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Disciplining science:

The impacts of shifting governmentality regimes on academic research in the natural sciences in the Czech Republic

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Declaration

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Marcela Linková

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1. Introduction: Institutional change in the scientific domain

> *"Well, one of the reasons our research group left the other institute was that we tried to push an internal assessment system there and there was no will. I guess I was more enthusiastic about these things then than I am today. I think it's a good thing that we came here and that the assessment system was in place here already, that was pleasant. My view of it today is that it's a tax we must pay if we live on taxpayers' money. We have to show some measurable result, and for us it's impact factor papers, there's no debate about that. Only I'm starting to think more and more that creative science, that's something extra. The impacts, a total looser won't hound them but you can get them with pretty standard research. That's the obligation to taxpayers, but towards me, it's to do something interesting. So when I talk to someone, I don't look at what impact factor he has. That's for our director, that's for the grant agency, that's all right. But it's not for me."*

In this account, research assessment is not singular; it is not stable, its contours change. Originally, this highly successful natural scientist was enthusiastic about research assessment, worked to institutionalize an assessment system with his colleagues at his previous research institute and failed. The will was not there. Through other exchanges with him and his colleagues who came in 2004 to his current workplace it emerged that 'the will' refers to various types of fears, fears of a whip being institutionalized, of insufficient performance, of being judged. The research assessment is a device to assess research performance but it is also a moral. It enacts a good, good particular for the natural sciences: impact factor papers published abroad. Between 2004 and 2007 a few things happened, and at the time of our interactions, he was not as enthusiastic about the assessment system as he used to be. One thing that happened was that the research assessment system in place at the new institution got complicated. With the arrival of this group, new epistemic practices and new machines for producing research results arrived, too. And perhaps particular epistemic practices and their equipment in one line of research (computational modelling and clusters) made it possible to produce many results quickly, much more quickly in fact, than different epistemic practices and their equipment in different research streams ('cooking' and other types of experimental bench work). So gradually the research assessment became a source of tension, which surfaced time and again during my fieldwork at the institute. Perhaps then, 'pleasant' refers not only to the fact that this particular institute was serious about performance and

had an assessment system in place but also to the fact that people at the institute could not say that the assessment system was introduced *because of* the new group. The tensions then might have been even bigger. The institute and the teams had to cope with this. But scientists started coping as well, and so another thing that happened was that some people started 'hounding' impact factor publications. Then there is creativity, a different sort of accountability, an obligation to oneself. Lastly, in 2004 the Czech Governmental Council for Research and Development adopted the Methodology for Assessing the Results of Research and Development (the Methodology). With this document, a certain type of research assessment that was previously cloistered at an institutional level travelled to the national policy level, and this was the third thing that happened. An originally clear notion of having a system to compare the work of people at one institute became an instrument to compare all scientists in the Czech Republic.

In this dissertation I analyze research assessment in the Czech Republic as a particular type of regime governing science. I examine how research assessment is done at a particular public research institution, a bioscience institute of the Academy of Sciences, what it brings to light and what it keeps in the shadows, how it is co-constitutive of effects in terms of organization of research work, teams and organizations; epistemic practices; research careers and research subjectivities, and how these processes are gendered. Further, I trace institutionalization of research assessment at the national level and its enactment in science policy documents. Through these analyses I explore the shifting social contract for in the period after 1989, with its attendant justificatory modes at policy level. I have developed the notion of a *dynamic triangle* with the three poles of dynamic organization, dynamic subjectivities and dynamic policymaking, to analyse an *entrepreneurial alignment* of the natural sciences in the Czech Republic in the period since the 2000s.

1.1 The dynamic triangle

The central organizing principle of the dynamic triangle is *competition*. The central argument of my thesis is that these three types of dynamisms, revolving around competition, are crucially inter-related and contingent upon each other, and it is this co-alignment of organizational, subjective and policymaking features that has effected a powerful change in the domain of the natural sciences (cf. Shore and Wright 2000: 61),

which is having effects not only in terms of how the natural sciences are organized today, what is happening with research careers and training but also, and very importantly, with how and what scientists can know. With this study I am contributing to the theoretization of institutional change of academic science, "as part of a necessarily larger effort to grasp the growing interdependence or confluence of previously distinct institutional domains" (Vallas and Kleinman 2008: 290), in this case the domain of scientific knowledge making and the domain of economy.

The concept of the dynamic triangle was formed gradually, as I analysed my research data and worked to make sense of the constitutive elements of the changes under way at the research institute. If Miller and Rose (1990) focus on the role of language as an intellectual technology of power that can bring persons, organizations and objectives into alignment, I focus on material-discursive practices of the entrepreneurial alignment attending the introduction of research assessment and with it growing levels of various types of competitions at the institute. The dynamic triangle then enacts a particular form of social contract for science the evolution of which I will address in chapter 5.

I have conceptualized the three types of dynamism as a triangle to underscore that there is not a hierarchy. I am not after some trickle down effects which would allow me to say something about the yoke imposed by policy on researchers who have to labour under new conditions. The three poles of the dynamic triangle help me to organize my research data in particular ways so that I can attend to the effects of increasing dynamism based on competition and competitiveness in the research system. It is often presumed that there is a causal, downwardly organized link from policymaking to institutions to individuals, that what is adopted at the policy level will organize the work of institutions in particular, planned ways, and that institutions will take care that individuals perform to this effect. Indeed, we see this straightforward expectation in Czech research and development policy, as I will discuss in Chapter 4. Against this assumption, I study how research assessment is enacted in the three poles of the triangle and how these three enactments effect organizational change. The links among these poles are multiple; it is a network of relations, there is no a linear or circular causality. I present here an account of complex coping, where organization, subjectivities and policymaking inter-lock and achieve effects, and the introduction of competition through research assessment is instrumental in achieving these effects.

The shifts embedded in the dynamic triangle can be read as an example of institutional isomorphism (DiMaggio and Powell 1983). Organizations in a structured field, such as science, "respond to an environment that consists of other organizations responding to their environment, which consists of organizations responding to an environment of organizations' responses" (ibid.: 149). The authors characterize institutional isomorphism as a "constraining process that forces one unit in a population to resemble other units that face the same set of environmental conditions" (ibid.), and identify three types of isomorphism (coercive, mimetic and normative) which cannot be necessarily disambiguated. Different types of isomorphism will be relevant to the particular shifts in the three poles of the dynamic triangle. Indeed, the three types of isomorphism are overlaid though each pole of the triangle may see the predominance of one or the other. Importantly, institutional isomorphism affords an examination of organizational power and survival (ibid.: 157), highlighting the role of dependence, centralization, academic credentials, sources of support or viable alternatives in achieving isomorphism (ibid: 154-156).

Furthermore, research assessments are not limited to the institutional and national levels. Other actors in the national and international arena range from grant and auditing agencies to commercial publishers and media organizations, with their own rankings, which may be mirrored in and translated into rankings at the national level, and these also enter the isomorphic process. However, while these other actors indisputably affect the life of individuals and institutions, and often very painfully (as when papers are repeatedly rejected and grants not won), I see these other actors as *accompanying* actors in the dynamism of the triangle. The rise in the importance of publishing houses and journals, grant agencies and various systems of rankings is an effect of the dynamization of the science field through the introduction of a governance regime built on competitive assessment. Although they have been part of the research system for some time, their role and power has increased with the shifts that are subject of my exploration here, at the institutional, subjective and policymaking levels.

My analyses are primarily based on an ethnographic study at a bioscience institute of the Academy of Sciences of the Czech Republic and so it is particular (I will return to this issue in chapter 2 when I introduce my research site and the methodological considerations). My focus on the natural sciences in this thesis is largely given by my data. My second reason is partly instrumental, partly collegial: in the Czech Republic

there are very few people who examine issues of governmentality and public accountability in science, and some of them were involved in the project mentioned above. Tereza Stöckelová has made major contributions to theorizing accountability, research assessment and their impact on the social sciences as well as to theorising the conceptual apparatus of science and technology studies (Stockelova 2012; Stöckelová 2012). The third reason for focusing on the natural sciences is that Stöckelová and I argue elsewhere (Linkova and Stockelova 2012) that natural scientists at the Academy of Sciences played a pivotal role in introducing research assessment at the national level in the Czech science policy. If Garforth and Stockelova (2012) assert that the natural sciences with the lab form the heartlands of science and technology studies as they developed since the 1970s, they also show that they form the heartlands of research assessment both in the UK and in the Czech Republic (Garforth 2012; Stockelova 2012). It is subject of this dissertation to examine whether and how research assessment actually works in these heartlands of the natural sciences. What effects does research assessment have in these disciplines? How is it appropriated and coped with? Is it singular? In the interplay of the three poles of the dynamic triangle I want to put the spotlight on some of the unintended consequences and inconsistencies which, at a time when the word 'excellence' has mushroomed all around, it is in fact quality, the oft recalled trope of science, that may be structurally compromised in the process.

1.2 Background to the study

This doctoral thesis is an outcome of my long-standing interest in governmentality of academic science in contemporary societies and its implications for knowledge-making practices. This interest was first motivated by my work, since 2001, aimed at advancing gender equality in Czech science. Gender equality is done in many different ways, too. And these different gender equalities do not always sit very easily together, as I explore elsewhere (Linkova and Cervinkova 2011; Linková 2011, 2013). Like research assessment, gender equalities are done within different governmentality regimes, which means that what they aim to achieve and what particular gender good is propagated differs (women's special role and skills based on motherhood; liberal political representativity and equality of opportunities; gender equality as a business case

contributing to economic development; a decentred enactment of contextualized, localized, temporary power relations, to list a few).

Very early, this concern with gender equality crossed with concerns about judging quality. The body of scientific literature on gender differentials in attributing merit has expanded significantly over the past two decades, and underscores the many different layers of women scientists' work being evaluated as worse, less competent, than men's.[1] Historian Margaret Rossiter, in fact, postulates the Matilda Effect (Rossiter 1993) in reference to Robert K. Merton's 1968 concept of the Mathew Effect in science, to underscore the systematic devaluation and invisibilization of women's achievements through the annals of science. Increasingly, as I became aware of other concerns with accountability, this issue of gendered attribution of merit started running up against the issue of excellence, research assessment, peer review, and performance indexes which were slowly becoming relevant for evaluating research in the Czech Republic in the first half of the 2000s.

These two concerns first coalesced in a study[2] into the role of science in Czech society, including the importance of gender equality in this vision of Czech science. In a chapter co-authored with Tereza Stöckelová we for the first time discussed two accountability regimes in evidence in Czech science which we called *science as discovery* and *science as enterprising* (Stöckelová and Linková 2006). Whereas interviews with high ranking researchers and research managers showed that they strategically combined both regimes contextually, policy makers and politicians unproblematically worked with the notion of science as enterprising, and stressed the role of science in building Czech economy's competitiveness and the utility of research results in terms of their commercialization and immediate economic profit.

Since then this topic exploded, in two respects: Firstly, the Methodology has become a hotly contested instrument for steering research, development and innovation in the Czech Republic. So explosive, in fact, the 2008 Research, Development and Innovation Reform (Vláda České republiky 2008) and its performance-based core funding

[1] Some of the recent studies include: Marchant, Bhattacharya, & Carnes (2007); Moss-Racusin, Dovidio, Brescoll, Graham, & Handelsman (2012); Steinpreis, Anders, & Ritzke (1999); Trix & Psenka (2003); van den Brink & Benschop (2011, 2012). The first seminal study of gender prejudice in research evaluation is by Wenneras & Wold (1997).

[2] Funding for this research study was provided by the European Commission, contract no. SAS6-CT-2004-003582.

mechanism had been that they stirred a portion of the Czech academic community to organize protests and demonstrations and to form a civil society organization Věda žije! (Science Is Alive!) in 2009. The R&D&I Reform mobilized a portion of the academic community, and so today there are several blogs[3] whose authors comment on developments in Czech science, and particularly research assessment. The Association of Innovative Entrepreneurship CR publishes a journal *Innovative Enterprising & Technology Transfer* where the Reform and Methodology are discussed regularly, from a very different perspective.

And protests and petitions have been organized abroad, too, against metrics-based assessments systems. In 2012 *The San Francisco Declaration on Research Assessment* (DORA) was launched, initiated by the American Society for Cell Biology together with a group of editors and publishers of scholarly journals. Recognizing the need to improve the ways in which the outputs of scientific research are evaluated, the one general recommendation of the Declaration is: "Do not use journal-based metrics, such as Journal Impact Factors, as a surrogate measure of the quality of individual research articles, to assess an individual scientist's contributions, or in hiring, promotion, or funding decisions." (The San Francisco Declaration 2012) The San Francisco Declaration has to date been signed by almost 11,000 individuals and 484 institutions, in addition to the original signatories. In Australia, controversies and protests have surrounded the efforts of the governments to institutionalize a metrics-based ranking system from the start (Linková 2012). The most controversial feature of the *Excellence in Research for Australia* (ERA), the ranking of academic journals, was scrapped in response to massive criticism in 2011. In the UK the protests against the Research Assessment Exercise and the successor Research Excellence Framework come from various quarters. Criticisms centre on the difficulty of measuring impact of scientific efforts outside the academy fairly and without compromising academic freedom and on the system encouraging short-termism. In the USA, UK and Australia 'poaching' of highly productive academics has become commonplace, which gives undue advantage to richer universities which can afford to pay exorbitant salaries though the practice is not in evidence to that extent in university that already perform highly (Gibney 2012;

[3] Daniel Munich at http://metodikahodnoceni.blogspot.cz/, Václav Hořejší at http://blog.aktualne.centrum.cz/blogy/vaclav-horejsi.php and Pavel Jungwirth in Respekt.

Marcus 2012).[4] In the UK and Australia, too, criticisms have been levelled against the financial costs of the assessment systems and the attendant rise of the administrative class in universities and research institutions. The particular critiques related to temporal aspects, classificatory issues and lack of transparency in how the classifications are done speak to a larger concern, the issue of changing the culture of science, where it is argued that the introduction of an assessment systems leads to dysfunctional and perverse reactions which may in the long run threaten science quality (Cooper and Poletti 2011; Vanclay 2011).

Secondly, over the past decade we can see a worldwide surge in studies and academic literature on the changing governmentality of 'research and innovation', as the domain has come to be called, through processes of New Public Management. In the Czech Republic, in response to the controversy surrounding the R&D&I Reform, the Ministry of Education, Youth and Sports contracted a consortium led by Technopolis to audit the Czech system of research, development and innovation, its first task being to review The Methodology. In its First Interim Report the consortium made several comments on the potential consequences of the Methodology, and concluded: "No other system of performance-based research funding allocation known to us and still in use is equally radical in its exclusive focus on the past and in its level of standardisation across different types of institutions and disciplines." (International Audit of Research Development and Innovation in the Czech Republic 2010: 17) The Final Synthesis Report clearly states that the Methodology is "not fit for purpose" and that it "suffers important weaknesses including reductionism, failure to address differences in inputs and outputs or to take account of policy requirements such as the national thematic priorities treating all institutions in the same way, regardless of their missions by using output indicators that are in practice arbitrary." (Arnold 2011) Needless to say, the audit did not resolve the controversy, nor could it have. The Methodology is political in the sense that it imposes a certain vision of science and certain organizational logics. Since

[4] While clearly the quality of the research infrastructure is very important, it is the combination of budget cuts and uncertain career outlook on the one hand and the ability to pay extremely high salaries on the other that combine to lure researchers with a strong publication record.

the claims being made on the Methodology at this moment are multiple and non-coherent, the audit could not have resolved the situation.[5]

The academic literature on the various impacts of research assessment is strongly located in the United Kingdom with its history, since 1985, of the Research Assessment Exercise but has spread since in response to the introduction of metrics-based systems in many countries. Critical studies and criticism from the academic community started to gradually emerge that pointed to the unintended effects of such managerial research assessment, from decreasing research quality as a result of changing publication practices (Anderson 2008; Gillies 2008), inhibiting collaboration (Shore and Wright 2000) to deteriorating psychological and emotional well-being of researchers (Chandler, Barry, and Clark 2002; Sparkes 2007). Others address the epistemic exclusions assessments bring (Roa et al. 2009), changes in academics' subjectivities (Shore 2008) and consider the double-edged vocabularies used to promote the audit culture (Shore and Wright 1999).[6] Some authors recognize an epochal change in the spread of novel accountability technologies in science and higher education (Dunn 2003: 60; Shore and Wright 2000: 57) and set out to analyse these changes in terms of changes in discourses, the rise of new institutions and mundane practices associated with the discourses and changes in professional subjectivities. Rankings have become a global practice, redefining science accountability in terms of quantitative measures (Sauder and Espeland 2009: 80). Analyses abroad focus on the role of various intermediary agencies, the role of academics in the evaluation-at-distance procedures or the rise of the new managerial class in universities. As will be made clear in this dissertation, the range of actors that play an important role in the new system of research governance in the Czech Republic is narrower and the distribution of roles is not so clear cut.

I dare say research assessment is a highly personal, embodied and affective issue for practically all scientists today; for me, too. In retrospect I wonder about acuity of researchers in the Czech Republic when they did not think much of the research

[5] On 29 May 2014 the Ministry of Education contracted the same company, Technopolis Limited, to design an assessment methodology for Czech research and development system in a CZK 40 million public procurement (EUR 1,545, 454).

[6] I do acknowledge that there is a body of literature on the positive impact of auditing practices; however, in my research I have encountered virtually no fully positive endorsement of these practices and if there was endorsement on the part of the research participants, they have mostly 'sobered up' over the last decade. For my part, the positive effects remain elusive. For these two reasons this paper is located in the critical stream of literature on research assessment and audit practices, and I return to this in the final chapter.

assessment being introduced in the Czech Republic in 2004 at the national level or hailed it as a device which will make it possible to separate the wheat from the chaff (some, in fact, continue to hold this position). Even if the Methodology was introduced without any immediate effect on universities and research institutions and their funding, its introduction as such presages a future possibility. In a film if you show a gun, it will fire. And surely, this gun of research assessment fired in 2009 when it became stabilized through the R&D&I Reform (Vláda České republiky 2008) as a device for distributing core funding. This functional stabilization achieved through an adoption of the reform document, however, has not precluded further tinkering and tweaking of the Methodology; quite the opposite. And it does not mean that it has actually been used to allocate core funding as had been planned, either.

Two caveats at the start: I took part in the protests in 2009, and I continued to protest against particular versions of quantitative research assessment that my own research institute decided to draft. Yet this dissertation thesis is not a cry against evaluating scientific activities and it most certainly is not a cry against increased public accountability of science at this time and age. Quite the contrary: by exploring the changing contract for science in the Czech Republic through the introduction of a particular type of research assessment and the impact this has had in the natural sciences, I want to underscore the opportunities as well as limitations in relation to the organization of science. I most certainly do not wish to advocate the state of affairs before the introduction of the assessment when the levels of non-transparency and nepotism may have been arguably even higher, and we still see some evidence of this today.

Secondly, this dissertation is not about the *controversy* surrounding the Czech Methodology for Evaluating Results of Research and Development. And it is not about *how to improve* the Methodology. The Methodology is a particular enactment of a certain type of governmentality regime; even if it is tweaked, and these efforts are ongoing, it will still fall within a particular governmentality regime. It will enact particular goods and relatedly bads, and there is nothing to suggest that at the policy level there is readiness to re-think the particular moral of the current Methodology. Concentrating on the controversy surrounding the Methodology would be distracting and would divert attention away from my core concern, the ubiquity and undisputed

presence of a particular type of 'coercive accountability' (Shore and Wright 2000) in Czech science today and its effects in the natural sciences.

1.3 Conceptualizing institutional change of science

Over the last two decades numerous scholars have pointed to fundamental re-orderings taking place in contemporary universities and research systems. One of the most influential debates revolves around the conceptualization of this shift in terms of Mode 1 to Mode 2 knowledge production developed by Gibbons et al. (1994) and revisited in (Nowotny, Scott, and Gibbons 2001, 2003). Mode 1 refers to the more traditional practice of science, "created within a disciplinary, primarily cognitive context" situated within universities and characterized by a polarization of discovery and application. In contrast, Mode 2 "is created in broader transdisciplinary social and economic contexts" and is based on the principles of convergence and synthesis. The authors analyse Mode 1 and Mode 2 in terms of five key dimensions: context, discipline base, social organization, accountability, and quality control. Mode 1 knowledge production is contextualized in the academic community, organized through academic disciplines, institutionalized in universities and long-standing research teams accountable to peers. Mode 2 knowledge production is open to negotiations with other stakeholders (the state, industry) and problem oriented, transdisciplinary, produced in various organizational contexts and accountable to various interests. Although the authors talk about process, their framework suggests a transition from one mode to another. Furthermore, these authors fail to consider issues of power and hierarchisation. In the 2001 book they unproblematically present the *agora* as public space in which "science meets the public" and in which the public "speaks back" to science (Nowotny et al. 2001: 247). In the agora, the authors claim, knowledge is subjected to testing by the state and the market, not exclusively private or public, and this is where "what will be accepted as a 'solution' is being negotiated." (Nowotny et al. 2001: 247) In their later work the authors concede that they failed to consider power relations (Nowotny, Scott, and Gibbons 2003: 181) but they insist on the agora as a solution to the contested terrain of scientific knowledge production. Their conceptualization has been criticized for homogenizing change and performing a quasi-political function (Felt 2009: 25).

Similarly, Cohen et al. (2001: 152) argue that the emergence of Mode 2 knowledge production as the preferred model is intertwined with political and economic exigencies.

I do not find such a binary conceptualization of the shifts under way in the scientific domain to be convincing. The mere use of a binary conceptualization petrifies the notion of a linear transformation from one point to another. Rather than conceiving of the current changes in science governmentality as a linear, bounded process, it is important to note that "changes in social conditions may determine which type becomes more dominant and which set of values gains greater legitimacy at any given time" (Lam 2010: 332). While some of the recent developments in science may appear to some to be altogether foreign to the traditional ethos of the research profession, changes and shifts do not create fully new values and ethics but rather alter the balance between existing ones. Similarly, Lam discusses "complex dynamics of organizational change that permit the co-existence of contradictory institutional logics" (Lam 2010: 308; see also Law 1994). And importantly, Stöckelová argues, switching between these various regimes is an instrument of management and control, and "potentially also gross manipulation" (Stöckelová 2009: 59)

There are, in fact, increasing numbers of scholars who are concerned with multiplicity of realities and their co-existence, with complexities. These authors recognize that there is not a single order organizing reality. In fact, they claim, there has never been such an order to start with although this has been the dominant notion for the Euro-American locations, the Moderns, as Latour calls them (Latour 2013). If we live in a world where the will to purify and the conditions of possibility for purity are in decline (Law et al. 2013) (as they are in a globalizing world with multiple claims to justice and, in fact, multiple justices), how do we make things matter, how do we organize the world and how do we make it cohere? If there is not a single order but orders, what are the ordering principles and who has the power to order in each?

This recognition raises important questions about how ordering realities occurs in material and discursive practices. Referring to Walzer's Spheres of Justice (1983) Law and Mol are concerned with how good and bad are ordered in various domains of society (such as economy, politics, sexuality and intimacy, science, culture etc.) (Law and Mol 2002: 8). Unlike Walzer who posits that each sphere of justice should stick to its mode of justification and that using other modes constitutes pollution, Law and Mol

are more interested in how it happens that a particular justificatory mode dominates a particular social domain, how different justificatory modes co-exist but are also displaced in a domain and how different justificatory modes travel from one domain to another (cf. also Vallas and Kleinman 2008).

These concerns are also mine. The notion of Mode 1 science revolves around the Ivory Tower where gentlemen of science make objective, rational and disinterested decisions about the quality and contribution of new knowledge to scientific progress. The community of peers engaged in communal, if not fraternal exchange and disputation where one is judged by merit and nothing else has been a very effective justificatory principle in science, one which managed to displace issues of power, exclusion and inequality. It is this professional accountability which has been increasingly interrogated from various corners of science as well as from the Othered "lay" society. But not only from there: increasingly, the professional accountability is interrogated by the market logic. It has been argued that the changes in the governance of science are incursions from the market logic, with its stress on competition, efficiency and application. Ward (2012) locates the shift in the domain of knowledge and education in the rise of neoliberalism, "an intensification of the relationship between knowledge and capital" which does not affect only the quantity of knowledge produced but also its quality (ibid.: 12).

In their analysis of the rise of neoliberal governmentality in the UK's higher education system Shore and Wright (1999: 559) refer to Strathern's (2005) concept of the *domaining effect* by which the conceptual logic of an idea associated with one domain is transposed into another domain with interesting and unpredictable consequences. It could be argued that what we are seeing today in science are various domaining effects, each with its own notion of good, scientific good. Similarly, in their contribution to analyses of the knowledge economy, Vallas and Kleinman (2008) develop the concept of *asymmetrical convergence* to examine the re-alignments in the academic and commercial biosciences in the US.

In line with the theoretical equipment of complexity theory I do not regard the domaining effects and the arising non-coherence in the science domain as a manifestation of uncertainty or disintegration, a temporary accompaniment of the currently shifting grounds. Rather, non-coherence is inherent to reality, always already

complex even if particular actors manage at moments expunge manifestations of non-coherence. Rather than subscribing to a particular regime change theory (such as the already mentioned Mode 1 and Mode 2 or the Triple Helix of University-Industry-Government Relations developed by Leydesdorff and Etzkowitz (1996), I am more interested in exploring what comes to the fore at the interface between and within individual domains. The visible and audible modes of policy justification, organizational practices and subjectivities in the science domain enact a particular value-scape, a particular social contract for science.

1.4 Post-war evolution of the social contract for science

Concerns with the state of science are not new. Thackray (1977) for example explored 19th century debates in the UK about the demise of UK science, just at a time when a tidal wave was picking up for new discoveries and theories at the end of the century. In retrospect, it becomes obvious that it is difficult to assess the state and quality of science at a present moment. What is perhaps more interesting is how such an assessment or measurement is done, what is zoomed in on and what is left out, invisible and irrelevant as a measure or unit of assessment. Analysing how assessments of the state of knowledge making are enacted thus helps us to understand the governmentality regimes in place to organize the domain of scientific knowledge production.

Assessments are always partial; they are performed from the vantage point of what is made visible, of "what is seen (and thus but more indirectly of what is looked at)" (Thackray 1977: 20). The promises that science makes to society are indicative of negotiations of epistemic authority and claims to truth as well as visions of good for society. The traditional notion of science, what Godin (2002: 9) calls "the ideology of the autonomous researcher", is one of a self-governing institution where peers make decisions about the validity and importance of the knowledge achieved. It is a form of professional accountability where "standards are monitored and enforced by professional supervisory bodies on the basis of peer review" (Bovens 2007: 456). According to an argument developed since the 18th century, science acted as a source of intellectual virtues as well as socioeconomic progress, and as such there was no need for governments to examine its output or worry about evaluation (ibid.: 9–10). Science was premised on the promise of future returns, the promise to ultimately "insure our health,

prosperity, and security as a nation in the modern world" (Bush 1945). In Western societies, science policy and measurement thus did not emerge as a way to control research and scientists, but rather as a way to safeguard research funding for basic science. Science and technology measurement was not constituted by economics statistics because it was not at that time concerned with measuring economic goods (Godin 2002: 4). It was the stabilization in the second half of the 20th century of peer review as the system for distributing research funding that further strengthened the autonomy of research and scientists (Guston 2000: 3). Building on the traditional presumption of virtue and integrity ensuring productivity and acting on the promise of social good, the social contract for science relied on self-regulation (Guston 2000).

At the end of the 1970s, this social contract for science started to crumble with examples of research fraud, but more importantly with increasing political perception that science was not delivering on its promise of economic profit (Guston 2000: 5). The trust and virtue that grounded the social contract for science were gradually replaced with monitoring and incentives. Nevertheless, until the 1990s science and technology statistics at national level focused on measuring inputs rather than outputs, still building on the idealized model of linear progress from basic research to application to market (Godin 2002: 5). The role of the state was thus seen in funding the research that the industry would not: basic science.

In his Outline for a History of Science Measurement Godin (2002) charts the roots of the economist accountability in science to the Organization for Economic Cooperation and Development (OECD). He argues that "this focus on science and technology enabled the OECD to define the (economic) terms of an important new challenge that had hardly begun to be mastered by governments." (Godin 2002: 11) This allowed the OECD to reinforce its primary mission of promoting economic development, via science and technology (ibid.), and so by appropriating the field of science measurement for its self-justification, "the OECD enabled itself to define science policy and measurement in terms that were essentially economic." (ibid.: 13). The second mission of the OECD was cooperation, and with its insistence that science was international *per se*[7] science could serve as a barometer of cooperation among countries.

[7] Here again we can see the natural sciences, the laws of nature applying everywhere, at the centre of policy making, together with the traditional notion of the republic of science envisioned in the seventeenth century.

Furthermore, Godin (2002: 15–18) maps a sort of division of labour in terms of statistical collection, between governments and national statistical office on the one hand, which specialized in collecting input statistics into science and technology, and university departments and private firms on the other, which started exploring statistical analyses of outputs, especially in terms of bibliometrics. More recently, with the introduction of assessment systems as instruments of steering research and development, we can see an increasing tendency on the part of governments and their agencies as well as national statistical organizations to actually start using output statistics as a basis for their decisions. The Czech Statistical Office, for example, now organizes its online Science, Technology and Innovation statistics[8] into "Science, Research and Innovation", "Financial and Human Resources", "Research and Development Results" and "Advanced Technologies", where the results section is further divided into Innovations, Bibliometry, Patents and Licenses. These statistics have been published online only for the past three years, attesting to an increased concern of the Czech government and the Council for Research, Development and Innovation with measurable applied outputs.

Since the 1980s, New Public Management (NPM) has made gradual incursions into various segments of public policy, including science and research particularly in the United Kingdom but increasingly in other countries of Western Europe, including Scandinavia, as well as other parts of the globe (Ward 2012). New Public Management is a managerial philosophy advanced with the goal of 'modernising' the public sector (including science and higher education) on the assumption that greater market orientation will bring cost efficiency to the public sector. Science policy has thus gone "from being a policy on science to being one in which science must be made to serve socioeconomic needs" (Godin 2002: 19, see also Dowdle 2006). A future promise was no longer enough, and the old contract was replaced with "collaborative assurance" based on monitoring and incentives with external bodies playing an increasingly important role (Guston 2000: 4). This form of NPM accountability included "a focus on management, performance appraisal and efficiency; the use of agencies which deal with each other on a user-pay basis; the use of quasi-markets and contracting out to foster competition; cost-cutting …" (Shore 2008: 293). The cornerstone of NPM is the assumption that introducing the principles of competition and competitiveness as we

[8] http://www.czso.cz/csu/redakce.nsf/i/veda_a_vyzkum_veda_.

know them in the private sector into the public sector will result in greater performance (Shore 2008). Shore and Wright (2000: 60) argue that while it is difficult to chart the audit rationality precisely, since the early 1980s various market mechanisms have been introduced in public sectors of most OECD countries, in an effort to increase efficiency, accountability and consumer's power over the public sector. The overall assumption informing the practices of New Public Management thus is that performance indicators will seamlessly transform into increased efficiency and performance. It could be argued, then, that this confluence of OECD's appropriation of science and technology as an instrument of economic development with the onset of New Public Management in OECD countries since the 1980s created a very particular situation in science where the ground was laid symbolically (through statistics and values of the NPM) and institutionally (new practices of NPM, auditing and assessment) for an entrepreneurial type of governmentality.

The stress on competition introduces a novel type of dynamics and dynamization into the science domain. Authorship and copyright are the invention of modernity and individualization, originating in the Romantic stress on the creative genius (Jaszi 1991). Clearly, attribution of authorship, and relatedly primacy of a discovery or invention, has been a crucial feature of modern science. In this sense, scientists and laboratories have long been in competition with each other. However, the introduction of NPM's principles of competition reorients competitive behaviour in science; NPM's competitive time is not oriented by long dureé of scientific discovery.

Research assessments introduce a particular temporal dimension: they occur periodically, whether they are organized by governments or institutions. The concomitant stress on distributing funding competitively shifts more funds to research funding providers such as grant agencies or ministries; grants are distributed competitively and assessed periodically and after completion against promised outputs. Introduction of competition thus dynamizes time, in that it becomes sequenced, or fragmented, according to these various assessments. The segmented time of assessment becomes a resource that must be used with maximum efficiency (Adam 2003: 51). Such time is money, literally, for institutions as well as individuals, as it becomes commodified in abstract, quantifiable and universally applicable assessment time.

While competition through research assessment introduces a specific type of temporality in the research domain, there are multiple time regimes in science (Garforth and Červinková 2009; Vostal 2014); time is multidimensional and situated. The particular enactments of temporality thus are not oriented solely by the competitive dimension; rather, they are negotiated, often against other rival demands and desires of researchers. Time is therefore non-coherent, and as I explore further in the text, researchers find ways to make it cohere, for example by distributing the different temporalities, different time demands. Importantly, though, the entrepreneurial alignment rests on endorsement and ubiquity of competitive time and the successful distribution of other temporalities that do not cohere with the competitive time. Competitive time abridges – laboratory, policy as well as subjective timeframes.

1.5 Science no more: The contested terrain of science accountability

One indication of the changes in science governmentality today is the disappearance of the word science from European and national policies. One potential, and prosaic, explanation may lie in the growing international hegemony of the English language in science and beyond. In English, science, in everyday speech, has a narrower significance than in Czech (or French for example). Even if science may sometimes (though not always) cover economic, behavioural and social sciences, it is never used to refer to the humanities.[9] Despite this rather simple explanation, I do not regard this shift as quite as innocent.

In the 1990s science has been displaced in Czech policymaking by 'research and innovation' and more recently still by 'research, development and innovation'. At the beginning of the 1990s when the first act governing public support was adopted in Czechoslovakia, it was called the Act on State Support for Scientific Activity and Development of Technologies (Provazník et al 1998: 56), and the collocation was used in the title of the newly established Governmental Council (hereinafter the Council)[10]. By 2000 when the first national policy was adopted, it was already research and development policy. This re-packaging of science as research and innovation is in

[9] I would like to thank Simon Smith for highlighting this point to me.
[10] The Council later changed its name to Council of the Government of the Czech Republic for Research and Development, and still later Council for Research, Development and Innovation; its official statute as an advisory body to the government has not changed but its perceptions and roles have.

evidence not only on the policy level but also more largely, in the academic community, in the titles of journals and departments. Should we be concerned and what does it mean analytically?

"Science might be dead, but then long live research!" With this proclamation, Latour (1998: 209) hails the coming of a new era in scientific knowledge production. For him, the traditional model of science, dissociated from society, its passions and ideologies, is untenable. The hope for science lies today in a "collective experiment", in the inter-mingling of various human and non-human actors, in "the New Deal between research and society". Here I concur.

Like some others, Latour identifies a break from the past in how the domain of scientific knowledge making is organized, in an ethical and epistemological distinction between science and research: "Science is certainty; research is uncertainty. Sciences is supposed to be cold, straight, and detached; research is warm, involving, and risky. Science puts an end to the vagaries of human disputes; research creates controversies. Science produces objectivity by escaping as much as possible from the shackles of ideology, passions, and emotions; research feeds on all of those to render objects of inquiry familiar." (1998: 209) For one so astute in opening so many of the previously sealed black boxes of science, it is quite surprising to see this level of susceptibility to easy binaries. Latour's own work (e.g., Latour 1993) shows precisely that this sort of mingling has always been part of science and that purification is part of a process of creating legitimacy and durability of scientific knowledge and with it the institution of science.

In his *The Scientific Life: A Moral History of Late Modern Vocation* Steve Shapin (2010) argues that in the US context as much basic research was done in the industry as in academic research sites from the early twentieth century even while modern science managed to preserve its image as a moral and cloistered undertaking. Today, we do not usually think of scientists as morally different from the rest of society. This had not always been so. When the study of Nature meant reading in God's Book, reading that book meant moral uplift for those pursuing natural knowledge. Gieryn (1983) in his seminal paper explores scientists' 'boundary-work' or attribution of particular characteristics to the institution of science for the purpose of constructing a boundary from other intellectual activities defined as non-science. In Victorian England, such

boundary-work chiefly meant drawing boundaries between science and engineering on the one hand and science and religion on the other. The former arguments concentrated on science being theoretical and enlightening, having knowledge and not profit as an end, and being experimental in nature. The latter concentrated on science's objectivity, being practically useful, empiricism and scepticism. Gieryn demonstrates the contextual nature of the boundary-work as science and scientists were working to establish the profession and enlarge university curricula. Once the study of nature was severed from God, the integrity shifted from individual scientists ennobled in the process of study to the *institution* of science as such. Shapin argues three factors were crucial in this shift: secularization, the decline of the idea of Genius and the rise of the idea of the Method, and the rise of science as a remunerated job. Bureaucratization was thus part of the success of the modern scientific profession at the start of the 20th century. Since the 1970s, and perhaps even earlier—with the atomic bomb and the Manhattan Project—we can see an erosion of the straightforward attribution of virtue to the institution of science and scientists. For the past thirty plus years, the scientific profession is coming under rising levels of scrutiny, and claims about the nobility and exclusivity of the profession are increasingly hard to sustain.

There are several factors in this. One is the clear ambiguity of many of the scientific discoveries and their impacts on the natural and social worlds. Gieryn argues that scientists sidestep this obstacle by erecting a boundary between "the production of scientific knowledge and its consumption by non-scientists (engineers, technicians, people in business and government" (1983: 789). But if the scientific profession does not claim moral responsibility for its undertakings and argues, as we still often hear today in the Czech Republic and beyond, that scientists are not responsible for the knowledge they create, that it is politicians or society who must decide on its uses, then it is understandable that controversies will arise as to who should claim this responsibility. And since the State and commercial industrial interests do not always act in the interest of general population, the issue of society, the public, having a say in science has come increasingly to the forefront over the last fifteen years. One of the Triple Helix conferences, in fact, asked the question whether the public should be regarded as a Fourth Helix in the University-Industry-Government Relations (Leydesdorff and Etzkowitz 2003).

The second factor for the rising scrutiny is epistemological. Since the 1960s positivist knowledge claims and the Method have come under fire from many different corners and not only in the social sciences and humanities. With the rise and development of postcolonial, feminist, globalization and other studies it transpires how science and its Method have been complicit in producing skewed or outright discriminatory scientific knowledge the goal of which has often been the preservation of the unequal status quo, differential epistemic authority of various groups and thus differential access to power. Many domains of the natural sciences are not exempt from this, either. Indeed, the natural sciences have pioneered the use of scientific (or pseudo-scientific) knowledge claims to draw cognitive boundaries around the axes of sex, race, class or so-called lay knowledge (for an overview see Anderson 2012, in the Czech Republic for example Tinková 2010).

Since the 1970s Science and Technology Studies (STS) have played no small part in undermining many of the accepted notions of scientific knowledge production. Some of these include the interrogation of representation as correspondence with reality, the universalism of knowledge claims as an effect of networking and translation, the social, affective and embodied as part of scientific knowledge production, and the stress on process, practices and materiality. However, recent revisits of the early STS research have focused on the complicity of science studies with science governmentality and specifically the practices of research assessment. In a special issue of *Science, Technology and Human Values* Garforth and Stockelova (2012) examine the "geo-political heartlands of the United States and Western Europe, and an often unreflexive focus on the natural and technical sciences" (2012: 231). In her account located in the UK Garforth (2012) scrutinizes the issue of making things visible as part of the practices of both STS ethnographic studies and research assessment, and stresses the importance of invisible practices in science – in terms of knowledge making processes and evaluation alike. Focusing on social science knowledge and research assessment in the Czech Republic, Stockelova (2012) revisits the concept of *immutable mobiles* central to STS, and argues that the concept is problematic when applied to knowledge production and research assessment in the social sciences and humanities. Both authors argue for "moving out into the peripheries of knowledge production", and stress that "it is important to avoid trying to squeeze data into conceptual frames developed for grasping the heartlands" (2011: 232).

Latour's New Deal built on the notion of a "collective experiment" risks to further, in my opinion, some of the early shortcomings of science and technology studies, namely the lack of attention to issues of power, centrality and periphery, voices and visibilities. It is quite telling, I think, that he should choose the distinction between science and research to illustrate his point. Unlike the messiness and materiality of science the early laboratory studies made visible as integral part of the scientific endeavour, here science becomes reified, solid and stabilized, cloistered in its Ivory Tower of dysfunctional epistemology. In its place Latour posits the seemingly open research. To make his argument, Latour purifies science, making invisible its inherent hybridity, as well as research, making invisible its embedment in power. Over the past decade especially we have seen proliferation of researchers and research into what is variously termed 'research and innovation systems', 'research, development and innovation' and others. Many of these studies are not even remotely concerned about opening any sort of black boxes, any sort of warm uncertainty and collective experimentation that Latour posits in the quote above. This binary (as the binary of Mode 1 and Mode 2 knowledge production and the notion of the agora mentioned above) may be, in fact, intended to create an impression of development, of improvement, an optimistic version of progress fit for the new millennium.

The particular set of values on which credibility of science was traditionally erected has eroded since the 1970s, in no small part due to the laboratory studies. And other theoretically approaches joined in, too. The erected edifice of Science has been crumbling, and with it, seemingly the power to make privileged knowledge claims. Separating science from research and enacting research as engaged, collective, and risky may be an attempt to salvage scientific knowledge making and confirm the expert standing of scientific knowledge. If this was what Latour was after in his distinction between science and research, and I think he was, the distinction between science and research is fallacious and cannot solve the problem of the eroding epistemic authority of scientific knowledge. If his solution lies in a 'collective experiment' and his most recent project (Latour 2013)[11] certainly confirms this, then I do not think the answer will lie in a strict delineation between science and research.[12] In the quote above, Latour relegates

[11] Latour is currently creating a platform for this collective experiment which can be joined at http://www.modesofexistence.org/.

[12] For this reason, too, I use the expressions science and research, scientists and researchers, interchangeably in the text. In fact, most people as well as researchers in the Czech Republic continue to

science to the purified version he had shown to be a particular achievement (Latour 1993b). I think there is merit in continuing to use the word science, but of course including its messiness, partial achievements of stabilization as well as attempts to make stabilized knowledge claims. As such, science can engage in a clash over its own enactments with other ordering principles that are making claims on science, particularly economic and market claims but also claims from identity politics positions, environmental positions and others. If we accept that science is never pure and that issues of power are always embedded in the claims to scientific knowledge, then this—I want to argue here—will create space for a governmentality regime where all partial interests tied to various social domains will need to account for themselves. And maybe in the process, it will transpire that scientific knowledge produced in one accountability regime can make more durable links, more durable claims. But also, equally importantly, there will be other accountability regimes that will keep check on what is purified, what is muted, invisibilized. This will be my question for the concluding chapter.

1.6 Research questions, contributions and thesis outline

I develop a model of changes currently impinging on the codes and practices that govern academic research in the natural sciences in the Czech Republic. My aim is to examine governmentality regimes and assessment machineries in the natural sciences in the Czech Republic. I explore these in terms of institutional, subjective and policymaking orderings. How people live and know in science and how people, institutions and policymaking interrelate is not haphazard or simply a matter of a choice; coping strategies are embedded in a mesh of institutional rules and expectations as well as, for some more for some less remotely, policy imaginaries. Importantly, these coping strategies are not enactments of matters purely scientific.

In exploring the governmentality regimes extant in the natural sciences, I want to contribute to understanding the multiplicities, multiple orderings, of contemporary science. I am interested in the interplay of justifications of actions, how are they used,

use the word science (věda) in normal parlance, and if they use a collocation (in recognition of the abbreviation "VaV" which in law stands for research and development) they use science and research. The words science, research and development all start with a V in Czech. The word science, and not research, was regularly used at my field site to refer to the production of scientific knowledge.

for what purposes. I will explore the various enactments of research assessment in various organizational logics, while I also make visible the othered, displaced and marginalized, modes of organization which sustain and contribute to scientific knowledge production.

Specifically, the thesis concentrates on the following research questions: How is research assessment practiced by individuals, institutions and policies and what effects does it have in terms of epistemic practices and research careers? How are the multiple ways in which research assessment is done, made to cohere and how is non-coherence tackled? How are the practices of research assessment gendered, and what are the implications for gender equality in science? What is the emerging contract for science today in the Czech Republic and what are the potential consequences of such a contract for science?

After attending to methodological considerations in the next chapter where I introduce in detail my research site, research data, position myself in the field and address issues related to data analysis and the methodological choice of cases in this dissertation, over subsequent three chapters I examine the three poles of the dynamic triangle. In the first of these I attend to the dynamic organization. I examine the entrepreneurial alignments of the institute that I studied in an assessment-based governmentality regime. Concretely, I will trace the transformation of the institute and the logics in which this transformation was enacted in terms of competition, funding and research careers. I look into how research assessment is done at the institute, how a straightforward notion of excellence is complicated and made to cohere with other organizational logics, and how research assessment is gendered. The next chapter explores the entrepreneurial alignment of researchers' subjectivities and relatedly practices in the dynamic organization. I examine how research assessment is domesticated by some and how it is highly conflictual for others. I then turn to how researchers are aligning and disaligning their epistemic, professional and caring practices in a highly competitive, assessment-oriented research environment, and how this is gendered. Lastly, I chart the evolution of the social contract for science in the Czech Republic since the 2000s and address entrepreneurial alignments in science policymaking revolving around the shift toward dynamic funding and attendant research assessment. Through an exploration of the issue of the wealth and waste of human resources and the position of women in science, I examine a duality in the vision of the social contract for science, which makes it

possible to displace the issue of equal opportunities in research as external, outside the remit of research and development policy. In the concluding chapter I revisit the entrepreneurial alignments in the research domain in the dynamic triangle and its epistemic, professional and gendered consequences and consider potential futures for science.

With this dissertation I hope to make contribution to theorizing institutional change, specifically change in the domain of science, after the practice turn in social theory (Schatzki, Cetina, and Savigny 2001). For practice theoreticians, the social is "a field of embodied, materially interwoven practices centrally organized around shared practical understandings" (Schatzki et al. 2001: 12) The focus on practices makes it possible to overcome the binaries that inform much of social science research (such as object – subject, structure – action). The focus on practice brings to focus the entanglement of the discursive and material in the enactment of reality. Another tenet of the post-humanist version of practice theory is that non-human actors do actually act, and their actions are examined as much and as important as human actions. Lastly, the practice turn stresses embodiment and affective work involved in practices wherein bodies and subjectivities are also constituted, enacted through practices, and importantly, partake in the processes of knowledge production. The body is then the nexus through which practices are constituted. Because social orders arise from practices which are founded on embodied understanding, they are rooted directly in the human body (Schatzki et al. 2001: 18). Through the analytical concept of the dynamic triangle I analyse the entrepreneurial alignment of organizational logics, of policymaking and of researchers' subjectivities as mutually constitutive of changes in the science domain through material-discursive, including embodied, practices of research assessment.

My second contribution is to theorising gendered organization (Acker 1990, 1998), specifically gendered effects of the new accountability regime as evidenced in research assessment *as practiced.* This claim is obviously not intended to read that there is not ample research into the position of women in contemporary science. There is, however, first, a gap in that the theory of gendered organization looks at particular issues of research careers (work-life balance, mobility and even the gendering of evaluation of research work) but this is rarely read against the governmentality regimes extant in science and their interplay with the organizational and individual dimensions. Considerations and potential solutions to issues of gendered research careers thus often

stand in opposition to findings related to the gendering of the overall research landscape in terms of demands on assessment and excellence, the idealized research career, mobility patterns etc. (for an exploration of these issues see the concluding chapter in Linková et al. 2013). Secondly, many studies of the position of women in science and gender inequalities either take a static analytical snapshot of the situation or are retrospective reconstructions. Rarely do these studies examine how gender is practiced in the academe, in the interplay of symbolic, institutional and individual, against the shifting ground of how research is evaluated.[13] My major concern then is whether the competitive, entrepreneurial, excellence-driven re-ordering of science can be made to cohere with an ordering where gender is not displaced and invisibilized. What sort of gender equality does the entrepreneurial ordering of science allow?

This brings me to my last point: I want to attend to the potential risks and opportunities that the new governmentality regime brings. If numerous studies on the history of science have shown that science has never been completely enshrined it its Ivory Tower and separated from society, it is now important to explore the possibilities of an active endorsement of this intermingling as a strategy for multiplying the sites in which governmentality of science can be located and claimed.

[13] Notable exceptions include van den Brink and Benschop (2011).

2. Setting the scene: Methodological considerations

Research in the Czech Republic is located in the institutes of the Academy of Sciences of the Czech Republic and other public research institutions (e.g. ministerial or sectoral ones), higher education institutions, and in the industrial sector. Established in 1952 as the Czechoslovak Academy of Sciences, the Academy was an umbrella organisation for various non-university research institutes, academies and the learned society that existed before WW II (Šíma and Pabian 2013: 73-74). The Academy was legally defined as the chief component of the socialist research base (Provazník et al. 1998; Šíma and Pabian 2013: 73). Although the communist regime continue to proclaim the importance of the unity of teaching and research, obsolete technical equipment and lack of teaching staff in universities together with the clear prioritization of the Academy of Sciences as the chief scientific organization continued to undermine this dual mission (ibid.). This situation was further aggravated by political interference in the personnel affairs in universities (Šíma and Pabian 2013: 77-78). During the communist regime the division of labour was strengthened between the Academy of Sciences as a research institution performing fundamental research, and universities devoted primarily to teaching.[14] The split was based on the Soviet model (which itself took inspiration from the French model and amplified it). Thus, while universities focused on tertiary education, the Academy had the right to award CSc. titles (equivalent to a Ph.D.) and controlled major research infrastructures. After 1989 the Academy faced pressure as a "remnant" of the Communist past and lost some of its powers (e.g. the right to confer a Ph.D. was transferred to universities[15]); nevertheless, it has remained, together with Charles University, the most important basic research organization in the CR.[16]

Despite periodic debates about the disbanding of the Academy of Sciences and integration of its institutes into universities, the dual nature of the Czech research

[14] This division had political reasons, too. While only people who passed the so-called "political assessment" in 1969-1970 after the 1968 occupation were allowed to teach at universities (to come into contact with students), the situation in the Academy of Sciences was more relaxed and sometimes served as safe-haven for some researchers. That is not to say, however, that political assessments did not adversely affect the Academy or that people were not forced to leave for political reasons.

[15] This has been one of the continued sources of tension between universities and the Academy because the Academy trains about 2,000 doctoral students through joint AS-university Ph.D. programmes (2,162 students in 2008) but the per-head contribution from the state goes to universities.

[16] In 2008 the Academy published more than one third of papers in impact factor journals and had approximately one half of all citations of published work (Akademie věd České Republiky 2009: 11).

system continues even while there are strong ties between the higher education and public research sector. Many researchers at the institutes of the Academy of Sciences teach at universities and there are joint doctoral programmes formed by institutes of the Academy and faculties. At my research site, almost all of the top ranking researchers taught at various Czech universities, and through a joint accreditation, the numbers of students at all educational levels were high. On the institutional level cooperation between the Academy and universities has been steered through policy instruments (e.g. common research programmes and centres).

There are important differences related to core funding. While universities receive their budget as per student payment for pedagogical activities and based on research performance assessment through the Methodology from the Ministry of Education, Youth and Sports, the Academy has its own budget chapter. Because of its own budget, the Academy of Sciences has retained power to organize its own research assessment as a basis to redistribute the budget chapter among its institutes. This duality of the public research landscape in the Czech Republic complicates unified research assessment in the Czech Republic, which I will attend to in chapter 5.

2.1 Research site and data

To carry out analyses for this dissertation I have used heterogeneous data gathered primarily through my participation in a European research project[17] within the framework of which I carried out fieldwork at a bioscience institute of the Academy of Sciences between September 2006 and August 2007. The director of the institute did not require that any special agreement to be signed and left negotiations of access to occur at personal level with team leaders. I was granted the consent by two group leaders at the first meeting with them.

I observed intensely in autumn 2006 and spring 2007. My observation data covered primarily two computational chemistry groups located in a building detached from the main institute, and I also observed regular group meetings of another team at the main building and research meetings of a group of researchers working on a research problem together. My access to the computational teams was greatly facilitated by my

[17] Funding for this research project was provided by the European Commission, project Knowledge, Institutions, Gender: An East-West Comparative Study, contract no. SAS6-CT-2005-017617.

participation at the computational chemistry autumn school spanning three days. I was invited to a party on the first night of the school where I had an informal opportunity to discuss the research project, gender equality and of course research culture and the transformation of the institute. Based on this, I became 'known' in the two groups because almost everyone participated in the party.

I could come as I wished to my research site; there was a workstation available for me. I was told at the beginning that if I worked at a computer there, I would 'blend in' since everyone in these two groups worked at a computer. As one of the senior researchers told me at the beginning "*come with your laptop and in time you will be indistinguishable from the other students. People will get used to you. They do research here; you do your own research here. You're just like us, only you do a different type of research*."[18] Although this blending-in certainly helped, it also made it more difficult for me to claim the researchers' time onsite. In contrast, my colleagues who conducted their observation in labs did not blend in; sometimes the lab spaces were very small, so their presence could be disruptive. But the factor of their visibility was conducive to people engaging with them. To overcome this obstacle, I started making concrete plans to engage with concrete people and made maximum use of informal settings to engage with scientists.

I would spend 3–6 hours, depending on availability of people, stamina and 'sources of action'. I observed people at work, joined lunches (and contributed to cooking and cleaning up), participated in research seminars, engaged with people in informal settings (lunch, dinner), attended lectures and joined people for having a smoke on the balcony. I joined people when they went to the main building to pick up equipment and on one occasion I spent a day shadowing a group leader. I made field notes and searched for additional information on institute's internal website through login on one of the local

[18] This does not mean that it was like that with everyone. At the beginning there was one computational chemistry team. As a result of a transformation I will discuss in the next chapter, the team separated two independent research groups with two heads, though they continued to share the premises and clusters. There was very little visible linked to this – only some researchers moved offices. In one team I continue to have unquestioned support; in the other one my support and presence was guaranteed by one senior researcher and the head of the team. The other seniors were against my presence and I really never talked to them. At the beginning we silently negotiated that I would be there but invisible to them. This went so far as to the fact that some people denied seeing me in the kitchen and would not return my greeting. Others greeted me but that was the extent of our interactions. At the beginning I thought that in time this may change but as time progressed and our positions vis-à-vis each other became stable, I realized that this would be so and that there was nothing I could do to persuade them to engage with me.

computers. I continued to make field notes at home and expanded on what I recorded on site.

Furthermore, I conducted individual and group interviews with group leaders ("the bosses") and independent scientists, postdoctoral fellows and students from other groups. In total, I interviewed seven out of thirteen group leaders (including the two women leaders at the time), thirteen independent scientists (six women), and a changing lot of twenty-four postdoctoral fellows and doctoral students, thirteen men (nine doctoral and four postdoctoral fellows) and eleven women (eight doctoral and three postdoctoral fellows). There were nine foreigners, six in postdoctoral position and three doctoral students. I have also talked during my participant observation with two BA women students.

These engagements included initial introductory life-course questionnaire to establish a baseline within the research project[19], and in-depth interviews on six selected topics that the international research team defined. These were followed by three focus groups, with junior researchers (PhD students and postdoctoral fellows), women researchers and group leaders. The focus groups, too, revolved around topics defined by the international research team. Issues of assessment, public accountability, research careers and science-society relations were always included. For each stage of the research I presented an informed consent form developed by the Czech team to each research participant which contained information how and for what purposes I could use the data generated and three alternative confidentiality clauses for them to select ranging from non-anonymisation to full anonymisation.

I also performed discourse analysis of institutional documents, which included annual reports, director's directives, monthly reports, minutes from the meetings of the Scientific Council (2000-2006) and the Council of the Institute (2007) and the institutional assessment system – in total 81 primary document (where some primary documents contain all documents for a given year of a particular type, such as, for example, 2005 Director's Orders); documents related to the internal scientometric assessment procedure; the 2005–2010 research proposal submitted to the Academy of Sciences; and the director's media appearances (3 documents). I also analysed national

[19] The life-course questionnaire concentrated on major journals and conferences in the field, work habits (how much people work), organization of teams and organization of the doctoral studies, and types of contracts at the institute, to name a few of the topics covered.

R&D policies and programmes, the R&D&I Reform, the Yearbooks of the Council for Research, Development and Innovation, and the Methodologies.

There were additional sources of data, dating before and after this research project. The second European project mentioned in chapter 1 ran between 2004 and 2006, and one part of the project looked into the role of science in contemporary Czech society and gender equality. Here I carried interviews with civil servants, policy makers, politicians and individuals who had been involved in preparing policy documents. These included eight interviews carried out in 2006 with civil servants, policy makers and politicians responsible for research and development policies and for gender equality policies, of whom five were responsible for Czech research and development and three for equal opportunities. Three interviews were declined by research and development policy makers, in two cases by high ranking officials at the Ministry of Trade and Industry[20] and in one case by a high ranking state official responsible for research and development[21]. The research civil servants, policy makers and politicians interviewed were men with one exception, gender equality policy makers were all women. Some of the respondents were high-level state administration officials; others were members of the Parliament of the Czech Republic, the Council for Research and Development and the Governmental Council for Equal Opportunities or civil servants at the Office of the Government. Additionally, I carried out nine interviews in 2009 with high-ranking researchers in the public, higher education and business enterprise sectors who had been involved in various capacities in shaping Czech science policy through membership in the Council for Research, Development and Innovation, its committees, ministerial expert bodies or who directly contributed to Czech science policies[22].

[20] The communication was very tense with the ministerial officials, and one of the interviews was declined because of a 'politically touchy' situation linked to allegations of fraud and corruption related to research funding at the ministry.

[21] In this case, the interview was declined on the ground that another person with whom I conducted an interview would present the exact same governmental position and therefore there would be an unnecessary overlap.

[22] Funding for this research project was provided by the Grant Agency of the Academy of Sciences of the Czech Republic contract no. KJB700280907.

2.2 Positioning myself in the field

There were several factors that had effect on how the various research partners I encountered throughout these research projects related to me. The ones that I consider significant were being a doctoral student and identifying as a feminist when asked (the two European projects both had an explicit relation to gender or gender equality in science so the topic frequently arose as a result of introducing the framework of the projects). During my fieldwork, group leaders of the older, pre-retirement generation had a tendency to patronize; I was the one who had to be explained how things "really" work in science. This was helpful as they were explicit about their values, expectations and their view of the workings of science. My feminist position and sensitivity (seen by some as oversensitivity) to gender bias and unequal distribution of gendered work had the effect that some women researchers discussed certain questions with me very openly; they tested their feelings and asked me for my "expert" explanation and justifications of their feelings and experience. Others, on the contrary, opened answers to almost any question with "I will probably disappoint you but I don't feel any discrimination". In some instances gender was invisible, especially in interactions with students, postdoctoral fellows and some of the younger group leaders and independent scientists. In other cases some, especially men students tested how "our feminist" would react to jokes about women. These occasions often provided an opportunity to build alliance with women students when I managed to respond to these situations adequately, which meant either being able to take the joke or responding with a converse joke, which made it possible for the women to join in laughter focused on men for a change.

In terms of research hierarchy, my position was ambivalent. Students related to me primarily as to a doctoral student, a person on the same position as they were, which often resulted in sharing criticism of the bosses, the "structures". On the other hand, my position as a coordinator of a European research project and a team leader at the Institute of Sociology distinguished me from the students and postdoctoral fellows, as did the attention I paid to issues of science policy and its impact on research work. In this respect I was much closer to independent scientists and some of the group leaders. My position as a doctoral student and project coordinator was unthinkable in the natural science institute. Doctoral students and postdoctoral fellows are not independent; they work on projects of their supervisors and group leaders. In this respect their independence means solving partial research topics but always under the leadership and

supervision of a boss. The PhD and postdoctoral positions are apprentice phases in the natural sciences during which students and postdoctoral fellows are inducted into research networks and practices, they acquire tacit knowledge of publication strategies and work to make a mark on the map of their research community, but always as part of a team under the leadership of a concrete person who guarantees the quality of the student or postdoctoral fellow.

My position clearly affected what information and data I was able to obtain. Older group leaders did not share with me internal tensions, information about personal animosities or problems surrounding the transformation I will describe in the next chapter. What they shared with me, the accounts of science and their position at the institute they gave me were ones that corresponded to my junior research position. Much like to other students, they described science and its workings as a beautiful and attractive but highly demanding endeavour. Everything was as it should be and they felt the responsibility to confer this "as it should be" to the younger generation (I will return to this in chapters 3 and 4). These individuals did not mention any criticism, and if they did, it concerned external issues such as increasing grant administration or the insufficient support in the Czech Republic provided to teams aiming to participate in European Commission's Framework Programmes.

In contrast, individuals who were dissatisfied with the order of things at the institute often shared with me such critical information, and in some cases they were very concrete. They built alliances for a different vision of the institute, with the explicit goal that the "picture" the project would paint overall, would not be only one of excellence and greatness. They assumed that the director of the institute steered our research team toward groups with no problems, and in some cases they felt it important to provide a corrective. And finally, there were people who refused to communicate with me altogether (see footnote 18). These included people who had no regard for the research project; did not appreciate the fact that I was a feminist or that the project had a gender orientation; or were critical of the situation at the institute or in the Czech science or both, and since they did not know where I stood on these issues or where my allegiances were, they were careful not to engage. I only learnt about their criticisms and reservations through other people as I tried to establish why these researchers did not wish to speak to me or outright ignored me. I also need to acknowledge that my critical position toward a unified system of measuring research performance may have affected

how people discussed success and assessment criteria with me; often, opening this topic resulted in longer discussions about how research work should be assessed, alliances in critique of a unified system or explicit refusal of the system on grounds that it, for example, disadvantages mothers-researchers with the responsibility of a primary carer. In these interactions space was created to open up aspects which would not be voiced in a different context, for example had I advocated impact factor assessment.

Although I could blend in, as I discussed above, thanks to working at a computer, in important ways I remained a *foreigner*. Doing an ethnographic study in a discipline that was not my own had clear advantages. Unlike potential embarrassment related to asking about things a chemist should know (cf. Faulkner and Becker 2008: 16), I could enjoy the epistemic foreignness and ask questions freely. Furthermore, this epistemic foreignness was a source of allegiance which *geographic* foreigners built with me. Highlighting on various occasions my epistemic distance and lack of expert knowledge in chemistry and physics, this foreignness created space for them to claim me for their "foreigners' club". While my epistemic foreignness was what apparently allied me with their club, they also felt that there were other moments where I was an insider: I spoke Czech and was local. Through mutual exploration of our different foreignnesses (Faulkner and Becker 2008: 18), our foreigners' club gradually established that my dual position might be an avenue for translating some of their concerns and aloneness. In any case, the mere existence of the foreigners' club composed of PhDs and postdocs who were not Czech indicates some of the boundaries that persist despite the unequivocal call for international mobility and the unproblematic imagery of mobility in policy and institutional imaginaries.

Some two months into my field research, I also managed to establish my gender foreignness on one occasion. Coming to my research site straight from an official meeting, dressed formally and in a feminine manner, with some lipstick and eye mascara, I clearly breached the labs' *mono-gender*[23]. I did feel like a freak on display, as a surprising number of people came to look and comment on my appearance. With this single breach, I 'outted myself' to another woman, a postdoc from Spain, who was in constant breach of the mono-gender and was extremely critical as well as extremely

[23] In her research in the field of high energy physics Knorr-Cetina coined the term mono-gender to refer to "the stylistic rendering of everyone as a physicist, regardless of their gender in the everyday life" (Knorr-Cetina 1999: 232).

saddened by what she perceived was her exclusion because she wore colours, skirts and makeup.

Being a foreigner had for me major ethical implications in that I tried to bring up some of the foreigners' concerns with the lab leaders and senior researchers who I felt could lend their ear to these concerns, not least because they were also keen to send their students and postdocs off on a postdoc abroad. While these concerns may seem very far from issues of research assessment and governance, I do not regard them as some external circumstance. The foreignness and loneliness may serve to extend the working hours; some people worked late and arguably could consequently produce more results. On the other hand, some of these foreigners were very alienated from their group, and there was a clear limit on the amount of support they could garner from their fellow PhD students and postdocs. Although the support from their supervisors was unquestioned, supervisors are consulted only as a last resort when the potential advice from other group members has been exhausted. These concerns would play out very differently in a highly internationalized research environment. In the Czech Republic where internationalization is still not very high, being a foreigner is an issue.

2.3 Data analysis

For the textual analysis I used Atlas.ti, a computer assisted qualitative data analysis software (CAQDAS), to organize and code my data and develop analyses. My initial coding was thematic and often entailed longer segments of text. Gradually, as particular research issues started coalescing, I added another layers of coding which were far more fine-grained, to capture valences and differences on these research issues. In this subsequent coding the units could be as small as a phrase or a sentence.

The issue of assessment and competition has been present at the research site straight from the beginning because of the transformation of the institute I will introduce in the next chapter, and so my data were replete with competition, the internal assessment system, ranking of scientists at the institute. At a later stage, when I was coding the group interviews, my attention was caught by how my research participants in all groups discussed "patience" – to work in the natural sciences, one has to be patient, who is not patient will not stay no matter how good they are, people leave because

experiments are not working out, the ratio between experiments that do and do not work is 10 to 90 or 20 to 80, according to different accounts. As I was going through these statements coding them, I realized that they strongly correlated with codes related to success, and what was striking in these codes of success were the adjectives: small, tiny, very small. I then turned my attention to various facets of success in all my material, including good feelings, recognition, publishing, research assessment. Statements related to success often entailed definitions of good science, how to do science, what is important to be a scientist. Sometimes definitions of success and good science were contrasted with other practices (collecting points, for example, or the gap between IF-based assessment system at the institute and recognition within a community of peers). Another topic where the coded texts overlapped or were located nearby involved changes – changes in the atmosphere at the institute, cooperation, values, and practices. As my coding process continued, I started organizing the data related to success into practices that had more to do with a competitive, metrics-based system on the one hand and other types of success located in the epistemic processes and slow times of the bench work.

Coding, in my experience, means that a person is better acquainted with her data. It is then more difficult to cherry pick the quotes that fit a given interpretation (Anderson-Gough 2004: 380). The ability to combine codes, seek overlaps, regroup codes and create categories, in short working with data, meanings, expressions, in ways that the researcher cannot presuppose is an enabling aspect of CAQDAS in relation to validity. As Konopásek says (Konopásek n.d.: 7): "...practically, through this procedure we arrange the situation in such a way that the result better corresponds to the ideal of *objectivity*: a researcher does not have full and direct control over what "comes out" of his research, but rather he functions as a *spokesperson* of someone or something else, a little bit unfathomable." (my translation)

The use of CAQDAS has become an instrument of accountability in qualitative research. It is a way to prove that a person is serious about qualitative research, that she can prove she has taken specific technical steps as proof of a proper procedure. As MacMillan and Koenig (2004: 180–181) argue, literature discusses various aspects of CAQDAS but very little attention is paid to critical assessment of whether or not to use it. To say no to software is practically impossible. Its existence creates the assumption that thanks to CAQDAS research analysis will be better. CAQDAS allows qualitative

research to appear as serious as quantitative research. During my fieldwork, particular at the early stages, I have on occasions resorted to this argument, too. When I was hard pressed during my fieldwork to prove that the results of my research would be valid, I referred to using software. This was always enough, as if software were a magic word which put issues of validity back on track. It never stopped surprising me how embodied this assumption was even though researchers in the natural sciences are aware that computer generated results do not provide straightforward answers, that scripts are written, and how they are written can and does affect results. The relationship between validity and use of a technology is not straightforward, in the social and natural sciences alike, yet the reference to use software always brought a closure.

No software, however, can itself provide a substantive interpretation or develop a subtle mesh of relations and links (Konopásek 1997). Interpretation is always already embedded in the coding process, and is enhanced by the researcher's familiarity with her data and iterative working with codes and meanings. In this sense, as much as data is generated during the research process through interactions in the field and with research partners, data generation and analysis also entails interpretation. "Validity of research and findings may have more to do with decisions that researchers make than established or documented procedures (Koro-Ljungberg 2010: 607), but capturing these types of decisions through technology is very limited. With its sophisticated functions, imaging and recording working with data, the CAQDAS creates material conditions for accountability; in fact, the integration of such functions in the software may be seen as necessitating the application of these procedures. Methodological sections which describe how a person worked with data have thus become a necessity but this does not apply to all persons equally.[24]

Accounting for the use of software was not necessary in the narrow Czech team working on the international project as we debated in detail our slowly arising ideas and

[24] People who have a status and a publication track do not have to be as rigorous. Several years ago a colleague of mine and I came across a paper where the author – a recognized professor – stated something along the lines that his paper builds on many years spent in an university environment, reading documents, exchanging with colleagues and working in administrative and managerial positions. My colleague and I could only sigh that such a statement would most certainly not be enough in a paper we were writing at the time. And it is not without irony that neither my colleague nor I could later remember which paper this was and so I cannot give a reference here. Is having a memory of this sufficient? Especially as it drove home not only the various standards applied to differently positioned researchers in the research system but was also a breaking point for me to get very serious about archiving and recalling papers and references?

interpretations. Validity in the team was ensured through debate and team work and did not have to be proven with images of Atlas.ti data management. From my experience, it is necessary to maintain openness to data, new connections and their regroupings. There is a time when I can work only with codes, such as when I focus on a particular analytical issue; at other times, keeping the big picture and openness to new contexts is just as important.

I join (Koro-Ljungberg 2010: 604) in her "worry that externalized notions and conceptualizations of validity promote simplistic and reductionist views of knowledge and data." It would be possible to read the focus on accountability through the ability to master a CAQDAS in the framework of the audit culture (Power 2003) and the technocratic approach to assessing quality of research. All too easily, the accountability process can turn from assessing validity as arising from the research process, reflexivity and positioning oneself vis-à-vis one's research field and data, to one arising from the ability to master a technological procedure.

Similarly to my position on research assessment, my caution and these critical remarks are not meant to disparage the use of CAQDAS or the opportunities and efficiency it can bring in terms of data management, coding and interpretation. It was, after all, previous lack of accountability for the knowledge produced and methodologies adopted (especially in relation to marginalized groups and locations and marginalized voices) that resulted in the creation of rich methodological literature. But as should be clear by now, my position is that the use of CAQDAS as such does not guarantee accountability, validity or ethicality of research.

The process of writing is another key component of creating valid knowledge. Through writing, arguments are thought out and built. As Konopásek (n.d.: 8) argues, interpretation is not built "so much on mental processes (reading) but rather – in quite down-to-earth manner – in observable material practices (writing)". Through writing and then reading what has been written, validity of findings and claims is established. Such was, precisely, the procedure in this present dissertation. I have thought and written on research assessment already, journal papers, sometimes co-authored, focused always on a particular issue. But the process of writing this dissertation, re-writing the chapters, having a supportively critical intellectual companion read drafts and comment, helped me build in the process the notion of the dynamic triangle. The dynamic triangle

was not there at the outset; there was the concept of the dynamic and dynastic lab, terms coined by my colleague Alice Červinková (Linková and Červinková 2013), to describe a transformation in the organization of teams which I reported in the initial reports from the my research site, too. But I did not set out the write the dissertation with the dynamic triangle clearly spelled out. I knew what issues and aspects were salient – research assessment, reorganization of research work and teams, changes in research careers and research policy – and that they were linked. But the intense process of going back to the data after quite a long time since being in the field, having conducted additional interviews and witnessed ongoing changes in the Czech research system, putting the individual facets together and, most importantly, writing this dissertation up in a highly concentrated time, resulted in the gradual emergence of the importance of the three poles of the dynamic triangle as a locus of the organizational changes in science I am about the explore in the next three chapters.

2.4 Generalizibility and the strategic choice of cases

My ethnographic data involves a single public research institute of the Academy of Sciences in the biosciences, one that has been for some time exceptionally successful, has presented itself and has been presented as a model to be emulated by other Czech research organizations in the media and the research community. Some members of its staff were instrumental in institutionalizing research assessment at the national level and have been staunch advocates of the need to foster excellence based on high impact factor publications. Others continue to work to this effect through membership in scientific boards, for example, of the Czech Science Foundation which recently replaced a previous support programme of postdoctoral grants with a new programme of junior grants, which are intended to be much more selective, intended only for a narrow group of excellent early-stage researchers.

In the previous chapter I argued that metrics-based research assessment systems have been shown to work better for the natural sciences, and my interest is to examine whether the process and assessment system function in the natural sciences on which they are said to be modelled. The choice of an extremely successful organization which has a high performance record according to such assessment criteria is a specific, extreme choice which makes it possible to study how and whether such an assessment

system works, for whom, and whether it creates any sorts of tensions and non-coherences. The choice of this research organization was therefore strategic as a *critical case* defined as "having strategic importance in relation to the general problem" (Flyvbjerg 2006: 229). If tensions and non-coherence related to research assessment arise in a high-profile, excellent research organization, it is likely that they will arise, too, at other institutes. Because this is a strategically chosen organization with a reputation and standing at home and internationally, it can be expected to be emulated, to serve as a model to which to aspire and according to which to measure other research organizations. Lastly, as discussed above, its staff members are an important part of the processes of public policy formation in research and development and contribute to public consultations. While the particulars of its financial situation, the history of its assessment system and other aspects will differ from other institutions, the process of institutional isomorphism makes this a strong candidate on which to model institutional changes.[25]

There are additional reasons why the case study approach is highly pertinent. Firstly, the wealth of detail accessed in a case study allows the researcher to form a nuanced understanding of the studied realities. Such a nuanced view together with the plethora of detail gives answer to one of the criticisms lodged against case studies, namely a bias toward verification whereby the researcher confirms her preconceived notions. The case study, building on ethnographic research, examines situations as they unfold in practice; a single case study is not guarantee to present a unified view of reality. There are multiplicities and contradictions. Furthermore, the field can speak back, can jolt the researcher (Flyvbjerg 2006: 235). The field, my field, presented much non-coherence, and it was the analytical process of making sense of the non-coherence and how it is managed that contributed to my development of the notion of the dynamic triangle. But the triangle was not a hypothesis I tested; it evolved through the examination of concrete happenings, unfolding practices of organizing, assessing and valuing research work. The next three chapters should be read as a narrative in an entirety (ibid.: 241). It is the gradual unfolding and layering of the entrepreneurial alignment through

[25] It has actually come to my knowledge very recently, in June 2014, in a general discussion about the Czech research assessment in a university setting that the research institute I studied actually contracted the same Technopolis Limited to design its assessment system back in 2000, that the Ministry of Education now contracted to design the Czech assessment system in June 2014 and which performed the audit of the Czech system of research, development and innovation in 2011. To my surprise, this information was not mentioned in any of the institutional documents I had at my disposal that were related to the institution of a research assessment system at the research organization.

competition where I saw the power of the case and wherefrom I also derived the power to present a normative reflection in the concluding chapter.

Additionally, to present various examples of non-coherence arising in relation to the research assessment in the organization, researchers' subjectivities and policymaking, I use cases. Flyvberg argues that one of the frequent misunderstandings of cases is that it is not possible to generalize on the basis of an individual case (ibid.: 224-228). The cases of non-coherence I present may be single instantiations of non-coherence; in others, there were more. What I consider important is not *how many* enactments of non-coherence I managed to identify at the institution but the *process through which the non-coherence is managed.* It is precisely because the organization, individual researchers and the policymaking manage to deal with non-coherence through, variously, its denial, domestication, conflict, separation and care, that the entrepreneurial ordering has managed to establish its power, so much so that some practices have been aligned in the entrepreneurial mode.

Lastly, the case presented may have some bearing for universities, especially as it is universities the core research funding of which is distributed by the Ministry of Education according to the research results recognized by the Methodology for Assessing the Results of Research and Development (with per student payment being the second source of income). Arguably, the dual role of universities as teaching and research organizations will necessarily give rise to other types of non-coherence that are not in evidence at institutes of the Academy of Sciences. Clearly, the role of students in the governance of universities is necessarily different from their position as students at the Academy of Sciences.[26] Despite this crucial difference, Stöckelová (2009) has already pointed to some examples of non-coherence between the ways a university setting is organized, and to novel instances of enterprising. This, I hope, will present an opportunity to explore some of the concerns I raise in this dissertation with colleagues who have conducted ethnographies of higher education in the country over the last several years.

26 Czech scholars have been looking into the massification of Czech higher education, and a research project was completed in 2013 titled Mass Higher Education in Institutional Settings: Ethnography of Academic Departments in the Czech Republic (GAP404/11/0127). Some of the results of this project related to the role and position of students include (Minksová and Pabian 2011; Pabian, Hündlová, and Provázková 2011; Pabian and Minksová 2011).

2.5 A note on language

My research presented in this dissertation was part of an international research project which was predicated on communicating in English. First drafts of research instruments (topics covered in interviews and focus groups for example) were defined in English; each team then translated these into their national language and adapted them to the specific features of the institution under study. The institute I studied employed foreigners, and therefore some of the data, including a focus group, were in English. My field notes were written in Czech. I coded in Czech but wrote texts, even preliminary analyses, in English – in anticipation of project reports but also prospective paper publication. Additionally, there was a foreigner in our team whose command of Czech was not sufficient to follow the team's discussions of the progress of our fieldwork, and so our Czech team largely communicated in English. Although we raised the issue of potential consequences of this (e.g., could we miss something?, are we free to frame issues in ways that make sense for the Czech reality?), this could not be resolved unless the Czech team members decided to exclude the foreign team member; and we did not.

Furthermore, all my presentations at the institute under study were in English so that foreigners in the lab could participate and, even more importantly, because it was customary – no one presented in Czech in the teams I observed, and this was another way to blend in. In fact, my command of English, use of concepts and references to literature was what helped me to establish myself as an intelligible researcher in my field. (The only thing that made my presentations patently different was the lack of cool images.)

Lastly, all the literature I work with, with the exception of the few papers and books authored by my Czech colleagues, is in English as my command of other languages does not allow me to follow scientific literature. In my own research practice, I come full circle to the international hegemony of English.

3. Dynamic organization: Entrepreneurial alignment of research quality

Czech research institutions in the natural sciences today aim to establish their standing and achieve a world-class status especially through impact factor publications. Certainly the institute where I carried out my fieldwork did. Papers in journals with a high impact factor, and especially *the Nature, the Science,* and *the Proceedings of National Academy of Sciences*, are often considered a measure of research excellence. But research quality has not always been defined in this specific way; in fact, this is a fairly recent development, one we are seeing particularly since the new millennium.

Research institutes and their organizational rules are important players that shape the functioning of the organization as well as the organization of professional paths of individual scientists. They operate in a complex web of relations to other institutions such as, *inter alia,* research funders, national and international assessment systems, other research institutes, the law and the government and its policies which all contribute to the particular shapes the organization takes, through a process of institutional isomorphism introduced in chapter 1 (DiMaggio and Powell 1983). Quite obviously, the structuration of the field of science, with the increasingly normalized and sequenced career paths (especially in the natural sciences) which I will introduce in this chapter, the growing role of international academic mobility, particularly at the early career stages, and the ubiquity of a particular type of status and reputation awards based on impact factor publications internationally, are major sources of institutional isomorphism. As institutions aim to achieve world-class status and make a mark on the international research map, the tendency toward isomorphism may be expected to increase. Institutes need to be recognizable in terms of their organizational processes and procedures; the demanding and highly structured training in the early career stages (doctorate, postdoctoral fellowships) in the natural sciences also contributes to this process. In this chapter I argue that the institutional isomorphism I discuss in terms of entrepreneurial alignment are linked to a particular type of competitive logic of research assessment.

The logic in which research organizations operate have changed significantly over the past thirty years, as I charted in Chapter 1, and this shift entails the introduction of a

type of governance regime involving interrelated changes in public policy, research funding, accountability, professional paths and the overall culture of research. In this chapter I will examine how the institution that I studied has been enacting this changing governmentality regime. Concretely, I will trace the achievement of excellence at the organizational level. Or I should rather say, achievement of excellences. Contrary to the common perception that excellence is something the academic community can spot when they see it, which implies omnipresent stability and definiteness, I will, firstly, argue that excellence is more complicated and indeed more complex. To borrow from Mol (2002), I will claim there is more than one excellence but then there are less than many. The way excellence is enacted institutionally is not haphazard. It is ordered but there are other organizational logics which order excellence somewhat differently. I discuss how these different logics of excellence are made to cohere at the institutional level.

This is not the only complication, however. In addition to excellence, a particular type of quality as I will shortly argue, there are other qualities which must be institutionally managed and without which an institution could not function properly. These, however, often remain in the hinterlands, displaced and kept separate. So secondly, I will analyse how excellence and its others are done in institutional settings, and how these are made to cohere. Thirdly, contrary to the common presumption that quality has no sex, that there is only good science and bad science as is often argued in the Czech Republic, I will discuss how excellence and research assessment are gendered at the institutional level and how the different logics brought by parenthood are made to cohere with excellence.

It is often assumed that changes in organizations come in response to an external pressure. In this case, it could be assumed that research organizations in the Czech Republic have responded to the introduction of the Methodology and changes in research policy I will chart in Chapter 5. My analyses here show that such a linear policy-institutional trajectory is not adequate. Secondly, the institute is one of some fifty institutes of the umbrella organization, the Academy of Sciences, with its own law, budget and rules. It could be equally assumed that this umbrella organization will have a strong effect on the organizing practices of its institutes. But there is no linear trajectory between these two, either, and I will examine how the institute I studied coped with this non-coherence. Lastly, it is often assumed that changes in the governmentality regime

in science aimed at opening competition and subjecting research institutions to increased scrutiny, thereby allegedly increasing their transparency, will also contribute to supporting gender equality, by disrupting nepotistic ties of old and opaque gate-keeping practices. My third argument in this chapter is that rather than eliminating gender inequalities the new governmentality regime re-orders the old ones while creating new forms of inequality befitting this regime.

In Chapter 1 I referred to domains and their various organizing logics. While there may be a dominant logic in each, there are other ones that contribute to shaping each domain, and it is their overlap and tensions that create a sociological interest. In his ethnographic study of a research institution Law in *Organizing Modernity* (1994) identifies four modes of ordering, defined as "recurring patterns embodied within, witnessed by, generated in and reproduced as part of the ordering of human and non-human relations" (ibid.: 83). The first one is enterprise which "celebrate[s] opportunism, pragmatism and performance" (ibid.: 75), performance "in the market by selling goods. Or in terms of some kind of market equivalent, by scoring well on performance indicators" (ibid.: 76). The second mode is administration, which "tells of and generates the perfectly well-regulated organization… hierarchical structures of offices with defined procedures… management as the art of planning, implementing, maintaining and policing that structure." (ibid.: 77). The third one is vision, a mode "profoundly elitist", which generates "charisma and grace, … genius and transcendence", it "tells of the ways in which visionaries cut themselves off from mundane organizational matters" (ibid.: 79). The fourth mode is vocation, which tells of "the creative and self-starting way in which expertise is properly linked to practice", "embodied skills", "the tacit knowledge acquired during the course of a professional training which comes to shape both perception and action" (ibid.: 81). I will employ this framework to analyze the modes in which the organization organizes itself, where, how and for what purposes. The modes of ordering are witnessed in recurring practices and in some cases these practices do not bespeak a single mode.

Modes of ordering	Quality	Institutional organization	Research careers	Science and society
Vocation	Embodied professional skill	Cooperation, communitarian	Apprenticeship, induction into peer community	Autonomy, peer review, long-term benefits
Enterprise	Competitive performance and ranking	Flexibility, competitive distribution of resources	Individualised, fragmented, competitive	External actors, fast economic benefits, marketable
Administration	Adherence to rules	Formal hierarchical structure	Normalised, standardised path	Bureaucratic accountability of process
Vision	Individual charisma	Exclusive awards, exempt from procedure	Individual talent and disposition, repute	Heroic figure of a solitary genius

These four modes of ordering underscore various organizing principles and priorities institutions must and do manage. Through an analysis of particular processes observed at the institution related to the judgement and establishment of quality, organization and distribution of institutional resources, organization of research careers and implicit visions of science's relationship to society, I will highlight enactments of research realities whose position is dominant and strong, and how their strong position is achieved. In a different setting, the realities of another research institution may be ordered predominantly in a different mode. Each of the modes of ordering is built on a set of values and thus enacts its own morality. What establishes quality and rewards for quality is particular to each mode of ordering. Changes in conditions alter which of these versions of good comes to the fore and take central stage. In this chapter I will chart the introduction of a particular type of assessment at the research institution, the type of good this entails, the effects I could observe this has had, but also, and importantly, other notions of quality that persisted and how the institution made them cohere.

In recent work on syncretism Law et al. (2013) develop an analysis of how such non-coherence arising from the meeting of different orderings is made to cohere. The authors identify six modes of syncretism, which they call denial, domestication, separation, care, conflict and collapse. In denial there is no acknowledgement of non-coherence; reality is purified, and the hinterlands necessary for the achievement of particular reality are abject. Denial bespeaks power relations and asymmetry, independence from a particular (and replaceable) actor who because of the power asymmetry is poorly placed to contest the denial. Domestication suggests co-optation through making realities commensurable, turning them into quantitative differences.

This purification turns an issue of substance into an issue of volume. Separation keeps non-coherence in different places, and so it is invisible. It materializes only when the different logics cross and their crossing becomes an issue. Care lives with non-coherence; it acknowledges that although it may achieve coherence through careful and caring tinkering, this is temporary, partial and in another context may be done differently, but then here and for this purpose it works. Conflict is about a clash of authority; it occurs when different logics come together and there is a will to keep reality pure, to oust the other logics. The last one is collapse where logics are not purified, are kept side by side, non-coherent. Remember the first encounter with "a certain Chinese encyclopaedia" in Foucault's The Order of Things (Foucault 1994: xv)? While such an ordering may seem nonsensical, it makes sense in some particular order, when the goal is to achieve a particular thing.

Modes of syncretism	
Denial	Non-coherence denied, reality purified, power imbalance and asymmetry
Domestication	Non-coherence co-opted, made commensurate, an issue of quantity rather than quality
Separation	Non-coherence displaced, invisibilized, managed through temporal spatial or other type of separation
Care	Non-coherence managed through tinkering, temporary and partial achievement, non-coherent realities acknowledged
Conflict	Non-coherence cannot be denied nor domesticated, realities have the power to speak, cannot be silenced
Collapse	Non-coherent realities posited side by side, opportunistic

As I already argued in chapter 1, non-coherence is an integral part of enacting realities, including the science domain. Non-coherence is not a phenomenon particular to shifts and changes; rather, it may be that in times of flux non-coherence becomes more visible. Furthermore, non-coherence does not necessarily lead to disintegration but rather to managing multiple claims and demands on institutions and individuals. As we will see in the analyses that follow, non-coherence is part of the everyday; for example, we are constantly interpolated by the conflicting, non-coherent demands of the family concerns, professional selves and personal desires. The crucial choice is which type of syncretism to manage non-coherence is selected. This choice is political in the sense that it entails a decision as to which type of engagement with power takes predominance. Some modes of syncretism build on power explicitly as when a different version of reality is denied, silenced. In politics we often find domestication, as when non-coherent arguments and justifications sit side by side in a document, a speech or a

law. In other contexts, such as medical treatment and illness, the mode of care is more appreciate as it stresses contextuality – sometimes it may be right to tell the truth but not always; in the mode of administration good care means that due process is taken and all available treatments administered. In the mode of vocation there may be recognition that severely ill people do not need due process, all types of treatment. Science very often, especially toward the public, has managed non-coherence through denial (differences do not exist) or separation (differences exist but must be addressed outside science). In this sense, reality making is political, always, because decisions are always made about what type of good to choose and what to do with goods that do not cohere.

3.1 Excellence: More than one and less than many

The institute where I carried out my fieldwork is fairly large, with around 500 research staff, and successful, with high international visibility. It was this particular reason that our research team originally selected this institute as its first candidate for the research. It was probably also the standing of the institute and its self-perception that featured powerfully in the decision of its director and the lab leaders to let us conduct our research there. In its rhetoric the institute aimed high. As one researcher told me, "*we aim global; otherwise it makes no sense to do it*".

In 2005 Act No. 341 on Public Research Institutions was adopted, which brought a change in the legal nature of state budget-contribution institutions, including all institutes of the Academy of Sciences. The reason for this legislative change was partially due to participation in the European Commission's Framework Programme, as the original state budget-contribution institutions did not fully comply with the definition of a public research institution, but there were other reasons as well. It was argued that the change in the legal form would give institutes greater freedom and would allow them to establish for-profit organizations to help them to commercialize their research results.

In the same year the institute started preparations for another transformation, following a recommendation made in 2004 by the institute's international advisory board set up to help to steer the institute towards achieving international excellence. When we started negotiating our access to the institute in spring 2006 and then started our research there, the legislative transformation was completely invisible, in the background and

secondary to the internal reorganization the institute was just undergoing. It appeared to be of no concern to researchers and was regarded as a distant administrative issue. On the other hand, the internal transformation was everywhere; corridor talk attested to fears and uncertainties as well as hopes: Who would be dissolved? Which groups passed? For some it was a major source of stress, a threat for their future, for others it was a source of great expectations, of getting things *"the way they wanted them"*. The defined goal for the transformation was to create *"an elite institution at a national level, above-average in the international contexts and distinguishable at the international scene" (P 77, L 11)*[27].

The transformation presupposed the award of a limited number of distinguished chairs, ten to fifteen senior research groups and five to seven junior research groups. Junior groups were awarded for three years and senior for five years, after which period the groups would undergo re-assessment. Senior groups would then be either moved to the distinguished chair status, their financing would be continued as a senior group or, if they did not perform well, would be disbanded "*uncompromisingly*" (P 77, L 22). The transformation and assessment of teams was tied to financial rewards. Team performing very well would be given a larger chunk of the institution's budget. This would mean more machines, more equipment and more man power and thus, as we shall see, more results. The teams that performed well would be better placed to perform even better in the future through having additional resources.

Institutional documents make it appear as if there were no national research and development policy. The transformation process was not linked in any of the documents available to policy concerns, specifically the Methodology instituted in 2004. Increasing scientific excellence of the institute was the *raison d'etre* behind the transformation process. The documents enact the process as standing alone, propelled by a self-contained logic of a research organization striving to be increasingly better, taking heed of the advice of the international advisory board, peers commanding respect, with international standing, constituted precisely to help to steer the organization towards greater international scientific status. The transformation builds on a diagnosis presented by the international board in 2004, which does not mention increasing the number of impact factor publications or keynote presentations. It speaks to a scattered

[27] In order to preserve anonymity, I cannot use the titles of the institutional documents. For this reason, I identify the documents by the number of the primary document (P) and the line in the text (L).

organizational structure and research focus as an obstacle to increasing the institute's standing. While there could conceivably be other routes to overcoming these organizational drawbacks, the institute adopted a competitive procedure to award funding for research groups to individuals whose scientific excellence was to be documented by impact factor papers in major international journals and invited lectures to international conferences. These were to serve as the major criterion "*of the highest relevance*" (P123, L34) in the transformation. The transformation based on a research assessment is enacted here as abstract, straightforward and de-contextualized, an unproblematic measure of an individual's scientific worth, a way to identify, separate the good and the bad.

Although at the moment I was entering the research site in 2006, it may have seemed that the stress on achieving the world-class status through publications in impact factor journals had always been in place, the process of visibly and vocally transforming the institute started around 2000 when an internal assessment system started to be discussed. The transformation was thus not the first instantiation of research assessment at the institute. The institute started assessing teams and individuals in 2000. The process was gradually tweaked and institutional documents signal issues that the executive bodies of the institute had to address. At the start the "*scientometric assessment*" consisted of an average number of IF papers per researcher (with a weigh of 0.65), summary IF per researcher (with a weigh of 0.25) and an average number of contributions in peer-reviewed collections and at international conferences (with a weigh of 0.1). Above-average teams were to receive 5% more per team member from the core funding. In 2004 it was decided that only scientists[28] would be included in the assessment. Since the start the results and rankings of teams and individuals were published, and there were suggestions that the ranking should even be displayed on the institute's website. In 2004 it was decided that the results would not have a bearing only on the distribution of core funding but would be also considered in internal competitions for small and large investments and distribution of lab space (P123, L 362). Later that year the executive body of the institute discussed "*certain penalization of groups which 'suffer' in their midst a scientific worker with low to zero publication activity*" (P 123, L

[28] "A scientist is a person who received the title Candidate of Sciences or the academic title Ph.D. He works independently, performs demanding and difficult work in his scientific field. He publishes regularly in domestic and foreign peer-reviewed journals and is usually a principal investigator of grant or programme projects." (Akademie věd České Republiky 2008)

237). In 2005 the executive body suggested that poorly performing staff should be disciplined or fired while it also recognized the positive effects the internal assessment was having on individual's performance (P123, L201). This was repeated in 2006 but then a new issue appeared. In 2004 a new team of computational chemistry came to the institute from another institute of the Academy of Science. In the course of two years it became obvious that there were significant differences in the speed with which experimental and computational teams can produce results that can be interpreted and written into papers. In discussions of the 2006 internal assessment in the executive body "*[S]ome voices proposed to separate the assessment of theoretical and experimental staff.*" (P124, L15). This issue surfaced time and again during my research through jokes and jibes among individual researchers, and continued to cause tensions and feelings of injustice, primarily among some of the group leaders in experimental chemistry. But the institutional assessment system, documents and leadership continued to deny this material-epistemic difference, thereby keeping the unity of the research assessment at the institute intact.

In what follows I will tell three stories of the research assessment at the institutional level, each problematizing in a different way the unity of the institutional research assessment. Contrary to the underlying assumption that research assessments are about measuring some inherent quality and merit of scientists and research groups, these three stories point to some of the material-epistemic complexities underlying research assessment.

The story of resources: Denial

This is a story where non-coherence arising in consequence of the introduction of research assessment is denied. The transformation of the research teams, enacted in the institutional documents and researchers' discussions, centred purely on research assessment. But of course, it was impossible to keep it locked only there. As the teams transformed into new groups, machines, equipment, samples and people were reorganized to belong to a particular group. The groups were not yet fully constituted, and discussions had already emerged regarding division of office space and equipment, which now formed a resource for a team as well as liability. In laboratories things get messy, literally. Cleanliness is a major concern in the biochemistry and molecular

biology groups. If another group just borrows the equipment, how to ensure that they keep it clean when they do not have any stake in it? The team owning the equipment has to take care of the equipment and is responsible for the equipment to be functional. But what if it broke down when another team was using it?

The issue of cleanliness was not the only one. More important, it started to transpire, were proprietary issues. Should other teams compensate the owning team for the use of equipment? And how, should they, for example, pay – an hourly rate or a lump sum? Here we can see one feature of the mode of enterprising. Collaborative features that were subterranean to the operation of the teams came to the surface of the enterprising organization, and became complicated. Things that were freely shared could now be seen as a commodity, as an instrument that played a part in the assessment of the team and individuals. If one team owned and controlled particular equipment, it also controlled its schedule and thus availability. Such control of a particular type of machine could mean that another team could be slowed down in performing its measurements, obtaining results and thus having anything to analyze for journal papers.[29] How to ensure that teams that controlled equipment did not use their position strategically, in the internal competition with other teams? The issue surfaced several times in group meetings, and while these discussions always closed with some sort of a reference to a gentlemen's agreement among group leaders, the fact that it kept coming back suggests that in an entrepreneurial mode equipment and samples became a resource, and this recasts how the group leaders treat them. The vitality of all types of resources becomes highly pronounced in the enterprising mode, its importance cannot be denied and it alters the way people regard these resources. It alters the reality of research collaboration within the institute where solutions that worked in the past had to be renegotiated.

Other resources (the number of students, the number of computers and the computational capacity) came to speak in the assessment, too. Unlike the experimental teams, the two new computational groups succeeding the original team did not (indeed could not) divide the large servers (clusters) necessary to perform their calculations, which were stationed in a special room with carefully maintained conditions. In the internal assessment as well as for the re-assessment of teams, they started competing

[29] In the next chapter these concerns will be address at the individual level, in terms of perceptions of the possibility and growing difficulty of collegial sharing of information about what a person is working on.

with each other and the rest of the institute. This is when the various capacities to perform 'jobs'[30] became a resource that affected calculations and thus results, texts, publications, performance and rank in the internal assessment system.

Modelling is based on the power of the computers. As the power and speed of computers accelerates, so do calculations. Some calculations are however very time and memory consuming. Therefore, a rule has been devised that no person can have more than three jobs running; if there are more, they get in a queue. Everyone had their own password and access and so this should be tracked easily. It was forbidden to give other people one's access login so that they could run additional calculations when their quota was up. However, several people in one of the computational groups complained that these rules were broken by the other group and that people used access to the boss's account when he was away. The group leader did not want to fight over this ("*we have not caught anyone with their fingerprints there*"), and hoped that the situation would get sorted out when his group buys clusters which would be controlled solely by his group.

The number of jobs a group could run then depends on the number of its staff, particularly students and postdocs. This issue also caused tension. The ability to attract students and postdocs depends on the group's finances. The institute had a rule that students would have one year's full time equivalent which they got paid. Usually, students divided this into three years during which they were expected to finish their doctorate. For the students, this was an important aspect of getting their degree at the institute, not so much in monetary terms because broken down it did not account to all that much in terms of a monthly salary but on a symbolic level, it meant appreciation as well as accountability to the group leader for completing their work. The ability to attract students, however, had also to do with the distribution of the space where the two computational teams were stationed. Closest to the entrance of the institute was the office of the more senior and older group leader. As a team member in his group remarked, "*students don't get past that door*". Of course, this was an overstatement and students did make it to the other group or independent scientists who worked in the group of this leader. But there was a clear imbalance in the number of students getting their degrees with this person. Since the institute assessed individuals as well as teams

[30] A job is a computation inputted in a computer to model a certain physical chemistry situation, such as the pH of surface water. Jobs vary in length, some are very short but the very complex ones run for as long as a fortnight. The software to run the jobs can be purchased but students and postdocs also write their own scripts.

until a year after I left the institute (when the individual assessment was dropped), it was very significant for these independent scientists to have students as well.[31]

We can see here that the introduction of a competitive assessment system affected very concretely research cooperation, sharing of equipment and perception of resources, and these were aligned with the competitive, entrepreneurial logic of the assessment. Furthermore, the highly individualized assessment that remained institutionalized even after the transformation into teams had the power to create tensions not only across teams but also within them. The individualized gaze of the internal assessment system turned machines and students into resources for which group leaders competed against each other and also against their team members, the remaining independent scientists. Lastly, the material-epistemic differences between different types of groups became relevant in terms of research performance and publication output. But the institute and its internal assessment system continued to keep all these differences and tensions mute, denying their existence and maintaining a single assessment to judge research and researchers.

The story of acknowledgement: Care

The picture arising from the internal documents and discussions relating to research assessment may create the impression that the only thing that mattered for the institution was excellence. The tender was claimed to be uncompromising. The discussions in the executive body show that over the years the institute got ready to introduce strict rules, and fire or discipline poorly performing individuals. But then, discussions and results of the tender procedure also show a different logic.

Sometimes it may be necessary to take into account other aspects of institutional life that the research assessment and stress on excellence make invisible. At the institute there was one group headed by a leader whose performance had not been historically

[31] The assessment of individuals was criticized at my time at the institute precisely because of the power imbalance between the group leader and the remaining senior researchers in these groups. After all, the group leader controlled and was responsible for distributing the resources, as well as for organizing work and distributing research topics. Furthermore, since the groups were awarded to a particular applicant, the group leader, and were reassessed as such, failure or success of the group was the responsibility of this group leader. The distribution of tasks and roles within a group could be organized to achieve the groups success, with different people expected to ensure different tasks. This meant that not everyone would necessarily have a high publication count.

very good. In the assessment of individuals he ranked in the bottom quartile and his team did not perform well, either. If one looked at the teams according to the performance in the three years preceding the transformation, his and his team's was indeed one of the worst. And yet, this person was awarded a group and has one today. In the entrepreneurial mode the group should have been discontinued, as there is no justification in terms of research excellence to warrant his position.

Regarded from a different mode of organizing, the picture changes. This person was extremely well organized, he understood institutional rules, was the 'right hand' of the director, performing many of the mundane tasks related to the management and administration of the institution. He also took care of some of the outreach activities, which do not show in the research assessment but are important for the institute in terms of its public image and its attractiveness for students in particular. This outreach programme was aimed at secondary school students, an important pool of potential future labour force, as we have seen in the story above. In the administrative mode, the merits of this person become visible.

So how was this non-coherence smoothed at the institute? The assessment of this person is highly contextualized; it is not only his research assessment but also other services for the institute which are deemed important. There is the logic of enterprising and the logic of administration and they do not cohere. In this particular case these modes are balanced; a contextualized way is found by the director and the institute's leadership to keep this person and his team. There is some tinkering of the research assessment, tinkering in the sense of considering other aspects. There is recognition of two logics having to co-exist, excellence in the entrepreneurial mode and functionality in the administrative mode. Both are useful for the organization. This solution does not establish a general pattern; it is a contextualized way to care, in a way, for the institution. This is the mode of care.

There is another way that care manifests itself in smoothing non-coherence at the institute. We have recalled already that the documents mentioned 'uncompromising' treatment of poorly performing teams. In discussions, however, even the most successful group leaders evoked a different value: "*we have to be social*" meaning that there must be some interim period of six months to one year for the teams to be closed down to wrap up. It is inconceivable to just fire people from one day to the next. This is

a concern that should not appear if we are strictly within the entrepreneurial mode where personal issues are of no concern.

The social played itself out in yet another way at the institute. Here is an early stage researcher who was disillusioned by the outcome of the tender procedure. The tender and its criteria held a promise to him to "*air out the dark spots*" but then when he saw the results which were not based solely on the results in the Excel table, he realized that other concerns, generational, played a role.

> *"I was at a birthday of this lady a month ago and I was looking at her photo albums. As I was looking at those photos, of course, on a personal level I got it. All our bosses were there, sitting around a fire with guitars, at our age."* (focus group with early stage researchers)

In the discussion of the early stage researchers that ensued other people also admitted that the procedure was not by the book but then "*they're just people*", it's probably "*much more complicated to deal with the current situation*", and they closed, semi-jokingly, that "*it may be a matter of time before we sit around a fire and make decisions in the councils*". In this account the strict rules of the research assessment are made to cohere with other concerns, in this case personal ties, camaraderie, and common history. Even if institutions may perform outwardly not to consider such issues at all when judging quality, such concerns may be in fact necessary. There is recognition of sociality, of bonds among people, of the fact that an organization can hardly be organized purely according to the competitive logic of strict performance and league tables. This situation, of course, is not clear cut: these early stage researchers are placed in an asymmetrical position vis-à-vis the leadership of the institute. They are poorly placed to complain, for example, directly to the director, the international advisory group or the executive body of the institute. Dissatisfaction with this sort of tinkering of the research assessment thus stayed in the background, silenced, denied; it did not get to the forefront where the logic of enterprising would clash with this logic of care. Discussions of nepotism and favouritism occasionally surfaced in other interviews, as well, but even when people complained, they did concede, when they for example discussed the composition of their own teams, that they would not want anyone tinkering with their decisions and that personal aspects are part of hiring decisions. It is not always only the curriculum vitae of a person and his or her publication record that persuades them to hire a person. And similarly, other concerns may persuade the

director, the executive leadership and even the advisory board that other considerations must be taken into account in the award of groups.

The story of two assessments: Domestication

The last story I will tell is a story of two assessments. For, indeed, there are two assessment systems in place at the institute. One is the assessment of publication activity, developed since 2000, which originally assessed individual and team performance and which assesses groups today. As I discussed, this is an assessment that falls within the organizational mode of enterprising, with its stress on competition and competitiveness, growing performance, and commodification of resources. This is the logic that organized the transformation of the institute and the formation of the lean organizational structure. It affected distribution of core funding, lab space and other resources, and was considered in internal competitions for infrastructures. It ranked people and teams according to a factor of publication activity and was used to guide the re-assessment of research groups. In this logic poor publication performance can have consequences.

The other system was institutionalized by the Academy of Sciences pursuant to Directive 2/2002 of the Academic Council of the Academy of Sciences in response to Act No. 421/2002 Coll., which replaced the existing salary system of 12 salary brackets with a 16 bracket system as of 2004. In accordance with the Directive the two original qualification levels (expert and scientist) was replaced with a system of five qualification levels (expert, doctoral student, junior scientist, independent scientist, senior scientist). The attestation system was further specified in the Career Order of Staff with Higher Education of the Academy of Sciences (Akademie věd České Republiky 2008), which divided the category of a junior scientist into two, postdoctoral fellows and associate scientist). The attestation was carried out for the first time in 2003, several years after the internal factor of publication activity, to assess individuals during their employment at the Academy's institutes. According to the Academy's internal norm, scientists are assessed according to "*scientific creativity and performance*" at least once every five years. It covers all researchers from the position of associate scientists (people with completed higher education) to senior scientists.

The attestation is located within a different logic. It builds on a notion of a stable institution where employees, scientists, grow professionally. It is a way to "*increase scientific quality of institutes, stimulate active attitude among assessed workers, provide opportunities for self-reflection and compare quality of researchers' work*" (Akademie věd České Republiky 2006: 3). There is no talk about firing people if their publication record is not high. Unlike the other assessment system which can be considered judgemental, this one is developmental[32]. It is based on a notion of a traditional career with a gradual progress up through an organizational hierarchy. It is an assessment system that entails a professional progress to a position of a scientific leader (senior scientist). But before one reaches this position, scientists perform complex research tasks, act as principal investigators and regularly publish. It reflects the historical organization of the Academy of Sciences where a person would come for doctoral training (formally candidature), and after completing the doctorate may stay, work scientifically and progress. Of course, this would entail fellowships and international exchange but there were no strict rules about leaving after completing the doctorate.

This attestation system was not addressed once during my field work and it would go unmentioned if I did not bring it up in interviews. There is a reason for this, given by the changing organization of research in the natural sciences which I discuss in the next section as a shift from dynastic to dynamic organising. The attestation bespeaks a greater organizational stability and durability, which is linked to a different organization of funding where core funding forms a majority of an institute's budget and ensures long-term development of an institution. It is a system where contract research staff does not exist or is marginal; scientists are assessed in intervals according to their rank and recommendation of the previous assessment, with the implicit presumption that if the review by the attestation commission goes well, their contract will be extended. There is no obvious logic of competition with other teams or other researchers; of course, there is an expectation, a benchmark of how a person should work and what the attestation commission should judge but it is a different logic from the re-assessment of

[32] According to Townley (1997: 6-7) *"[A] judgemental system places the organization's concerns with control and a centrally coordinated information system to the centre. Used as a tool for resource allocation… documentation is accessible to central administration and is the basis of compensation, promotion and disciplinary decisions*." A development system "*sees organizational benefits accruing from individual commitment to, and trust in, the scheme. These schemes are designed to identify individual strengths and weaknesses and develop skills and abilities. Intended to facilitate the flow of information, and reduce mistrust, they deny the link between appraisal and reward/punishment of traditional or judgemental appraisal schemes*."

teams where the two worst are to be disbanded. This, what I refer to here as dynastic organization of the institute, its funding and assessment is untenable in the changed conditions after the transformation and the introduction of competition in everyday procedures and practices.

To deal with this duplicity, the institute employed another mode of syncretism, a way of making non-coherence cohere, and that is domestication. What scientists submit for the attestation is the same information as that submitted for the internal assessment. The stress is again on IF publications, the factor of publication activity, invited or keynote presentations, domestic and international grants, presentations at international conferences, and patents. The institute's assessment criteria developed since 2000 domesticated the Academy's system when it was introduced, subsuming it within its competitive logic.

3.2 Dynastic and dynamic organising

The tender marked a crucial shift. Against the stability of the former teams, the transformation created space for a dynamic formation and dissolution of groups depending on their performance, stable publication record and ability to get grant funding. It is not only the institutional organization which became dynamic, but also the organization of research teams, too. Previously, there were few research departments with a head and several specialisations developed by independent scientists[33] who answered to the head and supervised their own students. The composition of such teams was stable, and changes occurred mostly upon the head's retirement. Upward career progress (team leadership) was limited but job security was high.

After the transformation, teams were re-organised along the mould increasingly found in the natural sciences: the hierarchy of the 'boss' (group leader) – postdoctoral fellows – PhD students – MA/BA students. The position of the independent scientist lost its moorings, and those who stayed came to be seen as remnants of the past, either headed for retirement or expected to apply for their own team in the next round of competitions. The institute was to be increasingly based on a circulation of early-stage researchers on

[33] The expression 'independent' is crucial here: it is a person with their own research focus who trains their own students.

short-term doctoral and post-doctoral contracts, with only a small core of group leaders and lab technicians having a more permanent contract (but let us recall the group leaders and their groups are periodically re-assessed).

Formerly, independent scientists were typically responsible for the development of partial lines of research, transfer of tacit knowledge in a lab, everyday organisation of work and socialisation of early-stage researchers and students, not to speak about administrative and caring work (Garforth and Červinková 2009: 136–137). Independent scientists also had a strong collective aspect which especially women independent scientists cherish (de Cheveigne 2009: 126–127). A large portion of the work independent scientists performed may be considered feminine in the prevalent gender order, involving mundane, repetitive and caring work.

My data show that while postdocs and senior students can, to a certain extent, ensure working in the novices and transfer of tacit knowledge, their temporary status at the institute cannot ensure these tasks fully. Moreover, the attachment of doctoral students and postdoctoral fellows to the institution is limited. Their temporary status loosens their loyalty and the power of the institution to make claims on their time and bodies.[34] The following quote shows that the change in the organization of research groups is having epistemic effects, as well. Not all tacit knowledge manages to be transferred, not all projects find continuity.

> *"It's not good when changes come in waves. If they do, it gets hectic. As we're doing more and more projects, it's more difficult to maintain but then you can't get spastic about it. Many projects have the life of a student, and that's all right. If a project picks up, then I have to look who's going to take over and then it's always good for there to be an overlap, at least a year so that a diploma student can work with a PhD student.*" (group leader, male)

[34] During my period at the institute, a small uprising was going on among foreign postdocs in particular, who refused to participate in the invited lectures the institute organized, particularly if the topic was totally different from what they worked on. Of course the institutional interest was different: to have as many people in the audience and give respect and recognition to the invited speaker. Group leaders were thus ordered to ensure that everyone attended. For the purposes of the invited lectures, the institute refused to recognize the different attachments that people will structurally feel to the institute given their different status. All scientists were considered to have an equal status and were equally exposed to institutional claims. Of course, in other situations, when the early stage researchers might need to invoke precisely this equal status, it was denied (such as when they wanted to address the issue of the multiple computational jobs running under one login in the other lab because this was affecting their ability to perform their jobs and thus produce papers, or when they wanted the institutional rules concerning the use of the institution's cars to apply to them as well).

Červinková analysed these changes in terms of a shift from 'dynastic' to 'dynamic' labs (Červinková 2010; Linková and Červinková 2013). The term *dynastic lab* refers to an arrangement where upward career mobility is minimal, often though not necessarily the labs are larger and researchers pass through the scientific path to the position of an independent scientist. Establishing one's own group is far more difficult; lab leadership is a matter of succeeding the lab leader after he leaves the institution. This is where "dynastic" comes from. This arrangement reproduces personal as well as cognitive continuity of research, and the lab continues along an outlined direction of doing science far more often than if a researcher established his own lab with his own co-workers in a dynamic system. The position of independent scientists is an important aspect of the dynastic lab.

In contrast, in labs organised on the dynamic principle the lab leader is the only fixed point and other members of the lab doing research – postdocs, PhD students and MA and BA students – work there for a limited period of time and gradually change. One notable exception is technicians as they are support in nature and not subject to the dynamics of change that pertain to fellows and students. The organisational arrangement of the lab is dynamic in that individual members do not form long-term links to the lab and the lab leader; after the completion of their studies or postdoctoral fellowship they leave. If they stay in science, fresh doctors move to a postdoctoral position and it has become a rule at bioscience institutions claiming excellence in the Czech Republic that this postdoctoral phase must take place at another institution, ideally at a prestigious lab abroad. Postdocs strive to secure another postdoctoral position or, ideally, the position of a junior lab leader. The apex of the academic path and a condition for remaining in academic science is the position of a lab leader and the formation of one's own team consisting of students and postdocs.

In Chapter 1 I have charted changes in the organization of science over the last thirty years, particularly as pertains to how science is held to account through performance-based research assessments, and how this is increasingly tied to funding. The dynamism we are seeing here in the organization of the institute and research groups is directly related to changes in research funding. The dynamism of funding necessitates dynamism in groups. The entrepreneurial mode of competitive funding aligns the organization of research work and research institutions but it also aligns epistemic practices. Excellence that the institute (and not only this institute) promulgates is

therefore not a result of some inherent trajectory toward growing research quality but is tied to a particular organization of research funding, funding which revolves around competition for grants, competition for short-term fellowships at the postdoctoral level and competition for teams at the institutional level, with an institutional budget tied to its performance.

We see that the entrepreneurial logic of competition is increasingly claiming the traditional core funding in science. At this particular institute, the internal assessment originally affected core funding, distribution of lab space and competition for investments; today, the core funding together with other sources of funding are distributed through the competition for entire teams. In contrast, the attestation system of the Academy contains no express link to obtaining competitive funding, and there are no rules governing the time researchers in various positions may spend at an organization (only a rule governing the time a person can remain in the position of a postdoctoral fellow). The professional paths of scientists in this system could evolve at a single institution, with their positions funded through core funding. As we have seen in the preceding section, the institute I studied managed to co-opt this system by domesticating it within its competitive, dynamic research assessment practices tied closely to changes in the allocation of research funding.

Clearly, the dynastic system would present a different set of problems; since upward mobility was extremely limited, there would be issues of favouritism and nepotism, and competition among team members to become a group leader's protégé can easily be imagined. My concern in this section lies elsewhere: the introduction of a competitive system at all levels (distribution of core funding for the institute based on assessment, distribution of core funding within the institute based on assessment, distribution of equipment, lab space and investments within the institute based on assessment) necessitates a particular work organization. If funding and resources depend on performance, and performance becomes money, literally, a high degree of uncertainty is introduced into the system. As a result, long-term team stability is not tenable. Fragmented and performance-based funding and distribution of resources fragments the organization of the institution and fragments the organization of an individual research career into stages with its own particular competitive sources of funding, again distributed through a system of assessment. My claim is that the introduction of entrepreneurial competition in the organizational pole of the triangle necessitates a

different organizational logic, one whose tempo is tied to the tempo of the assessment, is fragmented and unstable.

3.3 Multiplicity: Enacting a successful organization

We have seen in the preceding sections that with its internal assessment system and the transformation and attendant reorganization into a dynamic organization, the institute visibly enacts as the most important mode of its ordering the entrepreneurial, with its stress on competition and competitiveness and development of promising lines of research according to opportunistic considerations. The presented and presentable success of the institution lies in papers in prestigious (high impact factor) publications, development of international cooperation with prestigious research institutions, stress on patenting and ability to obtain grant funding.

This is reflected in the demands placed on group leaders. They must be able to function as excellent scientists as well as good managers who can control human resources, secure funding for their projects and students and postdocs. The stress is placed on an excellent individual who feels an opportunity and is willing to change his or her research focus to accommodate a new progressive line of research. Two groups at the institute expanded and shifted their research orientation in response to new opportunities to include protein and HIV research. Other teams were closed down during the transformation about which one of the existing lab leaders said: "*And we cannot say that these team leaders were bad, they only worked in a particular topic which probably wasn't so competitive here at the institute*." (group interview with group leaders)

However, in addition to the entrepreneurial mode, the institution must also ensure other types of activities and other types of organizational practices, successfully. In the administrative mode the goal is an efficiently and smoothly run institution, with a clear chain of command, and adequate and clear procedures. In much literature on the corporatization of the higher education and research sectors, the rise of a new managerial-administrative class has been documented, in opposition to the falling ranks of scholars and especially tenured or similar positions (Donoghue 2008; Giacalone 2009; Shore and Wright 2004; Steck 2003). These studies assess the rising salaries of

the new administrators and managers in universities against the growing precarization of research and teaching staff, and raise the issue that while new governmentality regimes are introduced to assess the performance of scientists, those who administer such assessments are not held to account, for example in terms of how they assist scientists in their research work. Other authors chart the rising administrative demands placed on researchers as a result of the growth of accountability practices (Shore 2010). These tendencies were not in evidence during my time at the institute. In fact, the tender for the new teams and the resulting "lean" organization of group leaders answering directly to the leadership (director) of the institute held out the promise *for* the group leaders of lessened administrative burden.[35]

The tender kept the research staff and administrative-economic staff separate; the tender was called only for research teams, and not, for example, for the head of the economic unit (P77, L33). Held to account and show the results of their work in terms of a measurable index were therefore only researchers; there was no assessment in place for the administrative staff, or an institutional assessment to address the potential changing administrative demands in response to the research transformation. Here we can see that the changing governmentality regime does affect only research staff held to account for their performance, rather than holding the administration to account for their support provided to the research staff.

Since I finished my fieldwork, the administrative section at the institute expanded, and includes today several new departments, built apart from the original administrative unit. These new departments cover grant, legal, patent, personnel and public relations issues. These new departments have not been created through a restructuring of the original technical and economic unit, which is a unit traditionally instituted in the Academy of Sciences institutes. This original unit has remained in place, separated from these new departments which are attached to the director's office. This may suggest that the institution is maintaining separation of the traditional unit and the new sections the need of which has arisen in response to the new demands of the entrepreneurial mode. At my time at the institute I was able to see only the beginnings of this additional administrative transformation. The first issue that the institute, quite tellingly, started

[35] Group leaders actually expected that the transformation would be good in terms of lessening the bureaucratic burdens and that they would have more time for research. It is a question whether they still would make this claim today; given the development I will shortly discuss my guess is they would not.

addressing was intellectual property. An auditor was hired to assess potential IPR needs of individual research groups so that the institute would not miss out on any potential marketable and commercialization opportunities. We can then see that to perform successfully in the administrative mode, the institute gradually carried out changes which help it to ensure a growing array of demands arising in the entrepreneurial ordering of the organization. Other institutes which do not command large sums of money arising not only by way of competitive funding but also through international patenting, can meet these new administrative demands arising in the entrepreneurial mode with more difficulty. In fact, there are not many research institutes in the Czech Republic that have as wide an array of administrative departments to ensure these individual tasks as we find at this institute today. More begets more does not apply only in terms of assessment of researchers and teams (thus increasing existing inequalities in resources), but it applies also in the administrative mode where more complex sources of funding involving greater arrays of actors (not just the researchers but also industrial firms and others) beget more administration.

In the mode of vocation, the concern lies with everyday epistemic practices. According to the research participants the transformation process played no role here; in their opinion the epistemic and organizational work are separate.[36] "*As for science, nothing dramatically changes*," stated a group leader. Success in this mode revolves around an everyday, often slow research process. Students and postdoctoral fellows play a significant role here because they work in partial steps on a bigger research project which will be transformed into a concrete result to be published in a paper. Attracting students at the institute is crucial for two reasons. Firstly, they are (cheap) labour force which crucially contributes to the work of the teams, as I discussed above. Secondly, students and postdoctoral students are an instrument for maintaining international cooperation when, for example, a group leader and the foreign team do not have a specific project to work on together (cf. Červinková and Vohlídalová 2012: 14).

The ability to attract students often rests in depicting research at the institution as progressive, world-class and joyous. Most lab leaders who are successful in attracting students talk about the freedom and creativity science offers. This freedom and creativity are not, however, unconditional. Supervisors may be unable to get enough

[36] I will come back to this claim in the next chapter when I discuss coping strategies researchers engage in to deal with the demands of the entrepreneurial mode.

students at various levels and consequently a doctoral student will have to perform tasks he or she considers second rate and which could be performed by students at the BA or MA level. In this particular case, getting enough publications to finish a doctoral project is delayed because the partial tasks this particular doctoral student was working on did not have the potential for a journal paper (field notes).[37] When this happens to a postdoctoral fellow, it is much more serious. In the case of the doctoral student, the completion of a PhD is delayed. In the case of a postdoctoral fellow this means an unsuccessful fellowship because in the entrepreneurial mode the postdoc is a cut-throat phase where fellows must get as many and as good as possible publications as they can.

Secondly, some, particularly male, doctoral students and postdoctoral fellows grudgingly discussed that freedom comes only later, when one becomes a leader and can define his own research topic. Such a depiction of science as the preserve of freedom and creativity may then have negative consequences. It keeps mute aspects of research which come to the fore in the other modes of ordering – increasing administrative demands and extreme levels of competitiveness. Furthermore, the vocational mode is not solely about freedom and creativity. It is also about the everyday, often painful process of knowledge making which in the natural sciences by many accounts involves a large degree of failure.

> "*You have to be very patient to be a successful scientist because some 80% of our results are negative… You have to be very patient."* (group interview, group leader, female)

The slow times at the bench work bring "*small*", "*minute"* partial successes, which bring joy in the everyday course of events. These small successes and patience with frequent failures, a certain type of perseverance, are very different achievements and come to the fore only in the vocational mode. We can see that with the introduction of competitive funding and competitive assessment the slow, everyday processes of bench work are reoriented, too. Freedom and creativity, early-stage researchers soon realize, are contingent upon the ability to win funding. What they need is not only perseverance and tolerance to experimental failures but also experiments that do not fail and provide material to write papers for the CV.

[37] At the institute doctoral students must have at least three paper publications with their first authorship. Some of those I interacted had more. Some, in fact, had many more papers *per year*.

The visionary mode centres on an exceptional, outstanding individual, exempt from the general rules. In the visionary mode, the concern is not to be a great manager-researcher successfully competing for funding and scoring impact factor publications. What matters in the visionary mode is "*having it*" what no one else has – an elusive quality one cannot learn or buy. One lab leader described a visionary referring to Prof. Wichterle, the inventor of contact lenses:

> "*You must have ... some have it and some don't. Wichterle had it, there is no greater name in Czech science.*" (group leader, research interview).

One crucial feature of the visionary mode is exemption from instant competition, the underlying logic of contemporary science. In the visionary mode long-term funding without having to annually account for measurable output is conceivable. In one such instantiation researchers at the institute referred to conditions presented by Nobel Prize laureates whose home institutions allowed them to work on the research for which they received the Noble Prize without having to show any measurable output. In various situations scientists mobilized their experience from abroad where an institution had "*confidence*" and did not demand immediate measurable results from their staff. The unavailability of such funding was seen as a major problem of contemporary science.[38]

Increasingly, we can see forms of this exemption in the various excellence chairs and excellence grants, both in the Czech Republic and abroad. These are typically highly exclusive, make hyper-visible the heroic individual, and command greater funding and longer duration. While these new excellence awards bespeak exclusivity and elitism and are enacted within the visionary logic, they are not in contradiction with the mode of enterprising as they can be awarded only on the basis of previous success in the mode of enterprising. The Academy of Sciences instituted one such award, the *Praemium Academiae*, in 2007 to create special conditions for outstanding individuals so that they could better develop their potential to the benefit of the Academy of Sciences and Czech science as a whole. "*The Praemium Academiae, that's the right stuff... The difference usually is that the person is colossal, and the rest is*....", declared one of the group leaders in reference to this exclusive award and the type of colossal person who can receive it (he himself a recipient of the premium). The institute itself instituted a special

[38] Scientists at the institute complained that to be able to complete a grant successfully, one cannot launch into uncharted territories. Some even see the five-year term of the senior groups as too short for a new topic to be able to fully flourish. I will return to this temporal and cognitive aspect in the next chapter.

class of groups, distinguished chairs. Only undisputed personalities with "*brilliant scientific results and clear international reputation*" could be awarded such a chair, which would constitute a sort of tenure, a permanent position where evaluation would be performed "*ad hoc, in a case absolutely extraordinary*" (P77, L21).

This success in the visionary mode is hard to achieve, given the organization of funding and assessment criteria. It is not only about there being very few people who actually 'have it' but also the fact that short-term assessment cannot make visible the type of special quality that is required. When one looks at the proposal form for the Praemium Academiae, there are no slots for impact factors, volumes of funding awarded or a list of grants received. The document contains merely the name, institutional affiliation and a brief description of the most significant results achieved by the proposed person. This is a very different enactment of an assessment procedure compared to the factor of publication activity, the competition for teams or even the attestation. It is, however, fully contingent upon precisely this performance preceding the exclusive award which is so normalized that it does not have to be listed as a precondition. As Felt and Stöckelová (2009: 84) argue, "competition and selection are imagined to be the basic mechanisms for being allowed to gain a place to work under better conditions". It can be argued that the institutionalization of these specific awards enacted in the visionary mode reinforces the mode of enterprising as they depend on previous success in the enterprising mode. These awards are highly exclusive and exclusionary, and underpin the technologies of selection at the lower rungs of the professional ladder.

These four modes, entrepreneurial, administrative, vocational and visionary, usually do not overlap. They are distributed institutionally, separated into their specific forms, procedures, spaces, departments and personnel, and their achievement can often be managed through separation. But the boundaries are not clear cut. We have seen that the entrepreneurial mode is trespassing into the administrative, the vocational as well as the visionary mode of ordering. I will return to the alignment of practices in the mode of enterprising in the next chapter as I examine researchers' coping strategies and changing subjectivities.

3.4 Gendered displacements

Navigating the traditional demands and expectations related to the scientific career has never been an easy issue for women researchers, especially when they had children. The absolute dedication to the scientific profession, the full concentration on a research problem, the distancing from practical matters of everyday life, all these traditional notions are easily complicated by the presence of children, but this complication (or "natural handicap", as I will analyze in the final section of chapter 5) in the gender order of things affects predominantly women-researchers. Researchers-mothers are thus often caught in a double-bind of competing demands placed in the gendered order on mothers and professionals.

> *"When I was coming back after the second maternity leave, I had such a bad feeling. For some I was a bad mother because I was back at work, for others an irresponsible researcher because I should have been back here a long time ago.*" (independent researcher, female)

In this section I will tell two stories where the coherence of the dynamic organisational logic is problematised by gendered concerns related to parenthood. In the first story of denial I consider the expectations and demands placed by group leaders on their students and postdoctoral fellows in terms of their performance where the non-coherence arising from parenting commitments are displaced. In the second story of conflict I discuss what happens when pregnancy cannot be willed away in the lab and interferes with the clear-cut organizational logic of the dynamic career progress.

The story of assessment and parenthood: Denial

While maintaining a masculine orientation of the dynastic lab with its stress on linearity and full concentration, the dynamic lab reorients it, along a notion of a highly competitive, opportunistic worker who concentrates on achieving the best results within the available time. This is how a group leader formulated it:

> "*A postdoc cares about one thing only. He wants to launch his career and he needs, in that year or two he will be here, as many publications as possible. A postdoc will give his soul. A postdoc will give his soul to science.*"

On another occasion this lab leader discussed the work exertion necessary in the research profession:

> "*Saturdays, Sundays, there is always work. Some call this workaholism but there is no other way. Either it gives you joy, and if it doesn't, you have to abandon it… I expect this sort of effort, who doesn't want it, shouldn't be here.*"

We can see here a simplistic either-or logic in terms of the job research offers and the effort such a joy should command. In this account, science subsumes all parts of an individual's life. This sort of work ethic must be enjoyed; anything less is a compromise. It introduces a moral against which individuals are judged. Motherhood and professional breaks are a mere post script which this group leader took into account only after I asked him in the interview. When this happened, the ideology of the disembodied worker revolving around total work deployment is replaced with a highly conservative and equally exclusionary, though differently, ideology of motherhood. The same lab leader continued in response to an immediate question: "And what if the family comes, do you apply the same rules in that case too?"

> "*Nothing can be done, now we're dealing with this, a great female Ph.D. student, she is happing married. There is nothing, no higher priority, you cannot forget about a family only for the scientific career, a family must come first, support from the family is necessary, my children can't imagine I would do something else than work …*"

This quote is interesting for several reasons. The first part of the quote before my insertion presumes great exertion necessary to develop a scientific career which will lead to an independent position. In view of the fact that this is a very successful lab leader, he also had the power to enforce these expectations. As van den Brink and Benschop note (2011: 12): "Standards of merit are constructed by powerful academics who stand to benefit from a construction that is presented as a precise, objective, and univocal measure of excellence." (cf. Myers 2010). Another interesting moment in this quote comes in response to the question how a family is reflected in these expectations. Even though I did not use an explicit reference to motherhood or women scientists and formulated the question to refer to family, the answer is significantly gendered. It excludes men as those who could be entangled in relationships of caring and is explicitly formulated from the perspective of women (great female PhD student). Smithson and Stokoe (2005: 156) use the term *generic female parent* or *generic she* to

describe how discussions of parenthood presume that only women and not men are concerned.

Noteworthy about the quote is also the importance attributed to women's parenthood (no higher priority, family must come first). In the case of women researchers there is thus not only the presumption that they will want to dedicate themselves to the family but there is also a value judgement which attributes women's motherhood the highest value. The family is something that cannot be 'forgotten' because of her research career. The morality against which a postdoc is judged is oppositional to the morality against which mothers are judged.

In view of the fact that this quote was framed by a question when and how many hours the group leader worked, it is telling how he returns of his *own* family in close of his answer: *"my children can't imagine I would do something else than work*". He evokes here the original notion of the idealized, uninterrupted, highly consuming work deployment which he demands from members of his group. The insert which came in response to my question thus underlies the otherness of parenthood in the dynamic lab. This is not to say that this group leader could not demand such an extreme performance in the previous organization of the dynastic lab. In the new arrangement, however, the performance takes on the particular orientation of building a publication track record and a competitive CV. This is, in fact, what the postdoc will give his soul for in the contemporary dynamic lab. To return to the quote above, in the forefront we see an absent father fully devoted to his work who claims that his family does not even presume it could be otherwise. Parenthood and concerns of care are denied in the dynamic lab, thereby making it possible to insist on the extreme and full work exertion expected particularly in the early stages of the research career. These value judgements are then used when assessing the excellence of applicants for postdocs. And even though we have seen in the previous section of this chapter that there may be space for tinkering with assessment systems to accommodate other needs and qualities in particular situations, the ubiquity of the assessment systems with their stress on excellence and the most competitive CV, has gendered impact when excellence is judged.

The story of mobility and parenthood: Conflict

At the time I met Ema, she was a doctoral student at the institute. She had been married for some time and was trying to get pregnant with her husband for a year. It was not working out. After seven years at the institute she was nearing the completion of her PhD[39], and found a postdoctoral position in a prestigious French lab. And then, out of blue, she got pregnant. She took a month to consider the situation and then, a month before she was starting her postdoc, informed the head of the French lab. The reaction from the French lab leader was harsh: Ema's pregnancy was improvised, she was called irresponsible. She was accused of putting the research group at risk and of withholding information. She felt betrayed by her supervisor who did congratulate her on the pregnancy but would not interfere in her interactions with the head of the French lab and so the Frenchman's wrath fell on Ema without any mediation, or so Ema felt. Ema thought this was unjust. Some people felt with her. Others thought she was a troublemaker because she did not keep silent about the situation and turned it into a public issue, discussed in the corridors, causing tension at least in two labs.

Another woman researcher, Milena, who has two children, offered to employ her on her grant until Ema goes on maternity leave. Ema was a doctoral student in one group and Milena was an independent scientist in another. Ema did not ask her boss to keep her for the remaining time, proud to have found a solution on her own with Milena. Milena did not inform her group leader of her intention to employ Ema. Both told their respective group leaders after they came to *their* mutual agreement. And the group leaders forbade this solution. After seven years Ema left the institution with a doctorate, unemployed before going on maternity leave which has serious consequences in the Czech social security system, with a taste (or better an aftertaste) of the importance of disciplining one's own body. As she told me, no one of course wanted to hear what this decision was really about: abortion.

In this story of institutional and personal conflict we see one aspect of the dynamic lab complicated by gender and sex related factors. In dynastic labs academic paths were conceived of along the linear, uninterrupted and stable lines of career progression, with

[39] This was an extraordinary amount of time compared to the other students. It was given by shifts in the research topics she worked on. As she became more and more proficient in the methods, she would perform more complex computations, but on different topics. Getting the calculations and publications that would paint a coherent picture within a single line of research for a doctorate thus became complicated.

a high degree of security, stability and continuity, as outlined above. A woman scientist who had a child and went on a parental leave upset the idealized linear and uninterrupted professional path but the long-term outlook of scientific work related to the stability in funding and legal protection in motherhood made the parental leave a manageable episode in a professional trajectory of the woman researcher as well as her lab. Also, historically, the maternity/parental leave was shorter than it is today; furthermore, before 1989 daycare facilities were far more easily available and their use, especially when the children are very young, was not stigmatized.[40] Career breaks and returning back to work depended primarily on reaching an agreement with the lab leader. Šaldová and Tenglerová (2007) argue that in the absence of *any* institutional rules addressing the combination of research and career breaks in Czech research institutes, the lab leader is the main actor who can smooth out or complicate the professional path of a researcher-mother. In most discussions on combining work with parenting at the institute, group leaders featured precisely as such crucial gate keepers, and by most accounts it was possible in the past to find solutions.[41] In the long-term perspective of the dynastic lab a maternity / parental leave was a relatively short episode which could have been integrated in the time of the lab.

Dynamic labs embody fragmentation. Positions are temporary, and individual phases of the academic path take place at different institutions. Postdoctoral fellowships are funded with competitive funding, either special postdoctoral fellowships for which the individual doctors compete, or grants won by researchers who then organize tenders for postdoctoral positions. It may also happen that institutes call for postdoctoral fellows and fund these positions with their core funding. Application for positions takes place in a highly competitive environment of an international academic labour market and clearly must be planned some time ahead. According to my findings, postdoctoral

[40] If 39% of mothers of children born between 1957 and 1974 stayed at home with a child 0-12 months and additional 37% stayed at home 13 to 24 months, with children born between 2000 and 2010 it was only 13% and 16% respectively. On the contrary, 44% of mothers stayed at home with a child 25 to 36 months and additional 27% stayed at home 37 and more months (Hašková 2011: 44).

[41] This started to change after the transformation. Lab technicians in one group for example wanted to take almost all their allocated holiday time in summer months to correspond with the two-month school break. A practice that was apparently possible before the transformation was not allowed now. In the dynamic lab, with the coming and going of postdoctoral fellows and heightened focus on results and publications, such a long absence became complicated.

fellows start looking for a new position some nine months before their contract expires.[42]

Ema's story underscores just how important planning is and how painful the failure to plan and time bodily processes can be. By bringing the issue to the open Ema refused to deny the presence of a different logic in the lab, a logic where it is not possible to make an either-or choice. In her book *The logic of care. Health and the problem of patient choice* Mol addresses the issue of choice in healthcare. She works with the term *logic* because she is after "the rationality, or rather rationale, of the practices" which "invites the exploration of what it is appropriate or logical to do in some situation and what is not." (2008: 8). Through the prism of the logic of choice and logic of care Mol explores some of the basic tenets of western philosophy: the controllability of our bodies, facts preceding decisions and the rational individual at the centre of the decision-making process. In contrast she shows that individuals often cannot control their bodies, that "[f]acts do not precede decisions and activities, but depend on what is hoped for and on what can be done" and that "we do not start out as individuals, but always belong to collectives already – and not just a single one, but a lot of them" (ibid.: 12-13).

In the logic of choice people make decisions, weight options, consider arguments. Choices they make, the logic of choice tells us, are individual, cut from the larger social milieu of a person, rational and dispassionate. The boundaries between the private and public are firmly drawn. The logic of care, on the contrary, makes space for, even counts on, mess. Reality cannot be neatly ordered and, importantly, involves emotions and bodies. The public and private interlace. In the logic of care, making a choice is sometimes not an option; making a choice does not necessarily resolve a particular situation.

The labour market is typically ordered in the logic of choice, building on a notion of a disembodied, free subject of the labour market making rational choices (Acker 1990). Or as ex-Prime Minister stated on the occasion of his speech launching the Year of Equal Opportunities in 2007: "A woman can freely decide not to have children and then I am certain that she has the same professional opportunities as a man." (Topolánek 2007) In the end, Ema had to make a decision, too, and by some accounts the time she

[42] This makes two-year and especially one-year fellowships very problematic as these researchers concede that part of their working time is taken up by searching for opportunities and filling applications.

took (one month) was too long. But too long judged against what? If she made the decision to have an abortion, would one month be also considered as too long? And which collective should she plead allegiance to?

With this story of uneasy choices and judgements, I wanted to underscore the new relational dynamic which enters relationships among variously positioned researchers. With the increasing short-term perspective which the postdoctoral fellowships bring both for the fellow and the lab leader, the institutional and individual competition for this sort of funding and the importance of competitive performance for the fellow and the lab, this sort of disruption due to prospective motherhood creates non-coherence which cannot be easily smoothed. The dynamism of the postdoctoral position and funding makes it difficult for both the parties (Ema and the French lab leader) to form allegiance to each other. The fellowship and the fellow are instrumental for each other in the dynamic career track. They are a resource for each other, an item on the CV and a source of productive publication work. In this case, neither interpolated the other strongly enough and thus, Ema chose allegiance to her partner and their future plans while the French lab leader chose allegiance to his lab and its future performance. In the dynamic lab this non-coherence created conflict, which is arguably more pronounced than it would be in the dynastic lab where, presumably, Ema may be able to go on a legally defined maternity/parental leave and then return to her lab. Here we can see that in the competitive academic labour market, with short-term contracts especially at the postdoctoral phase, new structural constraints arise as a consequence of the dynamism, which re-align and reinforce the gendered organization of the academic labour market.

3.5 Conclusion

In this chapter I have analysed the dominant organizational logics or modes of ordering orienting the natural science institute I studied. I have charted the transformation of the institute in an entrepreneurial mode, building on competitive research assessment by impact factor in particular. I have linked this transformation to shifts in research funding toward increasing levels of funding distributed on a competitive basis. And I have shown how funding changes reorganize research teams from dynastic to dynamic labs where doctoral students and postdoctoral fellows circulate around a group leader and where lab leaders compete for teams on an institutional level.

I have complicated these entrepreneurial shifts, along two lines. Firstly, research assessment at the institute, while it is dominantly carried out in the entrepreneurial mode with potential consequences in case of poor performance, takes into account other factors which the institute has to navigate. I have shown that some of the practices that complicate the pure vision of assessing excellence by calculating publication output are denied. In the research assessment the institution does not take into account temporal and material-spatial aspects of epistemic practices. Other aspects of the competitive assessment are made to cohere through care, as when the institutional assessment makes space for social aspects as well as the various needs of the institution and its functions. Other institutional features are domesticated, as when the attestation assessment is appropriated by the institutional assessment, and the different logic in the original document is co-opted by the mode of enterprising.

Secondly, the entrepreneurial mode of ordering with its assessment and stress on performance may be the one that features prominently in public presentations of the institute and has been the one which organized the transformation of the institute and its research assessment. But there are other modes of ordering which the institute must also practice successfully. I have argued that since my time at the institute the administrative mode of ordering increasingly reflects the entrepreneurial and competitive organization of the institute, particularly through establishment of a new structure of administrative departments. In the vocational mode science at the institute may be portrayed as creative and free but this notion is complicated in the dynamic lab by the long period of apprenticeship involving doctoral and postdoctoral training and the constraints of constant competition for funding. In the visionary mode of charisma and 'having it', the institute awards the rare distinguished chairs but these build on a preceding hyper-competitive performance in the mode of enterprising. Thus, these excellence awards may be seen as further reinforcements of competitiveness of the system, rewarding those who have been able to enact a highly competitive and productive self in the past.

The third complication of the entrepreneurial mode of the dynamic lab I considered was how gender is displaced at the institutional level of assessment and dynamism. I have argued that gendered aspects of how individuals are assessed and what criteria are employed in their assessment are denied. The general applicability of assessment criteria can thus be maintained, and with it a masculinized vision of research performance. In a story of conflict I have addressed how pregnancy and motherhood

complicate the enactment of the dynamic lab and the dynamic career path, in that pregnancy introduces a logic which does not fit the easy either-or binary of rational career choices required of early-stage researchers today as they move from one lab to the next.

We have seen in this chapter a powerful reorganization of a research institution propelled as if by some internal logic of doing outstanding science. But it is enacted in the entrepreneurial mode which orders what outstanding means. Outstanding results are impact factor publications, outstanding teams are dynamic groups with circulating early stage researchers focused on performance, in constant competition with each other and others internationally, for future positions and labs. Rhetorically, the institute continues to organize its practices in terms of striving for quality; its goal is an outstanding status internationally. With the assessment system, the transformation and new dynamism in funding and personnel what outstanding means is re-aligned. Science, outstanding science, quality, thus continues to be the dominant organizational logic in science, but the meaning of this has shifted. In the next chapter I will explore how individuals are reorienting their subjectivities in these changing conditions.

4. Dynamic subjectivities: Scientists' coping strategies for the entrepreneurial alignment

Research autonomy and freedom have traditionally figured as the cornerstones of an academic subjectivity (Henkel 2005; Vallas and Kleinman 2008: 291), together with complete devotion to the pursuit of knowledge (Winter 2009: 122). In the previous chapter we have seen how this notion is becoming problematised by changes in the governmentality regime in science involving competitive shifts in research funding and research assessment. Despite these changes the traditional image of science as a mission which makes claim on a whole person, still holds sway. Science continues to figure as an almost religious undertaking which demands subordination to its internal workings of knowledge production and self-discipline. "*Science is a terrible rule of an order*[43] *if you want to do it at a really good level*", maintains a doctoral student. The image of a fully devoted academic continues to be nourished even as it is becoming increasingly problematic; needless to say, for women-scientists it has *always* been problematic, as we have just seen in the last section of the previous chapter. But today, the notion takes on a distinctive meaning in an environment ordered around research assessment and competition for posts.

In the previous chapter I have focused on analyzing the shifting governmentality regimes re-orienting organizational practices and logics in the mode of enterprising. Here I will examine how researchers' subjectivities are being aligned with these changing conditions and what effects this is having in terms of their epistemic and professional practices. I will argue that researchers are adapting, and that their epistemic practices as well as how they enact their professional paths are aligned in the entrepreneurial mode of organizing. In this chapter I wish to underscore the importance of researchers' subjectivities and of the ways they cope with and contribute to shifting conditions to the achievement of the entrepreneurial alignment. The institutional isomorphism is not only an issue of field and organizational level predictors but crucial depends on researchers devising ways to align themselves, even if partially, with the changes under way.

[43] The word used in Czech was "řehole" which means the religious order and rule of the order but it is also used figuratively to mean hard lot.

Clearly, such alignment is not a one-way process whereby researchers are the passive recipients or active endorsers of change. On the contrary, by examining three types of disconnections, epistemic, professional and caring, I will analyze researchers' ways of coping with the shifting grounds of research assessment to argue that scientists are developing ways which allow them to deflect some of the new demands. But as we will see, for a large portion of them, this brings intense conflict as they cannot find a smooth way of aligning themselves with the new modes of organizing and the values this brings, which leads them to develop various forms of disconnections from science. In this chapter I address the ways in which coherence is achieved and non-coherence handled, how people deal with inconsistence, or *cognitive dissonance* (Festinger 1957). While I do not share the methodological individualism of the dissonance theory, it has been useful in highlighting the importance of aspects such as values, self-esteem, culture and multiple belongings to how people hand non-coherence (Cooper 2007). Non-coherence creates tension. Festinger insisted that "[P]eople do not just *prefer* consistency over inconsistency. …[they] must deal with the inconsistency…" (ibid.: 2). In exploring the entrepreneurial alignment of researchers' subjectivities, I will examine some of the ways in which researchers manage to handle non-coherence, through partial alignment with the mode of enterprising as well as through various types of dis-alignment from the research profession.

In academic literature there is vast interest in changing academic subjectivities in response to changes in the governmentality regimes and the onset of neoliberal reforms. Research literature particularly from the UK, Australia and New Zealand is concerned with the interplay between neoliberal forms of governmentality and subjectivities. Winter (2009) analyzes an "academic identity schism" between the academic manager and the managed academic in UK and Australian higher education institutions. To resolve this academic identity schism, Winter argues that academic managers must develop multiple interpretations and understandings of organizational problems, to be used strategically as they walk the "tightrope" between the traditional and new sets of values (Winter 2009: 128). In her ethnographic study of a social science institution, Stöckelová (2009: 59) identifies precisely such switching as an "important power strategy" to strategically call on the mode of enterprising and the mode of vocation as an instrument of disciplining academics (but not only them, as Stöckelová shows; such switching can be, for example, used to discipline study specializations, too).

In his study of restructuring of New Zealand universities Shore (2010) discusses the flexibilization and fragmentation of academic subjectivities and talks about "schizophrenic academic subjects" developed in response to conflicting institutional visions and managerial agendas (ibid.: 28). Shore and Wright (1999: 569) further argue that academics are caught "between two conflicting notions of the professional self: the old idea of the independent scholar and inspiring teacher, and the new model of the auditable, competitive performer." Numerous authors examine how the adoption of assessment criteria and practices actually renders both institutions and individuals accountable, auditable (Valentine 2004), creating "new kinds of subjectivity: self-managing individuals who render themselves auditable" (Shore and Wright 2000: 57; see also Shore and Wright 2004; Strathern 2000). There is also a major concern about the possibilities of intellectual work in the neoliberal regimes and the responsibility of scientists to challenge these new regimes (Canaan 2010; Davies 2005). Related to this is a concern with the formation of subjectivities of researchers because assessment systems do not change only they way people relate to workplace and each other but, "most importantly, to themselves" (Shore and Wright 1999: 559). These subjective negotiations thus encompass "resisting our own practices, it is about confronting oneself at the centre of our discomforts" (Ball and Olmedo 2013: 93). Archer (2008: 271) then asks whether researchers are realigning their values to fit the new audit regimes or whether the academe is attracting new types of workers.

Despite the large volume of highly critical literature on the impacts of neoliberal, assessment-based governmentality regimes, overt resistance has been rare (Archer 2008). Davies and Petersen (2005) express "surprise" there has not been more opposition and defiance. Other scholars have been quick to note, however, why it is difficult for the research community to challenge assessment systems. Resistance in science, as in other career professions, is difficult because it builds on a heightened degree of individualism, self-management, autonomy, competitiveness and ambition (Anderson, 2008; Karreman & Alvesson 2009). As Shore notes, "the values that most academics subscribe to (including self-discipline and a desire to produce quality research) have become instrumental in eliciting compliance and governing conduct" (Shore 2008: 291). On the other hand, the same desire to produce quality research and professional self-discipline may, in fact, function to interrogate assessment systems and uphold work ethics that subscribe to professional (vocation) rather than competitive

(enterprise) modes of self-organizing. Indeed, with a degree of anti-establishmentarianism and inquisitiveness some scholars will even claim that "[S]cientists are punks" (McCook 2011).

Davies and Petersen (2005: 93) speak of a combination of two technologies produced in the neoliberal academia which turn academics into governable subjects – "a technology of *agency* and a technology of *performance*" (stress orig.) both of which speak to scientists seeing themselves as "active subjects responsible for improving their own conduct" (Shore 2008: 284). While authorship has not always been a dominant feature of the scientific endeavour, modern science has gradually come to revolve around the individual scientist, the importance of discovery and claiming primacy for it. Performance and vanity have been as inseparable from the profession as cooperation and sharing. The new assessment systems change the balance here, and the individualized performance is today at the forefront, as I discussed in the previous chapter.

With the erosion of trade unions and gradual disappearance of organized protest at the workplace in the West, literature on workplace resistance and change has shifted from conceptualizations of 'big', overt forms of resistance to quotidian resistance, stressing the everyday mundane, individualized modes of opposition (Prasad & Prasad 2001; Prasad & Prasad 2000; Smith 2010; Thompson & Ackroyd 1995) and "small acts of defiance in the practices of everyday life" (Shore and Wright 1997: 10). Such conceptualization is well suited for the study of the research profession where overt forms of opposition, as we have just seen, are minimal, and attention is directed to the study of capillary exercises of power in the schizophrenic university (Shore 2010). Another important shift is away from seeing researchers as recipients of externally imposed change. Indeed, in the next chapter when I discuss the introduction of the Methodology in the Czech Republic we will see that there was a group of researchers in the natural sciences that was very active in promoting research assessment at the institutional as well as the national level, and these individuals were in my research sample. As we shall see here, in shifting conditions, researchers are involved in a constant process of adaptation and re-inscription of discourses and practices where they "have the leeway and flexibility to use their existing relations and understandings to incorporate, transform or resist new practices" (Lam 2010: 310). Forms of resistance may take the form of disattachment through processes of "unbecoming" (Archer 2008:

266), and I will explore here these "disconnections", as I term them, in terms of epistemic practices, doing the scientific career and practices of care.

These concerns related to the formation of researchers' subjectivities in changing conditions of governmentality, with its new demands on performance and practices of accountability, raise important issues about multiplicity. Researchers today navigate a highly complex terrain of academic institutions. The ways they enact their self as researchers are multiple and the fact that these subjectivities do not always cohere creates tensions. In the previous chapter we have seen that an institution can deal with non-coherence through denial as well as other modes of syncretism. But how do individuals deal with non-coherence, with "what it is to be neither one nor fragmented into many individuals" (Law and Mol 2002:11)?

4.1 Doing science, doing quality

In this section I explore researchers' positioning in terms of their notions of what it means to do quality science. I will work again with Law's modes of ordering (entrepreneurial, vocational, administrative and visionary) but this time I will apply them to how individual researchers order themselves. The six modes of syncretism will again help me to examine how researchers deal with the different demands and expectations placed on them and by themselves. Studies point to various forms and degrees of academic positioning and hybridization in face of the transformation of the research environment (Lam 2010; Sousa, de Nijs, and Hendriks 2010) and how these are gendered (Barry 2006). These studies further underlie strategic positioning depending on the place, interaction and circumstances, as well as the constraints on how academics position themselves, unable to "exist outside of the conditions and locations within which they are located and by which they are constituted" (Archer 2008: 282). In what follows I will tell three stories. One is a story of domestication where I examine how scientists re-orient their notions of science and accountability to answer the entrepreneurial mode of research assessment. The second story of conflict speaks to the refusal of an IF-based assessment system. The third is a story of separation which allows scientists to distribute research assessment in the entrepreneurial mode and doing science in the vocational mode in temporal and organizational terms.

The story of conditional alignment: Domestication

In my sample there was, in fact, only one person who wholeheartedly endorsed the competitive assessment system as a way to organize science and its quality, and even this person recognized the limitations of this system as applicable only to the natural sciences. Other scientists, when they endorsed the system, did so *conditionally* and re-oriented their notions of the assessment system and good science to fit. Frequently, researchers in this camp, mostly group leaders, framed their conditional endorsement in terms of the traditional ethos of science evoking individual achievement, independence and striving to be the best in global science. For them, rejecting assessment systems is a sign of a person doing poor science unable to produce excellent research publishable in international IF publications. In their account, doing great science has always meant international recognition and individual achievement; the only thing that has changed in their eyes is how this is established. They do not recognize the research assessment as an instrument introducing a totally new culture and instruments of control. Instead, they regard IF-based system of assessment as a novel method to establish what has always been important in science—what it means to do outstanding science. For them, this is not a paradigmatic shift forcing them to reconsider their underlying values of being researchers.

However, they do not regard the assessment system as entirely unproblematic; they see disciplinary and specialization differences, "*there are different habits of citations in different fields, and connected with it are the impact factors of journals. We think it's very difficult to compare so very different fields*", said one lab leader. The decisions where to publish create for lab leaders and independent scientists tensions as they are often faced, for example, with a decision to publish in a "*biochemical journal where more people will read it for whom it will be relevant but we publish in a chemistry journal because it gives us more points.*" They concede there have been attempts at the institute to develop a more fine-grained assessment system which would take specialization differences into account but these attempts exploded as the system became more and more complicated while not fine-grained enough to capture all the nuances that people felt were important.

Although the assessment system has these flaws and limits, they do not reject it as it can be counted on to always identify the top and the bottom: "*It's true that good people are*

in first places in each evaluation", said one lab leader and another continued: "*When you make these performances, the last twenty percent are always the same, whatever parameter you take.*" In the eyes of these group leaders, then, the system does what it should. They are concerned about underperformance and not spending money on poor research. If the bottom twenty percent is always the same, then in their eyes the assessment system provides a reliable basis on which to make evaluative judgements. Here disciplinary differences fade away; the ability of the system to establish who the best is can thus also be read as the ability of the group leaders to navigate the disciplinary differences in impact factors. In terms of funding distribution, the group leaders do not think that there should be one-to-one translation of assessment results into funding, either, again primarily because of disciplinary differences in terms of machines, labs and personnel, but they are in favour of "*something in this direction*". In their opinion, funding distribution should not be completely untested; rather, the assessment system and impact factors establish monetary worth, albeit partial, attesting to their proclivity towards a competitive system.

In these accounts, we can see the appeal of differentiating, establishing the worth of people. But there is also partiality, conditionality and provisionality which temper their endorsement of the system. These researchers perform well in the assessment system; they had succeeded in the transformation and continue to perform highly. They have a stake in the system because it is a system that enacts their success. It allows them to thrive, establishes their position and gives them power. They are emotionally invested in it and care about their rank in it.

> "*I enter the institute and there's almost no one around in the back office. I go to the kitchen and see the group leader with a bottle of Champaign. "Whose birthday is it", I ask. "No one's", says he, "but we're the first in the research assessment, they just published the table."* (field notes)

We can see in these statements of conditional endorsement, a sort of mutual domestication of the assessment system and of the traditional meanings of outstanding science. For them peer recognition relates to citations relates to impact factor, and so in their eyes there is an alignment along a linear line. The group leaders identify excellence with an IF and were critical, for example, of a test ran by the director when he modelled the assessment where the impact factor was divided by a median of the field. In this eyes, this resulted in levelling of the quality of journals because it

diminished the differences "*between very excellent paper-journals and average journals*" (focus group with lab leaders). This strategic alignment allows them to maintain their vocational research subjectivity while domesticating the entrepreneurial within it. This domestication is vital for achieving coherence of their research self, and is crucial for further promulgation of the system. Their ability to make the vocational and entrepreneurial cohere fosters, at the institutional level, the assessment system as a legitimate instrument of managing science. At the outset of this chapter I mentioned that resistance in science is considered by many difficult because the underlying values of the scientific profession include individualization, competitiveness and ambition. The conditional entrepreneurial subjectivities that group leaders in my sample in particular develop speak to these values precisely. In the next section, I will tell a story of those researchers who invoke the value of doing good science and self-discipline in the vocational mode, and the resulting oppositional, conflicted subjectivities they develop in face of the demands of the entrepreneurial mode of organizing.

The story of refusal: Conflict

This story of refusal addresses unacceptability of and the unwillingness to submit to the new governmentality regime based on the IF-based system of assessment. The critics, mostly independent scientists, do not talk about the partial shortcomings of the system. Their criticisms are levelled against the entire assessment system which creates non-coherence for them primarily in two respects. One is non-coherence between research quality and the assessment system; the second is non-coherence between the vocational and entrepreneurial collegiality/competition culture.

Research quality is a highly contested terrain for these researchers. They do not agree that the impact factor can capture quality. They talk about "*collecting points*" and the resulting "*dictate of the average*". One of the researchers calls those who are responsible for the assessment system "*hurray-revolutionaries*". They are critical of the changing culture which results in people discussing the *number* rather than the *content* of their papers. Their notions of quality centre on peer recognition within a group of specialists in their particular research area. Such a group of experts can be, in their accounts, very small, and the important thing is to get the attention of these specialists who will then cite their work and with whom they will exchange in conferences and

workshops. For these scientists, the implementation of the assessment system introduces foreign concerns to the profession, and reorients their work in ways they do not condone. The annual performance assessment of individual researchers is perceived to have turned an instrument of knowledge dissemination (papers) into a goal unto itself: *"…it's a tragedy. This is not what doing science is about; it does not contribute at all to a healthy intellectual environment"*. In interviews with these researchers, there are tones of anger, frustration and sadness as they discuss why the assessment system is flawed. Yet assessment figures strongly in their accounts of research quality and assessment, as an inescapable spectre they must come to terms with. These scientists' subjectivities remain powerfully anchored in the vocational mode, and the values of the entrepreneurial mode create conflict in their research self.

Another type of non-coherence the assessment system is creating for these researchers concerns sociality. The change in atmosphere is palpable, and researchers talk about signs of "*Darwinism*" at the institute. The system has re-oriented the research environment toward increased competitiveness, and they discuss reorientation of the collegial culture at the institute. *"I think we have crossed a boundary to the detriment of collegiality and ability to cooperate. A person considers with whom to cooperate in order not to end up poorly in the competition."* The research assessment has created new boundaries which are now given by the walls of the groups; heightened competitiveness is discussed in the spatial terms of "*the safety of our lab*" versus "*the corridor*". Free exchange with colleagues, which these researchers value, has come to be restricted to the safety of the lab (cf. Vallas and Kleinman 2008: 297). The corridor which figures as a place of former discussions of research problems is now off limits, signifying a danger zone where results and ideas can get "*stolen*".

With a high degree of discomfort and embarrassment they discuss the need to make constantly sure their work is recognized. Types of collaboration they previously engaged in without much concern, raise new demands: if before they may have done a calculation for a colleague and did not give it much thought, now they feel they need to make sure their name is on the paper. But this creates additional stress for these researchers because on the one hand they do not agree with the system of collecting points, on the other hand they are aware of its importance and feel they must protect their interests and those of their group. The subjective dissonance of having to ask a colleague to put their name on a paper is for them unpleasant and stressful. These

subjective negotiations are draining researchers' energies and they claim that "*joy is disappearing*".

In some cases they lament the disappearance of some sort of 'Golden Age': *"Under the spectre of formalised success, the real and deep values are disappearing"*. While we should remain alert to what those "real and deep values" may have been or whether, indeed, they ever existed in the first place, they point to a perceived shift in the organisation of research towards increased stress on research performance and competitiveness which are "mobilized as a symbolic resource in the construction of boundaries and identity positions and to locate themselves within 'the present'" (Archer 2008: 271). "The reference point of the story of nostalgia is not the past but the present," claims Ylijoki (2005: 561) The statements above, then, cannot be taken at their face value and do not speak to the quality of the research organization before and today. Rather, academic nostalgia functions as a mediator between the past and the present in the "process of coping with changing environmental conditions" (ibid.: 571).

These criticisms, often expressed in strong language, come primarily from people in the position of independent scientist. At the time I was engaging with these researchers, they were structurally placed to perform worse than lab leaders, the transformation was making their positions structurally obsolete and claims to excellence are harder to make (see chapter 3). They were predominantly located in the vocational mode of the bench work, the slow experimental time with a high degree of experimental failure. These were people who for various reasons did not aspire to group leadership positions (I will discuss one of these reasons in greater detail in section five of this chapter) and did not manifest a strongly competitive subjectivity. The transformation pushed to the background the vocational mode of organising research which focuses on the everyday mundane research tasks where "*small success*" is important and intelligible. The new governmentality regime had introduced a high degree of insecurity related to their position and their professional subjectivities. These researchers were hard pressed to find a way to balance their values and the new ones brought along with the assessment system. It is this inability to find a meaningful way to make the two cohere that created conflict, giving rise to discordant research subjectivities. Their critical stance and refusal of this sort of assessment does not, however, mean that they are not re-orienting and adapting their practices, as I will discuss later in this chapter.

The story of time and sources of accountability: Separation

The third way scientists at the institute, both group leaders and independent scientists, maintain coherence between the vocational and entrepreneurial modes is through separation. One type of separation occurs in temporal terms. This is an independent scientist talking about "*the formalized part of success"* being "*a bother at the end, you have to fill in and calculate some impact factor"*. On the contrary, "*managing to write a good paper which gets published, that's a good feeling and it has many tiny stages.*" Here we see the work of separation between the everyday bench work and the slow process of writing "*with a nice half day when things are going well and work flows*" and the end when the impact factor resulting from the bench work is calculated. In this way it is possible for many of the researchers to distribute the vocational self which makes claims on one's perseverance in terms of the ability to tolerate experimental failure and where success is counted in "*tiny stages…something very small*", and the entrepreneurial self of calculating the impact factor and recognizing the importance of this sort of assessment in contemporary science. Let us also recall the claim that "*as for science, nothing dramatically changes*", which I quoted in the previous chapter. Through such separation these scientists are able to carry on as if their research work has not been affected by concerns which they consider to be foreign, a bother introduced externally. I will return to this issue of separation in the next section as we shall see that these temporal boundaries are not clear cut and that "at the end" may in fact be "at the beginning". Yet this self-positioning work allows researchers to maintain a coherent self of a responsible scientist doing good science.

The second type of separation involves the sources in which accountability is located. I return here to the quote from the first chapter where a lab leader discussed the impact factor as "*the obligation to tax payers*", something "*for our director, for the grant agency, that's all right*". Here we can see the IF-based assessment system enacted as an external type of accountability, as a specific type of quality separated from everyday research and from the work of the team; the assessment system is something that does not concern the self of the scientist; it is a form of accounting for one's work vis-à-vis external actors. In this work of separation, the director becomes a proxy for the institutional assessment, the institutional embodiment of the assessment system. The second external actor is the grant agency for which the curriculum vitae of senior applicants with a list of publications are an important feature of project proposal

assessment. The third are tax payers who are reduced in the entrepreneurial mode to a recipient of a very specific type of accountability. The impact factor stands here as shorthand for the economic and social wellbeing of society evoked in the research, development and innovation policy, the product that basic science is thought to have to deliver to society.

Scientists and group leaders at the institute displayed an extremely limited notion of engagements between science and society. Here is the end of an exchange on the involvement of public in scientific issues and interfaces between public and scientists:

> Group leader 1: "*Yes, I fully agree with what she said, that we go out and communicate whereas the other thing would be that they come in and tell us…*"
>
> Group leader 2: "*Yea, whether we should listen to people tell us what they want us to do...*"
>
> Group leader 3: "*...or would...*"
>
> Group leader 2: "*....listen is ok*" *[All laugh]*

There was an overall agreement at the institute from PhD students to group leaders that it is necessary to explain to people outside of science what it was they were doing. In the exchange above, we can see this expressed as "we go out and communicate" which is juxtaposed to "they come in to us" and tell us what to do. There is a closure of the discussion as one group leader finishes off with the dismissal that to "listen is ok". The assessment system and impact factor are an instrument of separation. They allow these scientists to keep society at bay, as an accountability mechanism that justifies not engaging with the public on matters of content. In this, we can also hear an echo of the complaint that discussions inside the institute do not revolve around the content of papers but their impact factor. The impact factor which stands in for quality for the purposes of numerous external assessments, including the Methodology, the evaluation at the Czech Science Foundation and others, and facilitates assessment at a distance, makes it also possible to maintain distance from society when it is suitable for the researchers as a mechanism of boundary work (Gieryn 1983). So on the one hand, scientists and group leaders displace the IF-based assessment from a notion of science in the vocational mode and keep it separate as a type of accountability in the mode of enterprising. On the other hand, they claim the IF-based system as a way to separate society from science. It allows them to build boundaries around their scientific work; it

is a mechanism which precludes engagement. If the IF is high enough, society and tax payers are barred from questions and further engagement. The IF-based assessment system can thus function as a potent and seemingly indisputable instrument of maintaining the special position of science and scientists. In this complex work of separation we can see why it may be so important, especially for group leaders who are often the public face of science in the media, to create a research subjectivity where research assessment will be domesticated so that it may be used in other contexts as an instrument of separating other, public, claims on scientists and science.

4.2 Epistemic dis/alignments: Strategic publication

In the previous section I have argued that natural scientists live today in science where they are keenly aware of research assessment and increasing competitiveness and that the values on which this system is based often creates dissonance and necessitates negotiation. In this section I will examine the ways in which natural scientists cope with research assessment in terms of their epistemic and publication practices. With the word alignment I want to capture the process whereby practices located in one mode of ordering are re-oriented, aligned in the entrepreneurial mode. I will argue that scientists engage in these epistemic alignments even as they are aware of their negative impact and that in some early-stage researchers this may create an oppositional subjectivity which leads them to opt out of the system.

One clear consequence of the introduction of an IF-based assessment system at the institute is that far more thought goes today into considerations of where to publish in order to maximize point gains.

> "*I have always sent my papers to my favourite journal which everyone in my field reads and which has an IF of 3, but with the coffee grinder, yes, I sent it to Physical Review, which has an IF of 5. Of course it was published there but now fewer people in my field will read it because it's a journal for the whole of physics but then we'll get more points for it.*"

Perhaps the most obvious impact of the research assessment is thinking about the publishing venue and its IF. It was interesting for me to find out that in this culture focused so clearly on impact factor publications, early stage researchers did not receive explicit information about the decisions their leaders make as to where to publish.

Group leaders and supervisors do not discuss these issues with them, and so early-stage researchers learn the importance of journals *ex post,* by witnessing where the leaders submit and get published. Given the importance of IF publication and the strategic considerations of journal impact factors in publication practices at the institute, it could be expected that this sort of 'learning the ropes' would be explicitly included in the training. Despite the muted presence of these concerns, early-stage researchers are nonetheless trained in an impact factor culture. In the quote that follows we can see a senior PhD student in his last year discussing the design of a new project in terms of his publication strategies. Here the issue of publishability and impact factors is not something that comes at the end, as we have seen in the story of separation above, but rather it is a concern present from the outset in how researchers think about research problems:

> "*Of course when I am starting a new project, I approach designing it so that it is publishable. When I choose what to do, I am considering how to write it up, how to write it well.*"

The awareness of having to publish aligns the epistemic process, and consideration for publication features as an integral part of the process of knowledge making. Another aspect of the entrepreneurial culture in publication strategies is being the first to stake out a research field and being able to claim a certain result, to have a calculation or measurement attributed, making "relations among scientists in the same or overlapping areas of specialization seem increasingly market-like, as competition for academic capital impedes informal intellectual exchange" (Vallas and Kleinman 2008: 300). Early-stage researchers voiced criticism of this practice; in their eyes, this leads to "*fast publication*" of partial results "*before a really inventive result is obtained*" (field notes). This concern for primacy is related to citations, as the original work has to be cited in subsequent publications.

> "*They decided to publish the work and let the community know 'we're doing that, don't do it' ((emphatically)). So they sent a very small shitty paper to a very little unknown journal to protect it, and then they sent the second one telling essentially the same only a bit more, to a better journal.*" (postdoctoral fellow)

Furthermore, researchers report other gaming techniques such as salami publishing of "*chopping papers up*". Researchers lament the process of acceleration and the need to publish very quickly, voicing regret that it is now impossible to write a substantive

paper which would have major scientific impact. Instead, they feel the pressure to publish the "*smallest publishable unit*". Some group leaders are critical of this practice thinking that some of their colleagues are going "*overboard*", but then they concede: "*Well, but he's doing it well, it is, I guess, the right attitude*". Another practice, also criticized by some, is shingling which entails publishing the same result with a different twist in differently focused papers. The way a particular experimental result is reported, it may be set in different contexts, looked at from different perspectives. An experimental result can be made interesting for physicists as well as for chemists, which allows reporting the same results differently framed. This practice was particularly criticized in the chemistry modelling groups where some researchers were taken up on running essentially the same type of model but in a different system, with a different chemical element. Given the number of chemical elements and compounds, and the increasing capacity of computer clusters (discussed in chapter 3) the possibilities seem endless here.

The combined effect of the research assessment, competitive funding and the dynamic organization constrains the epistemic process in terms of opening new research questions and developing new methodologies:

> "*The way science is evaluated today, to publish fast, it doesn't pay to do something thoroughly. You can't have fifteen papers a year if you do it thoroughly, right. Some things don't get attention because you might find out that it was all right back then but you would loose two months before you send in the paper. The same here, it doesn't pay to start something brand new, bring a new method you will be developing here for two years because a student won't have a paper for two years. So you apply the same method to various systems.*" (doctoral student)

Opening a new research question is an issue. It is not problematic only in terms of one's own career in the entrepreneurial governmentality system where charting a new topic or method may mean inability to publish in good journals before a person establishes himself or herself in the community, which reportedly takes at least two, maybe three years. It has effect on other people, particularly students and postdocs. If a postdoctoral fellow comes for a year or two and wants to, as we saw in the group leader's quote in chapter 3, get as many publications as possible for his or her CV, then they cannot work on topics that will not yield publishable results. One leader described the process of dividing projects among students and postdoctoral fellows as giving them "*one that is*

certain, where publications will come, and one for the Nobel Prize but which may not work out". Competitive grant funding raises similar issues. Lab leaders jokingly talked about submitting "*safe*" or "*ready*" projects where they know the results are secured and will be published within the duration of the project (cf. Anderson 2008: 266). With overstatement they joked about submitting projects for research problems they have an answer for and have papers in the pipeline so that they could start working on a new topic where they do not have results yet, and sort of "*hide*" this new project in the one that has already proven to work.

These practices clearly show that researchers are strategically adapting their behaviour to the changing conditions even when they are critical of the impacts it is having on knowledge making processes. They are aware of the ambiguous nature of such strategizing and are trying to find ways to continue doing interesting research in conditions where a grant project, dissertation or a postdoctoral fellowship without publications is impossibility. They are aware of the problematic nature of some of these practices but especially group leaders see them as unavoidable and "*perhaps good*". We have seen in the section above that group leaders in particular are more prone to find ways to domesticate strategic publication in their makeup. Other scientists may feel more conflicted about these practices yet these concerns cannot be willed away. Here we can see another instantiation of being "interpolated" by the assessment system (Shore and Wright 1999) where scientists draw on a perception of their responsibility toward their students and fellows and the need to ensure they have publications, and this responsibility complicates their space for opposition and refusal.

Writing a paper is another practice which is undergoing this sort of entrepreneurial alignment. Writing has been a typical way for some time of sharing new knowledge in the natural sciences. In the vocational mode, it is a process through which scientists organise thoughts, contextualize their findings and place themselves on the map of a particular research problem. In the entrepreneurial mode where students need three first author publications in an IF journal to complete their doctorate and where papers continue to play a major role at every single step of the career that follows, the ability to write becomes a necessity of a different sort.

> *"I think it's terribly important to publish because a person finally makes an order in things when you write it up, formulate it and find all the literature to write the intro and place it within a context of*

> *others are doing. Without this you can't do it. But counting papers and ticking of items, that seems ridiculous to me. Two papers can be of a totally different quality. There are papers which, when I read them, I tell myself: that's awful, how could this have been published? And this may be a paper in a good journal. But then there are also papers where I think: I wish I could do this." (doctoral student, female)*

Early-stage researchers are not born with the ability to write, it is something they must learn, "*among the great ones there are those who know but who can't sit and write. But they have to learn*" (group leader, male). The doctorate is a phase where aspiring scientists must develop their competence to write a full paper. This is a gradual process where, first, they write up only the results. This is what constitutes first authorship, meaning that a person performed the experiment and has written up the methods and results section, while the boss is the asterisk author who writes the introduction and conclusions. Students and postdocs may give a paper a full try but "*few parts of the paper remain"* as one postdoctoral fellow phrased it, after the group leader takes over, because "*the way he looks at the problem may be different, and he can see some other aspect*".

Doctoral students and postdoctoral fellows are acutely aware that they must write, and experience insecurity if they feel writing is not coming easily to them.

> "*I do not have so much pleasure or entertainment from writing papers. This is one thing which I know is essential and important, and I cannot do it, I don't want to do it or I don't know how; because maybe it's not good to always be compared with other people ((nervous laughter)). But I know that it's really important and I don't have it. Maybe I can learn, maybe I can try. But I don't have the feeling ((slowly, uncertainly)) that I will be able to justify these things properly; maybe I can learn; maybe...*"

This is a quote from a postdoctoral fellow and shows a high degree of uncertainty related to her perception of her ability and willingness to learn to write papers. She is aware of its inevitability for her future career in science. Without papers one's existence in science stops making sense. We can see in the quote, too, an acute awareness of papers as an instrument of comparing people, of assessing people and her discomfort of being exposed to this constant assessment gaze. Writing here is lifted from the vocational mode of sharing new findings, organizing one's thoughts and "*acquiring knowledge and explaining things*", and is increasingly and strongly moored in the entrepreneurial mode as an instrument of competition. In this mode, scientists become

"*this machine for producing not shitty science because they are published in good journals, but there comes a point where I think you lose a bit of the perspective of what this job is about, at least for me*." Early-stage researchers realize that the ability to write papers is crucial and they locate it in the individualized responsibility for one's performance, it is about "*your personal ambition because if you publish less, you have less chances to be cited, to talk in conference and so your popularity in the community decreases.*" Being able to write and publish and to "*judge when it's publishable*" are crucial competences that early-stage researchers must acquire. This process involves, as we have seen, ambiguity, uncertainty and conflict. The quote above suggests that many doctoral students join the research ranks to "acquire knowledge and explain things" and through their training they learn the importance of IF papers. For some this creates tension and dissonance and their emotional investment in science is reduced. They develop distance as their values do not agree with the values of the entrepreneurial mode, of which the alignment is their first and most important example. In the following section, I will examine what I call professional alignment to discuss career imaginaries early-stage researchers develop in response to living and doing science in a dynamic lab.

4.3 Professional dis/alignments: Normalized precarity

Policy and institutional imaginaries of research careers in the biosciences today are fragmented, divided into a sequence of stages, and the progression to the next involves increasing competition for positions on a global scale. This competition, as we have seen, occurs primarily in terms of papers in IF journals, and the competence to write and publish is an integral part of one's assessment. In chapter 3 I have outlined the gradual shift from dynastic to dynamic organization of the research groups at the institute as one of the manifestations of the new governmentality regime, linked to changes in funding and research assessment. It may appear straightforward that early-stage researchers would learn to embody the dynamic logic of the organization. After all, they are constantly confronted at the institute not only with the rule that after PhD doctors leave but also a rotation of postdoctoral fellows. Here, then, I will examine how early-stage researchers relate to this fragmented career organization and whether, in fact, their plans and career enactments correspond to the dynamic organization of the research career. I

will examine a shift from the notion of science as a mission to science as a job, resulting from the increasing uncertainty especially in the early-career stages and the failed alignment between the dynamic career organization and early-stage scientists' professional imaginaries.

"*It appears to me as if everyone had one foot out the door, that people stay seems to be very often a result of coincidences*" (field notes, 14. 6. 2007). I felt the omnipresence of uncertainty at the doctoral and postdoctoral level very strongly during my research. "*I definitely don't know where I will be in 5 years' time; I know maybe in 3 years that I will be on a postdoc*", said a foreign doctoral student and his answer is not exceptional. The most frequent answer to a question about future plans of early-career researchers was *I don't know*. A large portion of early-stage researchers in the doctoral and postdoctoral phase wish to stay in academic science but they do not have any certainty as to how their situation will develop. A man from Cyprus in Prague on his third postdoc said: "*I hope that I will not go into the fourth*", and when asked whether his biggest current challenge is to find a permanent position he added: "*Not actually permanent. If it's permanent, it would be nice… More stable and in the same field… I wouldn't like to change completely the topic again.*" (postdoctoral fellow). This quote attests to the degree to which precarity is normalized.[44] The primary goal is to stay in academic science, if possible not as a postdoctoral fellow, and especially not to change one's specialization, again. A permanent position, a tenured position, is regarded as exceptional. Early-stage researchers discuss the possibility of exit forced by external circumstances (not finding a postdoctoral position or being unable to bring a family along on a postdoctoral position) rather than free choice. They would prefer to stay in academic science but the possibility that they may have to leave is not depicted as a loss or life tragedy; rather, it is an omnipresent alternative. How early stage researcher

[44] We lack statistical information on precarity on the academic labour market in the Czech Republic; research, development and innovation policies contain the objective, as I will discuss in the next chapter, of increasing volume of personnel costs funded through competitive funding and an increase in short-term contracts (Vláda České republiky 2008, 2009b). Countries that perform this sort of analysis confirm a tendency toward precarization. The SET for Success Report (Roberts 2002) prepared for the UK government states that contract research staff forms as many as 28% of all fulltime research staff (55% in the biosciences and 54% in physics (ibid.: 145). Less than 20% of researchers in these precarious positions ever find a permanent academic position (ibid: 12; Knights and Richards 2003: 217). Other British studies underscore that women find themselves in these positions more often than men (Garforth and Kerr 2009) and that the growing precarity and related competitiveness lead early-stage women researchers to the conclusion that "the short-term contract aspect of post-docing could not be reconciled with other aspects of their life, particularly relationships and family" and that "the competition for a permanent academic post was too fierce for them to compete successfully" (Lober Newsome 2008s:7).

discuss their career plans and future in science indicates that they have internalized job insecurity as a normal state, which alters their enactment of the position of a scientist not as a mission but as work, perhaps "*special work*" but work nonetheless. "*I don't sacrifice my leisure time to chemistry.*" (doctoral student) Another contrasts her research career and singing, saying "*before I started studying chemistry, I sang, and I want to sing even when I may not be working at the academy*." This reorientation in how they view academic science and their place in it suggests a self-defence strategy to cope with constant uncertainty and job insecurity which the fragmented academic career creates. Their engagement in science is contrasted with that of lab leaders who are seen as "*having to sacrifice a lot of free time, or more precisely their whole life. And that's the difference between me and them.*" Early-stage researchers are today forced to navigate the extreme demands of a successful research career, which they see embodied in the group leaders, and the constant possibility of getting stuck in a postdoctoral position or falling through the rungs at the next step. If previously sacrificing one's life may have held a promise of a stable and even successful scientific career, today sacrificing one's life at the postdoctoral phase holds no such promise. One process of re-alignment we are seeing is thus from the notion of science as a life-fulfilling mission which requires a whole person to one where science is a job to which increasing number of early-stage researchers are unwilling to make sacrifices. This unwillingness to make sacrifices in view of the uncertainties involved is compounded by another preference the early-stage scientists in my sample manifested and that was a preference for a lateral scientific career and rejection of the upward-looking career trajectory, with the apex in the leadership position.

In my sample there was only a small group of three men who had a clear idea of their future independence, autonomy and leadership. The goal for them was to achieve an independent position, establish their own team and make decisions about what they wanted to study. This leadership position is in their accounts crucially dependent upon finding a topic. If they do not find "their" topic, they cannot establish their own team and in such a case there is no sense in them remaining in academic science. For them, working "*under someone*" as one of them termed it, makes no sense:

> *"If a person doesn't have his topic, isn't independent, his own master, you can't do science. There are a few people here who are after a doctorate, after a postdoc, and still they have a boss under which they work, and that's bad.*" (doctoral student)

In their accounts we can see negotiation of the vocational mode (finding a topic and becoming an autonomous researcher) and the entrepreneurial mode (establishing an independent research position and winning funding for it). A meaningful future in science is contingent for these early-stage researchers on their ability to find coherence between these two modes of ordering.

Their endorsement of the dynamic organization and desire for independence does not at this stage, however, entail reaching senior leadership posts:

> "*As a person gets older, you turn into a prof. You do politics and assume professorial airs. I won't do that. The second phase is unpleasant but the first one isn't bad. Have some three people and try to do interesting science. Have a sort of pleasant feeling. But then, later, the paper work, that's plague.*" (doctoral student)

In this quote we see an endorsement of an equivalent of a junior group which this person has seen instituted at his institute, and a refusal of the administrative and managerial subjectivities he sees as inevitable in the later stages. It is interesting to see in this quote the implicit assumption that junior groups do not involve "paper work" and "politicking" as this doctoral student called it. In contrast, senior leadership is regarded as enacted within the mode of administration and enterprising, involving greater paperwork, negotiations and compromises. It is of course entirely possible that these assumptions related to junior and senior groups will change in time as these men progress up the career ladder. It is nevertheless interesting to see work of negotiation underway related to the preservation of a notion of science in the next career stage which is meaningful to these researchers. Furthermore, these young men are not oblivious to the complexity of finding one's own topic. This condition on the one hand falls within the vocational mode, with its stress on epistemic practices and everyday research. On the other hand, these researchers are aware that finding a topic is not an entirely free intellectual task: "*There is really no freedom to study what you want. If you have a topic that's not in, you don't have publications, money…*" In this quote we can see the mode of enterprising with competition for funding where one's topic must be in to be recognized for funding and in to be publishable.

Given the dominance of the dynamic organising at the institute, it has been surprising for me to identify a large group of both women and men early-stage researchers who disagreed with the notion of an upward career with the leadership position as the high

point of one's professional growth, and displayed preference for working independently in someone else's group. These early-stage researchers did not invoke "being one's master"; for them independence was related to being able to work independently in cognitive terms, being able to control a research project—a partial line of research that contributes into a larger research question which a group works on.

> "*The ideal image is a year or two on a postdoc abroad in a good lab and then return, get your own project, work on it and have good conditions. The best prospect is that I am with someone in a group and choose a project there. Having a group is a nice idea but on the other hand it's big responsibility, a lot of paperwork so I don't really see any advantages in having a group. But on the other hand I guess it's part of the notion of a career.*" (doctoral student)

Early-stage scientists perceive leadership positions as more managerial than scientific, as leading one from "*doing everyday science*", from research "*in the lab*", which is attractive for them. They juxtapose "doing science" and "doing career".

> "*A career is about hunting titles. You'll be famous, you'll travel around, lecture. That's the peak when you only function as a manager of your group and do paperwork, write grants, write grant reports, write papers. You sit on your butt at the computer and write. That's the peak of the career. But for me subjectively this is not the ideal; I want to stay in the lab and work with my own hands.*" (doctoral student)

There is recognition, though, that while the position of "*a research fellow, as they call it in America*" would be the best option for them, "*it's almost impossible.*" Related to this is the general issue of "*returnability from a postdoc*". The early-stage researchers voice a strong concern that in conditions where they have to leave an institution after their doctorate seriously compromises their future ability to find academic position in the Czech Republic.

These early-stage researchers are growing today into a system where internal assessment and stress on IF publications is a given. They do not dispute the need for an assessment to be in place; they recognize it has positive effects on people, including themselves, they are "*more efficient*", there is "*positive motivation*". In Chapter 3 we have also seen that these early-stage researchers had high hopes for the transformation process to "*air out the dark spots*". On the other hand, they are critical of strategic publication the assessment culture leads to, and they are also aware that the line

between the assessment system acting as positive motivation and creating highly stressful environment is thin:

> "*The atmosphere here, everyone can feel it, the assessment. I think it's a sensitive issue which demands balance... It would be enough if the boss pushed and stomped every morning how come something is not ready and that it must be published even if we do not know what it means. And that's a problem. Or if he said, look at him how much he has and you don't.*"

Many early-stage researchers were critical of the entire academic environment because of its competitiveness and hyper-competition. The constant necessity to compete (for grants, better position in team assessment in the internal assessment of publication activity etc.) was perceived as something that takes attention away from what should be a scientist's primary concern, doing experimental science. The preference they expressed for everyday bench work was framed by their rejection of the competitive environment. They are upset by the system as a whole—calculating impact factor, everyday visits to the *Web of Science* to see whether there is a new citation. Although they do not agree with them, these early-stage researchers internalize these rules and values, they compare themselves against them. If they do not fit, they develop a feeling that they should leave, that "*science won't be any worse for it*".

The competitive working culture was not regarded positively by a sizeable portion of early-stage researchers. I have already noted in the previous chapter that the position of independent scientists has become problematic in the new arrangement of the dynamic lab and institution, and early-stage researchers are aware of this. A postdoctoral fellow declared: "*Postdocs are no career. If you don't want to be a group leader a question is what other career you can have in science other than to decide to leave.*" The ambition to do science and not to do career, voiced by a large portion of doctoral students and postdoctoral fellows in my sample, thus raises important questions about organization of contemporary science, the efficiency of hyper-educating and training increasing numbers of people up to the stage of postdoctoral fellow without a system in place that can accommodate them in the long run and without a value system that can anchor their research subjectivity. This also raises the ethical question of exploitation as the demands of the dynamic system placed on postdoctoral fellows are extreme in terms of long hours, publication record, international mobility as well as exposures to work stress and stress related to the constant presence of assessment and comparison with others, as we

have just seen. Shapin (2010) persuasively argues that academic science may be undergoing changes, but the moral claims on individual performance of science as a mission continue to hold their position as the ideal against which early-stage researchers are measured. This ideal has been, however, re-aligned in the changing conditions, giving rise to a notion which on the one hand mobilizes the traditional devotion and sense of mission, and on the other hand introduces high levels of competition and precarity in the early career stages. We have, however, seen that because of the uncertain conditions of the dynamic system, early-stage researchers are developing subjectivities that allow (or perhaps force) them to disconnect from the traditional ideal of self-dedication and sacrifice and to develop a pragmatic professional self where the scientific profession is just a job.

4.4 Caring dis/alignments

"Each one of us is an arrangement", says Law (1994: 33), and this arrangement is not individualized; we always start out as belonging to "collectives already – and not just a single one, but a lot of them." (Mol 2008: 12-13). In this section I look into the collectives to which scientists belong; I examine two stories of belonging in terms of relations of care. I have noted above that care and responsibility may be a reason for re-alignment of epistemic practices in some researchers as they attempt to navigate the competitive system in such a way that their students and postdocs are well placed to progress. Here I will argue that relations of care create conflict for some researchers which result in their disattachment, dis-alignment from the dynamic organizational logic. The first story concerns application for a research group; the second is a story of family care work. In telling these stories I also want to examine the gendered constraints that the assessment-based, dynamic culture entails for individual scientists. I specifically use the word dis-alignment to underscore the process of distancing, agency and contrariness, which I lack in the term misalignment. Furthermore, misalignment, in its meaning of "placed or positioned wrongly or badly" suggest normativity and hierarchy, a place where a proper alignment occurs and against which wrong place is misaligned.

The story of applying for a group

There are many problems with group leadership, as I have just documented. Some researchers are concerned about the high degree of administration, politicking, sitting at a computer, and writing and not working with one's hands, not doing bench work. Then there is a problem of responsibility. When groups are organized in the entrepreneurial mode of a dynamic institution, there is a high degree of uncertainty involved. Teams are competed for, assessed annually in the internal assessment and periodically following the transformation. Assessment is at the forefront of the research group, not something that the group leader can forget about. It is a constant which orients epistemic and publication strategies as well as the sociality at the institute, as I discussed in the previous sections. At the time I was doing my field work there were only two senior women group leaders among 13. I raised this issue repeatedly given the concern with gender aspects in the project and my own concerns for gender equality. It appears that these two were actually the only two women who competed for senior groups. Discussions with other group leaders and independent scientists revealed that they tried to persuade other women to apply but these refused. In the logic of choice, which I introduced in the previous chapter, the blame is easy to attribute. If women do not want to apply, it is their fault that there are not more of them in leadership positions. (And just how frequently is this argument made in the Czech Republic!). Here I want to tell a different story.

In the group interview with women independent scientists the issue of competition, success and research groups was important. They discussed many of the issues I mentioned at the opening of this section. But they mentioned other ones, too. Concerns about pressure on performance and the awareness of future assessment were prominent. These women were highly concerned and they were afraid. They were not afraid *of* the competition, however. Rather, they were afraid *for* the team. They mentioned concerns about securing grant funding to keep the team together. They were worried that if they did not have enough money, they would have to fire people (cf. Vallas and Kleinman 2008: 293). They were especially concerned about the spectre looming on the horizon of the teams' reassessment in five years. What if they did not succeed? What would happen to all the people? In the logic of choice these concerns do not make sense: for their immediate professional development it is necessary to compete for the group, win the group, have the group leadership on their CV, then work and get publications, win

more grants and if they don't succeed, the dynamic logic of the natural sciences tells us, they can compete somewhere else, in another country, on another continent. In the logic of choice they should not worry about their team and colleagues. Their concerns, however, make sense in the logic of care. The decision to apply for a team has consequences for other people and their collectivities (here I am concerned with the professional but those outside science are important too), other concerns than just one's own professional development, rationally considered, irrespective of others.

In the dynastic lab career progression is limited and dynasties are rarely passed to women. In the dynamic lab opportunities may open for more positions though still very limited in numbers. But they carry new values, new concerns. The temporality and uncertainty create stress, bring concerns related to responsibility for the team, for one's colleagues that did not exist, or did not exist to this extent, in the dynastic organization. These concerns are, however, difficult to articulate in group leadership positions. Here, private, non-working life becomes less visible, something that is less relevant in terms of the organization of research work and the groups' performance. Group leadership positions therefore do not require only outstanding science and impact factor publications which establish excellence but also the ability to disconnect oneself from concerns about the future of the people in the lab. In a sense the dynamic lab is perfectly suited for this; after all, if the life of a doctoral student is three, maximum four years and postdoctoral fellows come for a year or two, then the group leader does not really need to worry. Maybe the institution will be fully dynamic in the future; as it is and was at the time of my research, there were still people in the independent scientific positions. The women scientists who did not want to apply for group leadership were in fact all in these independent scientific positions. So the concerns they were expressing about the future of teams were also concerns about their own future.

The already mentioned question posed by Louise Archer (2008) whether science will be attracting new types of workers or whether researchers will re-align their subjectivities is thus highly pertinent, not least in terms of the future of the logic of care in science. My results suggest that senior researchers and particularly group leaders who are already in the system are aligning their practices and subjectivities in the mode of enterprising, at least to a degree. They may be critical of the assessment and the culture it creates but they are enacting it through strategic action in terms of publication and research cooperation. In the early career stages, my findings show, there are very few

(and in my case male) researchers who endorse the upward scientific career peaking with group leadership. Considering the dual factor of there being far fewer women in the pipeline in the natural sciences and there being very few senior positions available, the gender imbalance in group leadership positions is likely to be reproduced. Importantly, early-stage male researchers also expressed these concerns about responsibility for the group. But in the given gender order and with the gender imbalance in the pipeline, it is likely that the people who will progress to the leadership positions will tend to be men willing to operate in line with the logic of the competitive dynamic lab.

The story of impact factors

The traditional notion of the subject of the labour market is a person immune to care work, with unrestricted temporal opportunities and narrowly focused on work performance and success (Runté and Mills 2004). "In organizational logic, both jobs and hierarchies are abstract categories that have no occupants, no human bodies, no gender." (Acker 1990: 149) This abstract employee position is filled with a disembodied worker who exists for nothing but work, and the closest to such an abstraction is a man who has a wife or a partner who takes care of his personal needs (and those of their children and home), and thus his work performance is not affected by 'external' factors (Acker 1990, 2008; Runté and Mills 2004; Smithson and Stokoe 2005). We have seen that such a notion continues to orient the functioning of organizations, inscribed in expectations of group leaders whose family arrangement often corresponded to the one just outlined and in the next chapter we will see it is also written into policies. The family, however, is not a collective that can be written out or one which scientists actually want to write out.

> "*Such a child has a VERY high impact factor …and citation index … So I, I slowed down big time, a year and a week ago.*" (postdoctoral fellow)

Here is a postdoctoral fellow at the institute, applying the language of one collective, the scientific, to another collective, his new family and a child. The priorities of the scientific, getting impact factor publications and citations, have to be negotiated with priorities of the familial, and this creates conflict, concessions must be made, there is

slowing down. Choosing a reference to two measures of performance in science, this postdoctoral fellow refers to the energy and performance necessary for parenthood, which the masculine ideal of the labour market makes otherwise invisible.

It is not surprising that in the gender order in place, with its morals and value judgements, family concerns were present in a majority of interviews with women in early career stages. Child free and single women mentioned the expected influence of a family (children and partner) on their professional career. Similarly to research carried out by Marks and Houston (2002: 334), women make "plans, not just about their careers but also about their anticipated caring roles as mothers". Early stage women researchers feel that they should or will have to reduce or even give up their work tempo and energy because of childcare. Family and children are inseparable from their consideration of their scientific futures.

> [You mentioned once that you think that when children come it will be worse. Maybe you could tell me what you think will be worse?] "*I think that when the children are small, I will reduce science a lot. I want to dedicate myself to children as much as I can. I definitely don't want to be the type who gives a six-month old baby to a babysitter. It's quite important for me.*" (doctoral student)

On the contrary, single, child-free men in early career stages did not bring up concerns about family in their future plans on their own. When I prompted them, they usually raised concerns about their ability to provide for a future family financially, and worried that science is not the profession that will make this possible. These differences are indeed not surprising given the gender order and the disciplination of women and men as regards the roles they are expected to play in a family.

Foreign postdoctoral fellows, particularly women, expressed surprise and concern how often they were called to account for not having a family and being on a postdoctoral fellowship in the Czech Republic. They felt that back home this pressure would not be as strong: "*but this is a really different picture from the Czech Republic, because I'm thirty, not forty and I do not have to have children*" (postdoctoral fellow from Poland). She, similarly to a postdoctoral fellow from Spain, felt that she had some ten years before she needed to think about a family. Her immediate plans involved going on another postdoctoral position to Israel and then hopefully the UK. She and her husband, also a scientist, made a decision not to try for the USA because of the distance but also

because they did not want to work fourteen hour days but only ten hours a day, which she felt they could in Europe.

Partnered early-stage researchers, with or without a family, both women and men, discussed various types of limitations on their research careers. Both men and women include their partners in their mobility plans. Planning a postdoctoral fellowship abroad involves manageable distances and there being direct flights to Prague. The USA is often ruled out because of the distance, "*America, I can't imagine that, I can't imagine going for two years, and this is a VERY BIG problem*", said a postdoctoral fellow.

> "*I will be looking for a postdoc now and I have to consider all the various relationships because my boyfriend wants a job here and so offers from the US are off for me because it would be unsolvable.*" (doctoral student)

In these accounts we can see that some men in early career stages integrate the family collectivity in their career plans, which result in similar constraints and limitations in terms of their mobility reach and publication activity as in the case of women. In her recent study of academic mobility of dual career academic couples, Vohlídalová (2014) analyses the gendered differentials in mobility of Czech academic couples. One of the findings of her study is the impact of the gender ideology of the couple on mobility and more generally professional development of the partners. In couples which show tendency toward more gender egalitarian partnership we can see a more distributed impact of the slowing down and having fewer results. In the competitive organization of the dynamic lab, this may result in researchers with family obligations and constraints (and these do not have to involve only children but also other caring relations) not being able to compete with other scientists who are without these constraints, those who will be able to free themselves and transfer those obligations onto another person. The space these early-career researchers create in their professional subjectivities for caring relations in the family is linked to a process of dis-alignment from the competitive subjectivity, especially in the postdoctoral phase.

In the logic of choice, again, such decisions are manifestations of individual preference and responsibility for autonomous choices he or she makes. Yet for the early-career researchers in my sample, the process of caring dis-alignment is not an easy either-or choice. They do not want to give up either their research career or their caring responsibilities. Neither do they wish to slack in either domain. Rather, they are

claiming time and space where both are possible, where a balance between work and family may shift but both are deemed important.

The competitive, entrepreneurial system of the dynamic organization does not recognize such caring concerns. Postdoctoral fellows discussed with me experience with applying for postdoctoral positions where any sort of a break resulted in the CV being "*tossed in the waste basket*". The gaps on the CV obviously do not entail only family caring; in fact, in this particular case the gap was a result of a time-off for an extended trip to India. In the competitive dynamic organization such gaps may be interpreted as a lack of dedication (let us recall Shapin's claim about the moral demands on individual scientists continuing unabated in the changing conditions). This gives rise, again, to the question of who will be able to progress up the career ladder. Because of the strong stress in the feminine gender on a particular performance of the maternal role in the Czech Republic, Czech women scientists are facing specific constraints. However, in this section we have also seen a number of male researchers in the early stages who are claiming their parenting and partnering role. The dynamic organization oriented on competition is therefore likely to reorient the old obstacles facing mothers-scientists and become an exclusionary mechanism for researchers whose caring work places limits on building a competitive CV, including a competitive mobility track.

4.5 Conclusions

In this chapter I have analysed the formation of scientists' subjectivities and practices and their alignment and dis-alignment in response to the introduction of research assessment and the related changes in the dynamic of research careers. I have argued that a small number of researchers at the institute, usually group leaders, manifest conditional endorsement of the assessment, aligning their definitions of good science with the IF-based assessment system through domestication. On the other hand, a sizeable group of scientists, mostly in the positions of independent scientists, experience intense conflict between their research subjectivity and the competitive one enacted in the entrepreneurial mode. The conflict and discomfort they experience is linked to the perceived inability of the assessment system to capture scientific quality, changes in sociality at the institute as a result of the competition and need to protect one's results. These differences are related to their hierarchical position, the types of tasks,

responsibilities and values their position carries and access to the individual modes of organizing.

The assessment system is threatening to thus-positioned scientists in two respects. Firstly, in the dynamic organization the position of the independent scientist is not institutionally anchored and there is no promise of a sustainable future. The assessment is indicative of entrepreneurial alignment of the dynamic organization which effectively obliterates their position in organizational terms. Secondly, the assessment enacts differences in performance between group leaders and independent scientists. In the previous chapter we have seen that despite different types of non-coherence resulting from different positions and epistemic practices, such non-coherence is often denied. Group leaders compete for and have control over resources and their distribution. In the dynamic system independent scientists become differently dependent on the group leader while they also become an expensive resource compared to postdoctoral fellows and especially doctoral students. In their criticism of the assessment system, independent scientists thus address various insecurities the assessment brings in terms of the future of their employment, work performance, vocational practices as well as self-understandings.

I have further argued that the conflict scientists experience does not manifest in explicit or overt forms of resistance. Despite the oppositional subjectivities they develop, researchers are responding to research assessment by strategic publication because "even when people oppose these systems of audit and inspection, they are nevertheless interpolated by them" (Shore and Wright 1999: 561). The assessment system aligns the process of knowledge making as such. With a heightened awareness of the abbreviated time of doctoral studies, postdoctoral fellowships, grants and assessment periods, it becomes vital to design research questions and problems in such a way that they yield publishable results within the given timeframe. The necessity of IF papers organizes the entire knowledge making process from the stage of developing a research design to the stage of publication. Strategic behaviour also affects the practice of writing as such. Writing papers features as a prerequisite of a career progress to be mastered during doctoral training. In a research assessment system based on IF papers, writing and the recognition of what is publishable becomes an essential competence. In the mode of enterprising writing becomes a specific practice where the stress does not rest so much

in sharing findings and organizing one's thoughts as it does in building a competitive curriculum vitae and high impact factor of publication activity.

Epistemic alignments are compounded by professional alignments, experienced as a result of the fragmentation and insecurity of early-stage researchers' future as scientists. My research has shown that only a small portion of early-stage researchers develop competitive subjectivities that fit the entrepreneurial mode of organizing, and these are not unconditional. They are contingent upon these researchers' ability to develop a strong research topic. Developing a strong topic bridges for these early-stage researchers the vocational and entrepreneurial mode where a robust research question meets their ability to get sustained funding for it and publish results of such research. In contrast, a large portion of early-stage researchers reject the competitive, upward looking career, and show preference for a more lateral scientific path, which corresponds to the disappearing position of independent scientists. Related to this is the stress they experience under the constant evaluative gaze of assessments and being constantly compared to others. The shift toward a competitive system, the related job insecurity and the stress of constant comparison and evaluation leads these early-stage researchers to recast research work as a job they may lose or from which they may opt out. The third type of alignment identified among scientists at the institute relates to caring. Certain practices of care are impossible to negotiate vis-à-vis the competitive system where a caring subjectivity stands in conflict with the individualized, competitive self on which the mode of enterprising builds.

Taken together, the epistemic, vocational and caring alignments and dis-alignments experienced in the entrepreneurial mode (Vallas and Kleinman 2008) are creating conflicted subjectivities and build powerful avenues of exit from the research profession. The introduction of research assessment and competition for posts and funding in the dynamic organization has aligned epistemic practices as well as research subjectivities. For the dynamic organization these alignments and dis-alignments appear to be a matter of no concern. There is only a small portion of group leaders and early-stage researchers who manifest alignment with the entrepreneurial system, even if it is conditional, and these scientists may then be better placed to progress in the system and maintain a subjectivity which will allow them to domesticate the demands of the competitive system in their research self. The rest of the scientists and early-stage researchers who do not find ways to align may exit or be forced to exit, falling through

the rungs of the fragmented dynamic career, making space for the future generations of doctoral students and postdoctoral fellows trained in the system. DiMaggio and Powell develop the term *anticipatory socialization* as one way to foster institutional isomorphism through the filtering of personnel who progress up to the extent that "individuals who make it to the top are virtually indistinguishable" (1983: 152-153). Similarly, Kanter (1977) referred to *masculine ethic* to underscore the elevation of traits associated with men and masculinity in organizations and particularly leadership positions, and used the term *homosexual reproduction* to describe the process of masculine reproduction in organizations.

I find these developments described in this chapter problematic if they should continue and be further reinforced. While the dynamic organization I explored in the previous chapter may have opened the academic space and contributed to disturbing nepotism and cronyism of the dynastic line, we have also seen that the types of performance, conduct and subjectivity that the system demands and promulgates are highly competitive. This chapter has shown dissonance with and criticism of the entrepreneurial alignment among some researchers, and its partial endorsement among a narrow group of others, those in leadership positions already or a few early stage researchers with an aspiration to leadership positions. The question is, then, what long-term impact this may have in terms of knowledge making. What sort of research questions and what sort of frames of reference will be used if the system lets through only or predominantly highly competitive individuals enacting the masculine ethos? I will return to these concerns in the concluding chapter.

5. Dynamic policymaking: Entrepreneurial alignment of the policy vision for Czech science

Some of the research into the changing governmentality in research and higher education starts from the presumption that "the non-consensual revolution from above" has been imposed upon research and higher education (Roberts 2004). In these accounts new accountability requirements are seen as having "emanated from state governments, with both governors and legislatures taking the initiative in establishing new policy", having "imposed new accountability requirements upon these institutions" (Dunn 2003: 69). The changes implemented are seen as external, and there appears to be separation between the policy level with one set of actors and institutions and individuals which respond to top down measures adopted by governments (Fleming and Sewell 2002; Prasad and Prasad 2000; Probert 2005). The policy level is depicted by researchers or their research respondents as an arena where orderings and their values are defined which then bear on the conduct of institutions and individuals (Deem and Brehony 2005). There appears to be a gap which an individual can hardly surmount.

Contrary to this, Tereza Stöckelová and I have argued that in the Czech Republic scientists, particularly from the natural sciences, were complicit in the introduction of a research assessment system in the Czech Republic when they initiated the introduction of an assessment system at the national level (Linkova and Stockelova 2012). Natural scientists were vocal in pushing for bibliometric-based evaluation, and it was only very gradually that problems related to simplistic metrics-based assessment systems started to be addressed in the academic public domain. Many of these natural scientists sat and continue to sit on various advisory, expert and policy bodies of the state administration, not least among them the Council for Research and Development, and thus have had direct influence on shaping Czech research, development and innovation policy. The entrepreneurial alignment of values, funding and assessment underlying science policymaking which I examine in this chapter, including national policies and other strategic documents, were thus adopted in full knowledge and with the endorsement of some scientists active in the policymaking process as well as representations of scientific institutions in the public consultation procedures, and attest to the fact that the

entrepreneurial alignment of science policy were not imposed but partially self-administered.

In this chapter, I build on an earlier paper published with Tereza Stöckelová (Linkova and Stockelova 2012)[45]. I will examine how science policy has been aligned in the entrepreneurial mode, with a specific focus on shifts in visions of the role of science in society, research funding and research assessment. Policymaking is the third apex of the dynamic triangle I introduced at the beginning in which the governmentality regime of Czech science has been overhauled over the past decade. I explore here shifting values underlying science policy which come with specific machineries (such as assessment and relatedly funding distribution). I continue to argue that these developments are inter-related, contingent upon one another, and that they inter-lock to create a powerful grid which orients, curbs and constrains research practices and subjectivities in particular ways. Contrary to others (Canaan 2010; Davies 2005; Deem and Brehony 2005; Shore and Wright 2004; Shore 2010; Smith 2010) I do not use the label neoliberal when I refer to these shifts in Czech policy. Although, as I will shortly argue, some of the changes we are seeing are definitely aligned with neoliberal developments abroad and are suggestive of a trend, the roles of various stakeholders, the minutiae of everyday functioning, the funding appropriations and the lives of individual scientists are not yet exposed in the Czech Republic to the same neoliberal practices and managerial and audit gaze as in some other countries and not to the same demands of academic economic activity.[46] This is not to deny emerging manifestations of these changes, though.

Policies are performative. They perform certain conceptualizations and discourses as acceptable, rational and seemingly the only ones possible. They bound imaginaries of how the world in a particular domain can and should be, "as if these were the only ones possible, while enforcing closure or silence on other ways of thinking or talking" (Shore and Wright 1997: 3). They are not merely textual creations but actors. As Matonoha argues (2009: 151), "[D]ocuments (in their discursive dimension) structure and affect reality – even above the framework of their functions and intentions – in a comparable manner as 'material' actors do." Policy documents are one of the arenas where struggles

[45] Section 4.2 is a revised version of a section of the paper. I use the text with the agreement of my co-author.
[46] While frustration and dark humour are certainly no stranger to our academic lands, we are not quite yet in the realm of assessment and administrative madness imagined by Ward (2014).

to control meanings and interpretations of concepts and words are fought. This includes evaluation systems, a "distinct cultural artefact" with "social consequences, locking up time, personnel and resources, as well as locking into the moralities of public management" (Strathern 2000: 2). By bounding the perimeter of their action, policies create not only their own symbolic and moral orders but also temporalities, socialities, spatialities and materialities. Policies have become a central machinery for the organization of contemporary societies (Shore and Wright 1997: 3), and merit attention because they do not only codify values and organizing principles of societies but they also contain "implicit (and sometimes explicit) models of society." (ibid: 6). Contrary to the frequent claims that they are instruments for promoting efficiency, policies are contested political spaces where some interests and visions of society are empowered and others are silenced. In Foucault's terms, policies are "political technologies" through which power conceals its own operation.

5.1 An evolving contract for Czech science

In Chapter 1 I have addressed the key role of the Organization for Economic Cooperation and Development in appropriating science policy and aligning it essentially with economic objectives, defining science policy as an instrument of economic development of societies. This economist vision of science and science policy was reinforced with the onset of neoliberal New Public Management since the 1980s in many countries of Western Europe and beyond. Although the Czech Republic did not join the OECD until 1995, the alignment between scientific activity and technological development and national economy was very strong in the entire post-war period until 1989. In 1962, for example, the government adopted resolution no. 147 on "increasing the role of science and technology in the development of production forces in the Czechoslovak Socialist Republic". The 1965 governmental decree on Planned Management of the National Economy identified "state tasks for science and technology" as the prime instrument to this planned management, with a view to "create economic pressure and material interest in full application of results of science and technology and support progressive tendencies in our economy". Twenty years later in 1986 in a discussion of the programme declaration of the government in the Czech National Council, the goal was articulated to "exert joint efforts for science to become a

truly immediate production force in our reproductive process and to prepare fully-qualified cadres for this work".

It is beyond the scope and goals of this dissertation to analyse pre-1989 policies governing scientific activity and technological development, but the gist is clear. Practically all the documents and parliamentary debates I have been able to collect in the parliamentary archive centre, regard science and the scientific base as a crucial instrument in "addressing key problems of our economy" (P2, L14). The link between theoretical research and "realization units" is articulated strongly. Various institutions had been established over the years to ensure an "intensification" of the interconnection between science and "praxis". There was the Ministry of Technology, which was replaced with the State Committee for the Development of Technology, the State Commission for the Development and Coordination of Science and Technology, the Committee for Technical and Investment Development and at the end the State Committee for Scientific-Technological and Investment Development.[47] The State Committee controlled the Office for Inventions and Discoveries, the Office for Normalization and Measurement and the Czechoslovak Commission for Atomic Energy.

There is interesting evidence that the system of planned management had to deal with methodological issues related to measuring performance of scientific workers, specifically measuring their work productivity for the fulfilment of annual plans. In 1964, according to a new methodology, the "scientific and developmental base workers" were excluded from the total number of workers in an organization because scientific and developmental work "always is and must be future-looking in nature, and seemingly does not contribute to increasing regular labour productivity" (P2, L12). Clearly, assessing an industry line worker and a scientific worker could not be done according to the same formula, the policy recognized. Against this backdrop, we might wonder whether the Methodology adopted in 2004 which I will shortly introduce in greater detail is not such a one-size-fits-all formula entailing the rationale of a production line. If factory workers can increase their output, why not scientists? In the 1964 debate of a bill of law we can see policy recognition that the "future-looking

[47] For an overview see http://cs.wikipedia.org/wiki/Seznam_ministr%C5%AF_techniky_%C4%8Ceskoslovenska.

orientation" ("perspektivní zaměření) requires a different set of target-setting instruments than an annual plan and its productivity objectives.

After 1989 transformations in higher education and academic research started almost immediately, resulting in the adoption of the 1992 Act on State Support for Scientific Activity and Development of Technologies (Provazník et al. 1998: 56). The act provided for the establishment of the Council of the Government of the Czech Republic for Scientific Activity and Development of Technologies (today the Council for Research, Development and Innovation) as an umbrella expert and advisory body of the government; established the Czech Science Foundation as a central body of state administration distributing competitive funding; and defined the conditions for funding science through institutional and competitive funding. The decade of the 1990s has been described by some individuals active at the policy level as a period of "rulelessness" without policy steering when science was left to "the rule of money" during the Klaus's administrations. To illustrate this rule of money, research staff fell from 137,927 in 1989 to 76,487 in 1991 with a low of 38,752 in 1994. R&D expenditures fell from CZK 21,420 mil. in 1989 to CZK 12,415 mil. in 1990, with a low of CZK 9,750 mil. in 1993 (Provazník et al. 1998: 65). The Academy of Sciences went from 13,896 R&D staff in 1989 to 7,127 in 1993 (ibid.: 69). Gross Expenditure on Research and Development (GERD) went from 4.08 % in 1989 to 1.06 % in 1993 (ibid.: 77), increasing slowly throughout the 2000s up to 1.88% in 2012.

It may not be far fetched to say that the 1989 political change, with its attendant shifts in research funding and privatisation of state property, wreaked havoc in industrial research. Companies were closed down or privatised, sources of funding vanished. New foreign owners closed down research branches in the companies they bought; the formerly free services industrial research provided to state-owned factories were either no longer required or were to be procured for pay but available resources were scare. The various shifts in the organisation and funding of industrial research resulted in an orientation toward short-term tasks, development and testing rather than research (Provazník et al. 1998: 192; also 177-82). Although a business enterprise research association (the Association of Research Organisations) was formed as early as 1990 in Brno as the "*first national association based on civic democratic principles in the area of science, technologies and innovative enterprising after November 1989*" (Asociace výzkumných organizací n.d.), the business enterprise sector did not gain much footing

in Czech science policy until the new millennium, a development I will come back to momentarily.

Policy documents and names of institutions created shortly after 1989 work with the collocation *scientific activity and technological development*. It can be found in the title of the new Act No. 300/1992 on the State Support of Scientific Activity and Technological Development; it featured in the title of the newly established advisory body to the government, the Council of the Government of the Czech Republic for Scientific Activity and Development of Technologies established under the law. Before the end of the millennium this has changed. A 1995 amendment introduced the collocation *research and development* in the name of the Council. The first national policy adopted in 2000 to steer the field was titled National Research and Development Policy (Vláda České republiky 2000). Still later, with the 2008 Reform, innovation was added. The Council and national policies today refer to *research, development and innovation*.[48] Although I did not appreciate the symbolic implications of the statement at the time, a top-ranking state official admonished me in a 2006 research interview for using the word 'science':

> "*First: in this country, as in the Commission, there are two sets of terms. Research and development is more common. These two terms are defined legislatively. The term 'science' is not defined there. Reasons are complex.*" (civil servant, male)

These differences are not merely rhetorical and they certainly are not innocent. They bring us back to the ideological tensions I discussed in Chapter 1 referring to Latour (1998). Over the course of the last twenty years we can see a shift in science policy, with increasing stress on economic profitability, commercialization and applicability. During the 1990s the few policy documents extant operated on the dual priorities of developing human knowledge and economic prosperity of the country (Vláda České republiky 1994). By 1997 the government emphasizes the importance of research and development "*not only* as a contribution to world knowledge *but especially* for the economy of the country and its education" (Vláda České republiky 1997) (my emphasis). The duality is maintained in the first National Research and Development Policy adopted in 2000 which defines "science [as] a matter-of-fact cultural value, belonging to basic cognitive needs of a person; on the one hand it satisfies human desire

[48] At the height of the clashes surrounding the adoption of the 2008 Reform and budget cuts in 2009, the law and the Council briefly referred to research, experimental development and innovation.

for knowledge and expanding one's own cognitive horizons, on the other hand it is a condition for producing material goods and permanent development of society and quality of its education. As such it has short-term and long-term goals." (Vláda České republiky 2000)

Subsequent national policies do not refer to expanding cognitive horizons or science as a cultural value. The 2004–2008 National Policy adopted in 2004 uses bureaucratic language to attest to efficiency of government when it states that "[P]olicy of research and development is a standard part of an integrated system of national policies dealing with main areas of functioning of society in most developed countries" and links research and development policy primarily to educational and innovation policy and secondarily to employment, information, industry and trade policies (Vláda České republiky 2004a). The 2009–2015 National Policy identifies "advances in research and development resulting in implementing innovation" as the only way for the Czech Republic to face global challenges where "from the national perspective sufficient efficiency in innovation is a necessary prerequisite for maintaining competitiveness, economic growth and social stability" (Vláda České republiky 2009b: 8–9). If in 2000 the National Policy aimed to tackle "prospective needs of citizens, society and economy of the Czech Republic", "improve the health, quality of life and increase satisfaction of citizens, competitive production of products and services, permanent development of society and its education and tackling global problems of contemporary and future world" (Vláda České republiky 2000), by 2009 the goal was to "stimulate the development of knowledge society which will lead to further increase of competitiveness of Czech economy and improvement of the quality of life of inhabitations of the Czech Republic" and creating "an environment which will motivate toward excellent research and creation of new findings usable in applications and at the same time will lead to an increase in demand for results of research and development from the application sphere and their transformation into innovative products and services" (Vláda České republiky 2009b: 9). The motto of the 2008 Research, Development and Innovation Reform is perhaps the most succinct expression of the entrepreneurial alignment of Czech science policy: "*Science turns money into knowledge, innovation turns knowledge into money.*"[49] (Vláda České republiky 2008) In

[49] This motto echoes the Danish government's catchword for their university reform, 'From idea to invoice,' arguing that academics should develop closer relations with industry and focus on results that would lead to innovations (Carney 2009; Wright n.d.).

the gradual shifts in tone and stress (from "and" to "not only but especially" for example), reordering of words and disappearance of others we can see the "fingerprints" of changes in the rationality of governance (Shore and Wright 1997: 14). In the governmentality regime today there are no traces of wealth of knowledge, culture or cognitive horizons; research materializes as focused on economic development and innovation, on products, the "application sphere" and economic results. The metaphor of research and development as the engine of national economy and competitiveness enacts knowledge production in terms of efficiency and economic performance (cf. Matonoha 2009: 151). Czech research, development and innovation policy today harnesses science for economic needs.

With these local enactments of research and development, their objectives as well as visions of society, Czech policy joins an international neoliberal *policyscape* (Carney 2009) of research, development and innovation. As I argued in Chapter 1 the global science policyscape is increasingly enacted in the neoliberal governmentality of economic development, competitiveness, stress on application, commercialization and marketability, with novel forms of accountability regimes governing institutions and individuals. The Czech science policyscape shares some of these overarching "imaginative regimes" (Carney 2009: 83) related to the neoliberal, growth-oriented EU and OECD research and innovation policy but the entrepreneurial alignment stabilized in policy documents has not yet produced the variety of bodies and practices introduced in recent years in some other countries (Hellstrom and Jacob 2000; Hemlin 2006).

In what follows I will examine the evolution of the local technology of a global research assessment script. With the elevation to the level of national policy and the development of a national assessment system, evaluating science has entered a new set of relations where diverse interests meet and clash.

5.2 Different orders of excellence: Entrepreneurial alignment of research assessment

In Chapter 3 I have argued that the introduction of a research assessment system at the research institution I studied was enacted as something inherent to science, as an instrument of building an outstanding research institution propelled by an internal logic

of global organization of science, without a link to science policy at home or abroad. We can see a similar ethos expressed by a high-ranking representative of the Academy of Sciences with regard to a process of transformation which the Academy launched shortly after 1989, with a view "*to create an autonomous institution that would evaluate itself; that would carry out superb research; and where the main thing would be scientists themselves, and not some bureaucratic management*" (academic researcher, female). The evaluation was, of course, about accountability and had major impact on the life of academic institution and individuals. But it was conceived of as professional accountability within a community of peers.

The introduction of research assessment at the Academy of Sciences in 1991/1992 was a novel step, one depicted by representatives of the Academy as a way to put the institution back on the global research map and return it to a self-governing professional accountability system. Research assessment was seen by the leadership and prominent researchers as a way to *de-politicise* science, of doing away with an award system based on party affiliation and loyalty. Research assessment was regarded as an objective, scientific, internal, self-administered process that would right past wrongs. It was a moral undertaking, with ethics firmly embedded in Mode 1 science (Gibbons et al. 1994; Nowotny, Scott, and Gibbons 2001).

> "*Before research results were of no one's concern; the institutional hierarchy was built totally independently of how much a person published, what he knew. I would even say that people did not really know each other in this respect. (…) It was only in 1989 that we did an evaluation of all researchers at the institute and everyone had to find how and with whom they cooperated so that it could be established whether their work had any impact but mainly to see how cited they were. Everyone had to submit their work, and suddenly this hacked the structures extant at the institute.*" (academic researcher, female)

This quote underscores just how strong the structuration of the science field was in the Academy of Sciences after 1989 in terms of the stress on research quality established through publications and citation, as contrasted with the previous system of awards based on political allegiance. Clearly, Western science was a strong centre, not only in terms of organizational practices and evaluation procedures but, very importantly, as a political centre. The older generation of researchers, who experienced a large portion of their professional careers under socialism, admonish even today early stage researchers

to cherish and value what they have, stressing particularly academic freedom and mobility.

> *"From time to time I emphasize to my doctoral students what immense opportunities they have today compared to us older ones, but they don't want to hear it anymore. We had to go through political-education courses; I was obligated to attend the Evening University of Marxism-Leninism once a week for three years! This was the only way for me to stay in academia, in pharmacological research. But they can't understand today that something like this could have happened and so when I am strict with them, they think I'm crazy."* (prof. MUDr. Alexandra Šulcová, CSc.) [50]

It was the explicit politicisation of science and its organisation before 1989, and the perception of the West, and in the biosciences namely the US, as an epistemic centre that oriented the transformation after 1989 and the values which navigated it. This political constellation and the moral orders attached created an environment where conditions for isomorphism were ripe, with researchers themselves were a powerful vehicle for institutionalizing research assessment.

After two rounds of assessments in the 1990s, the Academy of Sciences could feel self-satisfied and self-assured. As its ex-President stated: "*This was perhaps the best assessment because it was not influenced by anything from the top*." The scientometric assessment was not applied universally to all disciplines and institutes. There was recognition of differences: different speeds of publishing, epistemic styles and histories of research institutes. The assessment was "*based on enlightened scientometry, based on a person sitting there, scientists sitting there*". After these two rounds, then, there was a strong proclivity among some natural scientists to introduce research assessment at the national level. Representatives of the Academy of Sciences in the Council and its commissions became active in suggesting that research assessment should be performed nationally.

> "*My colleague Hořejší and I have been very involved in pushing for research assessment; he was the chairman and I the deputy chairman of the commission and thanks to us, thanks to the fact that we were ready and knew what we wanted, and we went strongly after it (...) You have to know what you want. To know what you want, you have to know the situation in science, in the functioning part, of course.*" (academic researcher, male)

[50] Interview in Czech available at http://www.zenyaveda.cz/prectete-si/rozhovory/prof-mudr-alexandra-sulcova-csc.

This is, in fact, a claim by one of the group leaders at my field site, and underscores the perception of bibliometric research assessment as an unproblematic measure of scientific work which can separate the good from the bad. It also underscores the self-assuredness of the Academy of Sciences, especially in the natural sciences, in terms of its own performance, seeing the assessment as a way to bolster its stature in the national research and development system. And it also underscores the normative features of the "functioning" (understand Western) parts of science. I must add here that this person was not a proponent of research assessment blind to disciplinary limitations. In discussions with him, he remarked on the difficulties the Methodology posed for the humanities and social sciences which his son had to navigate at one Czech university. While he recognizes disciplinary specificity, his belief that bibliometric assessment was appropriate for the natural sciences was intact and strong.

As is often the case, when objects and people embark on travels, they rarely go unchanged. And so while the original idea was arguably to support outstanding research, the resulting Methodology adopted by the government to assess Czech research at the national level was a different mechanism. The Methodology was introduced in 2004 based on Resolution of the Government No. 644/2004. One of the reasons stated in the justification report was: "*dissatisfaction of the research community with the fact that the system is incapable of appreciating the quality of achieved results in a sufficiently flexible manner*" (Vláda České republiky 2004: 10), reflecting the desire to nationally differentiate according to bibliometric performance.

The year 2004 was special not only because the Methodology was adopted at the national level. It was also the year when the Council took a decisive turn toward an economic, technocratic and industrial orientation. Representatives of both the business enterprise and academic research sectors identify this shift with the appointment of Martin Jahn, the deputy prime minister for economics, as chairperson[51]. He was the first chairperson of the Council that clearly advocated economic aspects of science and

[51] Between 1994 and 2004, before he became Vice Prime Minister for Economics, Jahn worked at CzechInvest, the state agency to support entrepreneurship and investments subordinated to the Ministry of Industry and Trade. In 2009 Jahn also served as a member of the National Economic Council of the Government, an independent advisory body of the government under Prime Minister Topolánek. After his political tenure Jahn started working in the automobile industry, first in Škoda Auto a.s., then Volkswagen. He is also the President of the Czech Automobile Industry of the Czech Republic. With 30%, the automobile industry is a sector which performs the largest portion of funding in the business enterprise sector; 98% of the research and development expenditures in the automobile industry is performed by business under foreign control (Úřad vlády ČR 2012: 17).

science policy. If in 2000 the annual report of the Council stated: "*In an effort to increase the material and intellectual well-being of society, an important role is played by science – research and development. Its support is a manifestation of the educational and cultural development of a country.*" (Rada vlády České republiky pro výzkum a vývoj 2000), by 2004 research and development were defined as an instrument for building competitive economy, and innovations were gradually making their way in.

The 2004 was thus a year when the business enterprise sector (BES) secured a staunch supporter in the Council for the first time since its inception in 1992. And it came precisely at a time, or perhaps because of it, when the national R&D budget under the Social Democrats started to grow. By 2011 21% of the public budget for R&D&I was invested in the business enterprise sector, a portion relatively higher than the EU 27 average. While the BES contributed 47% to the total R&D expenditures in the country (down from 56% in 2006), 60% of the total expenditures are performed in the BES. In fact, 98% of R&D expenditures from the BES sector were performed in the BES (of which 65% in foreign affiliations), 1% in the Academy of Sciences and less than 1% in the higher education sector.

When we look specifically at public expenditures in the BES we see that 70% (CZK 4.7 billion) are allocated in private domestic enterprises, 15% (CZK 0.8 billion) in public enterprises and 15% (CZK 0.8 billion) in foreign affiliations. (Český statistický úřad 2012) Thus, in the earlier paper, Linkova and Stockelova (2012) could not agree with Lepori et al. (2009) who argued that "[t]he high share of the private sector [in receiving public research funding] is largely explained by research institutes that were reorganised ('privatised') from state research organisation in the first half of the 1990s." Domestic private enterprises (and not public enterprises) consume a substantial chunk of government expenditures on R&D, without any clear contribution to public interest. The new direction taken by Czech research and development policy towards greater public support for private BES research was also reflected in the assessment criteria, the introduction of which coincided, as we have seen, with this reorientation of R&D policy and in particular the adoption of the 2005–2010 National Innovation Policy (Vláda České republiky 2005).

These shifts were crucial. Firstly, the assessment system was suddenly no longer discussed within a fairly homogenous circle of academics who largely shared the vision

and values of the assessment. The assessment was elevated to the national level where other actors were invited to contribute (from the BES, higher education institutions, state departments etc.). Secondly, the powers of the Council increased, and with it the importance of membership, which changed from being "*an honorary public function*" where "*dozens of outstanding scientists, research managers, representatives of industrial research and world of finance have worked selflessly*" (Rada vlády České republiky pro výzkum a vývoj 2000) to a place where partial interests came to be protected and discussion stalled, as the following quote illustrates:

> *"The turning point clearly came at the moment when Mr Jahn came in, who represented industry; and with him the moment came when all discussions stopped. (...) But the effort to pit two players against one another, that was there, this thing absolutely fascinated me because manners used in the Parliament were transposed here, but they are not used in the scientific world."* (academic researcher, female)

On its journey from the Academy of Sciences to national research and development policy, we can see marks of *re-politicisation* (Linkova and Stockelova 2012), something the research community may not have been prepared for. Research assessment became an issue to be resolved in a political arena, a mechanism of aligning research with a specific vision of public good and a social order. Located in a new policy context, with new stakes and new actors, the previously clear-cut role of research assessment as an instrument of separating good science from bad, acquired new dimensions.

The Methodology instituted a judgmental (as opposed to a developmental) system of research performance assessment[52] based on attributing points for defined results. The 2006 Methodology states that the point allocation for individual results was achieved "*after long discussions as a wide consensus of expert commissions of the Council and members of the Council which took into account the often conflicting positions of individual interest groups*" (Úřad vlády České Republiky 2006: 9). Despite this alleged wide consensus, some interest groups came to have a stronger voice. Disproportionately higher point scores started to be attributed to high impact journals and certain types of patents (gradually publication in the *Nature*, *Science*, and *PNAS* and US, Japanese, and

[52] According to Townley (1997: 6-7) "[A] judgemental system places the organization's concerns with control and a centrally coordinated information system to the centre. Used as a tool for resource allocation… documentation is accessible to central administration and is the basis of compensation, promotion and disciplinary decisions."

European patents garnered 500 points). These established explicit policy geographies focused on specific, particularly Anglo-American, countries and regions.[53]

Applied results which have not been subjected to any quality review by the Council, and are much more difficult to assess in terms of quality and impact, commanded double or triple value compared to results such as books which undergo peer review and which have been additionally reviewed by the Council.[54] The number of points attributed to applied results (even those that have not been reviewed in any way) thus came to command a lot more weight than standard academic results.[55] The impact of knowledge was reduced to very particular applicable, marketable results. With its exclusion of civil society organizations, the Methodology has consistently denied epistemic authority or even capacity to relevant knowledge stakeholders (Stöckelová 2012: 16–26).

Although the Resolution of the Government stipulated that the assessment criteria were to be known in advance, transparent and subject to review, the Methodology changed on an annual basis from its inception in 2004, thus introducing a high degree of uncertainty into the system. Shore and Wright (1999: 569) note the potential intentionality of such frequent changes and the insecurity they bring, as a precautionary principle lest researchers get wise to the assessment system and learn to game it successfully. With its stress on fast results and immediate application, the Methodology is an acceleration apparatus. I will return to this issue of time, already explored in the preceding chapters,

[53] In 2013 the Methodology was completely overhauled again. It will be based on a three pillar system. In the first pillar, points will continue to be allocated by type of publication output, the second pillar involves panel assessment of selected excellent results and the third pillar governs applied (non-publication) types of results (with point allocations down compared to the previous Methodology). In the new Methodology papers in journals not included in Web of Science, SCOPUS and ERIH in the social sciences, economics, psychology and a few other disciplines, which are assessed according to the same rules as the natural and technical sciences, will receive zero points in the first pillar of the Methodology. As of this year, then, many social science publications in Czech will have no monetary value irrespective of their significance in epistemic terms.

[54] The 2009–2015 National Policy set a goal to improve the situation by modifying the system for evaluating R&D results to "increase pressure on support providers and recipients to significantly increase the number and quality of results of applied research immediately usable for new products, technologies and services (Vláda České republiky 2009b: 17–18).

[55] Originally, the Methodology assumed that research was a level playing field and that all disciplines and all researchers were equal in their access to high impact factor journals and ability to generate income. To address the biggest distortions, the National Excellence Referential Framework was gradually adopted which attributed a slightly higher point score for publications in Czech journals and monographs in selected humanities and social sciences, but the marginalization of these disciplines was still in evidence. Disciplines included in the Framework were philosophy and religion, history, archaeology, anthropology and ethnography, political science, management and administration, legal sciences, linguistics, mass media, arts and architecture and pedagogy and education. Sociology (a discipline strongly linked to the national context), economics and psychology are not included, and are assessed according to the same criteria as the natural sciences.

in conclusions when I return to the epistemic impacts of the changing tempo of academic work.

The Methodology was an attempt to devise a one-size-fits-all formula[56]. In Chapter 3 we have seen how problematic such a one-size-fits-all mechanism can be even within a single research institute with a fairly coherent research agenda and how a uniform way of assessing research output denies various types of epistemic differences. On a national scale, such a formula must necessarily deny many different types of non-coherence. While the Methodology may strive to appear in policy as "a proper order [which] comes with the illusion that all relations can be specific and that it is possible to gain an all-inclusive overview" (Law and Mol 2002: 14), it is a classificatory system which serves to divide and rule and sometimes deny and rule.

5.3 Competitive funding: An instrument of entrepreneurial steering of research organizations and performance

The importance of competitive funding in the organization of Czech research has increased significantly over the years.[57] If in 2005 core funding was CZK 2 billion higher than competitive funding, by 2011 core funding reached only 82% of competitive funding. In absolute terms it was CZK 2.1 billion lower (Úřad vlády České

[56] It is this fact as well that the International Audit criticises when it stated that: "However it [the Methodology] suffers important weaknesses including reductionism, failure to address differences [...] treating all institutions in the same way, regardless of their missions by using output indicators that are in practice arbitrary." (Arnold 2011)

[57] In 2011 for the first time the Operational Programmes of the European Structural Funds came to play a major role in funding Czech research when the funding of Czech research from foreign sources jumped by 75%, with 85% of the foreign funding coming from Structural Funds (Úřad vlády ČR 2012: 123). Between 2007 and 2011 foreign R&D funding coming to the country quintupled (ibid.: 145). These foreign funds are performed primarily in the higher education sector. Governmental analyses of the R&D sector define as "big unknown" the portion of the state budget that goes toward co-financing of the EU Structural Fund projects in the country (Úřad vlády ČR 2012: 51). Estimates are at 16% of GERD in 2012 (Úřad vlády ČR 2014: 30). The impact of this jump increase in R&D funding from the Structural Funds on future gross domestic R&D expenditures has not been fully assessed. The issue of sustainability of the various types of projects is a major concern, and for this purpose the National Sustainability Programme I. has been approved. The first 17 projects will receive funding totalling CZK 2.3 billion (Úřad vlády ČR 2014: 159). Furthermore, because of the higher GDP in the capital city of Prague, which makes Prague ineligible to use the Structural Funds, only a very limited portion of Structural Funds could have been used by institutes and universities located in Prague which affected a majority of institutes of the Academy of Sciences and Charles University. The future demands on sustainability may create further cleavages between Prague and other regions. Tellingly, the governmental analyses do not contain any estimate of the future impact on R&D funding distribution in the country. I wish to acknowledge here these developments which have gained in prominence especially in the last three years and will have major impact on the Czech national R&D landscape but I do not address them in my dissertation.

Republiky 2012: 51) and the plan is to further increase the percentage of competitive funding with a policy goal of a 60 : 40 ratio by 2015 (Vláda České republiky 2009b: 18). In the Academy of Sciences of the Czech Republic core funding in 2010 amounted to CZK 4.5 billion, 10 percentage points less compared to 2005, receiving a total of 33% of core funding distributed in the country (Úřad vlády České Republiky 2012: 51) and around one half of its total funding (Akademie věd České Republiky 2013).

Competitive funding is distributed through seventeen budget chapters, with the Ministry of Industry and Trade being the largest research funder distributing CZK 3.2 billion or 27% of competitive funding, with private domestic enterprises as recipients. The Ministry of Education is the second largest competitive funding provider with CZK 3.29 billion or 26% and the third is the Czech Science Foundation with CZK 2.4 billion or 20%. Following the 2008 Reform, the Technology Agency of the Czech Republic started distributing competitive funding and in 2011, for the first time, it distributed CZK 777 million or 6.4% (Úřad vlády České Republiky 2012: 51–52). The 2009–2015 National Research, Development and Innovation Policy of the Czech Republic defines a further goal of increasing the share of competitive funding for basic research, and identifies as one of the problems that a larger portion of research staff salaries is not covered through competitive grant funding (Vláda České republiky 2009b: 18).

At the same time, competition has increasingly become the logic behind the distribution of core funding as well. According to the 2000 and 2004–2008 national research and development policies (Vláda České republiky 2000, 2004a) core funding was intended for "long-term development of research and development building on a integrated concept of activity of the organization in R&D". It was to be distributed without public competition to ensure "solid, long-term activity of research organizations" (Vláda České republiky 2000). By 2004 the policy provided for "regular and demanding assessment" of all institutions receiving core funding and there were first hints of focus on application and cooperation with the private sphere. Furthermore, providers were to use assessment results as an "important basis" to define the total core funding (Vláda České republiky 2004b: 6). Importantly, though, this assessment was to be performed on a disciplinary basis (Vláda České republiky 2004b: 14) and there was no link between the assessment and the application of the Methodology which was being prepared concurrently.

With the 2008 Research, Development and Innovation Reform, "the purpose" of assessment is defined as "distributing institutional expenditures for research and development among budget chapters" (Vláda České republiky 2009b: 10). The follow-up 2009–2015 National Policy (Vláda České republiky 2009b: 11) expresses the concern that distribution of core funding is "not sufficiently linked to the assessment of research work". Implicitly, the long-term nature of institutional support spanning four to six years is rejected as too long, and distribution of core funding is claimed to be a "claims based item" "lacking principles of competition for public funds" (Vláda České republiky 2009a: 3). The National Policy stipulates for a methodology for assessing results based on "bibliometric data, patenting activity and other indexes concerning the use of R&D results" (Vláda České republiky 2009b: 17-18). This goal of increasing the efficiency of public support for R&D is linked—through researchers—to another goal, to "use R&D results in innovation and improve the cooperation of the public and private sector in R&D&I".

The way to stimulate researchers to "create findings usable in innovation and to cooperate with enterprises", research institutions receiving core funding are to be motivated by a system for assessing the R&D results, the Methodology (Vláda České republiky 2009b: 20–21). Here we come full circle in a mechanism aimed to steer public research organizations not only toward competition but toward particular forms of research work, research cooperation and research results. This entrepreneurial alignment is to be achieved through trickle-down and trickle-up effects whereby institutions are to compete for core funding based on a mechanism which is to motivate individual researchers to produce specific types of applied results. In her analysis of the introduction of a points-based assessment system in Denmark, Wright (n.d.) identifies a similar goal of finding "a single technical measure" that would operate on the three scales of "the competitive state, the enterprising organization, and the 'responsibilized' individual" according to the government's ideological and political vision.[58]

Today, there is—surprisingly—no information available as to the volume of core funding distributed according to the Methodology. The R&D&I Reform section of the www.vyzkum.cz website does not contain any update on the implementation of the

[58] Needless to say, the formerly dominant professional governmentality regime has not, however, been superseded by the economist, performance and efficiency oriented governmentality (Bleiklie 1998), and they also co-exist in an uneasy, non-coherent mix.

Reform since 2009; the annual Analyses of the State of Research, Development and Innovation prepared by the Council do not provide any information on the implementation of this goal, either. Yet despite the problems facing the implementation of the Reform, not least because of the assessment mechanism and the planned reworking of the Methodology in response to the 2011 R&D&I Audit performed by the Technopolis consortium and the follow-up Individual National Project titled An Efficient System of Assessment and Funding of Research, Development and Innovation (IPN Methodology) signed into contract with the same Technopolis Ltd. in May 2014, the Methodology is having effects. It has invaded the life of research institutions and researchers and, in line with the 2009–2015 National Policy, research institutions are adopting assessment systems that often copy the Methodology.[59]

We have seen in Chapter 3 that the introduction of assessment and the transformation at the institute was originally linked to its effort to build an outstanding, world-class research institute, and a similar goal was in evidence in the initial assessment introduced in the Academy of Sciences. The effects of the competitive assessment, however, have not been linear and have resulted in re-orientation of epistemic practices and sociality at the institute. The dynamism of competitive funding precipitated a shift in the organization of research involving a shift from the dynastic to dynamic lab. The dynamism of funding has necessitated dynamism in organization. Increased volumes of funding distributed competitively on the national and international levels reorient the organization of research work and research groups and stall the ability of research institutions to offer long-term prospects to incoming researchers. Whereas the dynastic organization has been made possible by stable, long-term core funding for salaries of research staff in research organizations, the dynamic organization requires a principle on which to base the award of chairs, groups and postdoctoral positions. Research assessment is thus an integral part of this complex change. Or, put differently, with the introduction of research assessment as part of a particular governmentality regime in science, the areas that can be claimed by the assessment gradually expand, and so under the 2008 Reform core funding in the Czech Republic came to be claimed by the competitive logic. Core funding is no longer expected to serve long-term development

[59] If national policy and the Methodology were invisible when I was entering my research site in 2006, by the end of the following year group leaders were conceding that the Council of the institute was about to debate its internal assessment vis-à-vis the Methodology, concerned about the number of points attributed to Czech patents and the disproportionate difficulty of publishing a paper in a "really good journal" compared to Czech patents.

of research institutions pursuant to a research plan but rather to reorient research organizations and researchers performance toward particular types of results.

5.4 Efficiency, wealth and waste of human resources

We have already seen the degree to which the Czech research and development policy performs science in the mode of enterprising, as an instrument of increasing Czech competitiveness and economic growth in knowledge economy. Investments in and social relevance of research, development and innovation are justified in terms of positive economic impact. Interviews performed with policy makers underscore the degree to which representatives of the State embody this ordering of science policies.

> "*Science is becoming an important economic force in contemporary societies and creates conditions in which we live.*" (official, Office of the Government)

Another state official at the Office of the Government saw the benefits for society "*clearly in terms of development, economic development*". These state officials define returns in economic terms, "*that tax payers get back money which they put in it"*. The policy makers and officials were highly concerned about the "*sore point*" of Czech science, namely the inability of "*natural scientists, technologists, engineers being to recast their thoughts into an immediate profit for society*" (deputy minister, Ministry of Education).

The issue of "human resources" is framed in line with the entrepreneurial vision of science, "*our country is not such that we could find here other wealth than our brains; and it must be cultivated, investments must be made in it, and therefore I think that for the Czech Republic supporting research and development is a sensible way*" (deputy ministry, Ministry of Education). Policy makers repeatedly referred to the scarcity of natural resources in the Czech Republic and the resulting necessity of the country to "*put its bets on what is sometimes imprecisely called knowledge society the engine of which is science, research and development*" (official, Office of the Government). Some of these policy makers and officials mentioned the need to tackle the issue of human resources for research from the elementary educational level up, especially as regards attracting young people to the natural and technical sciences.

These opinions are mirrored in policies which consistently call for the need to increase the attractiveness of research careers and science since having "available human resources are basic precondition of such levels of development in research and development that will enable an increase in competitiveness of Czech economy…" (Vláda České republiky 2004a: 6). In all national policy documents we the find the call to increase the attractiveness of research careers, providing adequate working conditions (especially in terms of salaries) and increasing the number of researchers. The 2000 National Policy contains the goal "to increase the interest of the youth in research and development and activity in this field – their interest must be awoken and developed from the earliest school years" (Vláda České republiky 2000: 9). The 2004–2008 National Policy includes the priority of "increasing the prestige of research and development to maximally increase the interest of the youth in activities in this field. Furthermore, young people's interest in research and development will be supported through appropriate activities at elementary and secondary schools" (Vláda České republiky 2004a: 7). Finally, the 2009–2015 National Policy addresses "the lack of interest of the youth in the study of natural science and technical disciplines and in science in general" (Vláda České republiky 2009b: 26).

When prompted to address how this concern with human resources touches upon the issue of gender equality and the available resources of women in science, the framing research and development policymakers choose remains within the entrepreneurial mode, and the development of this particular human resource is recast as luxury we cannot afford: "*I will be honest. This is luxury which we can't afford since we have to make choices.*" (civil servant, Office of the Government) The high attrition rate of women from Czech science[60] is not regarded in research policies and by policymakers as waste that needs to be curbed; rather, developing women's potential is regarded as a luxury that Czech science cannot afford in its quest for a competitive place in global science.[61] Czech policymakers and politicians recognize the need to invest in human resource development, supporting mobility and making the best use of the resources the

[60] The Czech Republic has a balanced ratio of women and men who have the qualifications to work in scientific and technological positions. In 2012 women made up 48.4% of the core of human resources in science and technology, but only 27.4% in the position of researchers, the lowest percentage since 2001 when sex-disaggregated statistics started to be collected (Tenglerová 2014a).

[61] Interestingly, though for other reasons than the research and development policymakers and civil servants, policymakers responsible for equal opportunities for women and men also regard the issue of gender equality in research as luxury. Women scientists are seen as an elite group in a powerful profession, and there are more pressing and serious issues such as domestic violence or discrimination on the labour market of disadvantaged groups of women.

country has; on the other hand, women scientists are a particular resource which does not require attention and thus, implicitly, can be wasted. The perceived costliness of developing women's human resources denies its relevance and displaces it from policy documents. Interestingly, the concern with increasing salaries in research which is seen as a major instrument in policy documents to attract human resources is not regarded as costly or as a luxury we cannot afford.

Among policymakers and politicians responsible for equal opportunities for women and men, the notion of science is very different. The gender equality staff and policy makers work with a notion of science as an area on its own, self-governed and separated from society. "*It is necessary for the people inside the organization; I can't list measures, it must be defined by people who are inside,*" stated one parliamentarian in relation to the potential measures that may be needed in the Czech Republic to increase gender equality in science. It echoes other such statements from science 'non-insiders' and attests to the authority and the perceived exclusive position science commands. Science is located here outside society, as an area that cannot be governed through policy but rather must be controlled internally, by scientists themselves. In contrast to their counterparts responsible for research and development, these policymakers and state officials frame science as a specific and specialized profession, divorced from policymaking. Since their view of science is that of pure epistemological activity, separated from society, they do not identify gender issues in relation to knowledge production (such as assessing research performance, definition of research priorities, framing of research questions, gender-biased interpretations of scientific findings etc.). Because of the way they perceive science as immune to social values, they do not regard research and development policymaking as an area which should be of their concerns as an area of equal opportunities policymaking.

Work-life balance is the only issue where R&D policymakers and state officials concede action may be merited. Women scientists are seen as *handicapped* because of their maternal role and childbirth. According to policymakers this handicap is located in the "*natural order*" and the biological predispositions of being a man and a woman. The distribution of roles in the domestic sphere copies this natural order of things and creates an "unnatural handicap" related to the uneven distribution of roles and tasks in the family.

> "*Any measures which will help to overcome the natural and sometimes even the unnatural handicaps facing women scientists are welcome. I would not break my leg over it and I definitely do not think that the measures should disqualify someone else instead. But I am definitely a friend of support, state support even, related to laws and family and social policy so that women could fully participate in scientific research.*" (deputy minister, Ministry of Education)

However, even when the issue of combining professional career and family commitments is recognized, science is enacted as immune to these concerns, which are located outside, in social and family policy and in providing childcare facilities. These areas are of no concern to science or research policy and must be ensured by other areas of the state administration (Tenglerová 2011: 70). This displacement is possible through separation of the domain of science from society and social processes and through locating the issue of work-life balance in *the women's handicap*.

Research and development policymakers and officials manage to maintain the separation between science and work-life balance issues by mobilizing a particular notion of science, "*an incredibly specific activity"* which demands "*huge personal sacrifices*", a mission to which everything must be subordinated. This strategy allows research and development policymakers and officials (and not only them) to create space for displacing the blame for the low numbers of women in science on women scientists themselves. It allows them to argue that women opt out by having different life priorities and should not be forced to give up their "*womanhood*". They can thus maintain that the organization of research is neutral and its practices not gendered. "*I would not say that measuring research performance and comparing excellence in science is what would prevent women from full participation.*" (deputy minister, Ministry of Education) Conditions on the scientific labour market are regarded as uniform and placing equal demands on women and men; it is up to the woman whether she can make use of these equal conditions or not, and whether she makes the right choices, echoing here again the logic of choice we have seen at work in the previous two chapters. When she does, there is nothing to stand in the path to her success in research. These policymakers are, however, quick to add that it is women who *always* care for children of certain age and therefore they will "*miss the train*".

This separation between the gendered ordering of the scientific labour market and the gendered ordering of the 'caring labour market' makes it possible to displace the responsibility for scientific career and performance on women solely, without

recognising the structural conditions in which women (and men) make choices about family care and professional career. Women are thus held individually responsible for making certain life choices which result in the natural handicap mentioned above. In this way, structural discrimination is naturalized and located in women rather than in the gender ordering of the joined institution of the professions and the family.

> *"Research work has its rules and habits. And the thing is that it sometimes does not meet with the possibilities of the woman. She is not held back compared to the man; it places the same demands on both but the man manages better. For example, he sits until late hours of the night but the woman, she can't. And we can discuss whether this is discrimination. It is not, of course, there are men who can't either. And then there are women who can but they mostly do not have children.*" (top ranking representative, Association of Research Organizations)

Similar displacement of responsibility for managing work-life balance issues is in evidence among equal opportunities policymakers science, too. They, too, cast science as a specific domain of human activity, divorced from society and propelled by an internal logic of quality where motherhood is a handicap which makes "*women usable in a limited way"*. In this sense, the policymakers blame the victim without being open to re-considering organisation of research and values on which it is built. This hyper-visibility of work-life balance contributes to re-enforcing gender stereotypes in science about women and men. It effectively stops any further debate about gender dimensions in knowledge production and the organisation of research. Using the concept of *the policy of inactivity* Tenglerová (2011, 2014c) examines discursive practices mobilized as a strategy allowing no action, and identifies the concentration on women in their maternal role and the exemption of science institutions from any responsibility for existing gender inequalities. Policymakers, state officials and civil servants adopt politics of inactivity as a strategy to deny policy relevance of gender differentials in science, with the consequence that the high attrition rate of women from science remains on the margins, if not outright outside, R&D policy concern.

5.5 Conclusions

In this chapter I have traced the recent alignment of science policy, or policy of research, development and innovation, in the entrepreneurial mode and linked it to the

shift toward a neoliberal research policyscape globally in relation to supranational policies (OECD, EU). Abroad the reorientation of public policy (not only as regards research) is a result of larger neoliberal changes in these countries (Carney 2009; Hellstrom 2005; Hemlin 2006; Shore 2008, 2010; Wright n.d.). In the Czech Republic, in contrast, the shifts in research and development policy have not been linked to a unified governmental programme aimed to overhaul the public sector; nevertheless, we have seen that during the second half of 2000s research and development have been reconfigured as the motor of the Czech knowledge based society and economic performance. At the policy level, I would argue, isomorphism is most likely a result of mimetic processes in which uncertainty is a powerful force (DiMaggio and Powell 1983: 151) and the European level plays a major role (through legislation as well as the policy process) on which to model Czech research and innovation policy.

I have further traced the entrepreneurial alignment of science policy in the Czech Republic in concrete features of the policy, namely in the Methodology for Assessing the Results of Research and Development and in shift from institutional to competitive funding, including the introduction of competitive distribution of core funding. In following an assessment system from the institutional level of one provider, the Academy of Sciences of the Czech Republic, to the national level and its enactment in the Methodology, I also traced a shift from de-politicization to re-politicization of research assessment. Where at first, at the institutional level, the assessment served to rid science of political influence after 1989 and restore a merit-based system perceived by its proponents, particularly from the natural sciences, as a well-established, objective and unproblematic measure of one's research activity, at the national level it became a political technology operating as a seemingly objective and indisputable measure of evaluating research which served to prioritize particular types of visions of accountability of science to society. These imaginaries predominantly build on IF journal publications for basic science and on market oriented applied results with a potential for commercialization.

The shift toward competitive funding appears in policy documents to be a rational goal aimed at overcoming clientelism and slacking in the Czech research landscape. The introduction of a competitive basis for distributing funding, especially as regards core funding, builds on a notion of manageable trickle-down and trickle-up effects where policy goals aimed at institutions will result in institutions implementing assessment

mechanisms which will have clear-cut consequences in the form of a particular performance by the human resources at the institution. We have seen in Chapter 3 that such a notion is naïve and reductionist, as individuals do not behave in manageable, predictable ways even when an assessment system is introduced much closer to home.

Finally, by attending specifically to the issue of efficient and full use of human resources and the waste of women's human resources, I have shown the co-existence of two social contracts for science in evidence at the policy level. For research and development policymakers and policymaking the issue of human resources and women in science is located in the mode of enterprising. Whereas generic disembodied human resources are enacted as the most valuable resource that needs to be nourished, the gendered and bodied female resources are a luxury in the entrepreneurial ordering that Czech policy cannot afford. In contrast, for equal opportunities policymakers, science continues to figure as a powerful and specialized Ivory Tower of experts, separated from public policymaking, and as such exempt from their policy reach. This is reflected in how policymakers in both these arenas approach the issue of combining work and family commitments. In line with the notion of science as divorced from societal concerns, with special professional demands necessitated by the epistemic function, there is a tendency in both these policy camps to locate responsibility for navigating parenthood and professional career development in women researchers themselves.

While it can be argued that with the growing policy attention to research and development, which we can see increasingly since the 2000s, the vision, evaluation and governance of science slipped through the academics' fingers by encountering competing interests in the policy arena (Linkova and Stockelova 2012), the notion of science as separated from society, with its specific rules and values continues to hold in other areas of public policy, including equal opportunities. We will recall from the previous chapter that researchers at my field site actively maintained the separation from society, through references to "telling" as opposed to "listening" and through impact factor publications as a proxy for quality. This insistence on separation of science from society in evidence among researchers and some quarters of public policymaking is creating space for academic science to be increasingly interpolated by economic concerns at the level of research and development policy whose entrepreneurial alignment I have charted in this chapter.

6. Conclusions: Accountability of science in the dynamic triangle

This dissertation contributes to scholarship on the institutional change of academe and specifically of the academic natural sciences. I have explored changes that have emerged in the realm of academic knowledge making in the natural sciences over the past twenty years. To analytically conceptualize the ongoing changes I have developed the notion of the *dynamic triangle.* The three poles of the dynamic triangle are the dynamic organization, dynamic subjectivities and dynamic policymaking explored in the previous three chapters. These three dynamisms are inter-related and co-enact an increasingly entrepreneurial governmentality regime in academic natural sciences, one based on increasing competitiveness through research assessment. Through this dynamic triangle I explain the alignment of academic research in the natural sciences in the mode of enterprising (Law 1994). The mode of enterprising has, of course, been an important mode of organizing research for some time. Competition is an integral part of the profession, of the makeup of scientists. And of course, as I have argued here, it is not the only mode of organizing that researchers, institutions and policies engage in. Traditionally, the mode of vocation in particular and the visionary mode have been equally important. Today, the entrepreneurial alignment is giving more prominence to the administrative mode because managing the various types of competition requires a trail of evidence that has to be managed and administered.

The dynamic triangle is primarily an enactment of this entrepreneurial-administrative complex but what makes it so powerful, so capable of acting upon and through individuals, institutions and policies, is that it can harness the mode of vocation and charisma and align practices previously enacted in these modes, in its liking. Writing, we have seen, is a crucial academic competence and can be enacted in the vocational mode as a way to organize one's thoughts and experimental results but it can also be enacted in the mode of enterprising as an instrumental competence reflecting the stress on impact factor publications and involving the ability to recognize and strategically assess when and where a result is publishable.

The entrepreneurial alignment is not a *linear* process, I have argued. One type of science culture has not been replacing another. To return to one of the dominant conceptualizations of the change I discussed in the introductory chapter, we are not witness to a shift from Mode 1 to Mode 2 science (Nowotny, Scott, and Gibbons 2001, 2003) or any such equivalent. Rather different modes of organizing are used by institutions, individuals and policies strategically in specific contexts for particular purposes. With increasing levels of competition and research assessment adopted by organizations, policies and researchers, the entrepreneurial organizing has, however, gained greater legitimacy.

Secondly, the entrepreneurial alignment is not a *trickle-down* process. This development is not an imposition from a place on high to which researchers are subjected as passive recipients of change. The alignment has emerged in a network of politicians and policymakers, certainly, but also scientists, leading representatives of the Academy of Sciences, its institutes, and public universities as well as actors from industrial, business enterprise sector. This is not a system that developed behind the closed doors by cabinet officials. For many years, some of the authorities and leading representatives of the academic research sector went along willingly and contributed their share to developing national plans and strategies through membership in the Council for Research and Development, expert and advisory bodies, and the consultation process in which the Academy of Sciences and universities are included, to mention just a few. In fact, several natural scientists from the Academy of Sciences were prominent in institutionalizing research assessment first in the Academy and then at the national level. At least a part of the public academic sector has thus been complicit and active in reshaping itself. And it has really been only when the budget of the Academy of Science came under threat due to budget cuts in 2008 that some of the high-ranking scientists started to reconsider.[62] Vallas and Kleinman (2008) talk about "asymmetrical convergence" to address the ongoing traffic between the academic and private research sectors in the biosciences in the US, arguing that while the private research sector is

[62] Although I cannot confirm this on a large or more representative sample, my feeling is that if it had not been for the cuts, the leadership of the academic sector would not protest the 2008 Reform. One of the most active opponents of the Reform and the Methodology who was very active in pushing research assessment a decade ago conceded precisely this at a public event during the 2009 protests at the Faculty of Architecture at the Czech Technical University. "If it had not been for the cuts, it is likely that nothing would have happened", he said. And why? Because the shifts in public outlays for research would remain invisible behind the same budget appropriations for the academic sector. It was only because the research budget stagnated that the shift toward business enterprise, university and competitive funding became obvious. The shift necessitated a cut in the Academy's budget.

increasingly adapting the academic culture traditionally linked with universities, with stress on autonomy, freedom, collegial exchange and building reputation through publishing results in prestigious journals, the academic sector is increasingly adopting a culture of extreme competitiveness, showing inclination toward secrecy, which would be normally linked to the stereotypical images of the private research sector and protection of intellectual property. The research institute I studied is indeed concerned with intellectual property protection and has major revenues from patenting, but this is largely a result of one research group. Most other research groups are not directly involved in any knowledge commercialization. Thus, despite the common presupposition that the entrepreneurial alignment of academic science has been occurring in response to growing concerns with intellectual property protection, "the normative codes and practices in which academic science takes place have apparently begun to acquire an increasingly overt entrepreneurial cast even in the absence of licensing or patenting conventions or other manifestations of commercial activity" (Vallas and Kleinman 2008: 288).

Importantly, the emergence of this entrepreneurial alignment in the Czech natural sciences and academic sector in general was not originally linked to the rise in the neoliberal culture and practices of New Public Management which I traced in Chapter 1 in some countries of the West where research assessment has taken a firm hold. At the beginning of the 1990s it originated from very different sources related to the political transformation of 1989, with research assessment arising as a form of objective tool for the academic community to impose a de-politicized governmentality of professional peer accountability. The entrepreneurial alignment has occurred in different political conditions, which may be one reason why research assessment has not received as much critical attention in the country as abroad. Trickle-up processes must therefore be acknowledged as an important part of the stabilization of competitive research assessment in the Czech Republic. After 2000, and especially since the second half of 2000s I have traced policy negotiations revolving around "commercial ethos that has enjoyed the upper hand, especially in an era of sharpening economic competition and the global diffusion of neo-liberal economic policy generally" (Vallas and Kleinman 2008: 305).

Thirdly, the entrepreneurial alignment is not a *coherent* process. In the previous three chapters I have attended to various types of non-coherence where multiple

organizational logics coincide and there are contradictions that may generate tension and conflict (Vallas and Kleinman 2008: 295–296). Employing modes of syncretism (Law et al. 2013) I have attended to the various ways in which the research institute, individuals and policies deal with non-coherence. In line with Law and Mol (2002) I have analyzed various complexities involved in enacting research assessment by institutions, individuals and policies, and showed various types of non-coherence that must be managed. In terms of the dynamic organization embodied in the heightened competitiveness brought by a uniform research assessment, the institution denies some non-coherence arising in consequence of differences in epistemic practices, while other types of non-coherence are tackled through care and domestication. Research subjectivities are equally showing marks of entrepreneurial alignment, specifically evidenced in strategic publication and other types of epistemic activity. One way for researchers to deal with the non-coherence the competitive system brings is through temporal separation and differentiating between sources of accountability. However, the entrepreneurial alignment is not tackled uniformly by researchers in different positions. Group leaders who have succeeded in the competitive system, have gained in power and enact the competitive systems vis-à-vis their groups as well as others are showing a tendency toward partial alignment and manage to domesticate the competitive research assessment system. In contrast, independent scientists whose position is being obliterated in the dynamic organization and may have lost some of the autonomy they enjoyed previously, experience intense conflict over the current changes. Yet this does not mean that they do not alter their practices.

The competitive, entrepreneurial alignment of the dynamic organization has emerged as an internal process of building an outstanding research institution but depends crucially on a competitive distribution of core funding and other resources (lab space, investments). This entrepreneurial alignment of core funding is echoed in national policy whereby institutions are to compete for core funding based on a mechanism which is to motivate individual researchers to produce specific types of results. National policy and competitive funding allocation have been gradually aligned since the first national research policy adopted in 2000 toward a competitive, entrepreneurial vision of Czech science, which now is to serve to interpolate research institutions which are to interpolate researchers to perform according to a particular assessment system, the Methodology. But we have seen that natural scientists and the Academy started

interpolating themselves shortly after 1989. This has not occurred with the explicit goal of science contributing to economic performance of the country; implicitly, however, a seed of competition, being compared and evaluated, was planted as a moral undertaking the goal of which was to separate the good from bad, a measure of good science, international standing and reputation. As I already said, this was understandable: getting back on the global research map entailed assessing where the academic community stands. And as we have seen, separating the good from the bad was also about "hacking the structures", restoring important scientists to their rightful positions and getting rid of the effects of political coverage. But increasingly, assessing quality has come to be aligned more with the entrepreneurial mode where quality means increasing competitiveness, heightened performance and efficiency.

Academic literature dealing with the overhaul of the research landscape is often linked to analyses of the neoliberal spread in science policy along with the practices of New Public Management, from the private to the public sector. It concentrates on how people respond to these policy changes, how research subjectivities are altered in the process of becoming an auditable self. With the dynamic triangle explored in the previous three chapters my aim was to show how the entrepreneurial alignment works through everyday practices of organizing research assessment, research funding, research institutions, research selves and others, and that one change in practice introduced with a totally understandable goal of trying to devise a mechanism to identify outstanding researchers has consequences, unforeseen and unintended, which alter how groups, institutions and policies work and relate to each other, especially when the practice is expected to travel effortlessly as an organising mechanism. This is precisely the promise that research assessment by IF factors held for many natural scientists at the outset and for many it continues to until today. In linking these three dynamisms to explain the entrepreneurial alignment in the natural sciences lies one contribution of my dissertation to understanding the institutional change of the academe.

Before I consider the impacts of the shift to dynamic organization, I need to make one remark. These conclusions and this dissertation are not a criticism of the researchers I engaged with and who gave me their time and often also their friendliness or even friendship. In fact, I hope to have shown the huge efforts institutions and individuals are putting into making this machine of assessment work. This is not a thoughtless process, and for many of the people with whom I engaged managing the shifts, managing their

own personal alignments and that of their institution means intense negotiation, stress and non-coherence, as I showed in Chapter 3. These people work very hard to navigate the competitive demands while training early stage researchers and making it possible for them to set out on their own research path while doing research they consider important, meaningful, in some cases even hoping that their research will have huge impact in the long run[63]. But of course, making the system work means being interpolated, shifting oneself even if slightly, and this is what keeps this particular dynamic organization in its rails. I will now consider implications of the dynamic entrepreneurial alignment for knowledge-making and professional training in the natural sciences. I will explore epistemic implications, which will be followed by consequences for research careers and research subjectivities, and finally gendering of this entrepreneurial alignment.

Firstly, and perhaps most seriously, the dynamic organization with annual and other periodic assessments is having epistemic effects. "Where, in short, are the flying cars?", asks David Graeber (2012). If in the past generations the sci-fi of one became the reality in two generations' time, where are the major breakthroughs today? How come we are not landing on the Mars, building villages there? Graeber argues that "[W]hat has changed is the bureaucratic culture. The increasing interpenetration of government, university, and private firms has led everyone to adopt the language, sensibilities, and organizational forms that originated in the corporate world. Although this might have helped in creating marketable products, since that is what corporate bureaucracies are designed to do, in terms of fostering original research, the results have been catastrophic." Researchers have been altering their practice of knowledge making in the dynamic triangle. The acceleration brought about by the dominance of competitive funding has effects on how research work is organized and results in acceleration of the research process; research questions may have the life of a doctoral student. The necessity to guarantee publications makes it nigh impossible to submit grants on novel topics for fear that the research line may not generate publications in the duration of the project. The skill of writing has been re-ordered as a competence to be managed, involving a particular skill of accelerating the transformation of research results into publishable units. The constant spectre of group assessment, even in five- or three-year

[63] For example that they will be able to describe what happens in the atmosphere when kerosene burns in airplanes, how exhaust interacts with air molecules at high altitudes in low temperatures and what to do to design a solution that will be friendlier to the environment. Or curing HIV.

intervals as at the institute, forces researchers to consider the safety of their lines of research in terms of having a dense and continuous publication track record. Early stage researchers are trained in a system where this is normalized, where an IF paper is THE measure of success, where to imagine a different organization of research where competition is not so heightened and where times are slower brings up the images of the dead-come-alive[64].

Secondly, and relatedly, precarity in the research profession is on the rise. In the dynamic triangle job security has become an oxymoron. In the natural sciences incoming researchers are expected to be on the move, almost constantly. To have a competitive CV, some doctoral candidates opt for a programme abroad; where the move to another institution has gradually become a necessity is the postdoctoral phase. The competition for postdoctoral fellowships and grants has become extremely stringent. In the country, the Czech Science Foundation has recently replaced postdoctoral grants with junior grants, with the express interest of supporting excellence at the beginning of research careers. The grant agency set such strict (and clearly poorly thought out) eligibility criteria that it will eliminate large segments of early stage researchers from competing but perhaps this will have nothing or not that much to do with the scientific promise of these people but rather with their ability to be mobile at a particular life stage.[65] There is no such analysis for the Czech Republic but where it has been done, we can see that the percentage of people who can develop a stable career in science is not very high. Of the 47% and 31% of doctors in the UK and New Zealand respectively who embark on a scientific career, only 0.45% in the UK reach the full professor level and 3.5% a permanent research staff position (The Royal Society 2010: 14) while in New Zealand it is 1.8% and 16.7%. The increasing levels of precarity and the failing promise of a professional career is re-orienting how early stage researchers relate to science and their future in it. I have shown that only a very small fraction of early-stage researchers in my sample developed a competitive subjectivity and a clear upward-looking notion of their career. A much larger portion of scientists at the beginning of their career are looking for a more lateral professional development, an independent

[64] When my colleague Alice Červinková discussed the organization of labs at her research site several years back, the lab leader tried to find an English term for independent scientists to her doctoral and postdoctoral fellows (indeed, hers was a fully dynamic lab). She could not find the word and as she was thinking, one of her students volunteered: "zombies".

[65] The eligibility criteria entail the age of 35, maximum eight years after completing PhD in combination with an uninterrupted six-month postdoctoral fellowship abroad.

creative job in someone's group. With the shift to the dynamic organization, such lateral careers are disappearing. The position of the independent scientist is crucially dependent on dynastic funding, and in the dynamic system, these positions are not sustainable. There appears to be a growing gap between the portrayals of science students receive in their induction to academic science and the realities of the entrepreneurial alignment they experience during their doctoral and postdoctoral training. In consequence, early-stage researchers are manifesting epistemic, professional and caring dis-alignments from the competitive research self, which makes them less interpolable as well as more likely to consider opting out of science.

Thirdly, I have argued that the dynamic organizing in the entrepreneurial mode is having new gendered consequences whereby previous forms of gender discrimination are realigned, taking on new forms related to the overarching role of research assessment, and the organization of research careers, including mobility. The entrepreneurial alignment is thus creating new forms of inequality in the research system which do not replace the previous ones, but complement them and in the process reinforce some of the most problematic aspects of the gendered order. One of the dominant features of gendering reality is that the feminine is subordinated to the masculine in the order of things (Bem 1994; Bourdieu 2000). This under-evaluation of the feminine has been confirmed in recent studies in the sciences (see footnote 1) which show that women's scientific performance is systematically undervalued. Conversely, women also internalize the gendered norms of society, which results in them undervaluing their performance. In a science culture where assessment is all, gendered differentials in assessing someone's research work is bound to have major consequences. It is my argument that with the growing importance of the assessment culture and increasing competitiveness, gender inequality will increase unless the research community starts paying attention to the practices of research assessment and their gendering. My analyses of the gendering of this entrepreneurial alignment and research assessment constitute my second contribution, one made to the development of the field of gendered organization.

In an important move last year, the European Research Council, in an attempt to understand the large differences in the application and success rates of women and men,

analyzed its application process and the application form.[66] This analysis showed that having an item where researchers are asked to describe their biggest achievements is gendered. In a culture where boasting tends to be expected in men and where women who put themselves forward and boast are seen negatively, asking gendered individuals to describe their best achievements will have consequences. The gap between the self-description and the actual achievement in terms of appointments, publications and grants received turned out to be much wider among women. If the scientific system continues to increase competitive features in its practices and procedures without attending to how competition is gendered, the gendered gaps will increase in this new organization, after the gap has been slowly closing over the last thirty years.

Another aspect related to gendering science rests in the enactment of gender equality policies. Gender equality is a type of governmentality and it can be ordered in many different ways, too. In the 1970s arguments for gender equality tended to revolve around justice and representativity as women made their claims on inclusion and access. With the onset of neoliberalism in some parts of the globe and the introduction of the practices of New Public Management since the 1980s, the enactments of gender equality and feminism change. Fraser talks about the cunning of history in her analyses of how second-wave feminism has been domesticated by neoliberalism (Fraser 2009). Today, especially in research and innovation policies at the EU level and in many countries of Europe, we are seeing the dominance of the so-called 'business case' arguments which justify gender equality not in terms of justice but in monetary terms, in terms of profit and in terms of utility (Linková 2013). The business case revolves around arguments of making the best of human resources, waste we cannot afford and the loss of money states invest in educating and training women.[67] So with the alignment of science in the mode of enterprising we are also seeing an alignment of the particular justificatory genres for gender equality in science. The problem is that it is

[66] The following is based on a statement by Prof. Isabelle Vernos, the chair of the Gender Balance working group of the European Research Council, made at the Lithuanian Presidency conference Structural Change Promoting Gender Equality in Research Organizations, organized in Vilnius, Lithuania, on 21 and 22 November 2013.

[67] For example, at the Lithuanian Presidency conference I just mentioned Prof. Anne Glover, biologist and currently the Chief Scientific Adviser to the President of the European Commission, recounted her experience as the Chief Scientific Adviser for Scotland where she was unable to get the ear of her minister. In the end, she told him that they should introduce quotas for female students in universities. The minister was quite taken aback by this, saying that women already constitute over fifty percent of the student body. Precisely, responded Glover, and the percentage of women scientists is much lower so we're wasting huge amounts of money educating young women.

very difficult to prove the economic benefits of having more women in the scientific profession. Furthermore, much like scientists are interpolated by the accountability mechanisms, gender experts are interpolated by the business case. Hoping to effect positive change by continuing to do gender research into the organization of science they also work at policy level, often using (or having to use, they feel) these same arguments of loss, waste and utilization. While for example European policy documents can hold these types of non-coherence by separating different arguments into different sections of the document, it is much more difficult to hold these together as a single person.[68]

Importantly, this dissertation is not a call to return to the dynastic organization. I certainly do not consider the previous organization to pose some Golden Age to which we should return because things were better, people cooperated unproblematically and there was time to do proper and deep stuff. I concur with many of the criticisms lodged against the dynastic organization. There were petrified research programmes and calcified structures, high levels of cronyism and gender discrimination, it allowed slack. But then on the other hand, this organization created time, it created slow times in the lab as a meaningful organization of science, and it is this aspect that researchers in my sample missed. The typical notion of scientists as aging, white, dishevelled and unsocial men in white coats, bespectacled and with crazy hair does not exist for nothing. It has been formed over a long history of a certain organization of science which made it possible for such an image to arise. This image could have arisen only because being dishevelled, having crazy hair and being unsocial could have been taken as an indication of concentration on research problems in a staked-out ivory tower of seclusion where material or familial concerns did not reach. Today, this image is becoming untenable.

So what comes in its stead? Let us imagine a model. Let us input the information that we have now and let us run an imaginary script. Continue to increase the levels of competitive funding in relation to core funding, and increase the portion of core funding distributed on a competitive level. This is what the Reform has planned and it appears that in its quest for being the most competitive knowledge-based economy Europe will continue to push in this direction, too. Continue to increase the percentage of people

[68] On a personal note, being a gender scholar working to advance gender equality in science the interpolation by the two entrepreneurial alignments can be sometimes very challenging; yet I also believe that this position gives me quite a unique opportunity to analyze the various workings of the entrepreneurial mode of organizing both in science and in terms of gender equality and their intersections.

who will be forced to constantly compete for funding at every stage of the research ladder. Continue to increase the gap between top and junior academics in terms of job security and salaries (Afonso 2013). Input expected behaviour change in response to this competition and having to deliver particular types of performance that count, ensuring a stable publication track. The necessity to manage one's performance track means that knowledge-making practices continue to be subordinated to performance-making practices. Findings will be published at a moment they are publishable; projects will be granted that guarantee publishable result. Individuals will seek ways to enhance their performance, which will result in increasing 'cognitive enhancement' drug use among academics and students, a process that has already started (Vrecko 2013). At the end of the model we may be faced with a situation where at the top there will be the ones who are aligned or willing to align with the competitive mode of enterprising, learn to recognize publishable units, able to protect their selfish time against the hidden and necessary maintenance and sustenance work in the lab. We may end up with a hyper-competitive culture of extreme individualization where people on lower rungs, doctoral and postdoctoral fellows are inducted into a system, and if they do not like it, they can go. Well, they will have to go because the funding, the positions and perhaps their performance will not be there. When discussing the bleak outlook of this dynamic triangle model with a colleague, she commented: "dynamite combination". How long can this system sustain itself before it explodes? My fear sometimes is that it could be self-sustaining, at least for some time. Large portions of doctoral and postdoctoral fellows fall out or opt out of the system because of the dynamic organization, making constantly space for newcomers, and only a portion of those who are willing and able to compete at this level, progress up. The large question is what sort of knowledge will be produced, what benefits this organization of science will carry for society?

I have previously argued (Linková 2009: 96) that the particular notion of research quality we profess today, excellence based on IF papers, is "a version of Darwinian 'survival of the fittest' and that its dominance will not have impact only on researchers, their careers but that it will also "affect how we relate to knowledge in society", what imaginaries of the role and benefits of science we develop. On 20 February 2014 leading Cambridge dons called for a new approach to research assessment as a way to combat gender discrimination (Garner 2014), arguing that "conventional methods of achieving success in academia…appear to benefit men more than women". They call, in

fact, precisely for what my argument was in 2009, that we need to create a system that takes account of the "care for excellence", as Longino calls it (2008). It is necessary to recognize as relevant for research assessment, the types of activities that are hidden in the vocational mode, unaccounted for but necessary for research work, including teaching and outreach work. But we must, I believe, go beyond this. The concerns I have raised in this conclusion, relate to the role for science in contemporary societies we should strive for and the expectations we should have of science. It may not be flying cars but maybe solving environmental threats, issues of energy consumption, hunger and in the future presumably thirst. Well, in the process, we might come to flying cars, too.

To return to the introductory chapter, science has never been locked in its Ivory Tower (Latour 1993b) but has worked very hard to maintain an image of itself as precisely such an exclusive space where only a select few may enter. Because of the far reaching changes in the organization of the public domain, including science, it is increasingly hard to linger on to this notion. Literature on shifting governmentality regimes revolves around the re-framing of the relationship between science and society, highlighting the many new interfaces between the two. Indeed, stakeholder engagement has in many instances become an assessment measure in and of itself, and includes social, cultural, environmental, and economic types of impact. Clearly, debates continue as to how to assess these types of impact (issues of causality, attribution and timescales) (Bornmann 2013: 218–219) but despite these problems, many national and supranational policies, research funders and concrete research projects look for ways to address science-society engagement.

In the Czech Republic, these concerns are largely absent, taken up only by a few civil society organizations and university and research departments (typically by individuals working on the boundary between the two). Voices arguing for broader engagement of science and society are marginal (one such example is Stöckelová 2012). According to these critiques the Czech assessment system should be radically redesigned. These researchers' recognition of the deficiencies of the Methodology is not concerned with the technicalities of point attribution, as some of the other criticisms, but goes to the crux of "whose agenda is seen to hold sway, and whose definition of science is seen as legitimate" (Cohen et al. 2001**:** 162). It is perhaps not surprising that those who endorse the more entrepreneurial vision of science are not keen to open up the space to other

imaginaries of public good than the one of efficiency and profit, as this could compromise the exclusive position the entrepreneurial vision currently enjoys in Czech policy. More surprising is the continued insistence of the camp critical of the neoliberal and neo-technocratic governmentality on the exclusion of the (lay) public and society more generally, as we have also seen here among the group leaders at the institute. Contrary to the common assumption in the research community that insistence on the special place of science will protect its status, there is a real danger that the entrepreneurial interests in research will continue to chip away at the increasingly chimerical Ivory Tower. As it stands now, competitive economic concerns are the only ones that are recognized as relevant in Czech research and innovation policy and among some researchers. Opening up to other societal concerns and working with other societal actors could actually provide arguments to researchers in defence of science responsive to a wider array of values than just the entrepreneurial ones and in so doing help them to uphold some of the ethics they profess. In this process, I want to argue here, science would become more inclusive as it would have to negotiate with interests and voices previously marginalized, excluded or considered simply irrelevant. But it would also be able to start rebuilding a more solid position as a domain where scientific knowledge is created. Over the course of its history, science may have been extremely exclusionary, but it has also given working answers to many pressing problems. If, in an array of many interests and voices, the natural sciences can show and argue that its methods are solid, that they actually work, that processes of verification, sharing results and openness to peer review help to bolster the validity of particular answers to particular problems, it will also be possible to show why other answers do not work (as when research results are reported selectively or it is impossible to replicate research) (cf. Nichols 2014). Bruno Latour (2013) recounts one such instance of a natural scientist involved in the highly political debate on global warming (where obviously economic interests reign high). Engaging in debate and in the process attesting to the solidity of scientificity knowledge-making may, indeed, give scientists an edge. It will be vital that the natural scientists (and not only them) in the Czech Republic realize and recognize the entrepreneurial scenarios of development outlined in this dissertation and take them seriously. If science recognizes its place as one domain among others in society, with processes of deliberation and discussion, it can build its epistemic authority in these processes, and do so in a more accountable manner.

7. Czech summary

Disertační práce *Disciplinování vědy: Dopady proměňujících se režimů vládnutí na akademický výzkum v přírodních vědách v České republice* je příspěvkem ke studiu institucionálních proměn akademického prostředí. Věnuji se změnám, které se v posledních dvaceti letech projevují v oblasti akademického poznávání specificky v přírodních vědách, a konkrétně se zaměřuji na hodnocení vědecké práce. Dřívější analýzy systému hodnocení vědecké práce v ČR ukazují, že tento systém je navržen tak, aby spíše vyhovoval praktikám přírodních věd (Linkova a Stockelova 2012; Stockelova 2012). V disertaci mě proto zajímá, jak se hodnocení vědecké práce projevuje v přírodních vědách a jaké dopady má z hlediska poznávacích praktik, utváření subjektivit výzkumníků a výzkumnic a jejich profesního uplatnění, a v posledku i z hlediska budoucnosti výzkumu v přírodních vědách. Touto studií přispívám k výzkumu institucionální změny „jako součásti nutně širších snah o pochopení rostoucí provázanosti nebo souběhu dříve oddělených institucionálních domén“ (Vallas a Kleinman 2008: 290), v tomto případě domény akademického poznávání a komerční domény.

Jednou z vlivných konceptualizací proměn vědeckého prostředí je pojem věda Modu 1 a Modu 2 (Nowotny, Scott, a Gibbons 2001, 2003). Posuny od Modu 1 k Modu 2 jsou autory analyzovány v pěti dimenzích (kontext vědy, její disciplinární ukotvení, společenská organizace vědy, vykazatelnost a kontrola kvality). Věda Modu 1 odkazuje k tradiční organizaci vědy utvářené v rámci disciplinárních hranic uvnitř univerzit a výzkumných ústavů, která je charakterizovaná dlouhodobostí týmů a vykazatelností vůči akademické komunitě a polarizací mezi objevem a aplikací. Na druhou stranu věda Modu 2 je v tomto pojetí orientovaná na řešení konkrétních problémů, je utvářena v širších transdisciplinárních společenských a ekonomických kontextech, kde si nároky na vykazatelnost klade rostoucí spektrum aktérů. Oproti této binární konceptualizaci se připojuji k teoretickému proudu, který se zaměřuje na analýzy mnohočetnosti a komplexity reality. V tomto přístupu nejsou proměny vědy chápány jako posun od jedné organizační logiky k jiné, ale jako koexistence různých, často protichůdných logik (Lam 2010; Law 1994), které jsou využívány strategicky pro dosahování specifických cílů. V rostoucí míře se ve vědě projevují organizační logiky, které nejsou historicky spjaty s vědou, ale s jinými doménami společnosti, zejména pak s komerční doménou. Jde mi o analýzu toho, které organizační logiky si nárokují místo v akademických přírodních vědách, tedy toho, co Strathern (2005) nazývá doménovým efektem.

Proměny vědy zejména v posledních dvaceti letech souvisejí s obecnějšími posuny veřejné sféry směrem ke „kultuře auditu“ (Power 2003) a nástupu tzv. nové veřejné správy (New Public Management). Jde o manažerskou filosofii, jejímž cílem je modernizace veřejného sektoru na základě předpokladu, že silnější orientace na tržní mechanismy a zavedení principů soutěže tak, jak je známe ze soukromého sektoru, povede k vyšší efektivitě a výkonnosti (Shore 2008). Vědní politika se tak posunuje „od politiky o vědě k politice, ve které musí být věda podřízena socioekonomickým potřebám“ (Godin 2002: 19, viz též Dowdle 2006). Někteří autoři v rozšíření těchto nových forem vykazatelnosti ve vědě spatřují epochální změnu (Dunn 2003: 60; Shore and Wright 2000: 57), neboť žebříčky a hodnocení prostřednictvím kvantitativních měřítek se stávají globální praxí, která přetváří praktiky vykazatelnosti v oblasti vědy (Sauder and Espeland 2009: 80). Vallas and Kleinman (2008) pak ve své analýze přesunů organizačních logik mezi akademickým a soukromým komerčním výzkumem

v oblasti biověd ve Spojených státech hovoří o *asymetrické konvergenci*. Asymetrii autoři spatřují v silnější pozici organizačních principů, které mají svůj původ v soukromém sektoru v podobě vysoké míry soutěživosti a zavádění dalších a dalších principů soutěže v akademickém prostředí. Je to právě rostoucí míra soutěživosti (a nikoliv komercionalizace a marketizace vědění spojená s ochranou duševního vlastnictví, jak se často předpokládá), která v jejich analýzách hraje klíčovou roli v proměně akademického prostředí.

V disertaci se zaměřuji na analýzu zavádění systémů hodnocení výzkumu v přírodních vědách jako specifického režimu vládnutí a kladu si tyto otázky: Jak je hodnocení výzkumu praktikováno jednotlivci, institucemi a politikami a jaké efekty má na epistémické praktiky a profesní uplatnění ve vědě? Jak je dosahováno koherence mezi různými způsoby dělání hodnocení výzkumu a jak je zvládána nekoherence? Jak jsou praktiky hodnocení výzkumu genderovány a jaké dopady má zavádění systémů hodnocení na genderovou rovnost ve vědě? Jaká společenská smlouva pro vědu se utváří v současné době v České republice a jaké jsou její možné implikace pro budoucnost vědeckého poznávání v přírodních vědách?

Své analýzy zakládám na různých typech dat vytvořených primárně v rámci výzkumu provedeného v přírodovědném ústavu Akademie věd České republiky, kde jsem v rámci evropského výzkumného projektu[69] realizovala v období září 2006 až srpen 2007 etnografický výzkum a individuální a skupinové rozhovory s vedoucími týmů, samostatnými vědeckými pracovníky a pracovnicemi, postdoktorandy a postdoktorandkami a studenty a studentkami na bakalářské, magisterské a doktorské úrovni. Rozhovory zahrnovaly úvodní strukturovaný rozhovor, jehož cílem bylo získat základní data o epistémických a organizačních praktikách v instituci, dále probíhaly hloubkové rozhovory a tři skupinové rozhovory (se začínajícími vědci a vědkyněmi, ženami-vědkyněmi a vedoucími výzkumných skupin). Analyzovala jsem též ústavní dokumenty (příkazy ředitele, zápisy z jednání vědecké rady apod.) a dokumenty politik výzkumu, vývoje a inovací (národní politiky, Metodiku hodnocení atd.). Kromě těchto dat jsem využila data sebraná v dalším evropském projektu[70], který jsem koordinovala v letech 2004 až 2006. Jedna jeho část se týkala role vědy v soudobé české společnosti a genderové rovnosti. Provedla jsem rozhovory s tvůrci veřejných politik, politiky a političkami a úředníky a úřednicemi v oblasti vědní politiky a politiky rovných příležitostí pro muže a ženy. K textovým analýzám jsem využívala software na analýzu kvalitativních dat Atlas.ti, v němž jsem organizovala a kódovala data a rozvíjela analýzy. Tato kvalitativní data, zejména pak zúčastněné pozorování v kombinaci s dalšími typy textuálních dat, mi umožnila zkoumat proměny vědy v přírodních vědách v jejich *praktikované* podobě.

Pro analýzu probíhajících změn jsem vyvinula původní koncept *dynamického trojúhelníku,* jehož třemi úhly jsou dynamická organizace, dynamické subjektivity a dynamická tvorba politik. Ústředním principem fungování dynamického trojúhelníku je soutěž. Soutěž je zdrojem dynamizace v podobě akcelerovaného nastavení financování, fungování laboratoří i utváření subjektivit výzkumníků a výzkumnic. Analýzou zavádění principů soutěže v podobě systémů hodnocení vědecké práce v každém pólu trojúhelníku identifikuji podnikatelské nastavení (alignment) v klíčových výzkumných

[69] Projekt Knowledge, Institutions, Gender: An East-West Comparative Study (smlouva č. SAS6-CT-2005-017617) financovaný Evropskou komisí.

[70] Projekt Central European Centre for Women and Youth in Science (smlouva č. SAS6-CT-2004-003582) financovaný Evropskou komisí.

praktikách a subjektivitách. Konceptualizace proměn přírodních věd v ČR v podobě dynamického trojúhelníku je mým prvním příspěvkem k teoretizaci institucionální změny akademické vědy.

V analýzách pracuji s analytickým aparátem čtyř režimů uspořádávání (povolání, podnikání, administrace, vizionářství) (Law 1994). Ukazuji, že zatímco organizace, subjektivity i politiky výzkumu jsou organizovány mnohačetně, v různých logikách, dochází zejména od roku 2000 k jejich rostoucímu nastavení v režimu podnikání. Režim podnikání byl ve vědě pochopitelně důležitý i dříve. Soutěž je integrální součástí vědecké profese a subjektivit výzkumných pracovníků a pracovnic (Karreman a Alvesson 2009; Shore 2008). Jak ale argumentuji v této práci, nešlo o režim jediný a tradičně byly stejně důležité režimy povolání a vizionářství. Dnešní nastavení staví do popředí právě režim podnikání a také režim administrativy, který si vynucuje správa různých typů hodnocení a s nimi spojených aktivit (projektový management, ochrana duševního vlastnictví, komunikace vědy atd.). Dynamický trojúhelník je zhmotněním tohoto podnikatelsko-administrativního komplexu. Co jej ale činí tak mocným, tak schopným působit na jednotlivce, instituce a politiky a skrze ně, je to, že se mu daří do své logiky zapřáhnout režim povolání i vizionářství a nastavit praktiky, které byly dříve zhmotňovány v těchto režimech, ve svém rámci. Akademické psaní, například, je klíčovou akademickou dovedností a může být zhmotněno v režimu povolání jako způsob organizace myšlenek a experimentálních výsledků. Může být ale také zhmotněno v režimu podnikání jako instrumentální dovednost odrážející důraz na publikace s impakt faktorem, která vyžaduje schopnost poznat a strategicky zhodnotit, kde a kdy je výsledek publikovatelný a co tvoří publikovatelnou jednotku poznání. Zavádění principů soutěže a s tím rostoucí dynamika v systému zároveň znemožňuje, vymisťuje, některé dřívější praktiky, organizační principy a subjektivity režimu povolání. Například sdílení pracovního prostoru či přístrojů se v dynamickém systému stává zdrojem, a do hry tak vstupují úvahy o zpoplatňování jeho využívání, kontrole nad ním, možném zneužití takového zdroje v interní soutěži o publikace apod., přičemž kolegiální sdílení se stává obtížnějším.

Tři typy dynamismu, které se soustředí kolem principu soutěže, jsou propojeny a mají dopady v několika ohledech. Za prvé, to, co ustavuje kvalitu, se nastavuje v režimu podnikání. Skvělé výsledky jsou definovány jako publikace s vysokým impakt faktorem, vynikající týmy jsou dynamické skupiny, ve kterých cirkulují začínající vědci a vědkyně zaměřeni na neustálou soutěž s ostatními v rámci ústavu i mezinárodně. Ústav tedy staví na cíli dosáhnout mezinárodního věhlasu a jeho praktiky organizuje snaha o kvalitu. To, co kvalitu ustavuje, se ale zásadně od roku 2000 proměnilo. Za druhé, zavádění principů soutěže v podobě rostoucího podílu účelového financování a kompetitivní dělba institucionálních zdrojů na ústavu (včetně laboratorního prostoru, investičních akcí apod.) má vliv na to, jak je organizována výzkumná práce. Dominantním modelem začíná být dynamická laboratoř s vedoucím skupiny a cirkulujícím oběživem v podobě postdoktorandů a doktorandů, která je periodicky hodnocena a v případě nízkého výkonu může být rozpuštěna a nahrazena jinou, jejíž vedoucí prokáže kompetitivnější životopis a výzkumný plán.

Posun v definici kvality spolu s akcelerací v organizaci vědecké práce má, za třetí, dopady na epistémické praktiky. Důležitým úkolem v dynamické laboratoři je tak například zabezpečit, aby mladí vědci a vědkyně na ústavu získali co největší počet publikací, vybudovali si konkurenceschopný životopis, aby se mohli posunout na další vědeckou štaci v podobě postdoktorské stáže v zahraničí. To vede k omezení možnosti

vyvíjet nové metodologické postupy nebo se pouštět do nových témat, neboť existuje reálné nebezpečí, že takový postup nepovede k publikacím, což by mělo zásadně negativní vliv na profesní uplatnění začínajících doktorandů a postdoktorandů ve skupině. Vědci a vědkyně uvádějí různé praktiky strategického publikování, zejména zvažování výše impakt faktoru při výběru cílového periodika k publikaci. To, jak bude výsledek publikován, je zvažováno při přípravě výzkumného projektu a je součástí designu výzkumu. Klíčovou dovedností se stává schopnost poznat, kdy je výsledek publikovatelný, což vede k drobení výsledků. Nutnost garantovat publikace omezuje žádat o peníze na nové směry výzkumu, které nemusí nutně generovat publikace. Neustálý přízrak hodnocení výzkumných skupin v ústavu a možnost jejich rozpuštění tedy nutí výzkumné pracovníky a pracovnice, aby svůj výzkum orientovali dle nutnosti dosahovat průběžného a hutného publikačního výkonu.

Za čtvrté, tyto proměny v poznávací rovině mají dopad na utváření výzkumných subjektivit jednotlivců a v důsledku toho i na profesní uplatnění ve vědě. Ve svých analýzách ukazuji, že pouze malá část výzkumných pracovníků a pracovnic, většinou vedoucích skupin, vykazuje podmínečnou podporu podnikatelskému nastavení systému a do svého pojímání kvality včleňuje systém hodnocení založený na impakt faktorech. Na druhou stranu početná skupina vědců a vědkyň, zejména samostatných vědeckých pracovníků, zakouší intenzivní konflikt, který se týká pociťované neschopnosti systému hodnocení zachytit kvalitu; proměn mezilidských vztahů na ústavu v důsledku soutěže; a nutnosti chránit výsledky svého výzkumu v soutěži. Tyto rozdíly se váží k jejich hierarchické pozici, typům činností, odpovědnosti a hodnotám, které jejich pozice nesou. Dynamická organizace pozici samostatných vědeckých pracovníků ohrožuje, neboť v ní není systémově ukotvena a nenabízí udržitelnou budoucnost. Zároveň v době, kdy jsem prováděla výzkum, soutěžili vedoucí a samostatní vědečtí pracovníci mezi sebou. Nerovná distribuce zdrojů (studentů, peněz, přístrojů) však vytvářela nerovnováhu v jejich možnosti spolu soutěžit. Ústav nakonec hodnocení jednotlivců v roce 2008 zrušil.

V případě začínajících vědců a vědkyň jsem identifikovala pouze velice malou skupinu tří začínajících vědců, kteří vykazovali byť částečnou soutěživou subjektivitu a cíl dosáhnout na vedoucí pozice. Zároveň ale trvali na tom, že klíčovým faktorem, aby zůstali ve vědě, je schopnost nalézt silné téma. Prostřednictvím silného tématu (tedy i konkurenceschopného tématu, které si na sebe vydělá peníze) tak jsou schopni propojit režim povolání a režim podnikání a budovat si smysluplnou vizi své budoucnosti v dynamické laboratoři. Na druhou stranu ale početná skupina začínajících vědců a vědkyň preferuje ne-vertikální profesní rozvoj, práci v laboratoři jiného vedoucího. Jak ale sami reflektují, taková budoucnost přestává v přírodních vědách existovat. Podnikatelské nastavení, které začínající vědci a vědkyně identifikují jak z hlediska poznávacích praktik, tak z hlediska organizace vědecké dráhy, pak mnohé z nich vede k tomu, že se od vědy „odpoutávají“ a věda se pro ně stává prací bez jasného příslibu budoucího uplatnění.

Dynamické nastavení tak vytváří nová místa, kudy vědci a vědkyně mohou mezi příčkami profesního žebříku fragmentované dynamické dráhy propadnout z vědy ven. Z hlediska dynamické organizace nemusí jít o problém, neboť ta neustále potřebuje volná místa pro další a další doktorandy a postdoktorandy. Otázkou ale zůstává, koho taková profesní mlýnice na počátku profesní dráhy vynese vzhůru a zda je možné soutěživost a schopnost se přizpůsobit praktikám dynamické vědy identifikovat s vědeckým příslibem či výzkumnou představivostí. Druhou otázkou zůstává, jaký typ

poznání, společensky relevantního a odpovědného bádání, může vytvářet systém, který ve svém středu výrazně upřednostňuje soutěživé jednotlivce, již si jsou schopni, alespoň do určité míry, osvojit a zvnitřnit praktiky dynamické laboratoře.

Na základě analýz dále tvrdím, že nastavení v režimu podnikání není *lineární* proces. Jedna kultura vědy není nahrazována jinou. Naopak instituce, jednotlivci a politiky využívají různé režimy uspořádávání strategicky ve specifických kontextech pro specifické účely. Jak ale argumentuji, s rostoucí mírou soutěže a soutěživosti roste legitimita režimu podnikání, a dochází tak k pomalému, propojenému podnikatelskému nastavení ve všech úhlech dynamického trojúhelníku. Za druhé, nastavení v režimu podnikání není proces vnucený seshora. Oproti některým zahraničním analýzám, které proměny popisují jako důsledek zavádění vládních neoliberálních reforem, poukazuji v návaznosti na dřívější analýzy (Linkova a Stockelova 2012) na aktivní účast a spoluodpovědnost části české akademické obce a zejména přírodních vědců na zavedení kvantitativního systému hodnocení na úrovni institucionální i národní. Zároveň podnikatelské nastavení v přírodních vědách a českém akademickém sektoru obecně nezačalo probíhat v souvislosti s nástupem neoliberální kultury či praktik nové veřejné správy jako v mnoha jiných zemích, nýbrž má svůj původ ve snahách o depolitizaci vědy a výzkumu v ČR po roce 1989 a v nastavení režimu vládnutí, založeného na profesní vykazatelnosti akademické komunity, která vnímala bibliometrické hodnocení na základě impakt faktoru jako objektivní měřítko úspěšnosti vědecké práce. V českém prostředí tak ve stabilizaci soutěživého hodnocení výzkumu hrály důležitou roli procesy prostupu směrem nahoru od akademické komunity k vědní politice a institucionální isomorfismus (DiMaggio a Powell 1983), zejména ve své normativní podobě, kdy západní věda a její procesy (hodnocení na základě impakt faktoru a citačního indexu) fungovaly jako vzor. Po roce 2000 a zejména od druhé poloviny první dekády nového tisíciletí ale identifikuji konvergenci na úrovni české a evropské politiky výzkumu a vývoje soustředěnou kolem „komerčního étosu, který měl navrch, zejména v éře stále ostřejší ekonomické soutěže a globálního šíření neoliberální ekonomické politiky obecně“ (Vallas a Kleinman 2008: 305). Za třetí, podnikatelské nastavení není *koherentním* procesem. Prostřednictvím režimů synkretismu (modes of syncretism) (Law et al. 2013) zkoumám komplexity a různé typy ne-koherence, kdy se společně projevují různé organizační logiky, které mohou vytvářet napětí a konflikty (Vallas a Kleinman 2008: 295–296). V analýzách potom ukazuji, jak jsou tyto nekoherence zvládány institucemi, jednotlivci a politikami.

Podnikatelské přeuspořádání je též *genderovaným* procesem a vytváří nové formy genderových nerovností, které dále posilují dřívější nerovnosti systému. Jedním z klíčových aspektů genderování reality je fakt podřízenosti femininity maskulinitě v řádu věcí (Bem 1994; Bourdieu 2000). V posledních letech vzniklo množství studií, které ukazují, že vědecká práce a výkon žen jsou systematicky podhodnocovány (van den Brink and Benschop 2011, 2012; Marchant, Bhattacharya, and Carnes 2007; Moss-Racusin et al. 2012; Steinpreis, Anders, and Ritzke 1999; Trix and Psenka 2003; Wenneras and Wold 1997). Ženy si zároveň zvnitřňují genderové normy společnosti, což vede k tomu, že samy své dovednosti a schopnosti podceňují. Ve svých analýzách ukazuji, že podnikatelské nastavení posiluje maskulinní orientaci vědy zesíleným důrazem na soutěživosti, akcelerací procesů a lineární průběh akademické dráhy. Nárůst soutěživosti vytváří prostor pro vymáhání rostoucího pracovního výkonu, mobility a flexibility, zejména v raných fázích (postdoktorské fázi) vědecké dráhy, a tyto procesy jsou genderově podmíněné. Ukazuji dále, že přestože „rozvoj lidských zdrojů“ je dlouhodobým cílem české politiky výzkumu a vývoje, rozvoj potenciálu ženských

lidských zdrojů není uznáván jako legitimní a hodný podpory. Na politické rovině jde údajně o „luxus, který si česká vědní politika nemůže dovolit“. Genderová podmíněnost některých praktik podnikatelského nastavení spolu s popíráním legitimity nároků na rovné příležitosti ve vědě pak vytváří prostor pro to, aby genderové nerovnosti v přírodních vědách narůstaly. K tomu dochází zejména popíráním či oddělováním nekoherencí, které genderové aspekty spolu s aspekty péče v režimu podnikání vytvářejí. Analýza genderovanosti praktik podnikatelského nastavení a hodnocení výzkumu je druhým příspěvkem této disertace k výzkumu institucionální změny, v tomto případě k rozvoji výzkumu genderové organizace jako *praktikované organizace.*

V závěru práce se zaměřuji na důsledky, které toto podnikatelské přeskupení v dynamickém trojúhelníku může mít pro budoucnost poznávacích procesů v přírodních vědách. Svoji disertaci jsem otevřela otázkou, zda systémy hodnocení, které jsou zaváděny ve vědeckém prostředí a které jsou údajně šity na míru epistémickým praktikám přírodních věd, jsou pro přírodní vědy opravdu tak příhodné. V disertaci jsem ukázala, že akcelerace výzkumu na základě zavádění rostoucího množství prvků soutěže v dynamickém trojúhelníku vede k nastavení poznávacího procesu v režimu podnikání. Akademické poznávání je tak stále více interpelováno logikou soutěže a konkurenceschopností komerčního sektoru. Argumentuji, že pro to, aby věda mohla začít znovu budovat svoji epistémicky silnou pozici, je nutné v rámci akademické komunity otevírat prostor dalším zájmům a organizačním logikám. Takový krok totiž poukáže na parciálnost ekonomistních nároků na akademické poznávání, které v současné době posilují. Akademická věda přírodních věd by se pak již definitivně musela oprostit od chimérické představy slonovinové věže oddělené od společnosti. V diskusích se širším spektrem společenských domén a organizačních logik by pak ale mohla ustavovat znalostní nároky, a to způsobem, který by byl více vykazatelný.

8. English abstract

My analyses build on various types of data created primarily within the framework of a European research project Knowledge, Institutions, Gender: An East-West Comparative Study carried out at a bioscience institute of the Academy of Sciences of the Czech Republic where I conducted an ethnographic research between September 2006 and August 2007 as well as individual and group interviews with team leaders, independent researchers, postdoctoral fellows and students at bachelors, masters and doctoral levels. I also analysed institutional documents and research, development and innovation policy documents. To perform textual analyses I used qualitative data analysis software Atlas.ti, in which I organized and coded data and developed analyses. This qualitative data, and especially the participant observation, in combination with other types of textual data, allowed me to explore shifts in the natural sciences in their *practiced* form.

To analyse the changes I have developed the original concept of the *dynamic triangle* with the three poles of dynamic organization, dynamic subjectivities and dynamic policymaking, to analyse an *entrepreneurial alignment* of the natural sciences in the Czech Republic in the period since the 2000s. With the analytical concept of the dynamic triangle I link three types of dynamisms which have emerged from my examination of the changing governmentality regimes in science as constitutive of institutional changes. The central organizing principle of the dynamic triangle is *competition*. The central argument of my thesis is that these three types of dynamisms, revolving around competition, are crucially inter-related and contingent upon each other, and it is this co-alignment of organizational, subjective and policymaking features that has effected a powerful change in the domain of the natural sciences (cf. Shore and Wright 2000: 61), which is having effects not only in terms of how the natural sciences are organized today, what is happening with research careers and training but also, and very importantly, with how and what scientists can know. By analyzing the introduction of principles of competition in the form of research assessment systems in each pole of the triangle I identify entrepreneurial alignments in key research practices and subjectivities. The conceptualization of changes in the natural sciences in the Czech Republic as a dynamic triangle is my first theoretical contribution, specifically to the theoretization of the institutional change of academic science.

The entrepreneurial alignment is also a *gendered* process and creates new forms of gender inequalities, which further reinforce former inequalities in the system. In my analyses I argue that the entrepreneurial alignment reinforces the masculine orientation of science through a heightened stress on competitiveness, acceleration and the linear ideal of the research career. The increase in competition creates space for demanding increasing work performance, mobility and flexibility, especially in the early career stages (the postdoctoral phase), and these processes are clearly gendered, not least in relation to childcare work. Analyses of the gendered nature of the practices of the entrepreneurial alignment constitute the second major contribution of this thesis to research into institutional changes, in this case to the study of the gendered organization as a *practiced organization.*

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