. (.) Und das ist zum Beispiel einfach das Ende von einem Projekt das nicht fertig ist, und das ist nicht so… angenehm. Wir versuchen es zwar weiter zu führen, die ist jetzt in [einer anderen Stadt] aber… es geht alles noch langsamer irgendwie… wenn dann mehr Gruppen beteiligt sind… wir versuchen das alte ein bisschen weiter zu machen, aber es läuft im Moment nicht so. (f1.2: 476-94)

[lxi] Zusätzlich kommt auch, dass ich eine Gruppe aufbaue und Dissertanten habe und Dissertantinnen habe für die ich zuständig bin und die möchte ich auf keinen Fall auch in der Luft hängen lassen… (f6.2: 74-7)

[lxii] …die Biochemie alleine find ich schon sehr komplex und man kann… auch irrsinnig viel sich da hineinsteigern und reintigern… man hat ja selber auch nur 40, 50 Stunden Zeit zum Arbeiten. Und dann gibt's aber… [diese Technologie] ich hab das zumindest als was empfunden, was noch so viel Ebenen dazu macht, nämlich wie das Gerät zu händeln ist, wie du Experimente aufsetzt, wie du Experimente vielleicht neu schreiben kannst, wie du das auswertest, was du daraus machst, wie du das Strukturprogramm händelst, wie du die Dynamikprogramme handelst, wie du einen Impuls kalibrierst – also da gibt's zigtausend Sachen. Also drum glaub ich, dass es fast nicht, dass du da auf beiden Spezialist bist, glaub ich, ist fast nicht möglich. (FGk_pd: 501-13)

[lxiii] …die hat dann erst im Oktober 2006 begonnen, das heißt die hat die drei Jahre noch nicht einmal voll gehabt. Nicht, also das heißt die muss ich sowieso weiter betreuen und dann hab ich ja andere Projekte auch noch zusätzlich zu dem… Projekt an denen ich Dissertanten betreue und das überlappt. Also da gibt's nicht immer das große Chaos alle drei Jahre sondern das kleine Chaos fast jedes Jahr, oder so. (f6.2: 86-94)

[lxiv] Ich glaub, dass die Anforderungen immer höher werden. Was damit zusammenhängt, dass bei uns die ausgeschriebenen Stellen wahrscheinlich immer weniger werden. (m1.1: 434-6)

[lxv] Aber es gibt einfach zu wenig Stellen, und es gibt auch keinen Karriereweg. (m2: 197)

[lxvi] …wenn man das rein karrieretechnisch… betrachtet, ist das natürlich die Mindestvoraussetzung, dass man den nächsten Schritt macht und quasi eine unbefristete Stelle bekommt, nach oben hin ist das Ganze natürlich offen. Projekte, Drittmittel an Land ziehen und Forschungsgruppen leiten, publizieren, zweimonatlich im Science oder so was. (lacht) (m4.2: 525-9)

[lxvii] …kommen jetzt wieder zurück, und finden jetzt die Situation vor, dass sie all das gemacht haben – sei es jetzt mit Vor- oder Nachteilen – aber keinen Bonus dafür bekommen. (.) Und deswegen wird das jetzt eigentlich immer kritischer bei uns gesehen, ob's das wirklich bringt. (m2: 172-6)

[lxviii] Also innerhalb des Labors gibt's bei uns keine Konkurrenz (lacht). Also ist nicht so, dass jetzt vom Gruppenleiter zwei, drei Postdocs auf ein Projekt angesetzt werden und der, der zuerst fertig wird kriegt die Publikation. Solche, solche Labors gibt's auch. (m2: 485-8)

[lxix] …wir da in Listen verglichen werden. Im Endeffekt wird dann gezählt, wer hat wieviel Publikationen. Und wenn du… drei Jahre in Karenz warst, hast dementsprechend weniger Publikationen und (.) da sticht halt einfach dann die [Zahl]. (FGk_pd: 1347-50)

[lxx] …ich brauch halt auch meine eigenständigen Papers. Ist klar. Und halt auch eigenständiges Funding sozusagen. Dass ich mich… [für den nächsten Karriereschritt] qualifizieren kann. (f3: 818-20)

[lxxi] Man muss auch früh genug erkennen, dass eine naive Wissenschaft nicht möglich ist, in dem Sinne, dass man sagt es reicht wenn ich eine gute Wissenschaft mache… (f6.2: 346-8)

[lxxii] Und heutzutage ist so Projektschreiben (.), ich würd schätzen 10 bis 20% der, nein (.) 10 bis 15% der Zeit geht drauf. (m1.1: 584-5)

[lxxiii] Und da macht man halt Vorarbeiten. Wenn die gut ausschauen, dann schreibt man dann einen Projektantrag dazu. (f1.1: 602-4)

[lxxiv] Es ist sehr viel (.) Networking und sich verkaufen müssen – solche Dinge, und das liegt mir irgendwie nicht… Ich meine hier in dieser Nische geht es, aber diese Nische wird nicht mehr lange existieren wahrscheinlich. (f1.2: 109-13)

[lxxv] Man kann ein Hobby verfolgen sag ich mal. Dafür geht es sich aus, aber es sehr viel Wochenendarbeit inklusive, vor allem wenn man im Ausland ist, dann verbringt man die meisten Urlaube damit, auch wieder Kontakt mit der Heimat aufzunehmen. (m2: 853-6)

[lxxvi] …sie machen, was irgendwie möglich ist. (f9: 161-2)

[lxxvii] Also das war jetzt meine persönliche Erfahrung. Sprich, derzeit konzentriert man sich wirklich vor allem auf sein Projekt. Weil da muss einfach etwas rausschauen und da legt man den Grundstein mehr oder weniger. (m2: 856-9)

[lxxviii] Und der… [Postdoc] muss jetzt schauen, dass er irgendwo einen Posten kriegt, ja? Das heißt, der hackelt wie wahnsinnig, hat irgendwelche Kooperationen mit irgendwas, der muss schauen, dass er selber weiter kommt… (f4.2: 924-9)

[lxxix] …wenn die Verträge einfach auslaufen, müssen sie dann einfach weg und sich was neues suchen, oder sonst müssen sie, ein halbes Jahr oder ein Jahr pausieren und können dann wieder auf einen

vielleicht Sechs-Jahres Vertrag gehen, was halt für die Aufrechterhaltung und die Etablierung einer wissenschaftlichen Karriere ein völliger Wahnsinn ist. (f6.2: 362-7)

[lxxx] Es ist in unserer Situation sehr schwierig zu planen. Wenn ich, ich würd, ich hätt einfach sehr gern das Gefühl, dass man mehr als drei oder fünf Jahre Zeit hat, um an bestimmten Projekten zu arbeiten… dann könnten wir uns mehr um die wissenschaftliche Planung kümmern… aber so wie es momentan ist, planen wir hauptsächlich unsere eigene Zukunft. (m1.2: 289-94)

[lxxxi] ..hängt irgendwie alles in der Luft zur Zeit. Wir hätten zwar einen Masterplan, der, da ist die Frage, ob man das so umsetzen kann… Also ich verwend' zumindestens ein Drittel meiner Zeit damit, ums Überleben zu kämpfen und unbefristete (sic!) Anträge zu schreiben und, und also Stipendien anzufordern und so weiter, letzteres ist durchaus Teil von meinem Job, aber sonst, die ganzen Kämpfereien auf der… [Universität] sollte ich eigentlich besser im Labor verbringen oder Papers lesen in der Zeit, aber das ist zur Zeit nicht real. (m4.2: 156-70)

[lxxxii] …prinzipiell ist man, glaub ich, gut beraten, möglichst langfristig zu denken, wobei das oft, wobei sich oft Situationen ergeben können, gewisse Gefüge auf der Uni sich ändern können, wo, wo sich dann eine ganz andere Situation ergibt, wie man sich eigentlich gedacht hat und dass das, also das längerfristige Denken ist, ist, könnte dann hinfällig werden. (m4.2: 479-83)

[lxxxiii] …irgendwo in diesem Graubereich befinde ich mich. Also der Plan soweit, also so unplanbar er ist, ist schon, dass ich hier meine Gruppe weiter stärken möchte, aufbauen möchte und, dass ich hier Fuß fassen möchte. Wenn das nicht funktioniert muss ich eh gehen. (f6.2: 258-62)

[lxxxiv] …Charaktereigenzüge, was braucht man für eine Wissenschaft?… Ja, erst einmal großes Frustrationspotential… was Ärgeres gibt's gar nicht als wissenschaftlich zu arbeiten, vom Frustrationspotential…ich kann mir keinen Beruf vorstellen, der solche, solche Hochs und Tiefs hat und solche persönlichen Krisen, solche… Weil's eigentlich nur immer auf deinem Interesse und deinem, deinem Forscherwillen gründet. (f4.1: 963-9)

[lxxxv] Also so ein, ein Sicherheitsjob ist es natürlich nicht wie: okay, ich arbeite jetzt 40 Stunden in der Woche und es kommt sicher etwas raus. (.) Keine Ahnung, wie ein Handwerker oder sonst irgendetwas, der weiß: okay, ich kann Fliesen verlegen, also kommt am Ende was raus. Als Wissenschaftler sagst du auch: ich kann mein, beherrsche meine Methoden und das Konzept, und am Ende kann man trotzdem (.) entweder was Ambivalentes oder nichts Konkretes oder gar nix rauskommen… (m2: 645-51)

[lxxxvi] …das [ist] eigentlich das… was ich machen möchte. Immer (.) wissend, dass es wahrscheinlich gar nicht so einfach wird… und wird wahrscheinlich eh nicht funktionieren. Ich würd's gern machen (lachend)… im Prinzip hab ich immer noch das Gefühl, dass das eigentlich das wär, was ich machen möchte. (m1.1: 36-51)

[lxxxvii] Eins ist sicher: zu wissenschaftlichem Erfolg gehört Glück dazu. Dieser Faktor lässt sich weder in Anträgen noch in (lacht) Dissertationen einplanen. Grad, und das kann man den, den Kids nur sagen. Und wenn du ein Glück hast, dann kannst mit deiner Diss was Tolles machen. Und wenn du ein Pech hast, dann kommt halt ein Doktorat raus. Und sonst halt nicht viel mehr. Mein Gott. Hast halt ein Pech gehabt. (prof_m2: 930-47)

[lxxxviii] Ich glaube man muss erstens interessiert sein an der wissenschaftlichen Forschung, weil es mehr abverlangt als nur diesen 40 Stunden Job. (f6.2: 344-6)

[lxxxix] Aber ich sag jetzt einmal, es ist sind sozusagen langfristige Prospekte, mit denen man geködert wird, also ein bisschen so wie in einer Anwaltskanzlei, wo man dann permanenter Partner wird irgendwann einmal. Damit wird man zu Hochleistungen angetrieben. (m2: 748-52)

[xc] Speziell auch im Urlaub dann natürlich. Bei uns ist es kein Problem. Es ist eher so der Druck, dass man selber sich denkt, okay, wie viel Zeit kann ich mir frei nehmen? Und macht sich den eigentlich selber. Läuft das jetzt so gut, dass ich einen Urlaub machen kann? Oder soll ich eher keinen Urlaub nehmen?… Aber das macht man sich selber, das kommt jetzt nicht von außen, mehr oder weniger. Und das gleiche gilt auch für die tägliche Arbeitszeit, die sicherlich um einiges mehr als 8 Stunden beträgt. (m2: 875-83)

[xci] Auf der anderen Seite, die fehlende Absicherung ist also absolut enervierend. Wenn man nicht weiß, okay, wo ich in ein, zwei Jahren dann steh, ob das Projekt kommt oder so. Also es wär, da muss man halt wirklich dann Vertrauen in sich haben. (m2: 1001-4)

[xcii] Absicherung gibt's überhaupt keine, man kann nur hoffen, dass, sollten die Experimente nicht so erfolgreich sein oder so, so aufregend, wie man sich's erhofft hat, dass einem da der entsprechende Betreuer, das ist bei uns der Institutsvorstand, trotzdem gewähren lasst. (m4.2: 929-32)

[xciii] [Mein Laborleiter] hat eben weil er mich behalten wollt, hat auch irgendwie drei… Möglichkeiten gehabt, wie man mich, wenn ich das Stipendium nicht krieg, irgendwie anders finanziert. Also das wär eigentlich auch nicht schwierig gewesen sag ich einmal. (f4.1: 338-41)

[xciv] …es [ist] mir dann auch leicht gefallen bei ihr Diss zu machen, weil einfach alles gepasst hat und ich wusste, dass wird auch die nächsten drei oder vier Jahre so bleiben. Und ein anderer Aspekt war natürlich, dass [meine Laborleiterin] international sehr bekannt war… und das hatte ich einfach auch den Kongressen schon mitbekommen. Sie kennt sehr viele Leute, viele Leute kennen sie. Also wirklich Top Scientists in unserem Gebiet kennen sie. Und das ist natürlich für eine Karriere immer wichtig. Weil wenn du von einem No-name-Labor kommst, dann beäugen sie dich schon mal kritischer als wenn sie wissen: he, ich kenn den Gruppenleiter, ich weiß sie liefert gute, qualitativ wertvolle Arbeit. Dann hast du's auch schon wieder mal ein bisschen leichter. Obwohl's natürlich nicht dein Verdienst ist. Aber, also es macht's schon leichter und dementsprechend war das natürlich auch von großem Vorteil für mich. (f3: 103-14)

[xcv] …das ist auch ein Aspekt, den wir alle sehr wertschätzen, ist, ich bin danach [nach meiner Dissertation] noch ein halbes Jahr bei… [ihr] im Labor geblieben um Sachen fertig zu machen, [ ], publizieren usw. Und sie hat mich aber vorher fertig werden lassen und hat mich dann eigentlich als Postdoc bezahlt, ja? Alle anderen würden sagen: ‚Ja mach mal fertig, dann darfst deine [Defensio] haben. Und dann gehst.' Ja? Also natürlich als Postdoc kostest du mehr, ja? Und die hat gesagt: ‚Ne, mach mal die [Defensio] und dann zahl ich dich einfach weiter und dann suchst mal Job usw. … Und da hat [sie] mich… auch wieder sehr unterstützt. Sie hat mir eine Stelle angeboten, eine Assistentenstelle und, wo ich auch sagen muss, die meisten… Assistenten wirklich für den Professor arbeiten, ja? Also nicht nur zu 50% oder so, sondern wirklich eigentlich fast 100%… Und das war bei… [ihr] ganz anders. Sie hat gesagt: ‚Du arbeitest jetzt zur Gänze für dich.' … also ich hab hauptsächlich Sachen mal eingereicht, die eher prestigeträchtig sind und wo man nicht so große Erfolgswahrscheinlichkeiten hat. Aber dadurch, dass ich mal [ihre] Stelle… hatte, konnt ich mir den ‚Luxus' – unter Anführungszeichen – leisten, nur mal so was einzureichen. (.) … Das ist, das war mit… [ihr] halt so abgesprochen, dass ich das so mache. (f3: 116-268)

[xcvi] PhD1: Und der hat gesagt, ich soll das Projekt nachher weiterführen was er da hat, und zusätzlich kann ich, also ein weiteres Projekt etablieren, also und, und natürlich also eine Publikation könnte ich also mit, mit dem alten Projekt, dort schauen sicherlich noch ein paar Publikationen raus, wahrscheinlich hoch zu ranken. Und wenn das zweite Projekt zum Grund geht, dann hab ich, schaue dort wahrscheinlich hochrangige Publikationen raus. Ich mein, so ein System find ich meiner Meinung nach einfach, einfach besser. (FGg_jun: 866-72)

[xcvii] PhD2: Ja, das wär dann alles wieder eine Sache der Betreuung. Das… tun sich halt die meisten Leute nicht an, das genau zu überlegen und zu planen, und sich zu bemühen. … PhD3: Ich versteh genau was du meinst… (FGg_jun: 886-92)

[xcviii] Aber es ist selten so, dass eine Person ausschließlich an einem einzigen Thema arbeitet und das muss sie dann bis zum Schluss durchziehen. Also das ist nie der Fall. Sondern es gibt immer Ausweichmöglichkeiten. Es gibt genug andere Themen, und wenn man sieht, da kommt nix raus, dann kann man auch ein bisschen ein sichereres Thema, wo man schon Vorarbeiten hat, wo man weiß, okay, das dürfte was sein, das muss jetzt noch charakterisiert werden oder so. Man kann da durchaus gegensteuern. Wenn man allerdings einen Betreuer hätte, der jetzt, dem das egal ist, und der sich nicht drum kümmert – wie's an anderen Instituten teilweise der Fall ist – dann hat man aber wirklich ein Problem. (f9: 512-21)

[xcix] Natürlich ist es halt so, man macht seine Experimente, wie gesagt, wir haben eigentlich einen ständigen Austausch, und irgendwann mal sagt dann halt der Gruppenleiter: ‚Okay, ich glaub das reicht um es publizieren zu können. Versuchen wir's.'… Also ich verlass mich da ein bisschen auf den Gruppenleiter. Dass der ein Gespür hat, welche Zeitung das am ehesten annehmen würde. Weil ich einfach noch keine Erfahrung darin hab. Also da muss man ein gewisses Gespür entwickeln… mit der Zeit… Aber da verlass ich mich auf unseren Chef. (f7: 1353-415)

[c] … da gibt's dieses Szenario recht oft, dass man irgendwie zwischenfinanziert werden muss, am Institut haben wir die glückliche Situation, mehrere Projekte zu haben, das heißt, da kann man mit dem Geld ein bisschen hin- und hershiften. … Wenn da quasi eine kleine Arbeitsgruppe ist, die keine weiteren Projekte hat und es keine Möglichkeit der Zwischenfinanzierung gibt, dann wird's schwer. … also es hat jetzt bei uns mehrfach die Situation gegeben… wo die Leute einfach aussetzen… eine Kooperationspartnerin von mir ist jetzt mit der Dissertation fertig worden und es ist verabsäumt worden, den entsprechenden Antrag rauszuschicken… wenn man irgendeinen Fortbestand von

Personal sichern will, dann ist wahrscheinlich eine Overhead-Time von einem Jahr oder eineinhalb Jahre, schätz ich mal. Mit einem Kollegen hab ich geredet, der sehr viel Anträge schreibt, der sagt, man muss immer schon eineinhalb Jahre vorher denken, wenn das eine Projekt dann ausläuft, was kommt als nächstes? Also nicht nur denken, sondern schon schreiben." (m4.2: 976-1019)

[ci] Koautoren, das ist ein ganz heikler Punkt, find ich, irgendwo. Es gibt, ich hab z.B. ein Projekt gemacht… wo's eine Triple-First-Authorship gegeben hat. (.) Weil ich mir gedacht habt, okay, diese drei Leute haben… annähernd gleich beigetragen, ja. Und dann erfahrt man aber ein Jahr später, dass… eine dieser Personen… die würd eigentlich gar keine Erstautorenschaft verdienen, ja. Und eine andere von diesen drei Personen… die hat wirklich viel geleistet da. Und dann irgendwann ein Jahr später… kriegt man noch Informationen zugetragen, und dann denkt man, Teufel noch mal, das war damals eine falsche Entscheidung und… Oder auch der einen Person bin ich da mehr oder weniger was schuldig, weil die hat sich da wirklich viel eingebracht, ja. Das ist dann etwas, dann, das tut nachher fast weh, wenn man dann erfahrt, da hat jemand gelitten drunter… wenn man vielleicht mehr Informationen hat dazu, nachträglich sagen würde, Teufel, die Entscheidung, wenn ich's jetzt noch mal treffen würde, würde ich sie anders treffen. Das ist immer blöd. (prof_m1: 1744-58)

[cii] …wenn ich jetzt Leute betreu', bin ich mir auch der Verantwortung bewusst… es ist wahrscheinlich noch nicht so krass [wie] in Amerika, da sieht man, es werden vielleicht Leute oder mehrere Leute auf was angesetzt, und einer von denen schafft's dann und hat eine Top-Publikation. Und die anderen stehen dann schlecht da… Das heißt, wenn wir jetzt einen Dissertanten hab, möcht ich schon schauen, dass der in die zwei Jahre, wo er da ist, oder zweieinhalb Jahre, dass er da eine gute Publikation oder vielleicht zwei gute Publikationen hat, weil ich weiß, das ist dann sein Sprungbrett zum eine gute Postdoc-Stelle oder in der Privatwirtschaft oder wie auch immer. Also, man hat da eine gewisse Verantwortung… man kann nicht sagen, ich hab jetzt da, unter Anführungszeichen, ein paar Sklaven, die für mich arbeiten, und irgendwer wird dann schon was Gescheites herausbringen. Das kommt dann mir zugute… Man hat ja da, hat man natürlich auch eine Verantwortung. (m3: 278-94)

[ciii] Und dann muss man auch schauen, dass man sich eine gewisse Seilschaft oder irgendwelche Gönner, in dem Sinne, hinter sich hat, die für einen das Wort einlegen, gerade in den Bereichen wo es so kurzfristige Verträge gibt und wo man so von der Führsprache von Institutsvorständen und ähnlichem abhängig ist, weil einfach in der jungen wissenschaftlichen Karriere kann man noch nicht von einer Unabhängigkeit sprechen, die wir tatsächlich haben, nicht wie wir arbeiten, sondern wie wir sie tatsächlich haben, ned? [Aber da] braucht man sozusagen die Unterstützung von dem Professorenkollegium, die sagen: Nein, das ist wichtig, dass die Personen noch einmal verlängert werden. Wenn diese Unterstützung fehlt, dann im schlimmsten Fall – was auch vorgekommen ist – die müssen gehen… (f6.2: 351-62)

[civ] The original quote is in English language.

[cv] …es ist schwierig… es gibt… einen Fall, wo in einem Doktoratskolleg… da ist eine Dissertantin schwanger geworden und… die offizielle Meldung war, ihr Vertrag läuft trotzdem aus. Und nur der PI [principal investigator] hat… monatelang… bis hin zum Rektor… durchgestritten… dass sie danach noch das eine Jahr, bzw. die Zeit wo sie weg sein muss hinten anschließen darf von der Finanzierung. Aber eine rechtliche Sicherheit gibt es deshalb keine. (f6.2: 849-56)

[cvi] …ich war in Spanien vor kurzem, das war kein Bewerbungsgespräch in dem Sinn, aber das wär dort eine Möglichkeit für längere Dauer. Und ja, man hat schon so Kontakte… auch innerhalb von Österreich gibt's wahrscheinlich Möglichkeiten, aber nicht da in… [meiner derzeitigen Institution]… zum Beispiel gibt's jetzt die Möglichkeit… [in einer anderen Stadt] das ist vor allem deswegen möglich, weil ich dort mehr Leute kenn, also aus meiner früheren Zeit, und die würden jemanden suchen, aber die können nicht mit Sicherheit sagen, dass das möglich wird, so eine Stelle zu schaffen. Aber sie sind relativ optimistisch. (m1.2: 176-89)

[cvii] Karrieremäßig gesehen ist es wahrscheinlich schon so, wie alle sagen, dass man die Leut packt, also dass man Kontakte hat in irgendeiner Weise. (..) Ja. Bzw. teilweise, ich kanns ja nicht so beurteilen, aber vielleicht einfach die, so eine Art von Frechheit, sich auf alles zu bewerben, was nur irgendwie passt, vielleicht nutzt das was? (m1.2: 330-3)

[cviii] Also das meiste geht nur in Gruppenarbeit. Allein kann einer sowieso fast nie was schaffen. Da braucht man immer Leute, die, die einen müssen… irgendeine Analyse machen, während der sich auf die biochemischen Sachen spezialisiert und so. Also das geht nur in Zusammenarbeit. Das geht alleine gar nicht. (f8: 253-7)

[cix] Ja, man muss irgendwie ein Gefühl dafür entwickeln wann ein Projekt wirklich gescheitert ist und wann man noch weiter machen kann. … es ist der Normalfall, dass nicht alles gleich geht. Und oft einmal muss man sehr, sehr viele Umwege machen bis man zu irgendeinem Ergebnis kommt, das man eigentlich nicht vorhergesehen hat. (f1.2: 549-53)

[cx] Man präsentiert's natürlich regelmäßig in so genannten Labmeetings, und da kriegt man natürlich Input von den anderen Leuten. Das ist wichtig. Aber das bleibt auch wichtig, wenn man schon etablierter Gruppenleiter ist, dass man halt die Diskussion jetzt hat. (f3: 655-8)

[cxi] Und selbst einfach sagen, okay, die Zeit ist fortgeschritten, das, das sag ich jüngeren Kollegen immer wieder, die sich da gern ausnutzen lassen und ich sag's ihnen einfach, sie müssen hingehen und sagen, sie haben sich das so und so vorgestellt, da schaut nichts raus, ob's nicht die Möglichkeit gibt, bei einem anderen Projekt, wie auch immer, und das Ganze quasi irgendwie terminlich festmachen lassen. Sonst dümpelt man so dahin und sechs Jahre sind vergangen, ohne dass man quasi ein zusammenhängendes Thema bearbeitet hat, weil man quasi immer nur einmal da, einmal da eingesetzt worden ist oder so, das gibt es schon. … Bei uns ist es so, dass, dass eigentlich jeder, jeder selbst was machen sollte. Das sagt einem zwar niemand, aber da ist man gut beraten. (m4.2: 883-91)

[cxii] Und weil man sicher sein möchte oder wenn's vor allem ein High Impact Journal ist, dann schickt man's auch noch… zu anderen Peers, also Kollegen auf der gleichen Ebene, meistens außerhalb des Campuses dann. Also sprich Exkollegen oder Leute die man auf Konferenzen… getroffen hat… oder Exkollaborateure, und bittet sie halt um vertrauliches Behandeln, und was sie sagen würden. (m2: 307-12)

[cxiii] …wenn man ein ganzes Wochenende irgendwo hinfahren will, dann muss man schauen, dass irgendwer für einen die Wochenendschicht macht und so weiter und das ist natürlich nicht so eingeteilt wie bei einer Firma, das dann auch abgegolten wird, sondern da kommen die Leute, ja, gratis und… du musst quasi von einer Kollegin oder von einem Kollegen die, die wertvolle Zeit am Wochenende, die nicht abgegolten wird, abschnorren. Das ist natürlich ungut und es funktioniert bei den meisten auf persönlicher Basis dann schon, weil das sind quasi Gefallen, die man sich gegenseitig macht… (m4.2: 767-74)

[cxiv] Konkurrenz mit anderen Labors gibt's natürlich immer. Vielleicht nicht beabsichtigt von Anfang an… Also es kommt immer auf's Feld drauf an. Wenn das Feld groß genug ist, dann wird es sicherlich Konkurrenz geben, weil es gibt dann meistens eine brennende Fragestellung in dem Gebiet, und wer die beantwortet, der ist im Moment einfach top und hat es geschafft. Das wird's immer geben. Wenn das Feld ein bisschen kleiner ist, dann ist es meistens so, dass die Leute sich untereinander sehr gut kennen… und sie teilen sich das eher kameradschaftlich auf. Sag ich einmal. Oder es ist jemand in einer Position, wo er das einfach schneller lösen kann oder besser lösen kann. Und da wird der andere sich denken: ja, konzentrier ich mich auf etwas anderes eher. (m2: 490-501)

[cxv] Manchmal kommt's mir so vor, dass die Leute ihre Kompetenzen zurückhalten, deswegen wirklich nicht weitergeben wollen, dass sie sich eine eigene Position schaffen innerhalb der Gruppe, wo sie unverzichtbar sind. (.) Das ist mein persönliche Meinung, und grad beim Georg, das weiß ich, wirklich. Also das ist unumstritten. Weil ich hab ihn 100.000 Mal gefragt, ob er mir das bitte erklären, damit ich's selber weiß, damit ich das selber mach. ‚Na, nein.' … ich möchte seine Kompetenzen überhaupt nicht hinterfragen. Nur Wissensweitergabe passiert nicht. Und ich glaub, muss ich ehrlich sagen, [das] passiert bewusst. Weil wenn ich mich für eine Kompetenz in irgendeinem Fachbereich in einer Gruppe so abschotte von allen anderen… dann bin ich natürlich… unverzichtbarer… Hat aber nix mit Universität zu tun, muss ich ehrlich sagen. (FGk_jun: 257off)

[cxvi] Das kommt sehr drauf an auf das PhD Programm in dem die Leute sind. In dem DK Programm… gibt es… Committee Meetings, wo man… die Ergebnisse diskutieren soll und das soll ein helfendes Forum sein, wo Vorschläge gemacht werden: Dieses Projekt wird jetzt gestoppt, dieses wird nicht gestoppt, an diesem Projekt wird schon so lange herumgedoktert und es kommt nichts heraus… in welche Richtung entwickeln wir uns weiter. … Es gibt auch, sozusagen jede Person hat einen Mentor eigentlich, mit dem sie auch über diese Probleme sprechen kann. Der erste Ansprechpartner ist natürlich die betreuende Person, die auch Interesse hat, dass etwas weiter geht. Aber wenn aus irgendwelchen Gründen die Interaktion nicht so smooth ist, dann gibt es eben… diesen externen Mentor noch, der halt auch innerhalb der Universität ist, dass er auch versteht wo die Probleme tatsächlich liegen. (f6.2: 761-76)

[cxvii] Ja, das hängt wieder auch davon ab wie du in der Gruppe gestellt bist. (f4.2: 858-9)

[cxviii] Und dementsprechend ist auch das Umfeld sehr super. Weil ich hab genau die Leute mit denen ich (.) arbeiten kann. … für mein Dissertationsprojekt, ja, ist es wichtig, dass ich in einem wissenschaftlichen Umfeld bin, dass ich zB Martin und Karin habe und auch… [der Laborleiter] der mir gewisse Sachen macht. Und ohne dem, wenn ich irgendwo anders wär und das Thema hätte, wüsst ich nicht, ob das wirklich umsetzbar wäre… (f4.1: 393-763)

[cxix] Dann habe ich die Kritiken genommen, habe die noch einmal überarbeitet und habe das nachher an meine Kollegen ausgesandt und dann kam genau nichts retour… Also die Situation ist gleich: ich bin Alleinarbeiter… ja. (.) Mit meinem Chef habe ich glaube ich schon seit einem dreiviertel Jahr nichts geredet. (f4.2: 170-4)

[cxx] …und das ist halt ein bisschen unsicher von… der Seite her, dass du halt Probleme die… kommen, nicht… weißt, dass du fähig bist… sie zu lösen alleine, ja? Ich meine, ich habe… immer einen meinen Joker… [ein Forscher aus den USA] den frage ich immer… (f4.2: 137-41)

[cxxi] Aber eigentlich schließt es total an [der Arbeit einer Postdoc] an. Im Prinzip. Darum, darum hab ich eigentlich mittlerweile irgendwie die Panik, dass ich da nicht selber irgendwas komplett arges, neues mach, weil das sie eigentlich schon teilweise abgehandelt hat, ja? Aber… ich versuch das einfach zu erweitern und zu schauen und… zu testen. Also es ist jetzt nicht so komplett, dass ich nix Neues in der Wissenschaft machen werd, aber, aber es schließt schon sehr an. (f4.1: 387-93)

[cxxii] Und das ist halt wichtig, dass man sich… abgrenzt. Und ich hab die ersten Projekte halt zugeschrieben, dass die schon ein bisschen anverwandt waren mit den Dingen die ich halt vorher gemacht hatte. Weil natürlich, das waren Sachen wo ich auch schon publiziert hab dazu. Und wenn ich jetzt einen krassen Strich mache und sag: ich mach jetzt ganz was anderes, dann sagen die Leute: Hey, du hast keine Expertise, keine Vorarbeiten. … Aber halt so als Longterm-perspective hab ich mir dann ein neues Modellsystem… gesucht, und hab ich mich halt eingelesen und ja. Jetzt hab ich dann auch gefunden. (f3: 237-46)

[cxxiii] …das war… ein Projekt für zwei Jahre… ich hab schon mal sozusagen gewusst, ich kann, also, ein Jahr… Forschung betreiben… oder eineinhalb Jahre, und dann kann ich schauen, dass ich… wieder neue Geldsourcen für… mich… aufzutreiben… Es ist ja gut, wenn eine gewisse Competition da ist, aber man muss eine gewisse Kontinuität haben, dass man so was ähnliches hat, wie einen Tenure-Track oder irgendeinen… ich kann da forschen, und ich möchte weiter forschen, und ich werd nicht alle zwei Jahre ein halben Jahr lang damit verbringen einen neuen Antrag, um meine Position zu sichern, irgendwie zu machen, ned. (f6.1: 335-48)

[cxxiv] Es wäre vielleicht im Rahmen möglich gewesen, nur… dann hätte ich wieder keine Dissertationsstellen. Es ist so schon schwierig gewesen die halbe Stelle zu bekommen, weil ich ja sozusagen als Postdoc mehr verdiene als ein Dissertant… (f6.2: 49-53)

[cxxv] …mit einem halben Gehalt mach ich nicht weiter… (m1.2: 32f)

[cxxvi] Das Risiko ist… sehr groß. … das [eine Projekt] ist sehr risky,… sehr risky. – (.) ich hoffe, dass was rauskommt und aber sozusagen auch, dass ich mit kleinen Teilprojekten davon, dass ich kleine Teilziele formulieren kann, die trotzdem sozusagen zu einer Publikation führen können. Ich hoffe, dass ich so dann so… einen gewissen Output auch zusammenbringe, ned. (.) Ohne, dass ich dann Tag und Nacht hier bin und irgendwann dann vom sechsten Stock… den Kurzweg runternehmen muss, ned. (lacht) (f6.1: 469-77)

[cxxvii] Ich glaube ich habe zu riskante Projekte gehabt. Was ich heute versuchen würde zu ändern. Es war damals sehr schwierig, dass ich, weil eben keine Vorarbeiten da waren… ich bewege mich in das Gebiet, dass ich das besser mischen kann. Also von Beginn an sozusagen, mit diesen wenigen Vorarbeiten die wir hatten, wäre es taktisch nicht klug gewesen. Aber es war nicht anders möglich. … Riskante Forschung ist, weil es spannend ist und sichere Forschung, weil das die Cash-Cow ist. … Damit kann ich sicher publizieren und wenn ich Publikationen habe, sind erstens einmal die Mitarbeiter nicht frustriert, ich selbst nicht frustriert und ich kriege wieder Projektgelder rein, damit ich was arbeiten kann. (f6.2: 520-48)

[cxxviii] Viele Leute fahren zweigleisig. (f1.2: 288)

[cxxix] Vor allem wenn man z.B. eine Dissertation macht oder so was, dann sollte man… soll zumindest ein Teil davon ein halbwegs sicheres Projekt sein, damit es ein Ergebnis gibt für eine Dissertation. (f1.2: 288-91)

[cxxx] Man kann auf eine gewisse, wenn es ein gewisser Finanzierungsrahmen ist, dann weiß man, das wär ein interessantes Projekt, aber das ist mit dem Geld nicht machbar. Gerade auch NMR-Spektroskopie ist sehr geldintensiv und man kann sehr interessante (.) Kombinationen machen… Und dann muss man einfach schauen, wie viel kann man beleuchten, wie viel darf man [labourn],

wie, für, wie viel darf ich beobachten, damit ich's auch finanzieren kann, ne. Also, das wird sehr angepasst, weil's nicht anders funktioniert. (f6.1: 988-1000)

[cxxxi] Muss nicht sein, dass es sich negativ auswirkt, aber die, mir kommt vor, also ich merke es bei mir selber auch bei den Gedankenprozessen. Dass man einfach nicht die Freiheit hat und sagt, okay, das wär jetzt ein Projekt, das über längere Zeit geht und wo man Kooperationspartner hereinholen müsste… das kann ich dann vielleicht, wenn ich auf mich selbst gestellt bin, als Projekt ausschreiben, das macht ja auch durchaus Sinn, aber so, wie es zur Zeit ist, wo ich überhaupt nicht weiß, wer mein Gehalt zahlt… nach 2011, das ist halt (m4.1: 1289-95)

[cxxxii] Vermeiden, ob ein Experiment schief geht oder nicht, kann man nicht… die Lösung dafür ist eben versuchen sozusagen… sich selbst eben über die Finger zu schauen, kann ich meinen Ergebnissen trauen, die ich produziere, oder ist das Ganze insgesamt zu schwer für mich. (f6.2: 740-8)

[cxxxiii] Das heißt, das ist nicht zu 100% Job, wie ich vielleicht einen anderen Job sehen würde, sondern (.) es passiert schon einmal, dass ich privat auch, das passiert recht oft in letzter Zeit, weil ich gerade Dissertation schreib, dass ich privat dann auch (schmunzelt) lese einfach. Einfach, wenn man zuhause ist, also, wenn man das auch als Arbeitszeit rechnet, dann ist die Überstundenbalance wirklich brutal. (m4.1: 420-5)

[cxxxiv] Also entweder Glück oder Fleiß eines von beidem. (m1.2: 367-8)

[cxxxv] …in den Projektantrag muss man einfach irgendwas reinschreiben was, was sexy ist. Was, irgendein Schlagwort, das muss in irgendwas, muss Krebs oder HIV drinnenstehen oder irgendwas Anwendbares, obwohl wir eigentlich weit von jeder Anwendung entfernt sind. Wir machen wirklich, wirkliche Grundlagenforschung. (f2.1: 148-52)

[cxxxvi] Ich würd ein anderes Projekt beantragen, aber die Idee weitermachen… Also das ist jetzt überspitzt ausgedrückt… Offiziell arbeitet man an einem Projekt und arbeitet wirklich am Projekt, das man dann, an dem anderen Projekt, das man beantragt hat, aber dieses eigentliche Projekt, das damals abgelehnt wurde, kann man ja weiterführend machen. (m1.2: 891-6)

[cxxxvii] Das probiert man noch mal wo anders, oder man probiert es in einer bisschen besser verkaufter oder verpackter Form noch mal beim FWF, oder man schreibt was anderes und macht trotzdem das Projekt. (f1.2: 724-6)

[cxxxviii] …dass ich eine Sache ins Projekt reinschreibe die fast schon fertig ist oder so was. Das macht man nur… das ist sehr, sehr üblich, dass – also nicht nur bei uns sondern auch in Amerika – dass man ins Projekt reinschreibt, das was man eigentlich schon gemacht hat. Oder zumindest einen Teil davon. Damit kann man dann sagen: ok, da gibt es eine Publikation – relativ früh. (f1.2: 314-9)

[cxxxix] …dann gibt es noch ein paar so Nebenprojekte, wo man, was man nicht absehen kann. (m1.2: 682-3)

[cxl] …freies Projekt… (m2: 455-7)

[cxli] …also ich bin im Moment sehr am zweifeln was ich tun soll in Zukunft, weil mein [Stipendium] läuft noch ein halbes Jahr. Und wahrscheinlich werde ich noch… um ein [anderes] Stipendium ansuchen… um das noch ein paar Jahre fortzusetzen und… zu überlegen [welchen Job ich in Zukunft möchte. Und dann hatte ich dieses Bewerbungsgespräch]… Und es war relativ interessant eigentlich… weil… es wäre recht gut gelaufen… das ist etwas, was ich als Back-up jederzeit machen könnte. (f1.2: 27-85)

[cxlii] Das ist mir scheißegal. Weil ich denke mir, es ist meine Dissertation, es muss da auch nichts heraus kommen, ja? Vor allem, ich habe ja meinen Lebensweg schon – wie soll ich sagen – ich habe mich ja schon entschieden, dass ich aus der Forschung raus gehe, dementsprechend ist es mir komplett scheißegal, ob da ein Paper… – weißt du was ich meine? – ich brauche es nicht. Natürlich, wenn du… wenn du da drinnen bleiben würdest – ja? – da hättest du eh schon die Krise so mit: verdammt, ich habe jetzt drei Jahre gearbeitet und da ist noch nichts heraus gekommen, und vielleicht kommt aber dann auch nichts, überhaupt nichts heraus. Also das ist sicher… Ich glaube, da tätest du schon die Krise kriegen. Ich möchte nur mit der Diss fertig werden, darum ist die Krise nicht so groß. (f4.2: 134-53)

[cxliii] Das ist eine Umstellung, die ich im Kopf machen müsste, aber ich glaube nicht, dass ich groß, gedanklich was lernen müsste… außer eben die Skills die dort benötigt werden. Es ist ja genauso wie wenn ich hier in ein ganz ein anderes Projekt wechseln würde, gibt es auch neue Dinge die ich lernen muss. Also ich würde es mir zutrauen, ned? (f6.2: 468-72).

[cxliv] …hie und da kommt irgendjemand – wie heißt das – Career Lunch oder so was Ähnliches. Da kommt jemand der redet… der berichtet aus seinem Leben was er gemacht hat… Aber ich würde das

nicht als Beratung auffassen in dem Sinn. … aber ich habe selten die Zeit, da wirklich hinzugehen. Da koche ich mir lieber mein Süppchen. Vielleicht ist… das ist wahrscheinlich ein Fehler. Also wenn man glaube ich in so einer Struktur weiter kommen will, müsste man solche Sachen machen. Coaching und Networking und Mentoring und lauter solche Dinge. Das würde wahrscheinlich besser gehen. (f1.2: 755-73)

[cxlv] Also, aber was Genaues – ich mach mir erst Gedanken wenn's so weit ist. Ich bin kein Mensch, der so viel voraus plant, weil man wird meistens enttäuscht. Das heißt jetzt wart ich mal, weiß ich nicht wie lang's jetzt dann geht mit der Diss, wie lang sich das hinauszögert, ob's jetzt wirklich so aus ist wie's Projekt aus ist oder, das weiß man einfach noch nicht. (f8: 299-303)

[cxlvi] Oder man denkt sich auch: wurscht, wenn ich jetzt nicht Kinder krieg, dann krieg ich sie nie. Und sich absichern hin oder her. (m2: 996-7)

[cxlvii] [Die] Universität sollte quasi der Apparat sein, der einen soweit unterstützt… dass man seine Forschung durchführen kann… [Forschung] heutzutage läuft eh immer über Projekte ab und die meisten Leute… finanzieren sich eh selbst, und das ist eh schon schwierig genug… aber dass dann die Universität auch noch sagt, na, nach sechs Jahren, tschüss, egal, ob du dich weiter finanzieren könntest oder nicht, das ist schon sehr kontraproduktiv. (..) Und ja, also das ist von mir ein klarer Kritikpunkt an der [Universität]. (m4.2: 1301-8)

[cxlviii] …ich habe auch gemerkt wie ich selbst schwanger war, dass man manchmal sozusagen vom Personalbüro, von der Abhandlung, manchmal hat man das Gefühl, man ist die erste Frau die schwanger wird (lacht)… da wissen sie nicht wie das geht, da wissen sie nicht wie die Rechtslage ist. Also es ist wirklich sehr kompliziert dann eigentlich. Also es ist sehr bürokratisch aufwändig für etwas das eigentlich ganz einfach zu gehen, gehen könnte, ned? (f6.2: 861-7)

[cxlix] Ich glaub die machen das schon richtig. (.) Ich glaub das ist okay. Das ist sicher für den… wissenschaftlichen Fortschritt ist es gut, wenn's immer neue Leut [gibt]… und… praktisch nicht langfristige Verträge… [gegeben werden] und nur manche verlängert werden… Dass es für uns jetzt in, in dem Fall mit, mit Familie nicht passt, das ist im Prinzip unser Problem. Also (.) das, auf das können die wahrscheinlich keine Rücksicht nehmen… Weil ich glaub dass die, die richten das schon gut, ja. (m1.1: 958-68)

[cl] Bin ich mir aber nicht sicher, ob das eine Universität, ein Staat lösen kann, oder ob das [Problem] nicht jeder für sich lösen muss. (f2: 1034-6)

[cli] Da gibt man halt irgendwann Erfahrungsdummywert an, und dann -. Tatsächlich muss der arbeiten und schauen, was wirklich geht. Es gibt manche Experimente, die können in einer Woche gehen, sie können aber auch ein halbes Jahr dauern. (f6.1: 978-81)

[clii] …die Fördergeber sagen, sie wollen, dass das Projekt sicher durchführbar ist… die wollen schon soviel Vorarbeiten, dass sie ein Projekt nur fördern, das mehr oder weniger sicher funktioniert… wenn es eben fehlt an den Vorarbeiten, muss man ganz einfach versuchen, wenn die Idee so toll ist… Finanzierung aufzutreiben über andere Mittel, oder im Rahmen eines anderen Projektes, dass man Vorarbeiten produziert… (f6.2: 933-9)

[cliii] Eher stoische Typen… diese hohe Frustrationstoleranz braucht [man]… Die anderen, die nicht so stoisch sind, kommen meistens nicht so weit. Weil die werfen die Flinte häufiger ins Korn. (f9: 1597-9)

[cliv] I: … du also versuchst momentan auch eher so sichere Sachen zu machen, oder? Antwort: Zur Zeit auf jeden Fall, weil die Zukunft so ungewiss ist… (m4.2: 290-3)

[clv] …weil der Vizerektor sich halt gefragt hat, warum sollte man Leute unbefristet stellen, wenn sich deren Chef woanders hinorientiert. Und das war das Problem, und das ist halt auch die Sache, warum man, warum ich gehemmt bin, längerfristige, weitersichtige Projekte zur Zeit zu planen am Institut, weil das ist eben nicht so richtig klar, wie es weitergehen wird… (m4.2: 410-5)

[clvi] Prinzipiell find ich aber, dass quasi die Arbeitsbedingungen auf, grade auf der Uni, wo noch Platz für Grundlagenforschung sein sollte, es so sein sollte, dass man den Mut hat, risikofreudige Projekte anzugehen, weil das sind oft genau die Durchbrüche, die teilweise auch gebraucht werden in der Forschung… und wenn man von der Arbeitsumgebung her quasi dazu gezwungen ist, sich die, wie heißt es so schön, low hanging fruits, (lacht) die Früchte, die man am leichtesten pflücken kann, runterzunehmen, dann sind es halt auch die offensichtlichsten und dadurch auch wissenschaftlich vielleicht nicht so interessant oder relevant. (m4.2: 275-83)

[clvii] Man fängt was Neues an, und man findet in unserem Fall, neue biologische Zusammenhänge, Erklärungen, deshalb machen wir ja das Ganze. (f6.2: 557-8)

[clviii] …habe ich eigentlich hauptsächlich Theorie gemacht… Es ist zwar das unsicherste Thema meiner Diss aber das, was ich am meisten interessant finde. (f4.2: 117-21)
[clix] Es hat jeder seine eigene Spielstrategie bei so was. Interessanterweise… in letzter Zeit ist mir aufgefallen, dass meine Leute dazu tendieren, sogar das Risiko zu nehmen, nur damit sie eine super Publikation zustande bringen… und lieber nix Kleines machen, sondern… Wobei man normalerweise das Gefühl haben müsst, er müsst… sich lieber sicher [fühlen]… Ja? Das ist eigentlich, das ist erstaunlich, weil das hat sich, das hat sich zumindest bei mir verändert, dass sie auf das höhere Risiko gehen. (LCQprof_m4: 969-76)
[clx] Zweitens ist für mich immer auch das Team wichtig… dass es jetzt nicht allein an dir hängt, ja, sondern auch wesentlichen Teils deines wissenschaftlichen Umfelds, ja? Wirklich zum wesentlichen Teil. Weil du kannst ein brillanter Kopf sein, der aber… diverse Sachen nicht macht, also du bleibst trotzdem ein brillanter Kopf auf deinem, auf deinem Gebiet, aber du hast nicht die Möglichkeiten sie umzusetzen. Zweitens auch das wie, Diskussionen die dich nachher zu irgendwas anregen oder so. Das ist, das ist wirklich wichtig… (f4.1: 729-33)
[clxi] …im Nasslabor… es gibt eine Technikerin zB. Die ist immer drinnen und weiß immer wo was ist. Es gibt… [diese] Postdoc… die ist auch viel im Labor und weiß auch sehr viel. Die organisiert sehr viel im Labor. Also das sind so die Ansprechpersonen, wenn man im Labor irgendwas machen will. Die dritte ist eigentlich [diese andere Postdoc], die Proteine für mich schon gemacht hat, relativ viel, die aber… [auch im anderern Teil] arbeitet. Also die ist irgendwie so ein bisschen übergreifend. Aber die macht sehr, sehr viel im Labor. … Es gibt Leute, die machen mehr so [den einen Teil unserer Forschung] und… da gibt's Leute die sind mehr direkt beim [Laborleiter], und dann gibt's ein paar Leut, die sind… eher beim [senior Postdoc]… Und dann gibt's uns, wir sind ein bisschen… [der andere Teil unserer Forschung]… dann gibt's (.) die [Dissertantin], das ist, die ist eigentlich viel [mit uns]. Das ist ein bisschen mehr geteilt. Dann gibt's den,… der macht Diplomarbeit bald fertig. Dann gibt's… diese Dissertantin… (f1.1: 328-49)
[clxii] Aber ich glaub auch, dass die [beiden Postdocs] wollen auf sicher jeden Fall ihr Wissen weitergeben. Aber es ist halt momentan einfach, dass es die zwei zerreißt mit dem Kind. Jeder muss sein eigenes Projekt schauen, weil die Leut sind auch 37 und laufen jetzt noch auf einem FWF-Projekt. Und was tun sie danach? (FGk_jun: 2670-4)
[clxiii] Also so richtige, wirklich, so richtige Zusammenarbeit, da bin ich wirklich tief davon überzeugt, so richtige Zusammenarbeit, innerhalb der Arbeitsgruppe, gibt es nicht. (FGk_jun: 576-85)
[clxiv] Genau. Weil jeder ja auf seinem Feld vorwärts kommen… im Endeffekt interessiert sich ja jeder in erster Linie für das, was er selber macht… Weil, man will ja was zusammen kriegen. (LCQm6: 504-10)
[clxv] …trotzdem, hab ich das Gefühl, dass man in der Akademie noch trotzdem eher (.) forschen kann an interessanten Dingen. Man muss das halt dann geschickt verpacken, damit's (.) bei einem Antrag so klingt, dass es jetzt wirklich medizinisch unmittelbar relevant ist… Natürlich muss man das dann geschickt verkaufen. (m4.1: 644-53)
[clxvi] Aber so die Festlegung der Koautoren, die wird sicher von mir gemacht, aber natürlich schon in einer gewissen Absprache… ich bin da eher so, dass ich… lieber mehr Leute auf ein Paper raufschreib, weil ich mir sag, mir ist wichtig, dass die Kooperation funktioniert. Ich meine, es gibt diese Requirements of Coauthorship, ja, und da muss man ganz ehrlich sagen, die sind manchmal so streng, dass man viele der Leute runterstreichen müsste. Nur, dann werd ich sicherlich niemanden mehr haben, der in Zukunft mit mir kooperieren wird… Also, dort muss man ehrlich sagen, da verstoßen wir wahrscheinlich gegen diese Regelungen, aber dann könnten wir die Forschung nimmer machen… (prof_m1: 1716-27)
[clxvii] …da muss er einfach irgendwo den Brückenschlag dafür finden und einen Spagat und manches Mal auch ein bisschen artifiziell suchen, ned. Aber das ist einfach ein Teil des Spiels dabei. (f6.1: 1037-9)
[clxviii] Man muss eigentlich schon damit angefangen haben. (m1.2: 925)
[clxix] Und den Rest muss man halt dann riskieren und hoffen, dass es nicht so schlimm bewertet wird. … wenn man vielleicht dann doch ganz was anderes gemacht hat. (lacht) Was man normalerweise tut. Also bei uns zumindest ist es – eben dadurch, dass es nicht so vorhersehbar ist was man macht – muss man halt von den sehr detaillierten Projektplanungen, die man den Gremien präsentiert, immer abweichen. (f1.2: 319-330)
[clxx] …mach was anderes – habe ich immer gehört. Habe ich mir gedacht… ich habe ja… ein definiertes Projekt, da kann ich nichts anderes machen. Und überhaupt interessiert mich das – warum soll was anderes machen? (f4.2: 217ff)

[clxxi] Wir haben natürlich einen Chef, der, der komplett anders ist, na?… [unser Laborleiter] hat da sicher auch sehr, sehr großen Einfluss… weil er den Leuten kompletten Freiraum lässt. Im Prinzip, ja? Außer wenn er dich grad zusammenscheißt. (FGk_jun: 2395-9)

17. Abstracts

17.1. German Abstract

Wenn junge ForscherInnen in den Lebenswissenschaften von ihrem Leben und Arbeiten sprechen, erzählen sie auch von einer Reihe an miteinander verwobener Unsicherheitserfahrungen. In ihren epistemischen Lebenswelten verbinden sich Unvorhersehbarkeiten in Forschungsprozessen mit einer hohen Motivation unter Bedingungen flexibler Arbeitsverhältnisse, spezifischer Karrieremodelle und sich verändernder, teils uneindeutiger Erwartungshaltungen. Diese Dissertation analysiert solche Unsicherheitserfahrungen in akademischen Kontexten und beleuchtet dabei Artikulationslinien zwischen ihnen und breiteren gesellschaftlichen Bedingungen und Veränderungstendenzen.

Ausgehend von vorwiegend qualitativem empirischem Material (Interviews und Gruppendiskussionen) folgt diese Arbeit einem sehr breiten Begriff von Unsicherheit, der verschiedene Formen von Unvorhersehbarkeiten, Uneindeutigkeiten, Abhängigkeiten und Spannungsverhältnissen umfasst. Unter diesen Vorzeichen werden Unsicherheitserfahrungen junger LebenswissenschaftlerInnen als eine Form der Prekarität („embodied anxiety“) gefasst: einer generalisierten Erfahrung von Unsicherheit, welche ForscherInnen nicht ursächlich auf einen einzigen Umstand zurückführen, sondern welche sie als Folge einer Konvergenz von Bedingungen empistemischer Unsicherheit und subjektivierter Tätigkeit mit bestimmten strukturellen Bedingungen beschreiben. In diesem Kontext neigen epistemische und andere Unsicherheiten dazu, in einem existenziellen, verallgemeinerten Unbehagen oder einer verallgemeinerten Angst zu kulminieren, die hier als „embodied anxiety“ konzeptualisiert wird.

Die Analyse der Wege, mit denen junge ForscherInnen individuell und als Teil ihrer sozialen Zusammenhänge mit „embodied anxiety“ umgehen, zeigt, wie sie dadurch bestimmte Freiräume finden, sich eröffnen und gestalten. In diesem Sinne erarbeiten sich junge WissenschaftlerInnen den Rahmen alltäglicher Entscheidungs- und Handlungsspielräume teilweise selbst und tragen so zu einer Transformation ihrer Forschungskulturen bei. Weiters wird reflektiert, dass das Leben, Arbeiten und Umgehen mit generalisierten Unsicherheitserfahrungen auf implizitem Wissen und impliziten Fähigkeiten beruht und daher als verkörpert verstanden werden muss. Die Art und Weise, wie junge WissenschaftlerInnen lernen in akademischen Kontexten zur Veränderung von Arbeitskulturen beizutragen, wirft Fragen für die Gestaltung gegenwärtiger Wissensgesellschaften und -ökonomien auf.

17.2. English Abstract

When young researchers narrate their living and working in the academic life sciences, they also narrate a complex web of uncertainty-experiences: In their epistemic living spaces, contingencies of the research process pair with high motivation under conditions of employment uncertainty, very specific career models and changing and ambiguous expectations resting on them. This thesis analyses such experiences and sheds light on how they can be understood as linked to broader societal conditions and currents of transformation.

Largely building on qualitative empirical material (interviews and group discussions) this thesis follows a broad notion of uncertainty that includes unpredictability, ambiguity, dependency as well as tensions. Building on this understanding, it conceptualises uncertainty-experiences of young life scientists as a form of precarity (embodied anxiety): a generalised experience of uncertainty, which researchers do not trace to a singular cause but that describe as resulting from a convergence of conditions of epistemic uncertainty and subjectified activity with other structural preconditions in their academic environment. In this context, epistemic and other uncertainties are likely to be experienced as social risk or existential angst, conceptualised here as embodied anxiety.

Analysing how researchers individually and as part of their social relations cope with uncertainty-experiences, this thesis then describes how – while doing so – they find, create and shape spaces of freedom, negotiation and possibility. In this sense, young researchers contribute to the transformation of their research cultures from below. Further, it reflects how living, working and coping with generalised uncertainty-experiences builds on tacit knowledge and skills and can thus be understood as embodied. This thesis therefore contributes to the debate of whether and how researchers contribute to the transformation of their research cultures on the level of everyday tacit learning processes and decision-making procedures. Finally, it discusses how researchers' experiences of learning to manouvre in their epistemic living spaces might relate to questions of how to shape our present knowledge societies and economies.

18. Curriculum Vitae

Personal Data

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Educational Background

2006-2012	Doctoral Studies in Sociology (Dr.phil.) at the Dept. of Social Studies of Science/University of Vienna
2010	Guest researcher at the CESAGEN – The ESRC Centre for the Economic and Social Aspects of Genomics in Cardiff/UK
2006-2012	Contract researcher and „prae doc" assistant at the Dept. of Social Studies of Science/University of Vienna
1998-2004	Studies of Nutritional Sciences, History and Philosophy at the University of Vienna; Interdisciplinary Master thesis: *Die Ernährungswissenschaft als Emanzipationsstrategie der bürgerlichen Frauenbewegung*

Collaboration in Research Projects & Fellowships

04/2009-03/2011	*Uncertain Research Landscapes*, Dept. of Social Studies of Science/University of Vienna (DOC-fellowship of the Austrian Academy of Sciences – ÖAW)
01/2006-02/2009	*Knowledge, Institutions and Gender: an East-West Comparative Study (KNOWING);* Dept. of Social Studies of Science/University of Vienna (project funded by FP6 of the European Commission)
09/2007-03/2008	*Living Changes in the Life Sciences: Tracing the „Ethical" and the „Social" within Scientific Practice and Work Culture (Living Changes);* Dept. of Social Studies of Science/University of Vienna (project funded by GEN-AU/bmwf)
08/2006-08/2007	*Re-Thinking Biosciences as Culture and Practice: Tracing "Ethics" and "Society" in Genome Research – a Pilot Study (GOLD II);* Dept. of Social Studies of Science/University of Vienna (project funded by GEN-AU/bmwf)

Teaching Experience

2006-2012 Lecturer in Science and Technology Studies at the Dept. of Social Studies of Science/University of Vienna

Selected Publications

Felt, Ulrike, Lisa Sigl und Veronika Wöhrer. forthcoming. Multiple Ways of Being Together Alone – A Comparative Analysis of Collective and Individual Dimensions of Research in Two Epistemic Fields. In: Science and Public Policy.

Felt, Ulrike, Lisa Sigl und Veronika Wöhrer. 2006. Progress Report to the European Comission; Project „Knowledge, Institutions and Gender: An East-West Comparative Study (KNOWING)“, Project No. SAS6-CT-2005-017617. Available at: sciencestudies.univie.ac.at/fileadmin/user_upload/dep_sciencestudies/pdf_files/pdfs_abgeschlossene_projekte/knowing_sota_report.pdf)

(contributer to) Felt, Ulrike (ed). 2009. Knowing and Living in Academic Research. Convergence and Heterogeneity in Research Cultures in the European Context. Prague: Institute of Sociology of the Academy of Sciences of the Czech Republic. Available at: sciencestudies.univie.ac.at/fileadmin/user_upload/dep_sciencestudies/pdf_files/pdfs_abgeschlossene_projekte/felt__knowing_and_living_in_academic_research.pdf.)