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Authorship Practices in the Making: On Researchers' Situated
Normalities, Values and Visions of Responsibility and Care

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Knowing is the key to caring,
and with caring there is hope that people will be motivated to take positive actions.
They might not care even if they know, but they can't care if they are unaware.

Sylvia Earle

We tell ourselves stories in order to live. [...]
We interpret what we see, select the most workable of the multiple choices.
We live entirely, especially if we are writers, by the imposition of a narrative line upon
disparate images, by the 'ideas' with which we have learned to freeze the shifting
phantasmagoria which is our actual experience.

Joan Didion in *The White Album*

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Because the learning is not just learning of the factual. (BGSP 2_1.1, p. 18)

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PART I SETTING THE WIDER FRAME OF AUTHORSHIP IN CONTEMPORARY ACADEMIA

1. Authorship as a focal point in contemporary science

Decisions about scientific authorship are of paramount significance in today's scientific system. A researcher's publication record constitutes one of the decisive factors in their career trajectories because it plays a major role in funding decisions, the allocation of positions, and the awarding of prizes. This is reflected in the pervasive view that authorship serves as a "proxy for productivity" (Smith et al. 2020b, p. 1968) in the assessment of researchers. Moreover, Gibbons et al. (1994) note how the dynamics of what they have termed Mode 2 of knowledge production are "expressed tellingly in authorship patterns" (p. 34). One indication of this, Gibbons et al. (1994) argue, is the increase in the average number of authors per scientific publication. This rise in the number of authors has only continued since the 1990s in nearly all scientific (sub)fields, although the average number of authors varies between different research areas (Fanelli, 2020; Johann & Mayer, 2019). Consequently, it is not merely a matter of being listed as an author; rather, the specific authorship position in question serves to indicate the particular achievements and contributions made by the individual to the scientific body of knowledge, with the exception of certain domains such as the social sciences, mathematics, parts of physics, where alphabetical lists of authors are more common (Lissoni & Montobbio, 2015; Müller, 2012; Smith et al., 2020b). Furthermore, the scarcity of tenure-track positions and the pervasive phenomenon of non-linear career trajectories among researchers may inform the aspirations and imaginations of doctoral students and postdoctoral fellows with regard to their future in the scientific community (Boothby & Milojević, 2021). Consequently, decisions regarding authorship carry significant weight for them, while also holding implications for more senior and well-established researchers. Authorship practices can affect not only a researcher's career progression but also their decision to continue or cease pursuing a career in science. Beyond this, the recognition a researcher receives from their peers may also be impacted by authorship practices, which in turn affects their opportunities for collaboration, access to and scope of research equipment, not to mention their overall identity as a researcher. These are just a few of the potential consequences stemming from practices of authorship. Hence, authorship represents a focal point through which the core matters encountered by researchers and which are inherent to the scientific system in its entirety become apparent. As succinctly observed by Moffatt (2018)

"[a]uthorship is a window into the practice of science" (p. 209).

Authors' lists thus represent figures that researchers "inhabit and are inhabited by [...], that map universities of knowledge, practice and power" (p.11) to borrow Donna Haraway's (2018) words. The term 'inhabit' is particularly apt in this context, because this monograph focuses on the significance of authorship practices for researchers in the context of their academic work and professional lives. It does not delve into how assessors evaluate researchers' track records, nor does it investigate authorship patterns based on bibliometric data, which represent alternative avenues for examining scientific authorship. Instead, it undertakes an in-depth investigation of the narratives and valuations articulated by life scientists and physicists based in Austria, whilst maintaining a sensitivity to the situatedness of these researchers. Notwithstanding, as Ursin (2020) notes, "any such locally anchored identities will nevertheless serve as relevant points of view bringing different academics' voices into discussions concerning the nature of current academic work" (p. 322) and scientific authorship.

Hence, based on the analysis of qualitative data, this thesis focuses on how researchers from the life sciences and physics, at different stages of their careers, make sense of and value authorship decision processes, which in turn affects their sense of belonging and identity within the academic community. Foregrounding these aspects, emphasising the person-centred perspective, and acknowledging that authorship is not a detached element in contemporary academia, but is rather an inextricably embedded part, the concept of "*epistemic living spaces*" (p. 19), as elaborated by Felt (2009b), serves as both a point of departure and a conceptual foundation for this thesis.

1.1. Epistemic living spaces. Starting point and conceptual basis

Employing the concept of epistemic living spaces¹ sensitises to the importance of the "multi-dimensional structures — symbolic, social, intellectual, temporal and material" (Felt, 2009b, p. 19) that are interlinked and interdependent with authorship practices in manifold ways. Illuminating the epistemic living spaces of researchers in terms of their practices of authorship

¹ Section 4.3.3 offers a more detailed reflection as to how the concept shaped the analytical and writing process.

is therefore of utmost relevance in order to uncover the abysses that lie, so to speak, beneath a list of authors and thus, implicitly, beneath the bibliometric indicators. It involves: 1) tracing their relations with each other, 2) getting a feel for their working rhythms, their imaginations of the past and future of academia, 3) exploring the spaces in which they perform their work and the material objects they use in their epistemic quests, and 4) investigating how researchers value the aforementioned actors, objects, and logics and — last but not least — the authorship practices that make up their epistemic living spaces.

Furthermore, the emotional aspects involved must not be overlooked; they are also reflected in the notion of an epistemic *living space*. As Felt (2009b) elucidates, the term ‘living’ particularly underscores the ways in which researchers may be “feeling intellectually and socially ‘at home’, holding an understanding of the often non-codified sets of values which matter, [and to which they might be] feeling subjected to” (p. 19; see also Felt et al., 2013). In this context, it is also important to cite Boccagni and Duyvendak’s description of what it means to feel at home in a public space because the academic world as a workplace for researchers is nevertheless a public space:

People’s ways of making home in the public have also a strong emotional dimension. Attaching a sense of home to some extra-domestic circumstances is a highly selective, emotionalized, and temporally shifting experience. It is often related to particular settings, as well as to meaningful relationships and/or deep-rooted routines to be enacted there. (Boccagni & Duyvendak, 2021, p. 213)

Finally, the concept illuminates the ways in which a researcher’s epistemic living spaces may “both opening up and closing down possible degrees of agency” (Felt, 2009b, p. 19). Alternatively, as Sigl so succinctly formulates it:

Researchers are thus not just subjected to the conditions they are placed in but co-produce worlds of research and the knowledge that our social worlds build on by their attitude toward their environments and the choices they make from their respective standpoints. (Sigl, 2019, p. 118)

How researchers perceive their own agency in forming and moulding their epistemic living spaces, particularly with regard to authorship practices, is also reflected in their visions and practices of responsibility and care, which are discernible both in their narratives and valuations. It is not only the degree of agency that researchers experience but also the multi-

dimensional structures that interact with their visions and practices of responsibility and care, as well as their narrative sense-making attempts and valuations of authorship processes and decisions. Therefore, it can be argued that “[a]uthorship is [not only] a window into the practice of science” (Moffatt, 2018, p. 209), but also a lens through which to view the sense-making processes and valuations surrounding authorship practices. This, in turn, offers insights into the epistemic living spaces of researchers and their identities. The monograph thus contributes to a specific strand of literature in the field of science studies focusing on the subject of researchers’ identities, with the issue of values also representing a significant area of concern.

1.2. Research questions and their formation process

Drawing from the aforementioned observations and research interests, two overarching questions and three sub-questions emerged to guide the empirical analysis:

- *How do researchers make sense of current authorship practices?*
- *How does this reflect their values and their visions of responsibility and care in their respective research environments?*
 - *Sub-question 1: How do researchers make sense of the material, spatial, temporal and social dimensions that shape authorship practices?*
 - *Sub-question 2: How do researchers assign value to contributions and authorship positions, and the relation between them?*
 - *Sub-question 3: How do researchers value the overall authorship decision process?*

I consider it crucial to delineate how I have arrived at those questions and lay out the considerations that went into their formulation. The first question indicates it is researchers, who my research subjects are, and their acts of making sense that are the centre of my work, thereby necessitating the utilisation of qualitative data. The employment of the notion of ‘make sense’ alludes to the conceptualisation of narratives as sense-making devices, thereby indicating a following of both Czarniawska’s (2004) and Felt’s (2017b) work. The term ‘current’ signifies a focus on present-day authorship practices as research objects. Finally, the term

'authorship practices' is employed to convey the idea that any authorship decision constitutes a form of decision-making practice², encompassing decisions regarding the listing of an individual as an author on a scientific publication and, if they are listed, their position on the authorship list. However, the emphasis on researchers' sense-making suggests that the foundation of my data is comprised of instances wherein researchers retrospectively discuss and reflect upon authorship decisions. These decisions may have been made by the researchers, may have been relevant to them due to the inclusion or aspiring inclusion in the list of authors, or may have been conveyed through narratives shared by others.

Without embarking on a comprehensive examination of the extant discourse surrounding the notion of practices, it is pertinent to refer to the seminal conceptualisation of practices by Schatzki (2011). Building upon the extensive body of literature, as well as drawing on the field of Science and Technology Studies (STS), Schatzki (2011) conceives practices "as embodied, materially mediated arrays of human activity centrally organized around shared practical understanding" (p. 11). In my understanding of practices, I primarily adhere to the conception outlined by Ruppert and Scheel (2021), who, in turn, also refer to Schatzki (2011), among others, that "practices are *activities* [...] that are by humans and in relation to various materials and other actors that are part of a field of practices" (p. 33). In doing so, both epistemic living spaces as conceptual foundation and the understanding of practices attune me to the spatio-material, temporal and relational aspects. Part of these relational aspects are how practices are imparted and learned as well. Because I follow a constructivist-oriented grounded theory approach in the analysis, I also appreciate how "practice is an elastic word, which can be stretched or tightened depending on interest and orientation" (Gad & Jensen, 2014, p. 702), allowing me to remain open to emerging issues surrounding authorship practices in the analysis. The aforementioned reflections prompted the formulation of the first sub-question, which considers how researchers make sense of the material, spatial, temporal and social dimensions that shape authorship practices.

Moreover, by being interested in authorship practices, I strive to "attend to the beliefs and

² Correspondingly, in this thesis, the terms 'authorship practices' and 'authorship decisions' are used interchangeably.

values of actors, as well as available material resources and the external environment involved in a particular doing" (Ruppert & Scheel, 2021, p. 34). This assertion underpins the subsequent question, which seeks to articulate the idea that researchers' sense-making of authorship practices is reflective of their values, given the inextricable entanglement of practices and values. Thus, while the first main research question implies that narrative studies play an influential role, the second question indicates the crucial role of valuation studies in my research. In particular, the work of Heinich (2020a, 2020b, 2020c, 2021) and Boltanski and Thévenot (2000, 2021) have shaped my thinking about the acts of valuing, thus prompting me to adopt a pragmatic approach to valuations. However, and I would like to underscore this from the outset, I do not follow any prescribed value order, such as the orders of worth formulated by Boltanski and Thévenot (2000, 2021), despite finding inspiration in many aspects of their work. Accordingly, the second research question is indicative of my sensitivity towards acts of valuing, stemming both from my engagement with valuation studies and the concept of epistemic living spaces, with the epistemic and symbolic dimensions being of particular importance. In my analysis, I identified three dominant strands of valuation: firstly, researchers' valuing of contributions and authorship positions, which are closely interlinked, leading to the second sub-question of posing the question of how researchers assign value to contributions and authorship positions, and the relation between them. The third line of valuations pertains to the overall decision process, which corresponds to the third sub-question, which inquires into how researchers value the overall authorship decision process.

In incorporating the reference to researchers' 'visions of responsibility and care', the objective is to communicate how an investigation of authorship practices cannot and also should not evade the exploration of the individuals responsible for them or those envisioned to be responsible for making authorship decisions. Hence, it touches on assumptions and attributions of responsibility, which this thesis endeavours to uncover. Additionally, the assumption of responsibility entails a certain degree of involvement, specifically the act of caring to make 'good' authorship decisions. And as Mol et al. (2010) emphasise, "good is not something to pass a judgement on, in general terms and from the outside, but something to do, in practice, as care goes on" (p. 13). Accordingly, I am interested in who researchers consider to be responsible for caring for 'good' authorship practices, what they imagine care practices surrounding scientific authorship to be, and how they speak about care practices

they have experienced or heard of in the context of authorship. All these aspects are subsumed under the expression of 'visions of responsibility and care', with 'vision' including both how past practices were experienced, what improvements to those practices may be brought up, and what their ideal looks like. Yet, as researchers typically do not utilise the term 'care practices', an open approach in the analysis is essential to trace their reflections on responsibility and care.

Finally, the 'respective research environments' denotes the frame of references researchers employ in their processes of sense-making and valuing. Said frames encompass any individuals with whom researchers are or argue to be on a scientific publication. Thus, it can include individuals from their research group, their institution, but also other organisations, as well as researchers from the same or other subdomains and even other disciplines. In addition, the researchers may be surrounded by a variety of individuals, including bachelor's or master's students, technicians, as well as other PhD students, postdocs, or group leaders, to name the most relevant actors. Furthermore, the 'respective research environments' also signify the institutional structures in which they are embedded, which I have deliberately formulated to be relatively open, with the objective being to discern the predominant reference points among researchers. I am therefore interested in their "wider imaginations about the environment in which one lives, how that environment functions, what is valued in it, and actors' place and agency in a particular environment" (Felt, 2017a, p. 134).

1.3. Outline of the thesis

The thesis embarks with the exploration of researchers' environment, following the establishment of the topic's relevance in *Chapter 1*, which also encompasses the elaboration of the conceptual foundation and the explication of the research questions. Subsequently, *Chapter 2* illuminates contemporary authorship practices through the concept of epistemic living spaces. The concept of epistemic living spaces, which comprises five dimensions (the epistemic, symbolic, spatio-material, and temporal dimensions, as well as the social dimension), furthermore provides the structure for this chapter, enabling an extensive range of perspectives subject of authorship to be considered. These initial two chapters fall under the purview of PART I SETTING THE WIDER FRAME OF AUTHORSHIP IN CONTEMPORARY

ACADEMIA.

PART II CONCEPTS, DATA, AND METHODOLOGY begins in *Chapter 3* with the cross-cutting sensitising concepts guiding and sharpening my analysis. In addition to the conceptualisation of narratives as sense-making devices, drawing upon the work of Czarniawska (2004) and Felt (2017b), I elucidate the reasons for adopting a pragmatic approach to valuation and how narratives and valuations are intertwined. While Boltanski and Thévenot (2000, 2021) provide significant reference points, the work by Heinich (2020a, 2020b, 2020c, 2021) and others, who do not espouse a predetermined values and potential order, resonated more profoundly with my own standpoint. The third chapter concludes with an examination of how, drawing upon conceptualisations in the field of STS literature (such as for example Heuts and Mol (2013); Martin et al. (2015); Mol and Hardon (2021)), the intertwined concepts of responsibility and care matter in researchers' valuations.

Chapter 4 describes my engagement with the qualitative and diverse data corpus analysed. Following a detailed description of the data sets, the chapter puts forward a case for the methodological approach based in constructivist oriented grounded theory. Thereafter, a description of the process is given, along with a reflection on how the concept of epistemic living spaces shaped both the analytical and writing process. The chapter ends with a reflection on comparing data, given how the data corpus consists of diverse data sets, and how comparisons were encountered within the data.

PART III A COMPREHENSIVE INVESTIGATION OF AUTHORSHIP VALUATIONS indicates the beginning of the empirical section of the dissertation, thus the core of it, which necessitates its own introduction and outline in *Chapter 5*. Here, one can find a graphic, depicting the three valuation strands and the identified values and criteria. The chapter reaches its conclusion with two reflections on how two observed phenomena in researchers' valuations matter, namely the interconnectedness of valuations and how researchers constitute a situated normality through comparative, valuative acts.

The centre of *Chapter 6* is the exploration of how researchers value contributions, indicating whether they perceive such contributions as legitimate and worth of authorship or not. The

identified values are *work*, *knowledge gain*, and *publication* with *duration*, *proficiency* and *connection* as valuation criteria. In the following discussion, aspects constituting researchers' situated normalities are considered, and it delves into their impact on the valuation of contributions, alongside its investigation of valuations pertaining to specific contributions. Finally, the chapter outlines researchers' stances on the matter of considering a researcher's position in academia in authorship practices and the varying significance of authorship throughout different stages of a researcher's career.

Chapter 7 then investigates how researchers value authorship positions, given that the ranking of authors is crucial in the majority of research areas in the life sciences and physics. The guiding values are *credit*, *recognition* and *capital*. Researchers oscillate between indifference and an almost all-consuming care for specific positions, which are reviewed individually. The chapter ends with reflections on justifications researchers voice in talking about authorship rankings and how their prioritisations are discernible.

Chapter 8 constitutes the final chapter of the empirical section, exploring how researchers make sense of and value both authorship decisions and the underlying processes involved. The chapter thus addresses more overarching issues, beginning with a final reflection on how researchers' situated normalities matter, following the introduction of *clarity* and *fairness* as transcending values. Next, the chapter explores the taboo-like status of authorship, which is also reflected in researchers' repeated attempts to balance their careers and self-conceptualisations against epistemic values and the neoliberal logic pervading academia. Researchers' care practices surrounding authorship decisions are, in turn, shaped by these very imaginations and experiences of a career in science. The chapter and the empirical part conclude with reflections on how the multiplicity of authorship, which has been revealed, matters for researchers' works and lives in science. It also considers this against the backdrop of the metrification of research(ers), and how they come to terms with this multiplicity.

Finally, *Chapter 9* of PART IV FURTHER REFLECTIONS AND FINAL REMARKS, offers a concise consolidation of the five primary, intertwined findings that have been identified and provides several implications. I contend that researchers' valuations of authorship practices are shaped by two interwoven elements: their situated normalities and their identities. Both elements are

subject to constant flux, with the latter being particularly dynamic. This renders the judgement of the fairness of authorship practices multiple, shifting and situational.

2. The meaning and significance of contemporary authorship practises: An exploration through the concept of researchers' epistemic living spaces

To fully grasp the meaning and significance of authorship in contemporary science, it is essential to begin by describing the settings in which authorship decisions are made, which means illuminating researchers' epistemic living spaces. Such a description can be realised by adopting the concept of the epistemic living space as a principle for classifying, but also for selecting, the already existing works needed to prepare the reader for the topics relevant to their understanding of the empirical chapters. Moreover, as Felt and Fochler (2012) point out, the concept of epistemic living spaces performs analytical functions even when used as a tool for ordering. Consequently, the process of writing this chapter, which followed the drafting of the empirical section, has yielded further insights for the refinement of that latter section. This chapter is therefore closely aligned with the empirical findings of the thesis. As a result, this chapter does not purport to be a comprehensive account of the current work and framework conditions of researchers in the contemporary life and physical sciences. Instead, it aims to alert the reader to "the structures, contexts, rationales, actors and values" (Felt & Fochler, 2012, p. 136) which researchers bring to the fore in stories about authorship.

Moreover, because the analysis of the empirical data and the general approach to the research topic draws upon the body of work produced within the field of STS, works from this field provide the theoretical basis for this dissertation. The literature on authorship, integral as it is to STS, encompasses a range of perspectives, including an emphasis on ethical considerations in journals such as *Research Ethics* or *Science and Engineering Ethics* (see, for instance, Bülow & Helgesson, 2018; Helgesson et al., 2021). It also covers discussions of recent trends or phenomena concerning authorship, such as hyperauthorship, which is defined as the publication of works by 1.000 or more authors, often situated within the context of opinion pieces in journals like *Nature* or *Science* (see, for example, Chawla, 2019; Ioannidis et al., 2018). Furthermore, authorship can be investigated through the analysis of quantitative data sets, enabling studies such as the identification of networks through the examination of

author lists in journals like *Scientometrics* (see, for example, Wang et al., 2021). Additionally, authorship can be explored in the context of the pervasive neoliberal logic in academia, characterised by its emphasis on the continuous assessment of research and researchers, which is often presented in a classic STS journal, such as *Minerva* (Lekka-Kowalik, 2021).

Thus, following from the way Felt (2009b) describes five dimensions making up an epistemic living space, this chapter is structured along the epistemic, symbolic, spatio-material, and temporal dimensions, as well as the social dimension. The objective is to approach the topic of authorship practices with the comprehensive scope warranted by its complexity.

2.1. How the epistemic dimension matters

First and foremost, scientific authorship is widely considered and claimed to be an epistemic issue, with a list of authors being an integral part of virtually every scientific publication in a scholarly journal. In other words, the list of authors at the beginning of a scientific article represents the individuals who have contributed to the production of scientific knowledge. But authorship is not quite as simple as that, as numerous discussions in a variety of fields have shown. It can touch on issues of epistemic credit, career advancement, responsibility and accountability, and even draw in epistemic uncertainty. An editorial in a medical journal even declares that “[w]hat constitutes authorship is one of the most vexing problems in contemporary scholarly publishing” (Kharasch et al., 2021, p. 1).

2.1.1. *Authorship as a form of epistemic credit for contributions*

An intriguing entry point for investigating the subject of scientific authorship is provided by Moffat (2018), who characterises authorship as “a vehicle for the allocation of professional credit and blame in research contexts” (p. 199). In doing so, he alludes to the dual meaning, or indeed function, of authorship, which is one crucial aspect of the matter.

Moreover, it draws attention to the frequently used term ‘credit’ in the context of authorship, such as Helgesson, Master and Bülow (2021) writing of how “authorship is a matter of due scientific credit and fairness” (p. 27). The notion of credit is also a recurring theme in the context of guidelines or regulations attempting to govern scientific authorship. In the

recommendations of the International Committee of Medical Journal Editors (n.d.), which are widely used in biomedical journals as well as in a wide range of journals in the social sciences (see Chang, 2019), the first sentence in the section “Why Authorship Matters” begins as follows: “Authorship confers credit” (n.a.).

The question that inevitably arises is for what is credit given in the form of authorship? Simply put, credit is given for contributions to a scientific publication. However, not all kinds of contributions are rewarded with authorship. Guidelines therefore often further define these contributions by adding the words ‘substantial’ or ‘significant’. The University of Vienna (2006), for example, states in its guideline on how to ensure good academic practice that “anyone who has significantly contributed” (§ 2 (1)) should be listed as an author, while also giving an example of the possible types of contributions. How the epistemic dimension plays a key role in the context of authorship in research is exemplified in the Guidelines for Good Scientific Practice of the Austrian Agency for Research Integrity (2019), according to which “an independent scientific/scholarly contribution or another major contribution” (p. 9) should be acknowledged with authorship. Similarly, a representative, interdisciplinary study of authorship practices in Germany finds that “the writing of the text, the interpretation and analysis of data establish secure entitlements to authorship” (Hesselmann et al., 2021, p. 388), prompting the hypothesis that the entitlement is primarily derived from the epistemic quality of the contributions in question. In contrast, a contribution lacking an epistemic component, such as the “mere technical cooperation in data collection or the provision of funding and infrastructure to enable research does not justify co-authorship” (Austrian Agency for Research Integrity, 2019, p. 9). The Contributor Roles Taxonomy, with the apt acronym CRediT, also attempts to further clarify the meaning of contribution and, implicitly, credit, by outlining 14 different roles that individuals who contribute to a piece of scientific work may take on and which journals may require authors to declare when submitting a manuscript for publication (National Information Standards Organization, n.a.). For example, in 2009 Nature published a statement on how they had changed their policy from “‘strongly encouraging’ such [author-contribution] statements [to] we now require them for publication of original research papers in *Nature* and the *Nature* research journals” (Nature, 2009, p. 1078). Moreover, such initiatives can be regarded as a means of counteracting the phenomenon of bibliometrics indicators, which are “abstracting away from the process of work being done”

(Star & Strauss, 1999, p. 22) by providing a more nuanced insight into researchers' contributions.

However, the commonly observed lack of clarity, or more precisely, the vagueness of the guidelines, — for example regarding what constitutes a substantial or significant contribution — may be viewed and critiqued in a negative light (Helgesson et al., 2021). In the words of Penders and Shaw (2020), “many of the written and unwritten rules governing the distribution of credit and other resources in academia reinforce a long series of inequalities” (p. 363). The implications of such vagueness also manifest themselves in the findings presented in the empirical section of the thesis. Hence, the utility and prevalence of these guidelines in the everyday practice of researchers may be limited (Helgesson & Eriksson, 2019; Hosseini et al., 2020; Penders, 2017a). The resulting concern among researchers is evident in an opinion piece, in which a range of researchers propose recommendations to enhance “authorship transparency” (McNutt et al., 2018, p. 2557). The limited relevance of such guidelines is also exemplified by the phenomenon of hyperprolific authors, which “might also reflect idiosyncratic field norms, to say the least” (Ioannidis et al., 2018, p. 169). In their study of hyperprolific authors, Ioannidis and colleagues (2018) suggest that the “[l]oose definitions of authorship, and an unfortunate tendency to reduce assessments to counting papers, muddy how credit is assigned” (p. 169). Biagioli (2003) postulates that these various regulations, which seek to define authorship by outlining which types of contributions should be translated into “authorial credit” (p. 259), illustrate how “the notions of credit and attribution of authorship are not only fuzzy, but their fuzziness are codependent” (Biagioli, 2003, p. 254). Thus, he argues, “scientific authorship is not about rights, but about rewards” (Biagioli, 2003, p. 259).

The concepts of authorship and credit, as well as the concept of contribution, may be approached from a practical standpoint, in the form of guidelines, or from a theoretical perspective, as for example Biagioli (2003) does. What both approaches have in common is dealing with the subject from a distant standpoint. By contrast, this thesis seeks to examine the conceptualisations of those engaged routinely in the process of attributing authorship and credit, namely researchers. Putting researchers' understandings at the centre is critical because authorship is usually a “highly subjective decision, which is negotiated within

research teams” (Lissoni & Montobbio, 2015, p. 25), although disciplinary traditions naturally have an influence. Hence, this work aims to investigate the perceptions of researchers regarding what contributions they believe should be rewarded and what type of compensation in the form of a specific position on the list of authors they deem appropriate. Mentioning the subject of the authorship positions introduces another crucial aspect, warranting further discussion.

2.1.2. The interplay of authorship positions, contributions, and career stages

Authors may be ordered in a particular pattern to indicate various types of contributions and thus give differentiated credit. In the life sciences and physics, one can identify a “U-shaped structure in the relationship between author’s order and contribution — in which the most important authors of a paper are the first and last authors” (Larivière et al., 2016, p. 427). One countermeasure against this focus on only two out of several authors on a publication is a shared authorship position. Theoretically, any authorship position — first, middle, and last — could be taken to indicate equal authorship, in practice equal co-first authorship is most common and on the rise (Hosseini & Bruton, 2020).

As Smith et al. (2020b) so succinctly put it: “Ordering often requires comparing and contrasting the value of different individual contributions, giving more or less importance to various criteria such as the type of contribution (technical, intellectual) or the role in the project (supervisor, student)” (p. 1969). The empirical section of this thesis carefully traces those moments in researchers’ narratives in which they describe, reflect on, and value these processes of ordering, which are themselves processes of valuation. Furthermore, specific emotional and experiential responses may be elicited by particular positions, as illustrated by a piece in *Nature*. In this piece, several researchers were invited to share their insights as a first-time lead author on a *Nature* publication (Woolston et al., 2019), where, for example, they talked about “[p]assion gives you stamina” (p. 499) needed for such an achievement or how they “were pretty cynical about science” (p. 500) before the publication.

Because the data analysed for this dissertation is drawn from researchers in the life sciences and physics, the most common form of author ordering is based on the contributions made. Nevertheless, the data also includes researchers working in subfields with different ordering

practices — like, for example, in a theoretical subfield of physics, where an alphabetical list of authors is the norm. Alphabetical ordering is also a common practice in the social sciences (Lissoni & Montobbio, 2015). A slight variation of alphabetical ordering occurs when a researcher who has made significantly more valuable contributions than the other contributing researchers is listed as the first author, regardless of the first letter of their surname. In any case, the distribution of authorship positions based on contribution, which is the norm for laboratory-based fields, tends to place the first and last authors in the foreground (Sundling, 2017). The authors in the middle can be ordered alphabetically. Mongeon, Smith, Joyal and Larivière (2017) report an increasing frequency of a partial alphabetical order in the middle of an author list in biomedical research.

In numerous subdomains of the life sciences and physics, with the exception of such fields as high-energy physics or nuclear physics, the position of first author is usually granted to a junior researcher — either a PhD student or a postdoctoral researcher — who has made the greatest contribution (Braun-Munzinger, 2009; Waltman, 2012). This individual is usually involved in a range of activities, including the design and execution of experiments, the analysis of data, and the process of writing the article (Robinson-Garcia et al., 2020). In turn, the last author is often the senior researcher who supervises the PhD student or leads the group of which the PhD student or postdoc is a member. Additionally, this last author frequently assumes the role of principal investigator, bearing the responsibility of developing the conceptual framework of the publication or project from which the article arises and for obtaining grants to fund the laboratory and its infrastructure (Lissoni & Montobbio, 2015; Mongeon et al., 2017; Müller & de Rijcke, 2017). In line with this execution of different tasks depending on the career stage, an observation made by Garforth and Červinková (2009) concerning bioscientists is that “the processual aspect of becoming a legitimate and coherent epistemic subject was often framed in terms of learning craft and technical skills, rather than in relation to intellectual development” (p. 187). As they elaborate, this implies “that ownership of projects and research independence was deferred until later in the career — lab leaders set projects for junior researchers, who were not allowed to publish independently” (p. 187). Nevertheless, how both senior and junior researchers appraise this widespread practice across numerous fields of the life sciences and physics is investigated in the empirical section of this thesis. In contrast, the contribution of the authors in the middle is less clear; Robinson-Garcia and

colleagues (2020) contend that, based on their quantitative analysis of contribution statements, middle authors typically declare contributions in the form of resources such as reagents, materials, or analysis tools. However, as Sauermann and Haeussler (2017) establish in their study, contribution statements neither “inform about an author’s level of involvement in particular contributions [...nor provide detailed] information on the importance of different contributions to project success” (p. 8), resulting in disparate rankings. As this demonstrates, it is through the direct questioning of researchers that such nuances and insights can be elucidated.

Finally, the role of ‘corresponding author’ does not necessarily correspond to a specific position of authorship within an authors’ list. Rather, it identifies a specific role of responsibility. However, based on the available data, it has been observed that either the first or last author tends to be the corresponding author (Chinchilla-Rodríguez et al., 2024). Historically, assuming the role of corresponding author entailed managing communication (i.e., correspondence) with the journal and its editors on behalf of the entire author team. Nowadays, there may be multiple corresponding authors for a given publication, a phenomenon that can be partially attributed to the growth in (interdisciplinary) collaborations. Corresponding authorship also frequently appears to indicate a more senior status and possibly a leadership position and is typically associated with being the first point of contact in the event of any ethical concerns that may arise (Willems & Plume, 2021). However, as Willems and Plume (2021) also note, “the process of selecting a corresponding author is layered with assumptions and nuances” (p. 12), which give rise to a variety of differing authorship practices.

2.1.3. Authorship carries responsibility and accountability

As indicated at the outset of this chapter, the concept of credit can be considered to be one aspect of a dichotomy that constitutes authorship. The other aspect is that of responsibility and accountability. An individual receives not only credit but also assumes responsibility, either for the entirety of the work or, at the very least, for their own contribution, by being listed as an author on a scientific publication. This duality is also reflected in virtually all regulations and guidelines. For example, the sentence in the recommendations from the International Committee of Medical Journal Editors (n.d.) that follows the quotation

concerning credit at the beginning of Section 2.1.1 is as follows: “Authorship also implies responsibility and accountability for published work” (n.a). Hesselmann and colleagues (2021) put even forth the thesis “that publisher policies place disproportionate emphasis on the authors’ responsibility for review, scrutiny and accountability, [as] the main purpose of declaring authorship policies seems to be legal and reputational protection of publishers in cases of quality deficits or conflicts” (p. 388).

However, the substantial corpus of literature pertaining to scientific misconduct tends to address authorship in a somewhat tangential manner, focusing predominantly on the subject of scientific publications and their content. Furthermore, such issues are also addressed in editorials or opinion pieces (Ioannidis, 2018) or result in the publication of a manifesto or statement, as exemplified by the ‘*The Bonn PRINTEGER Statement*’ (Forsberg et al., 2018), demonstrating their status as a topic of concern that extends beyond the realm of pure research.

Research with this focus often seems to be motivated by concerns about how particular publication practices might affect the advancement of science (Collyer, 2019). In this context, researchers may engage in a critical examination of the phenomenon of the “proliferation of publication” (Hermanowicz, 2016, p. 174) or utilise terms such as ‘salami slicing’, to describe the practice of ‘splitting’ up findings in as many publications as possible. Or this body of literature may also look into the rise of so-called predatory journals, which do not conduct (diligent) peer review or any other kind of proper editorial practice (Bagues et al., 2019) .

In regards to the content of scientific work, this concern with misconduct tends to concentrate on researchers’ practices of manipulating graphs, fabricating or embellishing data, and other similar activities (Frisch et al., 2022; Vermeulen & Hartmann, 2015). In some cases, these practices may ultimately result in the retraction of articles, with initiatives such *Retraction Watch* facilitating greater traceability (see Oransky, 2020), or at least in an online discussion among scientists on websites such as *PubPeer* (Hesselmann et al., 2017; Singh Chawla, 2024; Walsh et al., 2019). Thus, this strand of literature provides insights not only into researchers’ practices of academic misconduct, but also into practices “termed ‘academic misrepresentation’ — i.e. new professional practices that many academics would see as

unethical but which have not as yet been (and indeed may never be) categorized as misconduct” (Biagioli et al., 2019, p. 401). It is notable that researchers themselves may be in doubt about the integrity and quality of their research practices (Schickore & Hangel, 2019).

Moreover, these works deal with the question of what structural conditions might push researchers into such practices, which are often presented as irresponsible (Sarewitz, 2016). For example, the prospect of financial remuneration in countries such as China for individual researchers to publish appears to have a corrupting influence on the scientific rigour of their work (Fanelli et al., 2015). It is also of interest how researchers who are not involved in such practices, but who are listed as authors on such a publication, can either overlook or be unaware of such practices. For example, Walsh et al. (2019) investigate the relationship between the extent of labour division and the likelihood of retractions, concluding that the greater the former, the more likely the latter.

This body of literature tends to view instances of irresponsible authorship practices as less problematic for the scientific system and the advancement of knowledge than irresponsible practices concerning the content and the act of publishing. That point has also been made by Smith and colleagues (2020b), who highlight the potential uninterest in this issue, despite the fact that authorship practices may have “broader ramifications for science, authorship disagreements may be dismissed as peripheral incidents of self-aggrandizement that have little impact on the advancement of science” (p. 1968). The potential implications of authorship decisions on researchers’ careers can be far-reaching, as has been previously outlined. Indeed, these implications may even constitute a significant factor dissuading individuals from pursuing a career in science. Furthermore, one might inquire as to how researchers who engage in irresponsible authorship practices, from which their careers benefit, act in other areas of their work.

The existing body of work on the responsibility of authorship practices primarily addresses the ethical issues surrounding the addition of a researcher to a list of authors for reasons that may be considered unethical. The terminology used to describe these practices is diverse. Indeed, Bülow and Helgesson (2018) contend that the field is characterised by a “terminological cacophony” (p. 3). The literature repeatedly employs the following terms:

guest, honorary authorship, and ghost authorship.

The former two terms, i.e. guest and honorary authorship, as well as the notions of gift or coercive authorship, are applied whenever authors' lists are complemented with a researcher who has either not contributed at all or not contributed substantially enough to a scientific paper to warrant inclusion (Bülow & Helgesson, 2018). As the notion of coercive authorship denotes, this inclusion is not necessarily a voluntary act on the part of (all of) the authors (Smith et al., 2020a). An imbalance in power between the individual claiming authorship and the others may facilitate such practices, as Goddixsen et al. (2023) emphasise, thereby underscoring the importance of considering the social dimension in the exploration of authorship practices. Moreover, the epistemic dimension may also inform considerations regarding the handling of what Bülow and Helgesson (2018) term "hostage authorship" (p. 1); they argue that "in hostage-like situations it may nevertheless be the right thing to do to accept a hostage authorship in order to be able to carry out important research, even though it will leave you with dirty hands" (p. 8). Tang (2018) contests the argument, asserting that it has the potential to "perpetuate a dark research culture" (p. 1) and may have particularly adverse effects on early career researchers. Similarly, Satalkar and colleagues (2020) underscore how the responsibility cannot be shouldered by any individual alone to take a stand, for instance, against practices such as hostage authorship.

In contrast, the term ghost authorship describes the exclusion of an individual who has, according to many guidelines regulating authorship, made a significant contribution to the publication and would therefore be a legitimate candidate for authorship. To clarify, the notion of an 'individual' is used in this context deliberately, because the status of the contributing person may be a factor in this eradication. It is inevitable that the well-known text by Shapin (1989) about the so-called invisible technician will come to mind in this context (see also Star & Strauss, 1999). Plantin (2019) examines this issue by investigating the role of data processors in social sciences, to give another example. However, as Wylie (2016) demonstrates in the case of fossil preparators who are omitted from authors' lists, they may perceive themselves as "empowered craft workers" (p. 89) who are confident in their identity as belonging to a different field of work than that of researchers. Junior researchers are similarly susceptible to being omitted from authorship lists. Larivière et al. (2020) as well as

Lissoni and Montobbio (2015) have highlighted the risk of junior researchers being excluded from authors' lists.

This thesis diverges from those works primarily concerned with 'irresponsible' authorship practices in that it focuses on any narratives told by researchers that centre around authorship decisions. Consequently, irrespective of whether the narrator or the guidelines in question would categorise a practice as irresponsible or not, accounts of authorship decisions are included in order to gain a more comprehensive understanding of the issue. Notably, authorship practices negatively framed in guidelines may not be perceived as problematic by researchers (Helgesson & Eriksson, 2019; Jabbehdari & Walsh, 2017). Furthermore, this approach enables the identification of researchers' efforts to enhance authorship practices through their care practices. Lastly, the inclusion of positive examples of authorship processes draws attention to aspects of academia that researchers may perceive as fulfilling or satisfactory. An example of this could be the communication of the findings of their research in the format of a scientific publication to the wider audience of their peers.

2.1.4. The entanglement of epistemic uncertainty and authorship

The process preceding the publication of an article may evoke feelings of insecurity, to varying degrees, among researchers, stemming from the inherent uncertainty associated with the scientific process, particularly within the domain of basic science. Uncertainty, as an inherent feature of science, has traditionally been the focus of researchers belonging to the first wave of laboratory studies in STS (Knorr-Cetina, 2002; Knorr-Cetina, 1981; Latour & Woolgar, 1986). In the context of authorship, "the complex entanglements of uncertainties related to epistemic processes and uncertainties related to funding and careers" (Fochler & Sigl, 2018, p. 353) may play a role at moments when researchers cannot (yet) estimate whether there will be a publication at all, and if so, whether they will be listed as authors or not, and possibly in what position on the list of authors (see also Sigl, 2016). In this context, the value surrounding so-called negative results, which are not included in papers, is frequently discussed (Fanelli, 2012, 2020). The exclusion of negative results from a published study is not only unfavourable for the researcher responsible but "omitting negative results might be problematic from a research ethics perspective, especially if the reason for omitting them is that they contradict one's hypothesis" (Helgesson et al., 2021, p. 27). Furthermore, the risk that one's own

contribution is no longer included in a publication may also increase as work is increasingly distributed and thus becomes more fragmented. In such circumstances, researchers may feel the “increased individual anxiety in coping with uncertainty” (p. 353) observed by Fochler and Sigl (2018) in their work, which also stems from unclear career prospects at the level of early career researchers (see also Nästesjö, 2021). One potential strategy for mitigating this anxiety is to initiate discussions at the outset of a research project regarding the distribution of authorship (Youtie & Bozeman, 2016). This issue exemplifies one instance of a nexus between the epistemic and temporal dimensions. Since authorship has multiple ramifications in researchers’ epistemic living spaces, the detailed analysis of researchers’ accounts of authorship consequently sheds light on issues such as how researchers wish to and do deal with epistemic uncertainty in contemporary academia. Yet, since the different dimensions comprising an epistemic living space are closely interwoven, researchers’ concerns about dealing with epistemic uncertainty also stem from the prevalence of the symbolic dimension, which is explored in the following subchapter.

2.2. How the symbolic dimension matters

The symbolic dimension “touches on values and modes of ordering” (Felt & Fochler, 2012, p. 137), and is therefore of paramount importance because this thesis aims to shed light on researchers’ valuations of authorship decisions. In their narrative valuations³, researchers provide insight not only into their own values and value orders, but also into those of the research system in which they are embedded and which “implement[s] a core logic” (Felt, 2015, p. 5). The dominant logic in contemporary academia is often characterised as new public management, or alternatively, a neoliberal logic. It pervades the scientific system through its emphasis on constant quantification and assessment of research and researchers and it undoubtedly constitutes one of the principal “structuring forces” (Felt et al., 2013, p. 513), which the concept of epistemic living spaces facilitates in examining.

³ By the term ‘narrative valuation’ I want to convey that researchers frequently engage in valuations in the course of their narratives. See also Chapter 3 and in particular Subchapter 3.1 and 3.2 on the intertwined nature of narrating and valuing.

2.2.1. The quantification of research and researchers

In line with this symbolic rationality is the assumed “convertibility” (Knorr-Cetina, 1981, p. 98), i.e. the transformation of the publication and one’s position on the list of authors into a “currency” (Knorr-Cetina, 1981, p. 98) for the advancement of the researcher in a highly competitive environment (Naidoo, 2016). Further, Latour and Woolgar (1986) demonstrated how, as in my case, authorship “credit has all the character of a currency” (p. 192) in their chapter on the cycles of credit. Accumulating currency, then, enhances researchers’ standing within their scientific community, which in turn increases their likelihood of being granted awards and funding, and so on. Currency may be first-author publications in high-impact journals, which are widely treated as an indicator of scientific excellence for more junior researchers. Conversely, as a researcher’s scientific career progresses, the significance of last authorship increases because it signals an individual’s ability to successfully conceive, and fund research projects grounded in innovative ideas, competently manage subordinates and collaboration with others.

Researchers’ epistemic living spaces cannot evade the so-called “multiverse of quantification” (p. 55), an expression coined by Di Fiore and colleagues (2023), which has infiltrated not only the private but also the public sector over the last few decades, with its accompanying ongoing assessments (Dahler-Larsen, 2012). The process of academic metrification was already underway in the United Kingdom and the Netherlands by the late 1970s and early 1980s and has continued to gain momentum subsequently (Fochler & de Rijcke, 2017). Brighenti (2018) even asserts that “the production of measures and related preoccupations turns into an increasingly larger share of their work” (p. 31) for researchers.

Alongside this multiverse, bibliometric indicators, such as the h-index and the Journal Impact Factor, can be conceived of as “judgment devices” (Hammarfelt & Rushforth, 2017, p. 170), which, for example, grant reviewers may turn to when assessing a multitude of suitable applications. Alternatively, the metrics inherent to contemporary academia can be conceptualised as “storying processes that contribute to collective modes of making sense of” (Gabrys et al., 2016, p. 2) the productivity of a researcher and their recognition within their scientific community. One prominent line of research within STS is the investigation of the manner in which said indicators are utilised in the context of assessment procedures

(Hammarfelt, 2017; Kaltenbrunner & de Rijcke, 2017, 2019; Lamont, 2009). Interestingly, as Reymert (2021) argues based on her analysis of evaluations of applicants for a professorship, “metrics foremost were applied as a screening tool to limit the number of eligible candidates rather than replacements of traditional peer review” (p. 73). This echoes Ma’s (2021) consideration of “metrics as time-saving devices” (p. 127), which can have detrimental effects in various assessment contexts but also in terms of exploring new scholarly literature.

However, as Fochler (2016) notes, the internationalisation of indicators by researchers confers upon them an even greater degree of influence than is the case with their utilisation in external contexts. In a similar vein, Jarke and Macgilchrist (2021) assert that “dashboards do not merely represent reality but instead reconfigure practices, relations and priorities” (p. 3). Correspondingly, one could argue that this is also true of Google Scholar profiles or author profiles in the Web of Science. Or, to quote an engineer interviewed by Wajcman (2019): “[W]e definitely think in terms of the tools we use” (p. 324). These observations often reverberate in prominent critiques of the neoliberal logic in today’s science system.

2.2.2. Lines of critique of the neoliberal logic pervading science

A common criticism levelled in both scientific articles and opinion pieces is the deleterious impact of research activities and, consequently, researchers being quantified as a result of the neoliberalisation of academia (Burrows, 2012; Lekka-Kowalik, 2021). Significant contributions to investigating the influence of metrics on knowledge generation are made by researchers working in STS or adjacent research areas (Falkenberg & Fochler, 2022; McCulloch, 2017). For example, “‘do-able’ research problems” (Fujimura, 1987, p. 257) may be regarded as more attractive (Rushforth et al., 2019). Or, to quote Paul-Hus and colleagues (2017), “[t]he current reward system of science seems to have little forgiveness for subject niches, new pursuits, innovative methods, autonomy of judgement, dead ends, or mistakes” (p. 483).

A further fundamental premise of this critique is that a narrow quantitative assessment of research does not fully encompass the diverse range of activities undertaken by researchers (Editorial, 2022; Watermeyer & Olssen, 2016; Wouters, 2020). This is particularly evident in the disregard of care practices, whether towards colleagues, students, research subjects, or laboratory equipment, in such assessments (Carrigan & Wylie, 2023; Davies & Horst, 2015).

Another prevalent viewpoint and critique is that the “continuous assessments are notoriously anxiety provoking and involve intense identity work as audit, accountability, monitoring and surveillance all come together to inform potential career advancements” (Knights & Clarke, 2014, p. 351; see also Edlund & Lammi, 2022).

Relatedly, one further potential drawback of a neoliberal approach, as postulated by Nicholls, Henry and Dennis (2021), is that “structural inequalities can become normalized and understood as personal deficits” (p. 71). This is similarly evident in any debates concerning the desirable or even essential virtues and traits of researchers, a discourse in which the symbolic dimension is of paramount importance. In such discussions, researchers’ passion for their work is a recurring theme (Ursin et al., 2020). Interestingly, the expectation that researchers should be passionate about their work serves a dual purpose. While one potential effect may be a high quality — or, to invoke the prominent notion of excellence, an excellent quality — of their scientific work, the other may be an increase in researchers’ well-being and general satisfaction with their work (Busso & Rivetti, 2014). Thus, researchers’ valuations of authorship practices are relevant because they can potentially decrease or increase both the quality of their work and their satisfaction. However, as Busso and Rivetti (2014) emphasise, “passion is often used as a dispositif for exploitation or, more specifically, as an immaterial reward, which often replaces economic earnings” (p. 10) or, in the context of this thesis, being named as an author or being placed in a certain position.

Furthermore, it is noteworthy that, in the past, researchers may have assumed a more positive stance towards the introduction of indicators. Müller (2021) presents findings based on interview data with senior life scientists who recount that at the beginning of their career “they argued for the use of performance metrics for hiring and promotion procedures in order to break up feudal and nepotist structures at universities” (p. 132). However, a crucial shift has occurred, namely “metrics have been reified, in the sense that they are used as nominal values rather than measures” (Ma, 2021, p. 127).

Yet, there is less active resistance than one would assume, as Stengers notes:

The relative passivity of the academic world in facing the ranking systems and “objective”

productivity comparisons that are reshaping academic life radically is sufficient to demonstrate how simple it is, even for people who are not naive or easily impressed or over-powered, to submit to questions that are not only irrelevant but that indeed sound the death knell for all that matters most to them. (Stengers, 2011, p. 58)

A number of STS researchers, or those from adjacent fields, might also espouse a more activist stance, proposing alternative approaches to the assessment of research activities, exemplified by the Leiden Manifesto (Hicks et al., 2015). These works may be attributed to the discourse on Responsible Research Assessment, which also addresses policymakers. Notable initiatives also include DORA (2013), the San Francisco Declaration on Research Assessment, the Coalition for Advancing Research Assessment (2022) with their agreement on reforming research assessment, the Hong Kong Principles for assessing researchers (Moher et al., 2020), and the report *The Metric Tide* (Wilsdon et al., 2015). One illustrative example on a more micro-level can be found in a self-declared feminist marine science laboratory, which established a strategy for handling the ranking of authors. The strategy “emphasizes process and equity rather than system and equality” (Liboiron et al., 2017, p. 3) and consciously assigns and values care work.

One form of resistance may be the practices of researchers that are often grouped under the concept of ‘gaming the metrics’. The expression can be applied to a range of practices that may be considered to border on misconduct, as well as to behaviours that are clearly irresponsible, but which nevertheless warrant consideration within a broader context (Biagioli & Lippman, 2020; de Rijcke & Stöckelová, 2020). However, the term can also be interpreted in a more nuanced manner, as a form of playful and ironic engagement with metrics (for an overview of the notions of ‘playing’ and ‘gaming’ in the context of metrics, see Wallenburg et al., 2018). For instance, Fochler and de Rijcke (2017) write about playing “what we refer to colloquially here as the ‘indicator game’” (p. 21). In this context, a noteworthy observation made by Mongeon and colleagues (2017) is that the majority “of the currently used bibliometric indicators (e.g. the H-index) do not take into account one’s position on the byline. Consequently, being a middle author may be paradoxically more rewarding, from a cost-benefit perspective, than being a primary or supervisory author” (p. 12). Thus, a researcher aiming to be an author on as many papers as possible with as little effort as possible may be gaming the metrics, to give one example. Honorary authorship, whereby a researcher’s name

is included in a scientific publication due to their standing in the respective academic field, in the hope that it will increase the number of citations, would be another example.

The pervasive pressure faced by numerous researchers to excel within the framework of a quantified scientific system is epitomised in the widely-known dictum 'publish or perish'. While this phrase is well-known not only among those concerned with that issue due to their research interest or their professional background but also among researchers from almost any disciplines, Biagioli and Lippman (2020) argue for complementation with the phrase "impact or perish" (p.1); according to the authors, this phrase more accurately reflects the "new forms of manipulation that do not affect the epistemic status of a publication but take place *around and outside* the claims" (p. 2), which they call "'post-production' manipulations" (p. 2). Examples of such manipulations encompass so-called coercive citations, whereby authors are pressured by journal editors to cite works published in the respective journal to enhance its impact factor. Another example is citation cartels, which boost the researchers' impact factors by belonging to that cartel. Both of these practices imply that high numbers of citations are not necessarily a reflection of high research quality (Fong & Wilhite, 2017; Wouters, 2020). Although such manipulation practices are not investigated in this thesis, they illustrate the performance pressure researchers may be feeling to perform well, not only in terms of the quantity of their publications but also in terms of the impact of their publications. Nevertheless, the latter is not possible without the former, thus being credited as an author is a prerequisite.

However, not all scholars subscribe to the common argument that the higher pressure to publish drives researchers to engage in questionable practices, including the inflation of findings, plagiarism, salami slicing of findings, and an overall negligence to publish as much as possible. For instance, Fanelli (2020) endeavours to critically dissect this narrative. He elucidates how, according to his own research, despite the overall quantity of publications linked to an author's name growing in most fields, the number of authors on a given paper has also risen on average, in fact increasingly so (Fanelli, 2020). The insinuation, it seems, is that a researcher's workload has not increased and therefore the pressure remains the same. The veracity of this argument, derived from quantitative data, is the subject of ongoing debate. The potential for counterarguments can be elucidated through the examination of

qualitative data, investigating how researchers experience and value processes preceding authorship decisions and the decisions themselves. Indeed, addressing that issue is the very core objective of this thesis.

2.3. How the temporal and spatio-material dimensions matter

Prior to delving into the significance of the social dimension and identifying the key actors in the realm of authorship practices, this subchapter offers deeper insights into the temporal and spatio-material dimensions. These two dimensions are grouped together because they are of lesser significance than the other two dimensions already considered and the last dimension, the social will be considered last. As with the other dimensions, both the temporal and the spatio-material dimensions must be conceived of as inextricably intertwined with the others.

2.3.1. *Quantification of time, projectification and nostalgia*

Time must be considered against the backdrop of the neoliberal logic that pervades academia, and “becomes a quantifiable resource that is open to manipulation, management and control, and subject to commodification, allocation, use and abuse” (Adam, 1998, p. 11). This understanding of time is evident in discussions pertaining to work-life balance, the limitations imposed by academic age in the context of grant applications, and the allocation of temporal resources in research proposals, along with other pertinent issues. In analysing researchers’ narratives on authorship practices, one inevitably encounters their “[f]eelings that time presents constraint, discipline, control and structure [which] are shared with the experience of time in terms of opportunity, points of reference and order or of celestial motion, the rhythms of the body and social organisation”, as Adam (1995) so beautifully puts it. Hence, as Felt (2017a) concludes, “[t]emporalities are therefore deeply entangled with how people can develop satisfactory lives in research, what degrees of agency they (believe they) have, or, more broadly speaking, how they assess their room for manoeuvring” (p. 133).

Another crucial aspect shaping the temporal dimension is what is commonly subsumed under the term projectification, meaning that research is predominantly funded by third party resources (Ylijoki, 2015). Projectification is crucial for the length of employment contracts,

especially those of Ph.D. students (Felt, 2017c). The associated assumption is that, within the time frame of the research project, countable units in the form of articles are then produced, which does affect the knowledge produced (Franssen et al., 2018). As Felt (2017a) reports, researchers' being obliged, due to project conditions, to constantly track their time spent, may potentially develop a "new kind of time consciousness" (p. 138). In particular, postdocs are affected by the prevalence of short-term contracts, knowing that they have to perform well enough in terms of publications and authorship positions within this period to either successfully move on to a group leader position, or at least, to another postdoc position with the promise of a subsequent unlimited position (Müller, 2014b). As Hammarfelt and colleagues (2020) exemplify in the context of the assessment of researchers for such unlimited positions, the temporal dimension is also evident in the construction of researchers' career trajectories, which are the basis for subsequent evaluation. The fundamental premise of such career trajectories is, as Felt (2017c) elucidates, "that by lining up indicators over time, stability, improvement or decline might be rendered visible" (p. 59), thereby facilitating assessments.

Temporal elements also arise when researchers discuss the current state of academia, whereby they imply or explicitly state, that the past was superior, even if they may not have ever experienced it. In her work, Müller (2021, p. 133) deliberately utilises the term 'crisis' through which she "aims at honoring a *feeling* of decay that permeates many interviews — a decay that is first and foremost a social one" (p. 133). In her renowned study on academic nostalgia, Ylijoki (2005) concludes how "[t]he nostalgic yearning for the lost golden age reveals current tensions and dilemmas through which the idealized past is then socially constructed" (p. 561). These sentiments of nostalgia must be contextualised within the broader framework of the metrification of science and the pervasive imperative to perform well, which may conflict with the desired level of intellectual autonomy and independence among researchers. Accordingly, "nostalgia concerns the moral order of academic work" (Ylijoki, 2005, p. 570), thus rendering it a sentiment of considerable relevance to this thesis.

2.3.2. *Spaces of work, authorship negotiations and emotions*

The spaces in which authorship is negotiated and enacted range from laboratories, where experiments are conducted, to offices, where writing and analytical tasks are performed, and

even to meeting rooms, where preliminary results are presented to other group members, as well as to sites of field research, where data is collected. A particularly significant location for this thesis is the laboratory, because the majority of researchers included in the data set conduct their work in this setting. Kerr and Lorenz-Meyer (2009) highlight the necessity of physical presence in the lab, which “comes from experimental routines but it is also linked to the predominance of project work: colleagues are needed to help with interpreting data, preparing samples, sharing material” (p. 133). Such interactions do not merely occur within the confines of a single laboratory; rather, they frequently extend across the boundaries of multiple laboratories.

The characterisation of place by Gieryn (2000) represents an intriguing point of departure for contemplating these locations. Gieryn (2000) lists three essential characteristics of place: it is “a unique spot in the universe [...], it has physicality [...and places] are also interpreted, narrated, perceived, felt, understood, and imagined” (p. 464-465). In particular, this latter feature directs us to focus on a crucial aspect, namely how researchers talk about their places of work. In line with this understanding, Davidson and Milligan put forth the following argument:

We can, perhaps, usefully speak of an emotio-spatial hermeneutic: emotions are understandable — ‘sensible’ — only in the context of particular places. Likewise, place must be *felt* to make sense. This leads to our feeling that meaningful senses of space emerge only via movements *between* people and places. (Davidson & Milligan, 2004, p. 524)

The last sentence of this quote highlights how a place is “also a relational location. Its meaning refers to other places and to other dynamic subject positions” (Löw & Weidenhaus, 2017, p. 558). Similarly, the (subjective perception of) distance or proximity to colleagues and superiors can influence the level of satisfaction experienced by researchers, as well as patterns of collaboration and competition (Taylor & Spicer, 2007, p. 329). In this context, one should not overlook “how patterns of power and resistance may shape manifestations of distance and proximity” (Taylor & Spicer, 2007, p. 329).

As outlined above, there are evidently structural conditions, such as the projectification of academia or the necessity to work in a laboratory, that work to strongly form researchers’

epistemic living spaces. Nevertheless, a further crucial line of enquiry is considering — to quote Tabboni, (2001) — “how and to what extent do social structures determine individual behaviour, and how in turn do individuals modify social structures to suit their own purposes” (p.21). In light of these considerations, it is imperative to investigate the role of the social dimension in authorship practices.

2.4. How the social dimension matters

In many disciplines, including most subfields within the life sciences and physics, research is typically conducted through a collaborative approach. Divisions of labour, both within a research group and beyond its boundaries, including across institutional and national borders, is a typical *modus operandi* for many researchers in the life sciences and physics, which is also reflected in the composition of (long) author lists (Hosseini et al., 2020; Larivière, 2012; Walsh et al., 2019). Accordingly, and as previously discussed, there has also been a notable increase in the number of individuals identified as authors on published works (Fanelli, 2020; Fanelli & Larivière, 2016; Johann & Mayer, 2019). The representation of this collaborative endeavour in a list of authors involves some degree of negotiation, ideally satisfying the interests of all contributors. Consequently, authorship practices and social structures are inextricably linked.

2.4.1. *The normality of collaborations*

Particularly within the domains of the researchers who constituted the data set for this monograph, but also more generally throughout academia, transinstitutional, inter- or transdisciplinary collaboration on a publication is a near-universal practice in contemporary academia (Jabbehdari & Walsh, 2017; Mäkinen et al., 2020). Interdisciplinary research is a near-inevitable consequence of contemporary academic practice. Lewis and colleagues (2016) speak of a “proliferation of new specialisms, many predicated on interdisciplinarity, colonising and exploiting the spaces — and synergies — between established disciplines” (p. 472), particularly in the life sciences. The implications of this also extend to authorship practices because authorship conflicts may be exacerbated by discrepancies in disciplinary cultures, as researchers may diverge in their opinions about “what is deemed *worthwhile*, what sort of knowledge and work is valued, and what *deserves* reward and recognition” (Lewis et al., 2016, p. 480) in the form of authorship or a particular authorship position. Such

disparities are also apparent in other areas of scientific work. To illustrate this, Edwards et al. (2011) delineate the challenges researchers face in making their data usable for collaborators or for potential future projects conducted by other researchers, work which is additionally and frequently devalued.

Fanelli and Larivière (2016) present an increase in collaborations as a potential response to the pressure to publish, which may allow researchers to be listed as authors on a greater number of papers while maintaining their level of investment in their research. Based on their data analysis, they demonstrate how the “[f]ractional productivity, calculated by dividing the total number of papers published by the average number of co-authors, was highest in the first half of the 20th century and declined overall” (Fanelli & Larivière, 2016, p. 5). However, Fanelli and Larivière (2016) acknowledge how the rather stable or even declining fractional productivity does not provide any insight into the burden researchers may feel of constantly working to expand their list of publications, which has been reported in qualitative studies (see, for example, Bloch, 2016; Di Fiore et al., 2023; Edlund & Lammi, 2022; Fochler, 2016). While quantitative approaches to authorship, such as that by Fanelli and Larivière (2016), offer valuable insights, they cannot provide a perspective on researchers’ experiences because these are not captured by analysing digitalised publication records in databases such as the Web of Science.

Furthermore, from an epistemological perspective, it is particularly intriguing to observe the emergence of a “rising fragmentation of tasks in team science” (Larivière et al., 2016, p. 431). This phenomenon indicates that not all authors on a scientific publication based on a team’s collective efforts are necessarily involved in all stages of the research process leading up to the publication. Moreover, in the context of social collaboration, the emotional challenges and the learning process inherent to (interdisciplinary) teamwork must also be acknowledged (Freeth & Caniglia, 2020).

2.4.2. *(Non-)caring, gendered relationships and emotional work*

The relational aspect of authorial practice is of paramount importance for two reasons. Firstly, it is a vital component for ensuring the viability of the division of labour. Indeed, as Kerr and Garforth (2016) observe, “patterns of affect and care for colleagues, careers and futures [...]”

also constitute laboratories and their work” (p. 17). In particular, the conceptual lens of care is deployed on numerous occasions throughout this thesis in order to enhance sensitivity to relational aspects, given that care is inherently “*relational*” (Martin et al., 2015, p. 631). Yet, as Martin and colleagues (2015) emphasise, “a person who cares must first be willing and available to *be moved by this other*” (p. 631), directing the attention to how “the capacity to respond is itself unevenly distributed and enmeshed within complex configurations and logics of power” (p. 635). Hence, efforts to trace care practices and visions of care must consider the wider context and how power permeates the relations involved.

Secondly, the relational aspect is a fundamental factor in enabling individuals to flourish and realise their autonomy. Jenkins and Neal (2023) elucidate this point, stating that “deep, persistent and nourishing relationships are foundational to people’s abilities to realize and maintain their autonomy” (p. 2). Consequently, relationships characterised by an absence of care⁴ may be perceived as constraining an individual’s capacity to shape their epistemic living space.

Furthermore, since “working is an activity infused with emotion” (Knights & Clarke, 2014, p. 337), emotional issues run through the layers making up the social dimensions and are present in any identity work researchers engage in as part of the analysed group discussions and interviews. Moreover, given the intertwinedness of the various dimensions, the common feeling of insecurity stemming from an atmosphere of pressure and competition, in combination with the precarious working conditions, “can be very bitter even though the creative and productive potential and promise of academic work can be equally sweet” (Knights & Clarke, 2014, p. 338).

While not a central aspect of this thesis, it is nevertheless considered vital to exercise sensitivity towards the potential influence of gender and class issues on authorship decision-making processes (see for instance Loveday, 2015; Ni et al., 2021; Sarsons, 2017). To provide a more general illustration, Hönig (2017) demonstrates in her research how female European

⁴ How care is conceptualised and has informed the analysis and interpretation of this work is further explored in Subchapter 3.3.

Research Council grantees are more likely to have a higher socioeconomic background than their male counterparts. One might even suggest that such a background acts as a kind of counterbalance to any gender discrimination. As Tudor and Yashar (2018) observe, the submission rates of female researchers are often lower than those of their male colleagues. Furthermore, the number of co-authors does not increase in the same manner as it does for male researchers. Or, when asked about past experiences regarding authorship practices, “[w]omen perceived that they received less credit than deserved, while men reported the opposite” (p. 1) as Ni et al. (2021) report.

2.4.3. *The main actors. The PhD student, the postdoc and the group leader*

So, who are the actors whose web of relationships is to be disentangled? Typically, the most significant relationships are those within the same research group or department, depending on its size. Indeed, research groups often form identities to which members subscribe to varying degrees (Schönbauer, 2018). Since the meaning and the value ascribed to authorship and authorship positions is presumably most strongly influenced by the career stage of the researcher, the relevant actors are presented by classifying them according to their career stage (see also Hangel & Schmidt-Pfister, 2017), distinguishing between the PhD or doctoral student, the postdoc and the group leader. The group leader also functions as the formal supervisor of the PhD student and may be listed as principal investigator on project applications and grants. However, it is naturally more intricate, as exemplified by the work of Laudel and Gläser (2008), who “propose to regard the academic career as consisting of three interrelated but largely independent careers [...namely] the *cognitive career* [...], a *community career* [...and] an *organisational career*” (p. 390). Or, as Hammarfelt et al. (2020) emphasise, “‘position’ [...] suggests academic rank, as well as ‘situatedness’ in the field of research, and geographical location” (p. 37). These aspects are addressed in greater detail in the following sections, as well as in the empirical section of the monograph. However, given the significance of the researcher’s career stage, in the empirical section of the dissertation, the career stage is, whenever it is known, either explicitly stated in the text or at least discernible in the description of the data cited in brackets.

Starting at the beginning of a scientific career, the figure to be discussed is that of the PhD student. In light of the conceptual foundation of this thesis, one intriguing aspect pertaining

to PhD students is how they “imagine for the first time, what such a living space in which they work would/should/could look like” (Felt et al., 2013, p. 514). PhD students have a kind of dual role because they are students, but also, as Boothby et al. (2022) point out, part of the “working population” (p.2). As ‘workers’ (i.e. researchers in training) they contribute to the growing body of knowledge (Larivière, 2012). This expectation is also reflected in contemporary graduation requirements, which usually require doctoral students to be first authors on at least one, but more often several, publications (Hangel & Schmidt-Pfister, 2017). Depending on which role is more in the foreground, either for themselves or for the others interacting with them, expectations and valuations of them differ. For a PhD supervisor, for example, is it to supervise PhD students because they are comparatively cheap labour and thus increase the scientific output of the group in the form of publications, or is it to mentor and thus shape the scientific offspring (see also Paradeise & Filliatreau, 2021)? Additionally, the specifics of a particular field of research also play a role, with one particular area of consideration being the distribution of tasks in combination with the necessary amount of supervision of doctoral students. The less time- and person-intensive the supervision required in a given field, the greater the number of PhD students that can be employed to carry out tasks that do not require as much involvement of supervisors or postdocs (Boothby et al., 2022). Moreover, depending on the size of the research group, PhD students may be actually trained day-to-day by the postdocs, who are still working on the bench, which is in contrast to many group leaders, who are more concerned with administrative or organisational tasks (Garforth & Kerr, 2010).

These roles, however, do not necessarily have to exist in a state of tension with each other. For example, by training them to set up an experiment, PhD students ‘automatically’ produce data. As students, they may anticipate being trained by their doctoral supervisors and this responsibility may also be formally laid down in guidelines by the university. Such training may include how to write a scientific article, conduct experiments, or write code, but it may also include the tacit transmission of values relevant to their profession (Davies & Horst, 2015; Felt, 2015; Nästesjö, 2021). Moreover, as Wylie (2021) notes, “undergraduates’ frequent requests for justification [...] requires practitioners to think about how and why they do that work and how to communicate those reasons to non-experts” (p. 154). This ultimately contributes to the advancement of knowledge, as Wylie (2021) posits.

Doctoral students are thus engaged in the process of “orientation, i.e., coming to grips with the epistemic, social and institutional space they are in” (Felt et al., 2013, p. 515). It is a period that can be very emotionally charged for the doctoral student (Bloch, 2016). However, these values may be significantly moulded by the tenets of neoliberalism. As Roumbanis (2019) notes in his ethnographic study of the lectures given by successful senior researchers to the scientific offspring, “When the professors advise their audiences to be as productive as possible, they reinforce a value by virtue of their power as expert evaluators” (p. 208). Consequently, those pursuing a career in research may experience a tension between the values and logics associated with the norms proposed by Merton (1985) and the acceptance and normalisation of “this neoliberal game, these unspoken rules (i.e. competitive individualism, surveillance, etc.)” (Na et al., 2021, p. 2; see also Mars et al., 2014). Hence, a relevant relationship for this dissertation is the one between the PhD student and the PhD supervisor, who, in many cases, will also be the group leader, and thus also the supervisor of the postdocs and technicians in the group (if there are any). The supervisor often plays a crucial role in “the adjustment of horizons of possibility” (Berli, 2021, p. 2) for a potential career in science and the related adopted “strategies of managing expectations” (Berli, 2021, p. 2). Antal and Rogge (2020) similarly highlight the significance of mentors, who may also serve as their PhD supervisors, in guiding aspiring researchers in envisioning and pursuing a career in science. Moreover, as Wang and colleagues (2021) demonstrate through a scientometrics analysis of publications with their co-authors and citations that “established supervisors do provide better and more academic resources and opportunities to doctoral students to facilitate their scholarly socialization” (p. 4923), specifically in terms of the size of the academic network and the prospective position of the PhD within it.

Nevertheless, the process of socialisation does not necessarily conclude upon the attainment of a PhD. Johnson (2020), for instance, investigates the concept of socialisation as an enduring phenomenon spanning the entirety of one’s professional career. Any socialisation processes are contingent on the act of storytelling and Czarniawska (2004) succinctly asserts how “scientists become scientists with the help of narrative” (p. 9).⁵ How this socialisation process extends to researchers’ valuations is also explored by Fochler, Felt and Müller (2016), who

⁵ The conceptualisation of narrative is further elucidated in Subchapter 3.1.

show how, after several years in academia and thus having undergone a certain process of socialisation, researchers exhibit “an ever-narrower regime of valuing their work and that of others” (p. 197). Since this thesis is concerned with the valuation processes of researchers in the context of authorship, this is a particularly relevant observation. Therefore, their finding that researchers who remain in academia “move from a heterarchical epistemic living space into a hierarchical environment” (Fochler et al., 2016, p. 197) raises the question of whether the findings of this thesis come to a similar conclusion.⁶

Moving up, another pivotal figure who merits consideration is the postdoc. Müller (2021) suggests that the postdoc “offer[s] a particularly valuable vantage point from which to explore the guiding values of contemporary academic work” (p. 134). The argument put forth by Müller (2021) is that, given the relative scarcity of positions with unlimited contracts (particularly within the life sciences), postdocs are compelled to navigate a competitive landscape and this, in turn, fosters an attunement to the values that are widely embraced within the academic community. In terms of relational aspects, they typically assume a significant role for PhD students, given their frequent presence in the lab as compared to the group leader, i.e. the formal PhD supervisor (see also Müller, 2014a). The care provided to PhD students may be perceived as a burden by postdoctoral researchers, because it diverts time and resources away from their own research agendas (Müller, 2012, 2021). Furthermore, it may diminish the likelihood of being designated as the primary author on a given publication. Correspondingly, as Sørensen and Kristensen note from their interviews with postdocs:

[They] talked about time as a scarce resource, that it was too time-consuming to do high-quality — or in many cases — high-quantity (‘enough papers’) research to succeed in academia, if one wanted more in life, either a family or to pursue other, non-academic activities. (Sørensen & Kristensen, 2022, p. 132)

Still at the level of a postdoc, they may turn to more experienced individuals for support. As Nästesjö (2021) observes in their data, that for a female postdoc, “consulting the PI of the project in which she works reduced uncertainty about performance criteria at the postdoctoral level” (p. 247). Additionally, postdocs may also be engaged in individual projects, which offer

⁶ See also Stark (2009) for his work on the notion of ‘heterarchy’.

advantages such as the frequently highly sought-after intellectual independence. However, disadvantages may also manifest, such as a sense of isolation and a lack of belonging to a group in which mutual support is provided (Nästesjö, 2021).

Ultimately, the group leader is often the final arbiter of the authors' list, or at the very least, has a significant influence on it, due to their superior position within the academic hierarchy. It is therefore evident that in their hands lies the "power of deciding who and what to delete" (Wylie, 2016, p. 89). As Knorr-Cetina astutely observes in her seminal work, *The Manufacture of Knowledge*, it is the group leader "who, after all, had a say in what left the institute by way of publication and what did not" (p. 106). It is therefore unsurprising that Fox and Nikivincze (2021) demonstrate that, in addition to a stimulating work environment and a high level of collaboration, a researcher's rank is a key indicator of prolific authorship.

In contrast to PhD students and postdocs, whose contracts are typically for a limited period, for group leaders, who are either Associate Professors or Full Professors, decisions regarding authorship are no longer a potentially decisive element in the process of acquiring a position. A slightly different situation applies to tenure track professors; for example, at the University of Vienna (n.d.-b), they are initially employed as Assistant Professors via a six-year, time-limited contract. The subsequent attainment of tenure and progression to Associate Professor is "contingent upon positive evaluation within the first six years of employment" (University of Vienna, n.d.-b, p. para. 3). However, for Associate and Full Professors one potential motivator for them may be the maintenance or even advancement of their standing within the respective scientific community (Hangel & Schmidt-Pfister, 2017). Moreover, Hangel and Schmidt-Pfister (2017) maintain that, for senior researchers, particularly "in the collaboratively organized sciences, their motivation to do research is mediated by their role as supervisors, chairs, or PIs" (p. 537). Consequently, the care practices employed by group leaders and aimed at the advancement of their group members may also inform their deliberations regarding authorship practices. In light of their roles, practices of care and responsibility may converge, given that the latter may be prescribed in documents. To illustrate, the *Code of Good Practice for Doctoral Schools* of the University of Vienna (2023), at which all the group leaders interviewed specifically for this PhD project are employed, as well as the supervisors of a significant proportion of the PhD students participating in the analysed group discussions,

dedicates one section to the responsibilities of supervisors. Interestingly, one paragraph pertains to the supervisor's responsibility to provide motivation and support, so "that the results [of the PhD project] are presented and published in relevant outlets" (University of Vienna, 2023, p. 2). The commonalities, differences, and interrelations, as well as the conceptualisations of responsibility and care, is further elaborated in Subchapter 3.3.

A number of works have already addressed the significant roles of group leaders from a variety of perspectives, with a particular focus on exploring how relational aspects shape epistemic, authorship, and publishing practices (Davies & Horst, 2015; Hangel & Schmidt-Pfister, 2017; Mars et al., 2014; Nästesjö, 2021). However, as Hangel and Schmidt-Pfister (2017) remark, "[w]orryingly, only few among our senior interviewees are aware of the possibility that their reasoning and acting may leave traces in the value sets of the junior generation" (p. 539). This observation is of particular interest for the present thesis, and the empirical part therefore also seeks to ascertain whether the phenomenon in question is reflected in the data corpus.

In essence, the objective of this chapter is the endeavour to elucidate and delineate the intricacies and relevance of the subject matter by contextualising each dimension with pertinent literature on scientific authorship and associated subject areas. Furthermore, the review of the extant literature yielded insights into research gaps, exemplified by Youtie and Bozeman (2016) who note "little attention has been given to the mechanisms used by individual researchers to resolve these [authorship] problems" (p. 379), which the thesis seeks to respond to. The identification of researchers' practices and visions of care, as well as their perceptions of responsibility, may provide a pathway towards the elucidation of such mechanisms. Finally, the monograph aspires to address the identified gap in the existing literature, as identified by Johann and Mayer (2019), namely the neglect of "individual authorship perceptions *in detail*" (p. 177). However, I embarked upon this investigation of researchers' perceptions not as a 'blank slate' but rather, I was imbued with a degree of familiarity with a number of concepts moulding the analysis of my data, which are elucidated in the following chapter. The subsequent chapter marks the commencement of PART II of the thesis, as the broader context of authorship within contemporary academia is believed to have been adequately delineated. PART II, meanwhile, places methodological considerations at the forefront and provides a comprehensive summary of the data corpus.

PART II CONCEPTS, DATA, AND METHODOLOGY

3. Cross-cutting sensitising concepts: "Picking one's way in an unknown terrain"

Throughout the process of data analysis for this dissertation, and in formulating the overarching research questions, it has become evident that there are cross-cutting sensitising concepts guiding and sharpening the analysis. In accordance with the recommendation put forth by constructivist grounded theory proponents, I will proceed to delineate the "sensitizing concepts" (Charmaz & Belgrave, 2012, p. 355), which have been a pervasive presence from the inception of my doctoral research project, exerting a profound influence on my intellectual pursuits. Despite their enduring role, a structured recapitulation of the interlacing of these concepts in this work has, until the present writing of this monograph, only been partially elucidated.

As posited by Bueger and Gadinger (2018), the social theorist Herbert Blumer (1954) is credited with pioneering the term 'sensitizing concepts'. Blumer puts forth the following conceptualisation:

The hundreds of our concepts — like culture, institutions, social structure, mores, and personality — are not definitive concepts but are sensitizing in nature. They lack precise reference and have no bench marks which allow a clean-cut identification of a specific instance and of its content. (Blumer, 1954, p. 7)

Nevertheless, this relatively general nature of sensitising concepts is not a disadvantage. Blumer (1954) emphasises this as he encourages researchers to "recognize that what we are referring to by any given concept shapes up in a different way in each empirical instance" (p.8). Hence, as he proceeds, "[o]ne moves out from the concept to the concrete distinctiveness of the instance instead of embracing the instance in the abstract framework of the concept" (Blumer, 1954, p. 8). In my case, the concepts of narrative, valuation, responsibility, and care have proven to be invaluable in analysing my unique data set, perhaps precisely due to their generality. Concurrently with the advancement of my monograph, and particularly in the context of my analysis, the concepts delineated in this chapter enhanced my comprehension of my case. The ensuing pages explicate the why and how of my

mobilisation and operationalisation of those specific concepts. In light of this, I ultimately concur with the conclusion of Mol and Hardon (2021) that “[c]oncepts are meant to be tools. They should not impose themselves on the analyst but serve the analysis” (p. 199). Or, in the words of Blumer (1954), this chapter chronicles the process of “picking one’s way in an unknown terrain” (p.8).

3.1. Narratives as sense-making devices

One essential concept for this thesis is the narrative⁷, frequently functioning as an “intermediary term” (Bueger & Gadinger, 2018, p. 69). Building significantly on the work of Czarniawska (2004), this thesis follows her argument that a “narrative is the main form of social life because it is the main device for making sense of social action” (p.11). As evidenced by the work of colleagues such as Squire et al. (2014) and Lawler (2002), this argument is also supported by a substantial body of research. However, it is crucial to avoid a common pitfall surrounding narratives, namely that they “are such a ubiquitous part of human understanding that it is easy to overlook the power they have in shaping speaking and thinking” (Wertsch & Batiashvili, 2021, p. 130).

In alignment with Felt’s (2017b) conceptualisation of narratives as “sense-making devices” (p. 55), a key objective is to elucidate, through narrative analysis, the ways in which researchers position themselves within the academic landscape and how authorship functions as an integral aspect of their identity and self-presentation (see also de Fina & Georgakopoulou, 2015; Squire et al., 2014). By examining the ways in which researchers position themselves within and through their narratives, it is possible to gain insight into the processes of identity work and the ways in which they “accomplish situated identities” (Deppermann, 2015, p. 307). Sense-making and the formation of an identity are closely intertwined, as evidenced by the proposition that “sensemaking is grounded in identity construction and that people make sense of their work activities under the influence of their individual-specific needs for self-enhancement (self-esteem), self-efficacy and self-consistence” (Brown et al., 2008, p. 1040). The examination of researchers’ sense-making narratives is a field of study not unfamiliar to

⁷ In this thesis, the terms ‘story’ and ‘narrative’ are used interchangeably. See also Squire et al. (2014) and Brown and colleagues (2008).

those engaged in STS. An illustrative example is the article⁸ by Mäkinen and Sapir (2023), which explores the sense-making narratives of Finnish and Israeli scientists on academic entrepreneurship.

Another aim of the narratological approach is to cast light on the epistemic living spaces (Felt, 2009b) of researchers, which are “narrative re-constructions of the structures, contexts, rationales, actors and values which mould, guide and delimit their potential actions, both in what they aim to know as well as in how they act in social contexts in science and beyond” (Felt & Fochler, 2012, p. 136). However, as Ursin emphasises:

[T]he academic identities presented in this [...thesis] are not to be seen as absolute representations of what individual academics are like; rather, these identities are products of socially situated actions that these academics perform when telling and constructing their stories of academic work. (Ursin et al., 2020, p. 322).

Moreover, I employ a “processual understanding of narrative and emphasise that narratives or ‘stories’ function as the social bond, or ‘glue’ that gives practices stability over time and space” (Bueger & Gadinger, 2018, p. 70). A constructionist understanding of narratives assumes a very similar stance, since it is “more concerned with stories as social events and/or social functions” (Esin et al., 2013, p. 205). Consequently, I posit that the narratives presented by participants in both the analysed group discussions and in the interviews are representative of those commonly circulated within their respective environments. Furthermore, I concur with Czarniawska’s assertion:

[W]hat people present in the interviews is but the results of their perception, their interpretation of the world, which is of extreme value to the researcher because one may assume that it is the same perception that informs their actions. (Czarniawska, 2004, p. 49)

Narratives play a pivotal role in (re)valuing a multitude of situations and practices, assigning responsibilities, facilitating a comparison between one’s own circumstances and an idealised version of ‘science’, and encouraging reflection on the perceived scope of action. Or, in the words of Flick (2004) “the narrative provides a general framework within which experiences

⁸ Interestingly, they draw on Appadurai (1986) and his theory of “*regimes of value*” (p. 4) to explore how researchers “negotiate coexisting regimes of academic science and industry” (Mäkinen & Sapir, 2023, p. 193), thereby also combining narrative with valuation studies.

are ordered, represented, evaluated and so on" (p. 94). In particular, individuals at the outset of their scientific careers recount narratives when seeking to make sense of the context into which they are entering. Indeed, "researchers' room for manoeuvring is shaped in and through narratives" (Felt, 2017b, p. 55). In particular, how early career researchers appraise the prevailing norms of authorship in their field and the discourses that surround them has the potential to restrict their scope for action and impinge upon their identity and conduct in research. Moreover, "while telling stories, individuals do not speak from a single position. As they draw on available storylines, public discourses and others' stories, storytellers' positions continuously change in relation to what discursive resources they deploy" (Esin et al., 2013, p. 206).

The analysis of narratives allows for the examination of the multifaceted, frequently imperceptible processes and influences at play (Czarniawska, 2004). To further specify, this thesis employs a constructionist approach to the analysis of narratives, on the grounds that it "also takes into account the broader social construction of that story within interpersonal, social and cultural relations" (Esin et al., 2013, p. 204). This dissertation follows a narrative approach that is attuned to the multiplicity of narratives and valuations inherent to the subject matter. Adopting this approach is a deliberate choice, as it allows for a nuanced understanding of the situatedness of the subject since "the same set of events can be organized around different plots" (Czarniawska, 2004, p. 7; Waibel et al., 2021). Both the multiplicity and complexity of these accounts, as well as the potential incongruities that emerge, reflect the lived experiences and work practices of researchers within the academic context (Squire et al., 2014).

Accordingly, the empirical part of the thesis also delineates the manner in which researchers position themselves when participating in the analysed group discussions and interviews, as well as the structural dimensions of their epistemic living spaces that they consider as impacting their practices. Thus, the emphasis is on "the *constraints problematized* by the actors" (Dodier & Barbot, 2021, p. 63). More specifically, the focus is directed towards "what actors see as being a constraint and how they position themselves in relation to the various forms of constraints that they consider themselves being exposed to" (Dodier & Barbot, 2021, p. 63). An awareness of these dimensions is integral to narrative analysis, because the

dimensions may not be explicitly referenced in the narrative process, and yet still exert an influence on the narrations in question (Villman, 2021). With this in mind, the objective of this thesis is to trace and examine the prevailing themes in researchers' accounts of authorship practices and to elucidate how these themes are interwoven and articulated in discourse, so as to construct a unified and coherent understanding for both the researchers themselves and for their audiences (King et al., 2019).

Furthermore, one often encounters the "production of explanations of events or justifications for the narrators' evaluations of narrative elements" (Carranza, 2015, p. 72) in the analysis of narratives. The act of narrating is intertwined with the act of valuing, which is explored in more detail in Subchapter 3.2. This nexus is moreover discernible in the examination of narratives, wherein notions of normality within a given social group are elucidated (Misztal, 2001; Nettleton et al., 2013). Normality links the act of narrating and valuing together, since "normality is critically bound up with notions of what is good and bad" (Bear & Knobe, 2017, p. 35). Accordingly, what is considered normal is subject to negotiation (Croce & Salvatore, 2017). One might also posit that "normality is a mere context-dependent social construct" (Freud, 1999, p. 333). It is noteworthy how the notion of 'normal' can be utilised to both describe existing circumstances and to imply a desired or ideal state. The capacity of the term to serve this dual function is a noteworthy phenomenon, as highlighted by Hacking (1990), who describes it as the "magic of the word" (p. 163). Correspondingly, as Villman (2021) outlines, in narrations, "the notion of normality might function both as motivation for and as resistance to conventional and institutional norms" (p. 83). Moreover, the normalisation of practices can be promoted through the act of narrating within the understanding of "*normalization in a more horizontal or processual manner*, i.e. as a token of social and individual, including linguistic/ discursive, behaviour and conduct in specific 'cultures' embedded by public/ private organizations or other social contexts" (Krzyżanowski, 2020, p. 436). Accordingly, the concept of normality is a pervasive theme throughout the third part of this thesis.

3.2. Tracing valuations in researchers' narratives

Especially in the context of the very dominant discourse on authorship practices not only

occurring in the scientific policy context but also in scientific journals and communities, taking valuation as an analytical lens to analyse researchers' narratives is particularly fruitful because the prominence of those debates underlines that in those discussions "central registers of value [...] are at stake" (Dussauge et al., 2015, p. 271). This so-called central register of value is what I explore with my thesis.

Hence, the manifold works emerging over the last years from the wide field of valuation studies have definitely shaped the way I examine the data set. Indeed, I am not the only researcher from the field of STS with a particular focus on science who has drawn upon this extensive body of work (see for instance, Fochler, 2016; Haddow & Hammarfelt, 2019; Hammarfelt et al., 2020; Kaltenbrunner & de Rijcke, 2019; Krüger, 2022). Engaging with works from the field of valuation studies means often coming across discussions on what notions are most suitable for the empirical case one wants to examine. Some argue in favour of making a "distinction between *valuation* (the process of giving worth) and *evaluation* (the process of assessing)" (Kjellberg et al., 2013, p. 20). Meanwhile, others claim that valuation can be sometimes "about assessing value, sometimes about producing it, and sometimes about both at the same time" (Helgesson & Muniesa, 2013, p. 4; Vatin, 2013). Heuts and Mol (2013), for example, work with the notion 'valuing' in their article, in which they strive to uncover what a good tomato is, because valuing is in their view is "best suited for exploring varied ways of performing 'good tomatoes', from assessing and appreciating, to adapting and improving. 'Valuing' also stresses that 'valuation' is active" (p. 130; see also Kjellberg et al. 2013).

This dissertation's focus resonates well with how valuation can be understood, especially after the so-called pragmatic turn (Heinich, 2021). For instance, Kjellberg et al. (2013) point out that the "meaning of valuation is not to be found in the object to which it refers, but in how that object is being referred to" (p. 18). Heinich (2020a) calls this particular form of valuation judgement, in contrast to the other described forms: measurement and attachment. In her understanding, judgement "is mostly a matter of words, ranging from mere common sense opinions to expert evaluations" (Heinich, 2020a, p. 84). This emphasis on those "evaluative acts" (Heinich, 2020a, p. 77) is an essential characteristic of the pragmatic approach and is particularly fitting for how this monograph endeavours to gain a deeper understanding of authorship practices (see also Heinich, 2020c). Thus, my work pays particularly attention to

the active component, which is inherent in the understanding of valuation by the so-called French economic school '*économie des conventions*', with Luc Boltanski and Laurent Thévenot as its pioneering fathers (Heinich, 2020a, p. 77; Jagd, 2011; Strandvad, 2014; Waibel et al., 2021). Boltanski and Thévenot (2021) "moved away from questions of classification and evaluation entirely, focusing instead on justification" (Kuipers & Franssen, 2021, p. 150), signifying a shift away from field theory. Kuipers and Franssen (2021) refer to these understandings of valuations as the "post-Bourdieuian sociology of evaluation, and its French 'cousin' the sociology of conventions" (p. 150). This terminology is employed to, on the one hand, point to the more processual approach and, on the other hand, underscore its focus on interactions (Kuipers & Franssen, 2021).

The focus of the thesis thus lies on the process of value attribution, which shall be uncovered through the analysis of researchers' narratives. As outlined in the preceding subchapter, these narratives are conceptualised in this dissertation as sense-making devices (Felt, 2017b). Such an understanding of them mirrors the processes underlying a valuation, which can be described, according to Kjellberg and colleagues (2013), as "world-making and sense-making activities" (p. 16). In other words, "people formulate arguments and critical stances with respect to their experiences and situations" (p. 4) as Bowen et al. (2021) summarise and which is a core tenet of the aforementioned "pragmatic sociology of critique" (p. 1158) in France. Moreover, valuations and the evaluative positions the narrators assume often play a crucial role in narratives (Wortham & Rhodes, 2015, p. 170ff.). Hence, the centre of interest of this thesis is valuation processes, because worth is actively produced through researchers' narrations. The notion of valuation is, therefore, used throughout the thesis when describing the processes present in researchers' narratives.

In contrast, in the case of formal evaluations of researchers and their respective publication lists, the aforementioned process of assessing, namely evaluators assessing the worth of such a publication list, is central. In such evaluation situations, (for example, when a university decides who to hire or a funding agency who to fund) the underlying assumption is that a publication, a citation, authorship in general, or a specific authorship position has an "intrinsic worth" (Heinich, 2020b, p. 217). Therefore, the terms evaluation or assessment are used whenever such formal procedures are described. Because this monograph's focus is on how

researchers talk about authorship practices, in the thesis's empirical part, the aforementioned formal evaluations are only of interest when referred to in researchers' own narratives. Consequently, only when such research assessments are "feeding and impinging on another valuation practices" (Helgesson, 2016, p. 94), are they considered.

When analysing valuations, remaining attentive to the environment in which evaluative acts are embedded is crucial, because the process of ascribing value is "reconfigured and tested out of existing corpuses" (Heinich, 2020a, p. 78). Heinich (2020a) does indeed argue that "the sociology of valuation should take into account not only the subject and the object of any (e)valuating process, but also its actual context" (p. 78). Taking into consideration the situation in which the process of valuation takes place is particularly interesting for this dissertation because its topical focus (in which the qualitative data analysed was generated) is not only authorship but also wider issues of responsibility and good practice in research. This dissertation thus analyses valuations concerning authorship that arise in the context of other discussed topics, but which are, in one way or another, entangled with them. Moreover, the various data sets were generated not only in interview settings with a researcher from the social sciences interviewing a scientist from the life sciences or physics but also in discussion groups where the other participants were either peers from the same or other research groups or possibly even their own group leader.⁹

Understanding "valuation as a social practice" (Helgesson & Muniesa, 2013, p. 4) sensitises one to the relational aspects that influence valuations, such as an awareness of who speaks with and to whom and is essential in the course of this analysis. Waibel et al. (2021) also stress the importance of being attentive to who the person articulating the valuation is and, possibly, to whom and in what kind of relation those persons are because relational aspects can impact valuations. As a result, Waibel and colleagues (2021) describe the concept of the valuation as a triad and claim there are three relevant positions: they differentiate between "the *valuee*, the *valuator*, and the *audience*" (38), whereby their critique is that especially the latter position is often overlooked.

⁹ The methodological aspects are elaborated on in more detail in the Chapter 4, which gives an overview of the data sets analysed and the respective methodology for doing so.

In my case, one can assume that a PhD speaking about the authorship practices in their research group, while their group leader is present and part of the audience, will speak differently than when in a discussion group populated only with other PhDs. The relational aspect and the resulting power imbalance between the PhD and the group leader thus shapes the articulated valuation. Moreover, PhDs, postdocs or group leaders speak from different standpoints due to their position within their research group, their experiences in academia in general, and with the respective assessments or with their ideals concerning science in particular, just to name a few.¹⁰ Nevertheless, despite these discrepancies, they also exhibit certain commonalities. For instance, they are all subject, to varying degrees, to the neoliberal logic described previously. How the researchers construct and situate themselves within and through their narratives is explored in the empirical part of the thesis. However, through their narrations about authorship, they might not only produce and assign value to the respective practices but also to their identity as a researcher and the contributions they made in the processes preceding the publication of a paper.

Overall, keeping those three positions described by Waibel et al. (2021) in mind when analysing the empirical data, has proved to be fruitful – especially if those positions are taken not as a rigid analytical tool but rather as an orientation point because, for example, the significance of the position of the audience is not the same in every analysed valuation. Instead, there can be the case of multiple (imagined) audiences with their varying desires and wants strongly influencing a valuation, which the empirical part discusses in detail. Moreover, being attentive towards the issue of “*multipositionality*” (Waibel et al., 2021, p. 40) is crucial, because it heightens one’s awareness of “how multipositional identities link distinct valuations” (Waibel et al., 2021, p. 40). Further, Helgesson (2016) discusses the issue of linked valuations in the context of the review process of academic papers and calls them “*Folded Valuations*” (p. 93). Thinking about how the valuations observed in researchers’ narratives are interconnected due to the issue of multipositionality and, therefore, possibly influence each other, is significant for the analysis.

¹⁰ This aspect is also addressed in Section 2.4.3, wherein the current scholarship on the subject is presented.

Additionally, one should be attentive towards both the context of the specific situation in which the evaluative act takes place, but also towards the “evaluative categories available to valuers” (Heinich, 2020a, p. 80) both within themselves and located on the exterior. In their work, Waibel et al. (2021) emphasise the external aspects impacting a valuation by pointing out the significance of rules and infrastructures, because those “shape, enable, and restrict valuations and their interconnections” (p. 35). Hence, when analysing researchers’ valuations, not only the situational context is considered, because valuations are “temporally and spatially situated” (Waibel et al., 2021, p. 35) but the broader context is as well, namely contemporary academia. This context, with a focus on the meaning and significance of contemporary authorship decisions, is explored in Chapter 2 through the concept of researchers’ epistemic living spaces. This chapter is crucial both for the reader but also the author of this dissertation because the second chapter elaborates on the broader context the analysed valuations are embedded as “axiological principles are cultural, that is collectively shared in a same temporal and spatial context” (Heinich, 2021, p. 72). Moreover, as the chapter focuses on the present meaning and significance of authorship, it offers insight into the “the nature of the object being evaluated” (Heinich, 2020b, p. 214), i.e. authorship practices in my case.

Furthermore, authorship practices, or more specifically, authorship decisions, can be conceptualised as valuation practices themselves, whereby the valuation outcomes materialise in the form of the naming and ranking of authors. A publication with its respective list of authors is thus a “valuation infrastructure” (Waibel et al., 2021, p. 46). Consequently, the list of publications in a researcher’s CV, a Google Scholar profile, or an author profile in the Web of Science can be considered as examples of “the digitalization of valuation” (Waibel et al., 2021, p. 46). Even though this dissertation analyses researchers’ narratives about authorship practices — either their own or those of others — and not the actual practices through ethnographic research, these infrastructures are nevertheless of interest because those valuations are interlinked. This connection is particularly obvious when researchers talk about authorship processes when they were in the position to make the final decision and, thus, ultimately shape the valuation. However, it is also evident when a researcher voices their critique concerning, for instance, a ranking of authors, which is the materialised valuation outcome of another researcher, and this infrastructure equally exerts an influence. Moreover,

researchers and their conceptualisations of themselves are also shaped by those digitised assessments — even if these researchers are rather critical towards them. Nevertheless, they might draw on those assessments to underline their value and consequently their identity as a researcher.

The field of valuation studies has undoubtedly influenced my approach to data analysis and I found it particularly illuminating in terms of what it enabled me to discern. Nevertheless, it required another conceptual basis, the concept of epistemic living spaces (Felt, 2009b), to gain a more nuanced understanding of the identified valuation processes. This is because some or even all five dimensions may be “implemented in valuation processes” (Heinich, 2021, p. 71) when researchers make sense of and value authorship practices. I delve into more detail on the essential role that they play in the valuation process in the empirical part of my dissertation. While the structural dimensions of epistemic living spaces do indeed reflect structures on a more macro-level, the investigation of (in my case authorship) practices can also provide such insights.

Bueger and Gadinger (2018) posit in their examination of practices that “pragmatism’s micro-analytical interests in actions, contingency and creativity can be combined with a macro-structural consideration of normative orders and moral principles” (p. 88). Heuts and Mol (2013) mobilise the notion of registers of valuing, because, in their opinion, the processual aspect is underlined by the gerund ‘valuing’ and the term register points to “a shared relevance, while what is or isn’t good in relation to this relevance may differ from one situation to another” (p. 129). It is not only Heuts and Mol (2013) who have developed so-called ‘value registers’ or ‘value realms’; Boltanski and Thévenot (2000, 2021) and their concept of ‘orders of worth’ represent a prominent example in this regard (Heinich, 2021). To give another example, in the 1980s, Appadurai (1986) introduced his theory of “*regimes of values*”(p. 4). Furthermore, Kuipers and Franssen (2021) write of how “actor-network theory-scholars theorized registers of valuing or ‘valuation regimes’ (p. 157).

However, with regard to my research approach, I align myself with the proponents of constructivist grounded theory and those who, such as Bowen (2021), argue in favour of “viewing justification as a social activity, a practical activity rather than indexing a set of distinct

and fixed worlds or domains” (p. 125) before conducting and concluding a grounded theory inspired analysis. Accordingly, I have not attempted to trace the identified orders of worth as outlined by Boltanski and Thévenot (2021) within my data set. Notwithstanding, I do not presume to suggest that the narrative valuations constructed by researchers in the analysed interviews and group discussions are isolated from their respective research environments; rather, I posit that they are intricately enmeshed.

In this regard, values transcending a specific valuation situation are acquired in various educational settings. Thus, for my case, the first years spent as a researcher are of special interest because it is during this period that researchers tacitly learn, often for the first time, how authorship is handled and what underlying values can guide practices around authorial practices (Heinich, 2020b). However, in the course of their unfolding scientific careers, researchers might experience how authorship can be handled in different ways when working as part of other research groups. Or they may find themselves in the role of group leader and, as a result, the person actually in charge of making the final decision about authorship rankings, for example. The empirical part thus considers the socialisation processes that researchers themselves undergo throughout their careers in science and which impart them with values that are shared more broadly within academia, at least, to a certain extent.

Consequently, and in accord with the pragmatic turn emphasising that it is by “understanding and explaining actors’ relationship to axiological principles (or ‘values’) through their contextual implementations and justifications” (Heinich, 2021, p. 70), I focus on those contextual implementations and justifications, which are, for my case the dimensions outlined by Felt (2009b) in her description of the concept of the epistemic living space. How exactly the dimensions are mobilised and what kind of functions they perform in researchers’ valuations is at the centre of the empirical part of the thesis. The reference to a dimension might, for instance, function as a justification for a practice. Therefore, I draw on the work by Boltanski and Thévenot (2000) and assume that a “justification makes an appeal to common resources that go beyond the situation” (p. 212). Furthermore, the same dimension can presumably be referred to in different ways to value and/or justify a certain practice when the valuation is guided by a different value (Heinich, 2020b).

3.3. How responsibility and care matter in researchers' valuations

So, what roles do responsibility and care as cross-cutting sensitising concepts play in this dissertation? As this Subchapter outlines, those two concepts are often intertwined in my case. However, in the context of authorship, issues of responsibility are often discussed in relation to research integrity and misconduct, and thus often come with a rather negative undertone (see also Section 2.1.3). Hence, in those discussions of integrity and misconduct, responsibility is often used as a synonym for accountability for the results presented in a scientific publication — i.e. researchers can be held accountable for the content of a scientific article. Consequently, it is stressed in such discussions that authorship comes with real responsibilities and is not only about recognition and reward. This resonates with the observations made by Adam and Groves (2011) and, according to them, responsibility is frequently “understood within a legal framework in which it is interpreted as liability, and/or a moral one in which it is understood in terms of blame” (p. 18). In alignment with Adam and Groves (2011), Davies and Lindvig (2021) also describe how this rather traditional understanding has been criticised in the literature on responsible research and innovation (RRI) and research integrity and they argue for a broader understanding of those terms, moving away from blaming an individual and towards the adoption of a more future-oriented approach.

However, because in the centre of this thesis is the analysis of researchers' narratives and not the content of guidelines and regulations concerning authorship developed by journals, universities or funding agencies, an open-mindedness is maintained toward the ways in which those researchers conceptualise their responsibilities or those of others in their narratives. Their understandings do not necessarily follow or align with what is outlined in the aforementioned guidelines and regulations (Davies, 2019; Felt et al., 2018a). This thesis can thus help “make a clarification of what is meant by ‘legitimacy’” (Boltanski & Thévenot, 2000, p. 214) based on the analysis of researchers' narratives. Or, in other words, “the moral value of given practices and objects is to be determined as part of our inquiry” (Bowen, 2021, p. 126). However, it can also be illuminating to examine if (and if so, at which instances) researchers refer to guidelines and regulations to, for example, underline the legitimacy of their raising the issue of responsibility. Overall, tracing the instances in which the researchers are “holding someone responsible, [or are] assuming responsibility” (Adam & Groves, 2011,

p. 18) in their narratives, is considered to be an insightful approach. A more detailed analysis can then determine whether there are any recognizable patterns concerning the topics for which researchers tend to assign responsibility to a specific person, or for which issues they perceive an institution to be in charge.

Presumably, due to an openness towards understandings of responsibility and care of both the interviewed researchers and those participating in group discussions, the concepts are, in many instances, entangled with each other. However, the following observation made by Puig de la Bellacasa (2017) facilitates sharpening the differentiation between care and responsibility because she argues, by drawing on Haraway, that “for what/whom we care for doesn’t necessarily mean being *in charge*, but it does mean being involved” (p. 90). Hence, as Mol and Hardon (2021) elaborate, “the activity of *caring* is not neutral: it is meant to culminate in something that counts locally as ‘good’” (p. 185). Further lines of demarcation between care and responsibility might crystallise with the ongoing analysis. Tracing practices of care can be challenging and in the view of Mol and Hardon (2021), “it is not always easy to say *who* or *what* does the caring and *who* or *what* is being cared for [...because] caring is performed across networks” (p. 191).

The entanglement of those two concepts becomes further apparent when considering how to mobilise care in the analysis because similar challenges arise when striving to trace responsibility in researchers’ narratives. For instance, the analytical lens used does not necessarily capture site-specific enactments of care, a possible tension that Martin et al. (2015) have also described. Moreover, historically, care practices have often been overlooked, which is something Shapin (1989) has also discussed in his well-known work on the invisible technician. It can therefore be helpful to look for instances in researchers’ narratives in which, from their perspective, their caring for someone or something has not been (highly) valued. Being sensitised towards the common mundanity of such practices of care can also facilitate their detection (Kerr & Garforth, 2016; Lindén & Lydahl, 2021). Furthermore, being attentive to when an “affectively charged sensibility characterised by worry, attentiveness, and thoughtfulness” (Martin et al., 2015, p. 629) is perceivable in researchers’ narratives, is another approach to tracing care. Such an approach seems to be both open enough about what kind of care visions and practices are traceable and also concrete enough to know what to look

for. For example, instances in which (mostly) early career researchers ponder over leaving academia can be then framed as moments in which the absence of care does become apparent. This sensitivity possibly also allows me to trace both practices and visions of care. Following the argument made by Heinich (2020a), who emphasises that the “logical incompatibility [of values] does not prevent them from being co-present in the actor’s experience” (p. 85), visions and practices of researchers might point towards different values guiding them. Heinich (2020a) even takes academia as an example to underpin her argument and derives that “scientists may well desire and seek recognition for their work while sincerely aiming for the disinterest and universality of science” (p. 85).

Overall, the concept of the epistemic living space, with its symbolic, social, intellectual, temporal, and material dimensions, can also enhance their understanding in the context of care and responsibility. Because care is “always a social process, one conducted through our relationships with other people” (Adam & Groves, 2011, p. 22), any valuations in which the social dimension is strongly present might also include references to the presence or absence of care (Martin et al., 2015). However, practices of responsibility might also be traceable in such narratives because, for example, a PhD supervisor has the formal responsibility of appropriately supervising their PhD student(s). However, such a formally defined responsibility might then be enacted differently in practice. Therefore, it can be fruitful to work out the relation between what researchers consider to be ‘normal’ and what might be viewed as ‘responsible’ and/or ‘caring’ in their narrations. Being sensitive towards what researchers describe as their situated normality allows further sharpening of my analysis of visions and practices of care and responsibility. Furthermore, references to the material dimension in researchers’ narratives can indicate care practices: caring for samples or possibly even instruments has been framed by Kerr and Garforth, (2016) as a care practice. Care and materiality are thus closely linked to each other (Lindén & Lydahl, 2021). To give another example, a layer of the temporal dimension might be referred to when a group leader talks about their responsibility to ensure their PhD’s publication output, allowing them to finish their PhD in time. The article by Davies (2015) also touches upon the care practices of group leaders. A care practice in the realm of the epistemic dimension might be a researcher double checking their findings before publishing them. Or a critique directed towards the habit of not including the group’s technician due to their position could also be considered the voicing

of a concern that the symbolic dimension too strongly shapes authorship decisions, which, in turn, might be then framed as a non-caring practice. Further, which dimensions, or layers of the dimensions, researchers perceive as inhibiting responsible and/or caring authorship practices is part of my analysis. The aforementioned examples serve to illustrate the ways in which the concepts of care and responsibility can be mobilised in researchers' narratives. Furthermore, these concepts aim to attune the reader with the locality of care practices (Mol & Hardon, 2021). The interplay between the two concepts and the five dimensions that the concept of epistemic living spaces outlines is explored in greater depth in the empirical section, but particularly in Subchapters 6.3, 6.4, 6.5 and 8.2, foregrounding the significance of structural aspects.

Moreover, because the overarching research questions examine how researchers' sense-making of current authorship practices reflects not only their values but also their visions of responsibility and care in their respective communities, it is essential to delve deeply into the reasons why researchers draw on their understandings of care and responsibility in their processes of valuation during the analysis. The observation made by Heuts and Mol (2013), which leads them to the conclusion that "[i]n the practices that our informants talk about, valuing is not a matter of casting judgements after the facts. Instead, it is part and parcel of a variety of activities that experts engage in to care for" (p. 136) serves as a highly valuable insight into how interwoven valuation and care are and, therefore, sharpens the data analysis for this dissertation. In the analysed researchers' narratives, there are manifold instances in which they describe how they or others are engaging in various care activities to ensure 'good' authorship practices, and thus ultimately a 'good' list of authors. However, it is also within and through their narratives that researchers engage in a "exploration of what it is appropriate or logical to do in some site or situation, and what is not" (Mol, 2008, p. 8).

In conclusion, this subchapter endeavours to provide the reader with the first insights into the complexity of the narrative valuations to be analysed in the empirical part of this dissertation and their relations to responsibility and care. Taking the observation made by Mol, Moser, and Pols (2010), that "it is taken as inevitable that different 'goods', reflecting not only different values but also involving different ways of ordering reality, have to be dealt with together" (p. 13), this thesis aims to not only uncover the various principles guiding the

researchers' valuation but also their entanglements with each other. Nonetheless, prior to the empirical section of the dissertation, in the following chapter the data corpus is presented and the adopted approach to its analysis is detailed.

4. Engaging with a wealth of data through the constructivist grounded theory

4.1. Introduction

While a plethora of studies approach the issue of authorship through the mining of quantitative data, thereby focusing on facets of bibliometrics, this work instead follows the individuals behind the indicators, so to speak, and their narratives on authorship practices (Andersen, 2023). It assumes that “by analysing how people talk to one another, one is directly gaining access to a cultural universe and its content of moral assumptions” (Silverman, 1993, p. 108). Consequently, the focus of the analysis is on the researchers and their narrative valuations concerning authorship practices, and the findings of this dissertation are rooted in the analysis of a qualitative data set.

Yet, adopting a qualitative approach does not entail disputing the value of quantitative works, as they provided valuable impulses and insights for this dissertation. For instance, the analysis of authors’ contribution statements by Larivière and colleagues (2016) could be conceptualised as an analysis of “behavioral data traces” (Ford, 2014, p. 2), which has sensitised me how researchers’ formal position is commonly linked to specific kinds of contributions. Hence, one aim of this thesis is to serve as a “complementary work” (Ford, 2014, p. 2) to publications operating with quantitative data sets. In this regard, I support the proposal put forth by Wyatt et al. (2017) about how “STS needs to be able to build bridges and to engage in multiple processes of translation between qualitative and quantitative approaches” (p. 103), the latter being in particular the field of scientometrics.

In contrast to bibliometric data, the data analysed herein could be classified as “observational and experiential” (Gabrys et al., 2016, p. 2), since, in many instances, both in the analysed interviews and in the group discussions, the researchers introduce stories related to authorship that they have either heard second-hand of or experienced themselves. However, in alignment with the tenets of pragmatic sociology, particularly as espoused by Boltanski (2011), I adhere to the viewpoint that “actors, thanks to their critical capacities and reflective

competences, possess relevant knowledge about the world” (Bueger & Gadinger, 2018, p. 89). Consequently, I regard an analysis of the authorship accounts of researchers as being pivotal in pursuing further knowledge, particularly in relation to my research questions and its associated sub-questions.

4.2. A synopsis of the data corpus

The circumstances under which the analysed narratives on authorship valuations were collected are outlined in this chapter. Such a detailed description of the analysed data corpus is considered to be crucial, because “[h]ow you collect data affects *which* phenomena you will see, *how*, *where*, and *when* you will view them, and *what* sense you will make of them” (Charmaz, 2006, p. 15). The reference to Charmaz is indicative of the methodological approach because she is renowned for her work in grounded theory research, particularly constructivist grounded theory, which she pioneered (Charmaz & Keller, 2016).

Prior to elucidating the process of data analysis, the data set is presented, given that two out of three components were obtained from research projects other than this PhD project. All of the researchers involved in those two research projects, Ulrike Felt, Maximilian Fochler, Florentine Frantz and Lisa Sigl, worked at or were members of the *Responsible Research and Innovation in Scientific Practice* research platform at the University of Vienna (n.d.-a) and agreed to the reuse of their data. Furthermore, the informed consent given by the interviewees and participants in the discussion groups and interviews allows for the re-use and secondary analysis of this data.

4.2.1. Interviews and group discussions in the context of ‘IMAGINE RRI’

One part of the data has been collected by Felt, Fochler and Sigl as part of the research conducted at the University of Vienna’s interdisciplinary research platform *Responsible Research and Innovation in Academic Practice*. The data examined in this dissertation encompass 11 discussion groups, each of which lasted approximately three hours, as well as 28 interviews conducted by Felt, Fochler and Sigl with researchers engaged in academic life sciences in Austria, yielding approximately 1,000 pages of transcripts. (Felt et al., 2018a, p. 220; Sigl et al., 2020, p. 1571). In pursuit of their research interests, they devised the card-

based discussion method IMAGINE RRI, outlining the considerations that informed its development and delineating the method's objectives in their article *IMAGINE RRI. A card-based method for reflecting on responsibility in life science research* (Felt et al., 2018a). The supplementary material, in the form of the discussion cards, is publicly accessible via a link and is governed by the terms of a Creative Commons licence (Felt et al., 2018b).

In addition to the broader application of card-supported discussion methods within the social sciences, IMAGINE RRI was inspired by the card-based engagement method, IMAGINE, which was previously utilised in citizen engagement. The development of IMAGINE was a collaborative effort involving Felt and other key contributors (Felt et al., 2014). Through IMAGINE RRI, the STS researchers "empowered researchers to appropriate the concept of RRI through shared reflection, and enabled them to connect their practices to wider and sometimes abstract RRI concepts and debates" (Felt et al., 2018a, p. 203). Moreover, it "also aimed to foster reflection on how the institutional context of researchers' work enables or hinders their engagement with ethical and social aspects of their work" (Felt et al., 2018a, p. 203). The research subjects' joint contemplation, the discussion of their everyday work practices (of which authorship practices are a part), as well as their deliberations about their working environments, which constitute their epistemic living spaces, provide crucial insights for the dissertation's focus.

Since parts of both the interviews and group discussions were relevant to this dissertation, I have included in Appendix II, those cards from IMAGINE RRI that frequently evoked reflections and discussions around authorship practices and were, therefore, more closely coded. The inclusion of only a selection of the cards does not imply that the remaining cards did not elicit any statements related to authorship. Rather, the presented set represents the most common cards and also serves the purpose of facilitating the visualisation of the method for the reader. The complete set of cards is also accessible for reference (Felt et al., 2018b).

In the context of my research, practically the only statement card of interest is the 'System' card, which is selected in the initial round of discussion groups if the participants perceive it to be the card with which they are in the strongest agreement or in the strongest disagreement (Felt et al., 2018a). In the statement, responsibility is ascribed to the system as

a whole, with the individual researcher regarded as the bearer of minimal accountability. A number of practice cards, each depicting a cartoon with a brief description and three questions, address authorship directly or indirectly. Examples include the cards with the headings 'Secrecy in Publications', 'Authorship Negotiations', 'Teamwork & Supervision' or 'Expectation & Work Reality'. In the experience of Felt et al. (2018a), "the visual element of the cartoons often takes an ironic approach to experiences researchers may have had in their work and induces laughter, sarcastic statements or other spontaneous reactions" (p. 210). For the second round, participants select two cards representing the most significant instances where issues of responsibility emerge in their work. The final phase "revolves around the conditions under which research is done and how these conditions may support or restrain researchers' capacity to consider questions of responsibility in making research-related decisions — in short, reflecting on the 'responsibility conditions'" (Felt et al., 2018a, p. 211). Of particular relevance to my work are the context cards 'Metrics & Quantification', 'Time Pressure' and 'Temporary Contracts', all of which come with a direct quote from a researcher. In particular, the moments of discussing relevant context cards illustrate the significance of epistemic living spaces for researchers, including in the context of authorship issues. For instance, temporal aspects emerged as a pivotal issue in the valuation of contributions. In addition, the various cards prompted reflection on the researchers' visions and practices of responsibility and care, which in turn relate to the second main research question.

4.2.2. Discussion groups from the project 'Borderlands of Good Scientific Practice'

The other part of the preexisting data corpus was collected as part of the ÖNB funded research project *Borderlands of Good Scientific Practice. Investigating a Global Phenomenon in its Local Versions* (Felt, 2016). It was carried out by Ulrike Felt and Florentine Frantz and for which early career researchers from the life sciences and physics based in Austria have participated in five group discussions (4-5 hours each), amounting to around 560 pages of transcripts (Felt & Frantz, 2022a). As Felt and Frantz (2022a) state, the objective of the card-based discussion method RESPONSE_ABILITY is to "support the cultivation of reflexive skills and sensitize researchers to both questions of value choices within research and to the structural boundary conditions they find themselves in" (p. 14) The discussion cards, designed to facilitate conversation, are subdivided into four categories: values that inform research activities, instances of potential transgression of good practice, ethical dilemmas, including

authorship quandaries, and structural conditions. This approach signifies the researchers' concern to allow the parties concerned, namely the researcher themselves "negotiate (with themselves and others) in which contexts certain practices can still be regarded as acceptable and where possible transgressions might begin" (Felt & Frantz, 2022a, p. 3), a process which is not as straightforward as it may be envisioned. For the design of RESPONSE_ABILITY, the concept of epistemic living spaces was also central, holding the position that "the ways in which 'good' knowledge is produced as inseparably intertwined with how researchers (can) live in science" (Felt & Frantz, 2022a, p. 3). In the same vein, a fundamental assumption of this thesis, arising from the concept of epistemic living spaces, is how authorship is inextricably entangled with the researchers living in academia.

Since I also worked on the platform *Responsible Research and Innovation in Scientific Practice* from January 2020 to July 2023, I was able to conduct some ethnographic observations for parts of the data collection process for Felt and Frantz's research project in October 2020. In October 2021, I also moderated a discussion group among physicists using the RESPONSE_ABILITY method, during which I also took some observational notes. However, this group was not part of Felt and Frantz's project.

As with the IMAGINE RRI transcripts, only specific sequences of the analysed group discussions were of particular interest. Consequently, I have once again incorporated those cards in Appendix III, which commonly prompted participants to recount authorship stories or to enter into debates surrounding authorship. As was the case with IMAGINE RRI, the other cards may also have initiated responses that were pertinent to the subject matter. The entirety of the supplementary material is also accessible on PHAIDRA, University of Vienna's online repository (Felt & Frantz, 2022b).

The value cards that researchers engaged with in the initial phase of the RESPONSE_ABILITY method often failed to elicit pertinent discussions for this thesis. However, three transgression cards introduced in the second phase, which participants were asked to order by decreasing relevance, frequently led to highly relevant discussions. These cards are titled 'Ownership of ideas', 'Co-authorship' and 'Power abuse', and frequently resulted in highly relevant debates. Because the "approach allows open discussion about how to navigate such borderlands of

good practice without moralizing behavior from the start” (Felt & Frantz, 2022a, p. 14), researchers may adopt a position with regard to the transgression cards that is at odds with guidelines regulating good scientific practice. A number of dilemma cards pertain to authorship-related conundrums, including those bearing the captions ‘Acknowledgement & Competition’, ‘Earning Co-authorship’ and ‘Honorary authorship’. As with the final phase of IMAGINE RRI, the concluding round delves into the implications of research conditions for research integrity and, in my case, authorship. My analysis indicates that the most pivotal cards in regard to authorship practices are ‘Lab/Group culture’, ‘Selling Research’, ‘Pressure & Quality’ and ‘Importance of Mentorship’. Furthermore, the participants in the discussion group that I facilitated in October 2021 could select one of three dilemma cards, with one card, specifically created by Ulrike Felt and myself, addressing the issue of ranking authors (see Appendix IV).

4.2.3. Interviews focusing on authorship

The third part of the data set comprises the 14 qualitative interviews I conducted with group leaders and postdocs in the life sciences and physics who were employed at the University of Vienna at the time of the interviews. Of these interviews, four group leaders and three postdocs worked in physics, while four postdocs and three group leaders were active in the life sciences. Given the underrepresentation of both group leaders and postdocs in the other two parts, it was evident that my data set would benefit from the inclusion of more senior researchers. Because these interviews all took place after the data collection for the two research projects, I was in the position to carry out a first and (partly) a second round of analysis of the transcripts of the 16 discussion groups in total, which informed my design of the interview questions (see the Appendix V and VI). Given that the first interviews took place a year and a half after the start of my doctoral project, the interview guide was also designed with the most recent iteration of the research question driving my project in mind. I understood the document with my questions more as a guide (i.e. I conducted semi-structured interviews), meaning that even though “the interview guide provides the same starting point given that it assumes a common set of discussable topics — each interview will vary according to what was said by individual interviewees” (Roulston, 2010, p. 14).

In regard to the interviews I conducted, it is possible to discern two distinct phases,

characterised by the formal position of the researchers who were questioned and the point in time at which the interviews took place. The first phase encompasses interviews conducted in October or November 2021, while the second phase encompasses interviews conducted between March and May 2022. Firstly, interviews were conducted with senior researchers who held the position of (deputy) group spokesperson or (deputy) head. In the empirical section, the acronym 'VU, PI' is used to indicate that the data has been collected by the author and PI, as an abbreviation for principal investigator. Secondly, researchers who had completed their doctoral studies and were employed at the university, the majority of whom held limited-term contracts, were the other subjects of my interviews. The researchers were primarily postdoctoral fellows, with two out of seven having unlimited contracts and therefore being formally regarded as senior scientists. It should be noted that the acronym indicating the data subset in question is 'VU, PDoc', the latter of which is an abbreviation for postdoc. The following paragraphs outline the approach taken to selecting the PIs and the considerations taken into account when contacting potential postdocs for interviews.

Using u:find, the online directory of the University of Vienna (n.d.-c), all (deputy) heads and group spokespersons of the Faculty of Physics and Life Sciences as well as the Centre for Microbiology and Environmental Science and the Centre for Molecular Biology were searched for. Furthermore, it was also stated if researchers hold a position at the corresponding doctoral school. In addition, the two lists I created, one for physics and one for life sciences, included ten scientists who were among the 'Highly Cited Researchers 2020' and were affiliated with the University of Vienna (University of Vienna, November 18, 2020). As might have been expected, all ten researchers are based in the fields of physics or life sciences. This reflects the general tendency to undervalue particular research areas, notably the humanities and social sciences, in the context of such rankings in academia (de Rijcke et al., 2016). As (deputy) heads or spokespersons, these researchers have certainly supervised doctoral students, led research groups and projects, had been involved in a large number of publications, and had been cited as (co-)authors numerous times.

The data was supplemented using the Web of Science, by including the number of publications listed there for each researcher as well as the h-index and the median citation percentile of the Author Impact Beamplot. This beamplot has been developed by Clarivate

and is promoted as a tool for responsible research evaluation “as it makes use of a field-normalized citation metric, does not unduly penalize researchers with gaps in their publication record, or disadvantage those who work in fields with distinctly different publication activity” (Szomszor, 2021). By developing this tool and emphasising its responsible features, Clarivate is responding to the numerous criticisms that have been levelled at the quantification of science, as discussed in Section 2.2.2.

The selection of potential interviewees from the two compiled lists of 80 researchers was made using different criteria. I was aware that the planned four to six interviews with PIs could never (fully) capture the diversity of practices (which was not the aim of this work). Nevertheless, it was desirable for the interviews to explore a certain range of authorship practices. Thus, one objective was to interview researchers working in a field that tends to have lengthy author lists, as well as others whose author lists are typically shorter. Furthermore, I deemed it enlightening to interview individuals with a notably high or low publication output in comparison to their peers, taking into account their academic age. Last but not least, the inclusion of a gender aspect was significant for me and I thus aimed for a relative gender balance in the selection of interlocutors, even though this was not representative of the two lists I compiled as a whole. From the literature reviewed, I was aware of the possibility of discrepancies rooted in gender roles with regard to the ways in which self-identified men and women negotiate authorship, or potentially even refrain from engaging in authorship conflicts (Sarsons, 2017; Tudor & Yashar, 2018).

The second round of interviews, which commenced approximately three months after the conclusion of the first round, entailed a comparable process of compiling lists of potential candidates and selecting them. The interviewees were researchers who had already obtained a PhD but had not yet progressed as far in their scientific careers as the first group, namely postdoctoral researchers. Once again, I utilised u:find and Web of Science and conducted further research on the respective homepages of the departments and/or research groups in order to obtain supplementary information concerning the postdocs and senior scientists. Following an exhaustive search, I compiled a list of 44 researchers affiliated with the Faculty of Physics and 143 researchers working in one of the following units: the Faculty of Life Sciences, the Centre for Microbiology and Environmental Science, or the Centre for Molecular

Biology. In a following step, the research groups or units in which previously interviewed PIs were involved were excluded. As with the selection process for PIs, my objective was to ensure gender equality. The male connotation of scientific excellence and the assumed greater responsibility for care activities were influential factors in this objective (Hanappi-Egger, 2012; Hönig, 2017; Nowotny, 2012).

The preceding interviews with PIs, already conducted and roughly analysed, heightened my sensitivity to the potential influence of international working experience on researchers' valuations of authorship practices (Davies, 2020). Consequently, I endeavoured to assemble a sample comprising researchers who had either pursued their doctoral studies abroad and/or had held a postdoctoral position outside of Austria within a research group. Furthermore, I assumed that researchers' narratives and valuations concerning authorship may differ depending on whether their current position is their first postdoctoral position or their second, third or even fourth. Thus, I attempted to interview both junior and senior postdocs, with two senior postdocs having an unlimited contract as a senior scientist.

4.2.4. Navigating qualitative data in a quantitative science system

There has been a notable increase in the use of quantitative approaches to study science, which can be seen in the growing fields of bibliometrics, informetrics, scientometrics, and webometrics. In light of the rise of neoliberalism, this development can be seen as a reflection of the "resurgence of assumptions and practices supporting the reification and bureaucratization of the logical-deductive paradigm of conducting research with its quest for quantification", as outlined by Charmaz and Belgrave (2019, p. 745).

In regard to my research case, I am firmly convinced that a qualitative approach is the optimal choice in light of my research questions. It would be erroneous to assume that every author contributing to a multi-authored publication mobilises the same value(s), ranks them in the same order and thus arrives at the same valuation outcome. Even if they reach the same valuation result, their justifications may differ as to why they or others are (not) rightfully authors and/or hold a specific place on the list of authors. A quantitative analysis of bibliometric data cannot account for these differences. Nevertheless, it is crucial to acknowledge that "[a]n interview is not a window on social reality but it is a part, a sample of

that reality" (Czarniawska, 2004, p. 49).

In addition, another premise of mine was that the disparate numbers (i.e. indicators) assigned to researchers based on their publication output and number of citations yield disparate narratives. This hypothesis was rooted in considerations of how a highly successful researcher, at least in terms of publication output, has either by default or through effort succeeded in aligning their valuations of contributions and authorship positions with those having a say in the creation of authors' lists. Moreover, these researchers have a rich pool of experiences regarding the formation of authors' lists. In contrast, I wondered whether the experiences of researchers with a shorter track record, despite being of a similar academic age, may encompass explanations for their relatively lower success in terms of publication output. Consequently, my attention was also drawn to the lines of argumentation and attributions of responsibility for their track record.

It is noteworthy that, as previously indicated, I employed quantitative indicators as a preliminary means of gauging the suitability of potential candidates for interviewing. Therefore, pursuing a qualitative approach in the context of my research interest is not entirely detached from findings rooted in quantitative data analysis. Indeed, the awareness and potential interconnectivity of the various interlocutors with their digital selves, as manifested in the form of a Google Scholar profile or a Web of Science Researcher Profile, surfaced in some of the interviews. For instance, a senior researcher made an incidental remark during an interview that they had consulted online sources to ascertain the number of publications held by their colleagues in analogous roles prior to applying for a grant. As this comment indicates, the digital manifestations of authorship decisions can undoubtedly impinge on the priorities of researchers, whether these are primarily epistemic motivations or strategic considerations, and on their self-conceptualisations. However, the implications of a quantified (digital) self can then be traced in qualitative data. Accordingly, my observation is consistent with the findings of Müller and de Rijcke who state:

[O]ur work makes clear how the increasing importance given to performance metrics in (e)valuation practices in the life sciences leads researchers in these fields to continuously think with indicators throughout the research process. (Müller & de Rijcke, 2017, pp. 159-160)

Ultimately, while I endeavoured to select interlocutors representing a diverse array of subdomains for my interviews, I did not attempt to elucidate and dissect all the authorship practices in their specific nuances nor did I endeavour to provide an exhaustive overview of the subject matter or to identify all the “situational features” (Trinkle et al., 2017) that exert an influence on authorship practices and on their perceived degree of responsibility. From the initial stages of my project, when I first immersed myself in the relevant literature, it was evident that, due to the outlined and assumed broad variety of situational features and conditions, as well as the diversity of authorship practices already identified in the literature, it was neither feasible nor sensible to attempt a comprehensive overview (Johann & Mayer, 2019; Moffatt, 2018). It may be hypothesised that a more efficacious systematic overview could be achieved through an in-depth analysis of large-scale quantitative data sets. Consequently, researchers’ sense-making processes and valuations have guided the direction of my investigation. Thus, the focus of my attention was on how researchers position themselves with regard to specific authorship decisions and how they may care for and, as a result, tinker with those practices.

4.2.5. *Tabular overview*

Below is a tabular overview of, for example, when the data was collected, what form of data was collected from which research subjects in the context of which project, in order to better illustrate this very diverse corpus of data. The table is presented in a chronological sequence, commencing with the data obtained at the earliest point in time.

Project	Description of data	Approx. number of pages of transcripts	Time of data collection	Institution(s) researchers belong to	Acronym used in empirical part
Imagine RRI	11 group discussions with PIs, postdocs, PhDs, master's students and research assistants from the life sciences	460 pages	08/2016 till 01/2018	CeMM ¹¹ as well as GMI ¹² , IMBA ¹³ , IMP ¹⁴ , and MFPL ¹⁵ Labs and researchers of the Faculty of Life sciences of the UniVie ¹⁶ , which are part of the VBC ¹⁷	IWS
Imagine RRI	15 interviews with PIs from the life sciences	380 pages	03/2018 till 11/2018	CeMM and GMI, IMBA, IMP and MFPL, both of MedUniVie ¹⁸ and UniVie, which are part of the VBC	II, PI
Imagine RRI	13 interviews with PhDs from the life sciences	190 pages	08/2017	GMI, IMBA, IMP, MFPL all belonging to the VBC	II, PhD
Borderlands of Good Scientific Practice. Investigating a Global Phenomenon in its Local Versions	5 discussion groups with PhDs, postdocs and master's students (2 discussion groups with life scientists	560 pages	11/2018 till 10/2020	BioTechMedGraz ¹⁹ (UniGraz ²⁰ , TUGraz ²¹ , MedUniGraz ²²), UniVie	BGSP

¹¹ Research Center for Molecular Medicine (CeMM)

¹² Gregor Mendel Institute of Molecular Plant Biology (GMI)

¹³ Institute of Molecular Biotechnology (IMBA)

¹⁴ Research Institute of Molecular Pathology (IMP)

¹⁵ Max Perutz Labs Vienna (until 2019 Max F. Perutz Laboratories, MFPL)

¹⁶ University of Vienna (UniVie)

¹⁷ Vienna BioCenter which comprises of six research institutions (GMI, IMBA, IMP, Max Perutz Labs, CeMESS and Faculty of Life Sciences)

¹⁸ Medical University of Vienna (MedUniVie)

¹⁹ BioTechMedGraz is the acronym standing for the research alliance of the UniGraz, the TUGraz, and the MedUniGraz (BioTechMed-Graz n.d.)

²⁰ University of Graz (UniGraz)

²¹ Graz University of Technology (TUGraz)

²² Medical University of Graz (MedUniGraz)

	and 3 with physicists)				
Partially research project BGSP and partially seminar 'Science career development II': Responsibility of and in research	Field notes taken during observation and moderation of discussion groups with physicists (master's students, PhDs and postdocs)	5 pages	10/2020 & 10/2021	UniVie	VU, BGSP
PhD Project Vera Ulmer	7 interviews with PIs (4 physicists and 3 life scientists)	140 pages	10/2021 till 11/2021	UniVie	VU, PI
PhD Project Vera Ulmer	7 interviews with postdocs (3 physicists and 4 life scientists)	100 pages	03/2022 till 05/2022	UniVie	VU, PDoc
	42 interviews & 16 discussion groups with master's students, PhDs, postdocs, PIs, and Research Assistants	1.835 pages	08/2016 till 05/2022	BioTechMedGraz (UniGraz, TUGraz, MedUniGraz), CeMM, VBC with GMI, IMBA, IMP, MFPL, both of MedUniVie and UniVie, and UniVie in general	

4.3. Constructivist-oriented grounded theory

The question of how to navigate the extensive data sets from both IMAGINE RRI and the Borderlands research project, with access granted from the outset of the PhD project, represented a significant consideration and challenge. At the beginning of my doctoral research, having access to such a plethora of data, collected by colleagues and superiors, was undoubtedly overwhelming. I experienced several instances of what Clarke and Friese (2007) label as “analytic paralysis” (p. 371). Following an exhaustive investigation, encompassing participation in a seminar on grounded theory conducted by Carrie Friese, constructivist grounded theory transpired to be the most appropriate approach. Additionally, I had previously utilised this method in my master’s thesis, providing me with a degree of familiarity and expertise.

Grounded theory itself was created by sociologists Barney G. Glaser and Anselm L. Strauss, who studied dying people in hospitals and published *The Discovery of Grounded Theory* in 1967 (Charmaz, 2014; Glaser & Strauss, 1967). Kathy Charmaz, a student of theirs, “introduced a variant” (Zaidi, 2022, p. 2) of their grounded theory, so to speak, coining the term ‘constructivist grounded theory’ and providing a detailed elaboration thereof in the two editions of her book *Constructing Grounded Theory* (Charmaz, 2006, 2014). Interestingly, Strauss’s understanding of grounded theory already contained some constructionist traces (Clarke & Friese, 2007). In the understanding of Charmaz (2014), since “grounded theory involves *reasoning* about experience for making theoretical conjectures — inferences — and then checking them through further empirical data” (p. 201), its methods can be considered abductive (Thornberg & Charmaz, 2013). Given that constructivists employ abduction with the aim “to identify patterns within empirical data and to make generalisations that derive from fine-grained evidence of particular and concrete cases” (Kosmützky et al., 2020, p. 180), thereby aligning with the research objectives set out in this thesis, the approach seemed well-suited. In the following subchapter, I will provide a more detailed account of why I have chosen to adopt a constructivist grounded theory framework.

4.3.1. *Making the case for the methodological approach*

Firstly, adopting grounded theory as a method of data analysis appeared to align with the

study's parameters, particularly given its iterative nature. Grounded theory's distinctive approach, wherein analysis commences prior to the conclusion of data collection, is a defining feature that facilitated its suitability for my data set (Charmaz, 2006; Charmaz & Belgrave, 2012). Accordingly, I began with an exploration of the extensive pre-existing corpus of data. In light of the preliminary findings of this ongoing "emerging analysis" (Charmaz & Belgrave, 2012, p. 348), I proceeded to complement and deepen specific aspects with a deliberate intent to "pursue these sites of silence" (Clarke & Friese, 2007, p. 372) in the 14 interviews I conducted in 2021 and 2022, while I commenced my doctoral studies at the beginning of 2020. It was therefore approximately one and a half years after the commencement of my doctoral research that I conducted interviews wherein the subject of authorship was a primary focus.

Secondly, my academic background, which encompasses bachelor's degrees in both Art History and Intercultural Business Administration, as well as a master's degree in Gender Studies, reflected a personal inclination towards a constructivist perspective. During the studies for my master's degree, I engaged with feminist critiques of science, a trajectory that ultimately led me to pursue a PhD in STS. In my master's thesis, which was grounded in feminist standpoint theory, I was sensitised to the various standpoints of my research subjects and myself (Haraway, 1988; Harding, 1991; Ulmer, 2019; Wylie, 2012). Since "constructivist grounded theorists engage in reflexivity throughout the research process, which fosters attending to the varied standpoints of both the researcher and the researched" (Charmaz & Bryant, 2010, p. 408), their approach resonates with my ontological and epistemological stance. My perspective has been shaped considerably by my three completed studies and my fourth ongoing doctoral studies in STS, a field that emphasises the situatedness of researchers (Law, 2017). This position was upheld not only during the process of data analysis but also throughout the interviews I conducted and the discussion group I facilitated, as I was aware that "[d]ata are co-constructed in intensive interviews, as with any method" (Charmaz, 2015, p. 1613). Or, in the words of Roulston (2010), the "[r]esearcher produces analyses of how the interviewer and interviewee made sense of the research topic and constructed narratives" (p. 60).

It is reasonable to posit that the postdoctoral and senior researchers I interviewed might have

provided slightly different accounts had someone of the same or a higher academic age questioned them. Similarly, the respective group discussion I facilitated with PhD students as participants, (who were, in a sense, my peers, even if they were from another discipline), might have yielded different insights with a different facilitator. In light of these considerations, I believe it is advantageous to have access to data collected by postdoctoral researchers or professors because they may have obtained slightly different narrations from the interlocutors due to their distinct positions within the academic community.

Thirdly, given that constructivist grounded theory is firmly anchored in pragmatism, it proved to be an ideal complement to the pragmatist approach to valuation that is employed in this dissertation (Charmaz, 2017; Heinich, 2021). As Charmaz (2017) elucidates, both pragmatism and constructivist grounded theory concur that reality is a social construct, situate experiences within the context of the social process, assume a reality that is not fixed or definite but rather malleable and in a state of flux, and postulate human agency, are aware of and interested in investigating multiple perspectives, and, last but not least, posit or empirically investigate processes. In light of these considerations, she encourages constructivist grounded theorists to “start with the assumption that social reality is multiple, processual and constructed” (Charmaz, 2014, p. 13). The pragmatic stance and the constructivist grounded theory approach are furthermore congruent with the concept of epistemic living spaces and correspond to my research focus on visions and practices of care and responsibility. In addition, an analysis of narratives guided by a pragmatist perspective aims to “result in representations of thematic findings generated via coding and categorization of data” (Roulston, 2010, p. 163), as is also prescribed in grounded theory.

Finally, the inherent comparative nature of grounded theory was another advantage, given the heterogeneous data sets make comparison inevitable. Hallberg (2006) even states how, in her opinion, the “constant comparative method, [...] can be seen as the ‘core category’ of grounded theory” (p. 143). It encompasses “making comparisons between data and data, data and codes, codes of data and other codes, codes and category, and category and concept” (Charmaz, 2014, p. 163). Moreover, comparison is so central and significant in this

dissertation that it warrants its own subchapter²³. The terms code, category and concept in the previous quote, however, point to the process of analysis prescribed by grounded theory. How this process unfolded in the context of this PhD project is explained in the following subchapter.

4.3.2. Process of analysis. Coding and creating categories through writing and mapping

Coding and categorisation in grounded theory are designed to facilitate the emergence of relevant issues that may not have been apparent at the outset of the analysis. In her work, Charmaz (2014) employs the concept of “emerging analysis” (p. 1) to illustrate the process of generating potentially new insights. However, the specific topics to emerge also depend on the analysts’ position, since “they *define* what is happening in the data” (Charmaz & Belgrave, 2012, p. 355). For this thesis, I alternated between working with the software ATLAS.ti and coding printed transcripts by hand, because I felt that each approach offered different advantages. For example, while using ATLAS.ti allowed me to quickly move between instances across different transcripts with the same code, working manually facilitated the appropriation of the data, so to say, in which I was not involved (at all) in either the conceptual or the empirical collection process.

Coding in grounded theory consists of “initial and focused coding [both of which] are emergent processes” (Charmaz, 2014, p. 144). While in the first phase, I attempted “to code with words that reflect action” (Charmaz, 2014, p. 117) and focused on smallest elements (such as words, sentences, or incidents) to mitigate the risk of drawing any analytical conclusions too early. Examples of initial codes I generated when going through the transcripts of the Borderlands project, from which I started my coding, included ‘adding persons/labs because of fame’, ‘addressing change impacting authorship’, ‘competing vs collaborating’, ‘experiencing pressure’, ‘overpowering supervisors’, ‘valuing being first’, and ‘negotiating authorship’ to better illustrate my process.

Having ended up with a very large number of codes, I began the second phase and moved

²³ See Subchapter 4.4.

on to focused coding, deciding which of the initial codes I had generated were worth pursuing further. One focused code that struck me as highly relevant to my research interest from the very beginning was a focused code that I named 'appreciating conceptual & technical activities'. The focused code emerged in the course of coding the transcripts of the Borderlands project. Repeatedly, the contrast between was often referred to as an idea (i.e. conceptual contribution) or work (i.e. technical contribution) by junior researchers was observed. This recurring narratively constructed dichotomy of idea vs. work, often combined with a reference to time, caught my interest early on and will be discussed in detail in Chapter 6. With the focused codes, I was also in a position to "sift, sort, synthesize, and analyze" (Charmaz, 2014, p. 139) the extensive body of data constituting the empirical foundation of my thesis. Because the vast majority of the data was not collated with a primary interest in authorship, line-by-line coding of the entire IMAGINE RRI and Borderlands project transcripts would have been neither advisable nor feasible.

The process of writing memos throughout the stages of my research, as recommended by proponents of grounded theory, proved invaluable in categorising the data I had gathered and in selecting which focused codes were worthy of further investigation on the basis of my research interests. As Charmaz (2014) notes, focused codes can be regarded as "tentative categories" (p. 189) and the analyst must then determine whether they warrant designation as categories for the present analysis. In the initial stages of the process, I wrote brief memos in ATLAS.ti. Some of these were subsequently expanded upon and used as the basis for papers written for the annual summer school for PhD students supervised by researchers at the Department of Science and Technology Studies at the University of Vienna, which I attended on four occasions. Alternatively, after manually coding particular transcripts, I transferred the most pertinent passages to a Word document, supplementing them with brief reflections of my own. Utilising the software Scapple also facilitated the mapping and clustering of codes, thereby assisting in the development of categories that "may subsume common themes and patterns in several codes" (Charmaz, 2014, p. 189). For instance, it was not until at least a year after the commencement of my doctoral studies that I began to recognise the potential for insight into the situated normalities of researchers through the lens of their narrative valuations. According to my Atlas.ti data, I embarked upon the process of coding such instances, in which the situated normalities of researchers became evident, in

May 2021. Similarly, my awareness and subsequent attentiveness to the multiplicity of authorship practices grew over time through my close engagement with the data. Even at a relatively advanced stage, I nonetheless reached the conclusion that a category of codes dealing with different aspects of publishing, such as 'exaggerating findings', 'valuing negative data' or 'valuing a publication', which I had initially considered for inclusion in a separate chapter with the preliminary title *The Publication Multiple*, would diverge from my primary research interest in authorship. As a result, I excluded it from my monograph, because it was superfluous to the main argument.

4.3.3. *Epistemic living spaces as a starting and finishing point*

All these various forms of engagement with my data certainly assisted me in reflecting on potential outlines for my monograph in general and the empirical section in particular. For that purpose, I selected and arranged particularly concise, previously coded quotes to illustrate the content of a potential (sub)chapter. Charmaz (2014) devotes an entire chapter to the subject of the writing process, given that a "grounded theory journey extends through the writing process" (p. 285). As an example, the process of finalising the structure for the empirical section of my monograph entailed a certain degree of challenge. Initially, I structured the empirical part along the five dimensions of the epistemic living space. While this structure yielded insights for a certain period of time and attuned me to how certain layers of the five dimensions are presented as modifiable while others are immutable, it was ultimately replaced with a structure based on the three identified valuation strands: valuation of contributions, authorship positions, and authorship decision processes. However, the composition of the second chapter along the five dimensions, which was undertaken subsequent to the division of the empirical part into three chapters, proved to be an invaluable approach for the presentation of the existing body of literature in its complexity.

Hence, it would be remiss not to acknowledge the profoundly positive effects of the concept of the epistemic living space, as well as the substantial impact of my understandings inspired by pragmatism, in relation to not only valuations and narratives but also care and responsibility. The influence of the concept of epistemic living spaces may have been particularly pronounced, because it accompanied me even before the commencement of my PhD project, having been utilised in my master's thesis. In contrast, the relevance and my

understanding of the other concepts became more apparent over time, as their employment proved fruitful. Hence, I followed Charmaz' (2006) recommendation to only employ "extant concepts [...which are] integral for understanding your data" (p. 68).

To elaborate, it was only through engaging with both the concept of epistemic living spaces and valuation studies that I was equipped to discern the instances within researchers' narratives wherein they articulated how authorship practices are informed by the spatio-material, temporal and social dimensions, or alternatively, expressed valuations. Furthermore, how the five dimensions of epistemic living spaces — the spatio-material, the temporal, the epistemic, the social, and the symbolic — matter in the narrative valuation processes of researchers is also illustrated by the frequent occurrence of the expression 'it depends' or variations of it, both in the discussion groups and interviews analysed, as well as in the interviews conducted that focused on authorship. This observation is in line with an argument made by Akrich and Rabearisoa (2016), since, according to them, the statement 'it depends' is an indicator of how "every phenomenon is made of cascades of relations between heterogeneous elements, including those usually considered as exogenous factors" (p. 136). Moreover, these elements can be perceived and thus presented in researchers' narratives as 'given', i.e. 'natural' and therefore immutable. Or, conversely, certain aspects are portrayed by researchers as modifiable — either by them or by others. Whenever modifiable, and thus improvable, elements are pointed out that, from the researcher's perspective, degrade the various authorship valuation processes, (envisioned) practices of care and responsibility are implicitly discussed and negotiated. Authorship valuations and their embedded practices of care and responsibility thus co-produce each other. In conclusion, the insights referred to in this section are rooted in insights crystallising well into the process of analysis and are only alluded to in this section to better illustrate the striking impact of the sensitising concepts, in particular the concept of the epistemic living spaces.

4.4. Comparing data and comparisons within data

The diversity and volume of my data set have undoubtedly posed a significant challenge, but upon concluding both this chapter and my analysis, I am confident that the diversity of the data set has enhanced the rigour of my analysis and the strength of my findings. A challenge

inherent to my data set, collated from two discrete research projects and augmented by a series of interviews, is my comprehension and utilisation of comparison. In some respects, one might contend that I endeavour to pursue a process that is analogous to the quantification of science, because it can be viewed as a “process of redefining our own worlds in terms that make them available for its comparative operations” (Stengers, 2011, p. 58). In this regard, when comparison is conducted in a manner akin to a forced imposition upon a collective, it becomes a subject of considerable scrutiny. To illustrate, during the 1960s and 1970s, the practice of comparison, particularly within the field of anthropology but across the social sciences more broadly, was frequently regarded as an “extension of colonialism” (Deville et al., 2016a, p. 21). However, a constructivist stance towards research, which is often present in feminist research and STS, is aware of the potential pitfalls of practicing comparisons. In order to counteract these shortcomings, a higher degree of reflexivity and sensitivity is employed alongside the recent reembrace of comparison (Deville et al., 2016a). Since one understanding of comparison is “an aspect of any kind of practice that invokes knowledge in the sense that all information implies some version of comparisons to other possible realities” (Krause, 2016, p. 45), a deeper engagement with the practice of comparison in general is vital. Furthermore, on the grounds that “STS is doing comparison” (Deville et al., 2016a, p. 24), it is imperative for a dissertation within this field to undertake a comprehensive examination of the subject matter (Kosmützky et al., 2020). Finally, with the data collected from two distinct research projects and subsequently augmented by the 14 interviews conducted, another compelling rationale exists for contemplating the nuances of comparing qualitative data and comparisons within data.

According to Kosmützky et al. (2020), researchers undertaking the comparison of qualitative data often follow “methodological choices based on constructivist [...] ontologies and epistemologies” (p. 180), coinciding with my way of thinking and the applied constructivist grounded theory. In engaging with the unavoidable comparison of the heterogeneous data subsets, I sought to adopt a perspective that recognised the act of comparison as “a practice of bringing material together and putting it in conversation, and not simply an assembly of empirical data with different characteristics” (Deville et al., 2016b, p. 106). So, what kinds of comparisons did I perform and why, and which comparisons did I encounter in relation to my corpus of data? A recapitulation of how I ‘made’ the corpus of data comparable and

incomparable is crucial, considering the position of Stöckelová (2016), but also of many other researchers: “(In)comparability is not a perpetual, given feature of the phenomena we investigate, but a result of *framing(s)* enacted by various actors — researchers included.” (p. 166).

Firstly, the present study compares two distinct data formats: namely, interviews and group discussions. Although one potential avenue of inquiry might have been to compare and contrast the two data formats by identifying their differences, I recognised through a process of comparison how there is one relevant and similar aspect present in the entire data set. Namely, researchers exhibited similarities in terms of positioning and the manner in which they constructed their identities through interaction with either other group participants, the facilitator, or the interviewer. My sensitivity towards these dynamics was also informed by engagement with pertinent literature. In order to gain a comprehensive and accurate understanding of the discussions held within the groups, it is of the utmost importance to consider the level of interactivity and performativity demonstrated by the participants themselves during the course of the discussions (Halkier, 2010). Indeed, as Barbour (2013) asserts, “focus group participants are engaged not just in presenting their own narratives, but in supporting or challenging others’ narratives, forging, testing and occasionally repairing relationships along the way, and, sometimes, in co-constructing accounts” (p. 320). Hence, a fruitful avenue for analysis is the close exploration of any contradictions and quandaries emerging in group discussions, as Barbour (2013) suggests. Ultimately, the analysis of the accounts, some of which are co-constructed, offers an insight into their perception of their standing within the scientific community and, consequently, their self-conception. Unlike the group discussion participants, the respective interviewers assumed a distinct and comparatively neutral role, more akin to that of group discussion facilitators. Nevertheless, an inherent characteristic of interviews is the performance of the interlocutors, since any “interviewees present themselves to their respective interviewers and, however silently, make and negotiate identity claims” (Charmaz & Belgrave, 2012, p. 350). Furthermore, as Czarniawska (2004) postulates, “we are never the sole authors of our own narratives; in every conversation a positioning takes place [...] which is accepted, rejected, or improved upon by the partners in the conversation” (p. 5), even when the conversation occurs in the context of an interview.

Secondly, the principal topic of interest among the researchers responsible for establishing the thematic framework for the group discussions and interviews of both IMAGINE RRI and the Borderlands project was not authorship. To illustrate the proportions, authorship was not a focus in any of the 16 group discussions studied and was only a central theme in 14 of the 42 interviews examined. Therefore, one might question of whether these data sets are comparable to begin with. However, the question of authorship and the attendant valuations were addressed in all interviews and group discussions to a greater or lesser extent and constituted a central thematic element on several occasions — this is also because there were cards or questions that directly addressed authorship or closely related issues (see Appendix II, III and IV). Hence, one may conclude rather than comparing the whole 16 group discussions and 28 interviews with the authorship-centred 14 interviews, I compared the relevant segments of the former with the latter. In this way, I agree with the reasoning of Deville et al. (2016a) that “comparison as a research practice is necessarily *reductive*, and this is not in itself problematic” (p. 26). Moreover, I would argue that precisely because authorship issues and practices were not the primary focus of any of the group discussions or by far the majority of the interviews, the fact that authorship was nevertheless raised without explicit prompting by the facilitator, by the cards used in the group discussions, or by direct questioning by the interviewer indicates its significance in the lives and work of researchers. Nevertheless, through comparison, I noted how the narratives present in the interviews I conducted were more nuanced, while in the other data sets, the narratives mobilised more resembled each other. In retrospect, this may appear unsurprising, given the framework in place, whereby the facilitator or interviewer did not repeatedly urge the researchers to probe further and, subsequently, encouraged the group participants to move on from a debate on authorship practices after a certain period of time. Nevertheless, this observation corroborates the existence of widely circulating authorship narratives with which even PhD students are conversant. The act of citing such narratives may therefore serve two distinct functions. First, they may serve as a reference point when recounting experiences and, second, the narrator may adopt a position in relation to these narratives (Sigl et al., 2020). The significant volume of data analysed thus allows a confident assertion to be made — namely, that the most prominent narratives are inherent in the data set and therefore form part of the findings. Additionally, through my analysis of the interviews, my sensitivity was heightened with regard to researchers’ sense-making processes and valuations, which were more pronounced in these

interviews. Conversely, this enhanced sensitivity also enabled me to discern the less distinct narratives present in the remainder of the data.

Thirdly, in contrast to the aforementioned two types of comparison, where the “comparative criterion — the so-called *tertium comparationis* — that makes differences in the (partially) identical units observable” (Kosmützky et al., 2020, p. 179), was predetermined through the configuration of the data corpus, the comparative criterion most relevant to my research interest only emerged through an in-depth examination of the data. Moreover, in the initial comparison, I identified the similarities between the various data formats, specifically how researchers construct their identities and position themselves through interaction. The second comparison enabled me to recognise the commensurability of the data subsets. Despite the diverging topical foci, the same authorship narratives recurred, differing only in their sophistication. In contrast, in this third comparison, I extracted patterns of difference rather than similarity, employing a comparative criterion that was not fixed at the outset of my PhD project.

While initially contemplating the potential value of a comparison between the disparate research areas represented in the data corpus, I soon ascertained that this avenue of investigation was neither feasible with the available data nor pertinent to my research question. Furthermore, in a relatively early stage of my research, I encountered the argument put forward by Hangel and Schmidt-Pfister (2017) that “[d]isciplinary differences appear less distinct than differences between collaboratively or solitary organized researchers” (p. 532). This finding served to reinforce my decision to refrain from making this comparison. Given the virtual exclusivity of my interest in researchers working with at least one colleague on their publications (and the resulting lack of publications with a single author), a comparison between researchers with single-authored publications and multi-authored publications was not a viable avenue of enquiry for me. Career stage emerged as a comparative criterion. While was partly inspired by Hangel and Schmidt-Pfister (2017), it was more than confirmed in my analysis, as it also revealed that researchers’ career stage is a clear demarcation line. As such, it echoes their assertion that there are “more distinct differences across career stages” (Hangel & Schmidt-Pfister, 2017, p. 532). In his research, Johann (2022) arrives at a congruent conclusion, identifying discrepancies in the criteria for authorship between junior and senior

researchers.

I wish to emphasise that, while I initially assumed such a distinction, possibly influenced by my role as a PhD student in an academic setting and my simultaneous socialisation into academia, the researchers themselves consistently compared and contrasted their experiences with those of their peers at different stages of their scientific careers, as well as with their peers at the same stage. However, with regard to the question of what researchers' narratives on valuations of authorship practices are and how they reflect in their visions and practices of responsibility and care (i.e. concerning my overarching research questions), the findings differed significantly between career stages.

Fourthly, a second relevant comparative criterion emerged through a comparison of the narrative sense-making processes and valuations of researchers at the same stage, particularly of postdocs and group leaders. The extent to which a researcher perceives the process surrounding authorship to be relatively unobstructed, a perception often reflected in a successful track record for the researcher's position within the scientific system, emerged as a pivotal criterion for my research focus. As with the criterion of the career stage, this finding may not be particularly surprising. Nevertheless, it was instrumental in identifying a core value guiding researchers' valuations of authorship decision-making processes.

Finally, interestingly, it is not only I, the analyst, who performs comparisons, but both the group discussion participants and the interviewees who often make sense of and value authorship practices in their narratives by comparing the practices they experience and observe. Telling stories involves the "negotiating and renegotiating [of] meanings by the mediation of narrative interpretation" (Bruner, 1990, p. 67), which can be accomplished through comparison. Consequently, the data set encompasses a diverse array of stories in which the narrator attempts to comprehend an authorship practice they have either encountered or have been made aware of. Hence, within their narrations researchers may have mobilised "observations made on one site, as lenses through which to discuss what occurred in other sites" (Akrich & Rabeharisoa, 2016, p. 140). The comparative work that both the researcher and the researched researchers engage in promotes relevant findings, since comparison "inevitably involves the forging of new connections between objects, persons,

and many other entities besides" (Deville et al., 2016a, p. 35). and the production of a "comparative relationality" (Deville et al., 2016a, p. 35). Or, in the words of Meyer (2016), "comparisons hold together different practices, places, and temporalities" (p. 300). With this fourth type of comparison being less a methodological choice than a finding, it is further explored in Subchapter 5.4.

PART III A COMPREHENSIVE INVESTIGATION OF AUTHORSHIP VALUATIONS

5. Introduction to Part III

5.1. The reflection of the epistemic living spaces in researchers' processes of sense-making and valuation

PDoc2f: [B]ut it is also like I feel, I mean, still science is bit of a hierarchical process. In many ways this is a bit eliminated but, I think, especially when it comes to, you know, wrapping up studies into publications like when you're starting out as a PhD student. And if there are political issues at hand especially regarding the authorship. It becomes a bit tricky to raise issues, you know, especially if you don't necessarily have the support of, let's say higher upsters because, you know, then it's a bit of a game of who can twist arms the best. PhD2f:/yeah/ Or sort of, at least, or being the reason for the whole thing. Yeah. I know. It's a, I mean, at the end I think, of course, the person that really probably puts in the most effort is, I mean, the person, the person that of course I mean have maybe done certain experiments but really putting the paper together, I mean, it depends a bit in the situation but at least in [] you know, I've overseen the whole process of really writing the drafts and really getting, you know comments from other co-authors and really integrating this all thing into one piece and then making the figures and editing and writing the revisions and all this. So, it's like in the end, I think of course, the supervisor is there, and you might discuss all this with him but, I think the final things is one or two people that really do the proof checking of the whole thing and but in the end, yeah. It's a bit, it depends a bit, I mean this is an individual situation of how you.

PhD2f: It's also the problem what, what kind of action should warrant authorship. I think this is what it you mentioned: the clinicians that perform a surgery and take out a piece of tissue and want to be on a paper for this [...]

PDoc2f: It depends. Yeah, I mean...

PI1m: The question is, have they like, would the paper exist without them? (IWS1, p. 21)

This exchange between three researchers in the field of life sciences at varying stages in their scientific careers is exemplary of the complexity of researchers' valuation processes in the context of authorship. Reading this snapshot allows a brief glimpse of the three different kinds of valuation processes that were identified in the comprehensive data analysis and which are consequently explored in this monograph: Researchers' grappling with how to value contributions, reflecting on who has the final say in authorship decisions, and, implicitly in this extract, valuing different authorship positions. Moreover, this short peek into researchers' valuation processes suggests how the five dimensions — the spatio-material, the temporal, the epistemic, the social, and the symbolic — shape researchers' epistemic living spaces and

are in interaction with authorship practices, which researchers particularly explore through the sense-making processes inherent in their narratives. As set forth in the first two chapters of this monograph, the concept of the epistemic living space, as developed by Felt (2009b), serves as the conceptual foundation, thereby facilitating a more nuanced analysis of the processes through which researchers make sense of and value decisions regarding authorship. In order to facilitate a more pleasurable reading experience, the term 'dimension' is employed in addition to expressions such as the 'epistemic realm', 'symbolic rationality', the 'social sphere', alongside notions such as spatio-material and temporal aspects, in order to refer to a dimension of the epistemic living space.

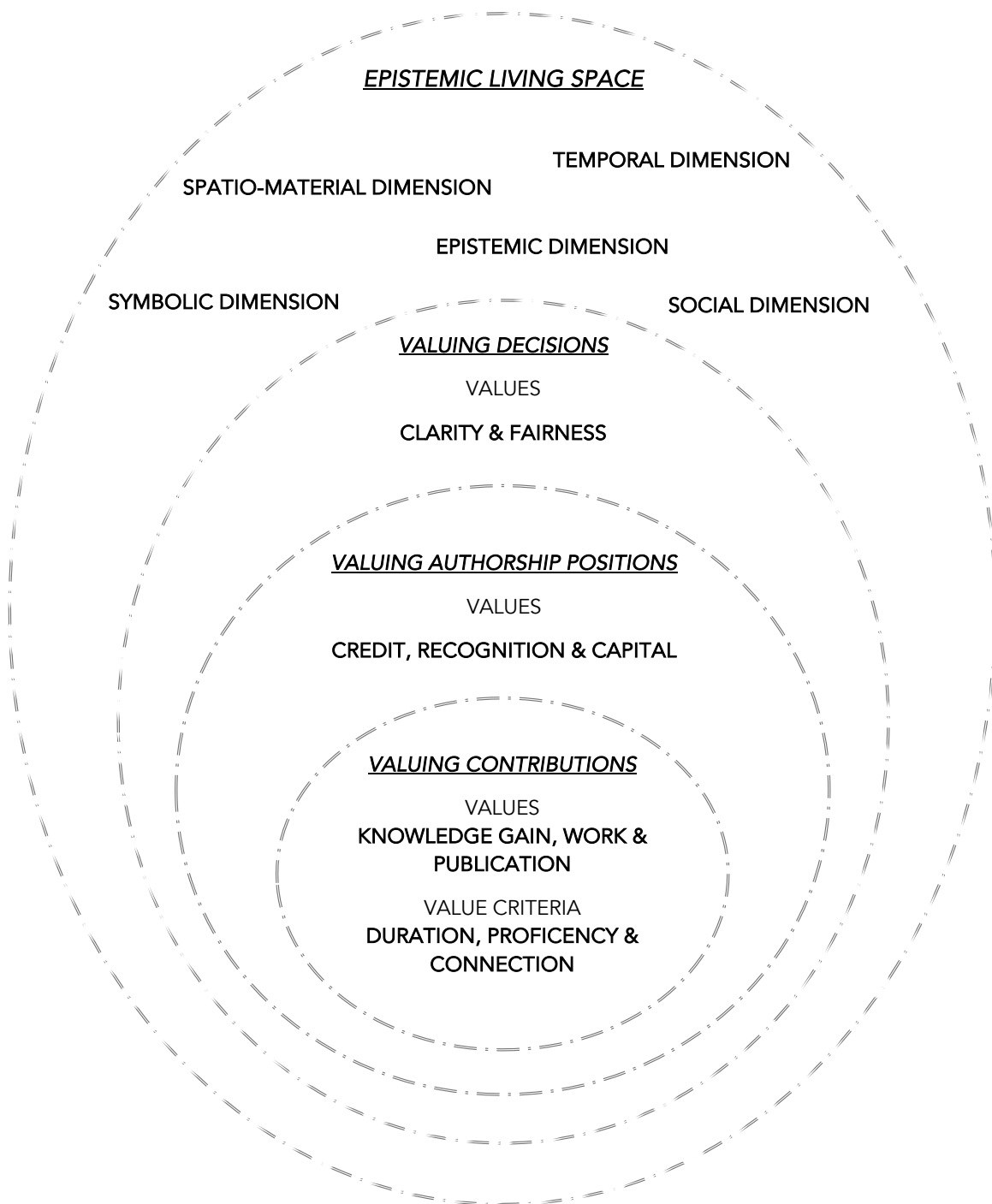
While the spatio-material, temporal and social dimensions alert us to the elements shaping researchers' situated normalities, which may provide avenues for caring for others or assuming responsibility, the epistemic and symbolic dimensions attune us to the values and possible value orders. The social sphere plays a distinctive role, insofar as some of its aspects are portrayed as immutable. However, it is within this sphere that the practices and visions of care and responsibility are deeply entrenched. Consequently, the five dimensions may be experienced by researchers as impeding their processes of valuation, which is evident in the aforementioned exchange. Several other key ideas can be drawn out as well: relations are structured by hierarchies, the symbolic status in a research field may play a role in authorship negotiations, materialities require certain research practices, and a range of different contributions may be needed to publish a paper. Furthermore, researchers may be dispersed across different spaces and dependent on collaborators who may be working in a different research area or even a different discipline, to put forward but a few of the relevant ways in which the dimensions can inform how researchers value contributions, authorship positions and, ultimately, authorship decision processes. In addition, these dimensions may inhibit, require or encourage specific (envisioned) practices of care and responsibility, which, in turn, impinge on researchers' valuations.

5.2. Outline of the empirical part

The structure of the empirical part of the thesis has been continuously refined and is closely aligned with the findings from the analysis of researchers' narrative authorship valuation processes. Accordingly, the empirical section is divided into three chapters, mirroring the three identified authorship valuation processes, and are as follows: researchers' valuations of contributions (Chapter 6), authorship positions and thus rankings (Chapter 7), and their broader valuations in relation to the overall authorship decision-making process surrounding authorship (Chapter 8). Each of the three chapters begins with a brief introduction to the values identified in the analysis. The chapter focusing on the valuation of contributions briefly presents the associated valuation criteria that were identified.

In order to facilitate the reader's attunement to how these values shine through in the chapters preceding, said ascertained values are shortly stated herewith: *Work*, *knowledge gain* and *publication* are the identified values guiding the valuation processes of contributions with *duration*, *proficiency* and *connection* as the accompanying valuation criteria supporting researchers in their justifications of whether authorship is adequate from their standpoint. Subsequently, *credit*, *recognition* and *capital* constitute the foundation for the valuation of the various authorship positions. Ultimately, the values of *clarity* and *fairness* pervade all valuations, but are most pronounced and presiding in the valuations associated with the processes surrounding authorship decisions. Furthermore, the various dimensions constituting an epistemic living space may facilitate, impede, or inform researchers' processes of sense-making and valuations. Indeed, it can be argued that these processes may co-produce the dimensions.

To visually represent this intertwined relationship between valuation processes and dimensions, a graphic was designed, inspired by the social worlds/arenas map, created by Clarke (2005, p. 109 ff.):



However, the focus of these three chapters is on how these values and criteria are not only mobilised, but also negotiated and contested in the valuation of researchers. In other words, I attempt to map the multiplicity of authorship valuations. By being aware of the “inextricable interdependence of epistemic practices, institutional rationales, individual biographical decisions, and political frameworks, which characterizes the lived experienced realities of researchers today” (Felt & Fochler, 2012, p. 136) and thus the researchers’ valuation processes, the interconnectedness is not unconsidered and subsequently also discussed throughout the chapters. In accordance with Akrich and Rabeharisoa (2016) I thus “pay

attention to the ways these relations are actively unfolded in the situations under study” (p. 137) because “these relations do not predetermine the subsequent story, but rather result from the involvement and action of various actors” (p. 136-137). This emphasis on the involvement of and action by actors further underlines the situatedness of those actors (Haraway, 1988).

The analysis was conducted as openly as possible, as outlined in detail in Chapter 4. Simultaneously, that analysis was informed by the cross-cutting sensitising concepts of narrative, valuation, responsibility and care (see Chapter 3). In light of the aptness of its content, I enclose a quote by Boltanski and Thévenot with the intention of providing the reader with a succinct illustration exemplifying what I attempted to achieve in my analysis of the material:

[T]he researcher is obliged, in her description, to adhere as closely as possible to the procedure the actors themselves use in establishing proof in a given situation; this approach entails paying careful attention to the diversity of forms of justification. By following the actors, researchers are thus led to make explicit a greater variety of forms of justification. (Boltanski & Thévenot, 2021, p. 12)

For this reason, the following chapters contain a multitude of direct quotations, ranging from brief excerpts to a few words or even a single word, illustrating the degree of detail in the analytical approach employed. This style of analysis allows for a very nuanced understanding of the values and justifications that shape the authorship valuation processes of researchers in the life sciences and physics.

5.3. Consequences of the interconnectedness

Permeating the three empirical chapters is the interconnectedness of valuations²⁴. With the aim of further enhancing the reader’s awareness of this issue and providing illustrations of the consequences of the interconnectedness of valuations on the work and life of researchers, this subchapter presents several distinct empirical examples. The temporal dimension exerts an influence, insofar as valuations can be shaped by past or anticipated future valuation outcomes. Moreover, these valuations can be connected by researchers through referencing

²⁴ This has been touched upon from a theoretical standpoint in Subchapter 3.2.

the symbolic rationality, whereby authorship in general, and specific authorship positions in particular, are conceptualised as a form of symbolic capital that is a valuable component in the assessments conducted within academia (see, for example, Fochler, 2016; Latour & Woolgar, 1986; Laudel & Gläser, 2008).

Reflecting on whether a valuation is retrospectively or prospectively oriented is fruitful when analysing researchers' valuations because this adds another layer of depth to the understanding of the subject matter. Prospective valuations of contributions, for instance, take into account the interconnectedness of valuations and may adjust current judgments accordingly if those judgements might negatively affect future valuations and assessments. For instance, one postdoc recounts that it is customary to attempt the inclusion of long-standing collaborators and/or some of their group members, even in instances where their experiments proved unsuccessful for the present publication. The positive valuation of their past contributions, and the expectation of valuable future contributions, supersedes the limited value of the present contribution. Alternatively, researchers discuss how their inclination to sustain a collaboration curtails their inclination to initiate an authorship dispute, as they are aware of their future reliance on a specific piece of equipment.

A further illustration would be how a perceived injustice subsequent to the establishment of an author list can affect a subsequent valuation process. A group leader in the life sciences recalls how she listed a PhD student of hers as first author in a paper because, looking back on the authorship ranking for a previous paper, she came to the conclusion "that was almost a bit unfair in retrospect, towards the person, towards the PhD [to not share first authorship...] and then I put him first, as compensation, so to speak" (translated by the author¹; VU, PI3f, p. 13). In this case, the group leader assumes responsibility for ensuring fair authorship decisions, one identified guiding principle underlying the valuation of authorship decision processes. This example is in line with the observation that when researchers reflect upon the interconnection of valuations, possibly through the lens of symbolic rationality, it is often coupled with researchers assuming or ascribing responsibility. This attributional process, in most instances, is accompanied by a reference to the formal position of the self or others, with the aim of either elucidating or justifying it. The responsibility, however, lies in enhancing the alignment between the outcome of valuations of contributions and of authorship

positions. Such practices or visions of responsibility serve to establish a valuable connection between the various judgments. Moreover, they exemplify the “interesting dynamic [...] as to characterising the folding of valuation practices as proper or improper, legitimate or illegitimate” (p. 99), to which Helgesson (2016) calls attention and which is particularly evident in both Chapter 7 on positions and Chapter 8 on decision processes.

5.4. Valuing through comparing spaces: Judging what constitutes

‘normality’

Finally, before embarking upon an in-depth analysis of the three identified valuation processes, this brief subchapter introduces and addresses the most prevalent mode employed by researchers in their narratives to facilitate and structure their valuations. The deployment of comparisons between material, virtual, or metaphorical spaces serves to draw out a particular issue, which may not inherently be spatial in nature. Conversely, through the process of comparison, researchers may reinforce their judgements as to how a particular phenomenon constitutes their situated normality. The idea of normality and its function in narratives and valuations is explored from a theoretical perspective in the respective subchapters of Chapter 3. Furthermore, the act of comparison is essential to the process of making sense of and judging what is perceived as normal. Indeed, as Villman (2021) observes, “[n]ormality always refers to a context; it is only normal in comparison to something” (p. 84). However, as a fundamental assumption of this monograph is the situatedness of researchers, inherent in the concept of the epistemic living space, this is further emphasised by the utilisation of the notion of situated normality. Interestingly, the utilisation of the terms ‘normality’ or ‘normal’ may signify elements that researchers identify and subsequently portray as an immutable condition in their narratives, and which subsequently impinge upon authorship decisions.

To return to the subject of comparison, researchers trying to make sense of how material aspects (do not) matter in their authorship practices may resort to comparing their field of research with other subfields or even to other disciplines in their narratives, thus using a spatial metaphor rather than talking about literal spaces. A group leader in theoretical physics, for example, describes his own field of research as “not typical” (VU, PI4m, p. 2) since it does not

require funding to set up and maintain a lab, which in turn shows how infrastructure-heavy research fields are perceived as normality. Interestingly, even though he works in a field where non-alphabetical author lists are the exception, it still “makes sense” (VU, PI4m, p. 2) for him to list the researcher who is responsible for raising funds for the infrastructure of a lab on publications coming out of the lab. He thus justifies an authorship practice that he perceives as ‘typical’. For a postdoc who also works in theoretical physics, the practice of rewarding the acquisition of funds with authorship is less understandable. For her, it is normal and perhaps even ‘good’, as the undertone in her story suggests, to not automatically co-author a paper with one’s supervisor as a PhD student, solely because the supervisor provided the funding. The notion of normality runs through many of the researchers’ comparisons, often introduced by comparing (sub)fields and declaring one ‘normal’ and the other a deviation from normality. A life scientist, when recounting his experience of the contributions of researchers on the last authorship positions, emphasises in his story that “the last authors were also involved in the content. So not like in medicine, that’s the director of the institute, that’s why he’s at the back” (translated by the author²; VU, PDoc4m, p. 15).

In terms of indicators, researchers may also make comparisons between their research subdomain and other subdomains belonging to the same overarching research field. To exemplify, when deliberating on the subject of potentially inappropriate review practices, a junior physicist proffers the observation that “in the Cold Atom Community it definitely happens” (BGSP 5_1.2, p. 21). Also, in light of the significance attributed to a given publication or authorship position, comparisons may be drawn. For instance, a theoretical physicist notes that “the focus on the paper, where you are in the authors’ list is quite high in our field, because our papers are all we produce” (BGSP 6_1.1, p. 30). A group leader from the field of physics clarifies her ongoing writing of project applications with the observation that she is not engaged in a field “where you will get impact factor or h-index to the level where you can compete with people [from the other subdiscipline...]. So, it’s just a different area, so, the level of citation is different. But the competition is there” (VU, PI7, p. 11). A life scientist also perceives similar disparities in the life sciences, which result in inequalities in the allocation of resources:

And as long as we give impact points on or resources on impact points, we are misguided.

Because there is a difference between being in molecular biology or stem cells, where I can publish any nonsense with the highest impact, and being in a solid biochemical science, where I can only publish with a tenth of the impact and then get kicked out because of it. (translated by the author³; II, PI20m, p. 40)

The perceived unfairness is attributed to discrepancies across research fields, which are not captured in impact points and other metrics. This issue is a significant concern for researchers and arises at various points throughout the analysed data corpus:

[I]n our research it's kind of really detrimental, because you work in these huge collaborations, on these huge experiments that take forever, but you are just measured at the same level kind of with people who have permanent output like you like you, you make crystals, I don't know how long it takes to make a crystal, but let's say a month and then you can publish a paper a month, whereas we have collaborations who work on, I don't know, for example the huge LSC machines for 20 years and then you can publish one paper. (BGSP 5_1.2, p. 29)

Researchers may also cite experiences gained in other work environments to illustrate why certain aspects are of particular importance to them, which is evidenced by the following comment:

I've had two very opposite experiences with my previous job and the project I'm working on now, and the group culture was very different, and I perceived the one in my previous job as negative, so this is something that is extremely important to me, that now I feel really good in my group. (BGSP 5_1.2, p. 26)

Such comparisons may also entail contrasting the authorship decisions encountered in one research group with the prevailing practices in another group. Or they might also mention other countries in their stories and illustrate, for example, through comparison, how the relevance of authorship practices is also shaped by the terms and conditions of employment contracts. To illustrate, a researcher who has been immersed in the field for an extended period of time offers a favourable comparison between her present working environment and her previous one abroad. She asserts, that "it is very much the academics who are the dominant driving force in academic life; not managers, not administrative stuff" (VU, PI6f, p 11). These moments of comparison may indicate what a particular researcher perceives as 'normal' and may also include judgements as to whether or not this normal is considered 'good'. Moreover, researchers may compare their own profession with other areas of work:

[B]ut I also do have enough freedom to think about new stuff and then maybe, depending

on the funding situation, I cannot do too much outside of, you know, like project that is not expected to be published, but I am happy enough that, you know, that the system is as such in comparison to, for example, dentistry work. I have enough freedom to do many things, so in a way I perceive that it's still fine. (II, P114f, p. 23.)

Through such comparisons, the respective individual provides a justification for the value they ascribe to their profession. In this case, the researcher highlights the greater freedom they have in comparison to dentists, while also addressing the common criticism that researchers' freedom is rather limited because of the prevailing symbolic rationality. Consequently, researchers may even contrast the present state of academia with an imagined past, which is typically portrayed in a favourable light (see for example Ylijoki, 2005). To illustrate this point, one researcher notes that the present scientific system "definitely excludes some kind of research that was, was probably more prominent in the past when scientists were bored aristocrats, I mean (laughing) it's a completely different approach" (BGSP 6_1.2, p. 50).

In essence, researchers' perceptions of their situated normalities in relation to authorship, a judgement that is likely to have emerged from an articulated or unarticulated comparison between different experiences, or experiences with observations or stories heard, or with ideas or aspirations about how science in general and authorship in particular ought to be, permeates the following three empirical chapters that form the core of this thesis. Researchers' situated normalities can also enhance our understanding of why they strongly advocate for a value, be it in guiding valuations of contributions or positions, or authorship decision processes, as they are ultimately expressions of how to enhance their situated normalities.

6. To be or not to be an author: Researchers valuing contributions

[I]t's always kind of a fight between the, whoever wrote the article or did the work, for example the PhD student, and then the PhD student knows who contributed the work and who should really be co-authors; and there's always this fight with your supervisor, who should be on the paper and who should not be on the paper. And I think most of the time we add too many co-authors that should not be on the paper. They could just go to the acknowledgements. (BGSP 1_1.1, p. 12)

Conflict over authorship, and the lines of conflict that frequently run between junior and senior researchers, as well as the negotiation of whether or not work is a value, are issues that this quote alludes to, and which are thoroughly explored in this chapter.

Contributions are translated into authorship based on the value ascribed to them by researchers. Value is allocated based on the values of work, knowledge gain and publication, which are, in turn, used to justify the decision of whether authorship is justified. In certain situations, such as the comparison of different contributions, researchers also use valuation criteria, namely duration, proficiency and connection. These criteria in general serve to emphasise that a critical threshold has been met, thereby justifying authorship. Alternatively, a certain value may be attributed yet correspond to a mere mention in the acknowledgements. Alternatively, the valuation outcome may be that neither authorship nor a mention in the acknowledgement is justified. Moreover, this chapter explores the perspectives of researchers on the matters that, in their opinion, hinder or potentially impede the process of valuation. Firstly, however, an elucidation of the observed values and valuation criteria follows.

6.1. Work, knowledge gain and publication as values

In many cases, the notion of work is raised by researchers when valuing contributions or when reflecting on how to value contributions. In certain instances, to substantiate the inclusion of the contributing researcher as one of several or numerous co-authors, it may be sufficient to assert that the production of the contribution necessitated work. For example, a postdoc justifies the value she assigns to a master's student's contribution by stating that "it's a lot of

work to make these" (VU, PDoc3f, p. 4). Or, for a PhD student in the life sciences, it is almost self-evident that if something has taken work, it is undoubtedly of value: she confidently states that if you "did some work you should be there" (IWS7, PhD13f, p. 31). Likewise, researchers may talk about how, in their experience, once work has gone into the creation of their contribution, it can be endowed with value and thus authorship is justified. These examples illustrate how contributions that are seen as work are perceived as inherently valuable by some researchers. Consequently, work is one of the values that can potentially guide researchers' valuation of contributions.

Work is not an unknown value. For example, Heinich (2020b) argues that work is the guiding value-principle whenever the value of an object "lies in the work it required and this work does not have to be valued by anything other than itself" (p. 221-222). Thus, researchers might infer that the processes of valuing contributions follow the rule that if "[y]ou do something, you get on the paper" (VU, PI7f, p. 3). Or, to quote another group leader, "everybody who was actually working on that study is going to be on the paper" (VU, PI1m, p. 1). Through these examples, it becomes clear how work can fulfil the characteristics of a value, namely to "be comprehensible and usable by all" (Heinich, 2020b, p. 221), in a variety of processes of attributing value to contributions. However, what counts as work is up to negotiation and context-dependent (Star & Strauss, 1999). Nevertheless, I employ "a *holistic* understanding of work" (Szymanski & Whalen, 2011, p. 6) in order to be open to any different conceptualisations of work by the interlocutors. Yet, the mere mention of work does not necessarily indicate that the underlying value is work. As Barker (2014) points out, in some contexts "a fundamentally instrumental view of work" (p. 9) can be observed, meaning that "work is considered as an activity that is undertaken for the purposes of something else" (p. 9). In the same vein, Heinich (2020b) describes how work can also be an abstract value-object, signifying the object upon which value is ascribed. It may be difficult, as Heinich (2020b) admits, to distinguish an abstract value-object, such as work, from a so-called value-principle. However, careful analysis revealed two other values that guide the valuations of contributions, in which work is seen as a means to make a contribution as valuable as possible.

Knowledge gain is one of the other two values. The value knowledge gain is clearly rooted in the epistemic realm (Felt & Fochler, 2012). Contributions are valued based on the principle

of knowledge gain whenever a contribution is considered valuable simply because knowledge was gained. Thus, any kind of knowledge gain is valuable and there does not have to be any potential application of it, since a value means that it can be “based on nothing other than itself” (Heinich, 2020b, p. 222). This is akin to the notion of ‘art for art’s sake’, insofar as knowledge is produced not for any specific practical purpose, but merely for the sake of knowledge itself. The espousal of this approach can be viewed as a counterpoint to the dominant neoliberal logic permeating the academic landscape and the concomitant demands on the scientific community to address the so-called grand challenges. This line of thinking and valuation does not stipulate that obtained knowledge must be applicable, transferable into patents, nor contribute to the mitigation of climate change or the cure of cancer, to cite but a few examples.

In the light of the value knowledge gain, but also of the value work, which is mostly favoured by junior researchers, the author’s observational notes (VU, BGSP), written down during the conduction of two discussion groups with early career researcher from the field of physics, are even more remarkable. The notes emphasise junior researchers’ astonishment when being provided with the university’s guidelines of good scientific practice (University of Vienna, 2006), as they have not been aware of their existence. Furthermore, their astonishment also results from the strong deviation from what is described in the guidelines and their situational normalities in terms of authorship. The contributions for which they are awarded authorship are typically ‘mere’ technical contributions, a practice that appears to be particularly prevalent in infrastructure-intensive research areas. However, it is often characterised as scientific misconduct in guidelines such as that issued by University of Vienna (2006). Yet, as several of them were keen to point out, adhering to these guidelines would render it extremely challenging for them to complete their doctoral studies, which require the submission of publications either as a co-author or even as the primary author. These stances appear to align with the standpoint taken by Jabbehdari and Walsh (2017), who contend that guest and ghost authorship should not be regarded so much as “form of deviance” (p. 875) but rather as an “evidence of changing norms” (p. 875), reflecting the shifts in research practices over the past few decades.

Furthermore, a third value is discernible, guiding the valuation of contributions, which

stipulates that both working and gaining knowledge are only considered valuable if they lead to a publication. A senior researcher describes an epiphany-like moment he had early in his career:

[T]hat you have to publish, because not now, to achieve something, that's not the point but. That, what makes it then, the science ultimately. To learn something and you have to pass it on, if you don't pass it on, it makes no sense, yes. Really not. (translated by the author⁴; VU, PI5m, p. 5)

In other words, a contribution is attributed value if the publication without it would be rendered unfeasible. Thus, in valuation processes governed by the value publication, the publishing of publication “has the capacity to put stop to argumentation” (Heinich, 2020b, p. 219). Against the backdrop of the ongoing debates about the obstacles to publishing so-called negative findings²⁵ this approach postulates that failed experiments or negative findings would seldom be accorded value, given their diminished probability of being published.

6.2. Duration, proficiency and connection as valuation criteria

Valuation criteria may further complicate matters, insofar as the process may be directed not only by a particular value but may also be influenced by a specific criterion. According to Heinich (2020b), a valuation criterion is “used to establish the extent to which an object possesses this ‘value’ — in the sense of the principle of valuation” (p. 221). Moreover, she states that it is the value “that makes that criterion a reason to attribute value” (Heinich, 2020b, p. 221). Valuation criteria are crucial for researchers’ justifications of why a particular contribution is considered valuable enough to be rewarded with authorship and not merely mentioned in the acknowledgements or omitted entirely. Moreover, valuation criteria enable researchers to substantiate the decision to accord greater value to the contribution of one researcher relative to another. These judgments may inform the specific authorship positions assigned to each researcher, which is further explored in Chapter 7. Only through the implementation of valuation criteria can the differences in attributed value between two contributions be brought to light. However, not all valuations require the implementation of

²⁵ See Section 2.1.4.

valuation criteria. In particular, for researchers working in a field with alphabetical author lists, valuation criteria are not relevant for the justifications of rankings, as long as the author list does not deviate from the alphabetical ordering of authors. Even in such cases, a researcher may refer to one or more valuation criteria in order to justify the inclusion or exclusion of a name on the author list.

The temporal dimension can present itself as a valuation criterion in the form of the concept duration through which researchers justifiably ascribe value to a contribution. The mobilisation of the criterion can be seen in the narrative of a postdoc in empirical physics who recounts how long he was in the lab and then in the office to make sense of the data generated. His emphasis on how it was “almost like the entire master’s project” (VU, PDoc3f, p. 4) to produce a particular contribution demonstrates how time is mobilised as a criterion for attributing great value to his contribution. Since the criterion duration is tied only to the value of work, time investment may function for researchers as a way of judging whether a contribution is ‘real’ work. As Barker (2014) argues, “[t]he idea that there is something called ‘real work’ implies that work has a true *essence* of some kind; it assumes a pre-existing standard or set of criteria that can be used to judge” (p. 5); duration appears to be one of these. The emphasis placed on temporal investment in this context is of particular interest because it appears to stand in contrast to the commonly held image of a flash of genius characterised by swiftness and ease.

A more comprehensive understanding of the reasoning behind the application of temporal investment as a valuation criterion can be gained by analysing instances in which researchers discuss work-life balance in academia. In one discussion group, a PhD in the life sciences shares her impression: “And I feel that sometimes in our work life, it’s not about what you are doing with your time, but it’s about how long you do something. So, sometimes it’s not really valued if I just finish up my work until five or six” (IWS12, PhD36f, p. 32). This observation coincides with the justifications given by other researchers for attributing value to a contribution on the basis of the time taken to produce it. In this line of reasoning, contributions are only valuable if their creation was not effortless and took considerable time. Consequently, early career researchers in particular, who are the ones mostly employing that criterion, struggle to attribute value to an idea on the grounds that for them it seems that it has taken only a few minutes to come up with it. In contrast, a senior researcher may perceive that the

development of the idea has been gradual and incremental, reflecting a cumulative process of research and thinking over many years.

There is often considerable variation in valuation processes observed across different academic ages. For a theoretical physicist, it is “obvious from ... I know the paper from the beginning to the end and I know how much I contributed to it” (VU, PI4m, p. 8). The relevant valuation criterion for him is his “contribution to solving the problem, more than time, of course” (VU, PI4m, p. 8), to do otherwise would not be fair to him. The question, therefore, for him is whether knowledge could be gained to determine whether a contribution qualifies for authorship. For him, no further specification through the mobilisation of a valuation criterion is necessary. Insights such as these into researchers’ valuation processes allow for reflection on how authorship conflicts may arise, as the valuation criteria and even the value-principle applied may differ.

Another discerned criterion is proficiency. Rhetorically raising the question of whether certain skills are indispensable for making a specific contribution is one way of applying the valuation criterion proficiency. Or, in the words of one postdoc, the point is whether “anybody could have done this with no training, or you had to invest in having some sort of training and some sort of skilled input” (VU, PDoc3f, p. 4). This valuation criterion is located in the epistemic realm and can be tied to the value work (i.e. any work that also requires certain skills to be carried out is to be given a higher value). In a similar vein, a junior researcher suggests that if external individuals “did two hundred samples for you, for example, why not put them on? Most likely they will also interpret the data for you, give you a quick overview” (BGSP 1_1.1, p. 23). In doing so, this researcher mobilises both the criterion of duration and proficiency. Another good example of the use of proficiency criterion is a PI who justifies listing statisticians or programmers by emphasising the skills required to perform statistical calculations or write a programme:

It could be anything that the person has done, which I can’t do either or my lab can’t do, yes, because sometimes I have statisticians with me somewhere, including a colleague who, for example, has written an extra program for us so that it works somehow, so for him it might be craftsmanship, not, because he can do it. (translated by the author⁵; VU, PI3f, p. 12)

However, if the respective researcher pays for a service or task — that she could theoretically do herself, either because she has the skills herself or no specific skills are required — the person performing the service or task will not be listed as a co-author. Another example is a life sciences researcher who, in her valuation of clinicians' contributions, distinguishes between whether their contribution includes not only the donation of samples but also whether the clinicians "really participate in the study" (IWS1, PDoc2f, p. 20). The researcher thus does not judge sample collection skills to be relevant and worthy of authorship. However, as Garforth and Kerr (2010) observe, the "blurring of the distinction between routine practice and epistemic production does not translate easily into the structured ways that institutions value both specific researchers and the different kinds of work that they do" (s.10).

Meanwhile, the valuation criterion connection, for example, emerges in moments in the discussion groups where researchers deliberate on whether a researcher's work was really essential for a publication. A PI sheds light on the criteria they might take into account in the process of valuing a contribution and turning it into a valuable — or, as it is commonly described in various guidelines — a significant contribution. From their perspective, whether a paper is feasible without a particular contribution is a pivotal aspect of their valuation, suggesting that publishing is regarded as the ultimate objective. The same type of justification is given by a postdoc who justifies listing a technician as an author in the middle of a publication by saying that she "couldn't have done it without her" (VU, PDoc3f, p. 7). Correspondingly, researchers may struggle to make sense of the practice of listing the group leader as an author if, in their view, the group leader's work is not clearly linked to a specific publication. Conversely, if researchers regard the group leader's work as a vital component of the publication output, they may perceive it as appropriate to list the group leader as an author. This is exemplified by the perspective of a PhD student who asserts that "our PI's are the key enablers for our research so I kind of get, without going to the lab, why they have to be on it" (BGSP 7_1.2, p. 12). In such cases, the application of the criterion connection in combination with the value publication assigns such a high value to the group leader's contribution that the valuation judgement is translated into co-authorship.

6.3. The co-production of collective research and valuation processes

Whilst it can be possible to attribute a particular contribution to a single researcher, there are usually a number of contributors to a publication, as evidenced by the increasing prevalence of collaborative research within contemporary research (Fanelli & Larivière, 2016; Jabbehdari & Walsh, 2017; Lewis et al., 2016; Mäkinen et al., 2020). In many of their accounts, researchers identify collaboration as a vital component of their situated normalities. Furthermore, they may consider it an aspect with the potential to complicate and impede the valuation processes involved in authorship practices. Thus, this subchapter rather foregrounds the sense-making aspects of researchers' narratives rather than their processes of valuation.

Accordingly, one postdoc elaborates how, in his experience, it is an exception rather than the rule to work alone on one of the various contributions that make up a paper. Even if a single researcher takes the lead, they might, for example, discuss a paper with someone else or ask another researcher for help. Thus, a single contribution may not necessarily be linked to a specific researcher but, instead, there may be a web of entanglements between different contributions and their respective researchers, potentially complicating the valuation of contributions. In the opinion of another junior researcher, "the interaction between human and human is the thing we should discuss more, because the scientific things we really discuss a lot [...]; so, yeah — I think that is the problem" (II, PhD27m, p. 7). Statements such as these underline the significance of the social dimension, which of course has multiple implications for authorship decisions that may be overlooked — not only in science policy discourses but also by scientists themselves. An illustrative example thereof is furnished through the following observation, made by a PhD student: "Because in scientific fields people are, try to, tend to ignore everything outside the science, try to mix everything, no side effects, nothing else, nothing personal, all science. But actually, it's not. As in any place it is just human-human interaction" (II, PhD27m, p. 7).

Working as a team can also mean that the pressure felt by the group leader is passed on to the group members. One group leader reflects that "when we have time pressure, I probably tend to push them, you know? Let's do this and do that" (II, PI14f, p. 20). But the group leader mentions how her own care practices and the care practices within the group can prevent

individuals from cracking under the pressure:

[B]ut I think I'm careful enough to see if it's too much for them or not and then, this is, my lab members are extremely good in team working, so I just ask also other people to help and then it's usually fine. (II, PI14f, p. 20)

However, researchers may also experience the interaction with other researchers as something positive, joyful and motivating. A senior physicist talks about how "the beauty of physics is scientific collaboration" (translated by the author⁶; VU, PI2m, p. 10). Collaboration can also mean being confronted with different approaches to writing an article and, thus, to publishing. A senior physics postdoc contemplates the discordant experiences she recently encountered while contributing to a paper spearheaded by her collaborator, and how these disparities will influence her future collaborative endeavours:

I'm at a completely different point or maybe anyhow I'll stay that way, but then you just have to accept that. [...] I'll just learn from that in the future and certainly not actively try to do something together again. (translated by the author⁷; VU, PDoc7f, p. 16)

The divergent valuation outcomes of collaborating researchers with regard to whether a draft should be submitted to a semi-recognised journal on the basis of its quality have an impact on research collaborations, without this necessarily being made explicit. Even though the researcher in the above quote makes her own position clear and also mentions the limited capacities of the other researcher in charge of the paper, she still comes to the conclusion that the specific collaboration is not valuable (enough) for her to further pursue in the future.

Certainly, an esteemed standing within the scientific community, coupled with a stable formal position — potentially one accompanied by an unlimited contract — may serve to enhance the appeal of scientific collaborations for some researchers. A senior researcher states that negotiations concerning authorship "are never really serious ones for me, everything is somehow taken as granted" (translated by the author⁸; II, PI15m, p. 11) but then rather self-reflectively adds:

[B]ut I'm more the kind of person that, when it comes to cooperation projects, I tend to take a back seat, because it's more important to me to enter into a cooperation than to insist on it — but probably, I can afford it, probably others can't" (translated by the author⁹; II, PI15m, p. 11).

Furthermore, he speculates that his approach to authorship negotiations may have some role model effect as he behaves like a “good cooperation partner who doesn’t fight over the most important positions in order to make cooperation possible, so to speak” (translated by the author;¹⁰ II, PI15m, p. 11). Thus, it could be argued that his strategy with regard to authorship negotiations and decisions is characterised by a sense of responsibility and caring that values the continuation of a collaboration over the position on a paper.

Additionally, researchers’ appreciation of teamwork may also stem from the awareness of the synergy that arises from the accumulation of diverse skills. This in turn enables the execution of research projects beyond the individual capabilities of a single researcher, thereby potentially leading to major publications. A group leader who values teamwork for these reasons consciously encourages it “in that I actually say: it doesn’t matter who did what — they’re going on the paper” (translated by the author¹¹; II, PI7m, p. 30). He thus argues in favour of not taking into account a researcher’s position in academia, a highly controversial issue that is further explored in Subchapter 6.8.

The material and social dimensions may also be intertwined when, for example, several researchers work together on the same machine. In the case of certain experimental set-ups, which require several researchers to assemble, the researchers involved are rewarded with co-authorship for their material engagement and collaboration over an extended period of time. Or a physicist tells how, in a research group in which she worked, measurements were taken in shifts and it was therefore “actually always clear” (translated by the author¹²; VU, PDoc5f, p. 2) that all group members who took measurements would be listed as authors on the publication. The reference to the necessary skills for discussing and interpreting the collected data signals the postdoc’s application of the valuation criterion proficiency, to which she justifies her attribution of a high enough value to be translated into co-authorship. Conversely, within the researcher’s current, larger research group, the members have distinctly defined areas of research, despite utilising the same instrument, which results in not all group members being listed as authors on every publication.

Another example of considering social aspects in authorship practices related to the use of infrastructure is when a research group relies heavily on a single researcher because they are

the only one who knows how to use an essential instrument. Citing the long period of time it would take to learn this skill can serve as a time-based justification for granting co-authorship, thus attributing even greater value to the contribution of instrument handling. The high value placed on such detailed knowledge of the operation of an instrument may even lead to the creation of a senior scientist position, because otherwise these skills may no longer be accessible and therefore usable if the researcher in question were to move to a new laboratory due to a limited contract.

6.4. Changing contributions over a scientific career

The nature of the contributions typically made by a researcher evolves over the course of a scientific career.²⁶ Consequently, researchers may present this change in their narratives as a 'natural' or 'given' process, thereby indicating that this is an aspect they must make sense of and accept, rather than perceiving themselves to have a significant degree of agency in this regard. This change in contributions may mean that "I'm happy to help if there are any questions, but myself standing [in the lab] and working for a longer period of time is rather not the case" (translated by the author¹³; VU, PDoc7f, p. 12). Again, however, 'it depends'. On the one hand, the number of research projects in which an individual is involved, or which they lead as principal investigator, appears to have a bearing on the matter. On the other hand, the researchers themselves also appear to exert an influence. One postdoc describes it as "extremely personal matters" (translated by the author¹⁴; VU, PDoc2f, p. 6), as one senior researcher may insist on running and understanding a code themselves, while in another collaboration the respective researcher is fine with seeing the results generated by someone other than themselves and simply having a discussion about the results. How a senior researcher deals with their diminishing involvement in empirical contributions may be related to their sense of responsibility, as being listed as an author on a publication comes with formal responsibility.²⁷ It may also be an issue of trust and therefore shaped by the social dimension. Or it may be a question of whether, in their understanding of what it means to be a 'good' researcher, it is (not) essential to fully understand the research they are involved in. For a group leader, however, it is crucial, both for themselves and for the other group members, to

²⁶ See also Section 2.1.2.

²⁷ See also Section 2.1.3.

acknowledge that they are “unable to understand all the [technical] details and why sometimes things would fit” (IWS6, PI6m, p. 25). Responsibility is thus partially attributed to other members of the research group, such as a lab manager or the lead author. A postdoc in the life sciences shares this distribution of responsibility because he believes that a last author, who is usually a senior researcher, must trust the first author of a paper, since they have more of a general understanding of the paper. However, this distribution of responsibility is also influenced by the size of the group (i.e. the spatio-material dimension comes into play). Within a larger research project, especially an interdisciplinary one, researchers from another discipline might have a rather limited understanding of each detail or figure.

One group leader raises another interesting issue related to this change in contributions as a career in research progresses, which also illustrates how a senior researcher’s contribution may also be valued by their group members. She hypothesises that her “getting people on the team that are definitely better at those [technical] than I will ever be again” (VU, PI6f, p. 6), may elicit a certain perspective particularly among postdocs:

[T]hey think they’re better than you because you can do technology skill one, two, three and I think but having experience and judgment are really important and understanding scientific frameworks are really important. I think that there is a tension there between people who think that that’s less important than what they are able to offer, but what they can offer is just a part of the whole big thing. So, we, supervisors are really burdened with keeping a team together constantly with the team wanting your job. (VU, PI6f, p. 6)

That the senior researcher’s observation is particularly concerned with postdocs may be due to postdocs’ potential desire to be named as the last author on a publication, a position usually held by the group leader or senior researcher in many life sciences and physics research groups. Researchers’ narratives, which primarily deal with the different authorship positions, are more closely examined in the following Chapter 7. However, in the context of this (sub)chapter, it is brought up to illustrate the tensions that arise from changing contributions over the course of a scientific career.

It is often challenging for researchers to place a high value on their own yet unfamiliar contributions, particularly when a shift in the typical contribution implies that other

contributions are no longer feasible due to an individual's increased commitments. A junior group leader grapples with her novel situated normality:

We're using a certain software package, I was a developer when I started it. So, I was programming and I don't know how to compile the last version of my computer. I would not be able. They shifted to a new language that I don't write. It's since three years that I have no time to learn it. This is not normal. (VU, PI7f, p. 9)

Due to her responsibilities as a group leader, she no longer has the time to focus on what is considered to be empirical contributions. In her daily life, she spends a lot of time writing project proposals, which she finds "frustrating. It's just not what... it's not rewarding enough, there's no immediate reward" (VU, PI7f, p. 10). In contrast, in her opinion, "with science you do something, you see a model. There is at least within a short period of time, say a week, two weeks, there's a reward" (VU, PI7f, p. 10). She further compares it to writing publications, which also do not have an "immediate reward" (VU, PI7f, p. 10). For her, doing 'actual' research, such as working with the aforementioned software — as opposed to writing publications and grant applications — "gives you like some kind of professional satisfaction which is not the outcome of some external judgment" (VU, PI7f, p. 10). It may be hypothesised that being subjected to the outcome of someone else's assessment, as is often the case within the context of publications and grant applications, has the potential to diminish a researcher's professional satisfaction.

6.5. Matters of distance and access

The physical distance between researchers and the potential for this to result in a lack of familiarity between them can play a role in the valuation of contributions. Indeed, these aspects may even be depicted as contributing to the complexity of the valuation processes. Moreover, it can be crucial for researchers to be in the 'right' space or to have access to certain spaces in order to generate contributions. In this context, the description provided by Gieryn (2000) is particularly noteworthy for its emphasis on the integral affective component of places. In his view, "[a] place is *remarkable*, and what makes it so is an unwindable spiral of material form and interpretative understandings or experiences" (Gieryn, 2000, p. 471). How important it is for researchers to be in certain spaces may be linked to their formal position, just as spaces are more or less accessible depending on this. Moreover, this subchapter

explores what the spaces are that researchers talk about in relation to authorship and what roles these spaces play for them. For instance, in their narratives, researchers are concerned with the spaces where authorship decisions are made and who is present or absent. The space-related aspects of the dimension affect authorship practices in a particular manner, for example, what it means for authorship negotiations and decisions that researchers move through and work in different spaces. These can include, for instance, the fact that their research entails field work, collaborations can span over several labs or even that researchers' main working space changes as their scientific career progresses. Therefore, this subchapter examines the issue of spatial distances between researchers, frequently giving rise to a personal distance, and explores strategies for reducing such distances. This aspect touches on issues of responsibility and also points to the ways in which care practices are shaped by spatial issues.

Valuation processes can be complicated by spatial issues, including the hindering the application of a valuation criterion. For example, in one discussion group, when contemplating the valuation of clinicians' contributions, a PhD laments the lack of knowledge of "how much time they spent working on this project" (IWS3, PhD6f, p. 17). The spatial separation between researchers hinders the valuation process, insofar as the valuation criterion duration cannot be implemented which determines (from the standpoint of the PhD) the amount of value attributed to the clinicians' work. A PI from another discussion group is well aware of the negative effect of spatial separation, even citing it as a reason for, in his opinion, the rather common reluctance to list clinicians as co-authors in his environment:

I do think that we sometimes undervalue the contribution of clinicians because it is constantly quite different to treat a patient in a province hospital somewhere versus to treat a patient in a way this patient can be used in a research study. (IWS1, PI1m, p. 22)

Given that researchers may have been patients in a hospital but have not usually been involved in a research study — either as a clinician or as a patient — many researchers have no in-depth knowledge of the skills and time required to collect samples, for example. Therefore, the absence of researchers from the settings in which clinicians work can have a negative impact on the researchers' valuation of the clinicians' contributions. On the other hand, a PhD who has worked in the clinical context in the past and has obtained consent from

patients, and therefore knows from personal experience how much effort it takes, appeals to the other participants in the discussion group to “take this into account” (IWS1, PHD1f, p. 22) in their valuations. She particularly emphasises how much time it takes, employing the valuation criterion duration and thereby justifying the amount of value attributed. Considering that this working experience (of being part of a medical study) is rather exceptional, the frequent absence of researchers in hospitals reinforces the difficulties of them coming to a collective valuation of the samples provided by clinicians, which can be observed in several of the discussion groups analysed.

Moreover, access to specific spaces is imperative for certain researchers, since without it they are unable to contribute. For both PhD students and postdocs in various fields of the life sciences, absence from the laboratory during pregnancy for health and safety reasons has a negative impact on their publication record. This is because they are excluded from the place where data is generated, which is considered crucial for publications and is therefore highly valuable in terms of the value publication. But absence can also be compensated with care, as illustrated by the story of an early career researcher who, together with colleagues, supports a postdoc who is rarely in the lab due to the demands of caring for dependents at home. By offering this support, the postdoc’s publication output is less likely to be negatively affected by their non-academic responsibilities. Discussions of how access to certain spaces is granted thanks to other researchers are examples of how relational aspects are intertwined with issues of space. Practices of care are also evident in the stories of supervisors who organise excursions and accompany doctoral students on field trips for some time to support them in their data collection. Such practices can also be conceptualised as an expression of a supervisor’s responsibility, since without their involvement, it would be almost impossible for PhD students to carry out this kind of fieldwork.

A revealing anecdote from a PI underlines just how much depends on being present where the work is done, and how it is then valued. He recalls the story of a senior researcher who “was making sure that he attends every meeting between his two postdocs and then wanted to co-sign the paper because he was attending. So, they ended up trying to meet without him knowing about it” (VU, PI4m, p. 12-13). For the two postdocs, however, the mere presence of their superior, presumably without actually contributing to their research, did not

justify the recognition of the so-called contribution of the senior researcher with authorship. However, since presence can be used as a powerful argument in authorship negotiations, in this case probably reinforced by the power of the senior researcher's position, the postdocs' solution to circumvent an authorship decision, or rather a valuation of a contribution they did not consider 'good', was to meet in secret. This story shows the downside of what a researcher's presence can mean in terms of authorship decisions, especially when it is difficult to trace who contributed what.

6.6. Valuing the provision and maintenance of infrastructure

Aside from reflecting on how spatial issues matter, researchers also negotiate how to value infrastructure-related contributions in their valuations, since, in a number of subfields in both the life sciences and physics, research is not possible without a wide range of equipment and different instruments. Of particular interest for this subchapter is an infrastructure-related contribution that is closely linked to the formal position of a researcher, namely the position of the researcher in charge of a lab. Consequently, it has been very fruitful to direct attention to how researchers who are not responsible for the lab, and in comparison, researchers who run a lab, value this kind of contribution, and pose the question "whether these relations between identities are relevant for the valuation processes at hand or for their social consequences" (Waibel et al., 2021, p. 38). Identities in this valuation situation differ — among other characteristics — in their 'ownership' of the lab infrastructure, thus representing an example of how "some identities' relations are further qualified by intimacy, others by dependence" (Waibel et al., 2021, p. 38), whereby the second aspect applies to these relations.

The contribution of group leaders in sub-disciplines of both life sciences and physics, which rely heavily on equipment and instruments for their research, often consists (in part) of maintaining the lab, e.g. writing grant applications or dealing with administrative and organisational issues. One could argue that group leaders thus greatly affect the material environment in which empirical research is carried out. Furthermore, the contributions of group leaders are, in general, divergent from those of early career researchers. However, this does not necessarily have a negative impact on how group leaders are recognised in

authorship practices. Indeed, an empirical physicist recalls that in a former group of his, the group leader (who was very successful in raising funds and providing the group with equipment) was always listed as the last author, as this was considered to be his “job” (VU, PDoc1m, p. 5). Consequently, this value judgement, founded upon the value work, is then naturally symbolised by co-authorship.

How differently the provision of funding and equipment can be valued in valuation processes of contributions is the central point of a story told by a life science researcher. She recounts a rather conflict-ridden authorship negotiation between her former PI and a collaborating PI, since the latter “was convinced that the second [PI] didn’t need to, didn’t deserve authorship because they didn’t do anything” (VU, PDoc3f, p. 7). The narrating postdoc seems to agree with this judgement of the collaborating PI, as she elaborates that the “only thing that they [her former PI] did was hire me and I was the person who provided the training, the services, and the collaborative efforts on the paper, whereas a lot of that took place on our home turf” (VU, PDoc3f, p. 7). By comparing her former PI’s contribution with her own contributions, the postdoc emphasises the low value she bestows upon the PI’s contributions in the form of providing the lab and hiring her. Furthermore, the inquiry into the indispensability of specific skills for the PI’s contribution, indicative of the criterion proficiency, serves to further substantiate the relatively low value attributed to the PI’s contribution, thereby negating the justification for co-authorship. Referencing the time she dedicated to completing the various tasks may serve as an additional argument in support of her valuations. From her perspective, the valuation criterion duration also results in a discrepancy in the value assigned to the contributions, because she perceives the PI’s hiring of the postdoc as his contribution, which is considerably shorter than the time required to complete her numerous tasks.

In contrast, for “the head of the group who owns all of the resources that were used [...] that’s [the group leader’s] justification for being on the list” (VU, PDoc3f, p. 7), as the postdoc observes. But what motivates the group leader to confidently claim authorship? The PI’s attribution of significant value to his contribution by providing the lab equipment can interestingly also be explained by the mobilisation of the duration valuation criterion. However, he may consider the time spent on the maintenance of the laboratory, for example by writing grants to acquire funds, as part of his contribution, thereby invalidating the

assertion that he has not done anything. Hence, the PI may have a different perception of his contributions than both the collaborating PI and the postdoc telling the story. Furthermore, while the ongoing maintenance of the laboratory infrastructure may not be so clearly linked to the joint publication for the postdoc and the collaborating PI, this link may be obvious to the group leader, and he therefore rightly applies the connection valuation criterion in his view. Or alternatively, from the PI's perspective, the provision of infrastructure is very valuable because it enables knowledge to be gained. Research in the PI's field is rendered unfeasible without the relevant equipment and, consequently, neither knowledge gained nor publications materialise. In turn, this renders the PI's contribution highly valuable. Adopting a temporal perspective, as proposed by Adam (1998), "helps to illuminate the nature of the gap between application and long-term effects and between the perception of symptoms (the *Merkwelt*) and the in/visible, in/direct, non/linear, non/proportional impacts of actions (the *Wirkwelt*)" (p. 35). Accordingly, this perspective may foster awareness of the perceived correlation between infrastructure provision and publication among junior researchers. In the words of Star and Strauss (1999), "[w]ork may become expected, part of the background, and invisible by virtue of routine" (p. 20), even when performed by an individual in a superior position.

The potentially great value attached to the provision of infrastructure is also expressed by an empirical physicist who tells how it seemed inevitable to him that the head of the laboratory where he carried out all his measurements should be the last author. In his words, he "couldn't refute that argument" (VU, PDoc1m, p. 11) about how crucial the provision of the equipment was to the publication. However, because this postdoc was aiming for a publication with himself as the last author at that point in his career, this narrative is underpinned by a certain regret that his research is heavily dependent on another researcher who gives him access to a lab (because he does not run a lab himself). To some extent, therefore, researchers who do not 'own' the necessary infrastructure for their laboratory are almost inevitably forced to value this contribution if the researcher in charge insists on it. On the other hand, the lack of equipment in fields such as theoretical physics may allow some leeway, since acquiring the necessary funds for one's own position is sufficient to achieve a comparatively high degree of independence.

Unlike the aforementioned cases, researchers who do not run a laboratory may also perceive this task as a valuable contribution. A postdoctoral researcher, whose group leader is no longer actively involved in research because his time is spent running the department, explains that without the “contribution” (translated by the author¹⁵; VU, PDoc5f, p. 3) of the group leader, no research leading to knowledge gain could be carried out. She uses this already familiar justification, pointing to the value knowledge gain to legitimise the authorship practice of always listing the group leader on the publications of the research group.

Moreover, even if researchers may concur with the attribution of value to the provision of infrastructure, independently based on the value principle in question, they may nevertheless conclude that it is not sufficiently valuable to warrant acknowledgement as an author in every publication linked to the respective laboratory. Rather, they may determine that it is only worthy of such recognition in one or a few cases. Alternatively, the attributed value may be perceived as aligning with the position of a middle author within an authors’ list, rather than that of the last author. The following chapter, Chapter 7, explores in greater detail the manner in which researchers strive to align the valuation of contributions and authorship positions.

Dependence on infrastructure is also relevant if measurements can only be carried out in another scientific laboratory. For a senior researcher, whose lab regularly carries out measurements for other research groups, it is evident that the measurements and their analysis are rewarded with co-authorship:

We have developed the methods, we do the analyses, we evaluate, we make sure that everything fits and we help them with the interpretation. For that, we have to be on a paper, if we’re not on it, it may not be me personally, it may also be my employees, depending on who does it and takes care of it. But it has to be that way. Because otherwise you don’t bother with it anymore. And that has also helped that we have then developed and yes not everyone has to measure with us, but is also okay. (translated by the author¹⁶; VU, PI5m, p. 17)

By mentioning how this confidence in claiming authorship for their contribution has helped the ‘development’ of the group, and how they would otherwise not ‘bother’ to carry out such time- and resource-intensive tasks, the group leader indicates the value that not only he and his group members, but also researchers outside his group, place on their measurements and analyses.

6.7. Valuing invisible contributions

In contrast to the preceding subchapter, the contributions of this subchapter are characterised by their immateriality, i.e. they are not traceable in the publication. This concerns the valuation of contributions that do not end up in the final version of publications because the overall argument changes or the experiment simply goes wrong. In analysing researchers' narratives, it is possible to identify instances where researchers struggle with how to value such contributions. This uncertainty is illustrated by the statement of a life sciences researcher: "Now you can also discuss whether it makes sense or not. I did something for the paper, it didn't work and you don't see anything of it in the manuscript and then you're still on it" (translated by the author¹⁷; VU, PDoc4m, p. 1). Whether this researcher is unsure which value should guide the valuation or which valuation criterion can support his valuation process is unknown. Later in the interview, however, he tells the story of not being listed as a co-author on an article for which he did a lot of work that did not go as planned. From his perspective, he nonetheless made some significant and valuable contributions. One could argue how the postdoc's contribution was therefore more of an invisible one. The situation appears to have been further complicated by the fact that the researcher responsible for the project, and therefore for the authorship list, transferred to another lab and proceeded to publish the results. The paucity of communication or information regarding the non-listing as a co-author results in the process of valuation and its guiding value(s) being opaque to the postdoc — a recurring phenomenon in the data corpus analysed, resulting in clarity²⁸ as a value by which authorship decision processes are judged.

In contrast, other researchers are quite confident in their judgement. One researcher argues that, "of course" (IWS2, Ma1m, p. 23), a researcher should be named as a co-author when they have done "so much work and produces so much data" (IWS2, Ma1m, p. 23), which is not included in the publication. Or a junior researcher objects to the exclusion of a colleague from the paper on the grounds that "at the end of the day, we did invest the same time, and the same time and energy" (BGSP 2_1.2, p. 14), thereby perceiving the exclusion as not "very fair" (BGSP 2_1.2, p. 14). This resonates with an argument made by Helgesson and

²⁸ Also see Chapter 8.

colleagues:

[I]f what is required is a substantial contribution to the research *presented* in the paper, then it seems that whether any of the junior researchers end up in the authorship byline will be, to some degree, a *matter of luck*, since neither an initial agreement or great efforts along the way is sufficient. This too seems unfair²⁹. (Helgesson et al., 2021, p. 27)

Furthermore, this type of data, frequently termed 'negative findings'³⁰, can be regarded as valuable due to its potential to prevent other researchers from committing the same errors and thus conserving time, thereby underscoring the importance of efficiency in the current science system. Additionally, it can preclude the superfluous utilisation of public resources in the pursuit of similar outcomes. Furthermore, such negative findings can also serve as a starting point for other researchers, thus indirectly leading to the advancement of science. Consequently, the epistemic dimension also exerts influence on these valuations, albeit imperceptibly at first.

There are also other types of contributions that are not, or less, noticeable within the final publication. A PhD student agrees with the authorship decision to list a technician as second author of a publication in which he was involved, even though "the technician didn't contribute to any figures, but she was, we all know, she supported every single experiment in one way or the other, so I had no trouble seeing that she deserves to be the second place" (IWS12, PhD40m, p. 30). Within the epistemic realm, the valuation of the doctoral student is thus understandable. Teaching a less experienced researcher a certain technique might be another example. Whether this counts as a contribution is debated in several instances. Moreover, even if it is agreed that it counts as a contribution, the question arises as to how long it counts as a contribution to a publication, pointing to the importance of the temporal dimension in the valuation of contributions. One researcher proposes to recognise this act with co-authorship on the first paper in which this technique is used, but not in subsequent publications.

Another example of a less tangible contribution is an oral contribution. An early-stage

²⁹ Valuations grounded in the value fairness are in the centre of the Chapter 8.

³⁰ See Section 2.1.4.

researcher justifies her valuation of an insightful conversation with another researcher as not being valuable enough to convert into authorship by explaining that “a discussion that’s not like talking for months and months [...] that’s like maybe an afternoon or maybe an hour, not even, then I think, don’t think the results could be that big that it suffices for a co-authorship” (BGSP 6_1.2, p. 28). The second part of the statement, in which the magnitude of the results is mentioned, demonstrates how, almost as an afterthought, the epistemic dimension is brought in to reinforce the temporal justification of the lack of value. While the first valuation, using the criterion of duration, is presumably driven by the value of work, the second seems to be governed by the value of knowledge gain.

One frequently encountered issue associated with such a contribution is that “it’s hard to get sometimes who had the idea first; if you work in a lab you’re talking all the time and you have lab meetings, and somebody says something” (BGSP 1_1.1, p. 19). Alternatively, researchers may ponder how to value a contribution in the form of a fruitful discussion with other researchers at a conference. Since, according to one early career researcher, by participating in a debate at a conference, the researchers involved “didn’t actually do any work” (BGSP 5_1.2, p. 9) authorship is not justified for them. They may amplify their valuation by contrasting it with the contribution of the researcher who “did all the work and wrote the paper” (BGSP 6_1.2, p. 27). However, by conceding that such a contribution to the discussion might be “an inspiring thing, so it’s important to point it out, but I would have put them in the acknowledgement (BGSP 5_1.2, p. 9), the early career researcher hints at how such a contribution might be given some value by basing the valuation process on another value. The value in question might be knowledge gain, as the researcher points to the inspirational quality of such contributions. Nonetheless, the value attributed to these contributions to the discussion is again limited, although another value is mobilised which, once again, does not warrant authorship. A PhD student offers a similar judgement, while recalling how a PI and a postdoc included them in the acknowledgement of a paper following a conversation:

[T]hat I think is totally fair because, I mean, I didn’t even expect that— it’s still one or two hours of my time versus like them actually having to do the work. But they acknowledged that, like, the ideas behind that came from conversations with me. (BGSP 7_1.3, p. 8)

The researcher sets forth their line of reasoning with a justification that is thoroughly

investigated and corroborated throughout this thesis: Namely, they contrast the considerable amount of work with the comparatively shorter time invested in a conceptual contribution.

6.8. (Not) considering a researcher's position in academia

While the preceding subchapter addressed the topic of 'invisible' contributions and ultimately 'invisible' co-authors, this one examines ways in which status affects authorship. The epistemic dimension is purported to play a pivotal role in determining the legitimacy of authorship, independent of the individual's academic age or formal position, as stipulated by authorship regulations. Yet, it is also subject to a variety of analysed narratives. In some fields of research, students work in a lab for the first time during their bachelor's studies and are thus already confronted with the prevailing normality of authorship practices in the respective environment. However, according to one life sciences postdoc, senior PIs in particular "have a strong opinion that bachelor's students, the work that they do doesn't count at all if you're... So, if you're not at least a master's student, then you're just training in the lab" (VU, PDoc3f, p. 3). Similarly, an early career researcher complains about the following cases:

[F]or example, if you work on a project or something then you should [get] co-authorship or authorship on a paper and then, because of the power abuse of the professor, you might not get or might, might say: well, you work for it, but we don't [add], I don't know, students or whatever to [it]. (BGSP 1_1.1, p. 9)

Both quotes are examples of how the power imbalance (i.e. the structure of relations in the social dimension) inhibits the valuation of contributions on either the base of the value work or the value knowledge gain. Valuations of contributions are thus often determined by the formal position — both of the individual contributor and of the researcher in the highest formal position. The symbolic dimension may also influence such valuations because bachelor's students have no 'need' to publish within the prevailing neoliberal logic. Furthermore, the way in which the researchers talk about this rather common authorship decision illustrates the frequent struggle of researchers to come to a valuation of the authorship practices they encounter and thus shape their situated normalities. Statements such as the postdoc's "I'm not sure I agree with that" (VU, PDoc3f, p. 3) can be interpreted as signifiers of a situated normality that they feel they have no agency to change but are unsure whether they can accept and live satisfactorily within.

Correspondingly, one postdoctoral researcher emphasises how “happy” (VU, PDoc3f, p. 4) she was that the authorship negotiations in which she took part focused on the value to be attached to the contributions of bachelor’s or master’s students, rather than on their status as students. Sometimes, it is only in hindsight, when one has been part of various other authorship decisions, that one deems it a “testament to [the] very good mentor I had at that time” (VU, PDoc3f, p. 2), who included her in the authors’ list — independently of her status as a bachelor’s student at the time. Another postdoc also portrays it in quite positive light, that in the first group he worked in as a bachelor’s student, the individual’s position in academia did not affect the authorship decisions and the inherent valuations of contributions. In his perception, the implicit question was “how much did you contribute” (translated by the author¹⁸; VU, PDoc4m, p. 1). A third postdoc emphasises that it is “self-evident” (translated by the author¹⁹; VU, PDoc5f, p. 5) for her to list, for example, a master’s student as a co-author on publications if they have made a significant contribution. When asked in an interview about the inclusion of master’s students as authors on publications, one group leader’s reaction is one of confusion:

I actually never thought about it. It’s the first time you...even...this kind of question even arises in my heart. I mean why not? In a sense what’s wrong with that? Somebody comes to me, that’s super cool work, and I publish it, so? Who does the work, gets authorship. (VU, PI7f, p. 5)

She goes on to say that she grew up “in the environment where if you’re good, you get paid and you get published” (VU, PI7f, p. 5), a (authorship) practice that she maintains and that is not unusual. Another group leader from the life sciences treats authorship for master’s students in a similar way. If they go beyond the usual scope of a master’s thesis to make it publishable, they are not only listed as authors, but even as first authors. A further life scientist tells each of the master’s students she supervises that they may be listed on a paper if the data they generate in the course of their master’s project is used in a future paper. And on rare occasions, when one of her teaching assistants makes an intellectual contribution, they are also listed as co-authors. A junior researcher arrives at the conclusion that “it depends from lab to lab. In our lab, our supervisor gives co-authorship to the technician” (BGSP 1_1.1, p. 22), thereby signalling their awareness of this not being the normality for all labs.

Stating that ‘it depends’ when asked whether a technician might be credited with authorship

for their contribution underlines how researchers' valuations are rarely straightforward. Rather, one or more dimensions are involved. Moreover, researchers may resort to comparing different contributions in order to justify their attribution of value to a particular contribution. One postdoc underlines how a technician once became an author because he did "a lot of weight work, which is difficult" (VU, PDoc6m, p. 7) while the other technicians involved "did a lot of preparing" (VU, PDoc6m, p. 7) for which they were not listed as co-authors. Another researcher's comparison between contributions where a protocol is followed and contributions where adaptations are made to the protocol serves as a justification for acknowledging the latter with authorship. The criterion proficiency is likely to play a decisive role in this comparison.

The epistemic dimension is mobilised implicitly in these valuation processes. By emphasising that the status of the contributor, be it a bachelor's student or a technician, is irrelevant to the valuation of the contribution, the symbolic dimension is subordinated to the epistemic dimension. For the postdoc making this valuation, the "gain of knowledge" (translated by the author²⁰; VU, PDoc5f, p. 6) is in the foreground and therefore the guiding value. Similarly, by stating that "we always say you have to have read the paper, that's clear, you have to have, so to speak, even if you only, I don't know, helped in the lab" (translated by the author²¹; VU, PI5m, p. 13), a group leader emphasises that even a 'mere' technical contribution that leads to authorship has an intellectual component, namely the reading of the paper. This epistemic component, articulated through the mobilisation of the criterion proficiency, is thus of greater consequence to her than the position of the contributing individual.

6.9. (Not) doing science for the sake of authorship

Since the symbolic dimension "touches on values and modes of ordering" (Felt & Fochler, 2012, p. 137), the predominantly quantitative assessments of both research and researchers are a crucial element of the dimension. Various works have discussed the impact of quantification and on researchers and their practices (see Subchapter 2.2). Because authorship practices are inevitably tightly intertwined with what is referred to in this thesis as the neoliberal or symbolic logic or symbolic rationality, that logic can play a prominent role in researchers' valuation processes. Moreover, the interconnectedness of valuations is

particularly evident at moments when the symbolic dimension is salient, because researchers' valuations may be informed by their anticipation of how the materialised outcome of the current valuation will be evaluated in future assessments. Additionally, whenever the symbolic dimension enters the arena, so to speak, researchers in many cases contest its dominance in valuation processes and tend to be in opposition to it, exemplified by the following quote:

[B]asically the public is funding us to kind of reach to a stage where we become, let's say, assistant professors or group leaders, etc. But we're always assessed in very short term, so we need to be very successful in the next three years, so that we can find a position next time. So, this means, if you're not very, very successful, you may have issues finding a job. Like, you know, the society invests for fifteen, twenty years to someone and this is significant investment, I mean this is like, let's say millions of Euros of taxpayers' money invested in one person and we don't provide an outlet, let's say a secure outlet for that person. So, this is bizarre, this is bizarre in any, any context. So, this kind of incentivized people to first of all go for the low hanging fruits, because you would like to basically get the maximum with minimum time and resource investment and this also means that you really, really, really need to be successful. (II, PI12m, p. 1)

Thus, authorship negotiations often seem to relegate the value of knowledge gain to the background. In such cases, researchers may be strongly influenced by the symbolic logic in the valuation of their contributions. In addition, the symbolic logic might be so pervasive that researchers subordinate many or even all of their practices to it. In such cases, "you need to be incentivized in order to share information, and the only way I see it is by getting some sort of authorship" (BGSP 6_1.2, p. 31), as a junior researcher characterises their perceived situated normality in how researchers usually operate. Or, in the words of a senior researcher, "if you feel the pressure to publish more papers then it's likely that not all of them will be as good as if you didn't have that" (VU, PI4m, p. 10). However, researchers may espouse a contrasting viewpoint or, at the very least, present themselves in a different light by declaring how "I'd rather have it out working and not have my name on it than have my name on it and not have anyone use it" (BGSP 5_1.1, p. 30).

In many of the researchers' narratives, there is a sense of internal conflict, presumably as a result of the perceived tension between the epistemic realm and the symbolic rationality. One researcher suggests that this unease is related to the tendency of many to "pretend to do science just for the sake of doing science, but it is still a profession" (translated by the author²²; VU, PDoc4m, p. 4). Comments such as these underline how researchers may be concerned

that they will be viewed more negatively by their peers if they appear to be (too) influenced by the symbolic dimension in their valuation of contributions. This assumption may reinforce the perceived difficulty or near impossibility of talking explicitly about authorship (order), because researchers may fear that others will then view them in a negative light.³¹

A potential discrepancy in valuation processes between senior and junior researchers may be attributable to the former's tendency to refrain from attributing value to work for its own sake, as well as their reluctance to operationalise duration as a value criterion. Consequently, it may prove challenging for them to comprehend junior researchers' assertion of being rewarded with authorship solely on the basis of their time investment, as evidenced by their substantial work hours. Accordingly, a group leader in the life sciences adopts the following approach with regard to the working hours of his subordinates:

I never looked at how much people worked, I didn't care, because I always said you have to do your dissertation, that's where we come back to publications, because what counts in the end is what you produce, what the output is, and not that you were there. (translated by the author²³; VU, PI5m, p. 1)

The quote illustrates that the researcher's focus is not on working hours, through which the time spent on producing a contribution becomes visible, but on the researcher's outputs. Yet, in the course of the interview, the group leader also talks about conversations with his group members who are overworking themselves and whom he tells, "It will work without it and you're doing great things anyway. Calm down" (translated by the author²⁴; VU, PI5m, p. 3). This suggests that the group leader's priorities may be subject to change depending on the circumstances and the issues that arise. One possible interpretation of this dynamic is that the leader's commitment to generating new insights may be secondary when it comes into conflict with the well-being of his group members.

Nevertheless, statements such as these point to potential discrepancies in researchers' valuation processes, which may manifest themselves in authorship conflicts or debates. These may arise from, or be influenced by, whether researchers see research as a profession or a vocation, or at least which is more prominent. One senior researcher speaks of a "cultural

³¹ This perceived non-addressability is explored in more detail in Chapter 8.

change" (translated by the author;²⁵ VU, PI5m, p. 2) that he and his colleagues are observing, by which he means that, in contrast to him and other researchers of his generation, younger researchers are increasingly seeing research as a 'mere' job like any other. That said, a researcher who is not (yet) successful (enough) within the symbolic logic may, on the one hand, be well aware that it is a profession in which one is more or less constantly assessed. Or, on the other hand, the framing of research as a profession may function as a mechanism to distance oneself from the feeling of being a failure as an individual as long as one has not yet assumed a stable formal position in academia. In a similar vein, Kreissl et al. (2018) present a range of boundary-setting strategies deployed by scientific offspring to maintain a viable existence within the academia, despite the quasi-permanent external assessments to which they are subjected. These external assessments also exert an influence on the valuation of authorship positions, as investigated in the following chapter.

As demonstrated in this chapter, the positions of researchers within academia, typically associated with specific contributions, significantly impact their valuation processes and this insight is also pertinent to the subsequent chapter. In addition, this chapter introduces specific orders in which, for instance, work takes precedence over knowledge gain or vice versa, or knowledge gain over publication. These value orders foreshadow those discerned in the succeeding chapter, whereby the ensuing orders are even more indicative of how researchers wish to present themselves.

7. A list full of positions: Researchers valuing authorship positions

We have a completely misguided culture when it comes to that — in the life sciences, that is. [...] We have wars, broken friendships, career endings, too-. Because, and this was another, is another mistake that universities make, because they only look at: who is the first author where, and who is the last author. That concerns group leaders, that concerns career or young people — if they don't have their first author paper, they don't get a fellowship. They don't get an EMBO Fellowship, they don't get a Human Frontiers, they don't get a BBSCR, they don't get a Wellcome Trust — absolutely essential, yes? And we have to somehow think of a different culture, how this, how this works. It can't work like that, yes, because it has simply become so competitive and so many have become that it doesn't work anymore. And just because someone does not have a first author paper, it may well be justified that he does not get into the career, but it may also be wrong. (translated by the author²⁶; II, PI20m, p. 43-44)

This quote illustrates the tremendous impact that the ranking of authors can have on researchers' work and, indeed, on their very existence as researchers and thus foreshadows the central themes of this chapter. Being listed as an author on a publication functions as a crucial symbol of one's work and skills as a researcher in the contemporary scientific system. Depending on the specific field of research, a particular position in the list of authors may serve to further signal the significance of the researcher's contribution to the publication. These authorship symbols are then listed in a researcher's track record and are assumed to signify the researcher's overall scientific quality. Accordingly, a researcher's list of publications plays a central role in the decision-making processes of hiring committees or panels reviewing research grant applications. Moreover, as these lists of symbols are also easily accessible, for example via Google Scholar or the Web of Science, researchers can relatively easily compare their own accumulation of symbols with others in their field. Thus, while a researcher's formal position suggests a certain accumulation of symbols, even within the same career stage striking differences in symbolic capital are possible, which in turn shape researchers' narratives and valuations.³²

³² Section 2.4.3 addresses this issue, along with a reference to the work of Laudel and Gläser (2008), who differentiate between the cognitive, community and organisational career.

Henceforth, this chapter explores researchers' valuations of the various authorship positions, while being sensitive to the role their own positions within academia may play. There is, of course, a certain degree of shared agreement about what these symbols signify or should signify. However, the conceptualisation and value that one researcher attaches to one of these symbols when telling a story is not necessarily shared (in detail) with other researchers who may be colleagues, superiors, collaborators, competitors or formal evaluators. But it is precisely this potential identified awareness of a discrepancy in valuations that is of interest and therefore any references in researchers' accounts are carefully analysed.

7.1. Credit, recognition and capital as values

Following a thorough analysis, three values — credit, recognition and capital — could be identified that predominantly guide the valuation processes of researchers in their narratives about authorship positions. A pivotal distinction between these three values lies in whether the valuation of an authorship position is informed by the preceding valuation of a contribution or whether it is shaped by the anticipation of future assessments. Thus, the "valuation constellation perspective [which] highlights the interconnectedness of valuations" (Waibel et al., 2021, p. 47) is for the valuation of authorship positions of utmost importance. While the valuations governed by the values credit and recognition take into account the past valuation outcomes of contributions, since they (are supposed to) materialise in authorship positions with a corresponding attributed value, the valuations directed by the value capital are more affected by assumptions concerning future assessment processes. Moreover, referring to the conceptual foundation of the epistemic living spaces (Felt, 2009b), it could be argued that the three identified values are rooted in different dimensions.

Expanding upon these two aspects, the value principle of credit is mobilised when judging whether the authorship position reflects the previously attributed value of a researcher's contribution to the publication, based on the value knowledge gain. Consequently, it can be argued that the value credit is primarily located in the epistemic realm, since an authorship position is deemed valuable because it signifies credit for a researcher's valuable contribution to advancement in science. In contrast, the value recognition means researchers attribute worth to an authorship position according to whether they perceive it gives the 'right'

recognition for a researcher's work. In such a valuation constellation, the preceding valuation of the contribution is directed by the value of the work, that is to say, the researcher's work has not necessarily yielded any particularly significant epistemic findings in relation to the publication, but it is deemed valuable because the researcher has invested significant time and effort. Such a valuation may be informed by the social sphere and taking these aspects into consideration may result in researchers, for example, caring about acknowledging everybody involved in a publication — irrespective of the outcome — and cultivating team spirit within the group, as well as for sustaining motivation and a sense of appreciation. Thus, the valuation processes of authorship positions with the values credit and recognition are strongly interlinked with the valuation processes of contributions, with the values knowledge gain and work as driving values.

In contrast, valuations of authorship positions with capital as guiding value entail that positions are judged on the assumption that future assessors will assume the position to be of value. These valuations are thus strongly affected by the symbolic rationality according to which symbolic capital in the form of a long list of publications, on which one ideally is named either as first or last author, must be accumulated. In the words of Fochler (2016), researchers guided by the principle capital, then, may have the "overarching aim to be as productive as possible in accumulating career-relevant capital" (p. 943). Consequently, narratives with valuations driven by the value capital bring to the fore issues surrounding the metrification of research and the quantification of researchers.

Irrespective of the standpoint adopted by researchers in their narratives towards the neoliberal logic prevailing in the symbolic dimension, a plethora of narratives pertaining to the reasons why an individual 'needs' which authorship (position) or not can be identified in the analysed data. Feeling in need of a specific authorship position is intertwined with caring for and highly valuing that position. Heuts and Mol (2013) also allude to the inextricable nature of valuing and caring: "Caring is an activity in which valuing is implied — both caring *about* and caring *for* have a 'good' at their horizon" (p. 130). A central theme of the following subchapter is thus to explore questions of the primary focus of the researchers' care, whether this is securing a specific position on a list of authors or whether it is directed towards other matters or individuals besides themselves. Furthermore, how researchers position themselves

in relation to the subjects and objects of their care, as well as those of others, is a further focal point, because it provides insight into the preferred or desired ordering of values. Indeed, as Martin et al. (2015) argue, care is a “selective mode of attention that directs action, affection, or concern at something, and in effect, it draws attention away from other things” (p. 635).

Thus, the following subchapter outlines how positions can oscillate between being an object of care and a matter of difference, often depending on a researcher’s position within academia. The subsequent subchapters (7.3, 7.4, 7.5, 7.6), dealing with an author’s position among others, being at the end and at the beginning of a list of authors, and serving as a corresponding author, shed light on researchers’ justifications for ascribing value to, and thus caring for, different authorship positions. The final subchapter (7.7) concludes with a reflection on the justifications expressed by researchers for their ranking of authors.

7.2. Positions: A matter of care but also a matter of indifference

To whom or to what care practices are directed is also a question of whether it is oneself who is in need of care. Hence, being sensitive towards differences within the same career stage is crucial, since, for example, a postdoc who has a tenure track position in sight or a postdoc who has moved countries several times and is on their third or fourth postdoc position without any promise of assuming a tenure track position in the near future may have a different amount of symbolic capital at their disposal — despite them possibly having the same academic age.

For illustrative purposes, the following narrative demonstrates the power the symbolic rationality can wield. A PI recalls the story of a collaborator of hers, who “is really great” (IWS5, PI5f, p. 23) and wanted to be last as well as corresponding author on a collective publication. Thus, in her view “he wants to pretend, that it’s his idea and that’s his work, and he doesn’t have a problem with it, because he is actually, I mean this is really our idea and our work” (IWS5, PI5f, p. 23). For her, last authorship is the materialised outcome of a valuation rooted in the epistemic realm and possibly the social sphere, since she underlines how both the idea and the work can be traced to her research group. Hence, the colleague’s approach does not reflect her own. Moreover, she values such kinds of behaviour within the social dimension not highly because “I would never dare to collaborate with somebody, then call him up, in a, in

a, in a Skype and say, I think I deserve, it looks strange if I'm not last author and I can fight for you" (IWS5, PI5f, p. 23). Interestingly, she begins her story by mentioning the greatness of the researcher, thereby softening her critique. Moreover, she also elaborates the significance of this kind of authorship for the researcher's future career later on. The great influence the symbolic dimension can exert, is visible by his example and the way that others attribute such significance to this authorship position. It can then almost function as an excuse for this behaviour or, at least, makes it more comprehensible when even a 'great' researcher resorts to such practices. Considering themselves in the position of another researcher is a common exercise researchers undertake in their narratives, predominantly when they ponder over what meaning authorship positions have for peers.

However, having accumulated what one deems as 'enough' individual symbolic capital facilitates directing care practices towards subjects and/or objects external to oneself. The following two brief examples are provided to illustrate this point. A senior researcher turning down the offer from a postdoc to list them as co-author by stating he "doesn't need it" (translated by the author²⁷; VU, PDoc2f, p. 4) can be taken as an example of such a care focus. Similarly, in the opinion of a postdoc, when talking about his former group leader and his little-noticed care for authorship positions, the postdoc gives as an explanation that "it doesn't matter at all to him where he stands" (translated by the author²⁸; VU, PDoc4m, p. 6). The postdoc presumably refers to how a certain authorship position is no longer valuable for the group leader in terms of the symbolic logic because it does not make a difference in any future assessment situations. Moreover, it shows how caring and valuing often go hand in hand. The reduced or even non-existent dependence on authorial symbols can facilitate practices of care oriented towards others: A group leader tries to signal his valuation of the contribution of a researcher who is listed as a second or even third author by making the researcher corresponding author, because then "he also has something that brings something. Because those are the three things that count, corresponding author, senior author [i.e. last author] or the first authors" (translated by the author²⁹; VU, PI5m, p. 12).

Notwithstanding, the question of whether researchers' behaviour in authorship rankings is subject to the principles of symbolic rationality cannot be entirely answered based on a researcher's career stage or on their perceived 'need' for particular authorship positions. For

instance, a postdoc recounts how he solely created the list of authors for a first authorship publication of his. Intriguingly, and in stark contrast to numerous other accounts, he indifferently mentions how he was responsible for compiling an author list because he was the one who wrote the paper. This aspect deviates particularly from the narrations told by many PhD students, underlining the importance of analysing stories from researchers throughout the various career stages. However, as a postdoc, the narrating researcher presumably works in a more independent manner than a PhD student. Nevertheless, the narration can also only be told in this way because of the non-existent intervention on the side of the two senior authors, listed as last and second to last authors, who have not complained about the postdoc's authorship ranking. Similarly, not much thought seems to have gone into him being corresponding author. The reason for him being corresponding author, he explains, is that he was the one uploading the paper and is the one who has "gotten all the annoying emails" (translated by the author³⁰, VU, PDoc4m, p. 6). After being asked in the interview whether corresponding authorship is attributed with value in his research context, as it is in many areas in the life sciences, he provides the following response:

I don't know, I didn't get the impression that it played a big role for us, but rather that being in the back is more important than who is now corresponding. At least I never realised that anyone was really interested in that. (translated by the author³¹, VU, PDoc4m, p. 6)

How can such deviating valuations of authorship positions or in this case a specific authorship label be explained? On the one hand, the postdoc's remark illustrates the lack of conversations among researchers about how and why they value various authorship positions and labels. On the other hand, it shows how care for authorship positions or labels can be reinforced if they are assumed to be of value in future assessment situations. Moreover, infrastructural aspects can also have an effect on authorship practices, as the last author either has to give someone else the password for uploading the paper and filling out the necessary information or do it themselves if they want to be corresponding author.

Stories told by researchers from particular subdomains, both in the field of physics and life sciences, in which authors' lists are alphabetically ordered — sometimes with the exception of the first author — underscore the relevance of first, last, and corresponding authorship in other research areas. These narrations display a paucity of discussion or conflict concerning

these authorship positions, thus demonstrating — through comparison — their relevance in other areas of research. Indeed, the valuations of contributions that determine whether a researcher is designated as an author, along with the subsequent valuations of authorship positions and the adjudication of which position bestows the 'right' credit for the knowledge gained, the 'right' recognition for one's work, and the 'right' capital within the symbolic rationality, are of relevance.

For a PhD in the life sciences, these latter valuation processes are more challenging because "positions are even more complicated" (IWS7, PhD13f, p. 31). Other participants of this discussion group agree by underlining the difficulty of such decisions and adding how "bad" (IWS7, MS8m, p. 31) those they can be. The observation that authorship positions are "a very sensitive topic" (IWS12, PhD34f, p. 11) shows how researchers care for them. But other researchers do not perceive author ranking as so tricky. One group leader, for example, explains how she deals with cases in which she is not sure whether to list one member of the group or the other as the first author of a publication: "[Y]ou ask them and you ask who thinks did more, and we never come to conflicts. That never ever happened" (VU, PI7f, p. 4). In addition, she may then choose to make one the first author on one paper and the other the first author on the next paper. Another senior researcher also adopts a more positive stance, arguing that the current system of authorship positions is "well set up and also clearly shows who was in charge, who did the hard work" (translated by the author³²; VU, PI2m, p. 3).

The different value ascribed to the various positions on an author list becomes also visible in how "no one really cares a lot" (IWS2, PDoc3m, p. 20) when they are listed as the third, fourth or fifth author. A senior researcher shares this sentiment, as in his experience, "honestly, the first author, the corresponding and the last author is being thought about, but the rest doesn't matter at all. It doesn't matter at all, because frankly speaking, whether you're fourth, fifth or third" (translated by the author³³; VU, PI5m, p. 13). Already as a PhD, many are well aware of which positions researchers usually care for in their respective research environment: "I know, first author is the leader and the last author is the correspondent, but outside that I don't really care [about] the order" (IWS12, PhD40m, p. 29). A prevalent narrative strand interrelated with the aforementioned predominant objects of care is the critique of the severe focus on specific authorship positions. To illustrate, for a researcher in the life sciences, the emphasis

on positions is challenging to align with the proliferation of long-term data sets in her field, which are the outcome of empirical work by multiple researchers over an extended period. Yet, the datasets are then published in a single publication for which only one person can be first or last author, thereby distracting attention from the other researchers who contributed significantly but are neither first nor last author.

Furthermore, the analysis of researchers' stories about authorship positions highlights the uncertainty and lack of knowledge, particularly among early career researchers, concerning authorship decision processes. This hesitancy presumably stems from the common opaqueness surrounding the authorship decisions of other researchers, which is further explored and discussed in Chapter 8. Moreover, they may also be unsure about to which extent their valuations of the first or last authorship positions overlap with the understandings of the responsible actors at funding agencies or in hiring committees who evaluate their publication records. Accordingly, a certain tentativeness may be noticeable when talking about those positions. For example, a postdoc, in the rather uncommon situation of compiling the author list for a publication, is unsure when asked about how others might assess his publication record, which usually identifies him as the first author and with, as of yet, no last authorship papers:

I just don't know if it wouldn't be more beneficial for me as a postdoc to be first author, because that would be more important for my career. I don't know. Last author would then be when you supervise people. I haven't thought about it that much because it wasn't that relevant. I would just think that first author publications would be more important for me right now. (translated by the author³⁴; VU, PDoc4m, p. 12)

His answer illustrates the not-uncommon lack of knowledge and the resulting attempts to make some sense of the authorship practices prevalent in his surroundings. Moreover, it shows how the significance of valuations of positions is also affected by one's own formal and symbolic position in academia. For the respective postdoc, the transition from his PhD position to his first postdoc position has been rather smooth. Hence, others have considered his publication list as valuable and he has not (yet) experienced any negative consequences resulting from authorship decisions deviating from his and his peers' valuations. This can mean that he may have not (yet) experienced a situation in which he had to make his valuations explicit, thus disclosing his understanding of why being first or last author is justified. However,

when asked in the interview for a further explanation of how he came to the conclusion outlined above, he argues that, for a postdoc, doing research is still more of a priority. This is reflected by placing the postdoc at the top of the list of authors since first authorship symbolises — for him — a significant involvement in research activities. Last authorship for him signifies more that the researcher manages several research projects and thus can function as an indicator for a certain formal position, tied to different, more managerial tasks. In the context of his current position, the status of first author is of greater significance for him because he perceives it as a demonstration of his substantial contribution to the advancement of scientific knowledge.

7.3. Middle authorship: Does it matter for your career and accountability?

Instead of commencing with the highly charged positions of first or last author, or the role of corresponding author, middle authorship is placed at the centre of this subchapter. This order is adopted to illustrate how researchers speak about an author's position, which is often of little concern to them, thus making the high value ascribed to the other positions and roles in the following subchapters stand out even more. Contrasting the value attributed to as well as the care directed to the positions in the middle with, for example, the position of the first author by declaring how "if I'm the first author, I don't really care who else is in the middle" (translated by the author³⁵; VU, PDoc4m, p. 6) or "if I'm the first author, I don't care who is there in this rest of the names" (BGSP, 1_1.1, p. 15), underscores how a wide range of researchers value the middle author position.

Comparing and contrasting the contributions while relating them to specific authorship positions in general gives insights into researchers' valuation processes. A postdoc recounts how he did the majority of the experiments and wrote the paper, while the two researchers who share second authorship provided "tremendous data which were essential for the paper" (VU, PDoc6m, p. 6). The aspect of comparison seems to be crucial in how researchers justify including researchers in the middle of an authors' list. One group leader describes that "typically in the middle there are people who have made some significant contributions but compared to the other people they're not...equally important" (VU, PI1m, p. 7). Also, collaborators who are not the lead authors of a particular publication may appear in the

middle of an author list. Depending on the field, researchers may have a certain expectation about the number of collaborators that are included in the list. For a life scientist, for example, it is “mostly logical that two people [of the other group] are involved, namely the one who is the boss, who organises it and so on, and one who sits and pipettes” (translated by the author³⁶; VU, PDoc7f, p. 8).

However, researchers frequently do not care for authorship positions somewhere among several other researchers because “this is not really helping you anyway” (IWS6, PDoc12f, p. 20). Coming to the conclusion that “one more in there doesn’t really matter” (IWS6, PDoc12f, p. 20) would be another example of a valuation being strongly shaped by imagined future assessments of their publication list. Rather than reflecting upon whether the authorship position, corresponds in their understanding, with their valuation of the contribution made, researchers may speculate how others might understand and correspondingly assess it — which in turn forms their own valuation of the position and the value they assign to it. Moreover, the frequently somewhat low value attributed to an authorship position in the middle in valuation processes informed by the epistemic realm may additionally discourage researchers to care for and thus value these positions highly. Researchers being listed as authors in the middle, may mention how they “just contributed a small part” (VU, PDoc3f, p. 20) to the paper, thus indicating the rather low value bestowed on their contribution following the value knowledge gain. Nevertheless, instances of researchers placing importance on the inclusion of further researchers in the middle of an authorship list can be observed in the data. The motivation behind their endeavour to incorporate all individuals who have made a significant contribution can be interpreted as an indication of placing considerable value on recognising every individual’s work.

Postdocs may often find themselves in the position of an author in the middle, since it “is usually the post-doc who supports several projects and therefore earns several papers, but from a middle position” (translated by the author³⁷; VU, PI2m, p. 12). The statement that postdocs are assigned to “whatever [position] in the middle, that’s not important” (VU, PDoc1m, p. 12) for engaging with and supporting the research of PhD students reflects the generally limited value placed on such (supervisory) activities in academia. It is also evidently demonstrated in the narrative of a PhD student about how a postdoc is virtually never the

primary author in their group in a collaborative paper, “even if the postdoc does of course a lot of work, but then it’s more seen as supervising PhD students” (BGSP 6_1.2, p. 7). In a similar vein, a postdoc speaks of “how much real work you are doing as opposed to how much supervision” (VU, PDoc1m, p. 12) when discussing the differing amounts of supervision he has undergone in his three postdoc positions. The question therefore arises as to how this differentiation can be explained. Apparently, researchers may perceive specific aspects of their daily work as ‘real work’ and therefore highly valuable, such as data collection or analysis, which result in either knowledge gain or publication. In such cases, the instrumental perspective on work seemingly prevails. Consequently, the value attributed to supervision is likely to be comparatively low because the valuation criterion of connection, presumably in combination with the value of publication, plays a crucial role in determining the extent to which value is attributed to the contribution in the form of supervision. Aligning this valuation with the value assigned to authorship position means likely ending up somewhere in the middle of an authors’ list. This rather common devaluation of supervision is particularly problematic for postdocs, who are often responsible for supervising PhD and possibly master’s students, since supervision can be quite time-consuming.

Middle authorship exemplifies shifts in research practices because an increasing number of researchers in both fields of the life sciences and physics are now engaged in producing ‘good research’, resulting in a greater number of authors (in the middle). In contrast, as various researchers posit, in an unspecified former time, ‘good research’ could be created by an individual researcher or only a few researchers, usually reflected in a short list of names. However, this is (for the most part) a myth, as outlined by Shapin (1989), who argues that a prevalent “understanding of scientific discovery tends to stress momentary flashes of individual insights as opposed to extended trains of collective work” (p. 561). The worry about having too many authors on a publication, often found in early career researchers’ narrations, might be explained by them holding onto an image of science from the past, even though they have never lived and worked in this undefined past (see for example Felt, 2017a; Ylijoki, 2005 and Section 2.3.1). This imagination in turn impacts their valuation of how many authors are the ‘right’ amount. Such concerns and considerations are also evident in researchers’ justifications for adding another researcher in the middle of the list of authors. Alluding to the generally rather low value attributed to middle authorship might be part of a researcher’s

justification for listing another researcher as a co-author somewhere in the middle of a list of authors. The underlying argument is that this listing does not negatively affect the value attributed to first authorship, thus it, for example, does not diminish the credit signalled for their epistemically significant contribution. Furthermore, it may also indicate that the researcher considers whether having an additional co-author will negatively affect the symbolic value, i.e. the capital gained from one's own authorship position, which in turn illustrates how the valuations of authorship positions are also interrelated. The concerns of early researchers in this context can be ascribed to their apprehensions regarding the awareness of more senior researchers, who typically wield significant influence over formal assessments, regarding the increase in the average number of authors per publication over the past decades. Or early career researchers might be worried about how the material working conditions of their research field, shaping authors' lists and thus resulting in longer or shorter lists of authors, are overlooked by other researchers in future formal assessments of their list of publications. Such a lack of knowledge may result in senior researchers diminishing the value attributed to an author's contribution, resulting in a diminished epistemic credit and a reduction in symbolic capital for the junior researcher. This phenomenon engenders a scenario in which junior researchers may find themselves dependent on personal endorsements from their superiors, as senior researchers may regard these as more valuable and reliable evidence of an individual's competencies than a list of publications. This, in turn, undermines the desired advantages of a metrification of research(ers).

Lastly, while guidelines pertaining to good scientific practice dictate that authorship invariably carries responsibility, researchers' accounts of their own sense of responsibility are more nuanced. Indeed, researchers may assert that they perceive their responsibility to be limited to their own contribution, rather than extending to the entirety of the publication. This perception of responsibility appears to be contingent upon the extent to which the specific authorship position provides a researcher with assumed credit. Interdisciplinary collaborations, however, present a particular challenge, as the responsibility for one's own contribution, even if one is listed in the middle, can be crucial. The example of a researcher with a computer-based role, who considers it "illusory that I can judge" (translated by the author³⁸; VU, PDoc4m, p. 15) the epistemic value of the experimental contributions, illustrates

this point. Consequently, he does not feel responsible for these parts, despite his awareness of how this is specified differently according to the common authorship guidelines. Nevertheless, in his role as the primary author, he considers it his responsibility to uphold the epistemic value of the entire article to the greatest extent possible, in contrast to the more passive role he would assume if he were among more than a dozen middle authors.

Similarly, researchers attribute a specific form of responsibility in their accounts to carrying out measurements for other researchers who do not have access to the instruments. Such tasks are not necessarily clearly signified in the listing of authors, as they are often translated with authorship positions in the middle. Regardless, one may be the sole author of a publication who can ensure that the section on the measurement methodology is accurately presented. This example demonstrates how a particular configuration of the material dimension in a research area, i.e. a dependency on equipment, is intertwined with the epistemic realm and ultimately affects researchers' responsibility. The often brought up time limitations of researchers, furthermore, may lead them to then focus 'solely' on those aspects of a publication, for which they have some kind of expertise.

A total absence of felt responsibility among middle authors may be subject to criticism from researchers, because it has the potential to increase both their workload and enhance their self-perceived level of responsibility. Nevertheless, omitting them from the author list can be incongruous with one's valuation of their contributions, which may warrant their designation as middle authors: "I can't kick them because they didn't do little enough. And then I feel frustrated that I'm supposed to write the paper, or the majority of the work, and hunt down these people to make them contribute" (BGSP 7_1.2, p. 9).

These lines of argumentation demonstrate how, drawing upon researchers' narratives, for many, responsibility for a paper varies depending on the authorship position, yet nevertheless does not cease to exist. Consequently, a researcher may decline to be listed as a co-author in the middle, as illustrated by the following narrative from the field of physics:

[A] paper was published based on some data that was taken before I joined [the collaboration]. And I had absolutely no idea, however, about that so I opted out. Because I don't want to be on a paper, like later someday someone sees and asks, 'you're on this

paper, I have a question about it'. No, sorry, I'm not responsible for that. I really don't want to take that responsibility for something that I was never involved in. (BGSP 7_1.2, p. 11)

Along similar lines, a quite experienced senior postdoc carefully talks to recently graduated researchers and junior postdoc and tells them "you have to also think about what you're doing and what you want your name associated with because now you will be followed by what you do" (VU, PDoc3f, p. 13). The purpose of these conversations is to convey his awareness that, in the context of formal evaluations of researchers' track records, the mere number of publications is not the only criterion for assessment. After a certain academic age, the reception of publications by the relevant scientific community is also taken into account. Consequently, these conversations can be construed as care practices, as care "seeks to improve life" (Mol et al., 2010, p. 15), a goal that the postdoc appears to be pursuing. The subsequent subchapter on last authorship provides further insight into how caring for others can contribute to career advancement and thus improve a researcher's life.

7.4. Last authorship: Who needs it, who cares for it and why?

The narratives about last authorship are very illuminating because it is not only senior researchers — those in the position of a group leader — but also postdocs who can be listed as last authors — making it a contested position. Or, in the words of one group leader, "last authorship is problematic because that depends on the supervisor" (VU, PI1m, p. 12), indicating the individual who usually retains the final say in case of conflict.

A compelling illustration of a conflict of this nature can be found in the account provided by an empirical physicist who places significant emphasis on the extensive nature of his contributions: "I was doing all the jobs from the first to the last author, to get the data, to think the experiment, to design the experiment, to get the result, to analyse, to write the paper" (VU, PDoc1m, p. 8). Hence, irrespective of whether the value work or knowledge gain, or even the value publication, guides his valuations of his manifold contributions, they are imbued with the greatest amount of value. Nevertheless, he is not the last author of the publication he talks about here because the researcher overseeing the lab claimed last authorship. This is even though, from his perspective, the last authorship position would be the one that indicates the legitimate credit for his contributions, and possibly the 'right' form

of recognition as well as potentially the 'right' capital. The postdoc tells that he is reminded of — and thus compares these kinds of authorship decision — to the behaviour of a child insisting on making up the rules of the game because they are the owner of the ball necessary for the game, since the lab leader argued that, without the provision of the lab equipment, the publication would not exist. Hence, the social and material dimensions are strongly shaped by the lab leader and the dimensions matter insofar as the postdoc is then listed as first author and the leader as last author. The lab leader would presumably only refer to the material dimension (i.e., him providing the lab infrastructure) when asked to elaborate on his valuation process when describing both the postdoc's and his own contribution(s). It is less likely that he would speak of the fact that, due to his higher hierarchical position and the related power imbalance and him attributing great value to being listed as last author, he simply pretends to highly value the provision of infrastructure and thus makes the authorship decision in his own favour.

However, according to another group leader, if the senior researcher "tries to see the whole thing neutrally [...so that] no false vanities arise, then it's a normal thing that the younger ones can also be the last authors, where you say that was clearly the focus of them" (translated by the author³⁹; VU, PI2m, p. 14). This researcher's statement indicates his understanding of 'normal' authorship decisions and how 'false vanities' should not influence authorship decision-making processes.³³ The fact that this researcher then goes on to emphasise that "in our environment here, which I know thus, it is looked after that the positions are deserved, where one stands" (translated by the author⁴⁰; VU, PI2m, p. 14) shows how highly he values ranking decisions that are not influenced by vanities on the part of either the senior researcher — who is usually the last author — or the more junior researcher, who aspires to be the last author. Rather, in his view, the epistemic value of the researcher's contribution should be paramount, since it is on the basis of this value that they either 'deserve' or do not 'deserve' last authorship. Another researcher appears to adopt a similar stance, since he deems shared last authorship or researchers emphasising being third author on a publication to be a "little bit silly" (translated by the author⁴¹, VU, PDoc4m, p. 10). This statement may be indicative of

³³ How the potentially more selfish character of researchers can influence this process is further discussed in Section 8.2.2.

a minor irritation on his part when he perceives others to be excessively influenced by symbolic rationality, in turn prompting them to make efforts to establish the value of such positions in terms of symbolic capital. Or it might not fit with his situated normality, in which only the first and last author make very valuable contributions from an epistemic point of view, for which they deserve proper credit. Accordingly, for a researcher, the designation of last author may confer visible credit for a contribution of epistemically significant value, for example, in the design of a study. In such a case, a researcher may not wish to concede last authorship: “[W]here the study is mine so to speak, I still want to be the last author and don’t necessarily want to put somebody else in that place” (VU, PI1m, p. 12). A sense of epistemic ownership may therefore assume a pivotal role, superseding the symbolic logic of ‘needing’ the designation of the last author. This is indicative of a recurrent shift in researchers’ valuations of last authorship as they progress through their scientific careers. In such cases, the underlying value directing the valuation of an authorship position is rather whether it gives the ‘right’ credit for the value attributed to their contribution in terms of knowledge gained. That this materialisation of the credit, assigned mostly in the form of last authorship — but also middle authorship in the case of collaborations — may also signify capital in the symbolic logic could also be a nice side effect. However, their care for an authorship position may be more rooted in the epistemic realm rather than informed by the symbolic logic.

Last authorship also carries significance because it facilitates identification of the individual responsible for the project that resulted in the paper, despite the team’s contributions. For a life scientist, last authorship shows “who is really responsible” (translated by the author⁴²; II, PI17m, p. 16). It is therefore crucial for group leaders, who are usually the last authors on papers coming out of their group, that the members are aware of how their work inevitably reflects on the group leader’s reputation. Accordingly, a group leader tries to convey the following message to his group members:

And none of it in the process is allowed to be sloppy or badly done or out of order or anything. That’s just our currency; that’s the credibility of me as well. And I say to them, I don’t want my credibility to be shaken by any blunders that you make. So definitely not. Because I only have the credibility. (translated by the author⁴³; VU, PI5m, p. 20)

Or another group leader assumes the stance that “if the work is exclusively of my group, then is my position absolutely irrelevant. I feel extremely high responsibility for every line that is

written on that" (VU, PI7f, p. 12). Quotes such as these also demonstrate the interconnectedness of authorship practices: The lack of diligence exhibited by a subordinate has the potential to negatively impact one's standing within the scientific community, given the interconnected nature of being an author on the same publication. This issue may be further compounded when the group leader occupies a position of authorship that is associated with an attributed overall responsibility for the epistemic value of the paper, such as last authorship.

Senior researchers' inclination to be credited as last authors may appear exaggerated to more junior researchers, given that group leaders' appraisals of their contributions may differ from those of their group members, as evidenced in Chapter 6. When asked if she was aware of the common critique by more junior researchers that a last author 'only' contributes the research idea, a group leader in the life sciences responds as follows:

Yes, I do and I begin — that's happened to all of us. I'm sure it happened to me when I was a PhD, but you reflect on it and a different idea changes your course by a degree or two. If you multiply that by a lifetime, you're going to end up in a completely different shore and that's what they don't get. [...] and this course correction of the supervisor is a gift. It is a conversation, and it changes lives, but it is forgotten, and it is undervalued, 100%. (VU, PI6f, p. 7)

Her response is rather understanding to the hypothetical struggles of junior researchers to understand why a senior researcher's contribution is deemed valuable — because she felt similarly as a junior researcher. Thus, the group leader's ability to remember how she felt in the position of a PhD student or postdoc, or simply to imagine herself in someone else's position, is likely to facilitate conversations with them and mitigate potential authorship conflicts. Alternatively, the following position adopted by researchers does likely not engender any conflicts:

I feel at my career stage, I don't really care too much whether I am the last author or not in a paper. So, if I can help somebody else's career by being the second last, then I should just do it" (VU, PI1m, p. 12).

Removing oneself from the last authorship position as a group leader can be interpreted as caring for one's subordinates because it may help to advance their careers. However, based on the perception of a postdoc, such care practices are directed at postdocs "who were

already a fit in a PI position. During such cases it happens quite a lot [...], to make sure that they have a PI position" (VU, PDoc6m, p. 9). This suggests that only if a postdoc has demonstrated their epistemic worth in the opinion of a senior researcher is the latter willing to game the metrics, so to say, indicating a certain order.

However, other senior researchers, even if they care about the careers of their group members, may not be aware of how important the last authorship can be for a postdoc. Or that being consistently listed as a postdoc with one's former supervisor may be a signal to others of insufficient independence from them and an absence of overall responsibility for a publication. This is exemplified by the narrative of a life scientist who has not been acquainted with the significance and value attributed by others to last authorship. Consequently, such responsibility is not reflected in her publication record. As she recounts, she was consistently second last author on publications with her former supervisor, even though she "was actually very independent from a very early age, but didn't formally demand it. Which I regret in retrospect, but afterwards you're always wiser" (translated by the author⁴⁴; VU, PDoc7f, p. 5). Interestingly, instead of discussing it openly with her superior, it took her mentor's "awareness that I need it more than she, who is retiring, and that it is of course objectively justified" (translated by the author⁴⁵; VU, PDoc7f, p. 5) for her to be listed as the last author. Meanwhile, other researchers, in retrospect, had the "luck" (translated by the author⁴⁶; VU, PI2m, p. 14) of already being last author as a postdoc, which they now recognise that it has "has helped a lot with research proposals or other things, even at a young age, to be the last author" (translated by the author⁴⁷; VU, PI2m, p. 14). Such examples demonstrate how "the narrativization of luck [...can be] indicative of a feeling of being 'out of control'", as noted by Loveday (2018) in her interviews with researchers with limited contracts.

7.5. First authorship: To feel (not) cared for

Who becomes the first author is typically not within the control of those who are considered potential candidates in this regard. While postdocs and group leaders may tangle with each other in terms of who is listed as last author, in addition to conflicts only among PhDs or, less likely, postdocs, first authorship can become a battleground between postdocs and PhD students, with group leaders often having to assume the role of arbitrator. A group leader in

the life sciences is keen to avoid such conflicts by making sure that in his “lab, people have their defined projects, so there’s no clash. [...] So, it’s very clear who’s first author” (II, PI10m, p. 21-22), exemplifying a practice of care in relation to the identified value of clarity in the context of first authorship. Kerr and Garforth (2016) have also employed the notion of care in a comparable context, describing how a group leader is “caring for careers” (p. 7) of their group members through their efforts to take “active measures to ensure that projects ‘did not bump up against each other’” (p. 7). However, such a clear separation may not be possible in all research areas or even research projects, indicating one of the many aspects forming situated normalities and, in turn, authorship practices.

If a group leader is also the supervisor of the PhD student involved in such an authorship conflict, they may feel a particular responsibility to support the student in accumulating enough first-author publications to qualify for graduation. Correspondingly, as one group leader argues, it can be conceptualised as a supervisor’s responsibility that PhD students have the opportunity to progress in their scientific careers by graduating “in a reasonable time frame” (IWS3, PI3m, p. 26), the length of which is dictated by the current structural conditions of the science system. Similarly, both PhD students and postdocs frequently attribute the responsibility for ensuring that PhDs have enough opportunities to be listed as first author or as second author to the respective PhD supervisor. This ascribed, but also formal, responsibility as a supervisor can lead to authorship rankings that are not necessarily perceived as fair by the supervisor — even though they make the decisions:

[W]hen a doctoral student is involved, it is normally the case that the doctoral student always comes first, if that is his work. Regardless of whether he has actually done the most work. That’s a bit unfair, isn’t it, so I’m never in first place. (translated by the author⁴⁸; VU, PI5m, p. 11)

A life scientist expresses a comparable perspective when discussing PhD students who have not yet published any papers, and who, in exceptional circumstances, may require substantial support, as she reports:

[They] were weak in formulating and writing, sometimes a lot of extra work was necessary, but we still didn’t demand it [, the first authorship]. Learning process, yes. I would perhaps do that differently today. Nevertheless, the PhD was still in front, because he also structured the experiments, so to speak, and so on. There is a lack of clarity. You can

discuss whether that's fair or whether he should go somewhere else. (translated by the author⁴⁹; VU, PDoc7f, p. 8)

Examples such as these highlight the importance of including narratives from researchers at different career stages in order to gain a full sense of the range of ways in which researchers value authorship practices. Contrary to the perspective held by many PhD students, who might perceive being listed in such situations as the first author as fair, these viewpoints represent a notable divergent stance. The previously quoted group leader's reflections on first authorship, however, continue and nicely illustrate how imagining oneself in someone else's position can further shape one's valuation of authorship practices:

I don't do that [listing myself as first author on a paper] as a matter of principle, because senior authors do that too, but I don't, because I think it's nonsense, I don't have to be in first place. Not even if I've done a lot for it, I think that should definitely be left to the younger ones, but for the postdocs it's often a difficulty because in a project in which a postdoc and a doctoral student work, it's always the doctoral student, because he needs it, who stands. So that's a bit difficult sometimes, to be honest. Because I think that the postdocs often deserve more than the PhD students to be in the first place. But...it's always difficult, you know, I mean, what is a first authorship, so to speak, really? (translated by the author⁵⁰; VU, PI5m, p. 11)

As this quote indicates, a group leader may not only value a particular authorship decision from their own position, thinking about their own contribution and the value they ascribe to their own and others' contributions, but they may also — because of their position and experience — think about how other researchers involved may value the decision. They may therefore be aware of potential discrepancies in the assumed valuations, which may be impossible to resolve.

Thus, for postdocs, it can be rather difficult to publish first authorship publications. Indeed, as a physics PhD student memorably notes, "the PhDs have the priority, and they have the pressure to publish to finish their PhD. The postdoc is not considered someone who needs" (BGSP 6_1.2, p. 7-8) first authorship as urgently. A researcher in the life sciences perceives it most challenging for postdocs to publish (first-authored publications), since they often support PhD students and may be then listed somewhere in the middle. They are, however, well aware of how crucial it is for their career progression to list such first-authored publications on their track record. Ensuring "that the postdoc is not demoted to help [...by

ensuring] they certainly have a programme where they are first and where others are, for example, in the middle and I am at the back" (translated by the author⁵¹; VU, PI3f, p. 16) is one example of a potential care practice by a researcher heading a group.

However, if a PhD has decided not to pursue a scientific career, this also affects the value they bestow upon particular positions, since, one could say, those particular valuation constellations will end for them in the near future. A postdoc recalls a PhD turning down first authorship for a paper by saying that they are "fine being the second author" (VU, PDoc1m, p. 3). It is likely that this decision was informed by the PhD candidate's choice to pursue a career outside of academia. Based on their stories, researchers tend to consider it less important to attempt to make themselves and their valuations of their contributions heard when they are unsure about whether they want to continue working in academia, since there may be no need to ensure that the value they attribute to an authorship position matches the valuation of their contribution.

One way of alleviating conflicts over first authorship is to split results into several publications, also reflecting the high symbolic value ascribed to first authorship. This practice, also known as salami-slicing, may allow listing several PhDs or PhDs and postdocs as first authors on a publication. However, it may impede the publication in higher-impact journals. Researchers thus ponder over whether such a publication in a highly ranked journal is valuable for them because of the value they and others bestow upon it according to symbolic rationality. And/or whether, within the epistemic realm, a "big paper" (IWS3, PhD5f, p. 15) is deemed more valuable. For instance, a PI regards it as "a bit silly sometimes" (IWS6, PI6m, p. 21) when he makes the decision to split papers up "for the sake of the people to keep their single first authorship" (IWS6, PI6m, p. 21). The researcher justifies his valuation with the negative effect it has on the epistemic realm, as instead of a bigger story, there is "this puzzle of things" (IWS6, PI6m, p. 21). But in terms of a supervisor's perceived responsibility for not just one, but all of their doctoral students, some may prefer not to have just one of their doctoral students in the spotlight, thereby seeking to mitigate potential future conflicts by splitting findings up to publish two or more and not just one single paper.

Striving for first authorship cannot be solely explained by the neoliberal logic prevalent in the

symbolic dimension, in which the accumulation of as much capital in the form of particular authorship positions and publications is the ultimate objective (see also Fochler, 2016). Researchers also care for “the recognition” (VU, PDoc3f, p. 5) of their work in the form of a materialised acknowledgement, i.e., authorship, and for visibly crediting the value of their contribution. Arguing for the significance to be credited for a valuable contribution in regard to the value knowledge gain, can be interpreted as an expression of researchers’ wishes that authorship practices prioritise the epistemic realm. Demanding that authorship decisions are rooted in the epistemic realm concerns not only the valuation processes of contributions but also the valuations of authorship positions.

Shared first authorship can be regarded as an authorship practice that aims to reconcile the symbolic dimension with the epistemic and potentially also the social dimension. Based on the observation of a researcher from the life sciences shared “authorship is becoming increasingly more common” (VU, PDoc3f, p. 4). A postdoc from the life sciences argues that, nowadays, in a time of interdisciplinary research with growing lists of authors, the frequent attribution of a lot of value to the position of the first author is no longer fitting. For him, it “becomes silly at some point to say that the one is now the most important” (translated by the author⁵²; VU, PDoc4m, p. 10). Such statements underscore the temporality of valuations, emphasising that they are subject to the prevailing ideal or normality at a given moment in time. Correspondingly, the practice of shared first authorship can be regarded as an attempt to encompass the material and social aspects, as well as potentially both the epistemic and temporal dimensions of current research. A shared first authorship can clarify — according to various guidelines regulating authorship — how the contribution generated by one of the first authors could not have been generated without the contribution made by the other first author and vice versa. They thus contributed equally in terms of the (epistemic) value attributed to their contributions as well.

For one researcher, with a shared first authorship publication, the authorship constellation does truly represent this description. She, however, adds how “[d]ifferent people could have different opinions what that means” (VU, PDoc3f, p. 5). Statements such as these point to the different understandings of authorship positions and labels among researchers, of which the researcher in question is well aware. Shared first authorship may also be the consequence of

a valuation process prioritising the social dimension, with the objective of averting conflicts by designating a researcher as the second first author. This is an illustration where another dimension wields significant influence, yet the materialised outcome in the form of the list of authors ends up being the same.

The outcomes of valuations of contributions are not only visible in track records of researchers but — depending on the authorship position — may also materialise as a citation. However, shared first authorship is not visibly credited if only the first author on the first position is cited. Correspondingly, a postdoc laments how citation practices do not truly reflect the intended signal of shared first authorship. It illustrates for her how this “whole system is really very, very stiff and hard to, hard to change” (IWS6, PDoc12f, p. 20-21). As conflicts surrounding authorship are perceived to be situated “inside the system” (IWS7, PhD13f, p. 30), the localisation of the responsibility is then argued to be among the researchers themselves who “with each other have to agree on how to deal with this, with these issues better and how to find a better way” (IWS7, PhD13f, p. 30). In some discussions of ‘the system’ and its perceived immutability, researchers hold the researchers at the top at least partially responsible because “they are kind more powerful to affect the system somehow” (IWS7, PDoc13m, p. 8). As the ‘somehow’ hints too, researchers’ imaginations of the possibilities for actions concerning ‘the system’ tend to remain rather vague. Nevertheless, within a research group, the group leader has, in the view of researchers not being in this leadership position, leeway for improving authorship practices. For an early career researcher, their situated normality concerning authorship practices “always also depends on each lab and on each PI” (IWS12, PhD34f, p. 29). In another life science discussion group, two PhD students also agree on how authorship practices depend “on your boss. And you’re completely dependent on your boss” (IWS7, PhD13f, p. 32). Thus, not surprisingly, junior researchers might describe themselves being “lucky” (IWS12, PhD40m, p. 29) when they tell a story about a time in which they were acknowledged with an author position aligning with their valuation of their contribution. Judging from the accounts of many doctoral students, quite a few face authorship decisions they do not agree with at some point in their first few years of research. It is important to note that this does not imply that the decisions of those in more powerful positions are wrong and the junior researchers are right. Rather, it is an example of the recurring gap in valuations between researchers at different stages of their careers, which may be exacerbated by a lack

of understanding of each other's valuation processes.

7.6. Corresponding authorship: Practical considerations and personal touches

A marked elusiveness is also detectable in researchers' narratives when reflecting upon the role of the corresponding author, who is likely to be either first or last author. For instance, a few researchers say that they have only learned of the value many colleagues attribute to corresponding authorship after several years of working.

A researcher who did their PhD outside of Europe recounts how her supervisor "didn't have this concept of the corresponding author as a sign of prestige or status or whatever it is" (VU, PDoc3f, p. 6). Her supervisor's corresponding authorship practices were rather guided by what they deemed to be "practical" (VU, PDoc3f, p. 6). In the postdoc's recollection, the supervisor probably saved some time by not being responsible for corresponding with the respective journal to which the publication was submitted. This demonstrates how aspects that comprise situated normalities, such as the widely experienced temporal constraints faced by researchers, can shape authorship practices. Based on her remark, the postdoc, who has been working for several years in Europe, is still unsure of which value principle(s) valuations are based on that attribute value to corresponding authorship as the 'or whatever it is' hints at. Her reference to 'prestige' could be interpreted as an allusion to its role as a form of capital in assessment contexts. Alternatively, it could be construed as an evocation of its value in enhancing one's standing within the academic community. Adding that, in her current understanding, corresponding authorship is usually tied to the position of the lab leader, she introduces the social sphere as another possible dimension shaping the meaning of corresponding authorship, since it can reflect the formal position. Alternatively, researchers may argue to deduce from who contributed what, who should be designated as the corresponding author. In a life scientist's understanding corresponding authorship — which for her is "synonymous with first authorship, so to speak" (translated by the author⁵³; VU, PDoc7f, p. 2) — encompasses the following contributions:

[T]he corresponding authorship should have the person who had the idea, who thought out the structure, who set up the organisational network to bring together the various,

with us it is often really different areas of work that contribute different techniques. That should be the person where the threads come together, whom I ask when I have a thematic question, not. So, that would be my understanding of that position. I would be very interested to know whether this is quite differently lived out. (translated by the author⁵⁴; VU, PDoc7f, p. 2)

Linking corresponding authorship to specific contributions makes it seemingly relatively clear; however, the postdoc is aware of how understandings can vary. Furthermore, the description provided suggests that the contributions are more indicative of an experienced researcher, such as a postdoc, as opposed to a PhD student on the cusp of publishing their first paper. Another example also illustrates that the value attributed to corresponding authorship varies depending on one's own conception of it, even if one has previously worked as a researcher in Austria or in the European area:

I have experienced that in some other branches, also partly in physics, it is very much appreciated to look at who is corresponding author, because it says between the lines who has really done something, and to pick out one from the order. As a personal touch, I don't handle it that way, so for me it has relatively little importance, who is corresponding author, for me that's really the one where you say, if there's a request from a journal, then please have your say, OK. But I have to admit that I'm perhaps not representative of how my colleagues do it or who might look at where the asterisk is with corresponding author. (translated by the author⁵⁵; VU, PI2m, p. 8)

Statements such as these show that established researchers may well be aware that their own conceptualisations do not necessarily coincide with those of their colleagues. Thus, there is a degree of scope for 'personal touches' that outsiders or researchers with considerably less experience may not necessarily be aware of.

The leeway that researchers may have to adapt authorship practices to their liking and their sense of what is 'right' may be (slightly) at odds with the practices of their peers. Authorship decisions following fixed rules, known to all group members, affect what researchers care for, insofar as the quasi-non-existent leeway concerning authorship practices makes their care practices less necessary. For example, a research group with the fixed rule of making the first author the corresponding author never has to have any discussions about who is named as the corresponding author. Transparent decision-making processes may require researchers to worry less about how they will be placed on a particular author list — but it also requires the alignment of prevailing authorship decisions with researchers' values — or at least the

acceptance of a mismatch on the part of the subordinate researcher.

7.7. Reflections on voiced ranking justifications: Which matter more?

As researchers generally care about positions such as first and last and the role of corresponding author, it can be concluded that these positions are highly charged and can even evoke emotional responses. As a result, the recognition of one's work and the acknowledgment of its (epistemic) value in the form of (first) authorship can instil positive feelings. Illustrative of this are the sentiments expressed by a junior researcher: "I am happy when I have a first author paper" (translated by the author⁵⁶; VU, PDoc4m, p. 13). Subsequently, stronger emotional reactions can occur when the subjectively legitimate claim to an authorship position is not fulfilled. Moreover, researchers' care being directed towards assuming a prestigious authorship position, possibly inhibits care practices being directed towards other researchers and possibly also towards tasks usually not being rewarded with valuable authorship positions. Caring for a specific authorship position can also provoke a behaviour rather negatively viewed from a standpoint strongly shaped by the social and also the epistemic dimension. As also touched upon in Subchapter 6.9, it is a contentious issue whether valuations are to be rooted solely in the epistemic realm and if they can and indeed should completely evade and outweigh the symbolic rationality. Frequently, researchers implicitly give insights into their perceived 'right' order of values by devaluing other presumed values guiding practices. Being on a paper for the sake of being on a paper, and consequently regarding authorship (positions) merely as representations of symbolic capital, is widely devalued in both the analysed discussion groups and interviews. Numerous instances in this chapter demonstrate this point.

However, not being aware during one's years as a PhD student of the tremendous impact of the symbolic rationality on a researcher's career, and possibly even during the first years as a postdoc, can have detrimental effects on one's career. A senior scientist talks about how in hindsight she "should have cared more because it is important and I should have pushed more to publish things that are interesting, but not quite complete, I think" (VU, PDoc3f, p. 9). Hence, caring — to a certain extent — for symbolic capital in the form of authorships and publications may be crucial for pursuing a career in science. Nevertheless, by emphasising

how her publications would have even been interesting, since her (preliminary) findings would have been potentially interesting for colleagues, she points to the epistemic value of these publications. Justifying the value of such a publication in terms of the potential knowledge to be gained from it is, in her eyes, potentially a more legitimate justification than saying that she should only have published more because of the symbolic logic. Thus, such a narratively constructed conciliation of the epistemic realm and symbolic rationality would allow her to perform well according to symbolic logic without abolishing or at least fundamentally subordinating the epistemic realm.

Such an in-depth analysis of the justifications put forward for adopting a particular position on an authors' list can yield substantial inferences regarding the prevailing imaginations among researchers concerning the scientific system, lending support to the proposition that authorship can be regarded as a focal point of academia. Referring to the epistemic value bestowed upon various contributions is a far more widely accepted means of justifying a particular authorship ranking. This is particularly evident in the narration of a postdoc working in a field in which the authors in the middle are usually alphabetically ranked. She justifies her divergence of this practice by listing a colleague of her second by speaking of her "sense" (translated by the author⁵⁷; VU, PDoc5f, p. 4), as the following quote shows: "because I had the sense that we contributed equally, purely from what was written and in terms of scientific input, what is in this publication, that I had the sense that I would like to evaluate that equally" (translated by the author⁵⁸; VU, PDoc5f, p. 4). Her justification for this different positioning of authors is located in the epistemic realm, as she stresses the epistemic value of colleague's contribution. Interestingly, she argues on the basis of her 'subjective' feeling that this is the 'right' valuation, rather than on the basis of 'objective' guidelines regulating authorship — reflecting the limited relevance of guidelines to researchers.

The epistemic realm is also brought up when talking about first authorship by mentioning that one has written all or the majority of the paper or by having been the one to initiate the paper. In research environments, in which the epistemic dimension is guiding valuations of contributions, ranking authors can be then experienced as being "actually self-explanatory" (translated by the author⁵⁹; VU, PDoc5f, p. 10). Similarly, from the perspective of a group leader, the individual who merits first authorship is evidently the person who buckles down,

or in the original German, he speaks of 'reinknien': "[T]hen it's clear the PhD who buckles down is first and if the post-doc buckles down, that's his baby, then he can be first too, that's absolutely justified" (translated by the author⁶⁰; VU, PI2m, p. 13). Likewise, for a life scientist "it's straightforward order, at least for the first author. If it's your project and you spend like 90% of time on it, you're the first author" (VU, PDoc6m, p. 1). Or as a physicist puts it: "the beginning starts with who did most of the actual working" (VU, PI1m, p. 4). From these statements, it can be seen that a clear sense of epistemic ownership and a strong commitment, which can also be evidenced by a considerable investment of time, bring with it a certain clarity when it comes to first authorship decisions – and also act as a perceived legitimate justification for why first authorship is granted.

The epistemic reasoning is also visible in explanations as to why a person should not be given first authorship, for example, if they do not play a leading role in important epistemic contributions such as data interpretation or writing processes or do not generate them:

[S]o I don't really want to write a paper for someone, completto, and then put them down as the first author...., I don't really want to do that either, because that's too much work for me and then it wouldn't justify it, because then the person is actually not involved in the most important process, namely the interpretation of the data, and the whole thing at all. (translated by the author⁶¹; VU, PI3f, p. 15)

The last author may also justify their position by stating how, in conjunction with the first author, they were responsible for writing the article and possibly provided the overall idea for the publication. A life scientist describes the senior author as being the one who "is really guiding the conceptual innovation and is responsible for getting the team of people together with synergistic skills to get the idea over the line" (VU, PI6f, p. 4), exemplifying how justifications can tap into dimensions simultaneously, in this case the social and the epistemic.

Yet, the attribution of value to specific tasks may go hand in hand with the devaluation of other contributions or at least a specific ranking. Such a perceived ranking may result in the formation of the belief that "the ideas and getting the whole story counts more than doing all the work" (BGSP 1_1.1, p. 19), as a junior researcher formulates it. This sentiment can be interpreted as a dissatisfaction with the perceived devaluation of their own contribution, thereby underlining the emotional ramifications of authorship rankings.

Moreover, through the comparison of different contributions, the value attributed to contributions can change, as the story of a postdoc shows. Additionally, the narrative illustrates which justifications are openly voiced, while other considerations may only be speculated upon. The postdoc recounts how in the final stages of publishing an article, an external contribution was added. This addition impacted the senior author's valuation of the technician's contribution, which in turn affected the planned design of the authors' list. According to narrating postdoc, "[t]he argument that the senior author made to me was that the contribution of the lab technician was more similar to my contribution than it was to the external" (VU, PDoc3f, p. 7), hereby strongly suggesting changing the postdoc's single to a shared first authorship with the technician. Yet, the further narration indicates how the postdoc assumes that the senior researcher's valuation of the contribution was not solely rooted in the epistemic realm, as she brings up what she calls the "PI's desire to try to help the technician with her career" (VU, PDoc3f, p. 7). However, not only the technician but also the postdoc "needed that publication and we both needed it pretty badly" (VU, PDoc3f, p. 7). The symbolic dimension presumably thus strongly shaped the particular decision process, making the rise of "some very heated discussions" (VU, PDoc3f, p. 7) no surprise. Ultimately, however, the decision was made in favour of shared first authorship.

Hence, from the postdoc's perspective, the PI's care practices directed towards the lab technician have a detrimental effect on her epistemic credit for the publication, and result in a reduction of her symbolic capital. This narrative underscores a potential oversight with regard to care practices, namely the potential implications for and feelings of the individuals who are not the subject of the care practices in question. Furthermore, it exemplifies how the valuation process of a contribution can be affected by care practices, resulting even in a different authorship ranking. While it seems, the PI justified the changed authorship design by referring to the epistemic realm, i.e., the knowledge gained because of the technician's contribution, the postdoc seems to assume relational aspects diluted the valuation process of the technician's contribution as well as the technician's 'need' for a first authorship publication. An epistemic-based justification might be considered by the PI as more legitimate than one rooted in the social sphere and/or shaped by the symbolic logic. Since the valuation process is rarely made explicit or reflected upon, in many cases researchers can only speculate as to why a particular authorship decision was made — in contrast to the comprehensive analysis

undertaken in this thesis, which attempts to outline the principles guiding the valuations and how one or more dimensions may have impacted the process of valuation. Based on their mostly implicit assumption of which value may have been guiding which valuation or whether a dimension, in their view, impaired the valuation process, researchers either criticise or agree with the valuation outcome, hereby also indicating which value(s) and dimensions they consider as legitimate to be guiding and affecting authorship valuations.

Researchers taking the symbolic logic strongly into account may have trouble comprehending valuations governed by another value. For example, a technician with his own side project tells how there were “possibly some resistances” (IWS2, MA1m, p. 12) concerning him being listed as first author. The technician, however, justifies himself being first author because he has “done the work and written half the paper” (IWS2, MA1m, p. 12). He thus taps into the already familiar argumentation of how work should be recognised with (first) authorship. Also, the mobilisation of the epistemic dimension through the indication of having written half the paper resonates with researchers’ common justifications for first authorship, by emphasising having not ‘only’ generated data but having been involved in the process of analysis. Interestingly, the technician then proceeds to further justify the valuation judgment of him being rightfully first author by pointing out how this publication and respective authorship position might be valuable for future assessments if he ever decides to pursue a PhD. He thus strengthens his valuation judgement by drawing on the symbolic logic presumably also used by those being rather resistant of listing a technician as a first author.

It might not necessarily be resistance, it can also be uncertainty whether a technician even “cares about the orders” (IWS12, PhD40m, p. 30) of authors on a publication, as a doctoral student in another discussion group muses. This wondering about whether care is also directed towards artefacts such as authorship (positions) even if it is of no value according to the value-principle capital, governing the symbolic dimension, underscores the prevalence of this dimension in researchers’ authorship valuation processes. In contrast, it may be more comprehensible if the researcher claiming first authorship is a PhD about to finish, already looking for a postdoc position. A group leader in the field of physics explains how, in the event of a conflict, she takes into account for whom the first authorship within the symbolic logic is more valuable so that their career progression is not inhibited:

Actually, I can from that point already [...] judge, for whom this paper, this career stage is more important in this situation. In the case of conflict, right. So, in the case of conflict between a PhD student and a postdoc, PhD student wins. They help. Because that is more important, because it might be one of his two papers, he needs for the defense and then, yeah. If the contribution is equal, PhD wins, for sure. But this is just the way to promote them, right? To make them graduate, otherwise, they don't graduate, and this is not good for anybody. Not good enough for me. (VU, PI7f, p. 4)

Hence, the symbolic logic is not automatically decisive for her authorship ranking decisions, but only in conflict situations. Moreover, such decisions are not made altruistically, as the second part of her statement alludes to, since a non-graduating PhD student may negatively affect how she is then assessed as a PhD supervisor. Or, in the case of one theoretical physicist, the outstanding contribution of a PhD student compared to the other authors resulted in the authors not being listed in alphabetical order. However, the exception was based on the status of the PhD student, because if the student "was a post-, a colleague rather than a PhD student, we would have written this alphabetically. But because he's a PhD student, he needs this for his career, and he clearly made more than we did" (VU, PI4m, p. 1-2). It is only because the PhD student's contribution has been given value on the basis of epistemic considerations that, in a second step, symbolic rationality becomes crucial and affects the ranking of authors — again indicating again a preferred value order.

However, to conclude the subchapter with an example in which a different kind of value order can be discerned, the pursuit of a perceived correspondence between the valuation outcome of one's contribution and the epistemic value assigned to authorship positions can be superseded by the awareness that science is "also a kind of a social endeavour" (IWS, PI1m, p. 19). Such an emphasis on the relevancy of the social sphere has the potential to result in an individual becoming a co-author of a paper with which they do not fully agree intellectually, primarily to avoid causing offence to the other authors.

It is not solely the significant value attributed to authorship (positions) within the symbolic dimension that imprint valuations, but also relationships characterised by power imbalances: "So, it's, I think it's kind of tough. Because on the one hand, you want your work to be recognised, but on the other hand you don't want to be blackmailed or anything by it" (IWS7, PhD15f, p. 32). Withholding recognition of one's work by not appointing a researcher to a

specific spot in the list of authors does possibly not only negatively affect how comfortable they feel in the social sphere but potentially also impedes one's success in the symbolic dimension, since no symbolic capital is obtained. Moreover, it is also rather disadvantageous for the epistemic realm because without any authorship (position) no material credit is given. The dependency of researchers, especially those at the early stages of their career, gives those in charge a means of leverage, also comprising the perceived value of the overall authorship decision process, which is further elaborated below in Chapter 8.

8. The great valuations holding it all together: Researchers reflecting on and valuing authorship decision processes

[T]hey just get themselves into messes because they don't want to hurt people's feelings or they're not transparent or they think they can solve it later or they feel that everybody should be committed to cooperation and then, they're upset when they're not. So, I see it all the time. (VU, Plóf, p. 9)

In her reflection above, a senior life scientist puts forward a number of possible explanations as to why conflicts over authorship (order) arise, foreshadowing issues that are to be explored in more detail in this chapter — and that have been alluded to in previous chapters. For instance, matters such as caring for others can result in harm being inflicted on others or manifest in the judgement that decisions are unfair. Moreover, the importance of what the researcher refers to as 'transparency' is further explored.

More broadly, this chapter explores a valuation strand that intersects with the valuations discussed in the two preceding chapters because the three authorship valuation processes are inherently interwoven. How these valuations interact and co-produce each other has been partially investigated in the preceding chapter on researchers' valuations of authorship positions, as those valuations are closely associated with researchers' valuations of contributions. This chapter further explores these intricate connections and delves into researchers' valuations of authorship decision processes. Consequently, this chapter examines valuation processes that are even more overarching in nature, as they encompass the valuation of contributions and authorship positions.

8.1. Clarity and fairness as transcending values

Clarity and fairness, which have been touched upon in the preceding chapters, are the overarching values ascertained through exhaustive data analysis. These values govern the valuation of authorship decisions, i.e. the decision-making process itself, and provide researchers with a frame of reference to identify areas for enhancement.

The emergence of fairness as a transcending value is particularly resonant with the work of Boltanski and Thévenot (2021), which sought to formulate a “theory of justice that would take into account the diversity of ways to specify the common good” (p. 14). In a manner analogous to the conceptualisation of what constitutes a value put forward by Heinich (2020b), Boltanski and Thévenot (2021) emphasise that justice “has the property of bringing disputes to an end” (p. 34). Notwithstanding its references to Boltanski and Thévenot (2021), the thesis — and in particular, this chapter — does not proceed from the six identified principles of justice, whereby “every principle of justice is associated with a universe of objects that constitute a coherent world” (Boltanski & Thévenot, 2000, p. 213). Accordingly, the present chapter sets forth the findings of an open-ended approach, the aim of which was to examine the justifications provided by researchers with regard to the fairness of specific authorship decision processes and more general authorship practices.

As a junior researcher observes, fairness is a central consideration in individuals’ everyday lives, but it is also a lens through which one can view the broader patterns and structures within which those lives are situated:

[F]or me fairness, when I thought of it, it had very little to do on a personal level and way more on a societal level. So, I think that was very interesting that for a lot of people was about am I first author or second author or whatever. And for me it was very much about like a very representation issue in general, a societal issue of fairness. Also how, which is not really in the card but how science is distributed across the world, for example, that like all the rich countries do all the research and have all the research opportunities. And researchers from poorer countries have to go into other countries to even be able to do research. And so, for me fairness was a lot of those topics in my mind. Um, like mashed up into fairness [laughing]. And I think it’s very interesting that for a lot of people it was a very personal question. (BGSP 7_1.1, p. 23)

While those more profound inquiries are undoubtedly pertinent and intriguing, they are not the subject of this thesis. Instead, the focus is on the situated perspectives of researchers on their situated normalities set against the backdrop of authorship.

In this context, the perception of fairness concerning an authorship decision may be associated with one’s subjective experience of how one is treated, thereby highlighting the intertwining of fairness and relational aspects. For instance, the statement of a junior researcher suggests that the exclusion of researchers may be perceived as “disrespect of

people who work on something and yeah, I just think, yeah it's disrespectful, it's not fair" (BGSP 5_1.1, p. 30).

The value of fairness also plays a prominent role in the story of a postdoc from the life sciences who critiques authorship practices that do not ascribe value to negative findings, since the researchers who generate such findings, from his perspective, "contribute something to the story, even if it is not published" (IWS2, PDoc3m, p. 20) and should thus still be listed as co-authors. By arguing how such contributions shape the story told in an article, even if this is not visible, this researcher emphasises the complexity that, in his view, must be acknowledged when compiling a list of authors. Interestingly, the postdoc explicitly states the value guiding such decision processes, namely the value of "fairness" (IWS2, PDoc3m, p. 20). Moreover, he seems to be aware how this is not a "right" (IWS2, PDoc3m, p. 21), but according to his conceptualisation of co-authorship, it is "like a reward to, like your efforts" (IWS2, PDoc3m, p. 21); acknowledging an effort is thus a fair authorship decision for him. The line of argument from the researcher points towards his expectations of how authorship should be handled, no matter if it is formally regulated or actually rather in conflict with many guidelines. Hence, there is neither the responsibility to act in this manner nor the right to demand such an authorship practice. Thus, such an authorship decision could be framed as a care practice. Interestingly, striving for fairness might be in tension with guidelines laying out a researcher's right to authorship, indicating the disparity between researchers' valuations of authorship practices and those of journal editors or actors in the field of science policy. Consequently, an inclusive authorship strategy, even if not fully aligned with established guidelines, can be regarded as a measure to counteract the various constraints inherent within the scientific system, "coming from the funding agencies, we have constraints coming from the tenure committees, we have constraints coming from everywhere. But at the end, if we claim to be smart people, we can find ways around this" (II, P12m, p. 13). However, others may argue that such decisions primarily shaped the symbolic dimension, involving deliberation over who 'needs' the publication and/or the authorship position, can be deemed as "not fair" (IWS2, PDoc3m, p. 20). Especially if the involved researchers "did the same amount of work" (IWS8, PhD17, p. 13). In the eyes of various researchers, fairness means prioritising the epistemic dimension over the symbolic dimension in valuations of contributions. It is important to note, however, that valuations of authorship practices can shift as a result of a growing body of

experience. For instance, a postdoc is now well aware that her former supervisor's authorship decision to entrust her with the first authorship position on a publication based on her bachelor's thesis is rather a "peculiarity" (translated by the author⁶²; VU, PDoc5f, p. 1) and not the norm. Interestingly, not all researchers openly incorporate judgements into their narratives; some researchers present their stories in such a way that they are simply stating the 'facts', i.e. describing their (partially) unfairly situated normalities, which they perceive to be unchangeable.

Clarity, on the other hand, is a governing value whenever researchers critique the common opaqueness surrounding the valuations both of contributions and of authorship positions. This opacity is partly due to the fact that the processes of valuing contributions can be guided by different values and valuation criteria, potentially creating a source of conflict. The desire for clarity may ultimately stem from a lack of knowledge, which in turn generates uncertainty regarding authorship decisions and the overall professional trajectory. In this respect, Adam and Groves (2007) persuasively argue that "knowledge has been able to enhance the predictability of the realm beyond the present and thus increase the structural security of social existence" (p. 7). Nonetheless, the absence of clarity appears to be the norm, particularly for early career researchers who are often struggling to make sense of the authorship practices they encounter. Correspondingly, one PhD student explains her choice of picking the Authorship Negotiations card in a discussion group, because, in her experience, "everybody knows the horror story, most of the time horror stories of people who are really, really struggling with this part at one point or another" (IWS7, PHD13f, p. 30). This statement, on the one hand, shows how (early career) researchers construct their situated normalities by drawing on their own experiences and also those of others. On the other hand, it illustrates how researchers' authorship valuations can vary, thus leading to conflicts.

Early career researchers frequently attribute the responsibility for ensuring clear and transparent authorship practices to the researcher who makes the ultimate decisions concerning the list of authors. This individual may be the researcher's PhD supervisor — if the researcher is still a PhD student —; in other cases, the supervisor may also be the group leader and/or the PI, who is the deciding or at least intervening in papers by (early) postdocs within a group. In contrast, within numerous accounts of group leaders, the notion of clarity arose in

one form or another when describing authorship practices, suggesting differences in their situated normalities.

The delicacy surrounding authorship is also reflected in the example of a senior researcher telling a postdoc something along the lines of how “usually it’s a dark subject in science to talk about authorship list” (VU, PDoc1m, p. 11). A junior researcher concludes that, in their view, “there is a lack of, of debate and openly discussing” (BGSP 1_1.3, p. 18) authorship decisions. Nevertheless, “everyone always talks about it” (IWS12, PhD37f, p. 28) in the experience of a different PhD. But those talks seem to mostly take place after an author list is put together and not in the middle of authorship negotiations and decisions. The conflicting emotions concerning authorship, which are present in many narratives from researchers, are also clearly noticeable in how a postdoc talks about authorship, as “[o]ne would like to be someone who doesn’t care at all, but one just doesn’t live in a world where it doesn’t matter at all” (translated by the author⁶³; VU, PDoc4m, p. 11). This statement also alludes to the frequently observed discord between researchers’ desired and imagined value order and the order of their research environment.

Subsequently, following this introduction to the transcending guiding values, Subchapter 8.2, examines aspects informing the situated normalities of researchers, which are to a large extent perceived as immutable within science. These situated normalities are, namely, having a limited contract in the early stages of a scientific career, it being rather the exception not to work in a collaborative setting within the data set analysed, researchers naturally being human, and academia being a highly hierarchical working environment. A thorough investigation into the consequences of authorship’s almost taboo status is presented in Subchapter 8.3, addressing its impact on researchers’ grasp of authorship practices and the intricacies of authorship debates. Additionally, an analysis of researchers’ inclination towards clear authorship decision processes is conducted in this subchapter. Furthermore, Subchapter 8.4 delves into researchers’ self-conceptualisations and their perceptions of others, set against the backdrop of symbolic rationality exerting its influence, which researchers both acknowledge and negate. A section is also devoted to how the crucial role of the group leader shapes the self-perceptions of both group leaders and those around them. In Subchapter 8.5, the challenges researchers may face in balancing care and fairness are explored, as well as

the objects of their practices of care that can be traced in their accounts. The chapter concludes with Subchapter 8.6, which offers broader reflections on how the multiplicity of values identified around authorship practices matters to researchers, also in light of the metrification of research(ers).

8.2. How researchers' situated normalities matter

The notion of normality has already been introduced in Chapter 3, and its relevance discussed, by exploring how both narrative and valuation serve as sensitising concepts in this thesis. In addition, Subchapter 5.4 outlined how, in many cases, researchers come to judge what their individual situated normality is through comparison. Whereas previously the focus was particularly on how researchers' situated normalities affect the valuation of their contributions, e.g. through the 'normality' of changing contributions over the course of a scientific career or the 'normality' of working predominantly in certain places, this subchapter presents those aspects that are deemed by researcher to be most relevant to the overall authorship decision process.

8.2.1. *Limited contracts and teamwork complicating decisions*

Limited contracts and collaborative work settings are both part of the situated normalities of the majority of researchers within the analysed data set. Furthermore, they are referenced either individually or together when contemplating the impact of structural conditions on authorship practices. For instance, there is a risk of working in a "too close collaboration" (II, PI16f, p. 16), as a group leader notes, potentially causing authorship conflicts among collaborating researchers. Conversely, working on a project alone, which is an exception in the analysed data corpus, affords the luxury of stating that "authorship at the moment is relatively little present" (translated by the author⁶⁴; VU, PDoc2f, p. 2), implying how no care for authorship is necessary in such projects. The greater the number of researchers involved in a research project, the more complicated authorship decisions might become, as a theoretical physicist muses, and for whom four or five authors on a publication is already a lot. A lower number of potential authors facilitates authorship practices because it tends to be clearer who has contributed what and in which manner. Meanwhile, the limited contracts of both PhDs and (especially) postdocs can inhibit the possibility of any one single researcher

working on the same research project from the beginning till the end in many instances. When discussing temporary contracts, the assumed negative impact on the scientific quality is also raised in various instances.

A group leader speaks with some conviction about how to deal with authorship issues that arise when one PhD student has worked on a project but then has left before finishing, after which another takes over and then manages to publish a paper. For him, if the first PhD student “made an actual contribution that then, you know, allowed this work to be done then I think they should be in the paper” (VU, PI1m, p. 8). This statement resonates with the valuation processes described in Chapter 6. The PI assigns a value to the contribution of the first doctoral student based on that contribution’s necessity for publication, employing the valuation criterion of connection. Given that the term ‘work’ in this context represents successful publication, publication is thus the paramount value.

In instances where researchers have transitioned to other labs and are engaged in other projects, their access to the material or digital space in which authorship is negotiated becomes further restricted — assuming that there is a discussion at all. This is exemplified by a postdoc criticising the authorship practices of a group leader he worked for. In his eyes, the group leader uses such situations to their advantage by providing new group members with unpublished findings from former group members, which are then published, “[s]ometimes without any improvement, they just publish it, and they exclude the whole or they put you in a very bad position in the paper when it was your main finding” (VU, PDoc1m, p. 9). Thus, the epistemic realm seems to not be the main dimension governing the group leader’s valuations of contributions. The respective researcher is unsure about the group leader’s value(s) guiding such practices, “maybe she didn’t like something, and it was not clear what this person didn’t like” (VU, PDoc1m, p. 9). This confusion, the postdoc expresses, can also be detected in narratives of other early career researchers, illustrating the opaqueness surrounding many authorship practices. Such transgressive authorship decisions are, however, not the norm for the postdoc but rather the exception.

Slightly different narratives concern the matter of handling valuable contributions by bachelor’s and, especially, master’s students after they have left the lab. Yet they are still

related to the larger issue of how to deal with the absence of researchers in authorship decisions due to their limited contracts. As they usually work in a lab only for a few months, it can be difficult to reach them after they have moved on because, in many cases, a publication is not finalised within this period. In particular, former master's students who decide not to pursue a career in science are often difficult or impossible to contact. The researcher(s) compiling the list of authors must then decide whether to include a former student in the paper, usually somewhere in the middle, even without their explicit consent. Visibly acknowledging someone else's contribution, deemed valuable for the publication, with co-authorship, even if the future author may not even care about it (anymore), can be interpreted as possibly being guided by their understanding of the value of fairness. Such an idea of the value of fairness might be informed by the epistemic dimension, in that fair authorship decisions involve recognising all contributions valuable in terms of the epistemic dimension. It is, on the other hand, unlikely to be driven by the symbolic dimension, since former students who are not seeking a career in academia will not take part in any future assessments in which this symbol of authorship might positively affect the evaluation.

Researchers joining a project after it has started can also have the impression of being in a rather disadvantageous position when it comes to authorship. For a postdoc, contributing to the analysis would be translated to authorship, hereby referring to the epistemic realm to justify the value attributed to their contribution. However, even though she, according to her story, explicitly stated this expectation, she somehow was not part of the publication. How this decision was made or on what value(s) the valuation of her contribution was based remains rather opaque to her but, in her impression, it was made solely by the researcher with the most power in the hierarchy. Whether the temporal dimension was decisive, with the postdoc not having been part of the research project for 'long' enough or whether the professor did not assign enough value to the contribution to acknowledge it with authorship from an epistemic standpoint or even whether other aspects might have shaped the authorship decision remains unknown. It does, however, point to how the many different dimensions can affect authorship decisions. The postdoc's main point of critique, however, seems to be grounded in the social sphere, where she condemns the power of the professor to singlehandedly make an effective decision. The postdoc's choice, to stop working on this research project after this decision, could be framed as a form of self-care.

Crowded workspaces, in general, may affect the potential clarity and fairness of authorship decisions because several researchers are working simultaneously on various research projects. The allocation of further resources, namely individuals, may assist in resolving a research question or, at the very least, accelerate the process. However, as a junior researcher notes, in terms of valuable authorship positions, “it’s the benefit for the PI, but I don’t know if it’s a benefit for the PhD students. Not of, not all of them” (BGSP 2_1.3, p. 6), since only one can be designated as the first author. In comparison, a junior physicist provides an illustrative example of clarity, given that within their research “group it’s pretty common that we have very well-defined roles in projects and it’s very (.), it’s, it’s sort of clear who will be the first author and who will be contributing” (BGSP 5_1.2, p. 18). Presumably, these roles have been defined and assigned by the lead researcher to ensure that contributing researchers are not lured into false hopes, even though the group leader may benefit in terms of publishable output. This example reflects the trade-offs that group leaders in particular have to make on a regular basis.

Meanwhile, the simultaneous involvement of PhDs in several research projects — even if the involvement is quite limited — can lead to “inflationary” (translated by the author⁶⁵; VU, PDoc4m, p. 1) authorship practices. Notably, this characterisation of authorship decisions signifies exposure to diverse authorship practices. In contrast to the group he was working at as a bachelor’s student, the postdoc’s research projects are clearly delineated in his current research group, due also to the separate research topics. In his experience, this results in less collaboration among the various doctoral students. However, it is the normality in this group that articles are published only with the PhD student and the supervisor on it — in contrast to the group in which he gained his first research experience.

Difficult decision processes can be averted if the ownership is relatively clear. However, even a shared first authorship can further complicate the process, potentially inhibiting the publication process as both first authors need “to take the responsibility to get it out the door” (VU, PDoc3f, p. 7). Rather than a diffuse responsibility circulating among several researchers working on the same project, a single researcher being in charge diminishes the vagueness surrounding the authorship ranking. Interestingly, the (slight) power imbalance, structuring the social dimension by assigning who is in charge of a research project, facilitates

authorship decisions. Such an argument among early career researchers is a rather rare example of how hierarchy can positively impact the value of authorship decisions. Moreover, time can also impede the assigned value of the authorship practices: The longer researchers are working on the same project, the more diffuse the ownership of the contributions usually becomes. This can then tamper the valuation and ranking of contributions, materialising in the list of authors, making the authorship decisions potentially less fair.

The narratives of researchers include rather positive stories as well. There is one, in which a supervisor ensures a researcher, who was at that time a bachelor's student and had already moved on, was listed as co-author on a publication based on a project the bachelor's student initiated. The respective researcher cites both her contribution to the project and the good relationship with the supervisor when telling the story of how she became co-author of this specific publication. This narration nicely illustrates how one aspect, such as the issue of limited contracts, often negatively affecting authorship decisions can be outweighed by, for example, caring relationships among the researchers involved. Even if, from an epistemic standpoint, her co-authorship would be legitimate due to her contribution, the senior researcher could override this valuation of the contribution due to their hierarchical power. The researcher, telling this story, adds how she looks back differently on the practices of her former supervisor as she now knows, based on her experience, "that that's not normal" (VU, PDoc3f, p. 2). In contrast, bachelor's students working for a very limited time on a project might be mentioned in the acknowledgements, "if they're lucky" (VU, PDoc3f, p. 2), thereby also implying a certain normality.

8.2.2. *Human nature at work in authorship negotiations and disputes*

Narratives in which the social sphere is at the centre cover the relations researchers have with their peers: both at the same or a similar career stage or with researchers whose academic age differs noticeable from their own (and who are thus most likely at another stage in their career). Besides the career stage, relations can also be characterised by whether they are with people from the same research group or with competitors or collaborators, possibly from another subdomain or even another discipline. Irrespective of the nature of the relationship researchers refer to in their narratives, it becomes evident that "personal relationships in work [...] do influence things" (VU, PDoc3f, p. 2) and thus shape researchers' situated normalities

and, ultimately, authorship decisions. Indeed, for almost all PhD students and postdocs, whose narratives were analysed, it is their situated normality that authorship decisions are either not made by them or are only made in collaboration with others. On the one hand, this dependency on others, or the impossibility of authorship valuations escaping human shaping, is described as immutable and is repeatedly said to have a detrimental effect on authorship practices. On the other hand, by introducing the value of fairness, researchers highlight the potential for human care practices aiming to make authorship practices as ‘good’ or ‘fair’ as possible.

Authorship disputes are a much-lamented issue among researchers. Speaking of “unnecessary conflicts” (IWS1, PDoc2f, p. 12) hints at the desired climate among researchers — one that is free of conflicts. Disputes being seen as unnecessary implies that there are, or should be, other ways of dealing with authorship issues. However, authorship is complicated by “human nature” (IWS1, PDoc2f, p. 13) or researchers’ “egos” (IWS4, PDoc6m & PI4m, p. 30), succinctly summarised in the following observation:

Per se, we are just normal citizens. We are not better, not worse — you can find all human traits in scientists as well. We are also egoists and we, also positive, and have our ideals or also, but it is just, yes - are just people, no? (translated by the author⁶⁶; II, PI7m, p. 3).

Human nature, possibly combined with a rather egoistic character, may increase the likelihood that researchers overestimate the value of their contributions relative to others or overstate their share when asked by journals how much they have contributed to a particular task. Thus, negotiations surrounding authorship “really sometimes brings up the worst of human nature” (IWS1, PDoc2f, p. 12). Not only in the context of authorship practices but also in other debates about science and scientists, the human factor, so to speak, should not be forgotten:

So, I think one thing that is very important not to lose sight is, so in the science community, you still have people, persons, like you know, these are real human beings so they have all the, you know, let’s say defects, they have their own issues, they have their, you know, they have their own fallacies, etc. (II, PI12m, p. 1)

Stories about these aspects may often be told in such a way that the narrating researcher cannot understand the behaviour of the other ‘egoistic’ researcher. A postdoc comes to the conclusion, after participating in research projects involving both theoretical and empirical

researchers, that in these cases, “[e]veryone just says about themselves, their work was more important” (translated by the author⁶⁷; VU, PDoc4m, p. 10). This complicates the ranking of the contributions of several researchers, which turns into a list of authors with more or less valuable authorship positions. As a group leader, one might attempt to mitigate the egocentric tendencies of researchers, potentially even considering it to be within one’s purview. One group leader, therefore, makes the following appeal: “The people who contribute, we should encourage them and the people who refuse or only think about themselves, we should eliminate them — that’s quite, so, that’s how I try to keep my group or interact with my group” (translated by the author⁶⁸; II, PI7m, p. 5).

The perceived egoistic nature of some researchers, which can be an advantage in authorship negotiations, raises concerns among other researchers about how non-egoistic character traits can be detrimental in such debates. One postdoc fears that it is due to his “personality” (VU, PDoc1m, p. 11), resulting in him not being “strong enough about that point” (VU, PDoc1m, p. 11) of deserving last authorship; so far, he does not have any last authorship positions in his track record. Apart from the fact that he sees the social sphere as a crucial element in authorship negotiations, such an argument also points to the perceived locations of responsibility for these authorship outcomes. Considering it to be his “fault” (VU, PDoc1m, p. 11) implies that he should take responsibility for the lack of last authorship rather than seeing it as a structural problem. Moreover, when junior researchers “complain in my head but not outside or will complain to my friends” (VU, PDoc6m, p. 7), their superior may have the impression that they agree with the respective authorship decisions. In contrast, junior researchers “who may have been less modest, but then felt, ‘Hey, I want to too.’” (translated by the author⁶⁹; VU, PDoc7f, p. 6-7) may be more likely to be included in author lists. This can be especially true if the researcher responsible for compiling the final list feels similarly, as can be seen when this postdoc talks about her decision-making process: “I was always very diplomatic and the atmosphere in the group was important to me, so that I often conceded something to people” (translated by the author⁷⁰; VU, PDoc7f, p. 7). Or a group leader talks about how, in the past, colleagues of hers “insisted on being last author but I was unaware at the time of what they were actually doing, even though I funded the research and conceived of the research. But if you’re a pushover, you’re a pushover” (VU, PI6f, p. 1).

Gender issues may also affect decision-making processes. For example, a senior female researcher recounting the story of a male researcher who assertively claimed last authorship, which in her view was unjustified, reflects on gender differences in authorship because “we women don’t fight that way, I think” (IWS5, PI5f, p. 23). She thus points to broader societal structures that influence the negotiation of authorship. Similarly, a female senior researcher talks about “a male colleague in science who was my junior — I was more senior than him — but he was always very pushy, and he was sometimes senior author” (VU, PI6f, p. 1). Or another senior researcher also believes that gender (roles) can affect the way researchers interact with each other; this has the potential to also affect authorship negotiations, as described:

[B]ut especially science at the high level or at the highest level needs a certain nastiness factor — I don’t say it quite, quite nasty — and women, usually in my experience, are not willing to really behave like (..), I shouldn’t say it, but they’re not willing to behave uncollegially or to behave aggressively or over-aggressively. But a lot of women who are very successful are — they have this, this certain elbow check, technique out and they do it. (translated by the author⁷¹; II, PI20m, p. 12)

This thesis, however, does not claim that the senior researcher’s observation applies to all negotiations around authorship but the inclusion of this quote is intended to illustrate how societal structures, moulding gender roles, might feed into researchers’ research practices in general and authorship practices in particular. Furthermore, given the information available about the various interlocutors, the thesis is limited in its ability to make assumptions about the potential influence of a working-class background on authorship processes. However, for instance, Loveday (2015) draws attention to the challenges associated with one’s class based on interviews conducted with university staff from working-class backgrounds (see also Langhout et al., 2007). As Ahmed (November 17, 2013) astutely observes, privilege functions as an “*energy saving device*” (para 8), an assertion that is indisputably valid in the context of authorship negotiations.

As the statement “of course, there are egos” (IWS4, PDoc6m, p. 30) indicates, this kind of influence on authorship negotiations is considered to be a given and thus constitutes the situated normality for researchers. This layer of the social sphere, which affects the valuation of contributions, is seen as seriously undermining the valuations that are made, since “in

principle it's super-easy to negotiate authorship" (IWS4, PDoc6m, p. 30). Meanwhile, a PhD student who articulates their disquiet on the matter, stating that "kindergarten relations in science annoy me a bit" (IWS8, PhD17, p. 14), as they impede the advancement of scientific progress, and, thereby, casting aspersions on this specific facet of the social dimension from a standpoint anchored in the epistemic realm. Balancing the objective of communicating scientific findings to peers and the public through scientific publications whilst perceiving that one's "position is in principle where it belongs" (translated by the author⁷²; VU, PI2m, p. 11), may not always be so simple. However, "if someone somehow has a very big ego and is unhappy [...], I can imagine that it's sometimes difficult to deservedly get the position that he would like to have" (translated by the author⁷³; VU, PI2m, p. 14). A senior researcher with a great deal of experience in handling authorship conflicts from more of a mediating position reports that she refers to the authorship guidelines of the most prominent journals in her field when a researcher unjustifiably claims authorship of one of her papers. This approach to dealing with such supposedly illegitimate claims by referring to authorship guidelines is a rather unusual approach within the data corpus analysed; typically, regulations are not brought up as a means of countering selfish behaviour.

8.2.3. Hierarchical structures (not) mirrored in authors' lists

Another layer of the social sphere forming researchers' situated normalities is traceable in researchers' narratives addressing the inherent power (imbalance) within the hierarchical structure of academia. Indeed, statements such that "normally the professor decides" (BGSP 5_1.2, p. 19) or "of course, in the end he [the group leader] decides" (IWS12, PhD35f, p. 29) reaffirm the instrumental role of the social sphere in shaping authorship decision-making processes and indicate a certain situated normality for junior researchers. Drafting a first version of the authors' list and the PI either adding or removing authors is deemed to be somehow better than not being involved in the process at all. These statements are underscored by the work by Satalkar et al. (2020), who describe those hierarchical relationships as one of the "systemic factors [which] seem to influence researchers' individual behavior" (p. 960). In particular, those who, due to their institutional position, possess minimal or negligible agency in the decision-making process when compared to those who wield more formal power, may feel that they are being subjected to arbitrary authorship practices. Junior researchers may thus perceive that the "hierarchy in science is sometimes kind of crushing to

the ones on the bottom, because I feel like PhD students do the most work and sometimes, they're the least valued" (BGSP 5_1.1, p. 4). It is this unambiguous juxtaposition of contributions, as evidenced by the author list, that can engender a sense of worthlessness in researchers when it feels like it mirrors the value others see in them as a researcher. A postdoc contemplating their prospective career as a group leader articulates the ensuing resolution:

I don't want to belittle the achievements of my employees who did the experiments themselves by saying that I'm writing the publication. Personally, I would find that extremely unfair and, looking back, it would have somehow diminished my motivation because I would think, 'If I did the work, I'm no longer the first author.' I definitely don't want that. (translated by the author⁷⁴; VU, PDoc5f, p. 10)

Furthermore, this perceived inextricable entanglement of a particular formal position with a specific, valuable authorship position can be viewed as rather negative. This kind of critique is usually voiced by lower ranked researchers and not by those who, for example, run a lab. Not all researchers assume such critical positions towards the group leader being listed as last author:

Actually, the group leader has never actively insisted on it in any form, but it has simply turned out that we do it that way. I think we then pass it on in some form to our doctoral students, who then also do it, and they in turn pass it on to the master's students, who then also do it. So, I think that was relatively clear for us. (translated by the author⁷⁵; VU, PDoc5f, p. 11)

In contrast to other stories, the position of last authorship does not seem to be enforced by the group leader's formal power. Of course, the power might be experienced rather tacitly. Nevertheless, it aligns with the valuations of the group members, making it a clear and potentially fair authorship decision. Moreover, the quote shows the significance of the environment as it works to familiarising researchers with specific authorship practices.

Interestingly, some more senior researchers tend to downplay the influence they exert. Having, for instance, a postdoc and a PI discuss who is and should be responsible for deciding who is listed as an author reveals potential discrepancies. A PI might understand themselves as "only the leader of a negotiation process" (IWS1, PI1m, p. 21). For a doctoral student, authorship decisions are related to and strongly impact the relationship with your supervisor. To underline this judgement, she starts telling the story of a lab she knows where group

members “were basically blackmailed by authorship. Like, if you leave now, then your stuff will be published but not with your name on it, so you have to stay in this lab” (IWS7, PhD15f, p. 32). Drawing on their observations, an early researcher recounts that, in various labs, “people have suffered quite a lot, they don’t even have data in the end of their PhDs because of the power of the supervisors” (BGSP 1_1.1, p. 9) — let alone publications with them as (first) authors. Experiences such as these point to the tremendous impact that the supervisor can have on a PhD’s work and life. It not only negatively affects the relational aspects but also their epistemic and symbolic position in academia when a doctoral student is not able to generate any data within a certain period of time, which can then be used in a publication.

Moreover, researchers’ narratives indicate how the symbolic dimension can interfere with valuation processes, leading to the inclusion in the list of authors what is commonly labelled as guest or honorary authorship³⁴. Speaking of ‘political’ aspects impacting authorship practices or describing it as “bit of a game of who can twist arms the best” (IWS1, PDoc2f, p. 21) hints at this issue. Potentially formed by their situated normalities researchers speak differently about cases where “four of the fifteen authors are just there for the politics” (IWS6, PDoc10m, p. 25) without even having read the paper. While a postdoc perceives it as “a bit annoying” (IWS6, PDoc10m, p. 25), a group leader states how this should not be the case and another researcher straightforwardly calls such practices “unethical” (IWS6, RA1, p. 26). Even if such authorship practices are forbidden according to authorship regulations, within the symbolic rationality, it can make sense for the case of “three different groups, international groups, three different countries, three famous groups, [where] maybe the big PIs want to put more weight on that” (IWS6, PDoc10m, p. 26), as a postdoc articulates while striving to make sense of these kinds of authorship practices. It is thus not only the formal position of the PIs impacting the list of authors but also their success in the symbolic dimension. For a postdoc from the field of physics, it is his situated normality to include both the PhD and the respective supervisor from the research groups his group is collaborating with, even though “you know that usually it’s only one people [sic] who is doing the job” (VU, PDoc1m, p. 4). The social and possibly the symbolic dimension are relevant in listing the supervisor, whereas the epistemic requirements concerning a contribution have to be fulfilled by the collaborating PhD. In a

³⁴ See Section 2.1.3.

similar vein, early career researchers in particular can be more critical of the practice that “of course” (IWS3, PhD5f, p. 16) collaborators are listed as authors, or that they “always have to be included” (IWS3, PhD6f, p. 17) while, at the same time, having the impression that the relationship between the collaborators and their supervisor may be decisive for the composition of the list of authors rather than the deemed value of their contribution.

8.3. The taboo-like status of authorship

A salient issue emerging from the analysis relates to how seldom authorship is deliberated in an open way. This has consequences for the manner in which junior researchers acquire an understanding of authorship practices, as well as defining the parameters of when and by whom authorship-related issues are to be addressed. Both aspects are covered in this subchapter.

8.3.1. *The tacit learning of authorship practices*

Researchers’ socialisation processes surrounding authorship, and the ease or difficulty of these processes, can shape their career progression and, consequently, the perception of how free of constraints they feel in science. Feelings of perplexity and uncertainty — but also of confidence — can be traced in researchers’ valuations of authorship negotiations and decisions, mirroring their socialisation processes which are elucidated in this section. In light of the premise that “learning and collaboration are the key mechanisms through which community membership, belonging, and identities are gained and engendered” (p. 10) as succinctly summarised by Kastenhofer and Molyneux-Hodgson (2021), this subchapter likewise illuminates aspects of the researchers’ identities that are evident in their positioning vis-à-vis authorship practices.

Notably, researchers may present the authorship practices with which they are most familiar as self-evident in their narratives. To illustrate, one researcher mentions how, in exceptional circumstances, an employee of a company that provides some of the services required for their research may be included on a paper if they are also engaged in research activities to some extent. She is aware that this creates “a certain lack of clarity” (translated by the author⁷⁶; VU, PDoc7f, p. 8) but since “that’s just what I’ve come to know, I wouldn’t have questioned it

for now” (translated by the author;⁷⁷ VU, PDoc7f, p. 8). Similarly, a PhD student elucidates their normality of what it takes for group leaders to become last authors:

[F]or me, when I read a paper, like the last person in the paper I always assume like this is just the PI and this is the funding. I don’t have a positive-negative view on how like — for me it’s just that’s the default situation. (BGSP 7_1.2, p. 13)

Or, in another case, the question of whether to teach a particular technique to new group members may not even arise and then “it’s just normal that always this person does it and then is on each paper” (IWS12, PhD34f, p. 29). The researcher’s invocation of the notion of normality in relation to this authorship practice suggests an assumption of its immutable nature. The remark by a junior researcher that “[y]ou grow into the system and then you’re there and then you’re part of the system” (BGSP 1_1.3., p. 18) also evinces a perception of powerlessness. Similarly, a PhD student reporting the practice of always including cooperation partners acknowledges that “it is not justified in ethical terms, but we do it, we all do it as, it is going on since the, like many years now, so everybody knows it” (BGSP 5_1.1, p. 30). To provide another example of a somewhat cynical standpoint, emerging only a few years into an academic career, in which the entire responsibility is allocated to what is vaguely referred to as ‘the system’:

I think the whole system with the publishing and impact factors, and I really hate the system, and I don’t think these things represent in any way one’s skills or professionalism or what you’re actually capable of, so I think I screw the system, and I don’t care so much, yeah. (BGSP 5_1.2, p. 20)

Strikingly, this quote illustrates the PhD student’s perceived lack of room for manoeuvre resulting in a form of disengagement, which is evident in the expression ‘I don’t care’. Conversely, other researchers may assume in their early years how authorship (ranking) is a “common sense thing [...which goes] along with just normal ethical values that you have” (VU, PI7f, p. 2). However, they may subsequently discover through conversations with others that they have interpreted an author list from one of their publications in a different manner. Meanwhile, a senior life scientist recalls her PhD supervisor “was not happy when any of his students spoke to other supervisors. So, we were not encouraged to gain additional information. It was very much a bubble” (VU, PI6f, p. 1). Such experiences can alleviate a heightened awareness among researchers of the extent to which their own normality may not

(fully) coincide with that of others.

The process of socialisation into authorship practices is likely to be more tacit, which can be evidenced from the following quote from a life scientist: "There was no one to tell me what was customary, what significance what position had and how it actually works, these structures. So, there was no training in this direction" (translated by the author⁷⁸; VU, PDoc7f, p. 1). Similarly, a junior researcher attests that they "don't know if our lab has rules" (BGSP 2_1.1, p. 13). A physicist who is now heading a research group talks about how, as a PhD student, he learned what he calls an "etiquette" (translated by the author⁷⁹; VU, PI2m, p. 3) through the authorship practices of his group members. Similarly, for a life scientist, it was "by simply seeing and learning and watching, how do they do it there" (translated by the author;⁸⁰ VU, PI3f, p. 3) that she acquired an understanding of how authorship is handled within a research group. In addition, having either one's own "wealth of knowledge" (translated by the author⁸¹; VU, PI2m, p. 3), or having colleagues who share their experiences, supports one in making what one considers meaningful choices. In particular, PhDs commonly report seeking advice from other doctoral students or postdocs with whom they have a close rapport, as attested to in the following quotation: "[U]sually you just ask those people in the group that are the closest like career-step wise" (BGSP 6_1.1, p. 43). Consequently, supervisors may have limited insight into any challenges their students may be facing. In response to an interview question asking how he assumes his students learn about authorship practices, a physicist replies that he assumes it to be "from the discussions that we have when we write papers. I mean where else? We don't do formal training on author, on the list of authors" (VU, PI1m, p. 11).

As demonstrated in the previous chapter, a paucity of clarity can be detrimental to a researcher's career; one life scientist recalls how she was unaware of the importance of corresponding authorship during her PhD years, which led her to decline it, although in hindsight she now sees how she would have been entitled to it. Moving to another formal and therefore different authorship position can also be challenging, resulting in the loss of "quite a few senior authorships in my early years of mentoring" (VU, PI6f, p. 1), as one group leader reports. As a consequence, her "switch to the role of senior author is not necessarily clear in my early papers" (VU, PI6f, p. 1), which may have had a negative impact on how others

then interpreted her publications from this period. Thus, clarity in authorship practices encompasses how contributions and positions are linked in one's research environment and how positions are interpreted.

Insights into the meaning and thus the value that others ascribe to different authorship positions can be gained in a variety of ways. For instance, "[t]hat the last authorship, so to speak, I'll say it casually now, is the head honcho, yes, that, you humorously acquired that yourself in the PhD, even if only through comics. So that was already clear" (translated by the author⁸²: VU, PDoc7f, p. 1-2). Clarity about authorship practices and positions does not necessarily require explicit conversations or formal training, as this statement illustrates, but some kind of information sources — even if these are 'only' comics. Alternatively, participation in engagement methods such as RESPONSE_ABILITY can be a revelatory experience. For example, one participant acknowledges in the course of the discussion group that through listening to the others, they "realised that, like, the environment in my group is very good" (BGSP, 7_1.2, p. 6), including its authorship practices.

Certain milestones in a scientific career, such as supervising a PhD student for the first time, can prompt reflection on previously unquestioned practices. In doing so, researchers may also consider how to continue certain authorship practices that are in alignment with their own values, as evidenced by assertions such as "was very clear to me all the time" (VU, PI1m, p. 1). For one group leader in the field of physics, such moments of reflection led him to the realisation that "it's an established system, so it doesn't make much sense to swim against the tide, but that's not necessary" (translated by the author⁸³; VU, PI2m, p. 3). Another physicist espouses a similar viewpoint, noting that within his research group, they "have a way how we do this, which turns out is practically how we have always been doing this" (VU, PI1m, p. 2). A postdoc in the life sciences who perceives the authorship decisions in the groups she has worked in to be relatively clear and would like to continue with such decision-making processes. Furthermore, maintaining a responsible approach to authorship practices is of the utmost importance to her:

I know this from hearsay from others, where de facto someone is written on it who has zero to do with it, not an issue at all. So that was always correct among us. There's almost never been anyone on it who hasn't done anything, so that's how I experienced it in my

own way. And that's something I would never do differently, because then it feels uncomfortable, so to speak. (translated by the author⁸⁴; VU, PDoc7f, p. 5-6).

Having occupied different roles in a variety of authorship decision-making processes may alter researchers' perspectives on past authorship negotiations. One researcher recounts the experience of another researcher who she perceived to be somewhat overly assertive during the authorship negotiation process. Subsequently, that overly assertive researcher expressed regret for their behaviour, stating that the value of their contribution did not align with their demands regarding authorship. Or, in hindsight, a researcher may conclude how she "was lucky to work, say in a very decently handled atmosphere" (VU, PI7f, p. 3), an observation that alludes to other encounters or accounts of deviating experiences.

Furthermore, researchers may gain additional perspectives or further insights when advancing to roles of greater influence in authorship negotiations, where they are more involved and face a greater range of claims. A life scientist states that "I'm just now collecting [experiences], where I'm really pulling the strings more" (translated by the author⁸⁵; VU, PDoc7f, p. 7). Such shifts in symbolic and formal power can be reflected in shifting authorship positions, which may be difficult to stomach even if one agrees with them in principle. A group leader remembers:

[F]rom first to last [author] was a leap that wasn't always easy for me, but then you have to say, OK, the others also deserve to be in front and then it's somehow clear that it should be that way. (translated by the author⁸⁶; VU, PI2m, p. 4)

It cannot be overstated how vital it is to consider the position of the researchers talking, making sense of, and ultimately valuing, since their position within academia moulds their narrations. A senior researcher describes reflectively how "I probably wasn't as critical before as I am now. I certainly have more pointed ideas about life and science now and can perhaps articulate them differently" (translated by the author⁸⁷; VU, PI5m, p. 16). Consequently, it may be hypothesised that the senior researcher's presumed confident articulation of his stance in authorship decisions is not solely attributable to the authority derived from his formal position, but also to his refined opinions acquired through many years in science. Whether researchers of any academic age feel they have an opportunity to have a say in authorship decisions, and when they feel the ideal time to do so, is detailed in the following section. However, given

the tacit nature of the acquisition of authorship practices, researchers commonly encounter difficulties when attempting to engage in discussion on the subject.

8.3.2. *Discussing authorship: A question of responsibility and timing*

Authorship debates — if authorship (ranking) is discussed at all — are frequently confined to two or a small number of researchers. In many instances, those engaged in such debates are the very individuals who are subsequently designated as the first and last authors on a given publication. Hence, researchers may be absent from the space where authorship decisions are made, even if the final authors' list is crafted by members of their research group. Furthermore, one aspect of the social dimension, namely power because of a researcher's formal position, may also restrict access to the physical or virtual space where authorship is negotiated. According to stories of researchers, when multiple research groups are working together on a publication, the authorship decisions may be made only among the leaders involved and without any involvement from the various group members. Thus, authorship decisions can take place figuratively — but also literally — behind closed doors. Moreover, it illustrates how "[s]pace makes action possible and is itself the field of action" (Löw, 2008, p. 28). Conversely, one's absence from a particular space may preclude certain actions. Additionally, this can foster a culture in which authorship practices are felt by researchers to be rather opaque. Hence, even within a research group, the comprehension of authorship practices among the group members can be rather limited. The perception of opacity in authorship decisions is in many narratives evident not only among PhD students but also among postdocs.

The widely criticised absence of communication prompts deliberations on the matter of responsibility for initiating a conversation about how authorship decisions are made. Moreover, the process of unpacking one's own valuation processes within a conversation, thereby counterbalancing the opaque nature of authorship decisions and rendering them more valuable by making them clearer, can be regarded as a practice of care in this context. So, who feels and who is perceived to be responsible for initiating open conversations?

Several researchers consider debates on authorship (order) to be more of a collective endeavour and thus distribute the responsibility among the involved researchers. For

instance, a postdoc explicitly encourages in a discussion group PhD and master's students to "speak up and claim your rights" (IWS4, PDoc6m, p. 31) as in the end they also have "some kind of responsibility" (IWS4, PDoc6m, p. 31) for their career in science. Exercising this responsibility is, in the opinion of many PhDs, almost impossible. As one doctoral student articulates, the prevailing sentiment may be that they are not "able to say something, like not, not have the courage to say something because it could hurt you" (BGSP, 6_1.1, p. 24). They depend on the superior researcher not only for the finalisation of the authorship ranking but also for the prolongation of their contract or future recommendation letters. Early career researchers are especially likely to bring up that a conflict might have negative middle- and possibly long-term impacts on their career in science as reasons for the avoidance of an authorship conflict. The assumed negative effects thus impede the enactment of their responsibility.

Moreover, researchers' attribution of responsibility to the person in charge can also be explained by their fear of being viewed critically if they initiate a conversation about authorship. This concern arises in a multitude of instances in the data. A postdoc explains his concerns regarding asking about authorship in the following manner:

That gives the impression that I'm only interested in being on the list of authors and that I'm less interested in the content. And this way of putting it across, I find it totally interesting in terms of content, but my time is limited, and I have to know now how much I'll be rewarded for it, that's just difficult to formulate. (translated by the author⁸⁸; VU, PDoc4m, p. 11)

Subordinating the epistemic realm and prioritising the symbolic rationality is, in the view of the postdoc, usually disapproved — also by him. Nevertheless, he is fully aware of the tremendous significance authorship has on his career in academia. For a PhD or postdoc, being demoted to a less valuable authorship position can be perceived as "the end of the world" (IWS2, PhD4f, p. 16), or at least feel that way due to their rather insecure position in academia.

Meanwhile, a group leader who has hundreds of publications does not necessarily care if they are not listed as a co-author. Hence, they do not approach researchers not listing them and their group members afterward because they may not be aware that, for their subordinates,

the situation feels very different. Through this comparison to what impact it might have on researchers' future career, researchers more or less explicitly voice their critique of authorship decisions that they have come across. In many cases, this critique might never have been heard by the superior researchers. Accordingly, a researcher heading a group may have the impression that their group members "are perfectly fine and they are not [...and] you only find out when a situation blows up and it's a real disaster" (II, PI16f, p. 15), as one group leader puts it. Consequently, she advocates fostering "a culture where people can discuss things openly and can say things that they feel [there] is a problem" (II, PI16f, p. 15). Alternatively, researchers opt to maintain silence rather than "push. Or I didn't discuss, or I assumed I cannot discuss" (VU, PDoc1m, p. 11), as a postdoc recounts. Such narrations underscore the almost taboo status that authorship frequently possesses. This status impacts researchers in manifold ways. A PhD tells how she "always wondered" (IWS12, PhD37f, p. 28) about authorship when explaining her decision to pick the Authorship Card in a discussion group. Being intimidated to enter authorship negotiations can have negative effects, for example, if a more senior postdoc has no last authorships when applying for a grant.

Additionally, the limited experience of early career researchers in how contributions are ranked can cause them to doubt the legitimacy of their valuation, particularly in comparison with the valuation of more senior researchers. One group leader appears to be cognisant of this apprehension and arrives at the conclusion that "you just have to practise it, you have to do it regularly, so that people somehow also lose their fear of saying something wrong" (translated by the author⁸⁹; II, PI8m, p. 21). A senior researcher actively encouraging their group members to elaborate on their valuations; this can be viewed as caring. The objectives of such an exercise are twofold: firstly, to demonstrate that "nothing is set in stone, but it should just be a discussion" (translated by the author⁹⁰; II, PI8m, p. 21). Secondly, it can achieve the desired outcome of authorship practices, namely that "[e]veryone should ideally be satisfied with where they stand on a publication and also feel: okay, I'm appreciated here, yes" (translated by the author⁹¹; II, PI8m, p. 21).

Junior researchers in particular tend to more or less openly argue that the same person who has the power to finalise a list of authors is the researcher perceived as responsible for initiating conversations. Within the narratives of some early career researchers, instances can

be found in which they speculate about the possibility of group leaders intentionally leaving the question of authorship unresolved. This vagueness surrounding authorship is theorised to result in a higher investment among the involved researchers, leading to a 'better' publication. The group leader then profits from the valuable work carried out by others without acknowledging everyone with (a valuable position of) authorship, which is viewed as highly critical.

The issue under consideration here is not merely a matter of responsibility; it is also a question of the optimal timing for such conversations. This is evidenced through the analysis of data comprising numerous instances of deliberations on this matter. That illustrates that "responsibility also has to be understood as entangled with time" (Felt, 2021, p. 268). Moreover, researchers' examined narrations demonstrate "the power of time as an ordering force" (Felt, 2017a, p. 134), as discussions regarding authorship occurring at disparate points in time may yield divergent authorship decisions and ranking outcomes. It is plausible that the issue resides in the researchers' awareness of the substantial transformations a draft undergoes until it is published as a scholarly article. This evolvment over time, affecting the list of authors, is something that should be openly communicated, in contrast to making "certain promises" (IWS1, PDoc2f, p. 12), as one postdoc argues. Alternatively, this could be ascribed to the inherent uncertainty present in the nascent stages of any research project, which topics any publications arising from this project will address. Both aspects result in researchers grappling with the decision of when to initiate such dialogues.

To exemplify this phenomenon, a group leader arrives at the conclusion that it is "not practical" (IWS2, PI2m, p. 20) to engage in conversation at the outset, by highlighting the challenge of predicting the evolution of a research project or the allocation of tasks among team members. This rather common stance among PIs can be possibly traced back to their vast experience of the unpredictability in science, which some senior researchers also deem crucial for the progression of science. A junior researcher in a discussion group agrees with that stance, saying that "it's hard, so at the beginning of a project to say: these guys will be on the paper and in that kind of order, because it changes; so I don't know how you would make a rule or laws for how you put the authors in the paper" (BGSP 1_1.1, p. 12). A postdoc takes a similarly critical stance when recounting how, in a previous research group of his, "you

get on the paper once and then nothing changes" (translated by the author⁹²; VU, PDoc4m, p. 1). He thereby also challenges the practice of determining contributors at a specific point in time and subsequently fixing the authorship list, irrespective of any subsequent changes the publication may undergo.

Yet, researchers also question why peers are already in the process of writing the article (draft) without having decided on the order of authorship. One researcher recounts an "unhappy story [...where] it wasn't spelled out from the beginning" (VU, PDoc3f, p. 5) what kind of contributions were tied to certain authorship positions. Another asserts that, without such a conversation, "that's usually where the fault begins" (IWS6, RA1, p. 15). Otherwise, it "is not clear" (VU, PDoc1m, p. 10) when authorship is not openly discussed, resulting in a heightened likelihood of conflicts and/or a sense of a lack of recognition. For one senior life scientist, such an approach is "just a shock" (VU, PI6f, p. 9), as it almost inevitably provokes conflict, and yet it "happens all the time" (VU, PI6f, p. 9). A group leader tries to reconcile the unpredictability of research with the graduation requirements of PhDs and the need for postdocs to publish by adopting the following approach:

So actually, you discuss it beforehand, of course it's always the case that you don't know whether a publication will be published exactly as you thought it would be. And often you have so many that you say, let's make it two. Or that's not enough, let's put it together or something. That happens, of course, so you can't make it up one hundred percent at the beginning. But normally you can, and I'm very conscious of that when I have doctoral students, then you always try from the beginning, at least I always try from the beginning to think in terms of papers, to think in terms of manuscripts. And you know that they have to have these manuscripts as first authors. That means it's clear from the start that the postdocs who work with us need something of their own. (translated by the author⁹³; VU, PI5m, p. 12)

One potential solution proposed by researchers is the establishment of a recurrent dialogue on the subject. This, in turn, would imply that discussions about authorship need not be tied to a specific publication but that a more abstract and generalised approach to these discussions may be adopted. Such an objection, voiced by a member of a discussion group to the proposed timing of decisions regarding authorship can result in a shift in the perspective of another individual participating in the group, thereby exemplifying the positive effect of such discourse.

Despite there being no universally optimal moment for the discussion of authorship (order), the prevailing consensus is that the pursuit of “more transparency” (IWS2, PhD4f, p. 20) in the context of authorship is paramount, thereby ensuring that researchers are not confronted with immutable “facts” (IWS2, PhD4f, p. 20) at one point. By articulating these sentiments, the value of clarity is underscored and it highlights that discourse surrounding authorship and the respective position is not the prevailing norm. It also shows that this very normality frequently serves as the catalyst for conflict. For one physics PhD student, such considerations are not pertinent; however, this PhD student is cognizant of the fact that they have “been fortunate not to have to think about, because it’s usually, sort of, in my group it’s sort of clear” (BGSP 7_1.1, p. 15).

Overall, researchers’ reflections and considerations illustrate how “decisions about the timing of even the most habitual of actions are made on a unique basis and with reference to a particular context” (Adam, 1995, p. 22). Rather than there being one ‘ideal’ moment of speaking about authorship, the analysis indicates that early career researchers particularly consider authorship decisions to be ‘good’ when they are clear for the involved researchers. The argument that the unpredictability of research potentially inhibits such clarity is often refuted by declaring the researcher in the most powerful position responsible for (continuously) caring for clear authorship practices.

8.3.3. *Caring for clarity*

Alongside the use of the term ‘clear’, researchers also use the notion of ‘transparency’ when highlighting authorship decisions that they deem to be valuable. According to one researcher, “transparency is the best policy in this situation” (II, PI12m, p. 15), referring to the situation of negotiating authorship. Yet for him, a prerequisite for transparency to be fully effective is that “scientists are reasonable people” (II, PI12m, p. 15), which in the view of others may not always be the case.³⁵ Researchers’ efforts to create an environment in which clarity around authorship practices prevails may stem from their own experiences of working as emerging researchers in a context that they perceived to be rather opaque in terms of how decisions about authorship were made. Caring for clarity might mean actively initiating conversations

³⁵ See Subchapter 8.2.2 on the human nature of researchers.

with those for whom one is responsible, either as a group leader or as a supervisor, even if one is unsure of the impact:

This is probably a tired attempt to save others from making mistakes, or learning processes that were tedious for you, is probably only of limited help because you simply have to learn certain things yourself. But I always think that a little more communication is better than too little. (translated by the author⁹⁴; VU, PDoc7f, p. 16)

Clear communication, especially with those at the beginning of a potential career in science, is considered key by manifold researchers. One senior researcher believes that she has become “a good research scientist [...because of her] active reflection of how to communicate to the next generation” (VU, PI6f, p. 2), something she feels she did not experience in her early years as a researcher, which she deems “bizarre looking back” (VU, PI6f, p. 1). However, even though she does have conversations with her PhD students about authorship practices and how to approach a publication, she admits that even these efforts may not be enough:

I guess that is one thing I’ve noticed that even if we have discussed it, I should revisit it but sometimes I don’t because of the lack of time. Other things become more important during the process, and you think you’ve already discussed it. So yeah, it probably should be repeated once a year. It’s certainly repeated for every paper that we do, but not nearly in the depths that you and I have just gone through it [in this interview]. I’ll maybe just say this is why there’s a senior author and that’s why that and is that all clear or are you okay with it, along those lines. (VU, PI6f, p. 13)

Consequently, even senior researchers who are profoundly and actively committed to clarity may encounter constraints on their care practices due to external circumstances (for instance, in the form of time limitations).

Especially as research today is often a collective endeavour, which makes teamwork indispensable, from the perspective of a group leader, it is “of course important to communicate clearly from the outset: who has which project and who contributes to projects” (translated by the author⁹⁵; II, PI17m, p. 16); he and also the aforementioned group leader therefore attribute the responsibility to themselves because of their position as PI. The former group leader even goes so far as to say that “I take nothing more seriously than making sure that that is fair and transparent” (VU, PI6f, p. 2) when talking about how authorship is handled in her research group. For her, fairness and transparency are therefore the values of paramount importance in making ‘good’ authorship decisions. Another researcher,

meanwhile, keeps a written record of her conversations with the doctoral students she supervises — so that others can refer back to agreements made in the event of a dispute — thereby ensuring overall clarity.

Furthermore, a lack of care for clarity may also have adverse effects on the senior researchers themselves. Interestingly, researchers who have not personally experienced conflicts resulting from opaqueness surrounding authorship can nevertheless be aware of the adverse consequences that such situations can have:

[S]o it didn't actually happen to me, but I somehow experienced it, I think, and that's why I had the feeling, maybe my mentors or whatever told me, sort it out somehow, because if you say we're doing this together and suddenly you're somewhere in the middle and they're in the front and back, well, that can happen to you relatively easily if it's not clear. (translated by the author⁹⁶; VU, PI3f, p. 20)

Such examples illuminate the manner in which researchers' valuation processes, as well as their care practices with regard to authorship decisions, are situated within a broader context. The care practices of researchers may be complicated by the 'normal' processes that constitute their reality. For example, research is not entirely foreseeable, which illustrates how care practices are entangled with parameters that shape researchers' situated normalities. Consequently, for instance, the revision process may transform a seemingly clear ranking into one that is rather uncertain, as illustrated by the following account of a life scientist:

[A] PhD-student and a postdoc shared authorship on a paper, shared first authorship and the first author was the PhD student because that's how at the time of submitting the paper, it appeared clear that the student — although they both contributed equally, as it says — it was a project of the PhD-student. However, you know, your manuscript how it ends up published rarely looks like how it looks at the end. And after going through a nine-month revision, it seemed so the postdoc had actually done more, right? Especially intellectually, thinking about, coming up with experiments or address the reviewer comments and what you do then — do you change the order of the authors? (II, PI16f, p. 15)

The senior researcher highlights the considerable 'work' undertaken by the postdoc, which may have involved a greater investment of time, while also emphasising the proficiency required to successfully revise the manuscript. In this way, she initiates processes of valuation, which are examined in depth in Chapter 6. Nevertheless, the researcher's hesitancy in making a confident decision regarding the ranking is evident. Furthermore, her struggles appear to

originate from the potential consequences of modifying the order of authors, because, in her view, "it would just bring like chaos" (II, PI16f, p. 16) within her research group, as she may be perceived by her group members as less confident in her valuations concerning authorship (ranking), exemplifying how the interconnectedness of valuations can matter for researchers.

As the three sections of this subchapter show, the opacity surrounding authorship can have negative consequences not only for researchers at the beginning of their socialisation into academia, but also throughout their subsequent years in science. The subsequent subchapter continues to follow the career paths of researchers but focuses on how researchers navigate the tension between epistemic values and the neoliberal logic surrounding them.

8.4. Careers and (self-)conceptualisations in the balance: Researchers torn between epistemic values and the neoliberal logic

And he was like, yeah, I mean, the eLife-paper has just come out, but after the paper is before the paper, that's the first thing. And then he said that publishing papers is, of course it's nice and you need, you need it, but it's not giving him any kind of really long-term satisfaction. So, what he told me is, what he, where he draws much more motivation out of, is just going after his curiosity and get the knowledge and teaching other people how to do that. So, he was really very ambitious in teaching and that will, always sticks in my mind that just by nature, by a publication you're not being, it's not going to make you a happy person or anything. (IWS8, PhD19f, p. 27-28)

This quote succinctly exemplifies how researchers are commended when they understand themselves as individuals driven by curiosity, yearning to make scientific discoveries, and act accordingly, and then instil this idea in their scientific offspring. Consequently, values ingrained in the epistemic realm, such as curiosity or the advancement of science, are upheld. Conversely, the imperative within the paradigm of symbolic rationality to augment one's track record with quantifiable units, in the form of publications, is presented with a certain nonchalance. Nevertheless, a sensitivity to this tension between the epistemic realm and symbolic rationality reveals itself in how the latter may supersede and manifest in researchers' constructions of themselves or others in their narrations because it can exert tremendous power. However, it would be erroneous to assume that the symbolic logic is perpetually at the forefront of every researcher's mind and shapes all their actions. This subchapter

commences by exploring researchers' navigation of this tension and their personal anticipations for and experiences of a career in science, starting with PhD students. The subchapter then proceeds to discuss how researchers recognise, yet simultaneously mitigate, the impact of symbolic rationality on their professional lives. It concludes with insights into how subordinates reflect upon and potentially criticise group leaders, as well as how group leaders themselves contemplate their own role.

8.4.1. Ideals and expectations concerning researchers and careers in science

The recurring juxtaposition between epistemic values and the symbolic logic is already evident in the description of how doctoral students are imagined. Indeed, when researchers in leading positions discuss the type of individuals they seek to recruit as PhD students, they tend to emphasise the pivotal role of intrinsic, epistemically driven motivation, in contrast to a discernible strategic career planning approach shaped by the symbolic logic. Expressions such as "storm and stress" (translated by the author⁹⁷; VU, PI2m, p. 20) are used to describe the desired motivations of aspiring researchers. Or a group leader talks about how "ideally, we want the people with the greatest creativity and the greatest eagerness to pursue their careers in research. If possible, we actually want to bring them forward and create opportunities for them" (translated by the author⁹⁸; II, PI7m, p. 8).

Issues surrounding motivation and work-life balance frequently elicit emotionally charged responses, thereby offering insight into how the ideal researcher is envisioned:

I think all this employee thinking is completely inappropriate in science. We are all self-employed here. We develop our careers and drive our research forward. [...] These are simply people who somehow have their permanent jobs, go home at four o'clock and think: that's how it should be; yes, that's also nice and it's definitely more family-friendly - sometimes I wish that too — but it's just not like that in science, that's not the job. That's why I say we're talking about hobby — it's not a job, it's not a job where you're like salesman or something where you say okay, I, yeah? — we really either do it voluntarily, or you're not supposed to do it. (translated by the author⁹⁹; II, PI7m, p. 25)

However, a senior researcher points out that there is not a single, universally applicable force driving individuals to aspire to a career in science:

[E]veryone has a different motivation. One wants fame, the other wants to become rich, and the third wants to somehow save humanity [...] and then there are 200,000 other

reasons why you approach this career, why you got into it. (translated by the author¹⁰⁰; II, PI7m, p. 5)

That variety of motivations may then shape how researchers value how authorship is negotiated and decided. Nevertheless, several senior researchers bemoan or criticise junior researchers whose behaviour seems to be strongly influenced by the symbolic logic:

Well, there are people, and that has become worse over the years, in this generation especially in part, that people somehow have such a, how shall I say, straightforward view of their future and career and somehow know relatively quickly exactly what they have to do, must do, not, so as many papers as possible, so and where it's somehow about that, so how shall I say, where you have the feeling, sometimes it's more about the papers than about the science. (translated by the author¹⁰¹; VU, PI3f, p. 10-11)

Or another senior researcher talks about how the scientific offspring thinks "tactically [...]" What people do today is they look at how they can have the most success with the least effort. That's not a strategy, it's a tactic" (translated by the author¹⁰²; VU, PI5m, p. 2). However, junior researchers may perceive the situation differently and imagine the responsibility for their behaviour lies outside of themselves:

I could imagine myself at midnight in the lab every day, like, you know if I was motivated still. So, but, like, yeah, I mean, and I can't pinpoint that really to my PI — he's nice, everything is okay, like, but kind of somewhere this, and I kind of feel most of my peers feel the same. Like, it's killing your internal motivation, like the, I don't know, the system. (BGSP 7_1.4, p, 11)

Alternatively, a PhD student may justify their conduct by asserting that they "can't give everything up for free, like because I'm also very dependent on my career in science. So, like I'm, I'm not the gentleman, like, doing it for the fun of it building his own lab" (BGSP 7_1.1, p. 19). Also, senior researchers may invoke the past but rather than delving centuries into the past, they may reflect on the inception of their careers decades ago: "[A]ll of my career moves were based on, on the research projects and (.) and topics. And when I met the group, I felt that: yeah, this is, this is a group I can fit with" (II, PI16f, p. 10), thus valuing epistemic and social aspects more than the department's or university's reputation within the symbolic logic. Still, many senior researchers are well aware of the conditions of the current scientific system. The same researcher lamenting in the above quote also heavily criticizes the system's focus on output in the form of publications: "But this madness, not, well really madness somehow, how many papers do you have a year and what do you do and, ah, that's bad for the young

and that's idiotic in general, I think" (translated by the author¹⁰³; VU, PI3f, p. 26). A junior researcher vividly describes the consequences observed in their colleagues:

And I know two people that are really, really, really under pressure and lost the scientific drive, they don't want to work anymore in science, they just want to get rid of the paper and send it anywhere. They don't care about impact factor, they just want to finish the revision and just, just publish. So, I think it's sad. (BGSP 2_1.3, p. 2)

The previous group leader also mentions how they care for their group members and therefore, for example, "try to be relatively generous [in terms of authorship], because it's also time, what it cost them and then ultimately helps them, of course, and I don't care if I'm there alone (laughs) or not" (translated by the author¹⁰⁴; VU, PI3f, p. 13). Thus, senior researchers can be very aware that junior researchers 'need' publications and often mention how they attempt to support them. A researcher supervising PhD students assumes that they "feel a lot of pressure" (II, PI16f, p. 19). She does attempt to mitigate that pressure by telling them, "you will get a good postdoc, even if you don't end up with a Cell, Science, or Nature paper. So, you, it's not at this stage in your career that you have to, have that — so enjoy it" (II, PI16f, p. 19). Such conversations constitute a practice of care, whereby the objective is to ensure that the scientific offspring is not subsumed by the symbolic rationality. However, junior researchers may articulate the perception that "there's a lot of pressure on you to get your PhD done, to get your papers, whatever — and I don't think we, or at least I don't work well under pressure" (BSP 1_1.1, p. 42). One might conjecture that the postscript, with its reference to the deleterious impact on their work, is offered in the hope that the critique of the pressure is perceived as more legitimate. The subsequent deeply vulnerable self-description of a doctoral student powerfully exemplifies how PhDs may rely on care practices to buffer them from a potentially overwhelming symbolic rationality: "I remember myself three years ago and I remember and see myself now, and I see why I was excited three years ago and why I would say I'm currently near rock bottom" (BGSP 7_1.1, p. 11). However, others state that they were well aware of the current conditions in academia and therefore "wouldn't say [of themselves] that I went into my PhD with this kind of ideal view of how academia worked" (BGSP 7_1.1, p. 13). Another junior researcher concludes how "in the real world you, you need authorships and co-authorships and many publications which is underlying problem for many things" (BGSP 1_1.2, p. 4).

Yet, senior researchers tend to view any attempt by a junior researcher to emulate the symbolic logic negatively. Researchers who are also supervisors of PhD students may, therefore, aim to instil in their students an understanding of the primacy of the epistemic realm over that of symbolic rationality:

I'm not trying to make them publish at all price, I'm just trying to help them obtain results. Once they have a result then of course I would encourage them to write it up or I would write it with them, if I did contribute to it. So, it is not that publishing is the aim. The aim is knowledge. (VU, PI4m, p. 11)

It may be perceived by researchers acting as PhD supervisors as their responsibility to convey to their students the values on which they should base their practices. In other words, a supervisor may want to teach the scientific offspring in their group what to care about and, thus, what to value highly.

However, it is not only junior researchers who are significantly affected by the symbolic logic, as a senior researcher points out:

[T]he range of people who define themselves absolutely only in terms of what they have just published and how much money they have made and nothing else at all, is also there, so I mean, you have such people and such people. (translated by the author¹⁰⁵; VU, PI3f, p. 27)

However, the self-presentations exhibited during the analysed interviews and group discussions appear to be shaped by a shared understanding that researchers whose self-conceptualisations are primarily or significantly influenced by indicators and metrics are generally regarded with scepticism and disapproval within the scientific community. As is also manifest in the opacity surrounding authorship practices, the power of the symbolic rationality exerted is itself taboo. A PhD student draws a comparison between the dynamics they perceive and the television series *Game of Thrones*, in which political rivalries, intrigues and struggles for power play a significant role (Owen, August 29, 2017):

I think like the 'Game of Thrones' part of it has to be taken into account every step. Like, we all know it's happening, yet we pretend like it doesn't um, and, and yeah. To me that doesn't make any sense. Everybody's nodding. Yeah, because we all know it's happening. But you can't say! That's why we need open debates. (BGSP, 7_1.4, p. 7)

Nevertheless, researchers rather tend to emphasise how they "really want to contribute

something, right, so to improve something and so they're big problems which I want to solve" (VU, PI4m, p. 6). In a group leader's opinion, "[o]f course, there is also self-interest in some form, that you hope: well, maybe I'll get rich when I have a patent and I sell something; but primarily it's actually unselfishness with the hardcore scientists" (translated by the author¹⁰⁶; II, PI20m, p. 24). Such perceptions offer insights into the assumed and desired value orders of researchers. Nevertheless, although a considerable proportion of researchers may be unselfish regarding financial remuneration, such unselfishness does not necessarily equate to a similar approach to matters concerning the perceived justified reward in the form of authorship (positions).

In particular, senior researchers frequently express a desire to make a significant contribution to the advancement of their field of research. Naturally, their relationship to their field is distinguishable and more intimate than that of a doctoral student who has not yet established a reputation and position within an academic field. In contrast, a renowned researcher on the brink of retirement may assert, "I don't need to be on one more paper which isn't that important" (VU, PI4m, p. 8), interpreting 'important' as meaning that the paper does not really or significantly contribute to the field of research. An associate professor ponders why established researchers with full professorships continue to produce a steady stream of project applications rather than engaging in 'actual' research. She speaks with admiration of a senior colleague who dedicates a significant portion of his time to writing code rather than seeking funding:

But he wouldn't go and say, "Oh, I need funding, I need that." No. He is not going to do that. I want to do science if I... but again, it's already at his stage of career, at his weight, like professional weight, he can afford that. I still cannot afford that. What surprises me always is that people that can afford that, don't do that! And still keep being super ambitious. Well, but this is done, this is a personal choice. In a sense at my level, it's also already a personal choice. (VU, PI7f, p. 11)

It is imperative to consider the perspective of the researcher when analysing how they conceptualise other researchers in their narrations. Have they themselves traversed the career stage(s) and how much time has elapsed, conceivably producing rather blurred and idealised imaginaries, or are their conceptualisations based on their observations and imagination? To illustrate this point, researchers who have already completed their PhD many years or even

decades ago may recall the period as a special, undisturbed time, free from the constraints of the symbolic logic — possibly a description tainted by a certain nostalgia:

[A] golden time that no longer exists in such a carefree form. After that comes the postdoctoral phase, where you no longer have to write a doctoral thesis, but then you have to think about, well, what's going to happen afterwards, not, meanwhile, I mean, the clock is ticking, do I stay at the university, do I leave? (translated by the author¹⁰⁷; VU, PI2m, p. 20)

Still, for individuals who are either in the midst of or approaching the conclusion of their doctoral studies, the situation may be perceived quite differently. A junior physicist notes a change in their outlook on the future, potentially influenced by considerations of how one's track record may be perceived by others. In their view, this shift in perspective "gets relevant to the end of the PhD (laughing), when you have to like look back and be like: oh well, now I have to apply" (BGSP 5_1.2, p. 29). Another group participant responds with how they are "still in this phase where I pretend that I can keep many doors open" (BGSP 5_1.2, p. 30), exemplifying how even within the same formal career stages, there may be discrepancies in the perception of symbolic rationality.

The career stage that is perceived as particularly challenging, not only by those currently in those positions but also by those who have previously worked in those roles, is the postdoctoral stage. Reflecting on her awareness of the significance of regularly being an author on scientific publications, a group leader from the life sciences field concludes:

Awareness it's definitely there, not, that's just, the importance is different when I'm now, when I'm in postdoc years and I don't even have my first real job yet, so to speak, then I think, OK, either it's not going to be something or it's going to be something, but you know, I mean, you definitely have to produce in some way. (translated by the author¹⁰⁸; VU, PI3f, p. 26)

The precarious position in which one finds oneself, reliant on the assessments of one's track record by other researchers, serves to amplify the pressure experienced. A further group leader from the life sciences appears to concur with the view expressed by others that postdocs are in a rather unfavourable position:

And the people who can really complain about their work-life balance, and should, are the young postdocs and those who are just making the step to a professorship, they are

really poor, they are really under pressure in every way. But the doctoral students certainly aren't. So, from my point of view, no. (translated by the author¹⁰⁹; VU, PI5m, p. 3)

This senior researcher underscores the gravity of the situation by juxtaposing the experiences of postdocs with those of doctoral students. This comparison serves to accentuate the considerable pressure postdoctoral researchers are subjected to, which ultimately manifests in a detrimental work-life balance.

The interconnectedness of researchers resulting from the interconnected authorship valuations compels researchers to reflect on the potential necessity to adapt their behaviour in the context of authorship negotiations. A postdoc in the life sciences perceives herself to be relatively unaffected by the symbolic dimension, which "on the whole, it works for me, even on this relatively polite path that I'm on. But sometimes I notice that I also have to be careful" (translated by the author¹¹⁰; VU, PDoc7f, p. 7). Although she is cognizant of the pressure researchers may face in terms of publishing, she remains perplexed by the tendency of some to assert authorship positions from the outset, despite having yet to make any contributions. In contrast, PhD students may lack confidence in being able to eschew the constraints of symbolic rationality. This may engender the perception that there "is this one optimum way that you have to be in order to be successful in this system, and if you're not like that it's very likely that you will dropout at some point" (BGSP 6_1.2, p. 43) or, as another PhD formulates it, there is "no room for normal workers in science" (BGSP 6_1.2, p. 43). This resonates with the findings by Felt (2009a), who notes that "the system expels many — and we would argue, also very gifted ones — from it, for reasons of their non-conformity to norms" (p. 232).

In essence, these accounts revolve around what researchers identify as advantageous conditions, sources of gratification, and thus motivation, and structural aspects found to be exhausting and discouraging. Being part of a scientific publication as an author can be motivating, and "the sweetness of a publication appears to erase, or at least compensate for these insecurities because of the *"exhilaration"* (Senior Lecturer) this brings, although like a drug, the relief is usually temporary" (p. 344), argue Knights and Clarke (2014). Furthermore, assessment situations can also assume a pivotal role. A researcher who perceives their work as lacking sufficient recognition may consequently attribute less value to their work and

experience a diminished sense of belonging and confidence based on their experiences of being overlooked in authorship decisions, review processes, or hiring committees. Conversely, a researcher who has successfully advanced in their career and feels acknowledged for their contributions to science may feel confirmed in their identity as a successful researcher and exhibit another level of confidence and sense of belonging. Therefore, structural aspects can influence researchers' commitment to their role and the nature of the research they conduct, as a dependence or independence on the symbolic rationality can inform the direction of their research. This is illustrated by the following statement:

And I still have a bit of an idealistic core, actually I would like to start something quite risky again [...] but as I said, that's the dilemma with the career again, with: if you know, you now have three years or four years of contract, I have to publish, I can't afford now to be unproductive somehow for ten years - in the sense of publications. In other words, that's just the luxury of being rich when you have a permanent job somewhere and a lot of money. (translated by the author¹¹¹; II, PI7m, p. 36)

The rhetorical confrontation of idealism indicated by the notion of the 'idealistic core', signifying the epistemic realm, with the 'dilemma with the career', symbolising the symbolic rationality, exemplifies once again the recurring juxtaposition. The 'luxury of being rich', as interpreted through the lens of symbolic rationality, entails relinquishing the constraints of its subjection and traversing the epistemic realm with autonomy. A parallel perspective is espoused by another life scientist, who contends that "we should stop complaining and just try to gain to that freedom stage. Because when you are really successful, then you really have total freedom" (II, PI12m, p. 14). This quote exemplifies how "researchers appear both as actively deciding subjects and as subjected to and affected by the conditions surrounding them" (Sigl, 2019, p. 124), whereby their standing and formal position plays a decisive role.

8.4.2. Acknowledging and negating the power wielded by the symbolic rationality

In the context of everyday work, the potential pressure exerted by the symbolic logic through easily accessible indicators for comparison with others may be less perceptible for some researchers than in situations where writing grant applications or applications for positions is concerned. Moreover, whether a researcher's career or the continued existence of their group is contingent on the outcome of either a project or job application, it is likely to have an effect.

One group leader states that the pressure experienced when writing applications “doesn’t happen now. I’m quite satisfied with what we are doing” (VU, PI1m, p. 3), implying that this was not always the case in the past. Researchers tend to be aware of the metrics associated with their work. For example, a senior researcher in the field of physics answers that “yes, of course. Unfortunately, these are the things that one has to keep track of” (VU, PI4m, p. 12) when asked if he knows his most-cited papers. The addition that, for him, it is “a question of whether your work is noted” (VU, PI4m, p. 12) indicates that such numbers are not only of interest within the symbolic rationality but can also be of significance within the epistemic realm (for citation as a form of recognition see also Hessels et al., 2019). One might suggest that for numerous researchers, the impact factor, i.e. an “algorithm constitutes a legitimate stand-in” (Torenholt & Langstrup, 2021, p. 11) of themselves and their peers. A comparable stance is held by a researcher from the life sciences:

[W]hen you have published your results and [...] that are picked up and cited and incorporated by others — that’s a nice confirmation that the research has not been completely pointless. Nevertheless, for me this is not a main criterion with which we should put in the foreground. (translated by the author¹¹²; II, PI13f, p. 4)

Self-conceptualisations do not necessarily have to be grounded (only) in the symbolic logic but may also be shaped by the epistemic realm, since a ‘successful’ researcher most likely corresponds to the values of both. Thus, a senior researcher can confidently — and somewhat jokingly — say of himself that “I have achieved everything in life, and I am anyway, I am swimming on a wave, and I can do it until I retire, even if I sit down now and do nothing” (translated by the author¹¹³; VU, PI5m, p. 4). In contrast, a recently appointed associate professor offers another description:

[T]he pressure is very high. That pressure is there and it’s like ... University needs to go up in the ranking. They hire you, they pay you for bringing them up in the ranking. In one or another way you need to be useful. (VU, PI7f, p. 13)

A junior researcher also draws a distinction between who, in their view, “need[s] the work done so that you can have more papers, that you get more grants and stuff like that” (BGSP 2_1.1, p. 17). This is primarily those “at the beginning of the power (laughing)” (BGSP 2_1.1, p. 17), as the researcher humorously notes.

For a group leader, the pressure to perform is almost a default state for a researcher: “Well, that’s always there, right? I mean that’s part of the deal, so this you know, you know it before you started this career, so (laughing) [...] You don’t, you can’t complain about that (laughing)” (II, PI10m, p. 28). Whether everyone about to start a career in science is aware of this pressure and can, moreover, truly gauge how they will feel under pressure is debatable. Moreover, such perspectives shift the responsibility to the individual, rather than pointing to structural elements and demanding that they be adapted. Or, in the words of Loveday (2015) “[T]he onus is on the individual to aspire to secure their own success while structural constraints remain down-played” (p. 577). A life scientist succinctly describes how torn he feels to work within a system which values are at least partially in tension with his:

[S]o the system at the moment asks me to get the low hanging fruits as much as possible and then get my next job, etc., etc. But I’m not going to, I will keep saying that this is wrong. I could say: well, this is the system — I’m going to play the game. I will still continue doing what I’m expected in a way to do and also more, but I will also tell people that this is wrong. (II, PI12, p. 8)

However, there are also voices who portray the pressure to publish in a more favourable light, although they represent a minority viewpoint. A senior researcher talks about how being assessed also has positive aspects, “because then I also simply feel the pressure somewhere, that I really should do something — I believe that this rather promotes that” (translated by the author¹¹⁴; II, PI15m, p. 14).

Non-adherence to the rules of the system may have a significantly adverse effect on one’s career trajectory. A researcher speaks almost regretfully about how “it’s not on my mind, the publication record. It has never been. It should have been a bit. The reason... I’m a 19-year-old postdoc” (VU, PDoc6m, p. 4). Negating or deferring the symbolic logic may impede one’s scientific career. Reasons for that may be private circumstances such as care obligations, which overshadow the symbolic rationality. Moreover, it can potentially hinder one’s sense of self-confidence and belonging, considering the observable sentiment of satisfaction among researchers “when one seems to fit into a wider academic imaginary of performance, having a feeling of being in line with dominant expectations (i.e. synchronous with others), and, thus, being able to produce a coherent narrative of performance and progress” (Felt, 2021, p. 273).

There are also examples of researchers describing themselves as being fortunate because they “never had to worry about whether it’s enough” (VU, PI4m, p. 7) in situations where they applied, for example, for a tenure-track position. In the words of a group leader, he “was writing papers and the career was following” (VU, PI4m, p. 7). However, as the same researcher notes, at the beginning of his scientific career “there was less publication pressure and things like that, and it was easier to get a tenured position” (VU, PI4m, p. 7). It is thus imperative to consider the temporal aspect of such narratives, as this researcher himself asserts. Moreover, as Davies and Pham (2023) remark, “[r]eferences to luck function to retroactively make sense of trajectories that were framed as incremental and uncertain, whilst presenting oneself as pleasingly modest” (p. 293). The question of whether it is merely fortune or rather the existence of underlying structural privileges, such as gender, class, or place of birth, facilitating a career in science, represents a topic worthy of further investigation in future work.

8.4.3. Experiencing and valuing the role of the group leader

As has been evident throughout this subchapter, the career stage of researchers is shown to have a significant impact on their perceived leeway to uphold the typically desired epistemic values whilst also accommodating the demands of symbolic rationality. A greater scope of agency is frequently ascribed to the role of the group leader. Arising from this imagined stage of relative freedom and agency are two questions, which are addressed in this section: firstly, how do those in this stage, such as a group leader, experience it, and secondly, how is this position of the group leader appraised or criticised by individuals in subordinate roles?

One recurrent theme in the critiques articulated by early career researchers is lamentation regarding the paucity of leadership training for those assuming the role of a group leader. Becoming a group leader is rarely tied to how researchers interact with and impact the work lives of other researchers. In light of this, a PhD student posits that, while supervisors “really want the best, they sometimes just don’t realise how much power they have and how influential they are” (BGSP 7_1.2, p. 28). The predominant perspective is that group leaders exhibit a high degree of success in assessments, wherein metrics bear a decisive influence on the outcomes while these metrics are presumed to reflect the assigned value within the epistemic realm. However, many researchers stress in their narrations that, for them, a ‘good’

group leader is not merely interested in their own and the group's scientific output — irrespective of whether the advancement of science and/or the accumulation of symbolic capital is in the foreground. Whether group leaders' care for publications prevents them from ensuring that group members' contributions are recognised and acknowledged (with authorship) is up for debate because it can be difficult to judge from the outside which value primarily guides a group leader's care practices. Supporting their scientific offspring in ensuring positions in other research groups might be motivated by increasing one's own symbolic capital with authors' lists spanning around the globe. This motivation might then override any concerns about whether such a support of the junior researchers is justified in terms of the epistemic value to be bestowed upon their contributions, which neglects the epistemic dimension. Group members also judge the leaders of these groups based on how they "engage with students and people" (IWS5, PDoc9f, p. 40), thus underlining the significance of care for others in academia. The awareness of one's own capacity to influence and exert power can be conceptualised as a responsibility inextricably linked to that role, as one group leader reflects:

[T]he boundary between those two of this private sphere and sphere at work and all that becomes very, very blurred currently. And that's when perhaps the responsibility comes even more, because you have to be perhaps very careful as supervisor actually, not to put any pressure because I mean it's true that you can invade people privacy very, very quickly, and some people are very permeable to that. Others are completely resilient, which is, yeah, that's fine. But I think, I think it's, it's really, really complex. (IW6, Pl6m, p. 35)

The influence of a group leader on whether PhD students envisage pursuing a career in science can be considerable. Furthermore, it demonstrates how ensuring authorship or a particular valuable authorship position for another person is not the only manifestation of care; authorship is therefore not the sole parameter by which to trace practices of care. Yet, a caring superior is thought to be an uncommon occurrence for many researchers, as evidenced by the remarks of a junior researcher who cites it as "one of the reasons that makes me sceptical about staying in academia, knowing that I probably won't find a similar supervisor anymore" (BGSP 5_1.1, p. 9). Nevertheless, from an analytical perspective, these expressed criticisms warrant careful consideration. Many of them are undoubtedly valid and reflect the experiences of early career researchers. However, it is essential to recognise that junior researchers may also exhibit similar patterns of reasoning as to a PhD student who self-critically states, "I'm

projecting a lot of my problems with the system, whatever that is, on my PI" (BGSP 7_1.4, p. 11).

Another pervasive narrative strand revolves around the frequent absence of group leaders from the lab, whilst simultaneously holding the authority to make the final decisions on an authors' list. This may give rise to early career researchers questioning whether this negatively affects the fairness of a group leader's authorship practices. From their perspective, the researcher most involved in a research project, and thus with the more refined overview of the process preceding a publication, is best placed to make the fairest decision, at least from an epistemic standpoint. Thus, such a critique of the value of the group leader's authorship decisions in terms of fairness can go hand in hand with a critique of the group leader's lack of care in making their authorship decisions as 'good' as possible, since group leaders often shape the decisions to a large extent. Moreover, following this line of argument, early career researchers may wonder whether this absence is also an indicator of the limited extent of supervision and support provided by a group leader. For instance, a junior researcher may express frustration at the perception that "most of the time it's like the supervisor's always gone and always doing something and the postdocs are kind of in charge. [...] A supervisor should really, I don't know, spend time with the group" (BGSP 1_1.3, p. 9). A PhD student explicitly articulates their concern that the supervisor's absence may precipitate an unfair authorship ranking:

I don't know if my supervisor notices this, because you know, in the weekly meetings, we just discuss what we did and he's not there, you know, on a daily basis to see, so it might be that at the end he comes and says: okay, you know, the postdoc should be the first; so this is my dilemma. (BGSP 6_1.2, p. 4)

In light of the assertion that "[v]isibility is closely associated to recognition" (Brighenti, 2007, p. 329), an enhanced comprehension of their worries may be achieved. However, such an absence from the laboratory can be experienced in different ways and thus be valued differently. Early career researchers may also perceive not being perpetually under the scrutiny of their group leader as a liberating experience. Indeed, there are instances where the "disempowering effects of visibility" (Brighenti, 2007, p. 336) are in the foreground, manifesting as the impression of or actual experience of surveillance and control. However, one group leader is confident that she is "in touch with what's going on" (VU, PI6f, p. 5),

because she is also still involved in field work and accompanies her doctoral students on field visits for some time. Speaking of being in touch, because she is still doing fieldwork, she underlines how she still sees the engagement with this layer of the material dimension of her research as crucial. Another group leader has regular meetings with his group members, during which they report on their progress, to counterbalance that although he does “go in the lab [...] of course I don’t see most of the things that happen” (VU, PI1m, p. 8). In a discussion group, a PI talks about how he “always feel[s] rather guilty not to be in the lab enough” (IWS6, PI6m, p. 23). Meanwhile, another group leader perceives themselves not to be the sole individual in a supervisory role, as he articulates passionately in the context of a discussion group:

I think it’s also something, I mean you are all also supervisors, maybe you don’t realise that, but in fact, anything that’s happening in the lab and in the daily life, in your daily lab life, is not supervised by me at all. I cannot do that. It’s in fact supervised by the other students, that have more experience in this specific way, or postdocs or whatever. In a way that is also casual, so you’re supervised in a team, you all are a team, even if you don’t work on the same issue, I guess. And I think this is a very, if you can create this type of atmosphere, then I think this is like a shared responsibility, where you also can, as you said, you can say: no, I don’t believe it, or there was something wrong, or I did something not in the right way, and I think this creates the type of, I would say group responsibility, that we really have, a group supervision, that’s because, that’s what it is. It’s not, it’s not, it cannot be my responsibility alone, certainly I have to think about it and create a type of atmosphere, but the rest is, it’s we all, no? We have to work together. (IWS4, PI4m, p. 22-23)

Through this powerful statement, meaningful insights are offered into an individual’s reflections on their responsibility as a group leader and the simultaneous assumption and attribution of responsibility. Nevertheless, it can be deduced from the analysis that such explicit articulation is rare in day-to-day operations.

Furthermore, spatial distance, or rather entering another space, such as the group leader’s office, prevents a certain informality. Or, as one postdoc describes, having to explicitly approach the supervisor of a group of fifty researchers when one needs support, as the group leader is usually not in the immediate vicinity, is perceived as inhibiting. The result of this situation is that it is the responsibility of the individual researcher to actively request support from the group leader. Conversely, a junior researcher in the field of physics is keen to emphasise that their group leader “also goes to the lab, helps us sometimes, which is also

not really normal, I think, that the professor comes to the lab and screws things together, because he has fun" (BGSP 5_1.2, p. 19). Guzmán-Valenzuela and Barnett (2013) also describe such instances, in which more 'selfish' motivations may be in operation, given that researchers may experience research as being so fulfilling that they might "steal time from other activities in order to focus on it" (p. 1127).

Exercising one's responsibility as a PhD supervisor or the enactment of care, even without any formal obligations, is often translated into last authorship. Therefore, an invitation from a senior researcher to join a scientific cruise without the condition of being listed as a co-author on future publications is considered quite remarkable, as it seems to deviate from the situated normality of many PhD students. These invitations can be interpreted as a means of supporting and promoting scientific offspring, without wanting or possibly needing to benefit from such support and promotion in future authorship decisions. Moreover, since these invitations do not stem from any formal responsibility, given that the host is not professionally connected to the invited researcher, they can be conceptualised as 'pure' practices of care. Nevertheless, to gain a more comprehensive understanding, it is essential to provide space for the narratives of those leading the research groups. Otherwise, the significance of group leaders and their contributions to a paper may be overlooked, which is emphasised by a group leader from the life sciences:

I hope that we live in an academic system that really appreciates what the supervisors do. We've all been, I would say, had a lot of pressure to make sure that the PhDs feel better because they're collapsing under the pressure; fine. But let's not forget what the supervisors are doing as well. So, it can't all be about the PhD story of suffering. Yes, it exists but there is more. (VU, Pl6f, p. 7)

The interconnectedness of the authors of a given publication and their respective valuations is once again demonstrated, illustrating the intricate web of entanglements that researchers from all career stages are confronted with. Furthermore, statements such as these highlight that the process of valuation is not a one-sided endeavour, with the valuing researcher not being solely in a formally superior position. Taking the observations outlined in this subchapter into consideration serves to refine the understanding of researchers' justifications of what they care for in authorship decisions.

8.5. What to care for in authorship decisions?

As also supported by the findings of the preceding subchapter, a plethora of researchers concur that metrics and indices exert a disruptive influence on the scientific system, inhibiting care for decisions rooted in the epistemic realm with valuations of contributions guided, for instance, by the value knowledge gain. The implications of this disruption also extend to the desired care for others, meaning valuations striving to recognise the work of others may be overridden by the symbolic rationality. Consequently, interlocutors' narratives thus frequently follow what Torenholt and Langstrup (2021) coin as a "*logic of disruption*", inspired by Mol's (2008) thinking and work. Interestingly, these concerns are also articulated by junior researchers who have not encountered any disruption in the implementation of metrics and indicators, given that this development commenced several decades ago. Nevertheless, a number of these researchers perceive the quantification of academia, with its focus on the number of publications and citations, as a rupture with the scientific practices of the past, which they have not experienced. This resonates with the observations by Ylijoki, leading her to the following conclusion:

[N]ostalgia does not describe the actual past but rather the problems and tensions of the present. It is an idealization of the past, the function of which is to clarify the values and morals of the organization, which in turn can help to come to terms with the conflicting pressures of the current situation. (Ylijoki, 2005, p. 555)

However, within the context of group discussions, instances emerge wherein competitive practices, aligned with a neoliberal logic, are presented as a continuation of the conventional practices that have historically defined the conduct of scientific inquiry. The participants adopting this stance advocate the view that such conduct has been instrumental in propelling scientific advancement and has resulted in notable discoveries. Thus, both a logic of continuation and disruption can be identified in researchers' narratives, mirroring their heterogeneous positions and perceptions. As Torenholt and Langstrup (2021) observe, in the data "situated and sometimes co-existing ways of making sense in continuation [or in assumed discontinuation] with historically specific, local practices" (p. 14) can be encountered.

8.5.1. *Balancing relational considerations and fairness*

The balancing of epistemic values and symbolic rationality is not the only juxtaposition researchers perform in their narratives. They also contemplate the potential adverse consequences of decisions on authorship being strongly characterised by the social dimension. An early career researcher is highly critical of such authorship decisions, perceiving it as “the real problem” (BGSP 2_1.2, p. 16). They have “the feeling that very often this authorship is defined a lot based on what kind of relationship you have with the person, and this you see a lot. It’s not always about the scientific input, but a lot of what kind of relationship you have” (BGSP 2_1.2, p. 16). Another junior researcher arrives at a similar conclusion when discussing favouritism: “I think this is a very common stuff which happens, which effects a lot of work” (BGSP 1_1.1, p. 9). Those sharing that standpoint would prefer the epistemic dimension shaping all authorship decisions, thereby ordering the social after the epistemic dimension.

In contrast, another line of argumentation that can be found in narrations and discussions on authorship favours the social dimension as guiding authorship decisions. Researchers might claim that the feeling of being left out (i.e. not being listed on a paper as co-author in the middle) can foster “some kind of distrust within the groups” (IWS4, PDoc6m, p. 30), thus severely undermining the work climate. Hence, adding someone to a list of authors seems to be more favourable for some researchers than creating an atmosphere marked by distrust.

However, pursuing an inclusive authorship strategy can also be deemed to be unfair towards researchers who are not part of the respective research group: “[I]t’s not always fair, definitely. But (.) that’s the, that’s the other side of having everyone on the paper (laughing)” (BGSP 2_1.1, p. 15). A PI who implements such a strategy in their group explains how he does not want to exclude others and prefers to add them to a list of authors. He is well aware how it “may create two classes” (IWS4, PI4m, p. 30) because his PhD students will have rather long list of publications in comparison to PhD students from other groups when graduating. He further adds that “it’s not justice” (IWS4, PI4m, p. 30), pointing to the value of fairness, but also notes how “it’s just good for them to have a paper more” (IWS4, PI4m, p. 30). The latter point is indicative of the PI’s prioritisation of his PhD students’ success in assessments taking precedence over the principle of fairness. He further justifies the authorship practices

prevalent in his group by arguing that, from his standpoint, authorship disputes are “very rarely” (IWS4, PI4m, p. 30) about whether another researcher is added in the middle of the paper’s authors’ list or not. According to him, in the centre of authorship negotiations are usually into which authorship position a contribution should be translated. One could therefore perceive this second part of his explanation as another justification for his practice because co-authorship stemming from the less valuable authorship positions is rarely the cause for concern and debate among researchers. The absence of care for those authorship positions facilitates the ability of the group leader to care for their group members. In accordance with the argument advanced by Wallenburg et al. (2018), it could be regarded as constituting “a form of care” (p. 13) insofar as it “consists[s] in putting the metric monster to work” (p.13).

Additionally, it can be viewed as “more responsible” (IWS4, PDoc6m, p. 30) to rather include everyone who made a contribution. Another postdoc from the life sciences seems to share this standpoint as she explains how “I personally, and I think also my colleagues, follow in principle the philosophy, one writes rather too many people times also on the author list, as that one forgets someone and devalues thereby somehow” (translated by the author¹¹⁵; VU, PDoc5f, p. 4). A life scientist with his own research group bases his decisions regarding authorship on analogous considerations:

I try at least in the, within the group rather one too much than too little and if someone complains he should come and then justify. Yes, then you can see, if he has good reasons, you can change decisions. (translated by the author¹¹⁶; II, PI7m, p. 37)

The second sentence indicates the group leader’s awareness of his own fallibility, which, in his view, requires a certain degree of openness to reconsidering authorship decisions. Therefore, the researcher acknowledges the potential for subjectivity in his decisions, stating “whether one is always fair, I don’t know” (translated by the author¹¹⁷; II, PI7m, p. 37) — but, as stated above, he endeavours to achieve fairness in authorship decisions. Consequently, the pursuit of fairness in authorship practices should be regarded as an ongoing aspiration rather than a finite, static achievement. For a life scientist with supervisory responsibilities, continuous dialogue concerning authorship (order) is vital, as it facilitates clarity in authorship decision-making, alluding to how clear authorship decisions are almost a precondition for fair

ones:

And the PhDs, I actually talk to them a lot about it, because I want to prevent that from happening, it has to be clear from the beginning what the significance of what is. And from that point of view, they know that. They need first authorship, they also write it. Everyone who contributes is on it. And the people in the background, so to speak, who supervise and do the formalities and correct de facto and so on. Yes, they are actually aware of that relatively quickly, simply because all our work is structured that way. (translated by the author¹¹⁸; VU, PDoc7f, p. 8)

Such approaches to authorship could be framed as ones, in which caring for others is relatively highly ranked. Researchers justify such care practices by referring to the value of fairness, showing how care and fairness do not necessarily have to be in tension with each other. For example, a postdoc brings up the value of fairness by emphasising how crucial it is to include also individuals in the list of authors who may not have been involved in the writing process but who nevertheless conducted experiments. Hereby, the prioritisation of fairness is further legitimised by mobilising the epistemic dimension. Or, as a senior researcher acknowledges, in accordance with the prevailing guidelines and regulations, each author of a paper is expected to demonstrate the capacity to defend the entire paper on an intellectual level. However, this may not always be the case for papers where she is the last author. Accordingly, she does not consider this to be a salient criterion influencing her process of determining authorship. Nonetheless, it is of paramount importance to her that the respective authors “have really made” (translated by the author¹¹⁹; VU, PI3f, p. 10) a contribution, thereby outlining how ‘mere’ care for others does not guide her authorship practices but that it is rather contingent on her valuation of contributions, thereby ensuring fairness.

8.5.2. *Caring for oneself and caring for others*

Another recurring internal conflict mentioned by several researchers is how they feel torn between socially meaningful care work and the symbolic rationality, as they are often in a state of tension. A group leader provides a vivid illustration of the potential consequences arising from an individual’s inclination to care about others or to prioritise their own advancement in science:

I think there are different levels of appreciation. Maybe you don’t get appreciation from your boss, or from your institution, but maybe from your colleagues and from people that you, I think everybody needs to build up a personal network of people whom you trust,

and whom you help. And this cooperation — it is clear in evolution that co-operation is something that is rewarding and that pays back — so if you just do it for yourself, and you are really behaving very badly, you might be successful, but unhappy, and maybe being very cooperative, and, and you being okay with yourself, you might not be so successful, but you might be happy and you kind of, you have respect of yourself. And this is I think two things that, that balance, right? I think it does count if you look yourself in the mirror and you say I'm okay, my character, or if you cannot do it yourself in the mirror, because you just see something that you wouldn't, or you have to kind of look away. I mean this is personal and the institutional rewarding, and I think everybody has to balance the two of them. I think so, I don't know. (IWS5, PI5f, p. 37)

The group leader's prioritisation of caring for others over the accumulation of symbolic capital without consideration for others is evident in this statement. Another researcher concurs with this standpoint, and reinforces their argument by emphasising the vital importance of caring for others:

I think institutes require sort of a mix in people, and if you have everybody is, is you know, type triple A, then there's nobody to sort of fill in there, in the gaps, and help other people. Because they are always helping themselves, right? (IWS6, RA1, p. 30).

Moreover, a narrow focus on numbers may inadvertently overlook other factors that exert a significant influence on the indicators in question. A group leader in the field of physics justifies the variable number of papers PhD students are required to produce for graduation on the grounds of external factors. In his experience, there is a substantial difference between a student who starts from scratch on an experiment and another who begins an experiment that has already been running for some time. While for the one "there might be a paper after five years of hard work" (translated by the author¹²⁰; VU, PI2m, p. 15), if you "come to an experiment that has just been running at full steam for many years, you can get in quickly and reap, that's unfair, so that's how I need somehow to evaluate that appreciatively" (translated by the author¹²¹, VU, PI2m, p. 15). A certain degree of flexibility in the valuation of a PhD student's scientific output, expressed in the form of publications, can enable thus the circumvention of the predominance of symbolic rationality.

Yet, if researchers are under the impression that their peers do not care about them, which is evident in perceptions that, in order to "succeed you will have to exploit" (IWS5, PDoc8m, p. 41), they may subordinate their practices — at least in those particular environments or situations — to the symbolic logic as they may then fear that they otherwise do not succeed

in future assessments. Usually, such stories of exploitation are known from hearsay, although there are also many instances present in the data in which researchers report their own experiences. However, even if such kinds of narratives are not based on one's own experiences, it does shape early career researchers' situated normalities and imaginations of which values guide researchers, particularly those in powerful positions. Moreover, such kinds of stories often end with the respective researcher, enduring such power abuse, moving on to another position or exiting academia without making a formal complaint. Thus, institutional structure is rarely perceived as being capable of solving such issues. However, in the view of a PI, PhD students are also not (made) aware of the interdependence among those two groups, so to say, because a group leader's success also depends on their group members.

A PhD justifies what others might deem egotistical behaviour, subject to the symbolic rationality, as necessary because she is the only one who will care for her as an individual or for her career. The sentiment of that PhD is one of many examples of PhDs recounting how, even though a PhD supervisor is officially in charge and responsible for supporting them and their careers, they have had the experience of not being adequately cared for. The lack of perceived care can hinder the aspirations of those considering a career in science, as evidenced by this statement: "I'm reconsidering, like, if I want to stay or not. And I think one of the large factors was not having someone I could look up to, I could talk to, um, feel connected to" (BGSP 7_1.4, p. 3). There are, however, also instances in which researchers mention how they felt cared for by others. However, they may then highlight how they have "been very fortunate with my group and like I have my lab mates who have my back" (BGSP 7_1.1, p. 16). In doing so, they imply a belief about their situated normality which they perceive to be different from the situated normalities by many others. This is just one of a multitude of examples underscoring the indispensability of comparison in such inferences, as "presences in one site can trigger an attention to an object that does not exist in another" (Dewille et al., 2016b, p. 117)

In some cases, especially informal care practices among researchers at the same career stage, might even be acknowledged with co-authorship. A junior researcher describes their approach to authorship, noting that they "try to involve as many people as possible, so but they have to do something for that, it's not that they just get the authorship, but I want them

to do something, that they're also involved in that" (BGSP 2_1.1, p. 7). Yet, even when working in a "highly collaborative environment [...] at the end of the day the PhD students need to finish with a paper, so they do need to focus to some degree and it's a matter of reaching the right balance" (II, PI16f, p. 16), as an experienced senior researcher concludes. Achieving the right balance between supporting and collaborating with colleagues, while also focusing on the completion of one's own thesis, is a challenge faced by many PhD students, for whom time is also a limited resource due to their short-term contracts.

Researchers also talk about the kind of "support" (VU, PDoc1m, p. 12) that they have from the group leader. What practices they deem as supportive can become even clearer when they have worked as part of several different research groups. Having more work experience facilitates researchers to gauge which practices are, in their view, care practices. Through a comparison of how group leaders do (not) support their group members, researchers refine their situated normality and whether they deem a caring group leader as normal or as an exception. The assertion that they "luckily" (BGSP 2_2.1, p. 9) have not experienced power abuse evinces a particular understanding of what constitutes normality for many. Alternatively, the existence of care may be inferred from comments such as those made by a junior researcher who "never felt really exploited by any of my bosses or something" (BGSP 2_2.1, p. 7). A postdoc researcher talks about how, in her experience, it is "extremely rare" (VU, PDoc3f, p. 5) that a group leader grants a postdoc the last and possibly the corresponding authorship position. The exceptionality of such an authorship practice might be explained by the dominance of the symbolic rationality, often limiting care practices because they are frequently in tension with it. Additionally, the limited resources at the disposal of a group leader to provide care may be allocated to those who are deemed to be in the most need, as a postdoc contemplates:

I think postdocs are the last people they think about, because they [, the group leaders,] have so much to do. It is also appropriate that they worry more about doctoral students, who need more support, than about postdocs, and that as a postdoc you have to take the initiative when you need something. I haven't done much about it so far. (translated by the author¹²²; VU, PDoc4m, p. 14)

Similarly, a postdoc recounts how, during one of his first stays in a research group after completing his PhD, it became "clear for me then the PhD was their priority" (VU, PDoc1m,

p. 12) in authorship decisions. This priority is then translated into first authorships for the PhDs, making a postdoc's care for themselves seemingly a requirement.

Care for oneself is also evident in accounts of researchers eschewing collaborations with others or only participating in projects in which they are clearly in the lead. An explanation thereof may be found in researchers' experiences wherein they have found themselves wondering whether it is "good that this person is helping me, or it is not good actually at the end" (IWS2, PhD4f, p. 15) in terms of authorship when the helping person is perceived to be quite strongly influenced by the symbolic rationality. A postdoc similarly describes how he also came to the conclusion that he needs to be more selective concerning collaborations with other researchers. Unless he can be sure "that they will also appreciate my work, I would first be cautious about how much time I want to invest there" (translated by the author¹²³, VU, PDoc4m, p. 4). Time is a limited resource, and even though dedicating some time to support a colleague or a befriended researcher in their project aligns with the imagination of many researchers as to how social facets should exist within academia, if they get the impression that 'too much' time is spent on supporting someone else's research, researchers may question whether this time-intensive support is acknowledged with authorship. Those reflections are rather influenced by the symbolic logic as such care practices are not attributed with value within it, unless they turn into authorship. Talking about such challenging situations reveals the potentially conflicting pressures researchers can find themselves in. While they might desire an environment characterised by care practices for each other, they are well aware of the pressure that the symbolic logic exerts. A researcher describing such a situation attributes the responsibility to prevent this kind of inner conflict to the researcher(s) asking other researchers to contribute. Explicitly stating right from the beginning that the contribution will be recognised with authorship, aligns with the desired clarity. However, as a postdoc shares, in his situated normality, it is often the case that authorship is "not really clear" (translated by the author¹²⁴, PDoc4m, p. 3), which is something that applies to many other early career researchers. In addition, researchers may justify their scepticism towards supporting another person's research by referring to the epistemic realm and how being responsible for generating the majority of the findings is more fulfilling to them as researchers. Both the symbolic logic and the epistemic realm may exert an influence on the behaviour of researchers. Nevertheless, the emphasis in the narration is more likely put on the justification

located in the epistemic realm. It can be interpreted that this kind of justification is considered to be more legitimate than caring for the self and one's career. Independently, whether the care for the self or the aspiration for publications in which they are listed as first author, because it signifies how they are credited for their contribution to the body of knowledge, supersedes, it can still have adverse implications: The overall publication output is likely to be lower than for researchers contributing to various research projects and therefore being listed on more articles as co-authors.

In conclusion, being adequately cared for and/or having sufficient motivation to care for oneself, in terms of authorship, seemingly constitutes a prerequisite for successfully pursuing a career in science. Despite the evident disparities among the empirical cases, Law's (2010) conceptualisation of the "care of the self" (p. 64) within the field of veterinary practice captures how researchers' self-care practices can be conceived alongside their positioning towards symbolic rationality. Indeed, it is "about responding, but not responding too much. It is about being there, about sensitivity, and yet it is also about distance. It is precisely about self-protection" (Law, 2010, p. 64). Inevitably, if one wants to pursue a career in science, it is inevitable to take part in the game, i.e. 'responding' as Law (2010) calls it. However, 'responding too much', i.e. being overpowered by the symbolic rationality, is not advisable. Moreover, since, for many researchers, it is not a 'mere' job but rather a vocation, one's identity and self-confidence are often tied to the success of the own research. As a result, attempts to distance oneself from any failures, such as rejected proposals or papers, are often hindered. Self-protection (i.e. the act of caring for oneself) can even entail the decision to exit academia if the impression arises that one's commitment to succeed as a researcher is not warranted. The act of balancing that Law (2010) implicitly describes is visible in a wide range of instances described in this subchapter but also throughout the empirical part and certainly invokes profound reflections among researchers.

8.6. Reflections on how the multiplicity of attributing authorship matters

Well, I was actually shocked at how, when we first discussed a manuscript and authorship, how, how starkly different was the way people argued for their position or whatever. And that not everyone has the same attitude; and again, it plays a role whether one, I think culturally, it makes differences, what, what was my education, where do I come from, what

have I learned about this topic, and so on. (translated by the author¹²⁵; II, PI8m, p. 20)

A PI offers a compelling illustration of how the multiplicity of values can materialise, that has been evident through the three empirical chapters in the diverging valuation of contributions and the criteria mobilised, the invocation of different principles to legitimise the value attributed to an authorship position, and, ultimately, in the disparate judgements of whether an authorship decision is fair. Indeed, as Mol (2002) points out, “the good is inevitably multiple: there is more than one of it” (p. 177). And as the PI’s quote beautifully illustrates, researchers’ (first) encounters with the multiplicity of attributing authorship can evoke strong emotional responses. Furthermore, the explanations provided by the PI for the multiplicity point back to what has been presented regarding the tacit learning of authorship practices and also the recurring balancing acts researchers undergo throughout their careers.

8.6.1. Authorship: A personal and tricky thing

The following snippets demonstrate researchers’ awareness of the multiplicity of attributing authorship that is obscured by the “tame and civilised surface” (p. 94) of an authors’ list, a perspective that Knorr-Cetina (1981) has refuted over four decades ago, albeit from the vantage point that knowledge is manufactured. However, it could be argued that the issue is not the multiplicity but the lack of clarity surrounding authorship practices, which reflects the complex entanglement of various aspects involved. Yet, the multiplicity of values is likely to be of greater consequence for researchers in less well-established positions — both within their respective communities and in terms of job security.

So, how do researchers cope with the realisation that “there is not the one solution” (IWS7, PhD13f, p. 31), as is aptly encapsulated by one PhD student reflecting on authorship practices. Indeed, it is this very multiplicity that gives rise to the manifold struggles and uncertainties of researchers, as evinced by their narratives. Nonetheless — and it is imperative to emphasise this point — the multiplicity also opens up a space of possibilities to counteract some of the pressure exerted by the symbolic rationality. Additionally, as the widely held perception of authorship decisions as a “delicate topic” (VU, PDoc1m, p. 5) alludes to, the emotional ramifications of such decisions are not to be neglected, which further exacerbates the complexity. The struggles to reach a stable judgement, which is then translated into a

particular list of authors, is illustrated by the example of a PI who, while participating in a group discussion, reads a card discussing an authorship dilemma and considers it first to be “relatively trivial” (IWS2, PI2m, p. 20). However, when he subsequently recalls his own experiences in which authorship “really was a problem” (IWS2, PI2m, p. 20), he arrives at the conclusion that authorship is “not such a simple thing” (IWS2, PI2m, p. 20). Similarly, for a PhD student in the life sciences, it is “a tricky thing” (IWS12, PhD37f, p. 28). Likewise, the process of ranking contributions and, subsequently, authors involves for a PhD student, “some rather personal decisions” (IWS2, PhD4f, p. 15). In a discussion group, a PhD student, talking about authorship practices in general, makes a comparable point, seeing it as a “very personal, I think it’s not a, a scientific decision” (IWS7, PhD15f, p. 32). Or a life scientist says how “I personally felt” (II, PI16f, p. 15) when talking about a ranking conflict she was once involved in.

Such reflections about authorship practices and their challenges exemplify their highly situated and multiple nature. Moreover, these stances elucidate how researchers may feel as though they are thrown back to themselves in both translating their valuations into particular authorship decisions and in terms of judging them — possibly due to the common opaqueness surrounding authorship. The lack of clarity is also evident in a PhD student’s musings on whether “just because a person showed me how to do it, is he also allowed to or should he be on the paper then in the end?” (IWS12, PhD36f, p. 28). Utilising both ‘allowed’ and ‘should’ signals the PhD’s ambivalence about whether valuations of contributions are informed by explicit guidelines governing who is ‘allowed’ to claim authorship or whether researchers are more likely to adhere to tacit guidelines, rendering it the responsibility of the individual researcher to discern what they ‘should’ do.

Furthermore, this multiplicity may be the reason for, or at least reinforce, the intense debates among the participants in the group discussions analysed regarding authorship. In addition, it contributes to an enhanced comprehension of the pervasive references to authorship conflicts experienced by those participants or others, as well as the struggles to make sense of the authorship decisions discernible in individuals’ narratives. Further, the recounted debates among members of a research group, whether ongoing or recurring, likewise demonstrate how the multiplicity may impinge on researchers’ work and lives. This struggle

may stem from researchers' uncertainty about which principle to use to value their contribution and, potentially, which criterion to apply. Observations of this nature indicate the researchers' difficulties in achieving a more overarching, differentiated valuation of contributions, authorship positions, and, ultimately, authorship practices. Or, in the words of Wylie (2016): "We see here the striking complexities of the multiple effects and perspectives of social ordering, which workers are always simultaneously enacting, resisting, negotiating, and constructing" (p. 89).

This careful weighing of different authorship decisions is particularly discernible in the analysed discussion groups. The joint act of balancing several values, exerting different levels of vehemence, exemplifies how the multiplicity of authorship practices comes into existence. If the desired outcome of authorship decisions is to "keep everybody happy but still be fair and honest" (IWS12, PhD34f, p. 29), the difficulties researchers experience in this decision process become comprehensible. Sometimes, the wish "to maintain good relationships" (IWS12, PhD37f, p. 28) with one's colleagues can be in tension with the desire to "give credit where credit is due" (IWS12, PhD37f, p. 28), which has been the focal point of Section 8.5.1, wherein the efforts of researchers to strike a balance between care and fairness are delineated.

Early career researchers are attuned to the challenges associated with authorship practices, possibly due to the struggles many have to comprehend them. The act of authorship decisions is perceived by one PhD student as "quite hard [...] and I wouldn't want to decide [laughs] who is there" (IWS12, PhD34f, p. 29). More senior researchers may not necessarily find the decision-making process as challenging, even if they are aware that it can be a "tightrope walk" (translated by the author;¹²⁶ VU, PI2m, p. 10). Nevertheless, one senior researcher is convinced that, possibly after having engaged in valuations yourself, even if not voiced, "you have a good feeling, at what point are you worth for acknowledgement, at what point after all have you been a real help to say, OK, authorship" (translated by the author¹²⁷; VU, PI2m, p. 10). Consequently, the argument could be posited that both the knowledge of and the familiarity with the multiplicity of authorship valuations are decisive for how they are perceived to matter in one's situated normality. Or, in the words of Nästesjö (2021), "reducing uncertainty about how to demonstrate one's worth in accordance with 'what counts,'

[...facilitates] to organize the world and observe oneself" (p. 239), suggesting that growing self-confidence with increasing experience can also improve how at ease one feels in one's research environment.

8.6.2. *Taming the multiplicity?*

Consequently, some researchers — and PhD students in particular — may yearn for a sense of stability and security to mitigate their insecurities. To illustrate this point, a physics postdoc reminisces how his former supervisor told him "to do at least 15% of the work to be considered an author on the paper" (VU, PDoc1m, p. 5), a practice exemplifying the endeavours of senior researchers to render authorship practices more comprehensible to early career researchers. However, this practice is tied to the particular supervisor and, moreover, what constitutes 15% of the work can be contested. The same researcher justifies his authorship position on a particular publication by stating how many figures he has contributed to the total figures in the paper. His underlying assumption may be that making the quality of his contributions quantifiable strengthens the legitimacy of his valuation. Such an assumption may derive from the symbolic logic that seeps into valuations rooted in the epistemic realm. However, his approach resonates with a similar attempt by a junior researcher who also draws upon the quantification when legitimising their ranking in the authors' list by saying how "if I'm writing a paper, I'm doing a lot of work, like 60 or 70 percent of this work" (BGSP 1_1.1, p. 15). For PhD students in particular, having a concrete guideline may ease their struggle to make sense of the authorship practices and the underlying multiplicity of valuations. An intriguing aspect is that such a means of orientation may be either self-developed or provided by supervisors or peers, rather than being based on formal guidelines from a journal or university.

In light of the research interest of this thesis, the question arises as to whether researchers perceive this multiplicity of attributing authorship as a valuable phenomenon or whether a standardised approach to authorship is perceived as more suitably aligning with their expectations and representing the contemporary science system. Furthermore, consideration must be given to whether such a standardised and regulated approach is, from their perspectives, even feasible. Accordingly, the following paragraphs delineate the diverse stances researchers adopt when discussing the multiplicity of values, both within the setting

of discussion groups and in interviews.

In more general debates concerning authorship, the coexistence of contradictory arguments is detectable: Some researchers long for the establishment of “standardised authorship based on something objective” (IWS2, PhD3m, p. 23) and the abolishment of the hierarchy inherent in authors’ lists by stating the various kind of contributions. They may reason how, from their perspective, “there’s just too much about sympathy when it comes to co-authorships, so I wish when it comes to the ordering there was some guidance or guidelines” (BGSP 1_1.1, p. 15). Or they speak of an “ideal world” (IWS4, MS3m, p. 31) with homogenous authorship practices, knowing this is highly unlikely to become their situated normality “because there is no objective right” (IWS4, MS3m, p. 31) in their view, even though it can impact the progression of a researcher’s career.

Consequently, as posited by various researchers, a system in which “not only one person decides, but you have some guideline — and then you discuss and then it should be more fair” (BGSP 1_1.1, p. 15) is advocated, thereby underscoring the notion that fairness is the ultimate value by which authorship practices are judged. Interestingly, some researchers may have the impression that “most institutes don’t have policies” (IWS5, RA1, p. 15). Also, a master’s student in the field of life sciences who is already working in the lab shares this impression: “I mean it’s, I don’t think it’s really regulated. It’s really random, like for the people who publish, whether they include somebody or not” (IWS7, MS8m p. 31). This master’s student struggles to make sense of the authorship practices he has observed so far, as he cannot detect any comprehensible patterns. He is not an isolated case but rather exemplifies a shared feeling among early career researchers. Moreover, given that, in the views of several researchers, it is unlikely that an authorship policy can be agreed upon, even at the departmental level, further underscores the difficulties associated with authorship decisions.

Hence, a wide range of early career researchers tend to view the multiplicity of authorship practices that they know, either from hearsay or experience, rather critically, because it “can potentially also create pretty unfair situations” (IWS4, MS3m, p. 17). This judgment is also shared by a postdoc who has encountered a multiplicity of authorship practices in his several years of working as a researcher. He thinks group leaders “try to lead and [...try to be] fair.

But sometimes it's not really fair, sometimes not right" (VU, PDoc1m, p. 10). Such a critical standpoint can be better understood when taking into account the position of early career researchers in academia. When entering academia, they might be unaware of this multiplicity of authorship practices, and thus they themselves or others just then "happen to be in a place" (IWS4, MS3m, p. 31) in which specific authorship practices are prevalent. In the view of one junior researcher contemplating the possibility of offering constructive criticism regarding perceived unfairness in authorship decisions, "it depends from group to group. If your supervisor is quite flexible and nice you can discuss about it, and then you kick these people that should not be really co-authors" (BGSP 1_1.1, p. 14). In light of that multiplicity, a junior researcher may profess that authorship has "never really bothered me so far" (BGSP 2_1.1., p. 5), thereby highlighting the existence of narrations, also among junior researchers, who are satisfied with their situated normality — even if this stance is not a typical one at this career stage.

This contingency of authorship practices constitutes either an advantage or a disadvantage for them in comparison to the publication output of other early career researchers. Hence the multiplicity of attributing authorship, detectable in different labs but also in various locations, "potentially causes a big injustice sometimes" (IWS4, MS3m, p. 29) because the practices may be in conflict with each other. Therefore, one researcher may profit from a rather inclusive approach towards authorship and may score better in formal assessments, prompting the question of how this multiplicity is discussed against the backdrop of the quantification of research and researchers.

8.6.3. Metrics and multiplicity

The multiplicity naturally permeates researchers' track records; however, subject to a researcher's level of experience and their position, this may engender some concerns or barely any at all, because they are more or less subjected to assessments and outcomes. Irrespective of whether researchers themselves are affected, the meaningfulness of metrics in decisions such as the hiring of personnel or the funding of research can warrant deliberation. A junior physicist reflects on how "authorship is important for your impact factor for example, and it's still like used for how high-ranked you are, even though everybody knows that it's like actually it doesn't say anything. That's like a weird combination then" (BGSP 5_1.1, S. 34). Or,

in the words of the previous postdoc, “of course, the list of publications is an essential career criterion in science, by which research success is measured — sometimes justifiably, sometimes not so much” (translated by the author¹²⁸; VU, PDoc5f, p. 3). Indeed, a senior researcher who addresses the evaluation of researchers’ track records speculates that simple conversations with applicants for a position may prove more fruitful — after having made a first selection based on impact points:

I take a CV and look: does he have Science, Cell and Nature papers? No, left-wing bunch - yes, right-wing bunch, yes? Yet, the most important thing is to talk to people. Five minutes is enough to find out, smart guy or forget it, yeah? And that, we unlearn that. And also the universities, the universities can only count impact points [...] they count points and then they add them up, but they don’t look at the person. I think we have either consciously or unconsciously unlearned this. We have to do it as much as possible in personal conversations, otherwise counting purely impact points is a bad way, yes. (translated by the author¹²⁹; II, PI20m, p. 40)

In contrast, junior researchers may be unsure about what counts in assessment situations. Meanwhile, a group leader openly discloses in an interview how the h-index is, at least for him, not relevant when going through applications for a postdoc and that the number of citations is not a useful metric for evaluating junior researchers before five to ten years have passed since they finished their PhD.

Correspondingly, researchers tend to be rather critical of the quantification of researchers. Nevertheless, they find it challenging to envisage viable alternatives and also acknowledge the benefits of metrics. The following quote by a group leader is in alignment with observations by Müller (2021) among senior scientists who welcomed metrics as a means of countering nepotism:

You’re recognized by your grants and your publications, both are pretty important. And of course, you have to measure people somehow and measuring them by publications is probably the most correct way to do that. I mean, I come from this country like Cyprus where ‘who you know’ gets you places rather than your achievements and so by having a way that you can actually say: no, actually this person is better than this person, because you look at their publication record is, you know, you have to do that, otherwise- . And even still, it’s not done very well, but we need to, however it is not a perfect way, I understand, but it’s a way that you promote the people who should be promoted based on the quality of their research. (II, PI16f, p. 18-19)

The absence of alternative measures that are perceived to be more valuable based on the

identified value fairness, for instance, frequently results in the acceptance of metrics.

In conclusion, researchers may grapple with the multiplicity of authorship practises when they, on the one hand, do not feel fairly recognised for their work and do not see their contributions to the advancement of science duly credited. This may be due to a discrepancy between their own valuations and those of the decision-makers and/or due to the lack of clarity surrounding authorship-related valuations. On the other hand, since authorship decisions have implications for future assessments (as reflected in metrics such as the h-index or citations), several potential consequences may arise. The multiplicity of authorship allocation, coupled with the inability of metrics to mirror this, can engender heightened anxiety among junior researchers concerning their future career trajectory, as it may appear to be even less predictable — in addition to the inherent hazard of scientific research. This resonates with the conclusion of Haddow and Hammarfelt (2019), who note that for early career researchers “[t]here is a risk [...] that metrics will undermine that identity at a time when it is most precarious” (p. 140).

Moreover, when authorship practices are subjected to the dominant “regime focusing on individual productivity” (Fochler et al., 2016, p. 186) rather than existing within a “regime of valuation based on care and collectivity” (Fochler et al., 2016, p. 186), a shift in the behaviour and practices of care of the implicated researchers may follow. This resonates with Mol’s (2010) observation that “the one-dimensional character of ‘performance indicators’ clashes with the *multiplicity* of goods” (p. 226). Adopting a more inclusive approach to authorship so as to ensure that no person feels marginalised, engaging in conversations about the value of negative findings, and lending support to another person in their research without worrying about whether that is detrimental to one’s own progression, are examples of how practices attending to other goods than symbolic capital in the form of authorship (positions). Arguably, such practices can be discerned in the data analysed because of the multiplicity of how to value contributions and positions, despite several researchers sometimes perceiving difficulty in holding their ground against the symbolic rationality. Moreover, as has been noted in numerous places throughout this thesis, the degree of agency to withhold the symbolic rationality varies. Yet, subjecting oneself completely to the “*extrinsic* character [of performance indicator] risks to undermine the *intrinsic* motivation” (Mol, 2010, p. 226) and,

as one could argue, the identity of the individual as a researcher.

Consequently, such practices of care and responsibility could have the potential to enhance researchers' overall well-being and motivation. A key point to emphasise here is that it is this space of possibilities, opened up by the multiplicity, that facilitates such practices. Nevertheless, it is a space of possibilities, as evidenced by the numerous instances encountered in the narratives of researchers in which they struggle to reconcile their own values, those of others, and potentially those of what they refer to as 'the system'. Thus, this thesis embraced the insightful recommendation proposed by Annemarie Mol:

Attentiveness to what does *not* work. Those who want to improve the practices in which they work, should be on the lookout for tensions, frictions, problems. For sites and situations where different *goods* do not easily go together. For clashes and places where it hurts." (Mol, 2010, p. 227)

PART IV FURTHER REFLECTIONS AND FINAL REMARKS

9. Conclusion

This thesis sought to answer the following overarching research questions: How do researchers make sense of current authorship practices? And how does this reflect their values and their visions of responsibility and care in their respective research environments?

In what follows, I outline the five main conclusions drawn from the extensive analysis of the empirical material in the previous chapters. The five findings presented here are characterised by a commonality in that they all foreground researchers' perspectives and positions, insofar as they all emerged from the analysis of researchers' narratives with their inherent valuations. Through this lens, the findings also offer reflections on researchers' identities, which, as Wylie (2021) crucially emphasises, are based on "how individuals perceive themselves as well as how others treat them" (p. 146). Consequently, researchers' accounts are infused with instances where they express concern over how others perceive and might subsequently treat them.

The conclusion opens with a discussion of how researchers experience their outer worlds, namely their situated normalities, and which facets they perceive to be modifiable and unmodifiable, given that their values and identities are rooted in these normalities. It then moves on to reflections on the fundamental valuations of contributions, which are decisive for whether one becomes an author. These valuations exhibit a rift stemming from researchers' position in academia, shaping what they deem to be valuable. At the core of the subsequent finding is researchers' care for authorship positions, which mirrors both their position and how they want to present themselves and be perceived, giving rise to inner turmoil. The following finding builds on this, insofar as the striking opacity surrounding decision-making processes reflects the desired self-presentations among other (potential) co-authors. However, the researchers' desire for clarity surrounding authorship indicates a reluctance to compromise their image by open authorship conversations. Finally, the conclusion draws attention to how the transcending value fairness ultimately reflects a researcher's situated normality, the values guiding both the researcher's valuations of contributions and positions and the interrelated visions of responsibility and care, herein painting a picture of their identity as researcher.

9.1.(Un)Modifiable situated normalities: Researchers' perspectives

The analysis of researchers' sense-making processes concerning authorship, founded upon the conceptual basis of epistemic living spaces, yielded insights into how researchers "are regularly faced with complex arrangements of elements that organize the [...authorship practices] in which they are involved" (Dodier & Barbot, 2021, p. 65). In other words, the conceptual foundation illuminated not only what researchers narratively construct to be their situated normalities but also how they experience, think, and feel about said normalities, including which predominantly structural and immutable aspects they perceive as affecting authorship practices.

The concept of epistemic living spaces drew our attention not only to epistemic issues and concerns, as well as to the symbolic rationality visible in the dominant quantification of academia, but also to dimensions that have tended to be neglected in the literature so far. Thus, the concept particularly sensitised me to the temporal, spatio-material, and social structural, and perceived immutable aspects that researchers identified as shaping their situated normalities in relation to authorship practices. In turn, the mobilisation of both narrative and valuation studies in the analysis paved the way for distinguishing between moments when researchers tended to try to make sense of authorship practices, perhaps by pointing to the perceived immutability of certain aspects, and moments when they were predominantly concerned with engaging in valuations. These latter moments often included instances of researchers assigning or taking responsibility, or reflecting on how care practices might enhance authorship decisions. But even in reflecting on and envisioning how immutable issues shape authorship practices, researchers elaborated on how they, or others, can potentially mitigate possible negative effects.

Accordingly, the primarily structural and immutable aspects moulding researchers' situated normalities and, consequently, their authorship practices are briefly reviewed. Beyond this, the subchapter demonstrates how researchers carve out care practices even from such partially immutable conditions and how they ascribe responsibility to themselves or other individuals.

Employing the concept of the epistemic living spaces has meant attending to how spatio-material, temporal, and social working conditions in contemporary academia constitute what is referred to as researchers' situated normalities, and how these affect authorship practices from researchers' viewpoints. Herein, certain under-addressed aspects have been elucidated, because mobilising a concept engenders a "sensitivity that opens up (or indeed, attaches us to) particular worlds and forms of world-making, while closing down others", as de Laet et al. (2021) observe. Moreover, the crystallisation of researchers' situated normalities in the course of the analysis resonates with the argument by Croce and Salvatore:

Normality is the outcome of a knowledge production process that homes in on a given aspect of reality — such as a practice, a profession, or more generally an activity or role within a practical context — to single out and objectify the elements which are claimed to characterize it. (Croce & Salvatore, 2017, p. 276)

Stepping back from the list of authors and exploring the framing conditions informing researchers' normalities and embedded authorship practices thus provided insights into their emotional worlds; it also uncovered instances in which they contemplated how practices of care might tweak their normalities. As a reminder, the notion of normality was also deliberately selected because it possesses two distinct yet interconnected characteristics, as Hacking aptly articulates:

One can, then, use the word 'normal' to say how things are, but also to say how they ought to be. The magic of the word is that we can use it to do both things at once. (Hacking, 1990, p. 163) p. 163).

Consequently, the notion of normality is capable of indicating both immutable characteristics and aspects that are amenable to enhancement. While the characteristics of researchers' normalities evidently overlap, for example given they are all researchers based in Austria in the life science and physics, they may nevertheless share certain aspects only with specific others, thereby assuming that every single researcher experiences a unique situated normality.

Moreover, I would argue, that the unique composition of the data set analysed was an advantage in richly exploring their situated normalities. Because the majority of the data was collected with primary research interests other than authorship practices, it permitted probing

through the back door, so to speak, since researchers linked certain parameters of their normalities to issues of authorship unprompted.

One of the framing conditions often referred to is the normality of cross-institutional and/or cross-disciplinary collaboration (see Subchapter 6.3). Researchers talking about how their work is inevitably a collective endeavour nicely illustrates the interplay between modifiable aspects and unchangeable parameters, and how, in turn, this interplay affects their feelings. Emotions can range from simply accepting this need to work with others to citing this collective work as one of their sources of pleasure. From the analysis, it appears that when one is in a position to choose with whom one collaborates, the likelihood that it — and the resulting authorship decisions — will be a pleasant or, at least, a neutral experience is higher. This sentiment may also stem from the potentially lower importance of a single authorship decision for one's scientific career if one has already secured a well-established position in academia. Draining feelings, however, may be caused by the struggle to 'properly' value the contribution of colleagues from another discipline or to trust the valuations of their collaborators (see Subchapter 6.5). It could be speculated that it is not so much the physical distance as the social distance, the lack of familiarity — both in terms of the depth of relationships and of the knowledge of the specific (sub)domain — that is at the root of such concerns. In times of collaborations that stretch beyond organisational barriers, national borders, or even across disciplines, measures to mitigate the actual, and more crucially, the subjective, distance are of the utmost relevance. The initiation of open dialogues would undoubtedly constitute a significant development, a matter which is addressed in more detail in the finding presented in Subchapter 9.4.

A closely related issue is the concern expressed by junior researchers about whether group leaders can adequately value their contributions. To substantiate these concerns, junior researchers often cite the frequent absence of group leaders from the lab, whereas this absence is often viewed as normal by researchers across all career stages. Their concerns appear to be primarily rooted in the apprehension that, due to their absence, a group leader lacks familiarity with the development process of, and the substantial time commitment leading up to, their contribution. Moreover, the potential for such unfamiliarity can be further compounded by the changing nature of contributions over the course of a scientific career,

which can lead to a lack of full understanding and appreciation of the technical details involved. This change in contributions over the course of a scientific career is often deemed almost inevitable, with senior researchers avoiding this change being presented as the exception rather than the normality (see Subchapter 6.4).

Interestingly, the value of a group leader's contribution in infrastructure-heavy research areas is, in turn, repeatedly contested by junior researchers, reflecting their own unfamiliarity or lack of knowledge of a group leader's day-to-day activities. Consequently, they may not fully appreciate some of the underlying effort and resources inherent in the 'mere' provision of infrastructure (see Subchapter 6.6). This illustrates the difficulties young researchers themselves have in judging different kinds of contributions — i.e. they are not necessarily free from the struggles they suspect others of having. As a result, senior researchers also bemoan the perceived scarcity of recognition for their input and work by members of their group or by researchers who are more junior than they are.

Thus, the variety of contributions that typically make up publications and which are usually associated with certain formal positions in academia can increase the distance felt between the researchers engaged in producing a publication. The corresponding care practices of group leaders can aim to create a feeling among group members that they are in touch with the work of their subordinates, even though, in everyday life, there may be a physical distance between them. Conversely, in instances where the majority of individuals within a research group are engaged in all contributions and consequently possess a comprehensive understanding of the diverse contributions, it can prove challenging to distinguish the relative significance of each contribution. The attribution of authorship may therefore be perceived as disproportionate. In extreme cases, the mere presence of an individual in a space can lead to co-authorship, giving rise to criticism. Thus, a clear division of labour is often welcomed to avoid such conflicts and increase the sense of responsibility for one's own specific task. While the degree of specialisation is contingent on the nature of the research, the individual in charge can, nevertheless, facilitate or impede a certain clarity.

Looking through the lens of the concept of epistemic living spaces at researchers' narratives implies an awareness of the entanglement of the different dimensions and how the

dimensions themselves are multilayered. Moreover, while a particular layer of one dimension may be perceived as immutable within a research group, or even within a scientific community, its interwoven counterpart in another dimension may be considered to be modifiable by a caring researcher. The unmodifiable normality of limited employment contracts, at least among PhD students and postdocs, results in researchers entering and leaving working spaces. This may further complicate matters because epistemic ownership of contributions may be less clear when, for example, an unfinished publication is taken up by another researcher (see Section 8.2.1). However, in light of the constraints imposed by these limited contracts, the efforts of former group leaders or members of their former research group to secure inclusion on a list of authors after a person's departure became all the more noteworthy. Another layer of the temporal dimension, in the form of the identified valuation criterion of duration — i.e. taking into account the amount of time invested in the production of a contribution — can facilitate the differentiation between contributions that are perceived to be of equal or similar epistemic value, although this is a matter of varying opinions among researchers. Analysing such discussions has brought to light the ways in which researchers relate the different dimensions and layers that shape practices of authorship in their narratives to each other, thereby constructing their situated normality in terms of authorship.

How a dimension with its various facets is neither completely immutable nor modifiable is also evident in narratives foregrounding how the social sphere modifies authorship practices. This is because it is the exception rather than the rule that publications in the life sciences and physics are single-authored.³⁶ Thus, the human factor, so to say, plays a crucial role in how researchers experience authorship negotiations and, ultimately, how they experience their work and life in academia (see Section 8.2.2). That researchers may behave egoistically in authorship disputes further complicates authorship and may fuel conflict but it seems to be not entirely avoidable, given that a common conclusion — voiced in both interviews and group discussions — is that researchers are, after all, human and that such behaviour is therefore also somehow normal. Nonetheless, such behaviour arguably represents a tension

³⁶ And even in these rare cases, researchers are still affiliated with a particular department and are therefore immersed, to varying degrees, in the social life of the department and cannot completely escape the social element.

with Merton's (1985) norm of disinterestedness, and with the vision of a caring and responsibly acting researcher that many researchers hold. In particular, early career researchers' frequent disappointment with other researchers' egoistic behaviour suggests that, in their imagination or ideal, researchers should be above such selfishness. Obviously, the researchers did not explicitly refer to Merton and his norms in the group discussions and interviews analysed, but given their typically critical stance, this suggests a degree of alignment with the norm of disinterestedness and illustrates its validity in contemporary academia. This tension between observed behaviour and desired ideal could potentially be alleviated by not rewarding such behaviour in authorship negotiations. Otherwise, those who are very assertive in authorship claims could be more likely to advance in their scientific careers — even if their behaviour might be subject to disapproval. Furthermore, it can be argued that the manner in which researchers conduct themselves during authorship negotiations is co-produced with their identities. Consequently, a person of a more reserved character may get the impression that they are ill-suited to a highly competitive academic environment. This echoes the concern expressed by Fochler et al. (2016) about the potential emergence of "problematic dynamics of selection and self-selection" (p. 197) in academic environments that place a disproportionate value on the quantity of one's publication output. The ways in which such assertiveness is also shaped by gender roles, by cultural values that encourage or discourage open disagreement with superiors, or by the ease with which one moves through academic spaces given one's socio-economic background, suggest how broader structural conditions also inform how researchers experience and thus value authorship practices, although such questions lie outside the scope of this work.

Furthermore, while the hierarchical working structure in academia is not questioned per se — and thus is narratively constructed as one aspect of their situated normality — many of the researchers who have experienced more senior and powerful researchers being 'automatically' listed as (last) authors, or being added for strategic reasons, consider such practices to be an irresponsible exercise of a power imbalance in authorship decisions (see Section 8.2.3). Such behaviour on the part of superiors is therefore not taken as given, which is in contrast to the potentially selfish nature of researchers. Even if some researchers narratively take a certain extent of responsibility for speaking out against such authorship decisions that do not reflect their own valuations, the issue at stake is perceived to be the

irresponsible behaviour of a researcher whose decision can override all others. Essentially, in the discussions and reflections studied, researchers' attribution of responsibility for ensuring that decisions about authorship are made as 'good' as possible is grounded in the structural working conditions, since these conditions are perceived as offering more or less room for manoeuvre — depending on an individual's position within the system. The fundamental questions concerning the assumption of responsibility, the attribution of responsibility, and the encompassing nature of care practices are rooted in the imagined and perceived scope for manoeuvre. Indeed, researchers envision and construct practices of care and responsibility in light of this room to manoeuvre. Furthermore, how efficacious researchers perceive themselves undoubtedly informs their identity.

In conclusion, mobilising the concept of the epistemic living spaces is facilitated by not analysing researchers' narratives as an inextricable entity but instead as an entanglement of different dimensions, some strands of which are modifiable for some, while others are virtually immutable for all. As a result, I have been able to identify those parameters that constitute a certain situated normality in terms of authorship for researchers, such as cross-institutional and/or cross-disciplinary collaborations, the division of work within a research group, hierarchical differences, temporary contracts or the human factor, so to say. All of these can add to the complexity of authorship decision-making processes. Bringing these issues to the fore demonstrates that the mere analysis of author lists is insufficient for gaining a nuanced and fine-grained understanding of how researchers perceive authorship practices because it overlooks how the context modifies these practices from the researchers' perspectives. While existing works naturally refer to how neoliberal logics affects the meaning of authorship for researchers, the spatio-material and temporal aspects, and the web of relations, are rarely discussed and, if so, are not afforded much detail. Hence, presenting and discussing how the spatio-material, temporal, and social dimensions, based on the researchers' sense-making processes, characterise their each individual's situated normality with regard to authorship refines the perspective on and comprehension of the valuations discussed because they are not detached from these more structural layers. This argument can be extended to researchers' identities in relation to authorship, given that identities are also "subject to shifting structural and contextual" (Davies, 2021, p. 208) matters.

9.2. Authorship: A delicate balance between the amount of work invested and the knowledge generated

Two potentially conflicting approaches to the valuation of contributions were identified, which appear to be primarily due to the differing career stages and the interlinked differences in the typical type of contribution. The arguments put forward by PhD students, in particular, appear to be based on the premise of work as a value unto itself. To substantiate their claims to authorship, they frequently invoke the linked valuation criterion of duration, thereby underscoring the considerable amount of work invested, regardless of the scientific outcome. In contrast, more senior scientists tend to be guided by whether they represent a gain in knowledge. In addition, they may take the position that 'mere' work without any gain of knowledge is already recognised in the form of a salary and that additional recognition in the form of authorship is not justified. These very different perspectives are one of the reasons why an authorship decision can be interpreted as a lack of recognition of one's own efforts, especially by junior researchers. Accordingly, this thesis argues in favour of sensitivity towards the positionalities of the researchers involved in authorship practices.

Through an open coding process influenced by research in the field of valuation studies, the two values of work and knowledge gain emerged alongside the value of publication as guiding principles for valuing contributions. Drawing on research in the field of valuation studies has profoundly heightened my attentiveness to valuation processes, enabling me to analytically disentangle the three strands inherent in the valuation of authorship practices: the valuation of contributions, the valuation of authorship positions, and the valuation of the authorship decision-making processes.

But to return to the question of how researchers value contributions in their narratives, it became evident that the two more prevalent value principles, namely work and knowledge gain, may lead to starkly different decisions about who to list as an author on a publication (see Chapter 6). The third principle identified, publication, does not seem to be as likely to foster disputes, since it renders both work and knowledge gain as means to achieving publication. Interestingly, the propensity of researchers to advocate recognition of work or

knowledge gain through authorship is likely to be associated with their career stage. Even if the reasons for what could almost be described as a generational conflict occurring continuously throughout the analysis are not yet fully known, various possible explanations can be derived from the differently structured situated normalities of researchers.

Firstly, one potential explanation may lie in how contributions are likely to vary depending on the stage of a researcher's career. In more empirical fields, the scientific offspring, possibly as master's students, but definitely as PhD students, are responsible for setting up experiments, taking measurements and so on. Postdocs may mentor more junior researchers but may also be involved in more conceptual tasks. In contrast, senior researchers who head a research group are typically more involved in conceptualising research projects, polishing article drafts, selecting the right people for a research project, or monitoring the progress of any research or PhD projects they supervise, to give but a few examples. As discussed in the preceding finding, master's and PhD students, and the majority of postdocs, are separated from their supervisors in their daily work, which may compromise their knowledge of how much time their supervisor spends, for example, on revising article drafts; this, in turn, may impede the mobilisation of duration as a valuation criterion.

Secondly, since part of a group leader's contribution may be the overall conceptualisation of a research project — and which they also have to present in a manner that will attract funding — part of their contribution may have been made before a PhD student has even joined the research group, for example. In such cases, newly recruited researchers may wonder what the group leader's contribution to a publication is, or whether their contribution was significant enough to warrant authorship, if they overlook or are unaware of such earlier conceptual or organisational work. The extent to which a contribution can be seen as going back in time may also vary according to one's academic age or self-concept. Whereas a younger colleague might simply observe an idea being 'thrown into the room', from the perspective of the idea provider, this input materialises years of scientific work and knowledge. This is an illustration of "the multiplicity of viewpoints on the appropriateness of valuations" (Waibel et al., 2021, p. 56) that is evident in the analysed data.

Thirdly, the perceived uncontrollability of the tasks for which junior researchers are primarily

responsible could be an explanation for the possible insistence on the valuation of contributions based on the value of work. Bloch succinctly encapsulates the sentiments with which many PhD students in particular may not yet be so familiar:

Research is an activity that involves the whole person. This applies to all researchers, but is perhaps especially pronounced at the PhD level. This is because PhD life is particularly focussed upon research, and because PhD fellows have limited experience of the character of research. Research entails passion, but is also a special activity that concerns the 'not yet known'. Research is based on belief, trust and hope, but also on the experience of resistant challenges and related feelings of uncertainty and doubt. (Bloch, 2016, pp. 17-18)

Experiments may fail once, or even several times, and because they are in a more precarious position than researchers with permanent contracts — and are therefore heavily dependent on having a successful track record — PhDs students may favour the approach of rewarding 'mere' time-intensive work with authorship. Otherwise, the perception can arise that their advancement in the scientific field is contingent, at least in part, on luck. However, as Davies and Pham (2023) assert, luck is "a feature of academic practice, something with agency and a defining yet uncontrollable role in the lives of academics" (p. 288). Such an order of values, prioritising work over knowledge gain, can be interpreted as an attempt to mitigate the presumed haphazardness of scientific careers and, thus, to alleviate the potential anxiety that junior researchers may feel about their future career prospects. This interpretation corresponds to the observed "individualized and existential experience of anticipatory uncertainty" (p. 370) reported by Fochler and Sigl (2018) and who posit that "the multilevel dynamics of competition (at the group and the individual level) are a key factor" (p. 370) for it.

Fourthly, in line with the preceding interpretation is the assumption that a researcher might resort to arguing for their inclusion in the list of authors solely on the basis of the vast amount of work they have done or the knowledge that has been (indirectly) gained through their contribution. This is because others may see too great an interest in being named as an author on a publication as too invested in one's own career and not in science per se. Not (seemingly) acting as a disinterested researcher and, I argue, thereby not upholding one of Merton's (1985) norms, is seen as negative and almost offensive by virtually all researchers who raise this issue either in the analysed group discussions or in interviews. Nevertheless, being a

researcher is a profession — especially at the beginning of a scientific career, a rather precarious one — and it is actually unsurprising that this might affect authorship negotiations and researchers' efforts to be included in authors' lists. I would therefore advocate for a sensitivity to the nuances of researchers' motivations when considering requests to be named as a co-author on a paper. While such requests may, at times, appear driven by self-serving career interests, they could also be viewed as legitimate, provided that the boundaries of professional courtesy are upheld. Indeed, considering research as both an occupation and a vocation is a valid perspective, to which one might also be compelled if one has not yet established oneself.

Finally, wanting to be included may also be an expression of a desire to be recognised for one's work. As can be seen from the data, the repeated experience of not being recognised in the form of authorship when one feels that it is deserved can lead to severe disappointment or frustration and can therefore definitely dampen the enjoyment of being a researcher. I concur with the assertion made by Bloch (2016) that "[s]elf-esteem, belief and trust are not only dependent on the activity of doing research, but also on the comments and assessments [...] offered by supervisors and other members of staff" (p. 19). In some circumstances, particularly at the PhD level, such emotionally taxing experiences — which may challenge the construction of an identity as a 'successful' researcher — can even result in the decision to leave academia, as the analysed narratives demonstrate. Whilst not necessarily determinative, such experiences can undoubtedly be a contributing factor.

Therefore, on the one hand, a full understanding of how such conflicts can arise from basing one's valuation of a contribution on a different value might prevent some authorship conflicts or disappointments, because it might facilitate putting oneself in the position of the researcher whose judgement one does not agree with. On the other hand, taking care, especially as a group leader but also as a group member, that the other researchers in one's group as well as collaborators and others involved feel acknowledged for their work seems to be another recommendation that can be derived from this finding. Indeed, on the basis of the insights gained, it seems advisable to reflect on and explore other forms of recognition that go beyond authorship, since a lack of perceived earned recognition is a focal point that ultimately also shapes a researcher's identity.

9.3.Positions: The credit for my contribution, the capital for my career

The exploration of how researchers value authorship positions revealed that the valuations operate predominantly within a field of tension between epistemic reasoning and justifications informed by the symbolic rationality. Researchers may feel torn between these dimensions, each representing different values. However, while researchers may be reluctant to admit to the impact of the symbolic rationality on their work and life in academia, its effects on many of them are not to be denied. Nevertheless, as the analysis has shown, an argument for why one deserves a particular authorship position rooted solely in neoliberal logic is generally frowned upon. Indeed, as I have demonstrated, researchers' voiced justifications for a certain authorship position are predominantly rooted in the epistemic realm. This thesis therefore advocates for a kind of reconciliation with the symbolic rationality insofar as it would make it more acceptable for researchers to express when they feel constrained by the neoliberal logic of academia. Currently, this pressure appears to be negated in researchers' narratives; or to be more precise, it is portrayed as not being of concern to the subject, whenever the issue of authorship is contemplated.

While researchers at different career stages may disagree on the valuation of a contribution, with a junior researcher applying the value of work and a senior researcher basing their valuation on the value of knowledge gain instead, researchers working in fields with non-alphabetical author lists generally agree on which are the valuable authorship positions (see Chapter 7). In essence, irrespective of the value employed — credit, recognition or capital — researchers tend to conclude that the positions of first and last author, ideally in combination with the role of corresponding author, are the most significant. Such positions are indicative of the most credit for a researcher's work, the most recognition for the knowledge gained and/or the most symbolic capital for the publication, which might be advantageous in future assessments. Accordingly, researchers' focus — and one could argue researchers' care — may be directed towards these few positions in the ranking of authors and potential conflicts. As such, these practices of care demonstrate how "forms of attachment can work with and through the grain of hegemonic structures, rather than against them" (Murphy, 2015, p. 719). Indeed, their care arguably coincides with the attribution of value to these positions in the

broader scientific system. Therefore, a researcher's persistent lack of care for authorship positions generally means that the individual has decided to leave academia, since even a researcher who has achieved a position in academia detached from any assessment of their publication record may still wish to be recognised for their work or credited for any contributions to the body of knowledge of their scientific community.

But even if the overall picture appears relatively unambiguous, the detailed analysis revealed subtle differences lying mainly in discussions and reflections on whether a particular position symbolises the right recognition and/or credit for a researcher's contribution. In other words, researchers tended to be more concerned with whether a particular position aligned with the valuation of a specific contribution, rather than having insecurities about the meaning a given position might convey to future reviewers on hiring panels or grant committees. In such attempts to align the valuation of contributions with an authors' ranking, one can also trace moments of care when researchers elaborate on how they strive to treat others fairly in the ranking process if they have a say in it. This care might stem from an understanding of how important it is to give a researcher appropriate credit for their contribution and to recognise their work. Furthermore, in their stories, researchers set their sense of responsibility for a publication in relation to their authorship position, illustrating how the link between credit and responsibility are two sides of the same coin.

It is precisely in the narratives of the conflict-laden moments in which researchers disagree about the ranking of authors that it becomes clear which justifications are deemed to be legitimate. The justifications given to others refer primarily to the significance of a particular contribution for the creation of a publication or to the gain in knowledge it represents. It is questionable, however, whether all ranking decisions are based solely on such considerations and this is sometimes also questioned by the researchers themselves in their narratives. Interestingly, care for another's scientific career may be one reason why ranking decisions are not based solely on epistemic considerations. By caring for a particular researcher, others may feel overlooked but also not appropriately credited and recognised. A possible recommendation derived from this finding would be that such care practices should be consistent with valuations rooted in the epistemic realm and, in a second step, take into consideration the symbolic logic by prioritising a researcher who needs a specific authorship

position within the symbolic rationality more than another, if their contributions are (basically) indistinguishable in terms of (epistemic) value.

Denying the impact of the symbolic rationality on researchers' work and lives — and which is evident in researchers' conceptualisations of themselves and others (see Subchapter 8.4) — and ultimately, on authorship practices themselves, does not appear to be sensible. Instead, its influence should be openly acknowledged. In his foreword to the widely-acknowledged *Metric Tide* report, Wilsdon (2015) succinctly captures the crux of the matter: "Metrics hold real power: they are constitutive of values, identities and livelihoods" (p. iii). Such an explicit admission given, perhaps, in an informal conversation, coming from more experienced researchers may also enable young researchers to gain a more realistic perspective on the importance of authorship positions in any future assessment processes of their publication record. In turn, this may reduce the perceived pressure that some junior researchers feel and which can take on unrealistic proportions. Moreover, encouraging researchers to address their concerns in terms of the symbolic logic may prevent them from invoking unfounded epistemic justifications for claiming a particular position on a paper.

To recap, although the symbolic rationality is highly prevalent in researchers' narratives of authorship positions, virtually none of the interlocutors considered the predominance of this logic in decision-making processes to be desirable. By and large, they advocated for authorship positions to represent a material recognition of work and/or crediting of an epistemic contribution and not to be oriented towards future assessment processes. Yet, because the same judgment of which authorship positions are valuable is reached irrespective of which value is the guiding one, the underlying principle can remain rather obscure. Indeed, this facilitated researchers presenting epistemic justifications in the analysed interviews and group discussions, as well as in any ranking negotiations between researchers, so as to avoid appearing to be subjected to the spell of the symbolic rationality.

9.4. A striking opacity surrounding authorship decision-making processes

Precisely because authorship is an issue that concerns all researchers, and because virtually every researcher has a story to tell on the subject, it was all the more remarkable that there seems to be little dialogue and discussion between researchers in their everyday lives in which they make their valuation processes around authorship practices explicit. Consequently, this finding offers a pivotal insight into how researchers value the overall authorship decision process.

One assumption derived from this finding is how this non-addressability functions as a catalyst for authorship and/or ranking conflicts. These conflicts do not necessarily manifest in overt ways but can also take the form of covert feelings of betrayal, disappointment or withdrawal. Furthermore, the widespread opaqueness surrounding authorship practices can act as a burden, especially for early career researchers. Since early career researchers are usually the ones who do not, or only minimally, shape these decisions, opacity impairs their comprehensibility for early career researchers and, consequently, the predictability of these decisions. The resulting failure to make sense of authorship practices can cause feelings of distress, irritation, and anger and may even lead to the withdrawal of an early career researcher from academia. In light of all these observations, clarity has been identified as a principle driving researchers' valuations of authorship decision-making processes. In this context, the question of who is responsible to care for this clarity is the subject of intense debate in the analysed data corpus, whereby the position of the group leader is identified as the primary locus of responsibility.

With authorship being such a hotly debated issue, it was all the more striking how little researchers generally discuss authorship decisions with the other researchers involved (see Subchapter 8.3). This finding is clearly linked to the previously discussed findings and is indicative of how the valuation of decision processes supersedes the valuation processes of contributions and authorship positions; this is also reflected in the structure of the empirical part of the thesis. On the one hand, the physical distance between researchers at different stages of their careers — or at different places of work in the case of cross-institutional and/or

cross-disciplinary collaborations — can inhibit informal discussions and promote a sense of emotional distance, as evidenced by the analysis. On the other hand, as was also discussed in the preceding finding, researchers may refrain from actively inquiring with superiors as well as with peers about how authorship decisions are made because they do not want to appear as if they are only interested in accumulating as much symbolic capital in the form of authorship (positions) as possible. This is in line with researchers' frequent concerns about appearing selfish and thus not conforming to the ideal researcher. Accordingly, it is not surprising that a significant proportion of researchers consider it the responsibility of the highest-ranking researcher in the hierarchy to initiate such conversations. This allocation of responsibility was also prevalent in the analysed discussions and reflections on the ideal time to discuss authorship, where the overriding implication was the importance of talking about it at all.

However, not all researchers judge authorship decisions they have encountered to be opaque. Perceiving such decisions as mostly clear and comprehensible seems to be advantageous for a researcher's contentment with their identity as a researcher and potentially for their career progression as well, since their own valuation processes align with those of others who (also) have a say. This observation resonates with the assertion by Murphy (2015) that those who are "content [...] are] aligned with and comfortably oriented within dominant ways of belonging" (p. 721). At the same time, these researchers can find it difficult to relate to the potential struggles of others for whom the decisions are opaque. Thus, care practices in the context of this particular finding involve making explicit one's own considerations in valuing contributions and potentially ranking authors. Naturally, the potential ramifications of a certain degree of vulnerability upon the disclosure of such decision-making processes may act as a deterrent to researchers, and even those in positions of authority may fear such a vulnerability. Yet, a recurrent argument maintains that, irrespective of whether researchers concur with a specific outcome, understanding how the judgement was reached constitutes a clear decision-making process that is generally highly valued. Furthermore, imparting the intricacies of one's valuation processes to less experienced researchers while acknowledging the potential for missteps can foster a sense of confidence in those researchers to express their perspectives. Thus, the advantages appear to exceed the disadvantages.

Consequently, one recommendation emerging from the analysis is that the issue of authorship practices should be addressed openly, with the main responsibility attributed to the group leader or the master's or PhD supervisor, who is typically the same individual. This aligns with the predominant view observed in the data that suggests that these individuals typically have the final say in determining authorship on publications originating from their research group. Moreover, a general conversation about authorship practices within the research group, which should be initiated at the earliest opportunity when new members join the group, appears advisable. This should occur regardless of whether the new members have already graduated, are doctoral candidates embarking on their research, or master's students working on their projects. More specific and potentially recurring conversations are advisable when working on a particular publication, possibly with collaborators outside the group, to address any changes affecting authorship decisions. I deliberately limit these recommendations to individuals' respective research environments, as the empirical analysis suggests, in my view, that the multiplicity of authorship valuations, while it may create inequities, also provides room for manoeuvre in researchers' complex situated normalities. I therefore conclude with Pender's argument against a meticulous formalisation of authorship practices with monolithic guidelines:

Science, is however, no different from the rest of our culture. It is populated by real people, sensitive to real pressures. To view science as work helps understand it as the political practice it is, demystified and messy, in which highly educated professionals struggle and sometimes emerge victoriously and often less so. There lies an immense value in vagueness, for it allows political struggles on authorship to settle locally. (Penders, 2017b, p. 14)

Still, the findings presented in this thesis can provide a valuable framework for researchers at all career stages, enabling them to understand the nuances and complexities of how researchers' valuations linked to authorship can vary and enhance the clarity of authorship practices in their environment. This, in turn, can equip them with the necessary tools to engage in the arguably crucial conversations surrounding authorship. But the central precondition for such conversations, I would argue, is the recognition that fairness is a matter of a specific value order, bringing us to the discussion of my final finding.

9.5. Fairness: A matter of a specific value order

Fairness emerged as a value by which researchers judge authorship decisions and the underlying processes more broadly. However, as brought to light through analysis, reflecting on the fairness of authorship decisions encompasses an ordering of values corresponding to the multiplicity of assigning authorship (positions). This aligns with the astute observation by Boltanski and Thévenot (2021) that there is a “tension that weighs on the sense of fairness whenever it is called upon” (p. 358). Researchers’ valuation processes can be permeated by their desire to simultaneously care for giving credit for knowledge gained, the recognition of effort, and the accumulation of symbolic capital.

The creation of a value heterarchy through reconciliation, however, may not be feasible or even desirable in all cases. Correspondingly, there were many instances in the data in which researchers preceded or favoured the prioritisation of a specific value, thus arguing in support of a particular value order. One consequence of this potential process of value ordering is that researchers’ understanding of what constitutes a fair authorship practice is not monolithic, but rather multiple. This multiplicity is not only a result of the ordering of the values rooted in the epistemic realm, the social sphere, and the symbolic rationality — a process informed by a researcher’s positionality — but also a product of the spatio-material, temporal, and social matters constituting researchers’ situated normalities. Accordingly, this dissertation captures the complexity underlying articulations on the fairness of authorship practices. Hence, it provides a valuable contribution to how authorship practices matter for researchers’ work and lives in academia and ultimately how “identities are made and transformed” (p. 3) by authorship practices, to borrow the fitting synopsis by Hutter and Stark (2016) reflecting on how valuing things are “reciprocal interactions: making things and making us” (p. 3). Moreover, this thesis delineates how the employment of the notion of ‘fairness’ as a unifying concept within the discourse surrounding authorship constitutes a significant obstacle because it is necessary to acknowledge that fairness is contingent upon a specific value order.

The ramifications of fairness are manifold, as the analysis has shown. When researchers debate who should be involved in the process of deciding an authors' list or who should initiate conversations about authorship practices and thus assign and/or take responsibility for them, fairness is implicitly discussed. Fairness can ultimately be the underlying issue in discussions about whether a researcher's 'mere' work, irrespective of its epistemic value, should be rewarded with authorship as well. Or whenever researchers criticise the prevalent focus on specific authorship positions, i.e. the symbolic rationality that bundles a researcher's care and distracts from other matters — such as the significance of knowledge progress or the recognition of the work of others, to give just a few examples.

It is ultimately against the backdrop of the notion of fairness that "the different modes of ordering or different styles of justification or different discourses may also overlap and interfere with one another" (Mol & Law, 2002, p. 10). Interferences with fairness are particularly evident if the judgments lead to diverging compositions of authors' lists. However, discontent can also manifest when researchers assume that other values govern one's actions than are insinuated even if the outcome remains the same. Yet, as researchers' narrations indicate, they seldom engage in profound introspection, either on their own or with others, regarding authorship practices in their daily lives. As a result, researchers may harbour a sense of unease with specific authorship decisions but find it challenging to articulate how these practices contradict their conception of fairness. Furthermore, they may sense a deficiency in their ability to relate to the valuations of others.

However, the analysis I conducted of the reflections instigated in the group discussions through the various cards, the questions posed by the moderator, and, crucially, the interactions with other participants as well as in the interviews through the inquiries of the interviewer and the ensuing conversations brought to the fore both the sense-making and valuation processes. These conversations, in turn, have been shown to facilitate researchers' connections with and understandings of each other, thereby underscoring and amplifying the crucial nature of measures, such as the opening up of spaces — which especially the group discussions provided — to mitigate the opacity surrounding authorship and to recognise and come to terms with the multiplicity of attributing authorship.

Most notably, a sensitivity to the existence of “a plural, hierarchical, oppositional and therefore interdependent system of values” (Heinich, 2020b, p. 220) in combination with how researchers’ situated normalities matter and shape that system of values is essential. Thus, considering a researcher’s position within academia is of paramount importance because it can inform how contributions are valued and what potential blind spots may interfere with the valuation of disparate contributions by others. Moreover, it can point to the agency of researchers to resist but also to their experiences in counterbalancing the neoliberal logic prevalent in the contemporary science system. The subjugation of researchers, particularly those at the junior level, to the quantification of their research and of themselves is a recurrent and pervasive line of criticism. In response, senior researchers, in particular, may adopt a very inclusive approach to authorship, possibly not aligning with authorship regulations, in order to alleviate the pressures arising from symbolic rationality. In the line of thinking of Penders and Shaw (2020), such authorship practices could even be considered what they term practices of ‘civil obedience’, aimed at mitigating perceived systemic deficiencies.

Rooted in those perceived, frequently referenced shortcomings of ‘the system’, as well as what researchers consider to be their relevant frame of reference, are the allocations of responsibility; given that any discourse on fairness necessitates also an examination of whose responsibility it is to care for fairness. As has been carved out, specific practices and thus objects of care as well as divisions of responsibility may be perceived by some researchers as a risk potentially undermining or enhancing fairness in academia. Additionally, it is a widely held view amongst researchers across all career stages that the senior person in charge — i.e. the group leader — is the person where the lines of responsibility converge. This position also demonstrates that researchers primarily pin their hopes to an individual in their immediate environment, thereby undermining the situatedness of fair authorship practices. Nevertheless, neither those in other positions nor group leaders themselves absolve other researchers of their responsibility to care within their scope of actions.

A group leader can function as a catalyst, encouraging others to exercise and voice valuations, and offering their considerations and wealth of experience. Particularly in cases where more advanced researchers recall past authorship decisions that they might now see differently, especially in terms of their fairness, we see how understandings of fairness can shift. In

general, being in a position to compare and contrast a wider range of authorship practices may refine one's judgments in terms of fairness. Correspondingly, one's sense of fairness is shaped by a researcher's initial socialisation into academia, which is particularly evident in the narratives of group leaders about their early days as a researcher. In these narratives, they often position themselves in relation to the authorship practices they may have encountered as a master's student or, at the latest, as a PhD student. By pursuing either a very similar or a widely deviating approach, senior researchers indicate what they consider to be fair authorship practices. This also illustrates how a researcher's identity is not a static entity but rather undergoes continuous evolution in response to changes in positions, environments and experiences. Accordingly, some researchers, especially more advanced ones, may be more likely to consider a number of interrelated authorship decisions to judge their fairness, which might assist them in being less affected by a decision that they might consider unfair but which is then counterbalanced by another decision rather in their favour. Finally, one's conceptualisation of fairness, stemming from a particular order of values in the context of authorship appears to be tied to one's own conceptualisation as a researcher and the potentially divergent conceptualisation of the other researchers one is confronted with in authorship negotiations.

The analysis presented in this thesis thus demonstrates that a researcher's position and an array of other parameters are instrumental in valuing authorship decisions, thereby also moulding their conception of fairness. The present thesis, therefore, equips interested parties — such as PhD students, postdocs, newly appointed and experienced group leaders, administrative personnel at higher education institutions, or even professionals working in the scientific policy context — with an understanding of the interplay between various facets of researchers' situated normalities and researchers' positions, as well as the respective vocabulary to engage in a dialogue on fair scientific authorship.

Fairness as a guiding value is addressed last because it permeates all the other valuation processes identified in the extensive analysis, pointing to its superior status. However, the opposite argument could also be made, making the alignment of the other valuations a prerequisite for achieving fair authorship decisions. Furthermore, the prominent care for fairness among researchers suggests that, for many, their situated normality is rather to

assume that authorship practices are not (entirely) fair. In particular, those who have already made up their minds to leave academia may never make their criticisms or feelings of unfairness known. But researchers who are dependent on another researcher may also refrain from expressing their frustrations. This pattern of significantly perceived unfairness is consistent with the assertion that judging the fairness of authorship decisions is the most complicated valuation process, which is, in turn, informed by the widespread struggles of researchers to determine the fairness of an authorship decision, as evidenced in the data.

Hence, how a researcher's sense-making process of authorship practices reflects their values and their visions of responsibility and care, is contingent on their current situated normality and on their provisional identity. Consequently, fairness is ultimately a rather fragile value since so many elements converge in its valuation and any changing element can unsettle it. This finding aligns with the pragmatist perspective, which foregrounds valuation as an action. As articulated by Muniesa (2011):

[T]his idea of 'as an action' should be understood in the sense of a process, a form of mediation, of something that happens in practice, something that is done to something else, and so forth; value is definitely not something that something just has. (p. 32)

Fairness, therefore, must be constantly cared for if it is to be desired or, in other words, authorship is constantly in the making.

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Appendix

I. Abstracts

Abstract — English

Scientific authorship constitutes a focal point through which the central issues researchers face in their academic work and their professional lives can be brought to the fore. This thesis investigates how researchers in the life sciences and physics make sense of and value authorship practices in their respective research environments through their narratives. In doing so, the analysis provides insights into the values held by researchers surrounding scientific authorship practices and how they position themselves in relation to the values of the scientific system in which they are embedded. The monograph thus offers a nuanced understanding of the processes through which researchers — shaped by their respective positions within the scientific system — place value on contributions, authorship positions and overall authorship decision processes. These valuations, in turn, are indicative of their self-conceptualisations as researchers, as well as of their visions and experiences of the scientific system, and shape their sense of belonging in academia.

Building on Ulrike Felt's concept of 'epistemic living spaces' and following a constructivist-oriented grounded theory approach, the analysis encompasses 16 group discussions and 42 interviews with Austrian-based researchers across all career stages. This conceptual foundation is central to elucidating how, beyond the indisputable epistemic and symbolic aspects, temporal, spatial-material and social facets shape authorship practices from the researchers' perspectives, and how certain aspects are perceived as immutable and others as mutable. On that basis, the thesis maps what is termed the 'situated normalities' of researchers. It also explores how researchers with different situated normalities and degrees of agency envision and consider practices of care and assertions of responsibility to enhance authorship decision-making processes.

The empirical analysis reveals a striking contradiction: although authorship is hotly contested, the decision-making processes behind it remain opaque and are perceived as almost taboo.

This is particularly evident in how junior researchers often yearn for greater 'clarity' in authorship decisions. Without such clarity, it is argued, authorship conflicts can arise from the prevailing, albeit opaque, disparity in how contributions are valued. Junior researchers are shown to adopt 'work' as their guiding value, yet senior researchers prioritise 'knowledge gain'. Moreover, the analysis of researchers' valuations of authorship positions demonstrates that researchers may face the challenge of navigating the tensions between epistemic reasoning, on the one hand, and justifications informed by the neoliberal logic, with its quantification of researchers' publication records, on the other hand.

Finally, fairness is shown to be a transcending value in judging authorship practices, and as the revealed multiplicity in attributing authorship indicates, fairness is a matter of a specific value order. Accordingly, a researcher's sense of belonging and self-conceptualisation is informed by the alignment of a researcher's value order and the prevailing value order in their situated normality.

Based on these key findings, the dissertation aims to equip both researchers and higher education and science policy professionals with a nuanced understanding of the interplay between the (im)mutable aspects forming researchers' situated normalities and their positions within academia, along with the respective vocabulary to engage in a richer dialogue on fair scientific authorship.

Abstract — Deutsch

Wissenschaftliche Autor:innenschaft fungiert als Kristallisationspunkt, in dem sich die zentralen Fragen, mit denen Forscher:innen in ihrer akademischen Arbeit und ihrem beruflichen Leben konfrontiert sind, bündeln lassen. Diese Dissertation untersucht, wie sich Forscher:innen aus den Lebenswissenschaften und der Physik Autor:innenschaftspraktiken in ihren jeweiligen Forschungsumgebungen durch Narrative begreifbar machen und ihnen Wert beimessen. Damit ermöglicht die Analyse Einblicke in die von Forscher:innen im Kontext wissenschaftlicher Autor:innenschaft vertretenen Wertvorstellungen und deren Positionierung gegenüber den Werten des Wissenschaftssystems, in das sie eingebettet sind. Die Monographie generiert so ein differenziertes Verständnis für die Prozesse, wie Forscher:innen – geprägt durch ihre jeweiligen Positionen innerhalb der Wissenschaft – Beiträge, Autor:innenschaftspositionen und Autor:innenschaftsentscheidungen im Allgemeinen in Wert setzen. Diese Wertungen wiederum erweisen sich als Indikatoren für ihre Selbstkonzeptualisierungen als Forscher:innen sowie für ihre Vorstellungen und Erfahrungen mit dem Wissenschaftssystem und prägen ihr Zugehörigkeitsgefühl zur akademischen Welt.

Aufbauend auf dem Konzept der „epistemischen Lebensräume“ von Ulrike Felt und einem konstruktivistisch orientierten Ansatz der Grounded Theory folgend, werden 16 Gruppendiskussionen und 42 Interviews mit in Österreich ansässigen Forscher:innen aller Karrierestufen analysiert. Diese konzeptionelle Grundlage ist essentiell, um zu verdeutlichen, wie Autor:innenschaftspraktiken aus der Sicht der Forscher:innen über die unumstrittenen epistemischen und symbolischen Aspekte hinaus auch von zeitlichen, räumlich-materiellen und sozialen Facetten geprägt sind und wie bestimmte Aspekte als unveränderlich und andere als veränderlich wahrgenommen werden. Auf dieser Basis zeigt die Monographie die als "situierter Normalitäten" bezeichneten Forschungsumgebungen von Wissenschaftler:innen auf. Darüber hinaus wird untersucht, wie Forscher:innen mit unterschiedlichen situierter Normalitäten und Handlungsmöglichkeiten gedachte und erlebte Praktiken der Fürsorge und Verantwortungsübernahme konzipieren, um Entscheidungsprozesse in Bezug auf Autor:innenschaft aufzuwerten.

Die empirische Analyse offenbart einen evidenten Widerspruch: Obwohl Autor:innenschaft kontrovers diskutiert wird, bleiben die Entscheidungsprozesse dahinter intransparent und

werden als nicht ansprechbar erlebt. Dies manifestiert sich insbesondere im Wunsch von Nachwuchsforscher:innen nach mehr „Klarheit“ bei Autor:innenschaftsentscheidungen. Ohne diese Klarheit, so die Argumentation, können sich aus der Intransparenz bei der überwiegend ungleichen Wertung von Beiträgen Konflikte ergeben. Nachwuchswissenschaftler:innen orientieren sich hierbei am Wert „Arbeit“, während erfahrene Wissenschaftler:innen den Wert „Wissensgewinn“ priorisieren. Darüber hinaus zeigt die Untersuchung der Wertungen von Autor:innenschaftspositionen, die mögliche Herausforderung für Wissenschaftler:innen, die Spannungen zwischen epistemischen Argumentationen einerseits und neoliberal orientierten, der Quantifizierung von Publikationen folgenden Begründungen andererseits, zu navigieren.

Fairness erweist sich bei der Wertung von Autor:innenschaftsentscheidungen als übergeordneter Wert. Zudem wird anhand der in der Arbeit aufgezeigten Multiplizität bei der Zuschreibung von Autor:innenschaft deutlich, dass Fairness eine Frage der jeweils spezifischen Werteordnung ist. Entsprechend werden das Zugehörigkeitsgefühl und die Selbstkonzeptualisierungen von Forscher:innen durch die Übereinstimmung der individuellen Werteordnung mit der vorherrschenden Werteordnung ihrer situierten Normalität wesentlich mitbestimmt.

Ausgehend von diesen zentralen Erkenntnissen verfolgt die Dissertation das Ziel, Forscher:innen und Expert:innen aus dem Bereich der Hochschul- und Wissenschaftspolitik ein differenziertes Verständnis des Zusammenspiels zwischen den (un)veränderlichen Aspekten der situierten Normalitäten von Forscher:innen und ihren Positionen innerhalb der Wissenschaft zu vermitteln. Darüber hinaus wird ein entsprechendes Vokabular bereitgestellt, um nuancierte Diskussionen über faire wissenschaftliche Autor:innenschaft zu ermöglichen.

II. Selected cards from the project IMAGINE RRI

Statement Card - I
System A responsible researcher is just one element of the whole research system. Individual researchers cannot do much. The whole system — policies, funding mechanisms, universities, industry — should be organised to care for our future. We basically just do what the system expects us to.

Figure 1 (Felt et al., 2018b, p. 4)

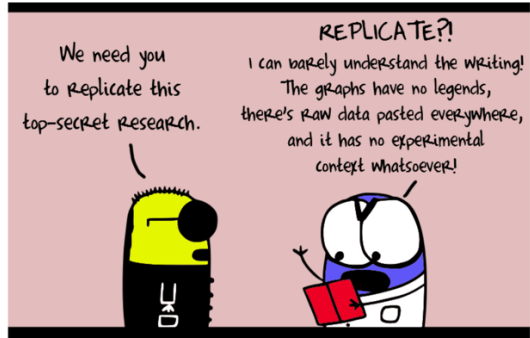
Practice Card - G
Secrecy in Publications  <i>used with permission, Nik Papageorgiou, theupturnedmicroscope.com</i> The purpose of publishing research results is to allow others to replicate and verify results, there- with making knowledge commonly available to others. How far do researchers have the responsibility to include all necessary information in a paper to allow replication? Is it justified to leave some of this information out to secure a competitive edge?

Figure 2 (Felt et al., 2018b, p. 8)

Practice Card - H
Authorship Negotiations  <i>used with permission, BP Coppola</i> Publications are a crucial for continuing an academic career. Being first or last author is most valuable and struggles over authorship are frequent. How can authorship issues be dealt with

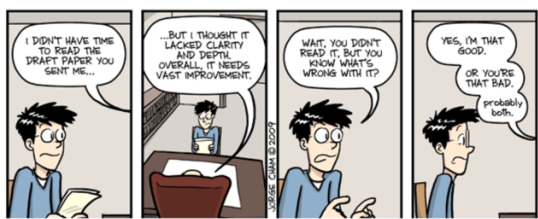
Practice Card - I
Teamwork & Supervision  <i>„Piled Higher and Deeper“ by Jorge Cham, www.phdcomics.com</i> Researchers often emphasise that the quality and efficiency of research depends on teamwork in the lab, feedback and supervision. Does everybody have the responsibility to support others and supervise? Can too close collaboration also become problematic? When is it ok to not spend time on supporting others?

Figure 4 (Felt et al., 2018b, p. 8)

in a responsible way? When should someone become an author? Who should be responsible for these decisions?

Figure 3 (Felt et al., 2018b, p. 8)

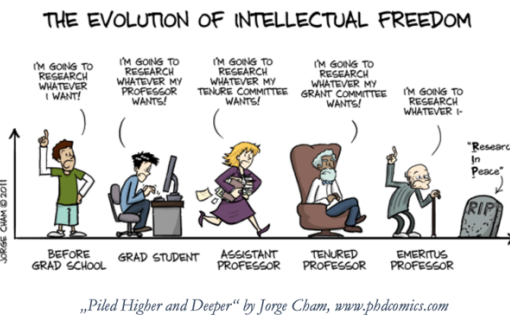
Practice Card - L
Expectation & Work Reality
THE EVOLUTION OF INTELLECTUAL FREEDOM  At the start of an academic career, researchers often have very idealistic imaginations. Yet, they quickly encounter the limits of funding, publication pressure, academic performance norms and many more. How far should/can we go to protect our intellectual freedom and internal motivations?

Figure 5 (Felt et al., 2018b, p. 9)

Context Card - D
Time Pressure
"Everything must be productive in a sense. And I think that can have negative consequences for research. Because maybe the quality is compromised when people have the feeling they must publish, publish quickly; I think that a lot of bad things happen then."

Figure 7 (Felt et al., 2018b, p. 12)

Context Card - B
Metrics & Quantification
"The 'currency' with which we are paid is completely weird: it's not about whether it was scientifically nice, whether it was something important in your field. Publications are the only things that count! We do so many other things — like education and supervision — but that's not considered at all. And, honestly speaking, it's not creating personal satisfaction to have a paper."

Figure 6 (Felt et al., 2018b, p. 12)

Context Card - I
Temporary Contracts
"You really get used to thinking in a project frame: what can I do to achieve something within a time frame of three or four years. You need this to be able to continue your career. It would be nice to think more about planning the scientific stuff, but the way it is right now, we're mostly planning our own future."

Figure 8 (Felt et al., 2018b, p. 14)

III. Selected cards from the Borderlands project

TRANSGRESSION - 1
Ownership of ideas
Unauthorized use and misrepresentation of authorship (plagiarism); appropriation of ideas of others

Figure 9 (Felt & Frantz, 2022b, p. 11)

TRANSGRESSION - 2
Co-authorship
Unjustified scientific (co-)authorship; claiming co-authorship of others without asking them; neglecting co-responsibility

Figure 10 (Felt & Frantz, 2022b, p. 11)

TRANSGRESSION - 6
Power abuse
Supervisors using their position to exploit young researchers; abusing position as reviewer for one's own benefit

Figure 11 (Felt & Frantz, 2022b, p. 11)

Dilemma - 3
Acknowledgement & Competition
At a conference, you hear a fascinating talk from a colleague working in the same research area as you. After the talk, there is a highly relevant discussion about potential pathways for this research. It motivates you to try out a new perspective on your own material and you want to publish the results. You are happy when you find out that you would be the first publishing on it. Still, you are not sure how you should acknowledge the fundamental ideas you got at the conference. What do you do?
A) I write a long acknowledgement thanking all the people of the discussion.
B) Nobody acknowledges all comments made at a conference by colleagues.
C) I offer one person who inspired me most co-authorship and send her the manuscript.
D) Is there another option?

Figure 12 (Felt & Frantz, 2022b, p. 13)

Dilemma - 5
Earning Co-authorship
You are writing a research paper together with another PhD and a postdoc. The writing was separated equally among the PhDs. The postdoc offered to integrate the different parts of the paper. The postdoc sends you an email that she had to do much more than she thought because your parts

Dilemma - 10
Honorary authorship
Your group is about to hand in a paper in a well-known journal. During the last year, you spent quite some time working on this experiment and you are very much looking forward to the article finally being off your desk. However, before sending the paper to the journal your PI suggests adding a further author to the paper for

didn't match up well. She thinks it's fair to list her as a first author although you did the largest share of empirical work. You do not think it is fair, but you are also not sure what would be an adequate reaction. What do you do?
A) I reply to her email that this is not what we all agreed on. I want to have a meeting and discuss it in person.
B) I write to my PhD colleague. We should agree on a first author and confront her together.
C) I write to my group leader and look for help. If he does not react, I would approach the commission for scientific integrity at our university.
D) Is there another option?

Figure 13 (Felt & Frantz, 2022b, p. 14)

strategic reasons. What do you do?
A) I respond by asking who this person is and on what basis they deserve co-authorship.
B) I accept the sudden appearance of names on our authorship list given the strategic reasons. Everybody does it because alliances are vital in science.
C) I complain about this last-minute co-authorship. I attach the University Guideline that explicitly forbids such practices.
E) Is there another option?

Figure 14 (Felt & Frantz, 2022b, p. 15)

RESEARCH CONDITIONS - 1
Lab/Group Culture
"Every lab has a different culture of how they work as a group. Some labs have a lot of meetings, co-operations and are a great place to grow into research. In others, secrecy, and pressure to perform and to spend more time in the lab are more valued. This changes your perception of what doing good research means."

Figure 15 (Felt & Frantz, 2022b, p. 16)

RESEARCH CONDITIONS - 3
Selling Research
"I have the feeling, it is no longer scientists but external actors judging on what counts as good research. Funding agencies fund the research that tells the most promising stories. Journals publish the catchiest articles. Personally, I don't agree with many of these decisions and it leads to overstating what is achieved."

Figure 16 (Felt & Frantz, 2022b, p. 16)

RESEARCH CONDITIONS - 5
Pressure & Quality
"Everything must be productive in a sense. And I think that can have negative consequences for research. Because maybe the quality is compromised when people have the feeling they must publish, publish quickly. As a consequence, we can no longer talk about the limits of our research or about what did not work so well."

Figure 17 (Felt & Frantz, 2022b, p. 16)

RESEARCH CONDITIONS - 6
Importance of Mentorship
"On my way to becoming a good researcher, scientific mentors and supervisors are central to guide my intellectual development. Their feedback is important for the quality of my work. However, due to dependency relationships and hierarchies, I cannot critique them or raise questions about their methodologies."

Figure 18 (Felt & Frantz, 2022b, p. 16)

IV. Specifically designed dilemma card

Dilemma - 1
Ranking Co-authors
After a lot of work, you are about to finish your first research paper as a PhD. How will the involved researchers be ranked on that paper? Actually, the postdoc who supported you in the lab urgently needs a publication and you feel that your supervisor also thinks that the postdoc should go first, while s/he goes last. Then there are external collaborators who — depending on your field — originally built the device you were using for your research or provided you with data which your research is based on. Yet, you think that you did the larger share of the work. For what kind of author order would you argue? And on what basis should that be decided?
A) I put myself as the first author on the paper and send it to my supervisor who can add and rank the rest of the names. B) I fear that putting the postdoc not first might have negative consequences for me and I comply. C) I ask others in the group if they had such a situation before I decide what to do. D) Is there another option?

Figure 19

V. Interview Guide Pls

Preliminary talk

- Thanking them for the opportunity to interview the person
- **Informed Consent** (2 versions; **both parties have to sign it**) Clarification of anonymity and use of the interview recording → direct quotations are anonymized in the dissertation and in any articles
- PhD project examines authorship and publication practices; as it is paramount for researchers to navigate the publication landscape and as it can be decisive for their career I want to understand how authorship and publication might impact the work and life of researchers; chose life sciences and physics as fields due to their diversity also within the discipline; adopt a person-centred perspective; adopt therefore a qualitative approach and conduct (problem-centred, narrative) interviews

Authorship and publication culture, presence in the everyday life & process of a publication and authors' list

- First, I would like to ask you to kind of look back into the beginning of your career as a researcher and describe how authorship and publications were handled in your first research group that you were a member of — maybe as a PhD student or possibly even as a master's student. So how did you 'learn' the practices, so to speak?
- Ask if necessary
 - How did you come to know how many publications you 'need' in a certain period of time?
 - How did you know in which journals you should publish?
 - And how did you, if applicable, learn which type of authorship is particularly valuable for applications?
 - Did you then experience it differently in other research groups etc. in the further course of your career?
- So, have the practices you just described to me changed over the years and, if yes, in what ways?
- In which situations and contexts are authorship and publications particularly present in your everyday work? Has the presence ever been stronger or weaker in the course of your career and why possibly? (*Dominance of metrics in general; job interviews; completion of a doctorate; application for a research project; omnipresent or particularly prominent in individual situations; more possibly before applications / appointments*)
- Does the University of Vienna as your working environment influence the presence of authorship and publications in your everyday work and, if so, to what extent?
- As I would like to learn more about the process of creating a publication and specifically crafting an authors' list, I choose a publication from your list of publications. Can you tell me something about the process of creating this publication (selection in advance) in which you are named as the author? Please describe the process step by step (questions are asked or selected based on what has already been said about the prevailing practices in order to avoid unnecessary repetition)

- What kind of contributions have the various authors made? When and how was the decision made who does what and by whom?
- Has it been negotiated, and if so, how, whether a contribution leads to authorship? Or was this (implicitly) clear from the start?
- How, when, and by whom was the order of the authors decided? Are authors usually ranked in this way in your field or, in your experience, do different positions in the list of authors have different meanings in your field, and if so, which ones? How do you know?
- Has anyone made a contribution, but is not on the list? (*bachelor's students, technicians, data collectors*) If necessary, ask: So, do you always have everyone on the paper? How is it then decided, for example, who will present at a conference? (*Proxies like conference lecture*)
- Is the process you described essentially similar to the processes of your other publications? What are possible differences and what are they based on?
- If there are traditions or (unwritten) rules in your field of research with regard to authorship and publication practices, are these reflected in the practices you have just described, for example with regard to the naming and ranking of authors? Or what are the possible differences?
- Can you think of an authorship and publication culture that works very differently and in what ways is it different? How and where did you come into contact with this? (*E.g. interdisciplinary cooperation; evaluation of applications or applications*)

Researchers' contributions and their valuation

- So, you contributed XY to the publication we discussed. Do you usually make the same kind of contributions to publications you are listed as an author? (*possibly refer to CREDIT System and its differentiation of contributions: Conceptualization; Data curation; Formal Analysis; Funding acquisition; Investigation; Methodology; Project administration; Resources; Software; Supervision; Validation: Visualization; Writing — original draft; Writing — review & editing*)
- Have the kind of contributions you make changed over the course of your career? (*E.g. PhDs often empirical / technical contributions; later on primarily conceptual contributions*)
- In your opinion, what are the most important contributions to a publication in your field and is this reflected in the authorship and, if applicable, in the order / positions of the authors? (*Comparison of the value of technical / empirical contributions to conceptual ones; collection of data by others; possibly: Do your colleagues see it similarly?*)
- In the process of creating a publication of yours which you just above, you mentioned that the doctoral students involved contributed XY. Is this how doctoral students' work on publications is usually structured, or what are their contributions? What kind of authorship does this lead to? (*Do they have their 'own' project or do several PhDs work together?*)
- What activities do postdocs usually carry out in your group and into which authoring position are they translated?
- If necessary, ask: As I can see from your list of publications, do you often work with people / groups outside of your research group, how do those collaborations work in terms of authorship?

Authorship as a negotiation process

- Authorship does often come with value in academia, so discussions and conflicts can arise or the topic is sometimes a kind of taboo. How do you experience that?
- Can you remember a situation in which there were particularly intensive negotiation processes? In your experience, which arguments are used to claim authorship and / or a certain position in such discussions?
- Can you remember a publication on which you were not named, even though you thought you deserved the authorship? Or have you ever been listed as an author without your knowledge or even though you actually didn't want to be?

Role as supervisor or group leader

- You also supervise doctoral students carrying out their PhD project. What does a dissertation look like in your department? Does the format of the dissertation possibly affect authorship and publication practices within your research group, and if so, to what extent? (*Presence in everyday work; doctoral students may feel under pressure; need a certain number of first authors*)
- How do doctoral students or new group members learn / experience how authorship and publication are handled in your group?
- And how do you suspect the doctoral students in your research group and / or your discipline experience the prevailing authorship and publication practices?
- To what extent do you as a supervisor support your PhDs with regard to authorship and publication? (*compensatory authorship for risky projects; distribution of tasks*)
- Are there aspects that are particularly important to you in conveying to doctoral students with regard to authorship and publication? (*Values (orders); research integrity; presentation of results; inclusion of technicians on a list of authors, etc.*)
- And with regard to your postdocs and their publication output? Do you support them and if so, in what way?
- There are more and more doctoral schools at the University of Vienna - also in your research area. You are even a regular member of the Vienna Doctoral School of XY. Can you maybe tell me a little bit about it, especially with regard to authorship and publications, i.e. graduation requirements? Were there, for example, discussions about the number of publications? (*Formalization of the responsibility of supervisors; see guidelines*)

Visions and Practices of Care and Responsibility

- If you are the (co-)author of a publication, do you feel responsible for it and, if so, for what specifically? How do you experience that with others? (*Responsibility for the entire publication, for one's own contribution or that, for example, everyone is named, etc.*)
- Do you see problematic aspects in the authorship and publication practices that you encounter in your everyday work? And if so, which ones? To what extent do these practices also have a positive or negative effect on the generation of new knowledge or innovations? (*Discussions on bordering or transgressing practices; increase in guidelines; salami slicing; honorary authorship; pressure to publish*)
- As part of the discussion groups with young researchers we conducted, they often discussed the fairness of authorship and publication practices. Have you ever encountered situations in which you or anyone else discussed or criticized the

fairness of the practices and, if so, why? (*Possibly: In the discussion groups, several doctoral students have argued that they don't think it is fair not to be named as an author when data is no longer relevant for the argumentation of the article. How do you handle this and what is your opinion on this?*)

- If necessary: Fairness seems to be a concern to you, how do you try to guarantee this in the context of authorship and publication?
- Are you familiar with guidelines on authorship and how relevant are they in your everyday work? (*possible discrepancy between practices and rules; possibly brief discussion on Regulations of GSP of the University of Vienna*)

Final Questions:

- Would you like to add something else?
- Thanking them for the interview and asking whether I can contact them again if I have any unanswered questions

VI. Interview Guide Postdocs

Preliminary talk

- Thanking them for the opportunity to interview the person
- **Informed Consent** (2 versions; **both parties have to sign it**) Clarification of anonymity and use of the interview recording → direct quotations are anonymized in the dissertation and in any articles
- PhD project examines authorship and publication practices; as it is paramount for researchers to navigate the publication landscape and as it can be decisive for their career I want to understand how authorship and publication might impact the work and life of researchers; chose life sciences and physics as fields due to their diversity also within the discipline; adopt a person-centred perspective; adopt therefore a qualitative approach and conduct (problem-centred, narrative) interviews

Authorship and publication culture, presence in the everyday life & process of a publication and authors' list

- First, I would like to ask you to kind of look back into the beginning of your career as a researcher and describe how authorship and publications were handled in your first research group that you were a member of — maybe as a PhD student or possibly even as a master's student. So how did you 'learn' the practices, so to speak?
- Ask if necessary
 - What did a dissertation look like for you (monograph; paper-based)?
 - How did you come to know how many publications you 'need' in a certain period of time?
 - How did you know in which journals you should publish?
 - And how did you, if applicable, learn which type of authorship is particularly valuable for applications for grants or jobs?
 - Did you then experience it differently in other research groups etc. in the course of your career so far?
- So, have the practices you just described to me changed over the years and if yes, in what ways?
- In which situations and contexts are authorship and publications particularly present in your everyday work? Has the presence ever been stronger or weaker in the course of your career and why possibly? (*Dominance of metrics in general; job interviews; completion of a doctorate; application for a research project; omnipresent or particularly prominent in individual situations; more possibly before applications / appointments*)
- Does the University of Vienna as your working environment influence the presence of authorship and publications in your everyday work and, if so, to what extent?
- As I would like to learn more about the process of creating a publication and specifically crafting an authors' list, I choose a publication from your list of publications. Can you tell me something about the process of creating this publication (selection in advance) in which you are named as the author? Please describe the process step by step (questions are asked or selected based on what has already been said about the prevailing practices in order to avoid unnecessary repetition)
 - What kind of contributions have the various authors made? When and how was the decision made who does what and by whom?

- Has it been negotiated, and if so, how, whether a contribution leads to authorship? Or was this (implicitly) clear from the start?
- How, when and by whom was the order of the authors decided? Are authors usually ranked in this way in your field or, in your experience, do different positions in the list of authors have different meanings in your field, and if so, which ones? How do you know?
- Has anyone made a contribution, but is not on the list? (*bachelor's students, technicians, data collectors*) If necessary, ask: So, do you always have everyone on the paper? How is it then decided, for example, who will present at a conference? (*Proxies like conference lecture*)
- Is the process you described essentially similar to the processes of your other publications? What are possible differences and what are they based on? (Possibly: *Could you name one of your publications where the process was different?*)
- If there are traditions or (unwritten) rules in your field of research with regard to authorship and publication practices, are these reflected in the practices you have just described, for example with regard to the naming and ranking of authors? Or what are the possible differences?
- Can you think of an authorship and publication culture that works very differently and in what ways is it different? How and where did you come into contact with this? (*E.g. interdisciplinary cooperation; evaluation of applications or applications*)

Contributions of researchers and their evaluation

- So, you contributed XY to the publication we discussed. Do you usually make the same kind of contributions to publications you are listed as an author? (*possibly refer to CREDIT System and its differentiation of contributions: Conceptualization; Data curation; Formal Analysis; Funding acquisition; Investigation; Methodology; Project administration; Resources; Software; Supervision; Validation; Visualization; Writing — original draft; Writing — review & editing*)
- Have the kind of contributions you make changed over the course of your career so far? (*E.g. PhDs often empirical / technical contributions; later on primarily conceptual contributions; especially postdocs kind of in-between*)
- In your opinion, what are the most important contributions to a publication in your field and is this reflected in the authorship and, if applicable, in the order / positions of the authors? (*Comparison of the value of technical / empirical contributions to conceptual ones; collection of data by others; possibly: Do your colleagues see this similar?*)
- In the process of creating a publication of yours which you just above, you mentioned that the doctoral students involved contributed XY. Is this how doctoral students' work on publications is usually structured, or what are their contributions? What kind of authorship does this lead to? (*Do they have their 'own' project or do several PhDs work together and what is the role of postdocs and senior scientists?*)
- What activities do the group leaders usually carry out in your group and into which author position are they translated?
- If necessary, ask: As I can see from your list of publications, you do often work with people/groups outside of your research group, how do those collaboration work in terms of authorship?

Authorship as a negotiation process

- Authorship does often come with value in academia, so discussions and conflicts can arise or the topic is sometimes a kind of taboo. How do you experience that?
- Can you remember a situation in which there were particularly intensive negotiation processes? In your experience, which arguments are used to claim authorship and / or a certain position in such discussions?
- Can you remember a publication on which you were not named, even though you thought you deserved the authorship? Or have you ever been listed as an author without your knowledge or even though you actually didn't want to be?

Role as postdoc or senior scientist

- Based on my data analysed so far, I have the impression that often PhD students are first authors and group leaders are last authors, which is often described as the most valuable positions. How is/was this handled in your previous and current work environments? *(If necessary, ask: Do you find it difficult to be named as first or last author on publications?)*
- Postdocs and senior scientists often take over the supervision of PhD students — especially in the lab, but also in the process of publication. What is this like for you? Is this form of supervision or support, for example in the form of authorship, valued or recognized?
- How do you learn/experience how authorship and publication are handled in a new research group?
- From your perspective, how do PhD students or new group members learn/experience how authorship and publication are handled in the current group? Do the PhDs have conversations with you as a postdoc or senior scientist about this, for example? And how do you think PhD students in your research group and/or discipline experience the prevailing authorship and publication practices?
- Are there aspects that are particularly important to you to convey to doctoral students with regard to authorship and publication? *(Values/orders; research integrity; presentation of results; inclusion of technicians* on the list of authors, etc.).*
- Do you experience support from the group leader with regard to your publication output, and if so, in what ways? Or from other people in your work environment?

Visions and Practices of Care and Responsibility

- If you are the (co-)author of a publication, do you feel responsible for it and, if so, for what specifically? How do you experience that with others? *(Responsibility for the entire publication, for one's own contribution or that, for example, everyone is named, etc.)*
- Would you say that postdocs with temporary contracts in particular are under a lot of pressure to generate publications?
- Do you see problematic aspects in the authorship and publication practices that you encounter in your everyday work? And if so, which ones? To what extent do these practices also have a positive or negative effect on the generation of new knowledge or innovations? *(Discussions on bordering or transgressing practices; increase in guidelines; salami slicing; honorary authorship; pressure to publish)*
- As part of the discussion groups with young researchers we conducted, they often discussed the fairness of authorship and publication practices. Have you ever

encountered situations in which you or anyone else discussed or criticized the fairness of the practices and, if so, why? (*Possibly: In the discussion groups, several doctoral students have argued that they don't think it is fair not to be named as an author when data is no longer relevant for the argumentation of the article. How do you handle this and what is your opinion on this?*)

- If necessary: Fairness seems to be a concern to you, how do you try to guarantee this in the context of authorship and publication?
- Are you familiar with guidelines on authorship and how relevant are they in your everyday work? (*possible discrepancy between practices and rules; possibly brief discussion on Regulations of GSP of the University of Vienna*)

Final Questions:

- Would you like to add something else?
- Thanking them for the interview and asking whether I can contact them again if I have any unanswered questions

VII. Original version of the German quotations

- 1 "das war fast ein bisschen ungerecht im Nachhinein gesehen, der Person gegenüber, also dem PhD gegenüber [...] und da habe ich ihn dann als Ersten, sozusagen als Ausgleich" (VU, PI3f, p. 13)
- 2 "waren die Letztautoren halt auch schon inhaltlich beteiligt. Also nicht so wie in der Medizin, das ist der Institutsdirektor, deswegen steht er hinten" (VU, PDoc4m, p. 15)
- 3 "Und so lange wir Impact-Punkte auf oder Ressourcen auf Impact-Punkte vergeben, sind wir fehlgeleitet. Weil es ist ein Unterschied, ob ich jetzt in Molekularbiologie oder in Stammzellen bin, wo ich jeden Blödsinn mit dem höchsten Impact publizieren kann, oder ob ich in einer soliden, biochemischen Wissenschaft bin, wo ich nur mit einem Zehntel des Impacts publizieren kann und dann deswegen rausgeschmissen werde" (II, PI20m, p. 40)
- 4 "dass man ja publizieren muss, weil also nicht jetzt, um was zu erreichen, das ist jetzt gar nicht der Punkt aber. Das, was macht er denn, die Wissenschaft letztlich aus. Irgendetwas zu lernen und muss man weitergeben, wenn man es nicht weitergibt, macht es keinen Sinn, ja. Wirklich nicht." (VU, PI5m, p. 5)
- 5 "irgendwas sein, was die Person gemacht hat, was ich auch nicht kann oder mein Lab nicht kann, ja, weil also manchmal habe ich eben Statistiker dabei irgendwo, also auch einen Kollegen, der hat zum Beispiel ein extra Programm für uns geschrieben, dass das irgendwie funktioniert, also für den ist das vielleicht Handwerk, nicht, weil der kann das halt." (VU, PI3f, p. 12)
- 6 "das Schöne an Physik ist wissenschaftliche Zusammenarbeit" (VU, PI2m, p. 10)
- 7 "ich bin an einem ganz anderen Punkt oder vielleicht sowieso werde ich das bleiben, aber das muss man dann halt einfach akzeptieren. [...] werde ich in Zukunft halt daraus lernen und das sicher nicht aktiv versuchen, noch mal was gemeinsam zu machen" (VU, PDoc7f, p. 16)
- 8 "aber jetzt nie so wirkliche schwerwiegende, das ist irgendwie alles selbstverständlich" (II, PI15m, p. 11)
- 9 "aber ich bin da eher derjenige, dass wenn jetzt um Kooperationsprojekte geht, dass ich da weniger, eher mich da zurücknehme, weil's mir mehr Wert ist eine Kooperation einzugehen, als dass ich mich darauf beharre — aber wahrscheinlich, ich kann's mir leisten, wahrscheinlich andere nicht" (II, PI15m, p. 11)
- 10 "guter Kooperationspartner, der sich hier so nicht um die wichtigsten Positionen reißt, um sozusagen Kooperationen an sich zu ermöglichen" (II, PI15m, p. 11)
- 11 "indem ich eigentlich sage: egal, wer was gemacht hat — die kommt mit aufs Paper" (II, PI7m, p. 30)
- 12 "eigentlich immer klar" (VU, PDoc5f, p. 2)
- 13 "Ich helfe gerne, wenn Fragen sind, aber selbst jetzt länger stehen und arbeiten eher nicht" (VU, PDoc7f, p. 12)
- 14 "extrem personenbezogene Sachen" (VU, PDoc2f, p. 6)
- 15 "Beitrag" (VU, PDoc5f, p. 3)
- 16 "Wir haben die Methoden entwickelt, wir machen die Analysen, wir werten aus, wir schauen, dass das alles passt und helfen ihnen bei der Interpretation. Dafür müssen wir drauf stehen auf einem Paper, wenn wir nicht draufstehen, nicht ich jetzt persönlich, es können auch meine Mitarbeiter sein, je nachdem, wer es halt macht und sich drum kümmert. Aber das muss so sein. Weil sonst gibst du es dir nicht mehr. Und das hat schon auch geholfen, dass wir uns dann entwickelt haben und ja muss nicht jeder bei uns messen, ist aber auch okay. (VU, PI5m, p. 17)
- 17 "Das kann man jetzt auch diskutieren, ob es sinnvoll ist oder nicht. Ich habe etwas gemacht fürs Paper, es hat nicht funktioniert und man sieht überhaupt nichts davon im Manuskript und dann steht man trotzdem drauf." (VU, PDoc4m, p. 1)
- 18 "wie viel hast du beigetragen" (VU, PDoc4m, p. 1)
- 19 "eine Selbstverständlichkeit" (VU, PDoc5f, p. 5)
- 20 "Erkenntnisgewinn" (VU, PDoc5f, p. 6)
- 21 "wir sagen immer du musst das Paper gelesen haben, das ist klar, du musst sozusagen, auch wenn du nur, weiß nicht, im Labor geholfen hast " (VU, PI5m, p. 13)
- 22 "tut so als würde man Wissenschaft nur der Wissenschaft wegen machen, aber es ist ja trotzdem ein Beruf" (VU, PDoc4m, p. 4).
- 23 "ich habe nie irgendwie geschaut drauf, wie viel die Leute arbeiten, das war mir wurst, weil ich immer gesagt habe, ihr müsst eure Diss machen, da kommen wir dann auch wieder zu Publikationen, weil das was am Ende zählt, ist das, was ihr produziert, was der Output ist, und nicht, dass ihr da wart" (VU, PI5m, p. 1)
- 24 "Es wird auch ohne dem gehen und du machst eh super Sachen. Beruhige dich" (VU, PI5m, p. 3)
- 25 "Kulturwandel" (VU, PI5m, p. 2)
- 26 "Wir haben eine völlig fehlgeleitete Kultur, was das betrifft — also in den Biowissenschaften. [...] Bei uns kommt es zu Kriegen, zu Bruch von Freundschaften, zu Karriereende, zu-. Weil eben, und das war ein anderer, ist ein anderer Fehler, den Unis machen, weil sie nur schauen: wer ist Erstautor wo, und wer ist Letztautor. Das betrifft Gruppenleiter, das betrifft Karriere oder junge Leute — wenn die ihr Erstautor- Paper nicht haben, kriegen die kein Fellowship. Aus,

die kriegen kein EMBO Fellowship, kein Human Frontiers, kein BBSCR, kein Wellcome Trust — absolut essentiell, ja? Und wir müssen uns da irgendwie eine andere Kultur überlegen, wie das, wie das funktioniert. Es kann so nicht funktionieren, ja, weil es einfach so kompetitiv geworden ist und so viele geworden sind, dass es nicht mehr geht. Und nur, weil jemand kein Erstautor-Paper hat, kann durchaus gerechtfertigt sein, dass er nicht in die Karriere kommt, aber kann auch falsch sein.“ (II, PI20m, p. 43-44)

27 “braucht es nicht“ (VU, PDoc2f, p. 4)

28 “es spielt überhaupt keine Rolle für ihn, wo er steht“ (VU, PDoc4m, p. 6)

29 “er hat auch etwas, was was bringt. Weil das sind ja die drei Dinge, die zählen, Corresponding Author, Senior Author oder die Erstautoren“ (VU, PI5m, p. 12)

30 “die ganzen nervigen E-Mails bekommen“ (VU, PDoc4m, p. 6)

31 “Ich weiß nicht, ich hatte nicht den Eindruck, dass es bei uns eine große Rolle spielt, sondern eher dass das Hintenstehen wichtiger ist, als wer jetzt corresponding ist. Ich habe zumindest nie groß mitgekriegt, dass das jemanden groß interessiert“ (VU, PDoc4m, p. 6)

32 “gut aufgesetzt und lässt auch ganz klar durchblicken, wer hat betreut, wer hat die harte Arbeit gemacht“ (VU, PI2m, p. 3)

33 “Ehrlich gesagt, der erste Autor, der corresponding und der Last Author wird sich schon überlegt, aber der Rest ist vollkommen wurst. Das ist vollkommen egal, weil ehrlich gesagt, ob du jetzt an vierter, fünfter oder dritter Stelle stehst“ (VU, PI5m, p. 17)

34 “Ich weiß halt nicht, ob es mir als Postdoc nicht mehr bringt, Erstautor zu sein, weil das wichtiger wäre für die Karriere. Keine Ahnung. Letztautor wäre wenn dann, wenn man Leute betreut. Habe ich noch nicht so viel darüber nachgedacht, weil es nicht so relevant war. Ich würde halt denken, dass Erstautorpublikationen für mich im Moment wichtiger wären.“ (VU, PDoc4m, p. 12)

35 “wenn ich Erstautor bin, ist mir relativ egal, wer noch in der Mitte dazwischensteht“ (VU, PDoc4m, p. 6)

36 “meistens logisch, dass zwei Leute involviert sind, nämlich der, der der Chef ist, der das organisiert und so weiter, und einer, der sitzt und pipettiert“ (VU, PDoc7f, p. 8)

37 “ist so der Post-Doc meistens, der mehrere Projekte unterstützt und deswegen auch mehrere Papers erntet, aber aus einer mittleren Position heraus“ (VU, PI2m, p. 12)

38 “illusorisch“ (VU, PDoc4m, p. 15)

39 “das Ganze versucht, neutral zu sehen, [...] keine falschen Eitelkeiten aufkommen, dann ist das eine normale Sache, dass dann auch Jüngere Letztautoren sein können, wo man sagt, das war ganz klar Schwerpunkt von denen.“ (VU, PI2m, p. 14)

40 “in unserem Umfeld hier, das ich so kenne, schon darauf geschaut wird, dass die Positionen verdient sind, wo man steht“ (VU, PI2m, p. 14)

41 “bisschen albern“ (VU, PDoc4m, p. 10)

42 “wer ist wirklich verantwortlich“ (II, PI17m, p. 16)

43 “Und nichts davon in dem Prozess darf schlampad oder darf schlecht gemacht sein oder nicht in Ordnung sein oder so. Das ist einfach unsere Währung, das ist die Glaubwürdigkeit auch von mir. Und ich sage ihnen, ich möchte nicht, dass meine Glaubwürdigkeit durch Schnitzer, die ihr macht, erschüttert wird. Also ganz sicher nicht. Weil ich habe auch nur die Glaubwürdigkeit.“ (VU, PI5m, p. 20)

44 “eigentlich schon sehr früh sehr selbstständig war, das aber nicht formal eingefordert habe. Was ich im Nachhinein bereue, aber nachher ist man immer gescheiter“ (VU, PDoc7f, p. 5)

45 “Bewusstsein heraus, dass ich das notwendiger brauche, als sie, die in Pension geht, und dass es sachlich natürlich gerechtfertigt ist“ (VU, PDoc7f, p. 5)

46 “Glück“ (VU, PI2m, p. 14)

47 “hat viel geholfen bei Forschungsanträgen oder sonstigen Sachen, schon in jungen Jahren, Letztautor zu sein“ (VU, PI2m, p. 14)

48 “wenn ein Dissertant dabei ist, dann ist es normalerweise so, dass der Dissertant immer, wenn das so seine Arbeit ist, an erster Stelle steht. Unabhängig davon, ob er wirklich die meiste Arbeit gemacht hat. Das ist ein bisschen unfair, nicht, also ich bin nie an erster Stelle“ (VU, PI5m, p. 11)

49 “schwach waren, im Formulieren und im Verfassen, manchmal war sehr viel Zusatzarbeit notwendig, wo wir das dann aber trotzdem nicht eingefordert haben. Lernprozess, ja. Das würde ich vielleicht heute auch anders machen. War dann trotzdem quasi der PhD vorne, weil er halt auch quasi die Versuche strukturiert und so weiter. Da ist eine Unschärfe. Da kann man diskutieren, ob das fair ist oder ob der dann woanders hin wandert“ (VU, PDoc7f, p. 8)

50 “Ich mache das [, sich selbst als Erstautor aufs Paper schreiben,] grundsätzlich nicht, weil das tun auch Senior Author, das tue ich aber nicht, weil ich halte das für einen Schwachsinn, ich muss nicht an erster Stelle stehen. Auch nicht, wenn ich viel dafür gemacht habe, das finde ich, sollte man auf jeden Fall den Jüngeren überlassen, aber für die Postdocs ist es oft eine Schwierigkeit, weil in einem Projekt in dem Postdoc und ein Doktorand arbeitet, immer der Doktorand, weil er es braucht, steht. Also das ist ein bisschen schwierig manchmal finde ich ehrlich gesagt. Weil ich finde, dass die Postdocs oft mehr verdient hätten, als die Doktoranden, da an erster Stelle zu sein. Aber...Es ist immer schwierig, weißt du, ich meine, was ist schon sozusagen, wirklich eine Erstautorenschaft?

- 51 "dass der Post-Doc nicht degradiert wird zur Hilfe [...] der hat schon ein Programm, wo er als Erster ist und wo dann andere zum Beispiel in der Mitte sind und ich hinten" (VU, PI3f, p. 16)
- 52 "wird dann irgendwann albern zu sagen, der eine ist jetzt der Wichtigste" (VU, PDoc4m, p. 10)
- 53 "sozusagen synonym mit der Erst-Autorschaft" (VU, PDoc7f, p. 2)
- 54 "die Corresponding-Autorschaft sollte die Person haben, die die Idee hatte, die die Struktur sich überlegt hat, das organisatorische Netzwerk aufgezogen hat, um die verschiedenen, bei uns sind es ja oft wirklich verschiedene Arbeitsbereiche, die verschiedene Techniken beisteuern. Das sollte die Person sein, wo die Fäden zusammenfließen, die ich frage, wenn ich eine thematische Frage habe, nicht. Also, das das wäre mein Verständnis von dieser Position. Es würde mich sehr interessieren, ob das durchaus anders gelebt wird" (VU, PDoc7f, p. 2)
- 55 "Ich habe erfahren, dass bei manchen anderen Zweigen, auch zum Teil in der Physik, sehr wohl wertgeschätzt, darauf geschaut wird, wer ist corresponding author, weil es zwischen den Zeilen heißt, der wirklich was gemacht hat, und einmal herauszupicken von der Reihenfolge. Als persönlicher Touch handhabte ich das nicht so, also für mich hat das relativ wenig Wichtigkeit, who is corresponding author, der ist für mich echt der, wo man sagt, wenn eine Anfrage ist, von einem Journal, dann bitte habe Du Rede und Antwort, OK. Aber ich muss zugeben, ich bin vielleicht nicht repräsentativ für das, wie es die Kollegen machen oder wer vielleicht schaut, wo ist Sternchen mit corresponding author." (VU, PI2m, p. 8)
- 56 "da freue ich mich, wenn ich ein Erstautorenpaper habe." (VU, PDoc4m, p. 13)
- 57 "Gefühl" (VU, PDoc5f, p. 4)
- 58 "weil ich das Gefühl hatte, dass wir gleichermaßen beigetragen haben, rein von dem, was geschrieben wurde und an wissenschaftlichen Input, was in dieser Publikation vorhanden ist, dass ich das Gefühl hatte, ich möchte das gleich bewerten" (VU, PDoc5f, p. 4)
- 59 "eigentlich selbsterklärend" (VU, PDoc5f, p. 10)
- 60 "dann ist klar, der PHD, der sich reinkniet, ist der Erste und wenn der Post-Doc sich reinkniet, ist das sein Baby, dann kann er auch Erster sein, das ist absolut gerechtfertigt" (VU, PI2m, p. 13)
- 61 "also ein Paper schreiben für jemanden, kompletto, und dann als Erstautor draufgeben.... will ich eigentlich auch so nicht machen, nicht, weil das ist mir zu viel Arbeit für das und dann würde das das auch nicht rechtfertigen, weil dann ist ja die Person eigentlich am wichtigsten Prozess, nämlich der Interpretation der Daten, und dem Ganzen überhaupt nimmer dabei" (VU, PI3f, p. 15)
- 62 "Besonderheit" (VU, PDoc5f, 1)
- 63 "Man wäre gerne jemand, dem es völlig egal ist, aber man lebt halt nicht in einer Welt, wo es völlig egal ist." (VU, PDoc4m, p. 11)
- 64 "Autorschaft im Moment ist relativ wenig präsent" (VU, PDoc2f, p. 2)
- 65 "inflationär" (VU, PDoc4m, p. 1)
- 66 "An sich sind wir ganz normale Bürger. Wir sind nicht besser, nicht schlechter — man findet alle menschlichen traits bei Wissenschaftlern genauso. Wir sind auch Egoisten und wir, auch positiv, und haben unsere Ideale oder auch, aber es ist eben, ja — sind halt Menschen, ne?" (II, PI7m, p. 3)
- 67 "Jeder sagt halt von sich selbst, seine Arbeit war wichtiger" (VU, PDoc4m, p. 10)
- 68 "Die Leute, die beitragen, die sollten wir fördern und die Leute, die sich verweigern oder nur an sich denken, die sollten wir eliminieren — das ist ganz, so, so versuche ich meine Gruppe zu halten oder mit meiner Gruppe zu interagieren" (II, PI7m, p. 5)
- 69 "die vielleicht weniger bescheiden waren, aber dann der Meinung waren: 'Hey, ich will auch.'" (VU, PDoc7f, p. 6-7)
- 70 "ich war dann grundsätzlich aber immer sehr diplomatisch und mir war die Stimmung in der Gruppe aber wichtig, sodass ich dann oft mitunter da Leuten etwas zugestanden habe" (VU, PDoc7f, p. 7)
- 71 "aber gerade die Wissenschaft auf hohem Niveau oder auf höchstem Niveau braucht einen gewissen Bösartigkeitsfaktor — ich sage es nicht ganz, ganz böse — und Frauen, üblicherweise meiner Erfahrung nach, sind nicht bereit, wirklich sich wie (..), ich sollte es nicht sagen, aber die sind nicht bereit, sich unkollegial oder sich aggressiv oder überaggressiv zu verhalten. Viele Frauen, die sehr erfolgreich sind, sind das aber — die haben diesen, diesen gewissen Ellbogen- Check, die Technik herausen und die machen es auch" (II, PI20m, p. 12)
- 72 "Position ist im Prinzip da, wo sie hingehört" (VU, PI2m, p. 11)
- 73 "wenn einer irgendwie ein sehr großes Ego hat und unglücklich ist [...], kann ich mir vorstellen, dass das schwierig ist, manchmal verdient die Position zu bekommen, die er gerne hätte" (VU, PI2m, p. 14)
- 74 "ich möchte aber auf keinen Fall die Leistungen dann von meinen Mitarbeitern, die die Experimente selbst gemacht haben, in dem Sinne schmälern, dass ich sage, ich schreibe aber die Publikation. Das fände ich persönlich extrem unfair und hätte auch für mich, wenn ich mich zurückerinnere, auch meine Motivation in irgendeiner Form geschmälert, weil ich mir denke, dann bin ich nicht mehr Erstautor, wenn ich die Arbeit gemacht habe. Das möchte ich auf keinen Fall" (VU, PDoc5f, p. 10)
- 75 Eigentlich hat der Gruppenleiter jetzt aktiv nie in irgendeiner Form darauf bestanden oder so, sondern es hat sich bei uns einfach so ergeben, dass wir das

halt so machen. Ich glaube, wir geben das dann in irgendeiner Form an unsere Doktoranden weiter, die machen das dann halt auch, die wiederum an die Masterstudenten, die machen das dann halt auch. Also ich glaube, das war für uns halt relativ klar. (VU, PDoc5f, p. 11)

76 "eine gewisse Unschärfe" (VU, PDoc7f, p. 8)

77 "das habe ich nur so kennengelernt, hätte ich jetzt nicht hinterfragt" (VU, PDoc7f, p. 8)

78 "Es gab niemanden, der mir gesagt hätte, was üblich ist, welchen Stellenwert, welche Position hat und wie das überhaupt funktioniert, diese Strukturen. Also es gab keine Schulung in diese Richtung" (VU, PDoc7f, p. 1)

79 "Etikette" (VU, PI2m, p. 3)

80 "durch einfach Sehen und Erlernen und Schauen, wie tun die das dort" (VU, PI3f, p. 3)

81 "Erfahrungsschätzen" (VU, PI2m, p. 3)

82 "Dass die Letzt-Autorschaft sozusagen, ich sage es jetzt salopp, der Head Honcho ist, ja, das, das hat man sich im PhD humorvoll selber angeeignet, sei es auch nur durch Comics. Also das war schon klar" (VU, PDoc7f, p. 1-2)

83 "es ist ein etabliertes System, da macht es nicht viel Sinn, gegen den Strom zu schwimmen, aber das ist nicht notwendig" (VU, PI2m, p. 3)

84 "ich kenne das vom Hörensagen von anderen, wo de facto jemand darauf geschrieben wird, der Null damit zu tun hat, überhaupt kein Thema nicht. Also das war bei uns immer korrekt. Es ist so gut wie, so gut wie nie jemand darauf gestanden, der nichts getan hat, also so wie ich es erlebt habe in meinem, auf einem Weg quasi. Und das ist auch etwas, was ich nie anders handhaben würde, weil, dann fühlt es sich unwohl an sozusagen" (VU, PDoc7f, p. 5-6)

85 "sammle ich halt jetzt erst, wo ich wirklich quasi die Fäden mehr ziehe" (VU, PDoc7f, p. 7)

86 "Und dann kommt dieses Dilemma, eigentlich habe ich viel gemacht, ich müsste fast an erster Stelle sein, aber ich bin der Betreuer, da sollte, also das passt nicht, es sollte eher der, der viel lernen musste, aber doch viel gemacht hat, an erster Stelle stehen, also von Erstem auf Letzten war ein Sprung, der nicht immer ganz für mich leicht war, aber man muss dann sagen, OK, die anderen sind auch verdient vorne und dann ist es irgendwie klar, dass das so sein sollte." (VU, PI2m, p. 4)

87 "Ich war aber wahrscheinlich auch nicht so kritisch früher, wie ich es jetzt bin. Ich habe jetzt sicher pointiertere Vorstellungen vom Leben und von der Wissenschaft nämlich und kann die jetzt vielleicht auch anders artikulieren" (VU, PI5m, p. 16)

88 "Das macht dann halt den Eindruck, es geht mir nur darum, auf der Autorenliste zu stehen und es würde mich inhaltlich weniger interessieren. Und dieses rüberbringen, ich finde es inhaltlich total interessant, aber meine Zeit ist limitiert und ich muss jetzt schon wissen, wie sehr ich dafür dann belohnt werde, das ist halt schwierig zu formulieren." (VU, PDoc4m, p. 11)

89 "man muss es einfach üben, man muss es regelmäßig machen, damit die Leute irgendwie auch den, die Furcht ablegen, da was Falsches zu sagen" (II, PI8m, p. 21)

90 "es ist nichts in Stein gemeißelt, aber es sollte halt eine Diskussion sein" (II, PI8m, p. 21)

91 "Jeder sollte am Ende idealerweise damit zufrieden sein, wo er steht auf einer Veröffentlichung und, und auch das Gefühl haben: okay, ich werde hier geschätzt, ja" (II, PI8m, p. 21)

92 "wo man dann halt den Eindruck hatte, da ist die Contribution nicht immer gleich viel, aber man kommt einmal rauf aufs Paper und da ändert sich dann nichts mehr dran." (VU, PDoc4m, p. 1)

93 "Also eigentlich bespricht man es vorher, natürlich es ist immer so, ob eine Publikation dann ganz genauso erscheint, wie man es sich gedacht hat, weiß man nicht. Und oft hat man dann so viele, dass man sagt, jetzt machen wir zwei draus. Oder das ist nicht genug, hauen wir es zusammen oder so was. Das passiert natürlich, also hundertprozentig kann man es sich am Anfang nicht ausmachen. Aber normalerweise schon und ich schaue schon ganz bewusst drauf, wenn ich Dissertanten habe, dann versucht man immer von Anfang an, also ich zumindest versuche immer von Anfang an das in Papers zu denken, in Manuskripten zu denken. Und weiß, diese Manuskripte müssen die dann als Erstautoren haben. Das heißt, es ist von Anfang an klar, dass die Postdocs, die mitarbeiten halt was Eigenes brauchen." (VU, PI5m, p. 12)

94 "Das ist wahrscheinlich der müde Versuch, anderen Fehler zu ersparen, oder Lernprozesse, die für einen mühsam waren, ist wahrscheinlich nur bedingt hilfreich, weil man einfach gewisse Dinge selbst lernen muss. Aber ich denke mir grundsätzlich ein bisschen mehr kommunizieren ist besser als zu wenig" (VU, PDoc7f, p. 16)

95 "natürlich wichtig von Anfang an klar zu kommunizieren: wer hat welches Projekt und wer trägt zu Projekten bei" (II, PI17m, p. 16)

96 "Also mir ist das eigentlich nicht passiert, aber ich habe es irgendwie miterlebt, glaube ich, und deswegen habe ich so das Gefühl gehabt, vielleicht haben mir das auch meine Mentoren oder was auch immer gesagt, irgendwie mache dir das gut aus, weil wenn man sagt, wir machen das zusammen und auf einmal bist du da irgendwo in der Mitte und die sind vorne und hinten, also das kann einem relativ leicht irgendwie passieren, wenn es nicht klar ist" (VU, PI3f, p. 20)

97 "Sturm und Drang" (VU, PI2m, p. 20)

98 "wir wollen ja idealerweise die Leute mit der, mit der größten Kreativität und dem größten Eifer, ihre Karriere zu, also in Sachen Forschung zu verfolgen.

Die will man ja möglichst eigentlich weiterbringen und denen die Möglichkeiten schaffen“ (II, PI7m, p. 8)

99 “dieses ganze Angestellten-Denken ist glaube ich in der Wissenschaft völlig unangebracht. Wir sind alle hier Selbstständige. Die unsere Karrieren entwickeln und unsere Forschung weiterr-, vorantreiben. [...] Das sind halt Leute, die dann irgendwie ihre festen Stellen haben, um vier Uhr nach Hause gehen und denken: so sollte es sein; ja, das ist auch schön und es ist definitiv familienfreundlicher — manchmal wünsche ich mir das auch — aber es ist halt in der Wissenschaft nicht so, das ist nicht der Job. Deswegen sage ich ja auch wir reden hier von Hobby — das ist kein Job, das ist keine Arbeit, wo man, wie Verkäufer ist oder so, wo man sagt: okay, ich, ja? — wir machen das wirklich entweder freiwillig, oder man soll es nicht machen.“ (II, PI7m, p. 25)

100 “jeder hat einen anderen Antrieb. Der eine möchte Ruhm, der andere möchte reich werden und der dritte möchte irgendwie die Menschheit retten [...] und dann gibt's noch 200.000 andere Gründe, warum man diese Karriere angeht, da reingekommen ist“ (II, PI7m, p. 5)

101 “Also es gibt schon Leute und das ist in dieser, also das ist im Laufe der Jahre ärger geworden, in dieser Generation überhaupt arg teilweise, also die, dass die Leute irgendwie so, wie soll ich sagen, so einen geradlinigen Blick in ihrem, in ihre Zukunft und Karriere haben und irgendwie relativ schnell genau wissen, was sie müssen, machen müssen, nicht, also möglichst viele Papers möglichst, also und wo es irgendwie um das geht, also wie soll ich sagen, wo man das Gefühl hat, manchmal es geht, es geht mehr um die Papers als um die Wissenschaft“ (VU, PI3f, p. 10-11)

102 “taktisch [...] Was die Leute heute machen, ist, sie schauen, wie sie mit dem geringsten Aufwand den größten Erfolg haben. Das ist keine Strategie, das ist eine Taktik“ (VU, PI5m, p. 2)

103 “Aber dieser Wahnsinn, nicht, also wirklich Wahnsinn irgendwie, wie viele Papers hast Du im Jahr und was tust Du und, ah, das ist für Junge schlimm und das ist überhaupt idiotisch finde ich“ (VU, PI3f, p. 26)

104 “versuche da schon relativ großzügig zu sein, weil es ja auch Zeit ist, was das die gekostet hat und ihnen dann auch letztendlich natürlich hilft und mir ist es ja wurscht, ob ich alleine da bin (lacht) oder nicht“ (VU, PI3f, p. 13)

105 “die Breite von Leuten, die sich selbst absolut nur über das definieren, was sie jetzt gerade publiziert haben und wie viel Geld sie reingekriegt haben und sonst überhaupt nichts, ist auch da, also ich meine, Du hast solche Leute und solche Leute“ (VU, PI3f, p. 27)

106 “Natürlich gibt's auch dann den Eigennutz in irgendeiner Form, dass man hofft: naja, vielleicht werde ich mal reich, wenn ich ein Patent habe und ich verkaufe irgendwas; aber primär ist es eigentlich bei den Hardcore- Wissenschaftlern Uneigennutz“ (II, PI20m, p. 24)

107 “eine goldene Zeit, die so sorgenfrei in der Form nicht mehr existiert, nachher kommt ja die Post-Doc-Phase, wo man zwar nicht mehr Doktorarbeit schreiben muss, aber dann kommt schon dieses Gedanken, na ja, wie geht es nachher weiter, nicht, derweil, ich meine, es tickt die Uhr, bleibe ich auf der Uni, gehe ich weg“ (VU, PI2m, p. 20)

108 “Bewusstsein ist es auf jeden Fall da, nicht, das ist nur, die Wichtigkeit ist unterschiedlich, wenn ich jetzt, wenn ich in Post-Doc-Jahren bin und ich habe nicht einmal noch meinen ersten wirklichen Job, sozusagen, dann denke ich, OK, entweder wird das nichts oder es wird was, aber Du weißt, ich meine, Du musst auf jeden Fall produzieren in irgendeiner Form“ (VU, PI3f, p. 26)

109 “Und die Leute, die wirklich sich über ihre Work Life Balance beschweren können und das auch tun sollten, das sind die jungen Postdocs und die, die gerade den Schritt machen zu einer Professur, die sind wirklich arm, die sind echt unter Druck in jeder Hinsicht. Aber die Dissertanten sicher nicht. Also aus meiner Sicht nein.“ (VU, PI5m, p. 3)

110 “im Großen und Ganzen funktioniert es für mich, auch auf dieser relativ politischen Schiene, die ich fahre. Aber manchmal merke ich, ich muss auch aufpassen“ (VU, PD07f, p. 7)

111 “Und ich habe trotzdem noch so ein bisschen den idealistischen Kern, eigentlich möchte ich nochmal was ganz riskantes irgendwie beginnen [...], aber wie gesagt, das ist wieder das Dilemma mit der Karriere, mit: wenn man weiß, man hat jetzt noch drei Jahre oder vier Jahre contract, ich muss publizieren, ich kann mir jetzt nicht leisten, irgendwie mal zehn Jahre lang unproduktiv zu sein — im Sinne von Publikationen. Das heißt, das ist halt das, der Luxus des reich seins, wenn man irgendwo eine feste Stelle hat, viel Geld hat.“ (II, PI7m, p. 36)

112 “wenn man seine Ergebnisse veröffentlicht hat und [...] die von anderen aufgegriffen und zitiert werden und eingebaut werden — das ist eine schöne Bestätigung, dass die Forschung nicht ganz sinnlos gewesen ist. Trotzdem ist das für mich kein Hauptkriterium mit, das man in den Vordergrund stellen sollte“ (II; PI13f, p. 4)

113 “Ich habe alles erreicht im Leben und ich bin sowieso, ich schwimme auf einer Welle und ich kann es bis zu meiner Pension, selbst, wenn ich mich jetzt hinsetze und nichts tue“ (VU, PI5m, p. 4)

114 “weil ich dann auch einfach irgendwo den Druck verspüre, dass ich also wirklich etwas machen soll — das glaube ich, dass das eher das fördert“ (II, PI15m, p. 14)

115 “ich persönlich, und ich denke, auch meine Kollegen, verfolgen im Prinzip die Philosophie, man schreibt lieber zu viele Leute mal mit drauf in die Autorenliste, als dass man jemanden vergisst und damit irgendwie abwertet.“ (VU, PD05f, p. 4)

116 “ich versuche zumindest in der, innerhalb der Gruppe lieber einen zu viel als zu wenig und wenn jemand sich beschwert soll er kommen und dann

begründen. Ja, dann kann man ja sehen, wenn er gute Gründe hat, kann man ja Entscheidungen verändern“ (II, PI7m, p. 37)

117 “ob man immer gerecht ist, weiß ich nicht“ (II, PI7m, p. 37)

118 “Und die PhDs, mit denen rede ich eigentlich viel drüber, weil ich eben verhindern möchte, dass das, es muss von Anfang an klar sein, welchen Stellenwert was hat. Und so gesehen wissen die das. Sie brauchen Erstautorschaft, sie schreiben das auch. Alle, die mithackeln stehen darauf. Und quasi die, die Leute im Hintergrund stehen sozusagen, die betreuen und das Formale machen und korrigieren de facto und so weiter. Ja, das ist ihnen eigentlich relativ schnell bewusst, allein weil alle unsere Arbeiten so aufgebaut sind.“ (VU, PDoc7f, p. 8)

119 “wirklich gemacht haben“ (VU, PI3f, p. 10)

120 “da gibt es vielleicht ein Paper nach fünf Jahren harter Arbeit“ (VU, PI2m, p. 15)

121 “kommen zu einem Experiment, das gerade im Volldampf läuft seit vielen Jahren, da kann man schnell einsteigen und ernten, das ist ja ungerecht, also so brauche ich irgendwie, das wertschätzend zu evaluieren“ (VU, PI2m, p. 15)

122 “Ich denke, Postdocs sind schon die Leute, wo sie sich als Letztes Gedanken machen, weil sie haben halt so viel zu tun. Es ist auch angemessen, dass man sich dann eher um die Doktoranden Gedanken macht, die mehr Unterstützung brauchen, als um die Postdocs und dass man als Postdoc dann halt selbst anregen muss, wenn man etwas braucht. Da habe ich bis jetzt nicht viel gemacht.“ (VU, PDoc4m, p. 14)

123 “dass die meine Arbeit auch würdigen werden, wäre ich erstmal vorsichtig, wie viel Zeit ich da investieren will“ (VU, PDoc4m, p. 4)

124 “nicht so richtig klar“ (VU, PDoc4m, p. 3)

125 „Also ich war eigentlich schockiert, wie, als wir das erste Mal über ein Manuskript und Autorenschaften, wie, wie stark unterschiedlich die Leute argumentiert haben für ihre Position, oder was auch immer. Und das man, dass halt nicht jeder die gleiche Einstellung hat; und wiederum da spielt es eine Rolle, ob man, ich glaube, kulturell macht das Unterschiede, was halt, was war meine Ausbildung, wo komme ich her, was habe ich gelernt über dieses Thema, und so weiter.“ (II, PI8m, p. 20)

126 “Gratwanderung“ (VU, PI2m, p. 10)

127 “man ein gutes Gefühl, ab wann ist man Wert für Acknowledgement, wann ist man doch eine echte Hilfe gewesen, um zu sagen, OK, Autorenschaft“ (VU, PI2m, p. 10)

128 „natürlich die Publikationsliste ein ganz wesentliches Karrierekriterium ist einfach in der Wissenschaft, an dem der Forschungserfolg festgemacht wird — mal berechtigt, mal eher nicht so berechtigt“ (VU, PDoc5f, p. 3)

129 “Ich nehme ein CV und schaue: hat der Science, Cell und Nature-Paper? Nein, linker Haufen — ja, rechter Haufen, ja? Dabei ist das Wichtigste, mit den Leuten zu reden. Fünf Minuten genügen um herauszufinden, smart guy or forget it, ja? Und das, wir verlernen das. Und auch die Unis, die Unis können nur Impact-Punkte zählen [...] es werden Punkterl gezählt und dann wird addiert, aber auf die Person wird nicht geschaut. Ich glaube, es, wir haben das entweder bewusst oder unbewusst verlernt. Wir müssen es soweit es geht in persönlichen Gesprächen machen, sonst, also rein Impact-Punkte zählen ist, ist ein schlechter Weg, ja.“ (II, PI20m, p. 40)