

Rationalities of Scholarly Discourse

A Cultural Sociological Analysis of Authorship and Publishing in the Humanities

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I, Marcel Knöchelmann confirm that the work presented in this thesis is my own.
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Abstract

Publish or perish is an ever-present topic in academia, but it is much better understood in the sciences than in the humanities. The humanities are historically geared towards a culture of thought articulated by historical-hermeneutical principles. Contemporary academia, however, is increasingly governed by formal rationality which induces a culture articulated by instrumentalization. Authorship and publishing practices are manifestations of these cultures. More and more, formal rationality prevails so that publishing practices become instrumental means for purposes external to scholarly discourse: individuals can advance their careers or institutions enhance their reputation and funding with formal publication records. This cultural shift from scholarly values to formal measurement helps explain why publishing practices are constituted the way they are today, and why many a scholar perceives immense pressure to publish ever more frequently, irrespective of scholarly discourse.

I draw on cultural sociological theory as well as on two empirical studies for my analysis. I utilise practice theory to explain the relation of cultures and practices as well as Max Weber's categorisation of rationalisation to ground my argument. I conducted a quantitative survey with more than 1,000 scholars in Germany and the UK as well as a series of 18 qualitative interviews with humanities scholars to determine characteristics of the empirical situation. There have been many recent claims of a crisis in scholarly communication: my analysis helps see this in a new perspective. It suggests a re-evaluation of the terms on which early career scholars compete, terms on which national policies and mechanisms such as the REF are built, and terms with which institutions and funders seek to legitimate the use of resources. Terms, as the analysis determines, that are increasingly tied to instrumental publishing instead of to thinking, teaching, or scholarship.

Impact Statement

How can the expertise, knowledge, analysis, discovery, or insight presented in my thesis be put to a beneficial use? I consider three areas of such benefit: the direct scholarly community, the wider institution of academia, and institutions concerned with scholarly publishing.

Firstly, the direct scholarly community—scholars in the areas of publishing studies, information studies, cultural sociology, or the humanities more broadly—may benefit the most from this thesis. On the one hand, I engage in theoretical discussions (for instance, about culture in information studies or putting contemporary sociological work to use in the context of authorship and publishing) which helps advance other scholarly pursuits. On the other hand, data from my empirical studies and my discussions of them are new contributions upon which other scholars can build new research pursuits. Most of all, I hope that other scholars can learn and reflect about systemic motivations to publish, and perhaps evaluate their own practices in relation to the practices I describe.

Secondly, the wider institution of academia may benefit from the insights of this thesis. As I engage in culture principles that become actualised in publishing practices, the underlying cultures are crucial factors of change and development. University steering boards, funding bodies, or committees responsible for career progression decisions usually have influential power on culture principles as their practices provide meaning as to what does and what does not count. The insights of this thesis may encourage them to re-evaluate the meaning of authorship and publishing.

Thirdly, institutions concerned with scholarly publishing—university presses, libraries, learned societies, or commercial publishing houses—can benefit from the thesis as I provide data about the empirical situation of publishing practices and determine

cultural driving forces within academia. Though my findings may not lead to previously undiscovered business models, I do point to ways forward in terms of reorienting value in authorship.

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List of Abbreviations

IS	Information Studies
JIF	Journal Impact Factor
MMR	Mixed Methods Research
NPM	New Public Management
OA	Open Access
OED	Oxford English Dictionary
PRFS	Performance-Based Research Funding System
QR	Quality-Related (funding)
RAE	Research Assessment Exercise
REF	Research Excellence Framework
RSE	Research Selectivity Exercise
UCU	University and College Union
UFC	Universities Funding Council
UGC	University Grants Committee

The fact that the maximum of formal rationality in capital accounting is possible only where the workers are subjected to domination by entrepreneurs, is a further specific element of substantive irrationality in the modern economic order.

Max Weber: Economy and Society.

And if changes in the medium of present-day perception can be understood as a decay of the aura, it is possible to demonstrate the social determinants of that decay.

Walter Benjamin: The Work of Art in the Age of Its Technological Reproducibility.

1 Introduction

It is said: either you publish, or you perish. You may not clinically pass away, but if you do not publish, your scholarly career collapses. Nevertheless, having published by no means implies you do *not* perish. But if you aspire to a career in academia, you have to publish, the more the better. This, in short, is contemporary academia's iron cage, its rationality embodied in scholarly discourse.

There can be a different rationality. Dilthey asserted that scholarly disciplines known as *Geisteswissenschaften* are those concerned with understanding humankind in its historical-hermeneutical embeddedness ([1910] 1970: 89). Habermas revised this foundation of humanities scholarship by eliminating its secret psychologism: hermeneutic understanding does not mean emphatically reconstructing a past mind; it means understanding the creation of a product (1971: 146). Correspondingly, the work of a humanities scholar is both this understanding *and* the creation of a product that represents this understanding. An ideal of rational discourse can be considered one in which all products—discourse artefacts such as journal articles, monographs, edited collections—optimally serve the purpose of representing understanding. The rationality of such discourse would be that of humanities scholarship *eo ipso*: all contributions to discourse would embody the interest of enlightenment; a mystical ideal.

Politics, economics, disciplinary developments, or matters of power seem to interfere with the possibility of such an ideal's realisation. Today, humanities scholars produce a lot of discourse artefacts, perhaps more than required for representing understanding. The question is: why do they do so? Somewhere between *publish or perish* and the *interest of enlightenment*, reasons must be found that explain why scholars publish the way they do. These reasons may oscillate between culture principles of an elusive intellectualism and the institutional manifestations of new public management (NPM) regimes; a dualism of

scholarly substance and formal rule-following, mystical ideal and managerial implementation. It is these culture principles that I aim to illustrate by using Weberian categories of rationality in order to understand publishing practices in the UK and Germany.

Research Question

From this follows my broader research question: *why do scholars in the humanities publish their scholarship in the way they do it?* This focusses the communicative practices to internal scholarly communication. Though external communication—between scholars and society—is highly bound up with internal communication in the humanities, the latter follows distinct social patterns and it is this nexus of patterns—culminating in *publish or perish*—that is at the core of my interest.

This theme is arguably still quite broad. There can be manifold reasons to publish depending on the research perspective. Possible standpoints could be those of scholarship, technology, or the social-institutional realm. Scholarship, here, refers to looking at scholarly discourse, i.e. why do you publish *this argument* in this particular *textual style*? A technological standpoint would look at the pragmatics of practices and artefacts, i.e.: why do you write in this *format*, submit text with this *software*, and aspire to receive that *materiality* of the published text? My approach is none of these. I focus on the social-institutional realm: what encourages or forces you to publish this number of publications in that time with those particular publishers to gain what kind of recognition?

Therefore, the *why* of my question is connected to the *culturally-mediated meaning* embodied in the practices of publishing: the symbolic value of authorship and its connection to culturally-mediated modes of recognition. Thus, first and foremost, I focus

on the scholar as an author (instead of taking the perspective of publishers or readers). When I refer to publishing, I refer to an author's desire to publish *X* with publisher *Y* because of *Z*. I do not refer to the technical elements of publishing such as minutiae of formatting an article and submitting it. I also do not refer to concrete *Xs* or *Ys*. I do elaborate on this abstractly in chapter 10, though. What I am trying to understand are primarily the *Zs* and the culture principles they are geared to.

Thus, the author as a writer or stylist is not of importance; the social subject resulting from the practices of publishing is. My combination of cultural-sociological approach and information studies (IS) leads to regarding information behaviour as social practices—practices that are intricately connected to artefacts (such as publications), discourses (non-scholarly or scholarly), subjectifications (scholarly personas), and culture (the meaning-making spanning temporal-spatial institutions). Subjects—scholars in the humanities—become who they are *as they actualise* practices and practices only exist *as subjects actualise* them: subjects and practices constitute each other recursively.

Crucial for understanding this constitution is the culture embodied by practices, providing individuals with meaning to interpret their worlds. This culture may be conceived of as the ideals of humanities scholarship or as a managerialism explained by a particular mode of rationality. This latter mode, I argue, enforces a perception of formal authorship as the epitome of proving achievement. In turn, this creates an imperative to publish, an imperative that is increasingly less bound to scholarly discourse. This imperative, articulated as *publish or perish*, creates an affectual structure that shapes reasons why individuals continuously realise publishing practices. While it *can* be the individual who engages in conversations about *publish or perish*, it is the collective that cultivates this conversation over time. Thus, aiming to understand social reasons for why scholars publish requires abstracting from individual action and focussing on structural explanations. That which lies

behind the articulation of the imperative—formal rationality in a competitive environment—is one such potential.

Owing to all these aspects, my approach can be narrowed down to a particular perspective, a lens through which I ask what theorising the imperative to publish can tell us about why scholars publish. Drawing on the notion of *publish or perish* can be related to a focus on internal scholarly communication, as said above, which is why I do not look at crossover books or recognition for scholarship in society more generally. This emphasis allows me the concentration to focus on a coherent frame as becomes clear, for instance, as I discuss the Research Excellence Framework (REF) in chapter 5. And the REF hints at another focus: I look at the UK and Germany as two specific exemplars that are well-suited for a productive comparison, comprising of different historical roots and different managerial cultures (as is visible in the debates on excellence). I explain this choice in more detail in chapter 3.3.

By connecting imperative and general motivating principles, I work inductively: I employ a specific social fact—pressure to publish—to work towards a more general theorisation about publishing practices. A deductive approach would require working with an existing theory which would amount to the testing of the individual in relation to something normative or abstract. Since I aim to understand what is available rather than critique the actualisation of a theoretical norm, this thesis generally represents an inductive process. And yet, my work comprises of a critical perspective and criticism naturally evokes questions of normativity. What is the ideal that allows me to criticise reality? I do not work with an ideal of authorship or publishing in this respect. Induction requires an openness in order to understand what indeed takes place. Instead of determining upfront what authorship or publishing is, I draw critical potential from my social theoretical ontology.

That is, the way *something* is constituted in respect to what it *means* or is *said to mean* allows me to criticise this constitution.

In other words, this is a question of means and ends. Consider the practice of referencing a monograph: what are the ends of this practice? Is the end to be found within the practice or the artefact: e.g. is the content of interest, which is why I discuss and cite it? Or is the end to be found without them, such as: I wish to support my colleague, so I cite her new monograph which will improve her *h-index* (I even might not have read the book). At the first instance, means and ends are substantively close to each other; at the latter, the practice of referencing is instrumentalised and fares as a means to some external end. If the latter becomes the norm and all practices become instrumental means, this can be called a teleological circle of instrumental practices, which is alienating the original practice. Such alienated condition is a potential of criticism.

I explain alienation in more detail in chapter 8 and relate it to the imperative to publish and the Weberian categories of rationalisation. This connection sets the argumentative frame, which is a critical approach, but does not pre-determine what authorship and publishing are (since this is what I aim to understand). This frame of alienation proposes to understand reasons to publish not in motivational terms, focussed on a somehow motivated individual. Instead, it allows me to understand reasons in structural, especially cultural ways; a methodological approach in which both subject and practices are abstracted from the individual and her actions (as I explain in chapter 3). In the end, it is not psychological motivation that explains practices, but the culture principles that helped establish them in the first place.

The Structure of this Thesis

This is very much an interdisciplinary study. I draw on social theory to understand a topic considered to be part of IS, and publishing studies in particular. Moreover, my focus on the humanities requires (and allows) me to draw on the history and philosophy of the humanities to illustrate culture principles. This combination, however, can lead to confusions where field-specific terminologies mingle. Conversations with my supervisors, colleagues, and especially my research stay abroad highlighted these instances. I try to avoid these confusions pre-emptively by spending considerable space towards illustrating and defining things.

The structure of my thesis shows where I do so: in the following chapter, I introduce key contributions to the body of research on publishing practices in relation to an imperative to publish (in the many guises this theme appears in the field). Thereafter, in chapter 3, I explain my methodology. My theoretical grounding illustrates practice theory and cultural sociology as well as the reasons why I recognise this approach to be optimally suited to answering the research question. This chapter includes various definitions of social theoretical terms including the core quintet of culture, practices, artefacts, discourses, and subjectifications.

The following chapters deal with concrete culture and practices. In chapter 4, I present and discuss empirical data from my exploratory survey, focussing on aspects of pressures to publish. I particularly highlight the differences and similarities between Germany and the UK. This chapter presents the key starting point for my broader thesis on the pressure to publish and how it is impacted by culture principles.

The following two chapters directly connect to this. In chapter 5, I go into details about the UK, the REF, and publishing practices. Chapter 6 mirrors this with a focus on

Germany and the *Exzellenzinitiative*. These chapters comprise of discussions of data from my interviews as well as relevant literature, and they connect to chapter 4 as they help understand the cultural background that explains the empirical realities discussed there.

In chapter 7, I highlight culture principles of humanities scholarship based on an inclusive definition, followed by a showcasing of empirical data from my own survey about authorship and publishing in the humanities. In chapter 8, I discuss culture principles of rationalisation in, firstly, Max Weber and fellow social theorists' conceptions; secondly, as it is embodied in authorship and publishing practices abstractly; and thirdly, as it enforces an alienated condition. 8.2 comprises of the theoretical core of my argument. Meanings of the terms publishing, authorship, and pressure are also laid out in detail there.

The chapters 9 and 10 further differentiate the understanding of culture principles as I discuss the need to strategize publishing, leading to authorship being an instrumental means. I also conceptualise publishing venues which lead to a stratification of scholarly discourse. Both chapters connect empirical findings from my interviews with relevant literature. As they discuss more abstract concepts about authorship and publishing, they apply to empirical realities in both Germany and the UK. However, major divergences are also discussed. Moreover, it is in chapter 9 that I explain *publish or perish* in more detail in relation to the social theoretical category of alienation (developed in chapter 8). By so doing, this chapter also articulates empirical issues discussed in chapter 4.

A note on the use of German literature: of the major works of German scholars that I build on, especially Habermas, Jaeggi, Rosa, Reckwitz, or Weber, I worked with the original German texts. However, where English translations are available (in most cases except for some works of Reckwitz and Weber), I cite the English texts to allow for a comprehensive follow-up for the intended (English language) audience. I provide

translations of direct quotes of other texts that are only available in German (such as those of Liessmann, Marquard, or Münch); this is highlighted at each instance.

2 On Publishing or/and Perishing: A Literature Review

Within research on publishing practices, the pressure to publish is a predominant theme. The many articles and books touching on it range from critical analyses of principles and practices to self-help guides that seem to be indifferent to the underlying problems. *Publish or perish* is the key term of reference for this theme but other terms such as the *publishing imperative* or simply the *pressure to publish* are common as well. A comparative search of the terms *publish or perish*, *scholarly publishing*, and *academic publishing* through Google Books Ngram confirms what individual literature indicates: notions of a publishing imperative date back to the first half of the twentieth century and even dominated over the other two terms in the years 1960-1980 (appendix A1).

Crucial for the assessment of this literature is that most of the articles and books are written from (or for) the perspective of a specific discipline or cluster of disciplines. While some forms of pressure and practices are reasonably transferable to other disciplines or countries, doing so always requires a certain degree of caution to not overgeneralise. There are many discipline and culture-specific principles that demand further specifications of conceptions about a pressure to publish. Nevertheless, even studies about publishing practices in, say, biochemistry, can help find out more about similar practices in the humanities—at least to a certain extent.

The structure of this chapter is as follows: I first outline publishing imperative as a theme in scholarly research (2.1), and then reflect on the origins of the term *publish or perish* (2.2). In the last sub-chapter (2.3), I present and discuss themes connected to *publish or perish* such as disciplinary differences, (un)intended consequences, structures of reward, and teaching.

2.1 The Imperative to Publish as an Object of Study

Publish or perish has become known far beyond the confines of academia as a paradigm or in the form of practices that are detrimental to fundamental ideas of scholarship. It comes as no surprise, then, that there are a variety of articles in popular media that implement the theme in wider discourses in the English-speaking world (Aitkenhead, 2013; Colquhoun, 2011; Kristof, 2014) or in Germany (Könneker, 2018; Pörksen, 2015). However, *publish or perish* is far from being coherent: not only is it referred to with different names, it also appears as an abstract imperative, as the most prevalent principle of a productivity and management regime in institutionalised academia, or, simply, as the current culture of scholarly publishing in general. Moreover, *publish or perish* is connected to a variety of concepts. Among others, *publish or perish* is an age (Rosa, 2010: 55), an aphorism (Rond and Miller, 2005), a climate (Relman, 1977), a culture (van Dalen and Henkens, 2012), a doctrine (Moosa, 2018: vii), a *Fluch* (German: curse; Könneker, 2018), a *Grundgesetz* (German: basic law; Barth, 2019: 13), an ideology (Vannini, 2006), a mantra (Guraya et al., 2016), a phenomenon (Miller et al., 2011), a slogan (Hexter, 1969), a syndrome (Colpaert, 2012), and a system (Lee, 2014).

Moosa offers one of the more comprehensive analyses of the publishing imperative with a critical perspective on the roots of neoliberal research and university management, and the consequences of too many publications. He looks at problems around peer review (2018: 123–134), ‘the ranking craze’ behind the REF and its predecessor RAE (Research Assessment Exercise) (2018: 157–163), or a variety of bibliographic issues throughout the book. This work gives a good overview of issues that surround the pressure to publish and Moosa tries to suggest ways forward in his conclusive remarks. However, just as he claims that the ‘way forward is to abandon the POP [publish or perish] culture’ (2018: 181), the work remains at a mechanistic level of analysis. Surely, getting rid of such culture would be

favourable. Most of Moosa's work exposes negative consequences that fare as good reasons for doing so. But these consequences alone cannot explain culture. Asking to abandon culture may, therefore, mean determining better mechanisms. And yet, without an idea of culture principles beyond those mechanisms, change may be hard to facilitate, or only up to a degree. This explains well the different approaches to the same object of study: Moosa looks at mechanisms and adverse consequences; I analyse culture principles embodied in practices.

Hyland and Thompson offer two alternative in-depth analyses of publishing, including its potential imperatives. Both their works look at institutionalised academia as a whole and differentiate between disciplines only where necessary. Hyland—himself a scholar of linguistics (for valuable insights into linguistic aspects of authorship and publishing, see: Hyland, 1999, 2001, 2003, 2004)—studies the construction of knowledge in respect to how authorship and publishing practices have an impact on it (2015). This is among the most comprehensive accounts of the publishing imperative and its consequences. It involves a variety of aspects of competition, reputation, and material scarcity that are involved in the problematisation of authorship and publishing. Hyland connects well these different aspects that are crucial for a thorough analysis: since 'the system of reward and the system of communication are one and the same, and because both material and symbolic capital are extremely scarce, academic publication is fiercely competitive' (Hyland, 2015: 17). I further draw on Hyland's analyses throughout my thesis so that reference to his work in this review is just an introduction.

Thompson's approach is less academically-minded: he looks at publishing pragmatically, in terms of the subjects and institutions involved. Most of all, publishing practices are geared towards the institutional processes of established publishing houses. In other words, Thompson tries to explain how publishing works institutionally. All the while,

he does not fail to point to the Bourdieuan forms of capital involved in the life of a publication (I critique the concept of capital in chapter 10). His comprehensive work can explain several aspects that other studies often only mention briefly or even erroneously. One of these is the so-called monograph *crisis*: the economics of publishing are complex since it deals with a variety of forms of trust, power, and culture. It comes as no surprise that ‘most academic publishers can come up with numerous examples of high-quality monographs that sold poorly’ (Thompson, 2005: 127), without a proper reason for the bad economic performance ever being found.

And even though Thompson is less concerned with scholars than with pragmatic processes, he points to some crucial social issues. For instance, he distils one of the key detrimental effects—the notion of temporal pressure—that the RAE had on authorship and publishing:

the temporal structure of the RAE has meant that many academics, in some cases under pressure from their heads of department, have sought to get books out quickly in order to qualify for the RAE, and have in turn placed pressure on publishers to speed up their reviewing and production processes (Thompson, 2005: 285).

I explore this issue further in chapter 5 in relation to my own interviews.

2.2 The Origins of *Publish or Perish*

The origins of *publish or perish* are obscure, a claim confirmed by the many reviews of the term. Many scholars refer to a—relatively—young article written to explore in more depth where the term itself was coined. This short but incisive article is authored by Garfield (1996)—a scholar today mostly known for his works in bibliometrics. Garfield sees the origin in either a statement Marshall McLuhan made in a letter to Ezra Pound in 1951, or a descriptive account of prestige in research cultures written by Logan Wilson in

1942. Both of these mentions are in quotation marks, however, leaving it uncertain whether they were coined or merely referenced by either of the two scholars.

In a study of the mobility and promotion patterns of US-based economics faculty in relation to their publishing activity, Skeels and Fairbanks state that '[i]t is very interesting to note this analysis suggests that quantity of publication rather than quality may be more important in explaining promotion patterns' (1968: 25). Though the data of the quantitative analysis of this study may have been partly disturbed by labour market demands and it is difficult to transfer findings to other disciplines, this study nonetheless offers early indications that measurable quantity fares higher than subjective quality. This leads Skeels and Fairbanks to conclude via a circular argument that the more competent publishing scholars may also be the more competent teaching scholars (1968: 25).

A decade later, in 1977, Relman found harsh words against the consequences of wrong incentives of a publishing regime. He stresses the 'inflation of bibliographies' and a 'seductive kind of dishonesty' effected by a climate of *publish and perish*, resulting in a practice that 'distorts the prime purpose of scientific reporting, which, after all, is the communication of new information and ideas, not self-aggrandizement' (Relman, 1977: 724).

An early proponent of *publish or perish* is Hexter as he connects dots between the 1968 student revolts and publishing requirements: he grounds the first broad publicization of the *publish or perish slogan* in student protests that happened amid the rejection of a scholar at Yale University who had not got 'already published work of considerable distinction' (1969: 61). Hexter makes the point that the scholar in question had had many other opportunities to teach other than Yale University and that the application of *publish or perish* to this situation means nothing more than an indifference to one's position in the field. In other words, the pressure to publish only arises if one wishes to be awarded a position at a

prestigious university, hinting at the harsh fact that the impact of structural pressure in the end depends on personal motivation. He makes some compelling points in his analysis of options of teaching and massification of education, but eventually offers nothing more than a reification of the stratification of scholarly artefacts and universities.

Lofthouse draws—partly in response to Hexter—the focus away from the question of the desirability of *publish or perish* towards a questioning of its existence (1974). He does so especially in the light of a disbalance between researching (and publishing research) and teaching: under a *publish or perish* regime, assessments and evaluations for careers and the existence of academics is biased towards research to the detriment of teaching. This is a premise that Lofthouse aims to rule out by leaning on several reports and studies that average research, teaching, and publishing habits. Consequently, though, he misses the chance to reflecting on the force a social imperative might have on the constitution of a subject and how this, in return, will affect wider culture principles. In this vein he concludes that

any very extreme form of the publish or perish doctrine must be rejected. But should an academic wish to publish, there is little to stop him doing so. Finally, [...] it is a pity that the “publish or perish” doctrine does not hold in a more extreme form than it does (Lofthouse, 1974: 75).

In a letter to *Nature* readership in that same period, as a response to the rise of authorship reputation and Journal Impact Factor (JIF) value, Mackay proposes to let ‘all papers from the department be published under the same fictitious name’ (1974: 698). The course of history has shown that this has not been taken up so far, though it may indeed be one of the simplest measures available to steer discourses away from their dependency on formal authorship.

Schachman tracks the origins of structural pressure, rather than the term, in the sciences in the US to the enforcement of state-funded research by Vannevar Bush after

WW2 (Schachman, 2006), which is generally considered a decisive moment in the increase of national research endeavours and endowments. Not only did this set off a new period of politically motivated interference in scientific research, it also successively triggered the employment of technocratic rule and peer review-informed decision-making.

2.3 Themes in the Literature on the Publishing Imperative

Publishing Imperative and the Humanities

There are some works that critique *publish or perish* or its consequences specifically from a humanities context. This repeatedly goes along with a critical assessment of the deteriorating economic situation of publishing in the humanities (the monograph publishing *crisis* that is positioned to be sort of a counterpart to the serials crisis that is more relevant to the sciences). By means of such attribution, authors disapprove of many of the systemic developments that are made responsible for the existence of a wrong productivity regime. Massification and *McDonaldisation* of scholarship are to be condemned, but the social forces stemming from the structures of scholarly communities in which most scholars thrived are only rarely touched on.

A most prominent example is Collini who is a regular critic of the development of humanities scholarship, assessment practices, and the purpose of the university in late modern UK (see, for instance, his publications in the *London Review of Books* (2003, 2010) or in *The Guardian* (2015, 2018), or his collection of essays (2012)). These works sum up various aspects articulating the culture shift that I highlight in my thesis. Collini vigorously discusses the necessity and value of the qualitative in all forms of humanities scholarship: from the cogency of authorship and the practices leading to publications to its forms of assessment and evaluation (2012: 72–85). Higher quantities of output or the enforcement

towards quantitative approaches to publications are posited to be detrimental to the nature of scholarship and publishing in the humanities. Other pieces concern unfavourable practices regarding publications and, connected to these, reports and citations in the humanities (2012: 146–152), or offer a critique of impact reports for which humanities scholars are required to turn, often superficially, impact of scholarship and resulting publications into reports or narrations (2012: 168–177). This, in the end, is but another form of pressure that rests on a favouritism of formal output. Relatable to Collini, Crane finds devastating consequences in the ‘institutionally generated pressures to publish’ in philosophy (2018). Not only does this give rise to a massification of scholarly publications; it also developed practices of gatekeeping submissions that is detrimental to the philosophical discourse.

Conceptually more substantial is Fitzpatrick’s influential study on authorship in the humanities (2011b). In a self-reflective approach, Fitzpatrick works out the negative consequence of prevailing assumptions on publishing and concludes that it is vital to understand ‘digital humanities laboratories and digital publishing experiments [...] as a key element of the infrastructure of the university of the future’ (2011b: 166). Particularly strong is Fitzpatrick as she explores how digital technology can facilitate new environments for collaborative approaches to authorship or peer review, discussing both conceptual bases and technological solutions. Fitzpatrick well balances the technological perspective, just as she explains that ‘technologies and cultures are mutually determining and thus must evolve in concert’ (2011b: 53). In this sense, my thesis is similar to Fitzpatrick’s study, only that, as Fitzpatrick’s telos is the collaborative approach as a *solution*, I focus on the analysis of pressure and practices more fundamentally.

Dalton assesses potential problems arising from ‘economic, social, political, and technological forces [that] have converged to produce an unstable present and an uncertain

future for the scholarly monograph' (2006: 251). Predominant in her study is the increasing prevalence of a culture of quantity that arose out of a mechanism of comparison and competition. This seems to dictate that prestige and publications outsmart quality scholarship; the former can be counted and compared while the latter remains a merely subjectively assessable accomplishment. Baveye's analysis of the economic crisis of publishing scholarship is to be situated in a similar critical standpoint where he demands a concerted effort to 're-emphasize quality over quantity' (2010: 191). Among his conclusions is that Open Access (OA) publishing will most likely make things worse.

Since monograph publishing is much more expensive than journal publishing, the shortage of funds for publications seems to hit the monograph-dependent humanities disciplines with full force. This can be seen in the ever-rising prices (as a consequence of declining sales per unit) amid ever more monographs being published (each with declining print runs). Jubb's end of project report of the *Academic Book of the Future Project* and some of the works incorporated such as Tanner's analysis of academic books submitted to the REF 2014 (2016), an assessment of the dilemma of OA monograph publishing (Knöchelmann, 2017), and the analyses provided in Lyons and Rayner (2015) confirm the development of publishing scholarship in the humanities. OA is offered as a solution with restrictions (see, for instance: Crossick, 2016, Eve, 2014; I further discuss OA in chapter 10). What such works on an economic crisis and its ramifications and potential solutions often do not touch on are the roots of this *always more*, especially the social forces evolving from an academic productivity regime.

Wolfe Thompson approaches this *crisis* of monograph publishing with a citation analysis as she tries to determine changing usage patterns in the discourse communities of literary scholarships (2002). She confirms that the scholarly monograph—especially that published by a university press—is not dead, for it still is the 'most significant vehicle for

scholarly communication in this field' (Wolfe Thompson, 2002: 133). However, Thompson concedes that the monograph requires careful consideration in policies and a restoration of its status in library collections. Two decades of digitalisation since this statement seem to only reinforce this requirement.

A telling dispute in the cultural articulations of scholarship, teaching, and publishing in the humanities is provided by Hanson and Heath, and Connolly. The former two offer an appraisal of classical education where they demand a restoration of the ideals and status of classical philology in academia (1998). While such demands may be in the well-intentioned interest of scholarship, the work nonetheless fails to incorporate some of the basic social and economic forces that may be responsible for the demise of a cultural ideal. Connolly's review, then, rectifies this in that she points to the problems of the status the classics have in the massification of higher education and a publishing imperative in scholarship (1998), pointing to Readings (1999) or Nussbaum (1997) for more comprehensive critiques of educational ideals.

Lastly, Savage criticises forced productivity in the humanities that focusses on book publications only (2004). This position is quite rare in the literature on productivity in the humanities: though Savage is in line with most others who criticise the bias towards more publications, he takes sides with articles in the humanities and criticises an evaluative imbalance towards books. In the end, however, forced productivity in the sense of an imperative to publish is what is of most concern for it lures scholars into premature publishing, a detrimental rise of one-dimensional thinking of output practices, and increased pressure on young scholars. He concludes that '[f]orced productivity has led faculty to develop defensive strategies based on the perception that quality is less important than quantity' (Savage, 2004: 44).

Publishing Imperative and the Social Sciences

Next to critiques of the imperative in the humanities, there are works that are concerned with *publish or perish* in the social sciences, or investigate the paradigm with sociological methods. Some of the empirical studies such as Borkenau and Kammer's (1988) or Miller et al.'s (2011) serve as strong points of reference for my own exploratory survey.

A widely received, early study of a publishing imperative with sociological and social-psychological means stems from Borkenau and Kammer (1988). Based at the University of Bielefeld, they tried to identify the connection of both subjective and objective indicators to publishing-related productivity. Most important for their study is the separation of tenured (n=42) and untenured (n=148) staff. Their results offer a formidable perspective on the construction of a publishing productivity regime: though there is no significant difference in the publishing output of the two groups, untenured staff indicated significantly higher rates of dejection, insecurity, and concern. Borkenau and Kammer discuss this aspect with reference to the higher education and research policy in Germany, where a vast amount of young scholars—the *Mittelbau*—is trained in research and teaching skills in temporary assistant positions without the prospect of ever gaining a tenured position or professorship (since those positions have been, and are, very scarce). This is a personal dilemma: aspiring to get tenured leads the individual to become ever more specialised in her field, which makes it harder to leave academia and pursue a career elsewhere. Only perseverance leads to success, and this means being productive in the terms the potential employer sets, irrespective of these being good for scholarship. Borkenau and Kammer identify that the feelings of concern and depression rise with the loss of prospective employment security, which means that pressure to publish may indeed be external to the intrinsic values of scholarship so that resulting publications are in parts

‘routine publications’ or merely *publication rubbish* (*Publikationsmüll*; Borkenau and Kammer, 1988: 78). It is indicated that, since untenured staff are on average not more productive than tenured staff, but more emotionally depressed, the diffuse prospect of a tenured position in German academia might not be seen as a good motivator to be more productive in both scholarly and philistine respects.

Schmitz builds upon this study with his own survey of the *Mittelbau* (n=86) conducted at the Vienna University of Economics and Business (1990). Regarding their employment potential, the respondents in Vienna seem to be slightly less concerned and negative than those in Bielefeld. Schmitz finds more motivational aspects regarding productivity and prospects of tenure. However, the highest correlation between his study and Borkenau and Kammer’s is the attribution of luck to personal chances: having had luck—for instance, publishing the right article at the right time—is deemed to be a crucial aspect for one’s career chances. This points to a (potentially perceived) contingency in the constitution of a scholar subject: a good network, competency in a field, and productive output may help a career, but without sheer luck it is much harder to be successful.

Other sociologists also mention the imperative to publish to be a social fact: for instance, Weymann discusses the situation of sociological scholarship in Germany and identifies a ‘massive (over)production’ of publications which ‘reflects the impact of the “publish or perish” principle as well as the weakness of an effective selection procedure within sociology as a scientific community’ (2016: 213).

In a sociological analysis of Bourdieu’s types of capital in relation to OA, Herb identifies the predominance of publications as a result of the importance of symbolic capital in institutionalised academia. For—as trivial as it may sound—‘reputation is derived from a given scientist’s publication record’ (2010: 4), and those scientists (and non-scientific scholars alike) who ‘have benefited from distributive mechanisms of capital have

little or no interest in changing these mechanisms or evening out these disparities—because the disparate allocation of power shapes and structures disciplines’ (2010: 5). This is the basis for the divide of power: senior scholars could indeed change an imperative that is pressurising aspiring scholars; but doing so would question and potentially undermine those mechanisms that senior scholars have had to pass to gain power and which then institutionally fix it. Here, more openness in the practices of sharing information (such as via publications) will not diminish this power but add another layer of power, since the *open* artefacts themselves become ‘subject to human interests and necessities like networks constructed for distinctiveness and power’ (2010: 5).

Miller et al. set out to study the pervasiveness and forcefulness of the *belief in publish or perish*. They did so by testing the perceptions of tenured and tenure-track faculty (n=448) in management studies at US institutions. Respondents were asked to indicate the felt pressure in different categories of publishing, potential sources of these, and their motivations to publish. The responses confirm that there is indeed perceived pressure and it is higher among untenured researchers. Yet the sources of pressure are obscure. The highly ranking category ‘pressure coming from myself’ does not provide a basis to infer any indirect sources such as institutional structures, career advancement, or simply scholarly reasons. Interestingly, the researchers frame and discover a ‘publication burnout’ that is highly associated with perceived pressure as an effect of the publishing imperative (Miller et al., 2011: 431). Moreover, the stress arising from a perceived publishing imperative is higher for both untenured and female researchers. Another incisive finding concerns the need to publish in peer reviewed (and, thus, branded) journals, which is said to be part of the reason for the decreasing innovation in authorship and publishing practices. Miller et al. conclude that ‘[t]he internal and external pressures management faculty feel, as well as the rewards they receive, explain why they publish significantly more articles in peer-reviewed

journals than articles in editorially reviewed journals, scholarly books, chapters in books, case studies, or textbooks' (2011: 438).

A comprehensive analysis of social forces in various guises can be found in the study of acceleration in institutionalised academia, in which Vostal (building heavily on Rosa, 2013, 2019) connects perceived and potential changes of temporal structures to forms of pressure, among which range publishing productivity and output assessments (2016). This work touches on similar theoretical issues such as alienation and structural pressure highlighting academic culture, and it connects directly to my study where an interviewee remarks:

[w]e read a tiny fraction of what is actually being published. So what we should do is to encourage people to publish less, but better stuff so that every single piece I have to read is actually really good. Because of the *pressure to publish*, people publish crap [...] (Vostal, 2016: 112; emphasis added).

Another, more pragmatic critique of acceleration and temporal structures by Berg and Seeber also stands in conceptual relation to Rosa (without explicitly building on his theory). Berg and Seeber describe the pressure to write and publish as key stressors which become a force on their own that eventually harms an individual's ability to write in the first place (2016: 86–87). The phenomenon in comparison here is that of an imposter syndrome which hinders a scholar from conducting her work well just because she thinks she is not competent enough for this work. Berg and Seeber offer some anecdotal evidence but do not conceptualise publishing and social imperatives in more detail. Nevertheless, their work is a lasting account of what they themselves call a culture: that is, a culture of speed.

The (Unintended) Consequences of Publish or Perish

A common theme in the literature and an often-implied premise is that *publish or perish* is perceived and passed on among members of scholarly communities as a *narrated* principle. Many of the research articles try to track the origins of this principle and its underlying or consequential practices that led to the existence or further development of a publishing imperative as a perceived rule with social force. Next to this, many scholars writing about *publish or perish* identify unintended consequences—especially those that lead to further worsening of the imperative—as its key danger. This seems to confirm that *publish or perish* is reified and accumulates reflexive force, making it necessary to situate and analyse it in a complex of interwoven developments, instead of as having merely a one-dimensional, linear progression.

For instance, van Dalen and Henkens study the unintended consequences that arise from the pressure to publish in a new productivity regime, characterised as ‘when the indicator of scientific productivity is measured only imperfectly and may crowd out other duties that are traditionally ascribed to academic institutions’ (2012: 1283). First and foremost, the authors find that the perceived pressure is higher in Anglo-Saxon countries than it is in Western Europe or countries with emerging economies. And among scholars who perceive such high pressure, the impression seems to persist that among its consequences are high amounts of published but unread texts. Another consequence of the publishing imperative seems to be that it indeed leads to scholars focussing more on refereed publications than on connections with policy makers, and that, because of this, their research feeds only into systemic reproduction rather than being applied to problem solving. This is most indicative for the sciences but can easily be translated into the humanities, where the concern is less facts for policy-making, but meaning and culture in

an accelerating and disintegrated society. A similar account of unintended consequences can be found in the anecdotal evidence of Neill (2008).

The many negative consequences on the integrity of research conduct stemming from a *publish or perish* paradigm are explored in a variety of ways. This is most significant for the sciences, where the problem of unpublished null results and publication biases are ever-looming problems. A connection of such a (pre-publication) bias to systemic publishing practices can be found, for instance, in Fanelli (2010). Katchburian suggests that researching and publishing has developed into a productivity regime in which taking risks and implementing a long-term focus are not desirable because of the constant need to publish (2008). Roland finds deteriorating practices in the presentation and discussion of research through articles that, she claims, are gaining ground because of a deterioration of academic culture itself that is less committed to and concerned with scholarship than with external assessments of performance (2007). Ryan criticises the evolving practices of gaming publications towards quantity which tends to lower quality or does not even offer new, original conceptions of research (2005). Lauf concedes that the developing publishing imperative to internationalise and orient publishing at rankings leads to questionable practices and a potentially problematic idea of quality (2001). Similarly, van Wesel analyses a variety of practices of authorship that arose out of the need to publish more and in high impact media, and this amid cases of serious research misconduct (2016). Practices such as publishing least-publishable units (the *salami-slicing* practice), added authorship (multi-authorship publications), or strategizing research to yield high output have been referred to as standard in the sciences already decades ago (Angell, 1986). It has to be admitted that within the literature concerning the publishing imperative, comprehensive surveys of proposals to rectify practices are far less visible than the critical accounts, which reinforces

the impression that—while some raise their voice about it—suggestions for a different future are no easy task due to the complexity of the problem.

Kun provides a comprehensive analysis of the conceptual dangers a publishing imperative and its underlying false incentives consequentially pose to institutionalised academia (2018). His core argument is that the ‘pressure to produce eye-catching results, which are publishable in prestigious journals, [...] undermines the integrity of science’ (2018: 2), to which he offers several premises that evolved this pressure. Among those are the competitive work environment due to short term contracts (the academic precariat), the reputational scarcity induced in a generally collaborative system, or the reliance on branded media and their prestige. He finds the most devastating consequences for early career scientists and that a solution against further systemic deterioration requires collective action. Though his analysis of the premises provides a solid ground for more in-depth exploration, he fundamentally disregards the inherent social dialectic that a structural imposition such as *publish or perish* must have.

More balanced is Bunz’s study of publication patterns in the field of communication studies. She finds confirmations for some productivity standards among faculty with higher ranks, but eventually concludes that those forms of productivity are only one form of performance. Reflecting this for university or research policies, other forms need to be recognised and incorporated as well.

Whether a scholar is to be considered prolific or to be having an impact on the discipline ought to be a very person-specific evaluation. Developing promotion standards for publishing that implicitly threaten faculty with “perishing” may not be the most effective long-term strategy (Bunz, 2005: 719).

It seems to be trivial, but such obvious advice was necessary 15 years ago and is so today.

Publish or Perish as a Result of Structures of Reward

Articulations of a formal rationalistic culture are practices concerned with reputation and reward. This is true for individuals and institutions as well as across countries and disciplines.

According to Rond and Miller, *publish or perish* simply 'signifies the principle according to which a faculty member's tenure is primarily a function of his or her success in publishing' (2005: 322). They assert this principle gave rise to the primacy of efficiency and productivity, which led to a race against time to churn out publications. Rond and Miller question whether scholars 'compromise on ideals' (2005: 322) because of wrong publishing incentives and, thus, contribute to their irrelevance. Consequently, the authors offer countermeasures that may help foster quality-based evaluation practices to prevent a further advancement of a publishing imperative; measures which may free scholarship from 'any epistemological and methodological hegemony' (2005: 327).

Similarly, Mathews studies the significance and viability of finding a third option, thus, rhetorically, leaving the dichotomy to either publish or perish behind by offering alternative research publishing procedures by means of an independent panel of referees (2007). Though this may work in a hermetic testing space, such a procedure may be unlikely to be realised comprehensively since no journal can simply be forced to work like this (since many are owned by private organisations) and an added mechanism of such referees would first and foremost raise the workload of academics to new heights.

Backes-Gellner and Schlinghoff provide an incisive comparative study of faculty productivity at US and German institutions in the field of economics (2010). They compare the reward structures available in the two systems such as tenure or professorship promotion and relate the incentive structures to productivity (which they define as

publications in high impact journals). Their quantitative analysis shows significant tendencies of scholars publishing more before a promotion decision is to be made, leading the authors to conclude that ‘the increasing dominance of “research prestige”-oriented reward systems enforces and accelerates the well-known publish or perish process’ (Backes-Gellner and Schlinghoff, 2010: 47).

An analysis of much more direct rewards of published research can be found in Quan et al. (2017). The actual cash-per-publication policy implemented in China has led to a profound realisation of *publish or perish* in probably its most devastating form. Though, on the one hand, it led to China being a much more visible and recognised scholarly performer in the context of international research; on the other hand, this came at the cost of *performance* being considered instrumentally where publishing productivity and prestige are the primary (rewarded) gains. An earlier comment by Ding predicted the resulting publishing imperative to become reality for scholars in China (2001), while other studies connect this imperative to potential research misconduct (Qiu, 2010) and the negative consequences it has on young scholars based on the imperial structures of prestige knowledge dissemination (Tian et al., 2016).

The Publishing Imperative in Relation to Teaching

Another key theme in the literature on a publishing imperative is the bias of measurable publishing output against teaching. Though the works concerned with this theme do not make a specific case for the humanities, the critical premise underlying this theme is especially important for the humanities. This stands in ideational relation to essential enquiries about the nature and value of the humanities which I discuss more in chapter 7.

Vannini asks whether scholars feel authentic either at teaching or researching (2006). He builds on a social conception of authenticity of the self where, in specific situations, one is 'feeling true or congruent with one's self' (2006: 236). Scholars are caught in practices shaped by managerial culture such as *publish or perish* (or *get grants or perish*) but succeed in negotiating the competing values that arise between systemic and personal approaches and objectives. Regarding the imperative to publish, for instance, this may mean that '[i]f an academic [...] is committed to her identity as a teacher she will dedicate herself to teaching even though such authenticating course of action may endanger her career' (2006: 254). The scholars observed reveal that an attitude of direct connection between student and teacher, in-depth explorations during teaching, and value-based approaches (instead of teaching for the sake of exams) are favoured. The underlying principles, however, have always to be defended against funding regimes and the questioning of the value of the humanities in the first place. Teaching is not itself experienced as a burden, but may be made to be perceived as such, because structural forces play research (measured through publications) out against teaching. This is conceptually close to alienation.

In the study conducted by Miller et al., a circular reasoning was tested which also provides insights into the teaching and research balance:

[t]he majority of respondents (53.6 per cent) believed that teaching detracts from being able to publish in peer-reviewed journals. Less than half (39.1 per cent) of the respondents, however, believed that pressure to publish in peer-reviewed journals detracts from their teaching' (2011: 434).

Boyes et al. similarly analyse a perceived imbalance between teaching and research (1984). Primarily, they propose a divide between size and purpose of institutions where the primacy of research (being assumed as the basis for a publishing imperative) is said to be strongly emphasised in research-focussed institutions. Problematic in this analysis is that the possibilities and difficulties of assessing research by means of publications are mostly

left unexplored. The researchers' conclusive remark that research and teaching should go hand in hand seems to have no impact on their premises of what publishing might entail.

In his critique of a productivity regime in the humanities that is biased towards valuing book publications only, Savage also makes the case that publishing is preferred to teaching, calling the latter 'teaching-as-punishment' which the unproductive scholars will have to face: 'whereas insufficiently productive junior faculty may be denied tenure and promotion, senior faculty may be threatened with loss of sabbatical leave or with a heavier teaching load to compensate the school for their failure to publish enough' (Savage, 2004: 44–45).

(Scholarly) Self-Help Literature

Next to the scholarly investigations and critiques of *publish or perish*, there are quite a few books and articles in the form of self-help literature that aim to provide tips to the struggling but aspiring individual for how to get published or deal with a publishing imperative. Among others, there is Lussier whose guide proposes 100 tips to improve one's ability to get published (2010). The curious readers of his will find his list of publications enticing, which Lussier, professor of management, offers on a 70-page long CV: as of 2016, no less than 437 publications are listed (Lussier, 2016).

The extent to which such a book is damagingly suggestive and symptomatic for the cause of the problems it sets out to offer solutions for can be read in a review article, in which McNulty states that 'this book is aimed mostly at doctoral students and early career researchers just getting started' (2013: 238), in other words: those who are assumed to be in need of a toolbox of strategies to game the system in order to make a career. Further into the book, the readers are offered advice in how to get 'the most number of publications

out of one manuscript or one data set' (2013: 239). In this approach, publishing has primacy over scholarship.

Other examples in this category of self-help literature are Glatthorn (2002) or Hills (1987). These books appear as if they were written to empower scholars in an institutional environment that thrives on a competitive, formalistic culture. Instead, what they are mostly doing is to reproduce some of the causes for the existence of this formalistic culture in the first place. Building on the need to churn out ever more publications, indifferent to the scholarship and discourses these should entice, these guides disseminate the principles of alienated scholarly communication. Thus, they contribute to the reification of the publishing imperative and the impression that there is no alternative other than to align and compete formally. Hills' edited volume is strikingly indicative for this sort of imprudent approach, where Bertrand writes in the first chapter that '[i]deally he [the author] would like it [the research] to be published in the most prestigious journal possible, because this would maximize the benefit to him by the fact that *his peers would thereby view it as a quality article*' (1987: 12; emphasis added).

A different study of *publish or perish* among academic economists employed at German-speaking research institutions seems rather to translate rising quantitative output with the notion that rankings of branded media reliably indicate quality. The study was conducted by Graber et al. and led to the establishment of the (now closed) website HowToGetTenured.de where aspiring scholars could research their potential for a tenured position based solely on their publication record. Lacking any critique of the practices dealt with, the researchers even go as far as to coin the *personal quality index* that provides an individual with an estimate of some form of quality by indicating the names of the journals in which reports of this research have been published in (Graber et al., 2007: 21; see also their study: Graber et al., 2008). It is unclear why quantifiable and categorizable statements

necessarily lead to performing as predictors. But it may come to no surprise that such predictors arise out of a discipline that is said to suffer from its lack of heterodox thinking.

In a similar category is Harzing's effort to be placed, who aspires to help scholars with more than just (good) advice. Harzing is 'the driving force' behind the publish or perish software that 'is designed to help individual academics to present their case for research impact to its best advantage, even if you have very few citations' (2019). An early reviewer of this software claimed that Harzing 'has provided a powerful tool which readers can use to do their own analyses and form their own judgments' based entirely on data from Google Scholar (Bensman, 2011). In order to get the most out of the software, Harzing fosters self-help with additional literature, for instance, *The Publish or Perish Tutorial* (Harzing, 2016), which teaches scholars to regard every search practice through the lens of citations.

Clapham's longer comment can likewise be counted as a form of self-help literature in that he tries to convince both novice and tired scholars to publish lest their knowledge ceases to have any purpose: he apodictically states that 'if you don't publish, you're wasting everyone's time and taking much-needed funding away from other scientists' and shortly after concedes that publications 'say that you are serious about research, and can take the scientific process all the way through to completion' (2005: 391). Undeniably, Clapham's probably well-intentioned advice hits the correct tone as long as one sees *publish (or perish)* to be *good* advice. That a different reality would be possible does not appear as an alternative here, though.

2.4 Conclusion

One can find many more publications that are concerned with the pressure to publish. Even more so, criticism of the contemporary approach to authorship and publishing can be identified at the roots of whole discourses such as open science (or open humanities). As well-intentioned as those discourses on openness and transparency are, they may not be suited as sustainable solutions or even analyses of underlying problems. I come back to this category of literature in chapter 10. As an overview of the key works in the realm of the publishing imperative, the here-included literature may suffice to give an impression of the complexity of the topic and the variety of meanings accorded to *publish or perish*. To be sure, articles from sources such as *Scholarly Kitchen*, the *LSE Impact of Social Sciences Blog*, or *Times Higher Education* may likewise include perspectives on the imperative to publish. Articles such as Anderson's on the value of publishers (2018), Warren's on publishing workload (2019), or Rudy's on the costs of being an art historian (2019) can be revealing about ideology or practical implications within anecdotal evidence.

What this body of literature also shows is, though mentions abound, *publish or perish* is often not related to specific culture principles of academia. The many self-help guides and the empirical studies on the imperative to publish are more than indicative for the reification at work where the arguments and ideas count less than the formal publication and a person's competitive attitude (see especially: Kelsky, 2015). The social forces that such culture principles may produce and evolve are rather rarely investigated. This is the key gap that I aim to fill with my thesis.

It should not be forgotten in this review that there is a special *publish or (and) perish* theme in fiction literature where its alliterative allure is potentially more important than corresponding practices: there are thrillers, set in the contemporary academic milieu, where one offers *Three Tales of Tenure and Terror* (Hynes, 1997), while another traces the work of

two scholars researching Victorian era literature (Byatt, 1991). The upside of this category of literature shows scholars a perspective where *publish or (and) perish* can even be worse: not just a paradigm detrimental to scholarship but indeed a life-threatening horror scenario.¹

Further to the works offered in this chapter, there is the vast amount of literature, usually located in the disciplines of sociology and philosophy, that looks at assessment and evaluation procedures, discourses, the academy and its subjects in general, or contemporary academic practices as by-products of modern society. Names of especial importance here are Bourdieu, Merton, or Münch. With works such as *The Specificity of the Scientific Field* (1975) or *Homo Academicus* (1988), Bourdieu has provided a critical analysis of scholarship and its practices and principles. Merton has accomplished a similarly impressive analysis of the social structures of the scholarly field which provide key terms to refer to essential mechanisms until today (such as the self-fulfilling prophecy: 1948; or the Matthew effect and the 41st chair: 1968). And as a contemporary critical sociologist, Münch has provided among the most comprehensive assessments of German academia, incorporating crucial comparisons to the US and the UK (2007, 2008, 2011). The principles and practices explicated in these works loom over modern academia as do the works of foundational as well as contemporary social theorists such as Weber, Giddens, or Rosa. To dissect their theories, and the many others included in my analysis, would go beyond what a literature review is supposed to do and would also provide a disintegrated account of the theories used and where they are applied in my thesis. These works are, therefore, not dwelled on in more detail in this chapter, but will find their position in the relevant chapters of the ensuing analysis.

¹ There seem to be more novels building on this theme such as: Blaisdell (2019); Castle (2017); Kinberg (2008). However, I have not read these and, thus, cannot say anything about their substance.

3 Theory and Methods

In this chapter, I present my theoretical grounding, including a justification of, and my path to finding, a suitable theoretical approach to studying the problems at hand (3.1-2). I then present my research design (3.3), before going into details about my two empirical research methods: exploratory survey (3.4) and qualitative interviews (3.5).

3.1 Understanding Action and Culture

At the core of this thesis is the question of the meaning and motivation of action: why do subjects act in a particular way? The meaning of action and its motivation may not be best comprehended when looking at the *doing*, though, but by looking at the underlying principles that connect a collective's corresponding *doings* and provides the individuals within such collective the necessary meaning. In this sense, I required a theory that helps me understand that which underlies action, to go beyond psychological states of decision-making, so to say. In this sub-chapter, I explain why I recognise practice theory to be optimal for doing so and how I arrived at this conclusion.

Meaningful Social Action

Ever since Durkheim theorised meaningful social action and approached the empirical world with this theory, sociology as a scholarly discipline is at its core concerned with ways to make sense of this: how can action be systemically interpreted, understood, and related to something not immediately tangible, a deeper structure, some form of meaning emerging out of interaction or culture that has a determining effect on action (Durkheim, [1897] 2006, [1912] 2008; for an overview of Durkheim's disciplinary

legacy: Smith, 2020). Moreover, as is visible in the transition from Comte and Durkheim's positivism to Weber's interpretative approach, how can this scholarly endeavour be achieved and justified in a non-positivist sense?

Since Durkheim's landmark outline, a variety of theories and conceptions have surfaced that all claim to explain action, meaning, agency, structure, or social facts from different perspectives, demanding recognition for being able to uncover some form of truth. Structuralists emphasise the central role of structures, as system theorists do for systems and their functional differentiation. Cultural sociologists see culture at the core of analysis, which practice theorists connect to practices. For symbolic interactionists, social analysis regards direct interaction as the key to unlocking knowledge while agent-network-theorists position subjects as nodes within networks at the centre of analysis. Neither of them would reasonably deny the existence of core elements such as culture, material structures, or routinised actions. But these theories' epistemologies demand that for some theories, some elements are given primacy over others. None of these approaches may be *true* than others: they simply offer diverse explanations and frameworks for interpreting different empirical phenomena. When I started this PhD in 2017, I was not yet decided on the specific approach and, thus, dived into sociological theories to determine the optimal framework for the empirical problem I was (and am) looking at.

From Interactionism to Structuralism

Since I am located in a department of IS, this is perhaps a good starting point for explaining my theoretical journey. I regard IS as a dualistic field, on the one hand driven by technology and, on the other, driven by a social perspective. These two perspectives

are always interrelated, but scholars usually give primacy to one in the process of studying the various ways of seeking, retaining, retrieving, archiving, or otherwise handling information and knowledge. I reject technological determinism: where materialist conceptions of power or history give primacy to artefacts or, more concretely, discussions of OA look at technology as a means to solve a more social impasse, I have strong doubts about such conceptions (I discuss technological determinism in regard to OA more comprehensively in: Knöchelmann, forthcoming). As I clarify below, practices surely do not exist without artefacts in interaction; but in my view, the social always precedes technology in giving it its meaning and purpose.

All the while, though I reject technological determinism, I still do not want to fall for an idealist conception either. I therefore searched for a balanced approach, a way of connecting action with some form of structural meaning, one that emphasises the connectedness of agency and structure. Symbolic interactionism would be one such way. Primarily theorised by Mead (Mead et al., [1934] 2015), symbolic interactionism conceives society, actions, or the self to be both physical and symbolic, and that subjects reproduce the symbolic components through their repeated interactions. This approach draws on phenomenological sociology, particularly Schütz's conception of intersubjectivity (Knoblauch, 2009: 304). Goffman's conception of interaction as dramatic performance is rooted in this theory (1956), as is such insightful contemporary scholarship as Hochschild's on the interconnection of emotion, action, and political and economic structures both in a micro (2012) and in a macro perspective (2018). This theory demands deep immersion and often temporally prolonged study of phenomena within social groups, requiring research methods such as ethnographies.

This interactionist approach is more radicalised in Garfinkel's ethnomethodology (Garfinkel, [1967] 1984; Garfinkel and Rawls, 2002). Ethnomethodology is a rather

theoretically-agnostic framework that is highly empirically driven. It was conceived as a response to structural approaches—especially Parsons’ earlier dominant functionalism—in that it is dedicated to the study of the practices of the production of intersubjective meaning in day-to-day interaction. Overarching culture structures are renounced—Garfinkel referred to the thought of culture-influenced subjects as *cultural dopes*—in favour of situational meaning-making (Abels, 2009: 88). Schütz’s phenomenology is influential here as well (Garfinkel was a student of Schütz): it is the intersubjective and situation-specific methods that make ordinary actions meaningful. The object of study is, therefore, the ways social facts are constructed and reproduced in a micro perspective.

Though such approaches have strong qualities for explaining meaning and interaction, they are very much micro driven, looking at emerging themes in interaction and phenomena. This makes them poised to miss deeper structural aspects of society or may even lead to cultural relativism where culture is but a strongly temporal and person dependent variable. This is epistemologically insufficient for my research where a strong, explanatory micro-macro link is a necessary condition.

Looking at the components of my field, practice theory seemed to provide a much more suitable basis for explaining empirical phenomena than interactionism, ethnomethodology, or forms of functionalism can do. Moreover, practice theory is a strong programmatic pillar within IS for it allows one to theorise a collective’s conduct vis-à-vis artefacts; in principle, just as a theory of scholars in interaction with publications requires. The importance of practice theory in IS is exhibited in the variety of its application in the field (see, for instance: Lloyd, 2005, 2006; Lloyd and Olsson, 2016, 2019; Moring and Lloyd, 2013; Pilerot, 2013; Savolainen, 2008; Wenger, 1998).

At the outset of my research, the idea of an institutional approach to practice theory seemed appropriate, so I developed the idea of a persona study: determining

generalisable scholarly persona based on their practices of authorship and publishing. Institutional here means that I also had a stronger perspective on publishers themselves. In essence, this is the task of most sociologies: determining the particular within generalisable collectives. However, I quickly dismissed this approach due to its simplification and, like interactionism, its lack of structural links. Determining generalised particulars in this way would have resulted in an overly descriptive approach where I would add analytical personas where there are natural personas already available. These newer, less artificial categories are pressure groups (I explain these in 3.4). For these reasons, I decided against personas and, by looking more closely at authors, decided to not take publishers as equal players in the field. This makes my theory stronger in its detailed argument about authorship instead of having thinner arguments on authorship paired with arguments about publishers. Some of the later stages of this decision-making took place while preparing the exploratory survey, so that *exploring* here literally means looking at options and determining the best frame for further analysis. To back up this interpretation with theoretical knowledge, I embarked—at the beginning of the PhD and ongoing throughout my research—on a strongly theory-driven journey that led me to a profound understanding of practice theory, its philosophical origins, and the both subtle and fundamental differences of its branches.

Engaging with Practice Theory

The starting point of my journey through practice theory was Giddens, particularly his work on structuration theory (1984), earlier outlines of it ([1976] 1993, 1979, [1981] 1995), as well as engagement with critics and companions (Archer, 1995, 2010; Craib, 1992; Giddens and Pierson, 1998; Jary and Jary, 1995; Leydesdorff, 2010; Lizardo, 2010;

Meštrović, 1998). This was accompanied by engagement with Bourdieu's work on praxeological theory and reasoning (1977, 1998). Though the latter provided the name for what was to become practice (or praxeological) theory, I was drawn much more to the former for its hermeneutical stance and the conception of the duality of structure. I followed the development and further detailed theorisations of practice theory along Schatzki's work (1996, 1997, 2002a, 2002b) and the more comprehensive conceptions of Reckwitz (2002, 2008, 2016b, 2016c, 2020a).

In short, practices are routinised social actions that are connected to discourses, artefacts, and subjectifications. All four components are influenced and shaped by cultural orders of knowledge. Culture is, thus, central, but at times lacks fuller theorisation in both practice theory and IS. This is why the *strong programme of cultural sociology* developed at Yale University became important for my theoretical grounding.

Culture, here, is defined as a relatively autonomous meaning-making and symbolism. This goes back to Weber's work in which practices as embodiments of culture is already visible in the social action as meaning-making ([1922] 1978: part 1, chapter 1). It is an always present variable that informs various aspects of social hermeneutics. These characteristics make for one of the subtle but fundamental differences to other theories: in the so-called *weak* programme—the sociology of culture as developed by Bourdieu—culture is a dependent variable; in the *strong* programme, culture is a relatively independent variable.² I relate to this at several instances throughout this thesis: the differentiation goes beyond the question of dependency and, rather, pertains to the question of the *relativity* of materialism and idealism (see, for instance, my discussion of the symbolic value of brands in chapter 10). And even to this

² For sake of clarity: if I do not relate either to the strong or the weak programme, I mean cultural sociology in general.

debate, we find contributions in Weber's work, which shows a rare balance of materialism and idealism. Exemplary in this respect is the famous switchmen metaphor in Weber's *Sociology of Religion* ([1920] 2006: 302): though interests are guiding forces for human action, it is world views based on ideas or intersubjective meaning that, like switchmen, determine the ways interests are actualised in the first place. To clarify the matter of culture, I explain the four key phenomenological areas through which it becomes articulated in the following sub-chapter.

3.2 Cultural Analysis with Practice Theory

Reckwitz issues the praxeological rectangular of cultural analysis, shown in figure 1, which posits to bring together the four relevant phenomenological areas: practices, discourses, artefacts, and subjectifications. Immanent to these four and centred in the representation are combinations of heterogeneous culture principles. Though culture is accorded a prominent position in the centre—showing how it influences all elements—the primacy of analysis is given to practices. In the following sections, I explain these four phenomenological areas in more detail in relation to my research.



Figure 1: Elements of praxeological cultural analysis (graphic created by the author, adapted from: Reckwitz, 2016a: 41).

Practices

Practices are routinised social actions. They are the centre of any praxeological analysis. As stated above and discussed by Schatzki in detail, they developed out of analytical structural properties and hold a strong sense of Wittgensteinian rule-following.³ This allows them to be conceived as coherent, institution-constituting, and structuring elements of social life. As a matter of social life, both practices and the institutions they constitute are influenced by, and influence over time, the principles inherent in cultures.

Simply put, a practice is an ‘organized nexus of actions’ (Schatzki, 2002b: 71). This hints at the two interlocking elements: actions and something related to institution that binds actions and enacts meaning and structure around them. Actions are the concrete performances actualising practices, and while this concreteness enables researchers to study actions vis-à-vis, the underlying elements of institution is the focal point of analysis. For without some form of institutionalisation and routinisation, action would be just a form of execution and not the actualisation of practice. This illustrates the often-confused terminology: operative action is praxis (as opposed to theory); a practice is the analytical category for explaining meaning, institutionalised action, or the affects responsible for why and how subjects engage in it. And as opposed to mere praxis, practices materialise as institutions in social life. They are social facts: practices determine how individuals act in regard to particular situations, artefacts, discourses, and so on.

In a more thorough definition of practices, Reckwitz connects these elements, showing the complexity in which practices are embedded. His definition sees practices as

³ Further on the aspect of rule-following, see the extensive discussions in Schatzki (1997: 291), Habermas’ excursus on Wittgenstein (1987: V.1), Wittgenstein himself, especially his private language argument ([1953] 1972: §202; §§244–271), or Kripke (1982: 105).

a routinized type of behaviour which consists of several elements, interconnected to one other: forms of bodily activities, forms of mental activities, “things” and their use, a background knowledge in the form of understanding, know-how, states of emotion and motivational knowledge (Reckwitz, 2002: 249).

Writing is the epitome as an example of culture and practice. Consider left to right, top to bottom, or the textual representation of argument and thesis, structures of texts, and reference systems. These come into existence through practices because of cultures and, through repeated actualisations of practices, influence culture, leading to a capillary differentiation of it (in the sense of an increasingly finer branching out). In a nexus with other practices: writing intertwines with the preceding research practices, and publishing and reading practices coming afterwards, constituting a bundle that may be referred to as scholarly communication. This is a common term that appears to provide straightforward meaning, but it also gathers within it a vast amount of diverse practices and, particularly important for my thesis, potentially conflicting principles arising from different cultures.

Epistemologically, it is crucial to differentiate here between a micro-macro-link of conflicting principles impacting the development or change of practices, and the ways practices appear conflicting, meaningless/wrong, or pressurising. As a phenomenological area, it is always the practice that is such attributable in regard to the individual; culture is *always mediated through* practices: the Western culture of writing comes about via practices of writing. Though individuals may give an account of a culture, a logic, an ethos, or such structural terminology, they do so as recognised *through* practices (or discourse about them). This is where the primacy of practices is most prominent.

As said before, analyses of action often come down to meaning and motivation: what does it mean to act in a certain way and why does someone do so? Meaning and motivation seem to presuppose each other so that one cannot be thought of without the

other. Historically, there is an interesting dispute of ‘teleoaffective orders’ (Schatzki, 1996: 125) which for a period served as the explanans of motivation within practices. This is now abandoned in favour of more open conceptions of affect (see, for instance, discussed in: Reckwitz, 2016e). In contemporary practice theory, the actualisation of a practice does not necessitate direct, subjective motivation, just as meaning is not as closely bound up with subjectivity.

Since practices gain much of their meaning through deeper generative principles—i.e. culture—searching for reasons to act is not necessarily geared towards a specific practice itself. This explains the shift of perspective from direct motivation to the constitution of a practice, for some form of affectual motivation is innate to it. This, essentially, cuts out the matter of subjective, psychological motivation from analysis. And this shift from individual action to social practice also demands a repositioning of the explanatory power of questions of motivation: *why does X do Y* becomes a question of *why is Y being done in this way repeatedly by the collective of which X is a member*. As *Y* is a practice, the establishment of this practice and its culture principles explain engagement with it. Thus, the reason for why *X* actualises *Y* is to be found within the practice and its embodied culture principles, not within some form of subjective motivation internal to *X*.

How does the feeling and perception of (negative) pressure fit into this conception? The discursive reporting to feel pressured to act in a certain way refers to a state in which affect is intoxicated or discordant (in regard to values or meaning). It effects the subject to *perceive* such practice with emotional hostility or alienation. Actualisation of the practice appears to be out of tune and, nonetheless, the individual pursues, since the motivational affect is rooted in the practice. Such affectual

discordance reverberates throughout my analysis under the categorical guise of alienation, which I further explain in chapter 8.

Discourses

I define discourse as a reference to the body of communicated utterances that, analytically, result in culturally meaningful or epistemically bounded text (including speech as text). This body always needs to be regarded as in interconnection with the discursive practices required for communication as well as the artefacts in which text is inscribed (or the (digital) space being the boundary of oral communication). Similar definitions of discourse abound in social theory, often differentiated or nuanced by the theory's corresponding ontology. The discussion on which I build my definition is that of Reckwitz who refers to discourses as, in summary, specific formations of practices that are forms of representation in which culture principles are structured and negotiated (2016a: 36–37).

Discourses are important in my thesis in two ways: scholarly communication, basically, is discursive practice; and *publish or perish* is as a (non-scholarly) discourse an articulation of culture. The connection of discourses and practices—both the way culture becomes inscribed in discourses by means of discursive practices and the way culture, through its embodiment in discourses, shapes practices reflexively—makes discourse an essential element of sociological analysis. But any such analysis always needs to abstract from the materiality of practices and artefacts to account for meaning and, all the while, retaining materiality and practices as essential for their existence. In other words, there is a categorical difference between content and the way content is brought to existence (or continues to exist); a difference that must not be thought of as an ontological dualism, though.

Discursive immateriality is discourse *eo ipso*: utterances as text and its social and cultural meaning structures isolated from artefacts and practices. Any utterance that contributes to the negotiation of something formally (time-space feasible: it needs to actually find interlocutors) and epistemically (it needs to be understood as a contribution to this discourse by interlocutors, or by those constructing a discourse reflexively) may become part of a specific discourse. This provides the analytic categories of (somewhat tautologically termed) discursive practices, discourse as meaningful text, and discursive artefacts. The ontological connection of these categories stems from the tradition of Foucauldian discourse analysis, articulated especially in *The Archaeology of Knowledge* (Foucault, [1982] 2007). Discourse is defined as ‘meaningful symbolic behaviour’ (Blommaert, 2005: 2); its analysis is a means for dissecting and critiquing the effects of power. I write more of Foucault in chapter 7 where I define discourse communities.

In contrast, textualist (or mentalist) approaches (e.g. the strong programme of cultural sociology) focus on textual immanence and symbolic meaning. They see, above all, regimes of signifiers in discourses (Alexander and Smith, 2001; Reckwitz, 2016d). This can be observed where Smith analyses symbolic justifications of war through ‘narrative structures within a civil discourse’ (2005: 3), or where Alexander, in his outline of a democratic theory, claims that justice is possible through civil solidarity ‘which [...] depends on the vitality of a fluent and provocative moral discourse’ (2006: 38). As is evident in these examples, just as the textual hermeneutic method of this branch of social theory requires, symbolic content of discourses is a category that is often given primacy over the way discourses come to exist. Practice theory remedies this by including the wider context of discourse including artefacts and practices.

Publishing practices and its reflexive constitution of authorship are largely discursive practices for they are those nexuses of actions through which scholars produce scholarly

discourse. The communicated, mostly textual resources—the content of an article, a book, a tweet, oral speech—are contributions to discourse in the form of inscribed artefacts—the article, book, blog post, conference setting etc. The inclusivity of this conception, its dependence on artefacts, and resulting from their combination, how certain practices and artefacts make discourses exclusive, is an important explanatory aspect of my thesis on which I elaborate more at several instances.

Next to profoundly scholarly discourses with their clearly defined borders such as through submission guidelines and epistemic practices, there are those utterances among scholars that contribute to other, more worldly discourses: about work, the office, lunch, or any other social context. As trivial as such non-scholarly discourses may seem, they can be significant for social analysis. Since subjects make sense of their life worlds by passing on remarks that, ultimately, signify culture principles, considering these remarks for analysis helps identifying culture. *Publish or perish* is such a discursive articulation of culture passed on within communities of early career scholars (which I explain in chapter 9).

Artefacts

Artefacts cannot be thought of without practice and culture (Latour, 1993: chapter 4). Artefacts are the material elements required for subjects to engage in practices. And as practices are the nexuses of actions bringing about contributions to discourses, artefacts provide the materiality for those discourses. Most communication has a material artefact which is the basis for its spatial dissemination or temporal conservation (a conception with which I am indebted to Innis' theory of communication and power: [1950] 2007: 26–31). The materiality of artefacts is not restricted to physical things but

includes the digital realm as well. Moreover, most objects of study within IS, termed information, require a material base as an artefact.

Visibility is a prime criterion for media artefacts and, in response, symbolic value. These two in interaction are discussed in chapter 10: they are materiality embodying culture *per se*. By means of this, artefacts have affectual character (Reckwitz, 2016e). As artefacts are culturally meaningful, particular collectives are drawn towards or repulsed by certain artefacts. And it cannot be emphasised enough that this embodiment is to be understood as a reciprocity: there is no hierarchy giving primacy to either artefact or its cultural meaning; these are imbricated and cannot be unlinked.

It is, therefore, not so much pen and paper that are elemental as artefacts, but digital text processing machines and their connections of all sorts: word processing, online submission interfaces, citation managers, textual and bibliographic online databases, connecting communicative infrastructures, the seemingly endless versions of journal and ebook platforms, and many more. Only to a limited degree does scholarly communication depend on paper, where a monograph or journal is printed. Much more important for an analysis of authorship and publishing today are the artefactual differences arising because of the ubiquitous digital realm, such as Creative Commons licences, the inexhaustive availability of artefacts online, or the shifts of meaning between the traditional print to the digital: differences between discourses are suddenly less physical and more symbolic and processual. Layout and font become less impactful, maybe even irrelevant. They can be changed individually on screen. The symbolic elements attached to an artefact such as brands and editorial names are residual but key markers. Moreover, the value of a brand may be replaced by knowledge of availability: the distributive power of an artefact may not be an issue any more since every digital journal can be distributed anywhere. The new issue is instead: how do potential readers

get to know that there is a particular artefact available? This is an essential shift from availability to visibility. I discuss this in chapter 10.

Subjectification

The matter of subjectification is perhaps the least intuitive phenomenological component, but it is vital for analysis. As pointed out earlier, practice and subject constitute each other recursively—subjectification is the corresponding reference, describing the recursive forming of subjects. If an individual repeatedly actualises practices, thus adhering to an institutions' norms and culture principles, she becomes a subject as a bearer of those principles. The subject is, in this sense, the abstraction of lived culture. It is in this *becoming* of a subject that analysis ceases speaking of individuals (just as practices supersede actions). This distinction is vital since I do not analyse concrete individuals (scholars such you as a specific reader or I as the author) but scholar subjects (of which you and I are instances). Subjects are analytic categories of concrete social formations and, in this sense, quite concrete as well: in the end, being called scholars can have real impact for you and me. This may just be the reference to a material existence *as* a scholar—a professor, lecturer, or student with an office and a contract. But this is also a culture reference: if you appear on a radio show, being introduced as a humanities scholar, many a people trust you are an expert thinker, well-versed, probably possessing a respectable collection of books.

People mediate all kinds of references through culture in this way and it has a reciprocal effect on the individual. How such imprinting of cultures on individuals plays a fundamental role is most popularly exemplified by Bourdieu's habitus (2015: 52). While habitus is criticised for its totalising and overly static influence (Sewell, 1992: 15) or its

circular determination of and through material structures (Alexander, 1995: 136–149), praxeological subjectification presents a more dynamic conception today.

It is of interest here how the subject is formed generally as a bearer of culture principles. Reckwitz has put forward crucial new theoretical links between practices and subjects. In *The Society of Singularities* (2020b: chapter 1), he works out the differences between the particular within the general, idiosyncrasies, and singularistic individualisms; thus, connecting a *doing generality* and a *doing singularity* to culture principles. These *doings* come about via specific practices and lead to forms of subjectifications. While this is always the case in at least a subtle form, the nature of contemporary societal practices is that individuals strive for more radical versions which bring about singularities: the general subject in Western society strives for singularistic appearance in one form or another.

With *Das hybride Subjekt* (2020a; no English translation yet), Reckwitz laid out a comprehensive theory of the development of aesthetic subject forms in modern history more generally. He shows how subjects bear the mark of culture *hybridities*, meaning that they draw on different and potentially conflicting culture principles at once. This matter of *hybridity* is relevant in chapter 6 so that I elaborate more on this issue there.

The matter of subjectifications can also explain parts of my methodological development through my PhD journey. As I explain above: I had an earlier research approach to constructing personas. Such personas are, in a less theory-driven conception, comparable to the subject(s) that I came to look at (the humanities scholar in Germany and the UK). The development from persona to subject explains how I broadly retained my approach even though I adopted a more comprehensive theory and terminology. The scholarly subject is differentiated by means of its adherence to culture principles—the modern scholar turned research manager or the traditional

intellectualist—or by means of her more pragmatic existence in the field—levels of seniority which I relate empirically to experienced pressure.

Understanding Culture

The remaining theory element to be explained is culture. Culture is both embodied in practices and shaped by their actualisations over time. The same is true for artefacts and discourses. By implying their importance, however, I do not mean to replace culture with these phenomena. What is replaced is the notion of structure and a narrow interactionist approach. Reckwitz discusses three alternative denominations of culture (2020b: 52–57): the normative (culture as distinctive lifestyle), the differential (functionally differentiated cultural institutions), or the holistic (referring to the whole of human conduct and behaviour). He further suggests all three denominations are unsuitable to capture culture in praxeological theory. There is no analytical externality of culture, which would culminate in what I term cultural dualism: those theories where culture is hovering above all phenomena but remaining conceptionally external to them, as structures to be presented as distinct entities.

A praxeological analysis does not seek to reconstruct culture structures to identify its influence. Instead, culture becomes evident *within* the phenomena themselves. Differentiating between situated knowledges that are formed by and explicated in communities of practice and the deeper orders of knowledge must take place through practices, artefacts, discourses, and subjectifications. These are the sites of culture articulations, maintenance, or change. Thus, ontologically, the site of the social is practices but, through practices, culture and its local mediation is made visible. The site ontology employed in IS shifts in perspective from community and local interaction to

culture and local mediation. Epistemes and meaning are formed by a complex of interwoven phenomena, often presupposing each other both conceptionally and temporally. This complex influences subjects in potentially conflicting ways where different backgrounds led to the acquisition of inconsistent principles: cultures clash, values become contradictory, meaning becomes erratic. The terminological referent for this interwoven complex is hybridity (Reckwitz, 2001, 2020a).

If culture comes about via practices, its influence does so as well. This produces a terminological hiatus: where culture is not accorded an externalised site, reference to it becomes more complicated. Throughout his works, Reckwitz talks of orders of knowledge, culture logics or principles, and even processes of culturalisation. They may likewise be called schemata, social language, or interpretative frames. This only shows, however, that there is no definite culture construction and that reference to it needs to be adapted to the form of knowledge and meaning in question. I remain with the term *culture principles* for matters of consistency.

In conclusion, since artefacts, discourses, and practices are those entities embodying and articulating culture, no extraneous structure is required to account for it. There is no difference in hierarchy, culture principles are implicit in practices or discourse. On the contrary, practices are to be understood *as* the actualisations of culture principles—the very basis of practices or action is *meaning* embodied in *doing* (instead of the physical process of this doing itself), as Bubner shows across various approaches to social or cultural analyses (1982: 9–60). And this goes back to the foundations of social theory as Weber defines social action in terms of and relation to its meaningfulness ([1922] 1978: part 1, chapter 1)—thus, already a century ago, alluding to the matter of culture principles being articulated by generalisations of interaction.

3.3 Overall Research Design

After clarifying the basic tenets of my theoretical approach with some of its essential epistemological distinctions, how I turned with it to the empirical world needs to be further set out. I do so in this and the following two sub-chapters: first justifying my overall research design, and then explaining my two empirical research methods in detail.

Within cultural sociology, some theorists go so far as to posit a ‘methodological agnosticism’ (Reed, 2017: 36). This may be translated as: no justification of method can be enough, but no method may justifiably be excluded either. The scholar has to configure the hermeneutics of her approach throughout the study. A hermeneutics where, as Reed further explains, ‘methods can be used in a way that aids the interpretation and reconstruction of meaning rather than claiming to eliminate the problem of interpretation’ (2017: 36). The question of method is then a question of finding a representation of the empirical world—of practices, artefacts, discourses, and subjectifications—that allows for an optimal interpretation and reconstruction of meaning. Denzin and Lincoln rightly claim that ‘this world can never be captured directly; we only study representations of it’ (2018: 897). The best way to deal with this impasse is to choose the method most suitable to balance theory and empirical phenomena.

Outline of Case Study Frame

The reason for restricting the research to a cluster of scholarship—the humanities—has both practical and ideational reasons. Firstly, with the tremendous differences between clusters of scholarship such as the humanities and, say, the natural

sciences, it seems very unlikely to reach sufficient depth in an enquiry if it were not restricted to a particular cluster. Secondly, my research question seems to be much less explored in the humanities than it is in other clusters of disciplines. And thirdly, the humanities are still fundamentally shaped by an intellectualism that touches on an ideal of historical-hermeneutical deliberation (as I explain in chapter 7) which seem to be more fundamentally in contradiction with formal rule-following and governmentality than the sciences (which are partly responsible for technocratic rules in society in the first place). In short, the nature of scholarship is geared to paradigms that seem to be essentially disturbed in modern academia. If this is the case, and considering that the humanities and their paradigms can be explained very well by their discursive practices, this makes for a compelling case study: looking at the culture principles in contradiction that are embodied in authorship and publishing.

To give an example: if scholarship is said to be based on subjective and qualitative judgement of arguments in the humanities, how can it be justified that the communication of those arguments is to be judged by the overly generalising heuristics of branded media? Looking at the medical sciences, quantitative judgements seem to be more justifiable since subjective or qualitative judgement of arguments is less applicable here (without saying that the JIF is good practice at all). In turn, looking at the humanities, can the culture principles be determined that really work through publishing practices and, thus, raise the seemingly ever-looming imperative? By suggesting this comparison, I do not wish to advocate that the humanities are in any way *more cultural* than the sciences—the social ontology explained above suggests there is no such thing as *more cultural* since culture is the meaning-making always impinging on routinised actions. But the sciences are progressing much faster, while the humanities seem to be rooted

more in their traditionalisms. *This* is one of the contradictions that provides the compelling frame for my research question as opposed to doing so for other disciplines.

Furthermore, I decided to engage in this research using a comparative approach. Comparative means that I can delimit the scope of the research and, by so doing, strengthen it by focussing on specific geographical sites as boundaries for cases. Such focussed case studies are particularly valuable in that they allow for generalisations from specific exemplars that are representative because of—not despite—their unique characteristics. Thus, while Hays says, discovering uniqueness is the key purpose of a case study (2004: 218), this uniqueness does not mean knowledge stays within a hermetic boundary. Much rather, the insights found within a case can help substantiate abstract theory inductively. Variations can be determined by empirical comparison. As in the language of Flyvbjerg, cases provide context that is essential for understanding and, thus, it is the context of the case that shapes the connection of theoretical rules and actualisation in the field:

it is important for the development of a nuanced view of reality, including the view that human behavior cannot be meaningfully understood as simply the rule-governed acts found [...] in much theory (Flyvbjerg, 2006: 223).

Most research design guides align in what makes case studies a proper frame for research. Hays' uniqueness of the cases grounding its explanatory power may be read here. Others talk of the significance of cases for context or research question (Leavy, 2017: 149). In the end, the question is whether the cases are fit *for* comparison—being comparable regarding the parameter under study—while also showing this uniqueness that makes a comparison valuable—if the compared entities are very similar, the case study may just be an enlargement of the case instead of a comparison. Taking these

characteristics into account, ‘case study designs can build up very detailed in-depth understanding’ (Lewis, 2003: 52).

In my case, I decided to focus on Germany and the UK as the sites for my case study. The two countries are comparable in terms of sizes of academic staff, numbers of universities, or scholarly publication output (there are differences, but not as large as between, say, the UK and Iceland; I compare numbers in the following sub-chapter). All the while, they are unique in particular characteristics: German academia is governed by a tremendously conservative career system—the UK has a rather progressive but strongly hierarchised career system; Germany shows efforts to manage excellence nationwide with only few institutions and a minor direct impact on publications—the UK’s efforts to manage excellence seem all-encompassing and are intricately intertwined with publications; German humanities scholars are very much culturally bound to a (Humboldtian) traditionalism that is fundamentally impacted by new governance practices and demands to internationalise—the UK has never been so strongly bound to Humboldt and, with its native rooting in academia’s lingua franca, English, scholars seem to be better equipped for the internationalist drive. This list is, of course, just an abstract; I go into details about these in the chapters 5 and 6. The characteristics make these two countries a good choice for an effective case study: effective particularly because of their uniqueness. This uniqueness is what I aim to capture in their cultural terms found in authorship and publishing.

Conclusively, these case study frame helps explain variations of the grounds of a publishing imperative which allow for a deeper understanding of its actualisation. Therefore, I do not endeavour to explain in-depth the two countries, their academic systems, or their entire publication outputs. Rather, I employ the two countries as mirrors of particular principles, explained theoretically as rationalisation, to determine

their working in reality. This is best seen in chapters 5 and 6 where I study the matters of excellence and NPM in detail in the two countries, respectively. And from a pragmatic point of view, besides these categorical characteristics, my proficiency in both the German and English language as well as my first-hand knowledge of the two academic systems allowed me to have an ample grasp of the empirical situations in the two countries.

Between Quantity and Quality

How do I look at the cases methodically? Qualitative and quantitative research methods are said to form a dichotomy, each with its distinct epistemological and ontological concerns, and providing complementary rather than contradictory strategies (Snape and Spencer, 2003: 15). In a narrow sense, non-qualitative methods are impossible, particularly if the scholarship is concerned with a social perspective. All quantitative research methods presuppose the qualitative definition of scope, field, or epistemology.

Talking of quantitative methods is an indicator for a focus on statistics, but this inevitably results in qualitative statements in one form or another. The undeniable line that divides empirical research practices into repertoires of quality and quantity correspond to epistemological questions of scope and scale. Empirical research methods can be concerned with the statistical affirmation or refutation of hypotheses in principle (finding patterns and determining themes), or with a gradual understanding, testing, or identification of their qualitative embeddedness (as in: how does a theme fit into or produce a pattern?). The former presupposes to reach a larger scale so as to achieve

statistical power; the latter can show an indifference to scale and answer its question with individual—or a few individually meaningful—references of information.

The distance produced by abstracting social phenomena into statistics illuminates the difficulty quantitative methods may impose on interpretative paradigms, such as the one I employ. Statistical information needs to be handled with caution in the process of theorising social issues for it tends to undermine or even disguise the intricacies of social reality. However, for exploring the prevalence of certain phenomena, they can be well-suited to inform and support a qualitative approach. I align in this argument with Schatzki who sees quantitative data as a valuable informant, while also emphasising the imperative of hermeneutics:

[c]omparative statistics [...] are conceptually impeccable when the categories of things measured unambiguously apply to the societies or peoples compared [...]. The possession of statistical information, however, does not substitute for understanding social affairs (Schatzki, 2002a: 26).

In this vein, I aimed to combine statistical information with information of a deeper understanding of social affairs. This would allow me to establish the prevalence of issues and social facts in a broader scale, without compromising on understanding the meaning and intricate working of these social facts.

The balancing of these pieces of information was based on a process of triangulation as a means to ensure validity and conceptual coherence (Maxwell, 2012: 102–106). The first step was to embark on an extensive review of literature. At first, this was much less coherent than the literature review chapter may suggest. This chapter is, more or less, a condensed version orbiting around core themes in their various guises. Establishing more theoretical foundations regarding social theory and the origins and substances of the humanities have likewise been part of that early process.

The following two steps of triangulation have been based on two different approaches of primary data gathering which combine statistical with qualitative information. As a subsequent process, the first step informed the other. The hypotheses to be tested by means of statistical surveying are based on the reading of literature. The themes enquired in the following qualitative interviews, then, have built on both the literature and the patterned insights identified in my statistics. Connecting statistics with qualitative data within my qualitative, interpretative approach meant that the research would gather more strength in positioning the central issue in the wider realm of the research—humanities disciplines in the UK and Germany. All the while, I would not have to rely on the ambiguity that always inheres in answering formal questions in a fixed survey and so, I would not have to compromise my analysis on the meaning of narrow statistical items. This resulted in the two-stage data gathering, starting with a quantitative survey followed by qualitative interviews.

The temporal-conceptual stratification of this process also means that my approach is not mixed methods in its narrow sense. Mixed methods research (MMR) means that at least each one set of qualitative and quantitative data is combined to answer an integrated research question (Leavy, 2017: 164). There may be more loosely conceptualised traditions of MMR, where, for instance, already the combination of different sets of methods for gathering and analysing empirical situations make for MMR. However, the *integration* of the two sets to answer a singular research question is usually considered essential. This is not the case in my thesis.

Though there is one overarching research question, the two sets of data serve different purposes in the sense of triangulation. The statistical method, and among statistical research methods the survey, was chosen to explore the topic widely, determining prevalence in scale. Explore, here, means that I aimed to answer crucial

questions that arose from the theory, especially concerning what the humanities are or whether pressure can be found on all levels of seniority, in both countries, or connected to third matters. That those issues have not been manifest in the literature made the exploratory nature of this survey a necessity. It can, in this way, be seen as a pilot study to my qualitative research. There was no pre-defined field of the humanities and it was unclear who would respond if I had asked within pre-defined heuristics such as *publish or perish* or specific structures. This informed my research process: instead of just contributing to an analysis of different datasets, the insights of the exploratory survey would direct my qualitative research method. This qualitative method was chosen to identify meaningful details behind the patterns indicated statistically. Both methods led to insights that helped me answer my research question; but they did so separately and subsequently.

Gathering Qualitative Data

While the use of an online survey to gather statistical information from geographically dispersed scholars would be an optimal quantitative method, the mode of enquiry for my gathering of qualitative information was less determined at first. As Denzin and Lincoln write about conceptualising qualitative research, it is

a situated activity that locates the observer in the world. [...] It] consists of a set of interpretive, material practices that make the world visible. These practices [...] turn the world into a series of representations, including field notes, interviews, conversations, photographs, recordings, and memos to the self (Denzin and Lincoln, 2018: 43).

Qualitative research methods are well-suited for dealing with the interpretative nature of my approach, but there is a broad variety of methods to choose from. They range from observations and narrative enquiry to interviews, (auto)ethnographic

methods, and those based on works and processes of art. Most of these methods are founded on inductive reasoning, generating hypotheses about the world by connecting individual phenomena and their meanings. Ethnographic approaches or interviews seemed particularly appropriate for my purposes and in relation to my theory. The former would allow for a deeper sense of meaning, while the latter would allow for a stronger case of comparability. However, considering the practices studied, the dispersed acts of writing and publishing, an ethnography seemed less well suited.

It is '[t]he interview [that] seeks to understand the meaning of central themes of the subjects' lived world. The interviewer registers and interprets the meanings of what is said' (Kvale, 2007: 11). Having less depth of meaning as in an ethnography but understanding and interpreting the *central themes* seemed appropriate for my task and, therefore, interviews were established as a suitable second research method. It remained a question of conducting interviews with several participants available at once, or individual interviews. This last methodical variable was resolved by looking at the crude logistical and thematic difficulties group interviews would pose. The former involved the quite obvious difficulty that it seems highly unlikely to find time slots to gather different (senior) scholars for group interviews. Thematically, this would have risked the openness of interviewees about the object: talking openly about pressure and a potentially defective system requires a private atmosphere in which individuals do not feel the social pressure to adhere to norms that may arise in group interviews. And so, I settled on conducting individual, qualitative interviews.

In the following two sub-chapters, I explain in detail the two methods that I chose to base my data-gathering on: first the exploratory survey, and second the qualitative interviews.

3.4 Exploratory Survey

Survey Design

For the conception of the survey, I relied on both my own experience in handling and taking part in surveys as well as literature on research methods explaining the subtle differences to avoid mistakes (in particular: Bethlehem, 2009; Bradburn et al., 2015; Gideon, 2012; Iarossi, 2006).

I decided on an online-based design that is open to participants from various backgrounds as well as being easily accessible. I looked at different online tools to facilitate such a survey and settled with Google for practical reasons: it is free, secure, highly adaptable, and allows itself to be incorporated into other websites. The last point was important because of the simple reason that it looks more professional to have a clear and simple landing site, with a direct and simple link (other than a shortened and apocryphal survey link that may look like spam). Therefore, the survey was accessible both on its original Google survey page as well as an iframe embedded in my personal homepage.⁴

Being exploratory in nature means that I wanted to explore phenomena in the field without having a clear sense of their prevalence. Based on the reviewed literature as well as experience with the humanities as a scholarly cluster, I sensed certain aspects, but often these had been assumed rather than tested. Exploratory, then, translates into the fact that a significant part of the survey enquired representative data about phenomena, such as amounts of articles or monographs published, or language used. Besides these, there were hypothesis testing questions regarding perceptions or felt pressure.

⁴ Original link, now closed: www.lepublikateur.de/survey

Exploratory also means for these questions that it was unknown whether the hypotheses to be tested indeed had significant statistical relevance. I had a set of hypotheses related to pressure to publish and a set of hypotheses related to assumptions about publishing practices in the humanities. I go into details about the hypotheses in the two relevant chapters about the humanities (7) and pressure to publish (9). Correspondingly, I divided the survey internally into two parts: determining aspects about the humanities as a disciplinary cluster and determining perceived pressure to publish. This internal division was not visible to the respondents, however. Screenshots of the original survey alongside a questionnaire reference guide can be found in the appendix (A3/4).

The opening page provided an outline of the purpose of the research, its timeframe, and requested audience as well as contact details. This page also included the consent form which scholars had to agree to sign in order to reach the actual survey questions. By so doing, scholars confirmed that they had read all instructions, that they understand their right to withdraw from the survey at any time, and that their responses will be used for anonymous analyses and potential publications. By agreeing to this, participants reached page two which included categorical questions about age, gender, time active as scholar or disciplinary background.

Being an exploratory survey, the conception of the request to participate as well as the questionnaire needed to be as open as possible. For this reason, I rejected the usual procedure of including only a list of checkboxes to indicate one's disciplines within a fixed set of options. This would have made the cleaning of the data and the analysis easier for the data would have been already fixed within categories. At the other end of the practice, rejecting checkboxes entirely would have led to a hotchpotch of various disciplinary referents other than humanities or *Geisteswissenschaften*. For that reason, I approached this matter with mixed means. I offered an open field in which I asked

scholars to indicate their discipline and afterwards provided a list of fixed clusters. As a result, the survey provides data about how scholars see their discipline fixed in a cluster, which is a valuable empirical companion to the theoretical explorations of chapter 7.

After the categoricals, I enquired mainly exploratory substance. These questions relate to a scholar's portfolio of publications, inclusion of OA, language, or non-traditional publication formats. I decided for some of these questions to ask for stratified answers to allow for easier replies. It is unlikely that scholars would know exactly what amounts of material they had published and so providing ranges (0, 1-9, 10-19, etc.) made these questions more accessible.

With page four, I moved to hypothesis testing questions, which all relate to perceptions, desires, or feelings. I mostly used closed questions with 5-step Likert scales: *Strongly disagree*, *Disagree*, *Neither agree nor disagree*, *Agree*, and *Strongly agree*. I have deliberately broken up these larger sets of items with Likert scales to diminish the risk of survey fatigue (such that a respondent merely clicks through the whole set of questions at one position of the scale). Such breaking up included non-scaled responses or the importance scale on page four, consisting of: *Not important*, *Slightly important*, *Considerably important*, *Crucial*, and *I don't know about this*. Within pages four and five, questions are mixed to partly disguise underlying hypotheses or break them into smaller units.

After having designed a first draft of the survey, I conducted a two-staged testing. This is necessary for identifying both internal problems such as question phrasing and external problems such as survey accessibility (Iarossi, 2006: 86–94). The first stage included working with a few colleagues (n~5) at the Department of Information Studies at UCL. This ensured having different backgrounds and academic levels to test the phrasing of the questions. For the next phase, I made use of fellows of the London Arts and Humanities Partnership to which I distributed the survey via email to await

responses and potential conflicts as well as by showing the survey directly (n~10). No substantial problems were reported during testing, which allowed me to move to data gathering.

Population and Survey Sample

The aim of this survey was to reach as many scholars in the humanities and social sciences in Germany and the UK as possible. As an exploratory data gathering, this survey should provide an idea of the prevalence of certain issues such as pressure to publish or perceptions of potential problems related to publishing across the diversity of scholars and career levels. After all, the inclusive design of the survey required an inclusive approach to data gathering as well. Next to clarifying the prevalence of certain issues, the survey should also account for disciplinary affiliations, making it necessary to keep the survey open without disciplinary discrimination between the social sciences or humanities. The population to be studied, therefore, was made up of all scholars in disciplines largely related to the humanities or social sciences in Germany and the UK respectively.

As a year of reference, in 2016,⁵ there were 19,653 scholars employed in humanities disciplines in Germany, and 21,425 in the UK, according to official statistics as shown in tables 1 and 2. Though the tables provide detailed accounts of those numbers, they are only comparable up to a degree. On the one hand, Germany and the UK have different ways of structuring the humanities. On the other hand, comprehensive data about academic positions across the UK and clustered by disciplines are not available; only statistics regarding contract salary as shown below are. That both

⁵ These datasets, provided in 2017, have been the most comprehensive source at the time of conceptualising the survey in early 2018.

academic positions and disciplines in the UK are accounted for in costs is telling in its own way. Germany's clustering is likewise rather broad.

	Professors	Non-prof. Lecturers	Scholarly Employees	Specialist Lecturers	Total
Geisteswiss.	4,631	341	12,276	2,405	19,653
Sozialwiss.	13,559	1,308	23,864	2,614	41,345

Table 1: Scholars in the humanities and social science in Germany in 2016. Data source: Destatis, 2017.

	Contract salary						Total
	< £18,412	≥ £18,412 to < £24,565	≥ £24,565 to < £32,958	≥ £32,958 to < £44,240	≥ £44,240 to < £59,400	≥ £59,400	
Cost centre of humanities disciplines	50	175	3,445	8,375	6,070	3,325	21,425
Cost centre of social sciences disciplines	35	335	5,760	15,815	17,330	8,620	47,895

Table 2: Scholars in the humanities and social science in the UK in 2016. Data source: HESA, 2017.

What these datasets show is that, above all, a direct quantitative comparison of the two countries based on these categories is bound to fail. The career systems and the way they are represented are not comparable and have internal problems as well: Germany centres around scholarly employees (also termed scientific assistant or *Mittelbau*). This institutional position allows for almost no information on career trajectories. Many careers begin and end in the *Mittelbau*. Moreover, because of the limited number of professoriates at individual institutions, non-professorial lecturers (the German *Dozent*) can be a dead end of a career, too. The UK's career structure seems much more progressive, albeit no less hierarchised. Scholars have the options to climb career ladders progressively and there are clear incentive structures. Designing a sample for these two populations required stratification, meaning that groups are used 'to achieve representativeness, or to ensure that a certain number of elements from each group are selected' (Dattalo, 2008: 5). Such groups may ensure that relevant shares of the population are captured. Introducing my own set of groups would safeguard a higher degree of comparability because of resemblance of group characteristics.

My initial sample design comprised of three levels of seniority and an additional group of tenured professors in Germany, as shown in table 3. It was in this step that I introduced the terminology of seniority to make career levels and trajectories, at least by contrast, comparable. The terminology obviously has its downsides for it evokes the notion of age. And yet, the tripartite division of junior-, mid-, and senior-level scholar makes for clear referents about positions in the system. These referents are neither clear-cut, nor bound to original career positions, but centred around comparable notions of levels. And as I show in my discussion of responses, these groups nonetheless come close to being referents to age.

Since this is an exploratory survey, meaning that the dimensions of statistical differences between disciplinary clusters or countries was unknown even by estimation, a calculation of statistical power to determine sufficient sample group sizes was impossible. Instead, I looked at other studies of institutionalised scholars (Ruiz-Perez, 2017; Teichler and Höhle, 2013), and tried to balance their sample sizes with feasibility for my approach. This resulted in an overall desired sample of at least 700 institutionalised scholars.

Germany		Junior	Mid	Senior	Tenured	Sub-total	Total
	Humanities	50	50	50	50	200	400
	Social Sciences	50	50	50	50	200	
UK		Junior	Mid	Senior		Sub-total	Total
	Humanities	50	50	50		150	300
	Social Sciences	50	50	50		150	
All:						700	

Table 3: Initial sample design.

Data Gathering

I aimed for a twofold process of data gathering to balance potential sampling problems due to framing or nonresponse biases as described by Bethlehem (2009: 209–245) or Hendra and Hill (2019). Such problems can arise if, because of the process of sampling, reaching the sample, and the response rate among the reached sample, the responses are skewed towards certain elements of the population. In the case of my survey, such skewedness could have meant overrepresentation of senior scholars or underrepresentation of one cluster compared with another. Nonresponse bias is among the most problematic issues of quantitative survey designs. Sampling errors can be dealt with quite easily, but errors not resulting from sampling can be more demanding as they are hard to detect (Dattalo, 2008: 7): they often remain opaque unless the survey design includes a detailed tracking of responses. I did not track opening or click rates of the distributed emails so that nonresponses have not been computed.

Another way of dealing with non-response errors is handling it pre-emptively. If potential response rates are known before conducting the survey, nonresponse bias can be reduced by conducting an adaptive survey which treats subgroups with known unresponsive biases differently (Schouten et al., 2016). However, such information about academics is not available, at least not to my knowledge. Finding reliable information about response rates as such is already difficult. There are a number of studies looking at the influences on response rates, finding a decline of response rates in general (Fan and Yan, 2010; Heffetz and Reeves, 2019; Hibberts et al., 2012; Liu and Wronski, 2018; Manzo and Burke, 2012). These suggest rates ranging from 13 to 95%, with outlines of appropriate levels of about 70%. Online surveys seem to fare much worse so that they tend to drag down the statistics in comparisons of different survey techniques. Comparably, the average response rates found in a meta-analysis 30 years ago were for

telephone interviews and mail surveys, 67% and 61% respectively (Hox and Leeuw, 1994).

The reality looks differently today, especially for surveys among scholars. One of the most comprehensive surveys of institutionalised academics in Europe was conducted by Teichler and Höhle (2013). They reached 17,745 effective responses in 12 European countries and claimed satisfaction with response rates of 10 to 36%. A study of OA attitudes conducted by Ruiz-Perez surveyed authors listed in Web of Science and achieved a response rate of 19% (2017). A major study of authorship values in tenure processes in the US received a response rate of 22% (Niles et al., 2020). This is in line with anecdotal evidence from scholarly colleagues complaining about the amount of surveys being sent out online and the burden of completing them that leads to survey fatigue—and so, to ever lower response rates.

To balance the likelihood of response bias in my survey—for instance, that maybe junior scholars are more likely to open survey invitation emails than senior scholars, or scholars in the UK doing so more than scholars in Germany—I also included learned societies and publishers as intermediaries to reach scholars. With such intermediaries, I introduced another path to the population for increasing the sample size and levelling out likely nonresponse biases. Therefore, my sampling frames—those indices from which I draw my sample—have been publicly accessible university staff directories for approach one, and learned society memberships for approach two.

Approach one builds on a simple randomisation after systematic frame selection, which reduces the workload but ensures that each group of my sample has the same probability of selection (like a lotto machine selecting random numbers). For the systematic frame selection, I gathered comprehensive lists of universities in Germany (from Hochschulrektorenkonferenz) and the UK (from HESA), from which I excluded

irregular institutions, universities of applied sciences in Germany, or institutions focussed on non-humanities/social science disciplines. I arrived at a sample frame of 83 in Germany and 134 in the UK. I deployed a number randomiser to determine 20 numbers out of each list of institutions. For these, I researched the relevant faculty websites for public email directories, the emails of which I compiled in Excel as lists with the columns name and email address. I deployed the same number randomisation to determine 50 randomly selected email addresses from these lists. This resulted in 2,000 email addresses overall, which made up the scholars to be invited via email directly, in smaller batches between April and June 2018. These emails included a short description of the purpose of this study (as included on the top page of the study itself, see appendix A3) and the link to the survey. I used the Outlook serial function within Microsoft Word to distribute the emails and personalised the text with the recipient's name.

The second approach is best described by a form of snowballing, though it actually is a mix of different methods: I used intermediaries to snowball, forward my invitations to participate. Firstly, I sent out requests to circulate the invitation to participate to learned societies. These included both major societies with hundreds of members (or fellows) as well as smaller specialised societies, all within the humanities and social sciences. For this, I contacted communication staff or other representatives of learned societies in Germany (n= 32) and the UK (n=33) across the two clusters of scholarship. I sent out the invitation to participate as an email attachment as well as a poster as PDF (as shown in figure 2). My intention here was to utilise learned societies as intermediaries and advocates to reach their members. The responses from the representatives were scarce. Some declined to participate, but most emails remained without reply. Some responded that they could not forward this via official ways such as the institutional

directory but could distribute this via informal channels such as social media or personal correspondence. One respondent stated they had forwarded the invitation in a JISC listserv for scholars in the UK (which I had not been aware of before). At another very large society in Germany, I found the invitation distributed via its email newsletter, potentially reaching thousands of members, though I had not personally received a reply from that society.

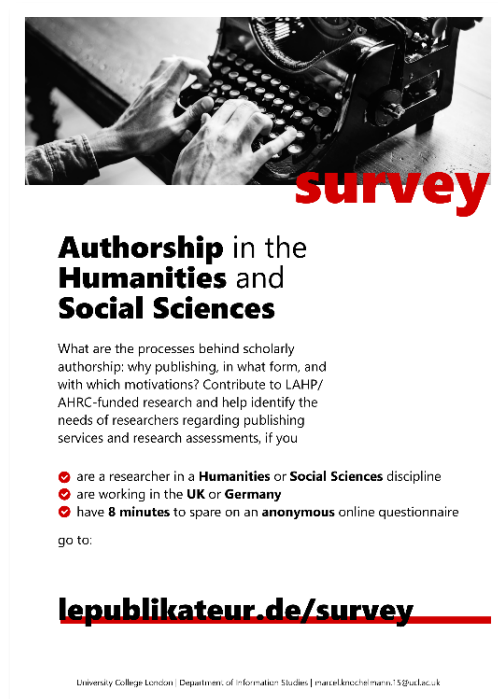


Figure 2: Poster invitation to participate in the survey; designed by the author.

Secondly, I tried to reach scholars by using further intermediaries, especially publishers. For this, I talked to university presses and smaller commercial publishers at the London Book Fair in Spring 2018, left a printed poster as a reminder, and also forwarded a digital reminder afterwards. My intention here was to reach scholars via editors from the publishing houses. With publishing houses as intermediaries, the idea was, scholars would be more willing to participate in the survey since publishers may advocate positively. However, the response rate from publishers at the book fair was

low. Most editors declined to participate, I assume, for obvious reasons of time and resource management.

Responses and Effective Sample

1,177 scholars participated in the survey, stemming from a variety of disciplinary and institutional backgrounds. I received responses between 23.04. and 27.07.2018. If I assume a response rate of 20-35%, similar to those in the cited studies above, about 400 to 700 responses may have come from direct invitations, so that about 700 to 400 responses have resulted from the snowballing approach via intermediaries. This, of course, are just estimations. Figure 3 shows the distribution across countries and gender. This is not equal, but both the HRK and HESA statistics show significant differences in distribution of gender between disciplines and career levels. This study could, unfortunately, not account for a detailed analysis of this. I do include some statistics about differing perceptions between gender, albeit not discipline-specific.

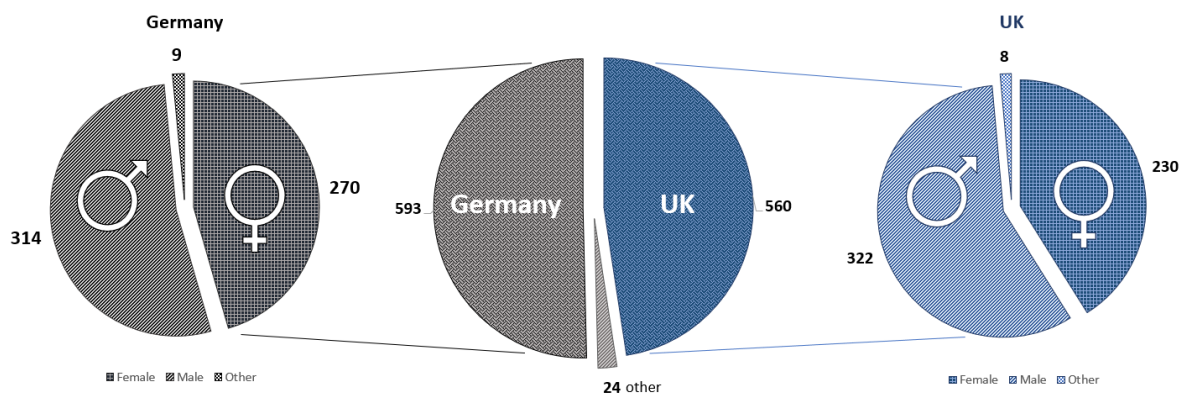


Figure 3: Distribution of participants in exploratory survey.

From those 1,177 valid responses, I excluded 160 as inappropriate for my study, resulting in an effective sample of 1,017. Those excluded comprise of 102 scholars from disciplines of engineering, natural sciences, etc. as well as 58 emeriti or non-German/non-UK backgrounds.⁶ Table 4 shows this effective sample, which includes a reconfiguration of sample groups for Germany.

This reconfiguration is based on answers in the item about academic levels. As assumed, because of the career structure in the UK, this sorting was quite straightforward here. However, the many open comments in Germany required a change in the approach. A clear junior or middle career position does not exist. What I had assumed as mid-level positions (such as junior-professors) indeed often count as senior positions. Moreover, there is a mass of scholars both early in their career or quite advanced who all fall in the group of scholarly employees (the huge German *Mittelbau*, as already mentioned above). Most of these allocated themselves into the position of junior level scholars, though they are, in fact, already quite advanced.

This is representative for the peculiarity of this overly generalisingly termed position. I reconfigured the terminology for this group and refer to scholars here as *non-senior* to separate them from junior-professors and *Dozenten*, condensed as *untenured senior* scholars. The group of tenured professors was left unchanged. This reconfiguration allowed for a statistical comparison of the two countries, despite their categorical and terminological differences. The forming of pressure groups further balances any statistical biases as it moves away from the notion of seniority entirely.

⁶ The countries do not represent the respondents' nationalities. The terms *German scholars* and *scholars in Germany* are used interchangeably, likewise are their counterparts for the UK. This was chosen to improve readability; it does not imply that all scholars that were institutionalised in Germany or the UK at the time of the survey are indeed German or British citizens.

Germany		Doc	Junior	Mid	Senior	Tenured	Sub-total	Total
	Humanities	17	148	34	52	106	357	545
	Social Sciences	14	86	26	18	44	188	
		Non-senior		Senior	Tenured			
Humanities		165		86	106			
Social Sciences		100		44	44			
UK		Doc	Junior	Mid	Senior	Sub-total	Total	
	Humanities	13	94	80	61	248	472	
	Social Sciences	13	89	66	56	224		
			Junior		Mid	Senior		
	Humanities		107		80	61		
Social Sciences		102		66	56			
Others:							160	
All:							1177	

Table 4: Effective sample of exploratory survey.

Table 4 shows that the survey satisfied the desired sample in nearly all groups except for senior and tenured social scientists in Germany. However, these groups are still within a range of ± 10 . The junior groups are very strong with many more than 400 respondents.

The following four figures show the distribution of scholars' active years as well as age, clustered according to their career level, across the humanities and social sciences. Active years refers to the respondents' own statement about how long they are active in academia, in absolute numbers. As can be seen in Germany, the non-senior groups are very dense with only little deviance and a mean of less than ten years. This doubles to seniority. Social scientists tend to be faster in their career progression than humanists are, which feeds into the assumptions that careers in the humanities are slower. Overall, this is similar in the UK. However, the general progression of careers (without the tenure system) seems to be more hierarchical: the groups of junior, mid, and senior level indeed represent a progressive system. The tenured professoriate in Germany disturbs such progressiveness in that sheer activity may not lead to a tenured position.⁷

⁷ See for detailed statistics: appendix A5.

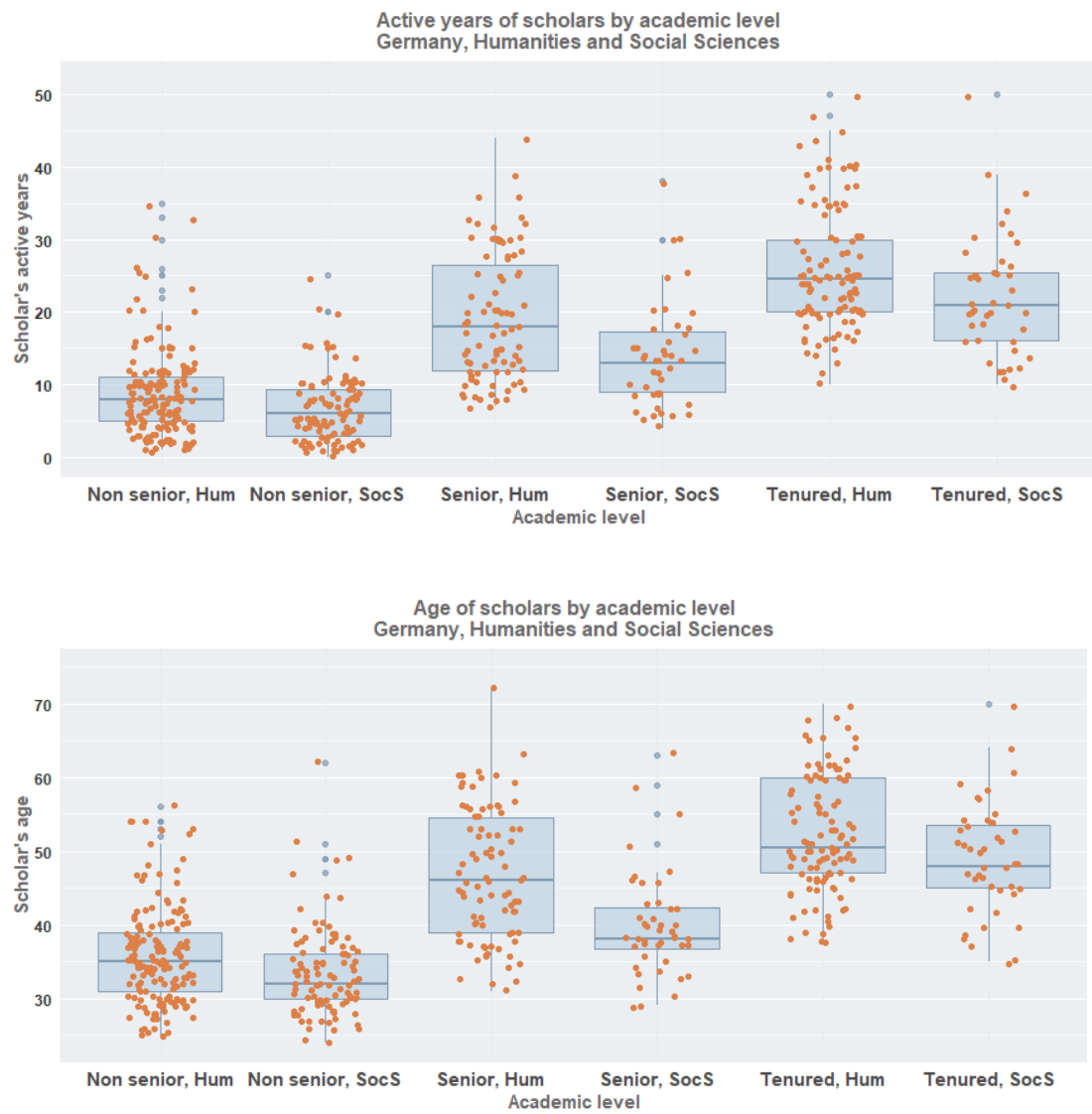


Figure 4: Active years and age corresponding to career levels of scholars in Germany.

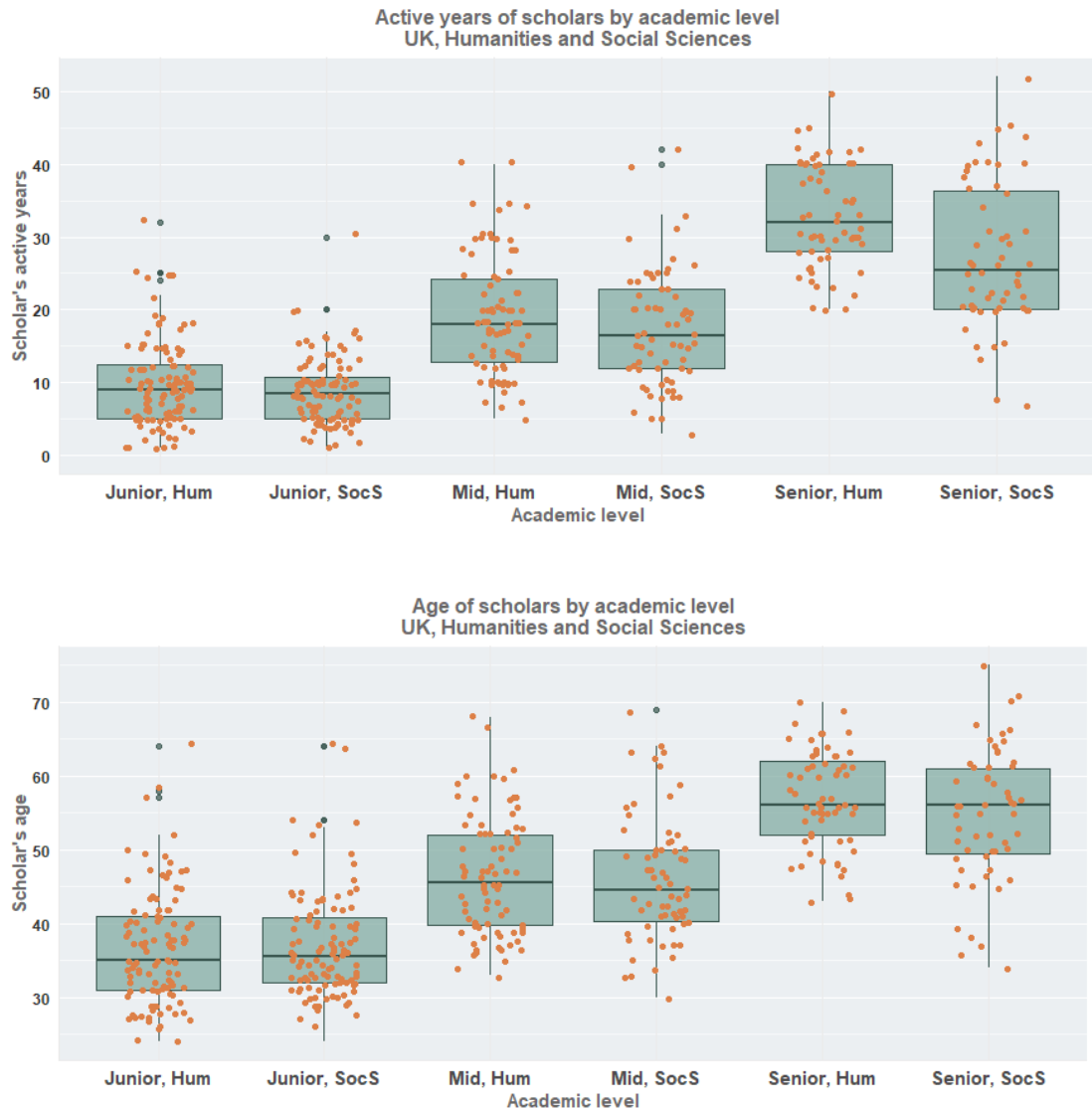


Figure 5: Active years and age corresponding to career levels of scholars in the UK.

In the overall effective sample of 1,017 responses, the correlation coefficient of age and scholarly active years is 0.884. This indicates a significant correlation so that further analyses can continue to work with only one variable. Therefore, and as scholarly active years seems to be more representative than age, I work with this variable for the analyses of pressure groups in chapter 4.

Ethical Considerations

This study has been cleared ethically in advance. I reported the intent to conduct this study to the departmental office and it was exempt from committee response for it did not involve deception or dealing with identifiable participants.⁸ To retain anonymity, I did not track responses and did not ask for names or email addresses in the survey.

Conduct of Analysis

I analysed the data using both Excel and R. I extracted the data from the survey page in CSV file format. I first cleaned the raw dataset, excluding non-valid responses (as per outlined above). I then transformed Likert scale output into integers for analysis in R. Furthermore, I added columns for condensed versions of semi-open items such as those regarding academic position and excluded open comment columns such as those regarding alternative publication types and the general open comment item. This cleaned raw data sheet was used for all further statistical analyses.

I conducted relevant tests in R to determine statistical significance of the responses based on the nature of the item as well as its distribution. These tests are relevant for all hypothesised (non-purely exploratory) items. Because of the nature of my survey items and its variables, most items required a test for non-parametric data such as Kruskal Wallis. The Chi-square test was used for items with categorical data. The results of these tests can be found in the tables in the appendix (A5).

⁸ The confirmation form can be found in appendix A2.

Pressure Groups

In the first step of the analysis of hypothesised items, I analysed the statistical distributions in order to determine the significance of prevalence. This resulted, for instance, in finding statistically significant differences of perceptions of publication pressures between scholars in Germany and the UK, or between the humanities and social sciences. In addition, I conducted an in-depth analysis of variations depending on pressure as a second step. For this, I looked only at the responses from humanities scholars at the questions *I feel pressured to publish more journal articles* and *I feel pressured to publish a monograph*. The modelling resulted in three distinct pressure groups:

- *no pressure* comprised of all those respondents who indicated only strongly disagree, disagree, or neither agree nor disagree;
- *low pressure* comprised of all those respondents who indicated agree for at least one of the two questions, but no strongly agree;
- and *high pressure* comprised of all those respondents who indicated at least once strongly agree.

I plotted distributions and ran significance tests for all relevant (hypothesised) items again to add a deeper understanding of those respondents who indicate strong perceptions of pressure. Moreover, I drew scatter plots in R for these in-depth analyses to represent the distributions of age and years active in academia to establish a pressure model. The corresponding findings are represented in chapter 4.

3.5 Qualitative Interviews

Having discussed the characteristics of the exploratory survey, I mirror this here with the qualitative interviews, starting with the design, continuing with sample and gathering, and arriving at the conduct of analysis.

Questionnaire Design

As is common with semi-structured interviews, the questions are held open, and function as a guide through the conversation. It may well be argued that a fully structured interview is unlikely to be conducted at all, since small deviations from previously designed structures naturally occur (Brinkmann, 2018: 1001). Semi-structured is the methodical approach of having a previously designed set of questions that allows for both planned and unplanned diversions. These are a means for establishing room for expansion and follow-up questions. The direction of such follow-up questions depends on both the interviewees' understandings of themes within questions and the interviewer's assumption that there may be more to say about a theme after a given answer. The result is a conversation that is informed and directed by interpretations of both interviewee and interviewer. Therefore, it is an essential hermeneutical means of gathering data, involving active listening and having an informed feel for a productive conversation (Brinkmann, 2018: 1003–1006; Kvale, 2007: 11, 63).

I conceptualised the structure of my interviews alongside core themes (developed through reading the literature and as a result of the quantitative survey) that needed to be covered, but could be changed along the chronology of the interviews: if a follow-up question directed the conversation to, say, the REF right after the opening question, I would pick up this hint and re-arrange the structure as the conversation ensues. I tested

the questions with colleagues in both Germany and the UK and arrived at the following set as my structure.

I created this set in both English and German, for language proved to be a concern. Since I conducted a study across two countries, I had both highly proficient or native English speakers and German speakers. Conducting interviews can be a delicate endeavour since the course of the conversation and the responses interviewees give is not an objectified process (Gideon and Moskos, 2012: 116; Kvale, 2007: 84–87). Moreover, my approach depended on ad-hoc activation of the interviewees so that they could reconstruct narratives as they felt applicable, requiring personal hermeneutical intelligibility. The single language approach was sufficient for the quantitative survey that was much more about the prevalence of existing categories than the qualitative interviews are. Because of this, I decided to refrain from conducting interviews only in English and did those with German scholars in German. This proved to be slightly more difficult in the process of analysing the information, but was, overall, much more successful in yielding information about the participants' life worlds than a single language approach would have been. For the direct citations to interview data, I translated responses from German to English for matters of readability.

The opening question is broad and, yet, directly dives into known but unknown ground:

- Why do you publish your research?

The idea for starting with this question is to get interviewees talking: usually scholars think they know why they publish research—to disseminate it. But my aim was to give interviewees time to think about what lies behind this, what dissemination may actually mean—for them or for the scholarship—and whether there may be other

reasons for why they publish. After the opening question, I aimed to guide interviewees towards such other reasons, especially those that may lie behind authorship:

- Is there a pressure to publish?
- Is *publish or perish* reality?
- Do you think that researchers must publish ever more or publish too much?
- Who tells them to do so/what are sources of pressure?

The reason for this set of questions is to get scholars to talk about potential pressure, to get to know whether it exists, and if it does, in what form and what may be sources of it. This set of questions is crucial for me for getting a feel for the scholar whom I am in conversation with: how does she stand towards the logic of publishing ever more and the structural pressure that may result from it? Is she affected by it? Does she reject its existence—only for her or even structurally? Potential follow-up questions for this set could go more towards discourses, what *too much publications* means, or what kind of narratives are available for passing on pressure among peers.

The next set concerns publishers and publishing brands:

- In what sense are publishing brands useful (or not)?
- What is the value of highly selective brands?
- What is the value of publications with low impact brands? Why are they published at all?
- What is relevant to you when you think about publishing a monograph, a journal?

The opening question for this set, again, is quite broad, leaving interviewees the option to either talk about the utility of brands as a reader or as an author, or both. This question involves an explanation of what I see as a brand for this interview: the names

of journals, publishers, or even publishing series—as they all function as branded references in academia. I anticipated that most interviewees would still conceive of brands as mostly high impact brands, so that the two following questions in this set turned towards the selectivity of such brands and the other end of the impact gamut. Triggered with a conversation about brands, I aimed to enquire what might be relevant for the interviewee when thinking about publishing. Since I am less interested in technicalities, I put this question here so as to confront scholars with their own approach to and handling of brands. Potential follow-up questions to this set relate to peer review, the nature of selectivity in scholarship, and network structures in getting published.

The following set of questions concerned the REF, or the German *Exzellenzinitiative*, respectively:

- Does the REF/*Exzellenzstrategie* have an effect on your publishing behaviour?
- Who tells you about the REF?
- What is excellence in your field?
- How would you define a productive scholar?

Since the REF is much more important and impactful on publishing than the *Exzellenzinitiative* is, the first two questions were to trigger much longer answers in the UK than their counterparts would do in Germany. For the UK, however, these questions are crucial for capturing potential sources of pressure and to further understand the effect of evaluation on publishing. The last two questions are conceptualised, more or less, as questions to confront interviewees with norms of what good scholarship is, how it can be represented for an individual, and whether structural strategies are helping to achieve these individual goals. The two questions may not look as triggering such thoughts, but while training for the interviews, I found they indeed do

so. The reason for this is that these questions are embedded within a conversation about potentially false concepts of productivity effected by structural rhetoric of bureaucratic notions of excellence and, thus, frame excellence and productivity as contradictory.

At this point in the interview, or at the end of it in cases where the structure may have changed, I aimed to confront interviewees with potential for change with questions such as:

- How can senior scholars/do you help junior scholars regarding authorship and publishing strategies?
- What or who needs to change to change publishing for the better?

The reason for so doing is that the literature and my quantitative survey unequivocally point towards adversarial developments of authorship and publishing and a disconnection between these and scholarship. What do scholars do about this? This set of questions is not intended to provide plans for change. The idea is more to trigger scholars to think about whether they or their colleagues challenge established practices and whether there is potential for doing so at all.

These questions may require clarification regarding neutrality: they presume that there is a problem in established practices and I as an interviewer cannot claim to be neutral here. The mounting evidence that there are indeed problems in practices requires that understanding this issue in more depth cannot simply stay at a neutral surface. In return, if an interviewee would indeed point out that she does not see a problem in the established practices, I would adapt the questions during the conversation to enquire whether the interviewee would, hence, not see the need for change. By so doing, as the interviewer, I aimed to stay neutral within my path of research and did not explicitly encourage interviewees to emphasise adversarial effects of contemporary practices. In

other words, if the interviewee emphasised the positive sides of, say, the REF or selective brands, I would not intervene to point out potential damages of these mechanisms. Overall, my interviews fall within the area of active interviews in which the interviewee is not just asked out but encouraged to be an enquirer on her own, searching within herself for meaning and narratives assisted by the interviewer. The, at times, confrontational and activating items in my structured conversations are included for this purpose. And it is also for this purpose that interviewees are allowed—and encouraged—to interfere with the protocol. The interviewees, their social environments, and structured meanings are objects under study; but as in Latour's sense of the single ontology of culture and object (1993: chapter 2), being object does not mean they are only a passive entity to be scrutinised. Like gravity in physics, they need to be given the opportunity to be in disagreement with theories raised about them (and other than quarks, they can report their disagreement discursively).

The last question, in this vein, would again be open with room for interpretation:

- If a colleague approached you with the idea of creating a new, independent journal, would you publish in it and, if so, what would be the necessary requirements of the journal for you to do so?

The purpose of this question adapts to several themes from throughout the interview such as brands, selectivity, and change. Since the preceding questions are about change and as most scholars would seek change in one form or another, I considered that many scholars would not reject the chance of publishing in a new journal. For if they did so they would potentially contradict their own desire to change something. The question, therefore, would allow to think about the underlying mechanisms more pragmatically: what is necessary for me (as a scholar) to publish in a new journal, what should this journal represent?

Sample

I aimed to approach comparable numbers of interviewees in both countries, at comparable institutions, and from within a range of core humanities disciplines. I applied a stratified random sampling method in order to reach that sample, as outlined in Hibberts et al. (2012) or Ritchie et al. (2003). The stratification variables were: country in the first level, and seniority and institution in the second level. First level means that the interviewees needed to be of equal numbers from Germany and the UK; the second level means that these scholars needed, roughly, to be non-senior or senior scholars and from an *elite* (or: high-impact/*Exzellenz*) institution or a non-high-impact institution of comparable numbers. Within these systematic categories, I desired to have an approximately equal but random mix of gender and humanities disciplines.

I determined to reach n=12-18 based on achievability as well as sufficiency. On the one hand, as Gideon and Moskos rightly claim (2012: 113), conducting interviews, turning them into transcripts for analysis, and extracting meaning for the search of patterns is intensely time-consuming. Since I had already acquired a substantial amount of data that established the fact of availability of pressure accompanied by the rich accounts found in existing literature, I had relevant empirical data both first and second hand already analysed as a means of triangulation. However, I did not want to substantiate my existing analysis with only weak empirical data about the meaning and sources of publishing pressure because of too few interviews. Because of this, I aimed at 12-18 interviews.

Having created these sampling requirements, I proceeded with an iterative selection process so that conducting the interviews spanned several months, from November 2019 to April 2020. Iterative means that I approached a range of possible interviewees via email and awaited their response. After conducting interviews with these

scholars, I invited scholars that were yet not represented, so that reaching 18 interviews would lead to an optimal sample. Overall, I approached 71 scholars, resulting in an average response rate of about 25%. The majority of scholars did not reply and only a few responded with a decline. As of April 2020, I had already conducted 18 interviews, while 2 scholars had agreed to be interviewed as well. However, these did not take place as the interviewees withdrew because of the crisis following the outbreak of the coronavirus. I decided to stay with 18 interviews, as originally intended.

The effective sample is represented in table 5. It shows that I have achieved equal representation from the two countries, in gender, and in non-senior/senior shares. I also achieved equal representation regarding excellence cluster or elite institutions in Germany and the UK; however, on the individual level of the scholar, I recognise this as a rather meaningless category because of its arbitrary and socially constructed nature. Regarding disciplinary distribution, I interviewed five historians, six philologists, and seven philosophers. For the sake of anonymity, I sorted interviewees into a list related to their attributes and assigned each an arbitrary name in alphabetical order that bears no resemblance to their original names. I use these names as referents to the interviews throughout the thesis.

	#	Code	Level	Gender	Discipline	Description
Germany	"Regular"	1 Albert	Non-senior	male	History	Non-senior Historian, Germany
		2 Berta	Non-senior	female	Philology	Non-senior Philologist, Germany
		3 Clara	Senior	female	History	Senior Historian, Germany
		4 Don	Senior	male	History	Senior Historian, Germany
	"Excellence"	5 Elizabeth	Non-senior	female	Philology	Non-senior Philologist, Germany
		6 Gerald	Non-senior	male	Philology	Non-senior Philologist, Germany
		7 Herb	Non-senior	male	Philosophy	Non-senior Philosopher, Germany
		8 Ian	Senior	male	Philology	Senior Philologist, Germany
		9 John	Senior	male	Philosophy	Senior Philosopher, Germany
UK	"Regular"	10 Karen	Non-senior	female	History	Non-senior Historian, UK
		11 Melissa	Non-senior	female	Philosophy	Non-senior Philosopher, UK
		12 Nora	Senior	female	Philology	Senior Philologist, UK
		13 Omar	Senior	male	Philosophy	Senior Philosopher, UK
	"Excellence"	14 Penelope	Non-senior	female	Philosophy	Non-senior Philosopher, UK
		15 Quintessa	Non-senior	female	Philosophy	Non-senior Philosopher, UK
		16 Richard	Senior	male	History	Senior Historian, UK
		17 Samuel	Senior	male	Philosophy	Senior Philosopher, UK
		18 Tabea	Senior	female	Philology	Senior Philologist, UK

Table 5: The effective sample of the qualitative interviews.

Ethical Considerations

This study was cleared by UCL's Ethics team in advance of the research being undertaken.⁹ It was exempt from more detailed clearance because it does not involve deception or dealing with identifiable participants. I rely on several steps to ensure anonymity and adherence to ethical survey conduct. Firstly, I refer to the interviewees throughout this thesis only with their code names. Initially, I intended to use categorical names, but these would have reduced readability and would not have been more secure than arbitrary code names. These code names are already introduced in the transcripts so that the real names remain visible only in a single table that shows the conversion between code and real name. This table is stored in a different folder than the transcripts are, allowing for higher security in a case of data theft. Secondly, I conducted a disclaimer procedure at the beginning of each interview. This involved stating that the interviewee is

⁹ The confirmation form can be found in appendix A7.

free to refrain from answering a question and would also be free to end the interview at any time. Moreover, I mentioned again that all interviews would be anonymised in the process of transcribing them. Opening the conversation with this disclaimer set the stage for the interview to be conducted under free consent on the side of the interviewees, making for an ethically correct procedure in working with human subjects and also allowing for a more open conversation (Gideon and Moskos, 2012; Oldendick, 2012). This information is also part of the consent form that all interviewees signed.¹⁰ These transcripts are also stored in a different folder from the transcripts. Furthermore, I deleted all instances in interviews where the anonymity of the interviewee was at risk and inserted the placeholder: *[left out for privacy]*.

Data Gathering, Transcription, and Analysis

I initially aimed to conduct more interviews in person and only a few via Skype, but the reality of time demanding academic jobs led me to change my approach: senior scholars, in particular, would only be available weeks ahead. This made it impossible to schedule many interviews at a university in the same week to conduct all in person. However, the option to conduct the interviews via Skype allowed me to stay flexible regarding my iterative process of determining the right interviewees for my sample.

The interviews took about 30 to 45 minutes on average. I recorded these using both my phone voice recorder and a recorder built in my laptop for backup. I opened all

¹⁰ An example form can be found in appendix A8. The original forms are only available on request for the purpose of the PhD examination. For the sake of anonymity, however, they are not attached and will not be made available to the public.

calls with a short introduction that explained my research topic and followed with the disclaimer, as stated above.

I initially transcribed the interviews using the software Amberscript, which supports a variety of languages and English accents. However, this worked only to a limited degree. Scottish accents as well as German resulted in defective transcripts. I either cleaned these transcripts or retyped sections manually in a second step.

Following advice for qualitative research in the social sciences (Kvale, 2007; Oliver et al., 2005; Ritchie and Lewis, 2003; Trainor and Graue, 2013), the best approach to transcribing interviews was to follow the purpose for why the interviews have been conducted. From the dependence on naturalised transcription of every utterance in linguistics to rather formalised representations of meaning in business studies, the range of interpreting utterances for their written representation is vast. And yet, not interpreting at all is impossible.

I settled for a roughly natural representation that allows for proper readability. In this vein, I did not account for each ‘hm’ or cleaning of the throat, and also did not include each ‘you know’ or ‘like’. Especially when these appear repetitively, they are a habit of the interviewee and so confer no meaning specific to the situation. Instead of using commas for representing sentence structure—and so potentially de-naturalising meaning—I used hyphens to indicate smaller pauses or breaks of the structure of the utterances.¹¹ All questions and responses have a time stamp for orientation. As an example, a transcript looks as follows:

¹¹ As a disclaimer: since I regularly use the em dash except for during the interviews, it seems reasonable to acknowledge that where I cite the interviews, the use of the hyphen is not a mistake or broken em dash. The sudden appearance of hyphens in the chapters 5,6,8, & 9 may look confusing, but it follows an established logic.

00:28:06

Marcel: Yeah. And do you- do you think that- culturally this rhetoric of excellence is helpful for academia?

00:28:19

Quintessa: It's hard not to see the REF- in a- this kind of hoop jumping exercise that takes an awful lot of peoples' time and isn't a particularly good way of evaluating the kind of the quality of research that's done in an institution. So- when I was based in Scotland- in my last job- I was working as an applied philosopher in a [left out for privacy]...

Two examples, a German and an English interview, can be found in the appendix (A9/10). All others are available on request.

Analysis and Coding

I analysed the interviews drawing on qualitative thematic analysis techniques, foremost a theory-driven, deductive approach to the data (Braun and Clarke, 2006). Most of all, the deep reading of interviews alongside transcribing, re-reading, and coding them offered me deeper understanding of the underlying themes and semantic nuances. Overall, I listened to and read the interviews about six times each. Iterative, structural coding allowed me to handle this process in a method-driven manner.

This approach to the methodical analysis of text appeared to be the most fruitful and suitable for my task for several reasons. Firstly, a qualitative-thematic approach was to be favoured over quantitative-linguistic approaches. Though the latter can offer precise understandings of text and utterances, it is a precision that comes about only in a narrow scientific sense. This sense is unsuitable to the analysis of meaning for which context— intra-textual as well as textual-serial and topical—are crucial. Secondly, within the different forms of qualitative-thematic approaches, a variety of coding methods are available. For my purpose, it seemed most suitable to mark up the interview texts to highlight internal meaning structures, both those that I intended to include during the conversations and

those that the interviewees raised. This provided me with an awareness of different terminologies and nuances in the understanding of topics such as brands, peer review, pressure to publish, etc. Since I did not define those elements in the first question (only in follow up questions, if necessary), several interviews brought different understandings to the conversation that should be reflected in the coding. And thirdly, iterative coding seemed only sensible since in the process of writing after the first set of coding, the knowledge of the topic is deeper and internal connections of meaning become more consistent. I, therefore, decided to conduct a second coding about two months after the first set. I changed the coding nodes for this, to account for my deeper understanding of the themes after drafting the outline of the analysis, and to allow to gain added insights. On the technical side, I used NVIVO 12 pro for the coding. Since I deployed a—technically—rather straightforward method, a general coding software sufficed so I used the one that I was most familiar with.

My first set of coding nodes comprises of 590 references made with 13 first level and 22 second level nodes. Among the nodes are, for instance, peer review (X—process, X—conformity), motivation to publish (X—communicate, X—authorship, ...), or scholar (X—excellent, X—productive, X—career & promotion). The resulting code book was a valuable and thick resource as a structured representation of the empirical situation. This resource allowed me to gain a deeper insight into the answers in a comparative approach for understanding the nuances of the underlying issues. The complexity, so to say, was partly resolved by aligning the unstructured meaning-making and descriptions of practices with each other across my interviewees. I used this resource to write the first conceptual draft of my theory and to hone my terminology, which then fared as the basis for the second coding.

This second set comprises of 224 references made on 6 first level and 10 second level nodes. Among these nodes are, for instance, accountability (X—ref, X—general), performativity (X—timeliness, X—quantity, ...), or visibility and network. No longer included in this set are rather straightforward nodes such as teaching or referents to the scholar as such (the excellent scholar, the productive scholar, etc). Answers to be coded for these nodes are well captured already during the first set of coding. And since these answers are less ambiguous and clearly defined, there was no need for a second coding of them. The other, here newly developed, nodes are more specifically tailored to the terminology that I developed to explain the empirical situation.

It is this process that makes the iterative approach valuable: after having had a theoretical idea and a practical notion by the unstructured reading of the interviews, I conducted the first coding which resulted in a more structured understanding of the empirical situation. This was fed back into creating my theory, which I conceptualised in a first draft. I then conducted the second coding with the revised nodes: note here that I did not use new terminology to find only answers supporting my theory; the new nodes—a terminological system of categories—were put in place to allow for the answers to either support or refute theoretical aspects. Especially of interest was the issue of accountability and how this category is different in Germany and the UK, where for the former, accountability is more of a traditioned element of bureaucracy, while for the latter, it is a dominant aspect of NPM in the form of the REF. This resulted in an even better understanding of the complexities of the categories. Ultimately, where the first coding was a reduction of complexity, the second coding allowed for the complexity to build up within the new terminology.

3.6 Conclusion

Next to the engagement with scholarly communication, I see the theoretical journey described here as among the most valuable aspects of my PhD. The profound understanding of theory and its connection to methodology helped me to gain much deeper insights about knowledge in the field and in interaction with other studies. This is true for both the theory and methods chosen as well as for some of those rejected. Practice theory allows for a social ontology, but it is by no means exhaustive on social theoretical potential. As Welch et al. put it, ‘theories of practice can be usefully complemented by other social theoretical resources’ (2020: 328). I do this by drawing on further cultural as well as critical approaches; practices, however, remain the locus of analysis.

By the deep immersion in the theory and by taking a theory-driven approach, I recognise myself as being much more confident and well-prepared for future scholarship. My experience at Yale University has been pivotal in this respect: after having struggled to relate practices to culture, engagement with the Alexanderian school of cultural sociology provided me the necessary insights for actually connecting how and why individuals act with culture in its fundamental ways. This is true even though—or perhaps just because—I reject the sole *strong* element of the programme and point to the *relativity* inherent in all branches of cultural sociology.

Moreover, engagement with established research practices in a larger effort such as through the survey and my interviews allowed me to experience the constraining and the enabling impact of rigour and scholarly specificity: constraining in that a lot of effort has to be put in the concept and analysis of such endeavours up to the point that two larger empirical studies during a PhD can become a demanding challenge; enabling in that the result of such empirical studies, conducted with rigour and in a larger context, lead to

reliable insights that can advance knowledge. And that is why the challenge was worth the effort.

4 The UK and Germany: Differences and Similarities

My case study frame allows to focus on scholars as cases in two particular field sites, Germany and the UK, and to take a closer look at the particularities of the empirical situations in the two countries. This focus is crucial to identify culture principles. In this chapter, I showcase data of my exploratory survey and discuss the empirical differences and similarities, before underpinning and explaining these empirical situations with political and cultural background understanding as well as with data from my qualitative interviews in the subsequent chapters 5 and 6.

In this chapter, firstly, I provide an overview of hypotheses and the corresponding quantitative data. Secondly, I discuss this data to provide the more nuanced understanding of the empirical situation and to emphasise country-specific particularities. Thirdly, I provide an overview as well as a discussion of the qualitative statements that were included in the predominantly quantitative survey. This further enables the reader to get a deeper understanding of the two countries.

4.1 Visualising Pressure: Data from the Exploratory Survey

Hypotheses

How can pressure to publish be determined empirically? By dealing with scholarly communication research, and the matter of *publish or perish* in particular, the existence of pressure seems to be a truism, but it is unknown to what extent it prevails or how it is to be differentiated across groups and levels of scholars. In the literature discussed in my review, this is expressed at least implicitly in many instances. I gathered data through my exploratory survey in response to this, as described in the methods chapter 3.4. I aim to substantiate claims about a pressure to publish with quantifiable perceptions from the field.

This acts as an indicator. Only by contextualising the quantified findings subsequently with qualitative notions from the field does this become relevant for a cultural sociological argument. To *substantiate* means to verify or refute hypotheses that all emerged by rendering the matter of pressure in more detail while drawing on the existing literature. These hypotheses are (I go into detail about them in the discussion below):

- Scholars in the UK feel more pressured to publish than scholars in Germany do.
- Junior scholars feel more pressured to publish than established scholars do.
- In the UK, publishing pressure is relatable to the REF.
- In the UK, scholars are negatively affected by the REF.
- Publishing pressure is relatable to perceived quality.
- The personal list of publications is a crucial factor for careers.
- Scholars receive publishing advice regarding publishing brands.
- Scholars consider there are too many new works (articles or monographs) being published.

Pressure Groups Overview

As outlined in the methods chapter, I built pressure groups depending on the combined responses scholars gave to the questions of how much pressure they perceive regarding publishing monographs and articles. As described, they are focussed on the humanities and abandon the notion of seniority since they cluster scholars into the groups *no pressure*, *low pressure*, and *high pressure*. Figures 6 and 7 show how the career levels and gender are distributed in pressure groups. While the majority of senior and tenured scholars experience no or little pressure, experience of pressure to publish is pervasive among earlier

career stages. Moreover, the experience of forms of pressure is higher among women than among men.

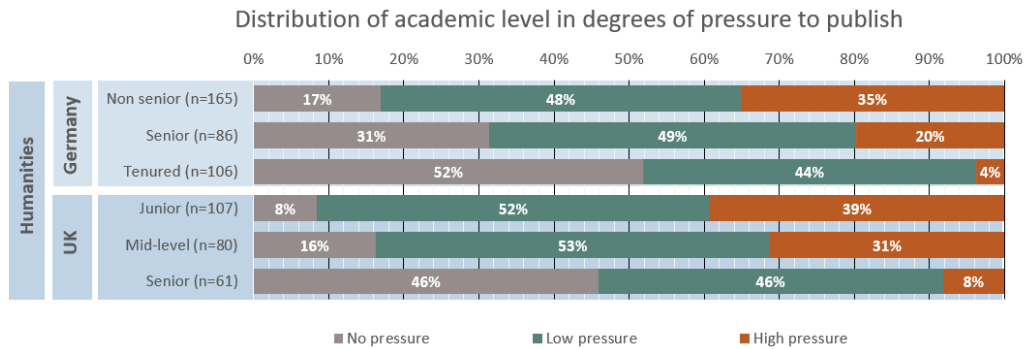


Figure 6: Distribution of career level in pressure groups.

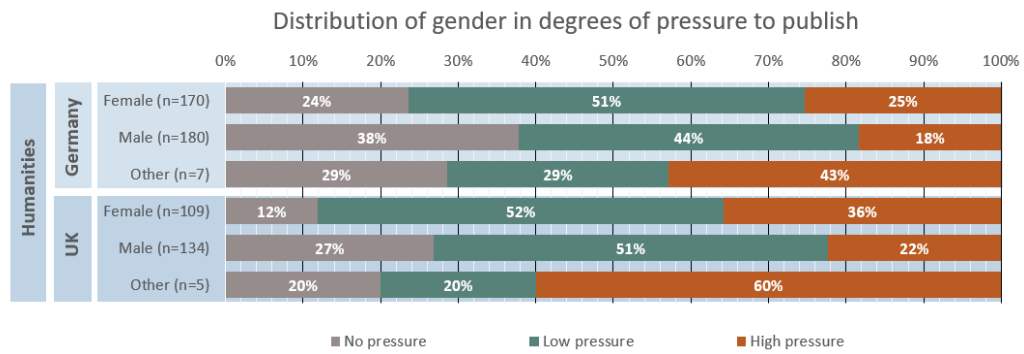


Figure 7: Distribution of gender in pressure groups.

Table 6 shows the distribution of disciplines within pressure groups, for selected disciplines with the largest shares. This is an indicator for understanding the share of disciplines within pressure groups. Since an analysis on the level of individual disciplines does not have sufficient statistical power, this component analysis is the best option.

Distribution of selected disciplines by degree of pressure to publish, Germany Humanities				Distribution of selected disciplines by degree of pressure to publish, UK Humanities			
Discipline	No pressure	Low pressure	High pressure	Discipline	No pressure	Low pressure	High pressure
History	15%	29%	13%	History	20%	26%	33%
Archaeology	9%	6%	5%	Archaeology	6%	8%	7%
German Philology	12%	6%	5%	German Philology	0%	2%	1%
Philosophy	5%	12%	19%	Philosophy	2%	5%	7%
Linguistics	10%	10%	11%	Linguistics	8%	6%	6%
Literature	5%	3%	4%	Literature	4%	5%	6%
Theology	4%	3%	8%	Theology	2%	3%	6%
Art History	2%	4%	6%	Art History	6%	2%	1%
Classics	5%	2%	0%	Classics	8%	4%	1%
English Philology	5%	1%	0%	English Philology	8%	10%	8%
Of total	71%	76%	71%	Of total	64%	70%	76%

Table 6: Disciplines in pressure groups

Publish or Perish (in Boxplots)

Figures 8 and 9 show respondents sorted into pressure groups. These correspond to the presentation of the entire sample in chapter 3 (figures 4/5), only this time sorted into pressure groups as opposed to career levels, first across the humanities and social sciences and secondly the humanities only. At first sight, this is a reversal of career levels: high pressure rests on the early career scholars the most. Interestingly, however, the earlier progressiveness of career levels visible in the UK is in regard to pressure rather found in Germany. High pressure focusses on the early career positions in Germany (with a standard deviation of 6.42), while in the UK, high pressure is much less focussed (with a standard deviation of 8.96).¹² This *loosening* of focus increases with a decrease of experienced pressure.

¹² See for detailed statistics: appendix A5.

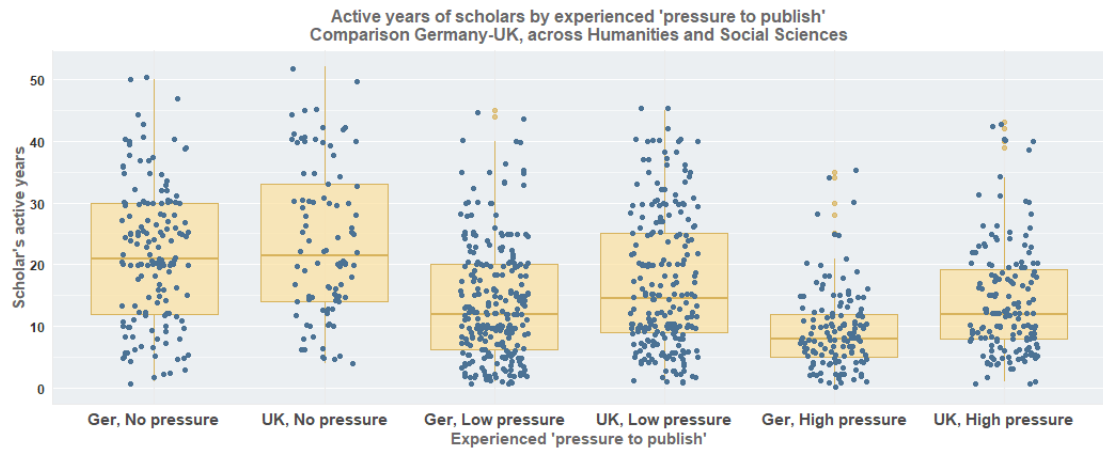


Figure 8: Experienced pressure to publish across the humanities and social sciences.

Focussing on the humanities, these effects are strengthened. It is indicative for the systemic pressure that works across career levels in the UK as opposed to the pressure that focusses on the early career positions in Germany. Think of how the REF may induce pressure more loosely around early to mid-career positions in the UK. In Germany, where such a mechanism does not exist, pressure is concentrating on *getting into academia* as such, and so focusses more on the earlier stages of a career.

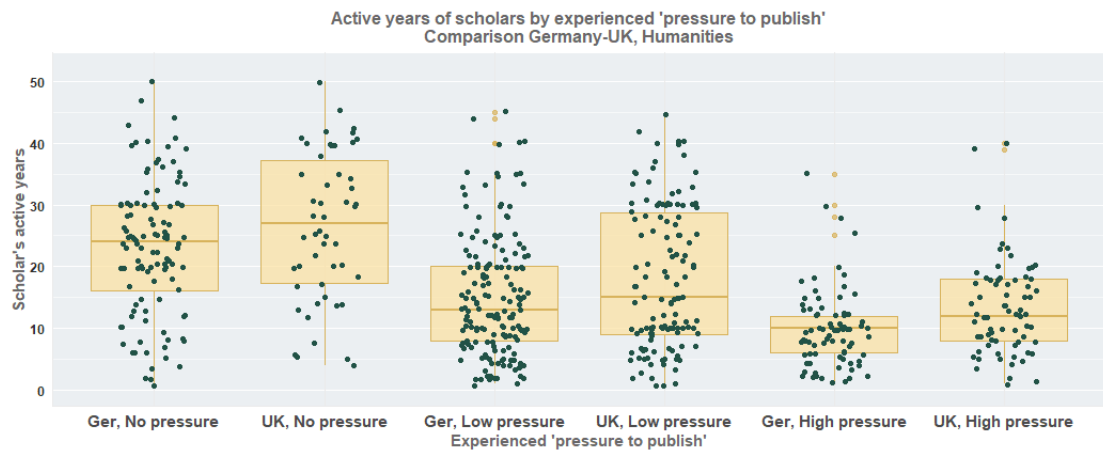


Figure 9: Experienced pressure to publish in the humanities.

Figures 10 and 11 represent the direct pressure to publish, on which the pressure groups are built. As can be seen, the pressure is overall very high with differences in regard

to country as well as to career level. There is slightly more perceived pressure for articles than for monographs.

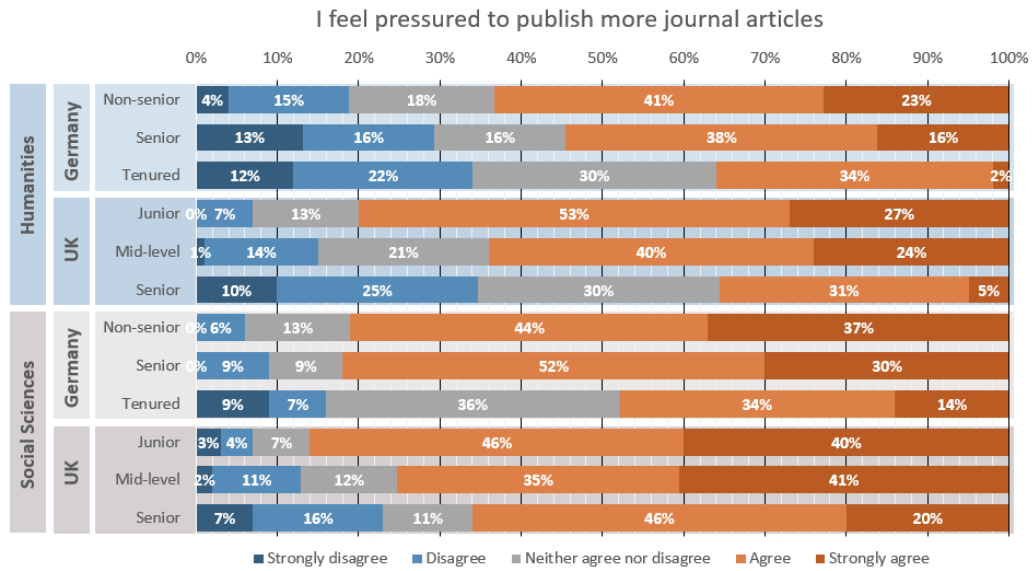


Figure 10: Pressure to publish more journal articles.

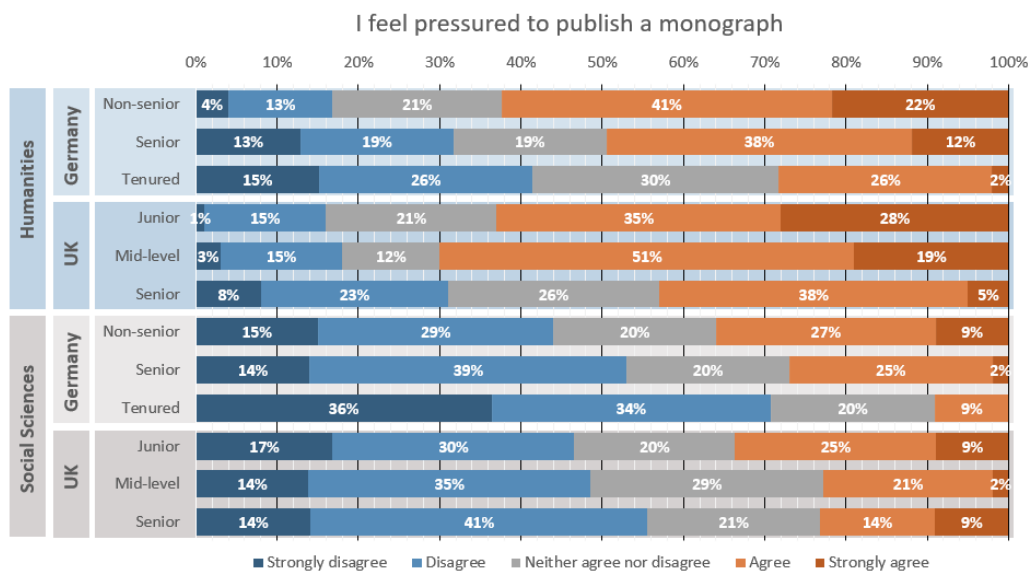


Figure 11: Pressure to publish a monograph.

The perceived pressure to publish is further investigated in relation to its temporality in figure 12 (in career levels) and figure 13 (in pressure groups). Again, there is more pressure experienced in the UK. Especially those who perceive high amounts of pressure

indicate a strong connection to temporality: over 70% of scholars in the UK experiencing high pressure at least agree that the quality of their research suffers as they feel pressured to publish faster.

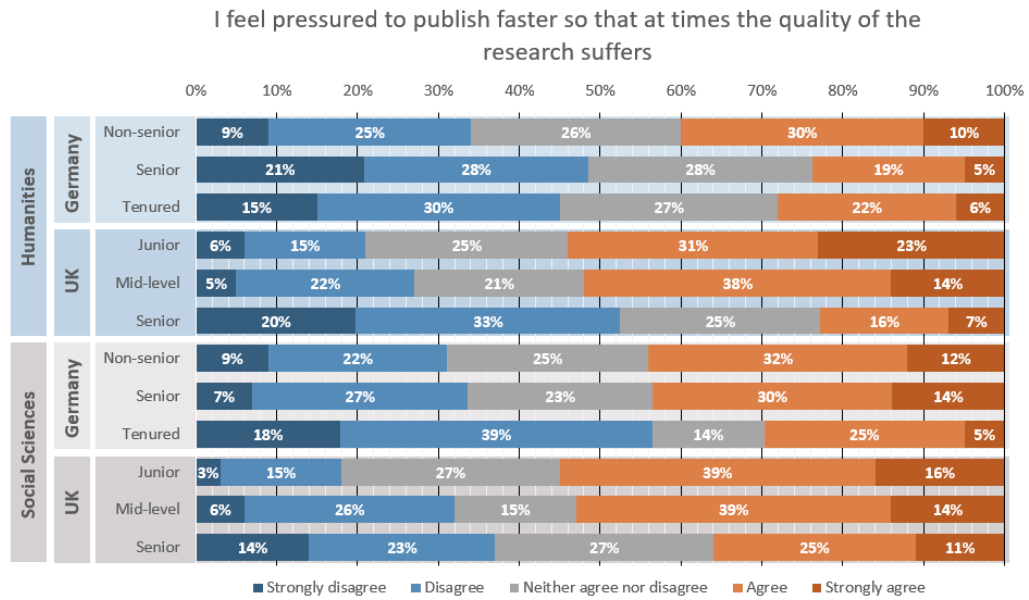


Figure 12: Pressure to publish faster in relation to quality.

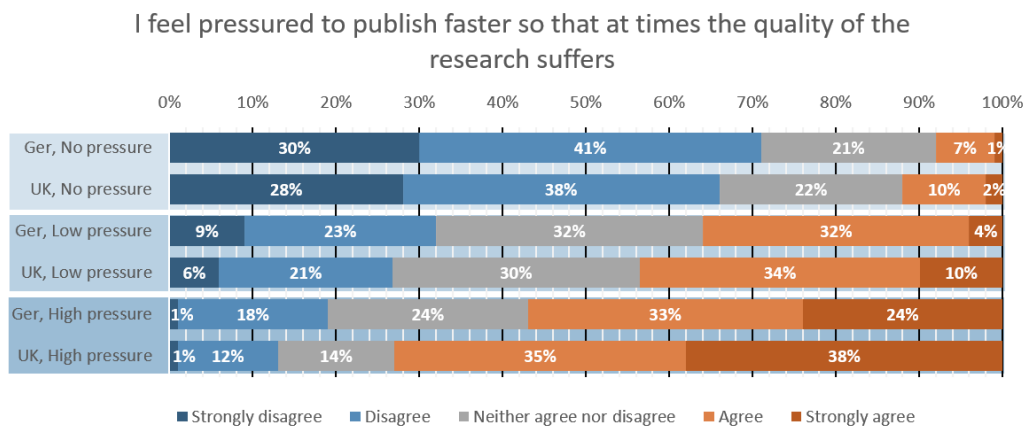


Figure 13: Pressure to publish faster in relation to quality, in pressure groups.

Turning to research evaluations, this difference between Germany and the UK is furthered. Scholars in the UK experience soaring levels of pressure to publish faster in relation to evaluations (such as the REF). German scholars rather reject this form of

pressure (in relation to the *Exzellenzinitiative*). This pressure is aggravated in the UK and reaches nearly 90% among those experiencing high pressure generally.

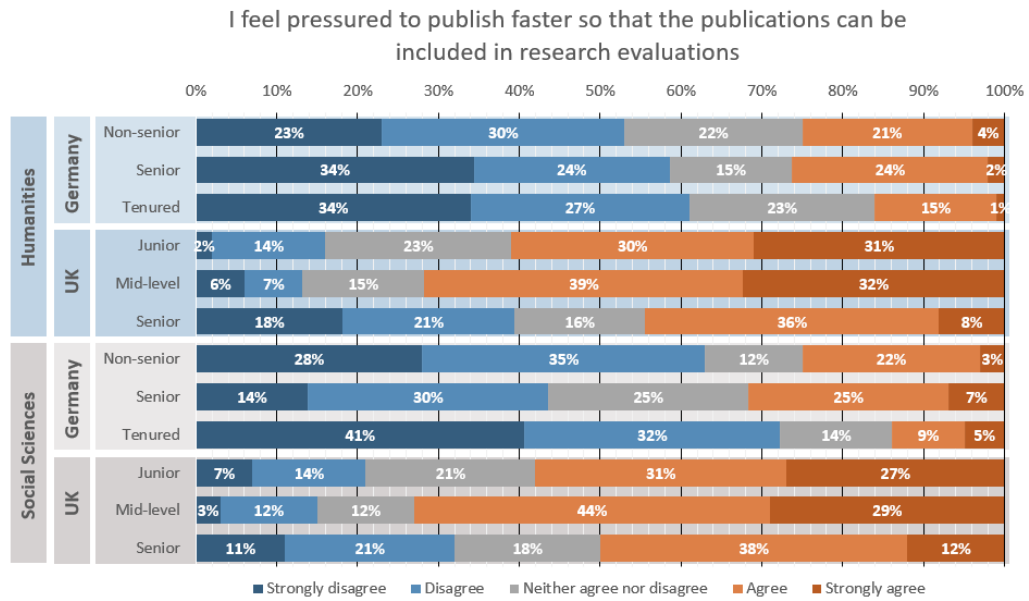


Figure 14: Pressure to publish faster in relation to research evaluations.

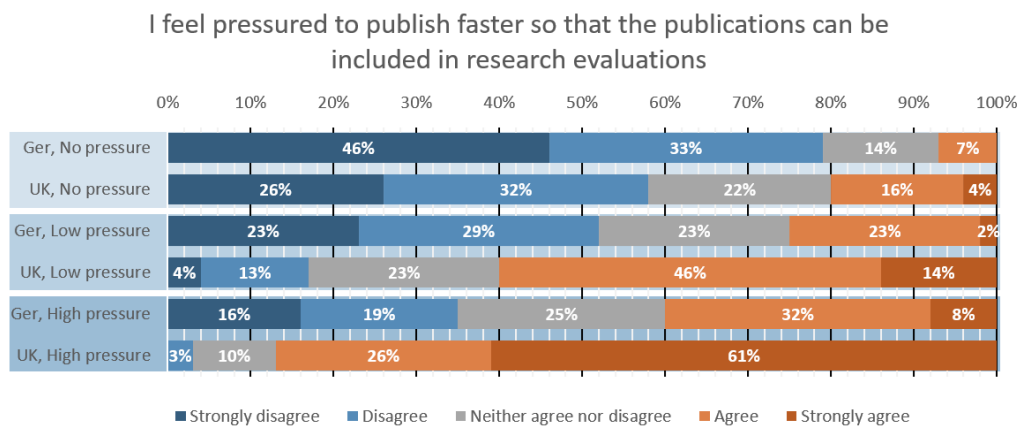


Figure 15: Pressure to publish faster in relation to research evaluations, in pressure groups.

Further confirmation of the role of research assessments is provided by the item represented in figures 16/17. Barely any impact is perceived by German scholars in this regard. However, impact reaches very high rates in the UK and it is predominantly negative, echoing the negativity of alienation. As figure 17 shows, the rate of negative impact reaches nearly 80% among those experiencing high pressure to publish generally.

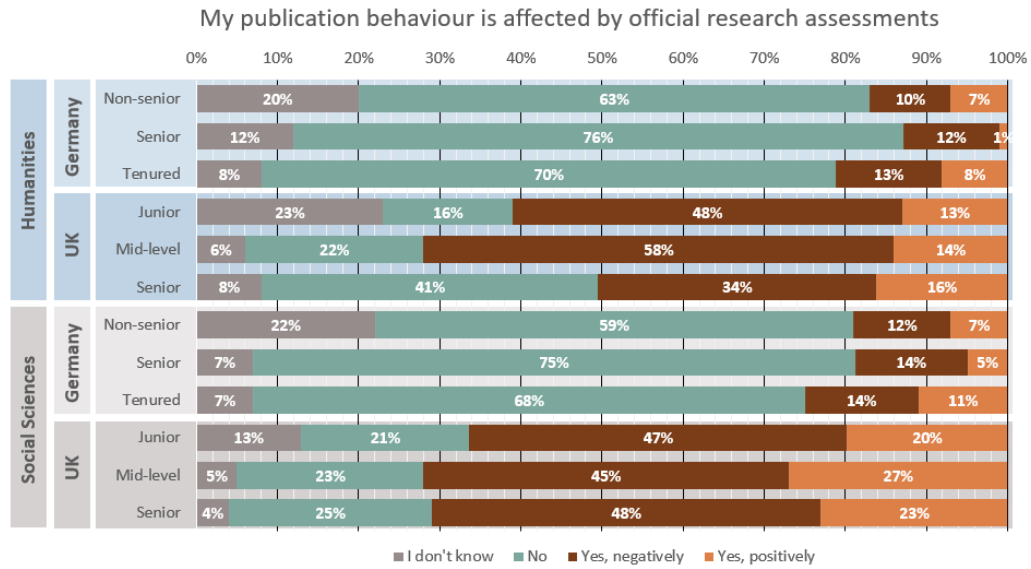


Figure 16: Publication behaviour affected by research assessments.

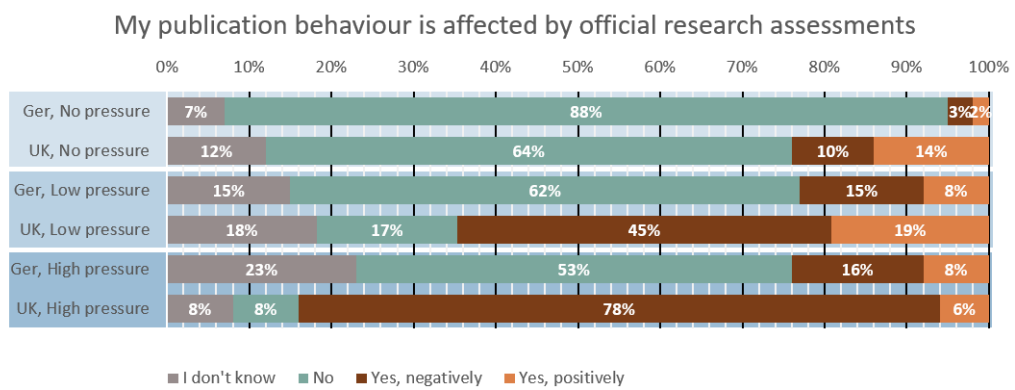


Figure 17: Publication behaviour affected by research assessments, in pressure groups.

In relation to official assessments stands OA which is predominantly institutionally or policy-driven, especially in the UK. And, seemingly correspondingly, pressure here is higher than it is in Germany where it is largely even rejected entirely. The country-specific differences are striking for this item. This pressure, therefore, pervades in the UK as OA seems to be enforced without, or beyond, an intrinsic relevance.

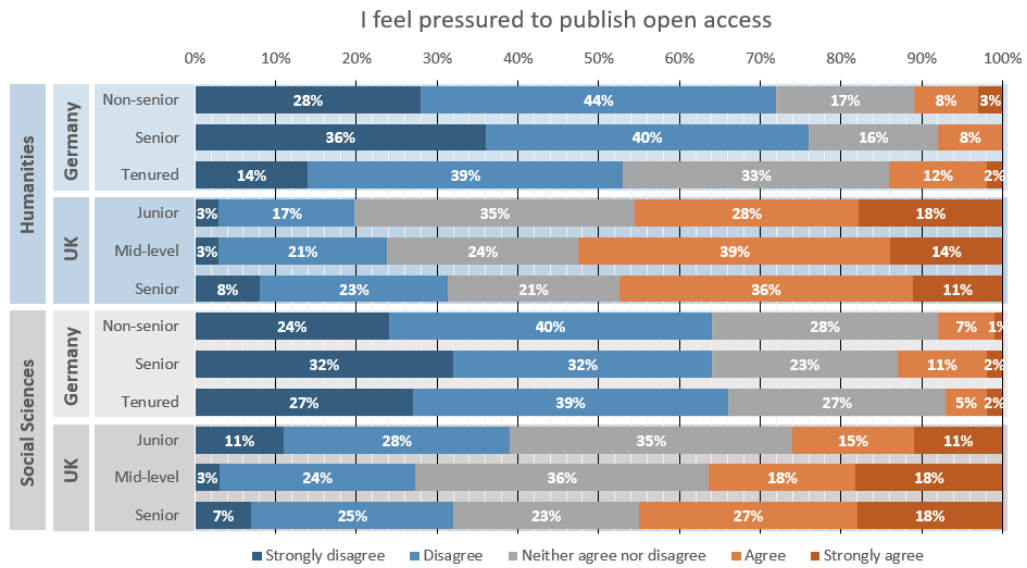


Figure 18: Pressure to publish OA.

Further in regard to country-specific differences is the item presented in figure 19. The advice to publish with a particular publisher, also known as *a-*, *b-*, or *c-lists*, is assumed informally but rarely formally confirmed. This advisory practice is strongly rejected in Germany and seems, partially, to take place in the UK. To be sure, this is not necessarily negative; advisory can be a positive practice of guidance. But in relation to institutional evaluation regimes, it is indicative that this practice seems to be more relevant in the UK.

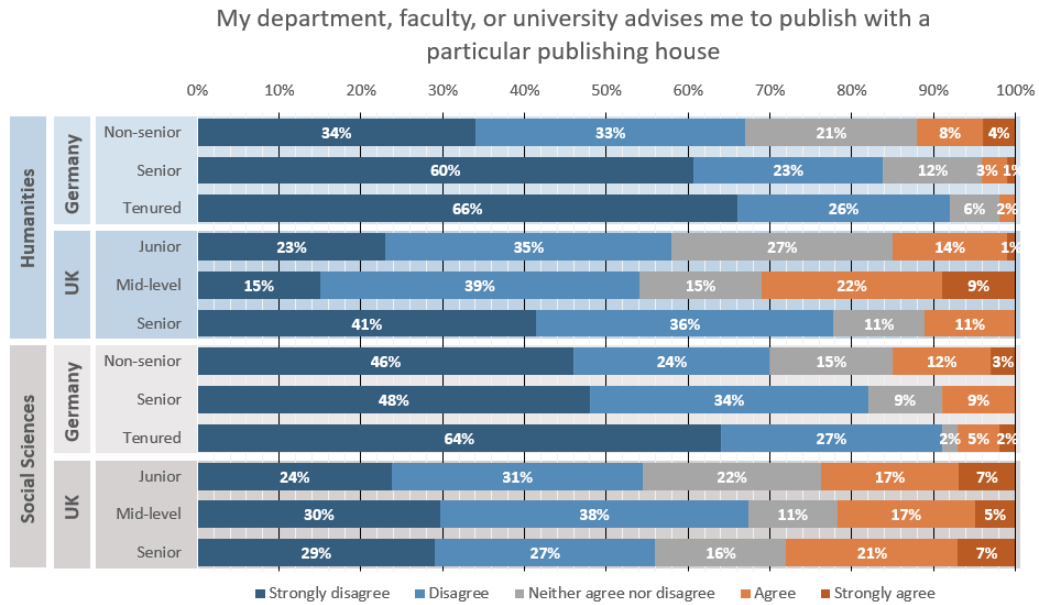


Figure 19: Receiving advice on publishing venues.

Further in relation to a general pressure to publish is the notion of too many publications. For articles (figure 20), this seems to be the case as nearly half of all respondents indicate that too many articles are being published. The rates increase in the social sciences. In regard to monographs (figure 21), however, and particularly across the humanities, this statement is rather disagreed with.

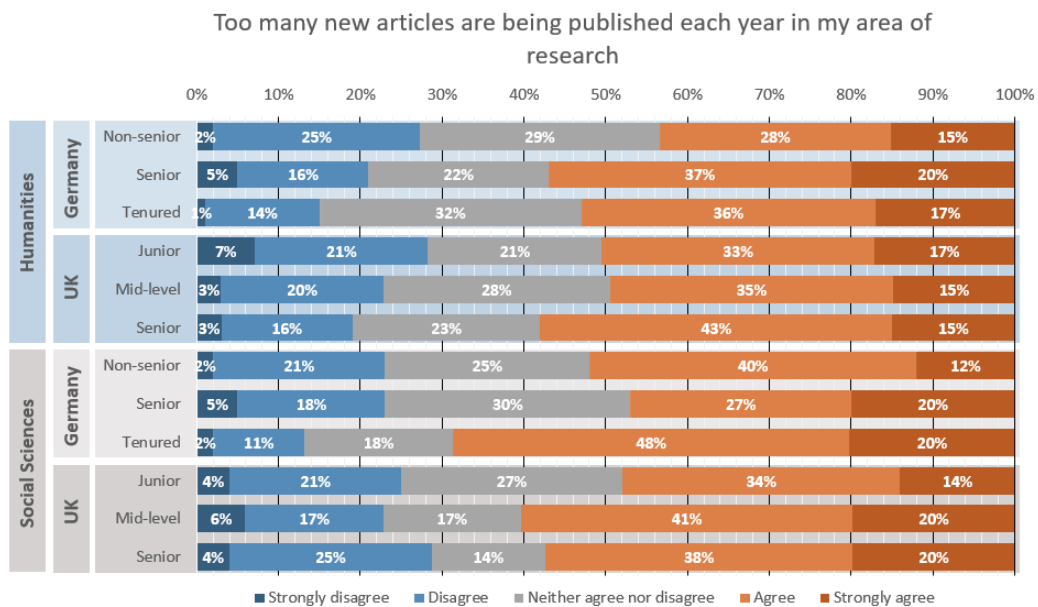


Figure 20: Too many new articles being published

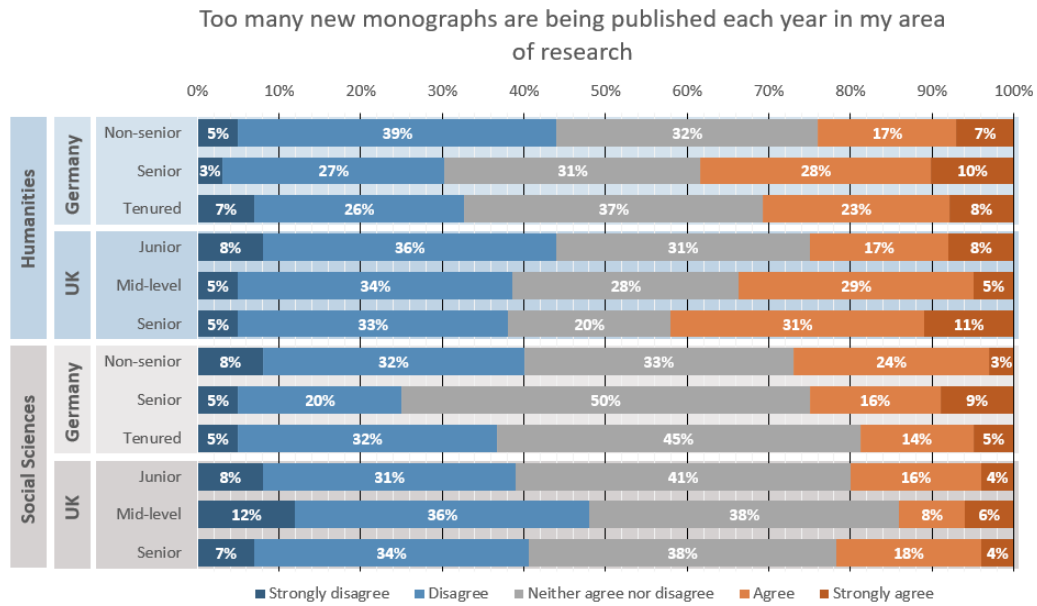


Figure 21: Too many new monographs being published.

As a final statement, figure 22 shows how, with increasing seniority (and, thus, with more established careers), the notion of the relevance of the strong publication list increases significantly. Rates are also much higher in the UK than they are in Germany, and higher among social scientists than among humanists. Of course, not yet having made a career does not allow one to draw conclusions about the importance of publications for a potential career. But, in turn, having made a career relies on the strong record of publications, as scholars themselves perceive it. It cannot be induced from this item a conception where the personal list dominates above scholarship as such or the record of teaching or even something else that made the career (as these items have not been enquired). But the statement suggests that the personal list is indeed crucial.

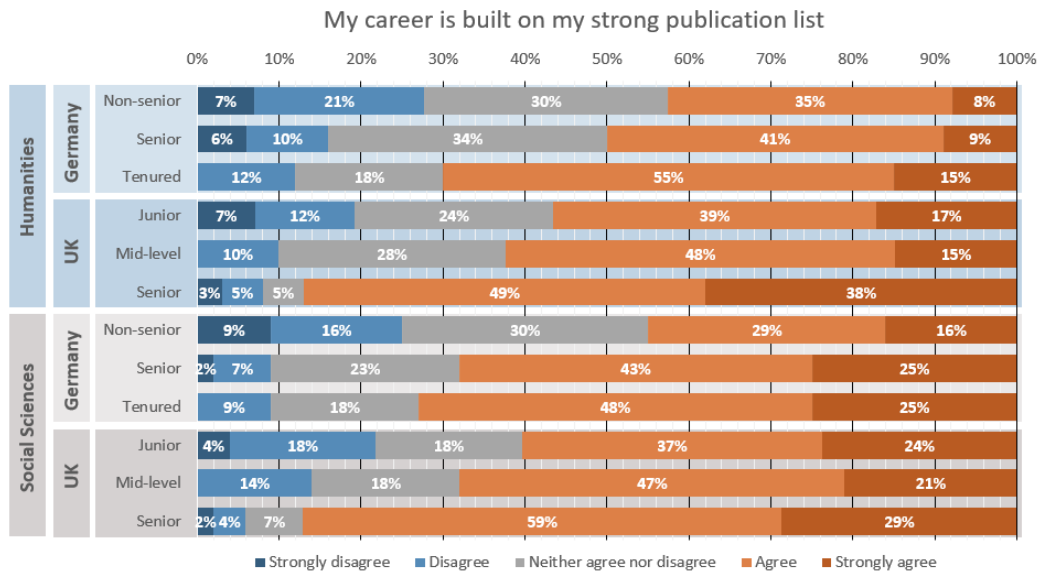


Figure 22: The importance of the publication list.

4.2 Country-Specific Forms of Pressure

The data of 4.1 show significant differences between the two sites of my case study frame, Germany and the UK. Though there are some similarities, especially in regards of publishing pressure, the number of items in which scholars in the two countries diverge is much higher than those in which they align. In this sub-chapter, I discuss this in detail.

Overview of Pressure (Groups)

Most instructive as a starting point for my analysis are the pressure groups and the distribution of characteristics across these. To fully understand this, the distribution of the same characteristics across units in the original sample has to be remembered (chapter 3, figures 4/5). These indicate that, in the UK, career levels are more focussed and progressive. There are also more similarities between the humanities and the social

sciences. This relates to the progressive and, as it were, more universally competitive nature of the career system in the UK.

This is different in Germany where the system is characterised much more by a hierarchy focussing on the end of a few professorial chairs with a bulk of non-professorial mid- and senior positions. The path of professorship is mostly decided quite early on during the competitive years of building the foundations of a career, which decide whether, eventually, a tenured position will be within reach or not. This is visible in the data as the early career scholars are quite close in terms of age, while the senior clusters are very loose and broad. Another characteristic in Germany is the discrepancy between disciplinary clusters. Said characteristics are predominantly visible in the humanities, while the social sciences seem to be closer to the UK. This can be explained by the dominance of the traditional mode of career advancement and the cultural notion of the *Gelehrte*, to which the humanities seem to be much more closely tied than the more progressive social sciences.

Turning to the forming of pressure groups, the appearance of clusters changes substantially.¹³ In this representation, Germany seems to be the progressive case. Experienced high pressure is very focussed on early career stages, progressing to the more loosely defined low pressure group. No pressure is experienced by a diverse, though decidedly senior group of scholars.

Contrary to this, experienced pressure is much less focussed in the UK. This difference is especially visible if the high pressure groups are compared with each other. As Germany shows a standard deviation of only 6.42 and a mean of 9.5 (median 8), the UK shows a standard deviation of 8.96 and a mean of 14.1 (median 12). This already anticipates

¹³ As said before, by turning from the representation of sample to that of pressure groups, the differentiation based on seniority is abandoned. This allows for a direct comparison of the countries which the categorical seniority did not allow beforehand.

the correlations to be explored subsequently: namely that pressure in the UK is not so much an early career pressure (getting on a path to a tenured position), but a much more universal pressure that also impacts scholars in higher positions. In this sense, these representations are a crucial foundation for further analysis.

Moreover, this early overview also allows to draw first conclusions about the hypotheses. Most of these can be confirmed without restrictions. Pressure to publish is higher in the UK than it is in Germany, and it is much higher among early career scholars generally. And yet, the distribution and subtle differences make this data relevant beyond mere confirmation of hypotheses.

The strong connection to official assessments in the UK, the perceived temporal pressure, and its relation to quality all substantiate the claims that authorship and publishing are essential practices through which a systemic pressure to perform is funnelled. The differences of distributions among early career scholars between the two countries shows the nature of progress and advancement of the systems and reveals the variations between Germany and the UK. In the case of the latter, pressure has a more distributed impact. These connections need to be looked at more closely as we turn to the next section.

Details of Experienced Pressure

As discussed so far, the structure of experienced pressure differs between the two countries. It remains to be explained, to what the differences are tied: what structural explanation can be found? I already indicated that the REF seems to be a dominant issue. This is underpinned by data in different respects.

The connection to research assessments becomes clear especially in figures 16/17. The overwhelming rejection of a perceived connection of publishing behaviour and assessments in Germany is opposed by very high rates of a perceived connection in the UK. Where the nominal remains descriptive, the relative impact as represented in pressure groups is instructive in terms of the distribution, especially in the UK. There is a near eightfold increase of a perceived connection between those who experience no pressure and those experiencing high pressure. This means that the negative impact of the REF on one's own publishing behaviour is experienced as being eight times stronger if one indicates to experience publishing pressure generally. In addition, the share of scholars indicating a positive connection more than halves between the two groups.

We can read from this data already the dominance that official assessments have on publishing behaviour. The negative impact indicated here needs to be considered a clear explanans for the broader formation of pressure in the UK as discussed above. And this can be extended with more detailed enquiries. Publishing behaviour as such, of course, is quite abstract. But questions of temporality and perceived quality help clarify the mode of impact of the more general experienced pressure. In this respect, the differences between the two countries are not as radical as with the impact of research assessments generally, but they remain substantial.

For the UK, it can be summarised that there are slightly more scholars who feel pressured to publish faster because of research assessments than there are scholars who experience that this temporal pressure has an impact on the quality of their work. Nonetheless, rates are soaring across levels of seniority in this respect.

In other words, temporality is a key issue in the UK. The impact of a temporal pressure in relation to research assessments dominates, but it needs to be differentiated since a fraction of scholars seems to experience this temporal pressure positively. However,

the overwhelming number of scholars who confirm the prevalence of temporal pressure indicate a negative impact. The same is true in respect to quality: though there are some scholars who indicate perceived temporal pressure alongside less of an impact on the quality of their work, most scholars claim that this temporal pressure indeed leads to a suffering of the quality of their work. This is a crucial indicator that lays the abstract basis for the discussion of interview data in the next chapters. Interviewees highlight an issue of temporality and quality as the most pressing concern in regard to the REF. This will contextualise the abstract notions discussed here.

Since a concern with publishing behaviour in regard to research assessments is much less of an issue in Germany, the connections with temporality and impact on quality also have to be read differently. It has to be borne in mind, most of all, that even among scholars who claim to experience a high amount of pressure to publish, merely one among six scholars claims research assessments play a negative role in this respect (while it is four out of five scholars in the UK). This difference to the UK is also visible in detail where the issue of temporality and perceived quality has the opposite appearance in Germany. Only few scholars claim that they perceive a pressure to publish earlier in order to have their publications included in research assessments. All the while, many more scholars perceive a temporal pressure generally, which they see as having an impact on the quality of their publications.

It needs to be inferred, therefore, that temporality has an impact in Germany as well, but it is much smaller than in the UK. Nevertheless, it is considered to have a negative impact on quality, but this is not connected to assessments. This feeds into the assumption, contextualised in the subsequent chapters, that the case of Germany cannot be focussed on a defined, direct source of pressure. The *Exzellenzinitiative*, unlike the REF, is not a material source of pressure, but comes with a rhetorical wave of productivity and

internationalisation that exerts more subtle pressure. This provides less space to being pinpointed analytically. The very mixed results visible in the respects discussed here, therefore, require a more nuanced approach to being contextualised. The framework of cultural hybridity will help in this respect (chapter 6).

Wider Perceptions about Publishing

Even beyond the perceptions of pressure in general, the differences between Germany and the UK are perceptible in the data. A special case on its own is the issue of OA. Only a fraction of scholars in Germany feels pressured in this respect. But it is of considerable concern to about a half of scholars in the UK, especially in the humanities. On the one hand, this may again be explained by the REF which emphasises the importance of OA and enforces a broader discourse even before new rules were established. This fed into a culture shift. The end of this, however, is not yet decided as publishers, funders, and institutions have to find suitable paths forward especially in material terms. It can be assumed, therefore, that departmental guidelines that clarify local approaches to the REF will include specific OA agendas. This is confronted by a lack of funding and institutional support as the debates around OA indicate (I briefly discuss OA in chapter 10). On the other hand, there seems to be more of an intrinsic value in OA in Germany as further data about monograph publishing in my survey indicate, as I discuss in chapter 6.

Quite subtle differences can be seen in regard to brands. Overall, there are only a few scholars who confirm they are given departmental advice on the matter of publishing brands. This issue is one of the often-heard rumours among early career scholars: that there are lists of publishers and that scholars are advised to publish with one from a particular list

(for matters of impact or visibility). This cannot generally be confirmed, especially in Germany. However, a few scholars in the UK seem to have experienced such forms of advisory feedback. This is not necessarily negative as such. But the data provides a noteworthy indicator for the value of brands, which feeds into my conception of venues and stratification, as provided in chapter 10.

No statistically significant differences between Germany and the UK can be found in regard to the number of texts being published. Generally, the hypotheses can be confirmed, though: scholars perceive that too many articles are being published in their areas of research. Scholars in Germany and the UK are quite close in their perceptions about this issue. However, this looks a bit different in regard to monographs. First of all, the hypothesis cannot be confirmed that scholars experience an over-publication of monographs. Though the differences between Germany and the UK are again small, there is more confirmation from the humanities than from social scientists in this regard.

The wider perceptions about publishing indicate that there are indeed similarities between Germany and the UK, as in the case of over-publication, alongside strongly opposing perceptions, as in the case of OA. This is an instructive mixture, since publishing markets seem to converge internationally so that an over-publication in one country may quite naturally also evoke similar perceptions in other countries. However, the matter of OA is rather nationally-driven in the context of policies and funding mechanisms.

4.3 Qualitative Statements

Next to the quantitative items, the survey included an open comment field. This field was supposed to allow any comments about the survey that the respondent may have felt necessary. In fact, an overwhelming number of 110 scholars submitted a comment (38 by

German, 72 by UK scholars), sometimes providing well over 200 words. The respondents represent the whole gamut of academic levels and age, and their fields range across the humanities and social sciences, from Catalan philology, German memory and identity studies, and prehistoric archaeology to criminology, economic geography, or the sociology of families and relationships.

Because of the richness of this dataset, I conducted an additional thematic analysis for this item. Of the 110 comments, 23 are about the survey itself, welcoming the research or criticising a particular question. The remaining 87 questions are comments qualifying and contextualising some of the topics touched upon in the survey. I tagged these comments to sentiments and themes, as can be seen in the appendix (A6). I provide and discuss selected references here to indicate the context of the comments.

Overall, the responses are very negative with only a few positive ones. These are mostly about OA or positive pressure, as a historian in the UK argues: ‘pressure motivates’. 17 comments are sorted into *mixed sentiment*. These comments are mostly explanatory in nature as they contextualise particular mechanisms, or are neither clearly negative nor positive. Among these are also such remarkable responses as: ‘I do not really believe in research. I see teaching as the main part of my job’. Or: ‘I publish only for my private pleasure and for my personal scientific knowledge’.

The majority of 66 comments is of negative sentiment. Predominant themes here are assessment regimes, OA, or paradigmatically quantity over quality. The REF is particularly in focus. It is claimed that the REF enforces ‘overpublication’ and that it is ‘an ugly millstone for academics and universities, alike’. It demands quicker publishing irrespective of quality or the development of scholarship. In a quite long essay, an experienced scholar in the UK relates the decline of quality to the REF as well as the matter of too many media, which themselves induce the issue of a supposed blind quality check. The respondent

considers this to be a farce as she has experienced inappropriate reviews and sheer unintelligibility in reviews. Further related to assessment regimes is the relentless speed of change, driven by official policy. The principles of the REF, ultimately, ‘allows already established scholars [...] to maintain much more visibility and hinders junior academics in the field to gain momentum’.

The pressure to publish is often related to career mechanisms such that it increases with the ‘pressure of an uncertain future in academia’. There is extra pressure on PhD students to publish. The large amount of reviewing work stemming from the often claimed too many publications is also a matter that is related to early career scholars. This, in turn, leads to a decline of the qualities of both the submissions as well as the processes of reviewing. In the end, the ‘pressure to publish in high quantities is crushing ECRs, such that only those who work 100+ hours a week seem to be able to find any success’. Alternatives are hardly mentioned; teaching only contracts and the systematically precarious contracts further put pressure on the scholar.

OA is problematised because of its lack of a coherent system and funding approach. The basic idea of OA to create a more equal publishing environment seems to have been lost in the practices of how it is established: it is more an institutional compulsion than a favourable practice. Quite often in this respect, but also at other instances, scholars lament the fact that publishers gain excessive profit margins.

Lastly, individual issues are also often mentioned, sometimes with outright hostility. Among those are the focus on metrics, the senseless increase of useless publications, the decrease of quality in response to this, research management practices that disregard actual scholarship in favour of performativity, and minimalization of disciplinary differences, which is said to harm the humanities in particular.

4.4 Conclusion

Opposing perceptions dominate the comparison of Germany and the UK. This is especially true in regard to issues of pressure and publishing behaviour. Temporality and the REF are the strong emphases that scholars in the UK articulate. Most of all, pressure is experienced widely across levels of seniority. This is a mostly negatively-experienced pressure. Rather than feeling a somewhat positive pressure, scholars in Germany experience no pressure in the mode of a direct exertion (such as through the *Exzellenzinitiative*). A temporal pressure is visible, nonetheless.

Moreover, the qualitative statements within the quantitative survey already provide some contextual differentiation. The REF is indeed a pervasive concern among scholars in the UK and its direct connection to publishing practices is often mentioned. In Germany, there is no such material issue and the *Exzellenzinitiative* seems to have much less of an impact, which connects to the quantitative data. Put forward here, however, is the concern of a lack of quality and the pervasiveness of being evaluated in terms of numbers instead of in terms of the quality of publications.

Most of all, this qualitative section is revealing in terms of its distribution in itself. Although the survey received slightly more responses from scholars in Germany, many more scholars from the UK took the effort to vocalise their concerns in the comments section. As there are some smaller essays, even amounting to 200 words, some scholars seemed to have felt a pressing issue which they wished to put forward here. As the sentiment is largely negative, the matter of publishing is indeed a field in which scholars perceive forms of alienation, making the discussion in chapters 8 (conceptually) and 9 (empirically) valuable contributions to understanding this issue.

Overall, this data is an instructive basis for looking into the specifics of the two countries in the following two chapters, laying the quantitative foundation for qualitative

enquiry. Of concern is particularly to clarify the impact of the REF in the UK and to contextualise the matter of temporality. In Germany, however, the source of the more abstract, subtle pressure will be clarified. Germany is divided on most grounds and does not show the clear tendencies as scholars in the UK do. Chapter 6 will clarify this internal division that highlights the forming of subjects.

5 The UK's REF: Authorship as Institutional Enhancement

Formal authorship is employed as a tool for the management of reputation and research resource allocation in the UK: because of the REF, scholarly institutions instrumentalise discursive artefacts to be awarded future symbolic and material value. The REF distributes research funds, it shapes discourse about the development of research and its excellence in the UK and internationally, it allocates labels of achievement with consecratory power, and it employs a large amount of administrators and scholars who plan and execute one of the largest performance assessments worldwide.

The REF strengthens formal rationalistic culture principles expressed as auditing and accounting, a culture that transcends the direct practices of the REF. Publishing practices embody this culture which is exemplified by scholars publishing *because* of the REF—or: scholars aim to become *REFable*. In this sense, the teleology of means turns from the REF to institutions to (again especially, but not only, early career) scholars whose publishing practices foster strategies to enrich authorship value. Institutions regard scholars literally as instrumental to their future: those scholars that are likely to perform better in a REF are deemed more valuable. In response, scholars have to make sure they fit into the departmental or institutional guidelines pertaining to the REF, both to be able to provide the institution with the required authorship value and to make sure they can keep their position. The communication of scholarship has, through the successive exercises of RAE and REF, become rationalised as a management tool for the gains of funding and reputation.

The REF is, thus, a material actualisation of a culture shift from scholarly substance to formally rational means. And yet, the way this culture shift shapes publishing practices is fractured by institutional practices: each institution realises responses to the REF on its own which means that each institution potentially mediates culture principles slightly

differently, even though there is one overarching institution of the REF. Because of this, the focal point for investigating the impact on publishing practices is on institutions and their ways of handling *REFability*. In this line of argument, I focus on the systemic shift away from principles of the self-sustaining academy towards a culture of auditing and accountability expressed through and mediated by the job market depending on authorship.

In regard to the REF itself: if not otherwise stated, notions of publishing are focussed on the area of *scholarship. Impact* or *environment*, the two minor areas of assessment of the REF, are only partially relevant during this discussion. However, they are actualisation of the same principles so that they, arguably, bear a similar impact on practices.

5.1 'I Need to Produce A Certain Number of Outputs in a Particular Period'

The quite negative image that the REF has in the literature has been confirmed in both of my studies. The interviews offer the qualitative context about how authorship is strategized in response to the REF: the notion of strategic publishing elaborated on in the preceding chapter becomes more nuanced here. And yet—or perhaps because of this—thinking and talking about publications in relation to the REF has a very mechanistic, instrumental appeal. The second sense of alienation (as in the teleology of instrumental means) is evident here and may be summarised as: the REF does not review past discourse as it was shaped from within—discourse is shaped from without to be reviewed by a future REF.

The REF in General

My interviewees were, overall, very negative, even showing consternation about the REF. Positive voices are scarce. Richard, the senior historian, is the most balanced in his perspective. He articulates the encouraging aspect that the peer review exercise that the REF represents is potentially the best option available to distribute funds. Richard demonstrates a sense of *scholarly* accountability in that scholars are mutually respectful while reviewing each other's work since, in the end, they are all in the same boat. Other positive aspects come from Karen and Tabea. The former claims that the REF can be good as an exercise to redistribute funds (though the interviewee does not further detail ways of or even potential for redistribution). Tabea asserts that the most beneficial outcome of the REF is the way it can push senior colleagues to publish: 'I have had older colleagues when I was beginning my career who'd never really published all that much and then have had to and published wonderful work'.

On the level of general perspectives, the managerial nature and institutional burden of the REF appeared as one of the themes of negative impact. For instance, Quintessa is very explicit as she voices anger about the definitions and standards that are implied either in the policy or the departmental structures she is located in. Particularly of concern for her is that empirical research seems to be valued more than theory or argumentative approaches (of the kind she authors). To that end, she feels as if her work is simply less valuable and, because of this, she is regarded as less valuable for the department or university. She says she got the sense that 'if you want a 4 star paper had- it needed to be like multi-centre international randomised control trial with a gazillion participants-' which she thinks is damaging to the assumptions about what constitutes good scholarship in her field. This statement is unlikely to be generalisable but illustrates her negative perception and the way the REF might, discipline-specifically, enforce a form of conformism.

Omar is also quite straightforward in his criticism. As a senior philosopher with roots in the US, he is very engaged in assessing the value or efficiency of the exercise, though only with negative results as exemplified by his conclusion: 'overall the REF is bad [...] it's a huge drain on resources' and 'I think they could do away with the REF tomorrow and there would be no downside and lots of upsides to British academia'.

This is also one of the ways Richard, usually REF-positively-minded, is critical about the REF. He notes, as does some literature, that 'one suggestion is that you could just do a per capita dispersal and actually that would probably quite closely reproduce the results of the REF'.

Others join the criticism of the efficiency of the managerially-driven practice. Melissa, for instance, says that the REF is a 'box ticking exercise' and people have to make sure that they are *REFable* (a neologism that expresses adherence of individuals to the rules of the REF, especially in terms of formally published performativity; the term appears in three of nine UK interviews). Quintessa further articulates the onerous nature of the practice and its added workload as it is 'hard not to see the REF- in a- this kind of hoop-jumping exercise that takes an awful lot of peoples' time'.

Another aspect of the institutional embeddedness is that of intelligibility: the question of who knows what about the REF. As the complexity of the REF is revealed in the literature, it becomes quite clear that individual scholars often do not know explicitly about the policy, but rather about the requirements put forward by their departments or job descriptions. In this sense, the non-senior interviewees often implicitly highlighted a sense of the complexity of the REF—a sense of not having fully understood how it works. Penelope was most explicit about this: 'I have some notion of it [the REF] but it's not like someone sat me down and communicated to me- we have this review coming up and you really need to do well'. This statement, again, may not be generalisable: it pertains to the

difference of institutions such that elite schools value freedom of scholarship and trust their scholars in regard to output; at non-elite schools, this seems to be very different in that these institutions foster their scholars towards using the REF as a cause to publish.

The REF and Publishing Practices

In regard to publishing practices, one of the key themes that emerged during the interviews is that pressure is systematised because of the REF. This pressure leads to strategizing authorship and publishing (and because of this, strategizing of scholarship). This seems to be much more, but not only, prevalent among early career scholars or those not working on permanent or long-term contracts.

Melissa acknowledges that the REF is 'an underlying thing I have in mind'. Karen says that she herself has not felt the pressure of the REF as she had a suitable number of submissions at hand when she was on the job market. Nevertheless, she articulates that the REF creates a lot of anxiety and pressure among non-permanent people. In that way, she draws a direct line between the impact of the REF on institutions and the academic job market. And she concludes with consternation: 'universities will advertise- kind of approach people for their publications for REF- to try and get people to move for the REF'.

In the opening question, Tabea already connects the purpose of publishing in general, and the basics of her job as an academic, to the REF as she is 'very aware that I need to produce a certain number of outputs in a particular period'. Like the former respondents, she echoes the strategic decision-making, but emphasises the temporalities of publishing: 'I just got a book about to come out and I probably revised its scope and the timetable in which I was working particularly strategically for it to come out in this REF-

cycle'. Still, she is ambiguous about the extent of the direct impact of the REF and the potential that the strategizing is already attached to a larger structure and ingrained in a competitive notion of the job market and career pressure in general. She explains that

in some British institutions there's now a great emphasis on quantity. And that is- tends to be because of needing to produce- have that number of things to go into the first REF cycle even if you are pretty junior.

The point about the systemness is also taken up by Samuel. His statement quite clearly evokes the sense that the pressure induced by the REF, and earlier by the RAE, is by now already systemic beyond the REF. Samuel claims that nowadays the mechanisms and pressures underlying the REF have been absorbed by the academic system: 'you could take the REF off and not much would change at least in the short run'. The job market itself, he says, runs on the principle that you need to have a certain number of formal publications of certain quality to get a job which, in the end, means that you are adhering to the principles of the REF as well. As a result, 'at the moment the immediate pressure I think is coming from the universities themselves more than directly through the REF'. And again, here, the most pressure on authorship lies on the shoulders of non-senior scholars, those with non-permanent positions.

Nora, as before the fiercest critic, relates pressure that is induced or upheld by the REF to the individual practice, which especially alludes to disciplinary antagonism and power: 'there is such hostility and there is so much authority vested in the REF- it gives a license to bully academics who do not follow the rules- who don't go along with the crowd'.

This point is also prevalent in another emerging theme, which points to potential content-related conservatism and practices detrimental to scholarship. Nora exemplifies how authorship is alienated as she remembers her early career time which coincided with

the beginning of the RAE in England and that she 'had started writing things for publication because I had understood in a general sense that that would help me to get a job'. Furthermore, in the contemporary environment of expectations and criteria, she puts forward a rather bleak account of the REF's reinforcement of conservatism:

the REF has promoted superficiality in publishing- [...] it has lowered the quality of the journals and the publishers- [...] it has narrowed specialisms- [...] it has led to career profiles which are extremely problematic. So for me the REF is just a disaster.

Melissa claims a similar position in regard to authorship in her field in philology where it appears to be the case that some content is more *REFable* than others. Just as it is claimed to be an effect of peer review, Melissa notes that scholars who are 'more tinkerers by nature' are less interested in 'working out big or controversial ideas' so that the pressure from the REF impacts these scholars more as it is less *REFable* content.

The last substantial theme, one that emerged entirely during the interviews, is that of temporality. Quantity is a key issue for strategizing publishing. As outlined before, it is often, or often needs to be, balanced with the few high-quality publications. The REF seems to reinforce this mode of strategizing, since quantity is bounded by the exercise. The pressure, thus, seems to be more on the temporality and genre of those few high-quality publications. Witness Omar who says that the REF 'has actually in some ways encouraged me to slow down my publications'. He goes on to talk about the strategic decision-making that the REF encourages: on the one hand, these are practical concerns that contributions to edited collections are not worth as much and that some publishers take ages to publish such a contribution; and on the other hand, the decision-making is based on genre-focussed concerns to work strategically on certain genres to publish within one or the next REF periods. Thus, Omar emphasises that the quality-related pressure is very much focussed on temporality.

Richard relates this theme to the history of the REF as it was much more about quantity in the past (before it turned from RAE to REF). Richard says that it was 'the more the better' back then. However, it 'never was a quantity assessment- it was always a peer review process but managers don't understand that'. He says the external research management enforced the quantity drive, which resulted in 'this kind of knee jerk- kind of metric educated expectation'. In its direct impact on publishing practices, Richard reiterates pressure both in its temporality as well as stemming from the competitive job market: 'I think that the REF and the job market both together of course cause people to publish too quickly'.

Individual Excellence and Notions of Productivity

My interviews included questions about the meaning of particular categories such as productivity or the excellent scholar. Answers to these questions can be revealing about the shared meaning in discourse, representing articulations of culture principles.

Regarding excellence, answers here fundamentally reiterate the critiques already witnessed about the REF more generally. Nora, for instance, rejects excellence entirely for its emblematic signification of corporatisation. Others such as John or Richard remark that attributes of productivity or excellence may not be easy to be applied to persons at all.

Some interviewees constructed a somewhat normative conceptualisation of excellence in relation to productivity *ad hoc*. An excellent scholar—substantially and not in the sense of rhetorical excellence—is really 'someone who spends a lot of their time kind of thinking about the ideas that they're grappling with' in the words of Quintessa. What comes through here, first and foremost, is the indifference to reward, particularly the systemic reward of output recognition or reputation through publications, the

performativity of formal publishing. Excellence in this sense of content and scholarly effort is more elusive in its nature than formal output is. It was in this respect also Quintessa who remarked that it may not even be focus as such that evokes the indifference to recognition, but the crude mechanisms of authorship in the humanities that do not allow for the accounting for *real*—pragmatic, contribution-focussed—authorship. Mentorships, discussions, even close readings often find no recognition in humanities scholarship except, perhaps, in informal acknowledgements.

Tabea refers to 'regular publishing' as the standard attribute of productivity, just as Penelope says that 'really intuitively a productive researcher presumably is someone who publishes a lot'. Karen remarked that a productive scholar is one who is 'trying to somehow integrate the different parts of the job' which includes research and teaching but also networking, helping junior colleagues, or being a mentor.

Some of these answers support my argument about the community character of discourses where these are not to be seen as silos that accrue publications but social communities of engagement with particular epistemes. Explicit in this respect is Nora's statement about productive scholarship:

I believe in a community of scholarship- [...] this isn't about elitism at all. [...] it's about the engagement with difficulty- it's about staying with difficulty- it's about knowing your field. It's about reading- above all it's about the ability to read closely- critically- and analytically in detail and acknowledging prior engagement with difficulty and engaging with that-

I come back to this theme in the chapter 6 where the answers are mirrored by German interviewees.

5.2 Desiring the *REFable*: Institutions and Symbolic Value

R(a)eification

Reviewing existing literature about the REF, there are many aspects this exercise (and the earlier RAE) is criticised for: the way it reproduces existing hierarchies (Dix, 2016; Münch, 2008: 134); the mechanism of redefining institutional roles or moving efforts away from teaching (Frank et al., 2019; Henkel, 1999); the way it promotes competition but not fully considering corresponding market mechanisms (Frank et al., 2019; Shackleton and Booth, 2015); the fact that it is quite an expensive publicly-funded exercise benefitting only a few institutions (Arnold et al., 2018). Resulting from this, it can be argued to be a classic example of an institution that is only giving the appearance of rational conduct while being primarily inefficient (Meyer and Rowan, 1977); that the results of the REF are transmuted from a funding into a ‘research *ranking* system’ (Brink, 2018: 82; emphasis in original) and that it is partly based on an instrumentalist principle of benefit (Brink, 2018: 168–177); that the costs of impact assessment of this mode will likely outweigh its benefits (Martin, 2011); or the way it puts pressure particularly on younger scholars (Archer, 2008). Ultimately, as Frank et al. call it, ‘the RAE/REF became a victim of its own success’ (2019: 81), which means it has acquired such a strong position that it has become a distortion of that which it was supposed to measure. In other words, the REF has fallen prey to Goodhart’s law.

Arguing about the early RAE, Loftus showcases the furthering of formal rationality in British academia and its strong characteristic of r(a)eification as alienation. With Lukács and Marx, he argues that

the tyranny of publication quotas, and the curious bureaucratic rigmarole that goes into assessing work, only to reduce that assessment to a string of one, two or three star ratings, is an indication of the heights to which RAE-ification is now pervading the consciousness of the academic (Loftus, 2006: 110).

This is the exemplification of a Marxian commodification and, in my terminology, the exemplification of the instrumentalization of publishing practices. It sets the argumentative background for how the REF shapes authorship and publishing, or rather how these practices embody the principles that underly the REF. In fact, this is a crucial differentiation: the REF does not put pressure on the individual. It does not even do so in the case of departments or universities as they are free to choose whether they wish to compete on the principles put forward by the REF. The REF is a funding mechanism with only relatively small financial impact, as I discuss below. The functioning of universities does not depend on it. Irrespective of funding, however, the reputational impact creates a structural pressure because of which institutions do care disproportionately strongly.

This perspectival shift from materiality to symbolism illustrates my argument that the REF is only the realisation and, thus, strengthening of wider culture principles. And, of course, this culture works beyond the humanities but has an especial impact here, just as the technocratic means explained in the preceding chapter impact the humanities in ways other than in the sciences from which they partially originate. And since impact of the REF is a top-down effect relative to the elite stratification of institutions in the UK, it is a top-down pressure to care: the symbolic value created of highly *REFable* scholarship fares as a crucial, marketable label in the pursuit of being a service-oriented institution in a market of education, research, and scholarship. Being better *REFable* (as well as *TEFable*) is deemed to symbolically signpost that an institution is providing value for money, that it is providing a highly rewarding scholarly environment to attract good scholars, or simply: labelling to be excellent in an environment where the rhetoric of excellence has material value.

Moreover, the label of the *X-percentage of world-leading content* is a means of social closure in the Weberian sense of the 'stratification by status' (Weber, [1922] 1978: 935). This comes about here in the historical power an institution already has to produce highly

REFable content. These institutions can further monopolise on the symbolic value (as well as being awarded further material means) to reproduce status and close the hierarchy to outsiders. I explain this in more detail in the following sections.

A Short Political History of RAE/REF

The RAE began under the auspices of Peter Swinnerton-Dyer, a mathematician at the University of Cambridge, who chaired the University Grants Committee (UGC) from 1983 onwards. The UGC was responsible for the distribution of core funding for research and teaching in the UK. This was a quite straightforward exercise then, since universities in the UK were less hierarchically organised compared with German universities. Researchers enjoyed a high degree of autonomy and flexibility with regard to research endeavours. Most funding was distributed based on unit size, with a few elite lobbying exceptions such as Oxford and Cambridge (Whitley et al., 2010: 53–54).

During the Thatcher government, public spending was to be cut down, which meant considerable reductions to university and research funding. It was the UGC's task to determine a process to allocate reductions systematically. In hindsight, Swinnerton-Dyer claims the key issue in this allocation was transparency (Jump, 2013). In other words, the question of which institution receives cuts on what basis needed to be made clear to all involved. Alongside this, however, demands for more accountability rose as well. Future funds were to be distributed on the basis of clear and competitive objectives, efficient management, and measurable results (which emphasises key culture principles of the politics underlying the rising neoliberalism in these times more generally). As a result, the Research Selectivity Exercise (RSE) was initiated in 1986, often regarded as the first RAE despite its name variant.

The exercise's methodology was much simpler compared with today's REF, since the UGC itself ran on tight budgets. The RSE consisted of a questionnaire distributed to all units of assessments, which could be formed of individual departments, groups of departments, or a part of a department at a university and, therefore, varied considerably in size. The questionnaire enquired about matters such as 'research income, research planning, and priorities' as well as a unit's 'five best publications from the previous five years' (Whitley et al., 2010: 55).

At this initial stage, the performance-based funding had little material influence. Nevertheless, complaints about a biased approach were voiced even during this earlier simple exercise (Jump, 2013). Likewise, the cultural development towards managing research more professionally is visible already at this early stage, as 'the authority of university management vis-à-vis its researchers increased' (Whitley et al., 2010: 55).

Backed by this development, the UGC was replaced by the Universities Funding Council (UFC) in 1988, which conducted a more sophisticated RSE in 1989 with an especial focus on publication data, including relative output numbers and connections to full-time staff. This funding mechanism was already dominated by the focus on publications and publicly visible outputs, which were to be peer reviewed. Units were subsequently rated, so that this second RSE already shows considerable similarities to the next funding round, then termed RAE, and to the later established REF.

In between the first RAE in 1992 and 1989's RSE, however, an important development took place: the *1992 Further and Higher Education Act* ended the binary divide between universities and polytechnics. This seemed to considerably increase competition for limited funds. It only did so formally, though. As Bence and Oppenheim show, depending on the 2,800 submissions from 192 institutions, 'older universities received 91% of available research funding, new universities 7% and colleges 2%' (2005: 146).

This shows that the earlier voiced concerns over bias extended even beyond substantial reforms. From this point onwards, and even though this was initially conceived of as a system for determining research funding in relation to cuts, the UK established for itself the basis of a performance-based research funding system (PRFS), which distributed funds on a competitive basis, until the present day's REF. The functioning and cultural underpinnings of such a PRFS are worth a closer look for understanding today's REF, to which I turn in the following section.

The REF and its Symbolic Value

In its own words, today's REF 'is the UK's system for assessing the quality of research in UK higher education institutions' (REF, 2020a: n.p.). Its threefold purpose is to provide 'accountability for public investment', allow for a 'benchmarking' of the research conducted in the UK, and 'inform the selective allocation of funding' (REF, 2020b: n.p.). To fulfil such expectations, the funding bodies responsible for the REF conduct an extensive process of expert review of publication outputs and impact statements every four to six years. All potential fields of scholarly enquiry are divided into four main panels, in 2021 comprising of a total of 34 units of assessment; panel D is the relevant section for arts and humanities.

The REF is a PRFS as well as a quality-related funding mechanism (QR). In other words, performance is rewarded if it is of certain quality. According to expert opinion in panel D, the UK is well-positioned in the world to lead scholarly discourses in the arts and humanities. Submissions in 2014 'demonstrated the considerable strength and contribution of arts and humanities research in the UK' with one in four research outputs being world-leading (REF, 2015d: 1). The merit is particularly based on the three strongest areas with

the highest world-leading scores: classics, English language and literature, and history. This proves—and it may not come as a surprise—many scholars in the UK are indeed world leading in English philology (at least 28.6% of their research output is; REF, 2015d: 12). For the sake of completeness, panel A also saw that ‘UK research in life sciences is in a very strong position worldwide’ (REF, 2015a: 3), and ‘the strength of research in the UK in physical, mathematical and computer sciences, and in engineering’ is proven in panel B by even claiming 26% to be world-leading (REF, 2015b: 1). Panel C witnessed an ‘indication of the resilience and strength in depth of the social sciences [...] within the UK’ (REF, 2015c: 2)—there might be room for improvement in terms of excellence in panel C.

For sure, PRFSs go beyond the semantics of world-leading and excellence. They are complex systems as they are deeply embedded in the symbolic and material structures of a national academic system and, in their historical development, shape scholarly culture through institutional practices. They are, in this sense, more than mere mechanisms for the distribution of funds but neither are they crude mechanisms of materialist elite reproduction. Hicks provides a comprehensive overview and defines five criteria to separate PRFSs from other performance-based distribution mechanisms: *output of research* must be evaluated *ex post* in a *national* system where the distribution of *public funding depends* on this evaluative exercise (Hicks, 2012: 252).

PRFSs are conceptually strongly related to NPM, only the aspect of the deployment of increased use of contracting, which is important for NPM, is not elemental to PRFSs (Hicks, 2012: 254). And yet, linking funding, and potentially the basis for life chances and careers, performance is at the heart of NPM. It gives institutional or governmental management a tool that allows for increased governmentality and the set-up of formal rationalistic strategy. Performance is deemed important to the extent that it is not only *measured* but *managed* in the way that assessment affects strategy not just reflexively, but

innately. Long-term development turns into contingent accumulation of short-term effects on a rolling basis. Any future scenario becomes fragile so long as early indicators and operative intelligence do not point towards successful completion of projected performance. This shift from measurement to management, hailed as reformative success, is described in peculiar terms such as the way reformers 'have transformed performance measurement into performance management by linking the assessment process with management of government strategies and tactics' (Kettl, 2006: 47). This raises the conceptual question of what kind of performance this is that is explicitly *managed* and not only *measured*? This already strongly indicates that it is not a performance driven innately by scholarship since the management comes from without.

PRFSs are, in this respect, mechanisms that aim for improvement. And even though the REF only recurs about every six years, its principled audit culture affects practices beyond the direct influence of the exercise in praxis. If regarded only in the light of such targeted improvement, it can be explained how institutional structures shift to align with the principles that are evident in the REF. PRFSs position control before autonomy and, thus, reduce trust in scholarly self-management. They force scholars to work according to institutional practices which much rather resemble those of corporate institutions than those of institutions of autonomous scholarship (even though being only about formal performance and not content). Such corporate structures function on the basis of strict hierarchies, reporting structures, and minutely differentiated responsibilities which allow for the auditing and management of performance. And yet, regarding PRFSs in this way falls short of accounting for the reasons of their implementation. It would mean conflating part of their impact on institutions with the reasons explaining why institutions align with—or strive to set themselves up for—the principles of the REF.

It would be presumptuous to claim government *actually* and *directly effectively* manages the performance of scholars. Instead, the effect of this management is to market the effectiveness of performance and, by so doing, compel scholars to perform in marketable terms, mediated by universities and departments. Arnold et al. assert that ‘while all those that have adopted PRFS have improved their performance, those that have not adopted such systems have also improved, so there is not [sic] clear relationship between PRFS use and performance’ (2018: 47).

The claimed effective management is, furthermore, refuted by looking at the distribution of reward that gives the REF its aura of elite reproduction. Rather than distribution in effective terms—or even *redistribution* allowing for future potential—the REF seems to be just big enough to claim a position as a major funding mechanism and, all the while, small enough to concentrate on the historical elite and quickly fade out on a short tail of excellent, but not quite world-leading units. This concentration as reproduction of hierarchy, potentially termed the support of elites, is built into the exercise where ‘government policy concentrates research funding in institutions where there is evidence of research of high quality’ in its own terms (Moore et al., 2014: ix). Much more than the management of performance, then, this is the management of Weberian social closure.

The way audit culture is embodied in contemporary academic practices, particularly those pertaining to accounting for authorship and publishing, raises awareness about the issue of reproducing hierarchy much more than it does to issues around the redistribution of funds or the enforcement of authentic competition. In the German context, Münch relentlessly argues that performance assessment practices need to result in a competition in which the best ideas are rewarded without social or structural hindrances (2007). In the same way, Arnold et al. claim that the quality relation in the REF should lead to more competition. The REF is, however, most of all a funnelling mechanism as most mercantilist

approaches are: 'over 50% of total QR funding [is] going to the top 10 universities' (Arnold et al., 2018: ii; see also: Dean, 2018).

Pinar discusses the distribution of funding and the weighting used during the 2014 REF. He points to the 'decision to give four times more weight to 4-star research activity relative to 3-star research' (Pinar, 2020: 105). This pertains to the humanities in particular where the qualitative judgement through peer review is already reduced to a contingent, one-dimensional ranking without context. This is, then, further estranged by somewhat arbitrary number gaming for the distribution of funding. The impact of this assumption primarily serves the larger beneficiaries who are strong in terms of world-leading performance (Pinar, 2020: see table 6). In these respects, the REF tends to promote a Mertonian Matthew principle in that the core beneficiaries of funding and symbolic value tend to be those, nevertheless, best positioned to obtain external funding, as Arnold et al. also confirm (2018: 48).

Moreover, there is a conceptual difference between performance-based funding (ex post) and performance agreements (funding, either ex ante or ex post, to realise a target performance) as Boer et al. discuss (2015). But if ex post performance-based funding (the REF) is considered a management and not simply a measurement exercise, how distinct can the line between the two modes of performance-related funding be drawn? I argue the line is blurred, especially where it is implicitly clear that funding strongly correlates with existing hierarchy, i.e. where institutions involved assume—and manage so as to achieve—an outcome and only raise the appearance of unhindered competition. This implicit notion is explored every REF cycle and, thus, the hierarchy reproduced with the official governmental label of world leading excellence. This institutionally outward-bound mechanism of reputation—as opposed to the inward bound mechanism of funding—shifts the discussion towards institutional motivation and marketability. Therefore, even though

there are some dynamic shifts among the recipients of funding, the REF, overall, seems to be a reproduction of power.

This indicates that it is reputation rather than direct funding that is the target of the REF. Indirect coercion through the allure of exoteric rhetoric is the means of manageability much more than it is direct management of esoteric discourse. This shift from material gains to symbolic reward needs to be put in perspective as it fundamentally alters the way the REF is to be seen: it stands in line with the ideas of consumerist education and excellence by ranking and rhetoric. It also moves the appeal to engage in the REF from a substantive to a relative notion: it is not a necessity to gain funding but to gain the label to be better in relative terms.

The aspect of the non-financial value is known in research and praxis. Hicks confirms this as she finds 'prestige rather than financial incentives as the main mechanism through which PRFSs work' (2012: 258). Grant, who in 2007 was Provost at UCL, puts it bleakly in an interview:

I suspect many vice-chancellors would prefer to go for an outcome that allows them to claim highest quality across the board rather than higher immediate financial returns because it is a powerful way of enhancing reputation from which other benefits flow, such as recruitment of students and staff (Lipsett, 2007: n.p.).

In quantitative terms, the added value of the financial means distributed through the REF can hardly account for the widespread and fundamentally systemic impact the exercise has on institutions and practices overall. Put in context, allocation of research funds through the REF accounts only for marginal income overall: '[r]esources distributed according to REF criteria account for only around 4 per cent of the total turnover of the higher education sector' (Shackleton and Booth, 2015: n.p.). This differs depending on type of institution, its research budgets and overall income, so that in a different perspective, the REF can account for 'about 6% of total England HEI income, and 20% of research

income from all sources' (Boer et al., 2015: 112). Since 'over 50% of total QR funding [is received by] the top 10 universities' (Arnold et al., 2018: ii), the overall number concerns mostly these top institutions; but each Pound received matters disproportionately more for the smaller institutions (since their overall income is smaller). The REF is materially small and still most institutions seem to work towards its principles. It can, thus, be deduced that the REF is pervasive enough to matter but not large (or intentional) enough to replace regular Research Council funding.

It is a paradox: the REF does not aspire to significantly substantiate beyond the elite (by means of funding) and yet, rhetorically, it substantiates pervasively (the conversation is everywhere). This is the case because 'as a government-backed kitemark, a REF rating provides signals that are useful to universities from a marketing perspective' (Shackleton and Booth, 2015: 1). This—the marketing perspective in a consumerist appeal to education and scholarship—is the primary motivation as to why universities and their departments or institutes care about the REF. It empowers universities to shift budgets towards becoming *REFable*, to manage recruitment and individual performativity in a way that allow for symbolic gains that can enable potential for future recruitment and performativity. All the while, preparatory management for this exercise and the immense value of rankings and excellence rhetoric prevalent in the academic field are mutually reinforcing each other. They are variations of a culture that is embodied by the institutional practices of evaluating, assessing, or auditing—the touchpoints at which the impact of institutional practice affects the individual.

Paradoxically, however, it needs to be remembered that such alienated practices are enforced by institutions themselves: it is departments and universities who mediate the impact of the REF and turns the audit culture into systemic practice. As my interviews show at several instances, the REF is only the looming structural guidance that shapes

institutional practice in the long run. Pressure on the individual is reportedly connected to notions of the job market, applications, or institutional guidelines and requirements, but not to the REF itself. But as this structural guidance, the REF fares as the meaningful referent for the culture principles it enforces in the long run.

5.3 On Being *REFable*: Individuals and the Pressure to Perform

How do practices of evaluating, assessing, or auditing impact scholars? This can be summarised by saying that scholars are pressurised to conform to institutional guidelines of performance (those that prescribe what is being evaluated, assessed, or audited). For some scholars, especially senior scholars with permanent positions or those who have a strong record of, or current ability to develop, scholarship, this pressure may mean choosing one publisher instead of another for a monograph because of its time to publication. It may also mean, simply, to finally finish an article, a positive encouragement to finish work that is very likely to be of solid quality. Seniors and the high achievers, so to say, are more or less formally pressured rather than fundamentally. This is different for the less secure or less well positioned scholars. My exploratory survey has already shown the high amount of experienced negative pressure in different perspectives.

Other studies confirm a similar impact. A survey (across disciplines) with about 7,000 responses conducted by the University and College Union (UCU) in 2013 substantiated claims about the impact of the REF on working conditions with perceptions from scholarly staff. Among other factors, to the REF is attributed raising stress levels, altering publishing practices, and disturbing early career trajectories. The sense of estrangement is implied as the study concludes that the REF leads to a 'compartmentalising of research into certain kinds of publication outputs, and an unhealthy climate of competition' (UCU, 2013: 45). Evidence is put forward about the practice of pressurising scholars in that institutional

expectations are raised for individual performativity. The impact of this on scholarly activity turns detrimental as maximising performativity means

focusing on more short-term research projects suitable for maximising journal article outputs for peer-reviewed journals, rather than more speculative research, longer term book-length projects or work for edited volumes (UCU, 2013: 27).

In return, punitive sanctions are put in place within institutions for staff not meeting expected output targets which make a basis for raising pressure and hardship on the individual level as well as for separating research and teaching on an institutional level. Particularly striking is, here again, evidence about the impact on early career scholars. For instance, requirement of confirmation for staff in probationary periods—most likely scholars early in their career—is sometimes linked to fulfilment of REF expectations (UCU, 2013: 35).

This is echoed by my interviewees who raise concern that the most devastating impact of the REF is on the job market for early career scholars. As the UCU study further confirms in cursory job description document searches, requirements are often the same for junior or senior staff. Surely, this does not matter in abstract, institutional perspectives. And yet, it is quite clear that the churning out of high-quality articles is much harder if the scholar is less experienced in writing and has fewer years of scholarship to build upon. The study bleakly concludes that the REF not only

distort[s] the nature of academic research, but it also creates unreasonable expectations regarding the research outputs of academic staff and exacerbates already highly demanding workload pressures (UCU, 2013: 44).

These issues are reverberated by other studies such as Arnold et al.'s who write that many scholars 'say that their university threatens them with career-related sanctions if their REF performance is poor', a claim that seems to be more 'prevalent among younger

researchers and women' (2018: iv; see also on early career job targets: Grove, 2018). And, as anecdotal evidence shows, this mechanism hits especially those with materially less secure backgrounds (Hesketh, 2018).

To reiterate, these issues are much less pertinent to high achievers and, correspondingly, scholars employed at elite institutions. Focussing on high-quality scholarship, communicated in a few well-crafted publications, is the chosen strategy here. Institutions trust that these scholars are world-leading and will contribute substance in a somewhat natural scholarly trajectory. These scholars are picked as the cream of the job market. Where there is less trust in quality, there is more pressure to enlarge the pool of potential: thus, increase quantity within an external timeframe. If you as an aspiring scholar feel that you are not part of that cream, you rush to artificially increase your productivity.

The pressure that the REF puts on younger or developing scholars is one about the construction of a somewhat scholarly-artificial value much more than about creating a meaningful scholarly identity. Those two can coincide, of course. But the rhetoric and hiring practices suggest the development of identity increasingly matters less. This is what Collini refers to as he writes that because of mere bibliographic measurement, younger scholars will be discouraged from conducting major projects that do not lead to continuous output but that are, if completed, 'worth far more than a whole CVful of slight articles and premature "syntheses"' (2012: 127). In her study of younger scholars, Archer explores those issues with devastating results for the development and meaning of identity or notions of scholarly meaningfulness. Performativity that is directly valuable to the (potential) department tops all other scholarly activities (which include the development of potential for future performativity—usually the basis for a long-termed scholarly identity). In the end, "'successful", authentic academic identities are rendered insecure, temporary and risky within regimes of performativity' (Archer, 2008: 392).

Scholarly authenticity and identity are only desirable by institutions as long as they do not prohibit constant output as measurable performance. In an ethnographic study, Sikes also writes about the shifting definition of roles because of the RAE and how this impacts intersubjective perception of status and prestige (2006). Henkel further expands on the shifting of identity (1999), where strategic institutional reorganisations led to different intersubjective appreciation of each other's newly-formed roles: the instrumentally research active individual is prioritised through the definition of new institutional roles.

These effects are just that sense of being *REFable* that was put forward in my interviews. *REFable* is, therefore, a euphemism for performance that may or may not be intrinsically scholarly valuable, but one that is formally measurable. And these shifts are expanding to average senior scholars where 'staff perceived as underperforming on research would be moved to teaching contracts' (Baker, 2020a; see also: Baker, 2020b), and also to very senior staff such that universities increased early retirement inducements resulting from a strategy 'seen by some as a sign of institutions shedding academics who they perceived to be underperforming' on the way towards the next REF (Grove, 2020).

Publish to Perform

What those studies as well as my own data confirm is that scholars, especially those who are less secure in terms of scholarly identity and potential output, become means for the institutions in the running up to the next REF. Their performativity in terms of formal authorship is a potential value of marketability for institutions—and it is for this marketability that the individual is granted the right to conduct scholarship at an institution, for a time limited by a fixed contract in which a quota of instrumentalizable publications is

to be produced. The scholar is supposed to benefit from being allowed to author text while being at an institution.

The kernel between supra-institutional elite reproduction and the reason for why many a scholar publishes the way, and especially at the time, they do is that life chances are fractured and somewhat contingent. In other words, opportunities on the academic job market turn into dependencies as a scholar does not get the chance to develop an identity but has the obligation to produce output. It is not the development of discourse that matters at first instance, but the individual, decontextualized contribution. Since discursive context ceases to matter as the primary path of development, strategizing authorship and publishing become the foreground of shaping practices. The scholar's value is the result of measurable authorship that make the scholar *REFable*, not those that define her as a good author, teacher, or thinker. Those aspects can coincide, but they do not have to, especially since performativity is based on very specific terms.

These terms are semantic manifestations of the culture shift: in their dependency on measurable output as well as in their discouraging of qualitative engagement in discourse including unmeasurable teaching. The meaning of *formally published* is, in the submission guidelines of the REF itself, held quite loosely, though. Since the REF is quality-related and quality text is to be put into the symbolically qualitative context of a particular brand, performance for the REF essentially means performance of the formal form. However, it would suffice to account for a preprint. The submission guidelines further state that 'research is defined as a process of investigation leading to *new* insights, *effectively shared*' (REF, 2019: 90; emphases added). This connects to preceding discussions of the meaning of both *new* insights—questions of the meaning and necessity of originality, novelty, or innovativeness of content—and *effective sharing*—where teaching is likely be seen as *no effective sharing*.

This has a strong impact on the meaning of labour in academia—the cultural semantics of how scholarship is perceived across communities. The impact of being *REFable* contributes to the shift from thought to output. This is in the words of Harvie a ‘valorisation of research, which is progressively alienating researchers from the product of their labour’ (2000: 110). The meaning of labour is substantively seen in the light of developing a scholarly identity which may likely be seen as a principal task of an early career *thinker*. Particularly in the humanities, the cogency of one’s contributions depends on the breadth of intellectual grasp, the ability to convince by argument, to put the specific into the frame of more abstract discourse—all such aspects that require time and the freedom to make mistakes, take wrong intellectual turns during the intellectual development. For sure, no pressure may not result in better contributions either—it may result in no intellectual development at all. But requiring to be *REFable* means not being allowed this time for development in the first place.

This pressure, in its impact on authorship and publishing among developing scholars, shows the *relation of relationlessness*: scholars engage in the practice, but its substance is estranged, the resulting artefact is a reified version of the once sought-after intellectual endeavour. Discursive achievement becomes the product of efficient calculation, a success somewhere between *being* a valuable contribution and *irrespective* of being so: the contribution, of course, cannot be without any value (since it would be of no value in the peer review of the REF); but if and how it develops in actual discourse is secondary (since for the REF, it is the instance of submission that matters, not those developing instances of reception).

As articulated in the interviews, the temporal pressure to publish is a dominant issue. The question of strategy, here as before, evokes the image of balancing quantity and quality within a particular timeframe. This pressure seems to counteract publishing for the

numbers only, since the REF does not value high numbers of output. This could, as such, be a favourable mechanism from the perspective of scholarly discourse. As suggested by Münch in the Marxian sense of alienation, the strategic publishing encouraged by such performance assessments result in a form of *assembly line production* (Münch, 2011: 219). Similarly, a notion of alienation is evident implicitly in a critique of the RAE, in which Henkel writes about the concern that the exercise ‘was shifting the balance of publication objectives away from communication of findings to the peer community to an even stronger emphasis on reputation enhancement’ (1999: 109). A rigid timeframe, inevitably, favours those who can produce within a timeframe. Thus, even though putting a cap on quantity seems positive, the underlying temporality is an alienating, production-focussing mechanism nonetheless, especially for early-career scholars.

Scientism: Enforcement of Conformist Discourse as Normal Science?

The argument about the (ir)relevance of the development of contributions within discourse can be differentiated in closer inspection. It can be argued that a focus on how contributions are set up to develop within discourse in respect to high-quality text has become part of strategizing publishing practice; an argument closely related to that made by Moed (2008). Anything that may improve quality will help an individual’s performance—paired with the fact that the REF is, in the end, a peer review exercise.

As I discuss in chapter 10, the consecratory impact of a brand does improve the performance of text by allowing for it to be distributed widely to specific scholarly communities, by making it materially visible and symbolically valuable. From this results that, even though only the submitted text and not its development in discourse matters,

and irrespective of the REF's guideline that claims publishers or citation impact do not matter for performativity in the REF, those instances do matter indirectly.

Imagine a scholar who publishes with a highly esteemed press early on in a REF-cycle. Her argument might get picked up, discussed, and reused, enforced by the fact that she is widely available in discourses (through the esteemed publishing brand). If she had only published with a smaller local publisher or even only released an informal preprint, the very same argument will not have fared as well. It is unlikely that this will not be considered during a review exercise in the REF. The niche discourses that the humanities are based on almost inevitably prohibit a blind review—those who are able to judge niche content know about what is contributed to niche discourse, especially if this takes place within highly visible brands. Further in this respect are publication choices to be seen that dismiss the high impact brands in favour of smaller brands that are quicker in their publication output, as some of my interviewees explained. This is also due to the REF which may require timely publication at the end of a cycle. Pettigrew claims that because of the RAE, scholars

are now much more attuned to where, when and how they publish. The spotlight of performance has become an uncomfortable zone for individual, group and institutional behaviour (2011: 348).

Bence and Oppenheim say in 'blunt terms, [that] the extent to which research is published and further cited in recognised journals has been accepted a major basis' for the RAE (2005: 138). And yet, it is unlikely that citation analyses of the kind of Moed's (2008) will provide deeper empirical insights about this argument in the humanities. On the one hand, the many mock exercises and strategizing already in place question the authenticity such an analysis may be able to justify. On the other hand, output in panel D remains ill-suited for reliable bibliometric analyses, as the *Metric Tide* report confirmed (HEFCE, 2015: 31), and my exploratory survey also indicated (figures 20-22).

Relevant in this respect is another aspect pertaining to the specifics of scholarship in the humanities: formalism enforced by principles of scientism. Since the assessment exercise is fundamentally driven and influenced by principles of scientific culture—just as, more abstractly, neoliberal auditing builds on the *scientific* management of human *resources*—the drive towards publishing journal articles instead of writing monographs, indeed the working on niche aspects instead of the larger ideas that the humanities are experiencing can be seen as an impact of the REF as well. Baggeley argues this is the case as he sees a ‘trend towards short-termism and narrowness of focus in British academe’ that negatively affects publishing practices in the humanities (2007: n.p.).

It does not affect just publishing, therefore; publishing practices affect scholarship in a circular way. But this circularity is hidden behind the justification of the system, the personal adjustment that is referred to by Pettigrew (2011: 348). And this can also be seen during my interviews when Nora talks about the ‘superficiality in publishing’ that was promoted through the REF. Expressed in this statement is the prevalent belief in brands and publishers, in their systemic complexity, and the justification of their existence: the belief that justifies through actualisations of practices that the way authorship is constituted is the best option available. In other words, and reiterating Bourdieu, this superficiality expresses the *illusio* of the academic field such that scholar subjects believe—or consent by means of their practices—that balance-sheet accounting of publications is a decisive criterion which can be prioritised above intellectual development.

Peer Review and the Scholarlyness of the REF

Moreover, since the assessment element of the REF is a peer review exercise, most of what is to be said, or criticised, about peer review practices (I do so in chapter 10) can be

transferred to criticise the REF as well. And similar to the fact that there is only little empirical evidence about the systemic impact of peer review, particularly in relation to enforced conformism or disturbed discourse, knowledge about this in relation to the REF is scarce as well. Existing literature tends to see the issue of performativity as a primary reward to be an issue of developing content that has the *feel* of performing successfully: the 'working out [of] big or controversial ideas' as Melissa says during the interview. It is an issue of prioritising some forms of content and focussing scholarly endeavours on the highest performative reward.

This may not be a mode of conformism. Quite the contrary, a focus on controversial ideas that evoke the sense of high performativity may run contra the establishment. And yet, it remains an issue where structural mechanisms impact content in a way that is disturbing to its development from within. This sense of focus and prioritising as formal concentration can be read, for instance, where Hicks suggests that the diffuse rationale of PRFSs often is 'resource concentration, encouraging international (i.e. English-language) publication and the general pursuit of excellence' (Hicks, 2012: 254). In such ambiguity of conformism inheres a notion of Kuhnian normal science which, as discussed in chapter 7, is unlike discourse development in the humanities. This ambiguity remains and can neither be refuted nor confirmed however, since I do not investigate discourse development itself.

In turn, one could argue in respect of the peer review that this is actually a *very scholarly* exercise, it is reading and judging the quality of content—is this not what discourse is about? I suggest this is a wrong intuition. On the one hand, the practice of judgement is not conducted as part of discourse and it does not feed into discourse in scholarly means. It is conducted so that a funding body can award money and reputation based on a rather arbitrary hierarchy of stars that reduces all qualitative specificity that a publication may concretely develop in discourse. On the other hand, it is the reflexive shaping of practices

through institutional pressure that affects scholars. They have to work towards arbitrary guidelines not because they are judged, but because institutions develop responses to a single regime as they see fit. The reflexive effect this regime creates, as is visible in the interviews, is to enforce an alienation of authorship which is not a *very scholarly* exercise.

Impact and the Single Market

A challenging aspect within the conception of the REF concerns the idea of a single market, which is already visible in the Browne report. Collini offers a critical reading of Browne's ideas which seem to have aimed to convert UK's higher education into a 'lightly regulated market in which consumer demand [...] is sovereign in determining what is offered' (Collini, 2012: 178–179). The idea of such all-encompassing submission to a single market is what I discussed above where market opportunity turns into market dependency as all instances are subsumed under the imperative of a single regime. This single regime is, here again, the performativity of formal authorship per individual scholar. This is, firstly, not a focus on scholarship but on a suggested number of formal representations of scholarship; secondly, a focus on representative scholarship as opposed to teaching; and thirdly, a rigid stratification of representative scholarship as the outcome of the exercise.

Bulaitis furthers such critique by drawing on the history of output-led assessment practices in UK policies (2017). She points to the development of accountability rhetoric and direct impact assessment since policy making under Thatcher, paired with cuts in funding. It resulted a paradigmatic shift towards favouring instrumentalizable research and quasi rationalistic distribution of funds: competition and best seller impact decide what is being prioritised. In effect, 'knowledge is made useful in its transfer into the marketplace and immeasurable values are disregarded if they cannot be reified' (Bulaitis, 2017: 4).

A similar sense of the way the REF impacts on the development of scholarship can be read in critiques of impact case studies. These can have a 'potentially disastrous impact' in the humanities (Collini, 2012: 171) since there is a systemic neglect of the consideration and judgement of whether and how impact is desirable. Impact is defined 'as an effect on, change or benefit to the economy, society, culture, public policy or services, health, the environment or quality of life, beyond academia' (REF, 2019: 90). Though this can be specified in more detail for relevant areas, the fact remains that there is an underlying assumption of instrumentalism.

The idea is that scholarship that can be and, indeed, is externalised and distributed outside of its esoteric discourse is somewhat more beneficial. It is a neglect of the way scholarship impacts on people—students, avid readers, scholars—which partake in practices of society and, thus, turn esoteric discourse into desirable, principled human conduct without manipulative detour. It is an expression of the principle that teaching is inferior to narrated cases of external impact. For sure, teaching *can* be submitted during the REF. But it requires 'corroborating evidence for impact case studies' (REF, 2019: 13), such that teaching needs to be put in an eventful, narrative context where mere notions of learning or debating do not suffice. This is the nature of performativity and expressed by Hazelkorn in regard to audit culture in academia:

[o]nce policy seeks to promote knowledge with value and impact beyond the academy, there are implications for disciplines and fields of study, scholarly practice, and the choice of research projects: what research is being undertaken, what should be funded, how it is managed, how it is evaluated and measured, and by whom (Hazelkorn, 2015: 28).

In addition to the impact of focussed impact cases on the development of scholarship, it is a focus on authorial narrators who are able to frame scholarship such that it optimally performs. In a persuasive ethnographic study of a mock REF review of impact case studies, Watermeyer and Hedgecoe articulate substantial claims often implied in

criticism of the REF. They note that reviewers struggled with differentiating between research and impact. The latter is always framed by the former—notwithstanding, causality between the two is hard to qualify. Reviewers, in the end,

furnished only with bibliographic information related to outputs, resorted to a conventional academic criterion for quality: whether or not the article had been published in a peer-reviewed journal and the relative standing of that journal (Watermeyer and Hedgecoe, 2016: 658).

Their conclusion provides an account of impact as a mercantilist sense of narrated singularity where the most performativity is reached by the best seller. Impact case studies ‘signal the continuing neoliberalisation of higher education’ as well as the ‘commodification of seemingly every facet of academic life’ (Watermeyer and Hedgecoe, 2016: 663). The authors, thus, poignantly summarise how efforts of formal rationalistic policy alienate scholarship.

5.4 Conclusion

In this chapter, I elaborated on the argument that the REF shapes authorship and publishing practices by enforcing formal rationalistic culture principles. This results in an alienation of authorship in that it rationalises the production and communication of knowledge as a management tool for the gains of funding and reputation. There is profound evidence for some aspects of this in existing literature and I connected this to the specifics of publishing practices in relation to my own empirical findings. It remains a peculiarity that the rhetoric of the REF focusses so narrowly on it as a distributive exercise—a peculiarity, however, that stands in line with NPM.

The result of this exercise is similar to making sure only the cream of a cake is visible, while the rest of the cake, indeed whole other cakes, stay invisible. This cream, termed excellence, comes about in labelled statements, representing scholarship that is of ‘[q]uality

that is world-leading in terms of originality, significance and rigour' (REF, 2014: 60). While this may sound like an exercise that honours merit above all else, this applied insinuation of meritocracy is an illusion, just as the idea of meritocracy itself is as well. This illusion suggests that the REF is an exercise about the effective (re)distribution of funds based on historical performance. Primarily, however, the REF is about reputation. Performance, here, is constructed to be most representative in terms of reputation within the existing stratification of universities. In social theoretical terms and picking up Fraser and Honneth (2003), the REF is neither about redistribution nor about equal recognition.

For all those aspects considered in this chapter, the REF is exemplary for a culture shift from scholarly values to formal rationality, a shift of how the meaning of scholarly labour is perceived not in intrinsic substance but as instrumental means. In this respect, PRFSs and the REF in particular are *good* ways to increase a nation's visibility and reputation in terms of excellence and contributing to supranational elites—*good* as in instrumentally effective. This, however, is not necessarily based on, and certainly does not improve, scholarly merit in its intrinsic sense. It is essential to point out explicitly that this rhetoric and the distribution of labels of excellence do not pertain to scholarly communities. It is hard to imagine scholars interested in other scholars' thought who first evaluate their institutions' star-rating.

Concluding the discussion of the ways the REF impacts authorship and publishing—and, by means of these, scholarship—the question remains why the application of a PRFS in the form of the REF needs to have such a widespread and fundamental impact. As Hicks puts it, 'PRFSs encourage excellence at the expense of equity' (2012: 259). In other words, a mechanism that is supposed to feature a few as a form of fostering and promoting an elite affects the whole system negatively, at least in the humanities. This disciplinary differentiation is crucial here: where there are large laboratories, clinical trials, or research

centres to fund, concentration of such funds can make sense. However, the humanities have none of these. Except for selected research projects, most endeavours in the humanities do not rely on substantial financial funds but on the freedom to develop arguments without any restraint. And for that reason, it is also that diversity which may thrive inversely proportional to mechanisms of concentration: many smaller places with niche perspectives seem to allow for the necessary freedom to develop.

Concentration may foster the notion of excellence since many good minds are clustered; but this excellence may as well exist without artificial clustering, scattered across many different places. It is, in the end, a particular culture that is at the root of what is termed academic freedom or autonomy. The REF, suggesting certain criteria are more desirable than others, necessarily disturbs this autonomy as it proposes a prioritisation. Peer review may be able to deal with this the best of any such evaluative exercise since authority to apply criteria during the exercise is also distributed.

Acknowledging all the negative effects discussed here, a study of attitudes finds that researchers across disciplines generally perceive *changes* made to REF rules to be positive (Weinstein et al., 2019: 40). The authority over the distribution of authorities, therefore, is perceived to improve its principles. And yet, the very same study of attitudes indicates that scholars in panel D have the most negative impressions, relative to scholars in other panels, about the way the REF fulfils its purpose and how it influences scholarly culture (Weinstein et al., 2019: 96). Thus, there *are* some positive remarks about the REF. But overall, as both my interviews and the vast literature on the topic indicate, the REF is a quite negative institution.

And in the end, the peculiarity of widespread evaluation exercises is that they are 'perverting a procedure [...] which is inherent in modern science and scholarship anyway' (Liessmann, 2006: 100; translation by the author). Next to oral discourse or teaching, most

of what the humanities have is discourse in the form of published artefacts in which scholars take on each other's work recursively. Evaluation *constantly* takes place here. The very basis for the symbolic value of contributions and brands comes about via the discursive approval and appreciation *within* discourse. This could be termed a scholarly-immanent culture principle of intellectual auditing. Looking at contributions, at the mechanisms of review and rejection in esoteric scholarly communication, would suffice for assessing and determining the quality of discourse. Instead, the REF and its underlying principles—exemplified as auditing and accounting of formal terms—needs to be seen as the embodiment of consumerism and *Thatcherite nationalism* that Readings laments (1999: chapter 3; see also: Harvie, 2000: 109). It is a raising of awareness for a nationalist sense of performance irrespective of the cultural specifics of a nation-state, its society and sub-cultures, so that consumers of *world-leading* education and scholarship find their ways to the excellence of UK's institutions. In the following chapter, I refer to how this turns out in Germany.

6 Cultural Hybridity in Germany: Traditionalism and Excellence

There are subtler, semantically contradictory, but nonetheless systemic forms of an instrumentalization of publishing in Germany, where academia is caught in a cultural hybridity. Firstly, there is a race to catch up in Germany, a desire to be legitimated in an international *market of scholarly ideas*. To fit this market's terms, publishing practices of German scholars have to adapt to an international context. And while the German *Exzellenzinitiative* has almost no direct impact on authorship and publishing, it exemplifies rationalistic culture principles where excellence and efficiency are guiding terms without consideration of a local specificity of humanities scholarship. Secondly, formal authorship is a tool of bureaucratic accountability. To publish, in this sense, means to legitimate and fulfil requirements of funding—without the publication of proceedings, it seems, a conference is not finished. In turn, discourses become crowded by unread accounts of conferences or, on terms of similar traditionalist logics, with qualifying scriptures. This is a particularly German issue.

The imperatives of excellence and internationalisation in Germany resonate a culture shift towards formal rationality. And yet, formal proceduralism persists also from earlier modern times of a traditionalist humanities. Scholarly subjects, thus, bear the mark of a cultural hybridity so that publishing practices are bound to both historically grown principles and new progressive terms in varying contexts.

This hybridity is a matter of subjectification, a conception exemplified by Reckwitz: cultures of different times and their semantics are superimposed on each other, making for an imbrication of potentially contradictory principles; they construct a canvas of instability and frictions which shape the developments of subjects (2020a: 97). The new subject is reproduced by means of practices geared towards shifted culture principles which ascribe differing meanings to artefacts, practices, or the self. The earlier

subject formation marks a cultural difference, a *constitutive outside* (Reckwitz, 2020a: 98), which represents dismissed or foregone principles within itself from which, all the while, the subject still draws positively. This creates a dynamic of meaning that becomes expressed in potentially contradictory practices. Reckwitz theorises this hybridity comprehensively as he executes a study of hybrid subject formations during the aesthetic development of modernity. With pertinence to my argument, he concludes by pointing out the *unsurprisingness* of modern culture which, despite all efforts of progress through hyperinnovation and claims of renewing everything, it time and time again reaches back to dismissed culture principles of former periods of modernity (Reckwitz, 2020a: 626).

This mechanism can be witnessed in German humanities today where the subject of the *Gelehrter*, the eminent scholar, ceases to dominate as principles of excellence and internationality impact the practices of scholarship. Without having arrived at a stable new subject form, scholars struggle between traditionalism and excellence—*both* exert logics of formal rationality that become articulated by the constitution of authorship and publishing, as I explain in this chapter.

6.1 ‘Every Conference Needs to Be Buried in a *Sammelband*’

Exzellenzinitiative

In direct relation to the REF, the German *Exzellenzinitiative* was a matter of enquiry during my interviews. In summary, it has almost no direct impact on publishing practices. It is criticised for its bureaucratic and biased approach or blamed for its tendentious labelling. Most of all, as explained by Gerald, it is a huge machinery about reputation and money where it is ‘silly that the excellent university will be given extra funding’. So far, therefore, this seems to be a miniature critique of similar terms as those of the REF.

Despite the little direct influence it has, the meaning given to the *Exzellenzinitiative* is much more a highlighting of a general culture trend. Where the REF has a direct influence on the job market and publishing practices as well as being the manifestation of a certain culture, the *Exzellenzinitiative* is much more of the latter and much less the former. It exemplifies Germany's movement to enforce internationalisation and instrumentalization for the sake of symbolic value. This was repeatedly stressed during the interviews. However, overall, the *Exzellenzinitiative* is by far not as important for publishing practices as the REF is, even indirectly.

Internationalisation and Accountability

To enquire the meaning of internationalisation and language seems essential in a German context, especially in order to contextualise the (Lingua Franca-)English language publishing practices witnessed in the exploratory survey (figures 13/14). Since the humanities are strongly connected to regional cultures and languages in which they are rooted, German humanities scholarship addresses an audience primarily fluent in the German language (unless it is translated because of extraordinary circumstances).

Some interviewees directly mention a trend such that German humanities discourses are moving towards English. Albert articulates that 'there are ever more people writing-publishing in English'. The reason for this is increased visibility and impact in an international context, as some interviewees such as Albert or Don claim. And yet, Don concedes that 'well as a basic principle, I always write in German because I think in German'. To publish in English means for him to have his work translated professionally.

Herb expresses gratifyingly the humanistic ideal of diverse languages where contributions to discourse can be made in either German, English, or French—and though

not all can write in all these languages, at least all participants can read them. This is changing. He declares a 'dereliction of language-specific discourse' and a decrease of the quality of discourse as it moves towards English as the primary means of exchange. Publishing decisions are increasingly based on language such that high impact journals, even those with a long history of German scholarship, prefer English to German submissions today. In a similar vein, Clara mentions that the internationality of publications is looked at in career and promotion committees. There seems to be the assumption that to be more international means to be more valuable for the institution.

Accountability through Publication Artefacts

Another theme specific in this context is that of accountability and legitimation. I did not consider this theme before the interviews; my interviewees raised the issue mostly independently, making accountability a grounded topic. This theme articulates a practice that can be found in the literature in a few instances only. It puts publishing in the light of a crude proceduralism, a ticking-a-box-nature in relation to rule-based bureaucracy that stand in an odd culture relation to Humboldtian principles. Publishing artefacts are perceived as criteria for the fulfilment of formal requirements, and they seem to have only little value for readership. As comes through in the interviews, these publications are referents of legitimation for a conference, a project, grant funding, or even the field at large. This, literally, is the scholarly analogy to the internet meme of *pictures or it didn't happen*.

This is particularly the case in regard to funders in Germany as several interviewees mention. Elizabeth talks of the inevitable expectation of research funders 'that conferences which they funded will result in a *Sammelband*' (German for anthology or conference proceeding). She dismisses this practice in harsh terms, declaring that 'every conference

needs to be buried in a *Sammelband* [which] further drives the masses of publications towards ad absurdum’.

Ian relates this to a positivistic feedback loop which evokes the impression of a Matthew effect in circular, technocratic reasoning: ‘so third-party research funds are attracted with a mass of publications- which then result in a mass of publications- only to attract research funds’. He concludes that the allocation of third-party funding is entrenched with formal publications since such artefacts, especially its quantity, are a core ‘measurement of success’ for funding. Don similarly remarks on the circularity of having to publish to get funding only to have the requirement to publish more. He is among the most hostile towards this practice and his account gives a comprehensive representation of it which is worth quoting at length:

the *Sammelbände* get out of hand. I think everybody recognises this. And this need to make a conference for every project or specialised research- to publish everything is really terribly much. And then if such *Sammelbände* really quickly- chop- chop- without selection- well you do a conference- 15 papers- all are being printed without anybody looking- could you potentially leave a few bad ones out. There is really little effort to make those things in any way coherent.

This requirement of formal legitimation and accountability is driven not by discourse but by a sort of traditionalist reproduction. Authorship literally seems to be an instrument of formal duty here so that publishing practices are geared towards this end without much consideration of discourse. The interviewees have nothing positive to say about this except for Albert. He sees some positive aspects in the masses of conference proceedings, besides the pressure to publish that they create for the individual author. Albert claims that such *Sammelbände* may eventually become ‘important reference points in a discussion- even though one wouldn’t have thought so prior to publication’. In this sense, the publication is mostly an archive of the conference or the project. I discuss this matter in more detail below.

Individual Excellence and Notions of Productivity

Before discussing this, I turn to the theme of articulations of culture through the meaning of excellence or productivity that already started in the preceding chapter with UK scholars. Asked what my interviewees perceive as characteristics of a productive and excellent scholar, the interviewees referred to a variety of issues which are predominantly bound together by a critique of the meaning of these concepts.

In terms of productivity, Berta criticises that it is common to be ‘at least 80% measured with publications’; likewise, Albert argues that productivity is a matter of interplay between amount of funding awarded and material published. Excellence is criticised in the German context, less fiercely than in the UK, primarily for its meaninglessness for scholarship and indifference for the content it rhetorically claims to relate to. Herb explicitly struggles to use excellence in any way non-ironically—evoking the meaning of performativity. In his terms, excellence ‘means just the opposite of excellence-means to just somehow please’. Ian claims that ‘excellence is a defective term- since it really doesn’t refer to anything but processes of competition’. Clara and Don both echo such meaning in that they connect excellence with competition.

Also highlighting culture, the interviewees (indeed, both from Germany and the UK) implicitly share a rejection of comprehending productivity in simplistic terms. In referring to what a *productive scholar* may be or what excellence may be able to denote positively, the interviewees move away from reductionist statements about output and open a dichotomy of combining multiple skills and focus on qualitative contributions. The answers here often converge, transcending what the interviewees felt to be wrong understandings and aligning in a normative basis for values that could well also stand for a different idea of publishing practices. Productivity and excellence become mere referents where some use either of the terms, interchangeably, to denote skills or content. Others see in productivity and

excellence a sort of hierarchy where productivity is the combination of a variety of skills necessary to be a good scholar, while excellence is an advanced effort that is evident only in content (but that usually slips through the statements of excellence in use rhetorically). To be sure, neither my questions nor the answers implied that productivity and excellence necessarily refer to each other or that these terms are in some way connected. But by asking successively, the interviewees did connect the two and created more of a new normative background.

Witness how some interviewees refer to the combination of different skills: Berta says about an excellent scholar to be one who ‘combines several skills within herself- namely all those that a scholar is regularly confronted with’. Ian disentangles this and claims that productive scholars can appear in different guises, each being productive in her own way such as through teaching, research, administration, or public communication. Ian, therefore, refers to a plurality of capabilities and talents. Herb, again in reply about excellence, articulates such plurality by saying that an excellent scholar is one who ‘takes the time for good research- who takes the time for teaching that is really connected to this research’. For Herb, an excellent scholar is primarily interested in passing on thought without paying attention whether this practice rewards reputation or recognition, for recognition simply may not be the first objective. This already refers to the other element of the dichotomy, the extra qualitative effort, that added focus on the content of scholarship which most interviewees wish to connect to excellence (and partly also productivity). It is a focus that very much alludes to the sense of ultimate ends in discourse.

Herb talks of a ‘productive dispute’ where productive refers to a scholar who engages with her work and the people in her field in such a way that others feel encouraged by or attracted to. Similarly, Elizabeth articulates her idea of productivity as discursive

‘participation’ instead of mere output: the premise here clearly is engagement in a complex of back and forth instead of one-way output.

In these respects, the normative ideas the interviewees provide go beyond the systemic premises of competitive output measurement. They transcend struggles of rhetoric and align, in a sense, in thinking in terms of values rooted in scholarship living through diverse discourse. Also manifest in these answers is a plurality, a matter that articulates partially the hybridity of culture principles that I explain in more detail below.

Most of the criticism appearing in the disambiguation of productivity and excellence, however, raises questions of whether the performativity of formal publishing is supportive of discourse community engagement. The discomfort the interviewees articulate suggests this may not be the case. I look at those issues in more detail in the following sub-chapter, beginning with a focus on the *Exzellenzinitiative*.

6.2 The *Gelehrter* and New Managerial Culture

To understand the development of cultures in academia and publishing—the differences and similarities between Germany and the UK—it seems crucial to provide some background on the *Exzellenzinitiative*. It is often discursively related to the REF which seems to be justified in regard to its cultural impact. However, the material base of this funding and award exercise is quite different, which explains its more subtle, less direct effect on publishing practices. I start by explaining the *Exzellenzinitiative* before abstracting and providing an understanding of connected culture principles in Germany more generally in the subsequent sections of this sub-chapter.

A Short Political History of the Exzellenzinitiative

The German *Exzellenzinitiative* is less straightforward than UK's REF, both historically and mechanistically. Its inauguration is not based on funding cuts but related to Germany's goal to become an internationally more visible and esteemed base for research and scholarship. It is much less connected materially to publications or bibliometric performance. This becomes visible in my survey data as pressures to publish are much less connected to the *Exzellenzinitiative*, even though its cultural impact of excellence is visible in the qualitative interviews.

The idea of the *Exzellenzinitiative* began with a paper on innovation strategy published by the German Social Democrats in 2004, which claimed Germany needed elite universities of international standing (Sondermann et al., 2008: 10). Criticism focussed on the obvious elite-nature of this claim, which opposed the traditional German institutional configuration. Even though critics gave proponents of this idea the attribute of a *Harvard complex*, it was rapidly taken up in political and public discourse and the idea developed into a full-fledged strategy to incentivise new monopoly structures within the traditional homogeneity of German universities (Münch, 2007: 47). After considerable political back and forth, the official strategy was released in 2005 and aimed for the establishment of five elite universities.

The methodology of the awarding exercise was, first and foremost, connected to project funding. The institution-wide award was subsequently geared to the establishment of such funded projects. In detail, this award was based on successive rounds of evaluation in three areas: graduate studies of Anglo-American example, excellence research clusters (basically newly-formed temporary and collaborative research centres), and concepts for the institution-wide enforcement of future research excellence. Universities applied in two competitive rounds for funding of these three areas (580 applications in total with 85

funded projects/centres; Sondermann et al., 2008: 11). The funding was awarded on the basis of peer review with internationally renowned scholars.

Subsequently, based on the interplay of these three areas and especially the future concepts, nine universities were awarded the status of *Exzellenzuniversitäten*. Political instruments of this kind are always double-edged swords, however. Consider a tax: it often leads to increased income for the state alongside a reduction or diminishing of that which is to be taxed. This can have exceptions, of course. But most newly-introduced (or radically-raised) taxes follows such a pattern (exemplary in this respect is the tax on tobacco products). The impact of the *Exzellenzinitiative* is quite similar in this respect: 37 universities were funded in the first round but only nine universities got the official award of the *Exzellenzuniversität* (and only 2.3% of the overall funding was awarded to universities in the former East Germany, meaning that concerns of biased approaches were raised in Germany, too; Pasternack, 2008: 20).

In other words, the core aim of the establishment of *Exzellenzuniversitäten* was the apparent political target. The underlying strategy, however, seemed to be the enforcement of new structures and the break-up of the homogeneous university structure more broadly. This already shows that the *Exzellenzinitiative* was never conceived of being a key funding (or funding reductions) mechanism as the earlier modes of the REF were, even though the rhetoric it triggered results in a contrary appearance. The establishment of graduate schools—a highly-criticised institutional type very unlike the traditional German university—got broad uptake and universities prepared for future funding rounds of this kind beyond the target of the *Exzellenz*-status. Similarly to the REF, though, the strong focus on research and excellence led to a decreased focus on teaching as well as to processes of monopolisation of higher education resources by fewer institutions (Pasternack, 2008: 23).

This development is still visible today. Between 2010 and 2012, a second round of the *Exzellenzinitiative* took place, with a third round that began in 2016 (then renamed *Exzellenzstrategie*, though this renaming did not gain much ground in discourse). Based on this, there have been ten official *Exzellenzuniversitäten* in Germany, which will receive added funding of about €13 million each per year for seven years (Warnecke, 2019).

As suggested already by early critics, until today, there is the rhetoric of elite monopolisation and excellence on the one hand, and the material reorientation of institutions towards targets that arise from without the practices of research and teaching (Münch, 2011: 18). In this respect, the institutional pursuit of forming excellence in Germany is quite similar to the REF. However, the initiative is much less focussed on publication histories and instead rewards future concepts. It also does not approach all institutions generally but invites applications on their own terms.

The *Exzellenzinitiative* is primarily project funding that is transformed into a longer-term institutional funding. So far, this seems to align with Germany's transition to an increased share of individual project funding as opposed to organisational core funding. However, alongside this funding reorientation comes the symbolic referencing of elitism in the guise of the awarding of *Exzellenzuniversitäten*. This is supposed to entice a marketing effect beyond the German borders and to monopolise attention on a handful of institutions with added esteem. This political strategy breaks with the Humboldtian idea of the German university and especially with the cultural notion of the German *Gelehrter*, to which I turn in the following section.

From Humboldt to the Exzellenzinitiative

Since the Humboldtian university reform and the dominance of bourgeois educational elites, the German *Geisteswissenschaften* have been a resort of the *Gelehrter*, the learned burgher, the erudite, even highbrow, socially-sophisticated academic (Hamann, 2009: 42; Münch, 2007: 170, 2008: 166). Weber explicates this culture as he renounces democratic access and claims the *aristocracy of the mind* in German academia ([1917] 2015: 10). Humboldt himself claimed real scholarship is character-forming in respect to national (Prussian) interests ([1810] 2017: 155).

This culture ideal centred on individuals and constructed a scholarly subject that dominated the intellectual elite in Germany as well as in Europe more generally. This resembles the authorial professor, equipped with institutional cultural capital, as explained by Bourdieu for the academic field in France (1988: 36). Practices of communication embodied this culture but, all along, this was much more persistent in Germany than in other European countries such as France or, formerly European, the UK. The impinging impact of NPM arrived only recently in Germany. Its bearing is felt only more harshly where its formal rationalistic terms increasingly tie practices to a formal rule-following. In response, the ideal-type values of humanistic scholarship abate to exert power. Just as a hybridity of culture development suggests, culture logics of traditionalism persist for some practices, while new governance changes the instances only partially in which the logics come to bear.

There are arguments in the literature of how NPM enforces a thinning of ends as it replaces the Bourdieuan *Homo Academicus* with an *Homo Oeconomicus*—a subject that has to justify its quality managerially instead of academically (Münch, 2011: 94). Quality has to be *managed* today where contribution has been *judged* in previous times. In the language of

Liessmann, this is the turning from knowing in terms of a humanistic ideal to the industrialisation of knowledge (2006: 39).

As the semantics of managerialism suggest, rules have to be set which aim at formal objectives so as to enable manageability. The *Exzellenzinitiative* is emblematic for this. It resembles the REF as it articulates new managerial culture principles and, yet, it is far less dominant than the REF. Instead of an assessment of all institutions (in connection with a rating of all), German institutions are free to engage with a limited contingent of rated results. There are now eleven universities (or clusters) awarded who refer to themselves as universities of excellence (BMBF, 2019).

This less dominant position is visible in my data as well as in the literature. Both my exploratory survey and my interviews affirm that the *Exzellenzinitiative* exerts much less systemic power than the REF, especially in regard to publishing practices. Rather, it embodies new governance principles of the German academic sector, thus, articulating a culture shift. These principles aim to break with the homogeneity of German universities by encouraging the construction of excellence. German universities are to be stratified into elite and non-elite, as is the case in anglophone countries (Wohlrabe et al., 2019: 20). This outsources the notion of quality, taking it from the realms of discourse to those of governance which ushers in a logic of mistrust and where the means to determine productivity become an end on their own (Münch, 2011: 113), one that is irrespective of the actual development of scholarly discourse.

Connecting this literature with my interviews, the culture shift becomes evident, especially in the many claims of a need to internationalise articulated by most interviewees. In terms of publishing, the formal rationality of new governance is particularly embodied in the context of internationalisation. National funding approaches among which the *Exzellenzinitiative* is to be counted have the clear objective of ‘improving the international

competitiveness' (Schroder et al., 2014: 224). Where the grounds of competition shift towards the construction of reputation external to discourse, the terms on which scholars are to compete likewise shift from scholarly substance to formal rules. The need to internationalise scholarly output to establish worldwide authorship visibility as a matter of excellence is a chimera in regard to scholarly substance; and it resembles principles of NPM *per se*.

There is evidence for the increase of formal rationality in the new governance of Germany's academic sector in that all universities are to be treated as equal, uniform institutions. Most tellingly, this meant that smaller institutions aiming to copy the traditional research university in substance but, due to lack of resources and historical specificity, achieved only *provincialism* (Seyfarth and Spoun, 2010: 84). This exemplifies the nonsensicality of an imperative of a single market: an imperative of internationalisation, here, as it is equally demanded from all institutions.

Excellence and Internationalisation

Internationality is in this respect claimed to be a precondition of success of national academic systems, since they allow for the increase of citation rates, collaboration, and mobility of scholars (Aufderheide and Neizert, 2016). Mechanistically, this seems to be correct. But the question to pose here is whether this principled increase impacts the meaning of scholarship in a way that affects culture sustainably. Put differently: should internationality be a principle *eo ipso* with which scholarly labour is to be judged? Looking at both my interviews and the literature discussed here, it seems to be the case that the answer given to this question is positive. Publishing practices become geared towards internationality as the perception of published artefacts is mediated by a culture that

favours international output above regional specificity, irrespective of discourse. This insinuates a formalism where, indeed, internationality gains a symbolic value that can be measured without qualitative judgement of content.

The chimeric character of internationality reveals its—potentially disastrous—formalism where its pragmatics meet the realities of scholarship. Leendertz traces the historical development of fixed-term positions at the Max-Planck-Society, one of the largest networks of research institutions in Germany. She relates the increasing precariat to the desired flexibility of the learned society to be able to compete internationally, especially regarding rankings and quantitative comparisons (Leendertz, 2020: 26). The sense of formal rationalistic managerialism is very legible in this account.

Remember here also the data that I provide in chapter 7.2 about the use of secondary language in German humanities scholarship: English or a mix of English and German dominate. And yet, internationality is not global inclusivity and neither specific to discourse. The drive towards English cannot be justified by means of reception or content. As Stekeler-Weithofer states, actual internationalisation becomes manifest through the *reception* of a *diversity* of languages, instead of through mere *publishing* of *English* as is currently evident (2009: 36), a statement that reiterates the humanistic ideal from my interviews. Boehm repeats this very ideal for art history and bemoans the discipline's canonisation as anglophone discourse (2009: 62). It is manifest even in obituaries that “‘internationality’ is a more pressing imperative for academics in Germany than the US’, as a discourse analysis by Hamann and Zimmer shows (2017: 1427). Jehne reiterates the ideal for the discipline of history in Germany where the reception of different languages is still common, while the publishing practices, ideally, also tie language with topic focus of scholarship. And yet, he recounts the following, telling story (Jehne, 2009: 60): a funding body initiated the invention of a rating but had to realise that the parameter

internationalisation (in terms of a diversity of languages and geographies) would lead to the exclusion of most of the esteemed American journals (which require English only). This made the impasse obvious that esteemed Western journals not necessarily connect with the scholarly ideals rooted in diversity or regional specificity.

Gumbrecht affirms, referring to Luhmann, that the university and the humanities in particular are systems with the justified affinity to *increase* complexity (instead of the usual social systemic function of a reduction), a complexity required to resemble contemporary world's intricacy and density (2015: 23): the matter of expressions of the human mind is a labyrinthine entanglement. Homogenizing governance practices can only ridicule this affinity as they steer scholars away from deliberation and dialogue. Such homogenisation, and particularly the drive towards English as a primary means to communicate scholarship, becomes manifest in German humanities and risks the regional anchoring of language and culture towards which scholarship is bound (Seyfarth and Spoun, 2010: 88). This is not to say that no new questions can be formulated—intercultural topics abound in contemporary society. But where the object of study may allow change, method and perspective are those elements bound to the regional.

All the while, this actualisation of NPM governance culture evokes a conflict with the traditional, deemed quasi-feudal hierarchies of the *Lehrstühle* (professorial chairs) within German universities. It is because of this combination—tenured professors maintaining reputation and position, formal rationalistic terms of competition among the rest—that lead to the empirical situation that the young bear the most pressure induced by new governance (Ullrich, 2019: 2–4). They bear disruptions of unstable subject formations where traditional and newly enforced culture principles confront each other.

This is different to the situation in the UK where the REF induces pressure more distributed across levels of seniority; in Germany, this is very much geared towards steps of

career advancement. This is visible in the way perceived pressure is distributed in my exploratory survey: among German scholars, pressure groups take the form of distinct groups much more than among UK scholars, indicating bottleneck situations of career advancement linking to pressure; this is much more linear and diffuse in the UK (figure 36). Until career advancement takes place, young scholars remain under the auspices of senior professors and formal rationalistic rules demand scholars gain visibility among this establishment for which the internationalisation of one's publications is made a key mode of practice.

6.3 Publishing Accountability and Bureaucracy

The Sammelband as Symbolic Achievement

There is another articulation of formal rationality, one to be found in the strict rule following stemming from bureaucratic legitimation in Germany. It imposes on scholarly endeavours that everything has to be published. Publishing artefacts become a means to being held accountable, a means of fulfilling project, funding, or public liability requirements. This is best explained where one of my interviewees states that every conference needs to be *buried in a Sammelband*. It highlights the burden, borne by cultural hybridity within the scholarly subject, that bureaucracy persists while the instances of bureaucratic rule-following soar. This shapes publishing practices as a formal means, quite well outlined by an alienated condition.

The practice of formally publishing conference proceedings in the humanities in Germany emerged topically in my interviews without me triggering it. Both junior and senior scholars referred to this and they did so mostly disapprovingly. And this seems to be an issue particular to the humanities. In other disciplines such as those of engineering or

computer science, conference proceedings are regarded as a core genre for the communication of scholarship. The practices, generally, are the same as for journal publications, only that its representation centres around the talk, not the resulting publication: preceding both talk and publication, however, is the selection process facilitated through peer review or (conference/editorial) board decision-making. Thus, because conference proceedings have such a high value for these scientific disciplines, key advances of the field are also communicated in those formats.

In the humanities, however, key advances can take place across formats (while the monograph is partly still reserved a key place, as my survey indicates). Talks at conferences, however, are rather discussions resulting from previously published scholarship. Conferences are, as John referred to them in my interview, the social sites of debates in the humanities that, so to say, extend the formal realms of previous written publications.

That this social site becomes re-packaged as a formal publication through conference proceedings or the *Sammelband* seems peculiar as this practice is tied to traditional reasoning in times when other practices advanced progressively. To be sure, turning lecture into book has a long tradition in the hermeneutic disciplines. Consider only exemplary works of Weber ([1917] 2015), Kermode ([1967] 2000), or Habermas (1971) which were all lectures first. And yet, if there are simply too many lectures, this becomes messy. There are simply not that many Webers, Kermodes, or Habermas.

Such publishing artefacts seem to create archives instead of communicative means. Scholarly communities have to bear a cultural hybridity where traditional reasoning persists, and new governance principles drive the instances in which the reasoning is applied. Such instances are enforced as German scholars have to conspire ever-more: collaborate in inter-institutional projects or contribute to conference-type meetings of varying sizes. The result is the release of published artefacts that pile up on library shelves without readership—

those interested in the reception have usually already received the message during the projects or meetings. In the critical terminology of Münch, these masses of artefacts contribute little to discourse and cause a feeling of *over-satiation* among scholarly communities (2008: 176). Such *over-satiation* is well documented in my interviews as well. Likewise legible in my interviews and as Münch also asserts, submissions to the *Sammelbände* are often half-baked (Münch, 2011: 160): they have been discussed already at conferences and no one expects them to be read, so that only little effort is put into their creation.

Expressions of concern about a shift from journal articles towards edited collections can further be traced, reverberating throughout the discussions of the development of publishing practices of different humanities disciplines in Germany: both Fohrmann (2009) and Jehne (2009) confirm their dominant position for philology and history. Ultimately, these publications cease being direct communicative means as they exist primarily as archival material. This impacts on publishing practices in subtle, reflexive ways where authorship is considered to be a means of accountability so that it is borne by a formal proceduralism. Nevertheless, it signposts a form of productivity for the author.

Qualification and Formal Proceduralism

Such proceduralism is most pertinently visible in the persistence of formal legitimization of scholarly qualifications. There are some terms that explain the peculiar German tradition of requiring scholars to publish their theses: a literal translation of the term *Qualifikationsschrift* would perhaps be *qualifying scripture* (essentially theses). Correspondingly, *Dissertationsdruckzwang* and *Abgabepflicht* refer to the mandatory printing and depositing of the PhD-(or *Habilitations*-)thesis. The *Dissertationsverlag* is the category of

publishing houses that is (sometimes exclusively) focussed on publishing such theses (usually without any editorial emendation).

These terms prevail for a reason, their semantics highlight the principle of bureaucratic rule following irrespective of discourse. There is little debate about the necessity of the mandatory printing and depositing, which often results in early career scholars taking their PhD theses directly to publication. Stumpf elaborated on the legal issues behind these practices and concludes that they are tied to traditionalist academic culture rather than state law (2016). Again, this issue was raised in negative sentiment during my interviews at several instances: senior scholars almost unequivocally claimed a later publication is always worthwhile. Putting effort into changing thesis into monograph proper benefits both author and reader. And yet, the traditional ruling persists.

The result is that text which is considered to prove a scholar's ability to conduct scholarly work in accordance with a scholarly community's established practices of legitimating knowledge is required to contribute to an established discourse. In a sense, the PhD-committee can be considered to form its own discourse and the thesis is its central contribution next to the viva. Think of the lengthy literature reviews, extensive adding of references, or voluminous methodology sections: these prove a point for the qualifying author; they are hardly ever of interest to the discourse community. A devil's advocate might ask how many established scholars are interested in the very early career's first writings—and why, in turn, these need to be traditional publications?

Moreover, there are ways to facilitate accessibility to PhD theses without formal publication. Digital repositories, just as in the UK, might suffice as an archive. Scholars may take time to improve the thesis before aiming to publish it traditionally, thus turning qualifying scripture into a valuable contribution to discourse—and many senior interviewees in the UK seem to support this by reasoning that this only improves both text

and resulting scholarly identity. The thesis requirement is reduced to keep holding the thesis accessible here. In Germany, instead, universities even reserve the application of the title (Dr. X) until the thesis has been formally published. This is the material articulation of the culture principle that exerts pressure on the recently qualified Dr. X to go on publishing the thesis, thus engaging in a practice for which the publication is a formality. In addition, there is an odd empirical finding in Germany's largest academic survey, showing that the formal quality of PhD theses in Germany is across fields perceived as substantially better than the actual quality of content (Ambrasat and Heger, 2020: 37). A formal rationalistic interpretation of this may be: better be formally safe than intellectually daring.

Actualised in this practice is a traditional culture principle that claims scholarship is complete only if it is externalised; a principle, however, that is indifferent not just to discourse but also to technological means available today. This proves empirically the Alexanderian conception that culture persistence can be stronger than technology (see the discussion of culture in chapter 3): scholars could just make their theses accessible digitally or informally in many ways, but the *illusio* of the traditional publication persists.

The historically grown requirement exemplifies how publishing practices mix up or disturb a normative ideal of publishing for communicative purposes: the internalised purpose of the thesis (qualifying the scholar) or the conference proceeding (fulfilling the rules of funding and archiving the discussion) is externalised (to contribute to discourse with indifference to readership). To be sure, not publishing theses or proceedings formally can by no means be translated into a situation where these texts are unavailable. But, in return, the existence of such a referent—a successfully defended PhD thesis, or a conference held—may by no means translate into a need for formal publication.

In the UK, the hasty publication of the PhD thesis is more directly connected to the demands of the job market; it is the institutional interpretation of managerialist culture

manifest in the REF. In Germany, such culture is evident as well, but it is paired with a traditionalist logic in a way that permits change, putting the early career scholar in a deadlock. This young scholar subject is bound to the earlier days of the *Gelehrtentum* with its dependence on the *Doktorvater*, even though this *Doktorvater* is partially resolved already in the hybridity of scholarly management. And still, the proceduralist rule-following of authorship and publishing demand the submission to formal rationality which impacts scholarly development.

6.4 Conclusion

In this chapter, I traced the cultural hybridity of tradition and progressiveness that inheres in the institutional humanities in Germany. Principles of excellence and internationalisation are embodied in publishing practices in ways to resemble anglophone discourses. The culture principles of manageability induce a rationality that legitimises and fosters an instrumentalization of publishing. The same principles are actualised in the governance and funding regime that demands ever-increasing rounds of collaboration and conferences. All the while, German humanities are still tied to the residue of bureaucratic proceduralism and traditionalist intellectualism, demanding that all steps taken need to be accounted for through traditional publications. Thus, there is a contradictory set of reasons that is to be acknowledged to answer the question of *why scholars publish*: the constitution of publishing practices is geared towards both traditionalist *and* managerialist culture principles. Germany's systemic transition to a new governance is an articulation of this as scholar subjects struggle with their rooting in the intellectualism of times past.

This allows us to consider inherent logics of hybridity, as the conception of Reckwitz proposes (2020a: 31). Traditional culture persists while new developments induce new formations of practices and reasoning, creating transformations which bear deficits and

internal contradictions. Scholars are caught within this hybridity. The practices that make them the scholar subjects that they are require them to endure the burden of contradictory doings. German scholars are, in this sense, caught between the culture principles of traditionalism *and* progressivism. Both together enforce a formal rationality that lets them perceive publishing artefacts as procedural means without scholarly liability to discourse or readership—at once because of bureaucratic rule-following as well as through an excessive enforcement of symbolic valuation.

Compared with the preceding chapter about the REF, there is no singular direction of progress, a sort of struggle that directly exerts influence on practices that effect pressure among scholars. The cultural hybridity is much more an articulation of partial change impacting a community with a strong and power-dependent cultural inheritance. It is for this reason that the interviews do not show as much concern or even anger about the progressive change (as is evident for the REF). The traditional culture towards which subjects are naturally drawn is partially responsible for the practices as they are constituted today; criticising them would mean criticising the suggestively stable pole of the shared cultural past—a past that is by managerial culture to be deemed a constitutive outside while it is still drawn on.

Instead, what is criticised is the progress of governance expressed as just this managerial culture. On the one hand, this seems justified from the scholar's perspective, since this progress demands an inattentiveness to discourse. On the other hand, scholars seem to blindfold themselves by looking only at progress, while this progress is, in this instance, rather to be criticised in its contending hybriditous connection with a traditionalism. To be sure, the many conferences and artificial, scholarly-unreflective collaborations may be a stigma on their own; but in regard to the instrumentalization of publishing, they need not be the driving factor of concern.

Ultimately, the *Thatcherite Nationalism* encountered in the UK's REF is likewise visible in Germany—except that the denotation *Thatcherite* may be defective. Short governmental cycles may provide the keywords, but they are usually name-giving references to larger culture trends which they merely exemplify. In the case of excellence and internationalisation as mechanisms aiming to improve a nation's standing, *Thatcherite* may be a referent only to the larger drivers of neoliberalism and its governmentality that are entrenched in contemporary culture, being actualised in almost all institutions of society.

And yet, where the UK's humanities have always been stratified into elite and non-elite, Germany's *Geisteswissenschaften* are rooted in a homogeneity of scholarliness that defines itself through its culture ideals. The ivory tower was coherently inhabited by all humanities scholars in Germany—in the UK, this was true only for a smaller circle within elite institutions alongside a cluster of institutions serving more worldly ideals of education for private sector careers. The cultural shifts articulated by excellence resemble each other in terms of the formalism of means and the objectives of the two countries. But where Germany and the UK have different historical roots, the actualisations of these means differ as does the impact on scholarship. Whereas in the UK, the actualisations effect fundamental disturbances on the job market which is ever-increasingly based on measurement of formal authorship, in Germany, formal rationalistic terms cause a more subtle, more reflexive change of the meaning of authorship and publishing. Published artefacts are much more affected by how they are constituted and what purpose they serve which reflects on and shapes practices. This only underscores the cultural mediation of material artefacts.

The REF serves as an elongation of the principles articulated by *publish or perish* such that the scholar has to legitimate her productivity through published artefacts. In Germany, the impact is more differentiated: the imperative to internationalise effects a change of the

ways to engage in discourse—even the question of which discourse to contribute to in the first place; the proceduralism of accountability, instead, can much less be understood in terms of individual pressure in relation to subsistence. It is a systemic pressure where the reproduction of the disciplines depends on practices that slowly clog the pipelines of reasonable communicative practice. In both cases, nonetheless, an enforcement of an alienated condition becomes manifest, legitimated by culture as authorship is externalised from the realms of discourse. The latter impact is a practical problem, one that affects readership as it does authorship. The former impact, internationalisation, may become a substantial problem for scholarship.

Tools to increase productivity and internationalisation claim to foster collaboration and invite international scholars into a nation's academic institutions. This appears to be a favourable endeavour. And yet, the indifference to the purposes of scholarship—the substantive irrationality—may mean a focus on such an endeavour without acknowledging the societal and cultural role the—to be internationalised—scholarship inhabits regionally.

In other words, what is cut off by enforcing international scholarship in every project? What remains if more and more contributions to discourse are forced to be contributions to a regionally-unspecific discourse? Will this enforce, in one of my interviewee's terms, a *dereliction of language-specific discourse*, an abandonment of the humanistic ideal of regional cultures and languages co-existing in scholarship? This thesis is not the place to answer these content-specific questions, unfortunately. But by analysing publishing practices, it becomes evident how the grounds of discourse are indeed increasingly geared to a sort of funnelling, impacting the culture principles which give meaning to discourse itself.

7 A Culture of Deliberation: The Humanities and its Ideals

After having clarified the empirical realities of perceived pressure in Germany and the UK, the question of what constitutes publishing culture in the humanities as opposed to the social sciences remains open. I answer this question in this chapter in two steps: firstly, I look at some historical origins and conceptual foundations that may unite disciplines into the humanities (7.1); and secondly, I offer an empirical approach by providing data from my exploratory survey about authorship and publishing (7.2). These two sides complement each other as they provide different qualitative and quantitative approaches to determining characteristics of the humanities. They complete the outline of publishing cultures that I explored empirically in my exploratory survey, thus, adding depths to the preceding chapter about Germany and the UK.

7.1 History, Hermeneutics, and Expressions of the Human Mind

What do I refer to when I say *humanities*? I could follow a pragmatic path and claim, with Bod, that the ‘humanities disciplines are the disciplines that are taught and studied at humanities faculties’ (2013: 2). But the matter of faculties is quite complicated. You often do not find a humanities (or: *geisteswissenschaftliche*) faculty in Germany so that this definition will not suffice. Moreover, such pragmatic approach would reveal little about the underlying culture of scholarship in the humanities. Looking a bit further at historical roots and paradigms helps identify principles of scholarly culture which, in turn, help set the frame for determining what the humanities can be, in an inclusive way.

Etymology

The origin of *humanities* shows that the concept has not developed linearly as a branch of scholarly enquiry but has been tremendously shaped by different cultural influences. The *Oxford English Dictionary* (2009) defines *humanities* as

a. Literary learning or scholarship; secular letters as opposed to theology; esp. the study of ancient Latin and Greek language, literature, and intellectual culture (as grammar, rhetoric, history, and philosophy);

and

b. The branch of learning concerned with human culture; the academic subjects collectively comprising this branch of learning, as history, literature, ancient and modern languages, law, philosophy, art, and music.

The first definition reaches back to Caxton's translation of Jacobus de Voragine's *Legenda aurea* in 1483 and is much smaller in scope than the second definition, the appearance of which dates to 1855 (OED, 2009). Rüegg traces the term's origin to Cicero's use of *studia humanitatis* and, in a university context, to a university chair termed *litterae humanitatis* at Bologna in 1512 (Rüegg, 1996). Only gradually, over the course of centuries until the mid-nineteenth century, the *studia humanitatis* developed as a disciplinary category on its own, first taught by individual scholars such as Lorenzo Valla or Erasmus of Rotterdam, and included in arts curricular only later during the broader establishment of humanism as a unified movement in the nineteenth century.

Humanism as a period—as well as the use of the term humanities in a university context—were fuelled by the circulation of more and more books with contemporary texts after the invention of the printing press (Pederson, 1996). This is particularly noticeable where the printed book served as a means of circular enforcement of culture change beginning after 1500: printing made classic texts and interpretations of it available on a

broader scale which allowed for broader uptake for study, which, again, spurred interest in the medium (Febvre and Martin, [1958] 2010: 262–287). Similarly, the development of the reformation is intricately connected with—as well circularly enforcing each other—the development of print shops, the aesthetics of print artefacts, and their efficient logistics (Pettegree, 2016: 267–278). This connection is also true for the UK, as Caxton transferred the uptake of printing with moveable letters from continental Europe to England (Hellinga, 1999: 67–68), which helped to nourish humanism through the potential to study contemporary texts in the English language. That the rise of the semantics of humanistic scholarship is geared towards the increase of the circulation of textual artefacts is an alluring characteristic in the light of today's claims that humanities scholarship is burdened with too many textual artefacts while the interest in enlightenment is decried, as I discuss below.

The importance of the terms *arts* and *philosophy* can likewise be looked at through the lens of the forming of the modern university. The later use of humanities as a 'branch of learning' (OED, 2009), starting in the middle of the nineteenth century, is placed in a period where humanism developed more thoroughly into a pedagogical and epistemological branch. This seems to have influenced university structures in the UK much more than in continental Europe, where, for instance, many of the German universities (and only some traditional ones in the UK) still resemble the medieval structure with their faculties divided into philosophy, theology, and jurisprudence, next to faculties dedicated to social and other sciences. Looking at German faculties, one sees barely any resemblance to the UK's humanities division. Rather, it is the *Philosophische Fakultät* (literal translation: Faculty of Philosophy) that comes closest to what is today a Faculty of Humanities, being home to disciplines such as philosophy, history, or different philologies.

The division *Philosophische Fakultät* goes back to the early meaning of philosophical enquiry, and developed from the *facultas artium* of the medieval university (Gieysztor, 1996). This *facultas artium* was one of the four faculties at continental, medieval universities, which derived their structure from the early University of Paris. This *facultas artium* was accompanied by the Faculties for Theology, Jurisprudence, and Medicine. The latter faculties were higher than the arts, the study of which was the *propaedeutics* for studying at those higher faculties. The subjects of the arts were the *septem artes liberales* (bearing resemblance to today's liberal arts). This, thence, is the historical connection between today's institutional manifestation of disciplines in the UK and Germany, seen in the Faculties of Arts and the *Philosophische Fakultät*. In Germany, as in other continental universities, the *facultas artium* developed into the *Philosophische Fakultät*. Alongside this, philosophy evolved as an own discipline out of dialectics/logic of the *trivium* of the *septem artes liberales*, earlier still including natural philosophy (today's natural sciences).

Geisteswissenschaften and Humanities

It is no coincidence that the period when the humanities were formed institutionally in the UK was also the period when the debate about the *Geisteswissenschaften* (humanities) as opposed to *Naturwissenschaften* (natural sciences) proliferated in Germany (Hamann traces the social forming of this period in a comprehensive study: 2014). Taking a closer look at those terms will result in difficulties for literal translations, though: the words have different origins, developed individually, and have incoherent meanings even in their own languages.

The meaning of the terms *sciences* and *natural sciences* have converged since the eighteenth century. The terms moved closer to each other and are nowadays often used

interchangeably (Wierzbicka, 2011), leaving a void for translating *Wissenschaften* into English. *Wissenschaften* is the German term for the ‘totality of human knowledge’ (Garland, 2012: 301), and is used as the basis for all scholarly enquiry, comprising of both teaching and research in a Humboldtian sense. The word has its origin in the Latin *scientia* (Duden, 2018), as has the word *science*. Yet, a translation of *Wissenschaften* to *sciences* only works in an historical context as *sciences* is too closely connected to natural, life, or formal sciences today. And already as the forming of the humanities as a loosely unified cluster was still in its early stages, researchers in scientific disciplines got their individual reference in the English language as *scientist*. Again, a translation is misleading as *Wissenschaftler* are more than *scientists*: the word *Wissenschaftler* is still used for all *researchers*. In the light of this, translations of *Wissenschaft* always require specification: *scholars* and *scholarship* in the arts and humanities, *scientist* and *science* in, well, the *sciences*, with the social sciences *in between* depending on their own contextual understanding.¹⁴ The unspecified institutional reference is, at best, *academic*: an individual employed as research staff is a professional academic—the general *Wissenschaftler* in Germany. Only, what is the *Geist* in *Geisteswissenschaften*?

A translation of *Naturwissenschaften* seems to meet the meaning of *natural sciences* (or even simply: *sciences*) quite well, but a translation of *Geisteswissenschaften* into English struggles with the meaning of the word *Geist*. *Geist* can best be translated with *spirit* or *mind*. Its adjective *geistig* might even better describe the meaning of *Geist*: for translating *geistig* into English one ‘must use phrases as “created by mind” or “mind-dependent”’ (Rickman, 1979: 60). Dilthey often used the phrase *Objektivierungen des Geistes* to identify the objects within the scope of the *Geisteswissenschaften*, which are best translated as ‘entities within our experience’ (Rickman, 1979: 61). In other words, the concepts and feelings that make up

¹⁴ My empirical data in the next sub-chapter confirm the ambiguity of this: there is not always a clear disciplinary separation, but an individually-defined *in between* state of humanistic or scientific social sciences; see especially figure 6 below.

stories, languages, religions, ornaments, or cultures. Thus, where the *humanities* are genealogically bound to *human*, the *Geisteswissenschaften* are bound to *mind*—and this, the *human mind*, is where the two meet conceptually. Bearing these considerations in mind, for readability and as modern dictionaries show as most applicable, I may let the terms *humanities* and *Geisteswissenschaften* refer to the same cluster of scholarship in this thesis despite their differential historical roots.

The Historical-Hermeneutical Disciplines and the Interest of Enlightenment

Reflecting on the genealogy of the humanities: what is the connection of *philosophy* and the *humanities*? With Kant, Martens and Schnädelbach try to argue that without the interest of enlightenment, any pursuit of philosophy is *merely* textual scholarship among others (1985: 34). The *merely* is odd since, thus, the authors see a hierarchy within humanities scholarship such that those owning the holy grail of knowledge, the philosophers with an interest in enlightening humankind, are somewhat above the ordinary and vocational philologists, historians, theologians, or linguists. Within Jaspers' thought, a similar yet more humble distinction can be found (1953: 9).

Questions arising from this are: is it bad to be *merely* humanities? And: is there an interest of enlightenment to be found in the pursuit of philosophy—or the humanities more broadly—today? Humanities scholars generally do not seek interference with practical applications as the sciences do, which is why the humanities are defended as *not being for profit* (Nussbaum, 2010). All the while, considering that scholarly endeavours seem to be moving away from intellectual daring to institutional reproduction in the mass university (see the discussions in: Liessmann, 2006; Münch, 2011; Readings, 1999), integrating an interest of enlightenment into these scholarly endeavours seems to be a

noble, even mystical ideal, which only few may be able to achieve. The two questions in combination even transcend the enquiry into philosophy and turn on the humanities in general. Their self-comprehension is grounded in the notion to understand what it means to be human and, by passing on this understanding to others, to change lives (Thomä, 2019: 101). This is similarly noble, but just as equally difficult to maintain: the burden of praxis in its simplest sense—scholarship as a job with insecurities, social inequities, and institutional power plays—seems to disrupt the ideal of the interest of enlightenment.

Becoming practical impacts thinking and what scholarship can achieve. Philosophy stands out as an exemplary case here. Marquard is prominent as a philosophical whistleblower as he articulates contemporary philosophy's *Inkompetenzkompensationskompetenz* (German for: the competence to compensate incompetence)(2020a). His argument is that philosophy lost its power to other disciplines or societal fields and can no longer hold a substantially contributing competence themselves. The once highly regarded scholarly pursuit, starting institutionally in Ancient Greece, has lost its power to the church, to the natural sciences, or to politics. It remains a nostalgia for the once powerful competence that has left a void which is to be compensated for (Marquard, 2020a: 38).

This is pertinent to the wider question posed above: Marquard relates a substance of philosophy to being the substance also of the humanities more generally: 'to be an experience of life for those who as yet have none; to be wisdom of old age of the not yet old [...]' (2020a: 34; translation by the author). This substance is essential for comprehending Marquard's understanding of the humanities. He also suggests the humanities are *inevitable* because of the rise of the *exact* sciences, which try to resolve the subject from the experimental process (2020b). He, thus, turns a common argument around: it is not the rise of objective knowledge and the corresponding development of society in modernity that risks the project of the humanities; just this rise and the

modernisation make the humanities a necessity. This is an inversion of the common humanities defence line.

Society evolves, and experiences are burdened by an ever-faster evolution during modernity (Rosa, 2013); its processes of rationalisation reinforce Weberian iron cages (Douglass, 2018); individuals lose their sense of commonality and become more and more calculating subjects (Simmel, [1903] 2008)—I discuss this in more depth in relation to academia in chapter 8. Pertinent to Marquard’s argument is that this speed and the resulting estrangement need to be compensated for as they produce ever faster futures. And as any future requires an origin, securing origins is vital in times of faster futures. The humanities are the best resource for maintaining senses for and of origins, as they teach hermeneutical competence—the prowess to interpret and understand—which keeps origins alive and makes futures inhabitable: the ‘humanities help the traditions, so that subjects can endure modernisations’ (Marquard, 2020b: 178; translation by the author).

What does the often-emphasised principle mean that claims: the humanities ‘*investigate the expressions of the human mind*’ (Bod, 2013: 1; emphasis in original)? The humanities help individuals and collectives understand themselves by teaching the hermeneutics for deciphering what it means to being human, by being concerned with the *Menschengeschlecht* (probably close to: humankind) as Dilthey, the German theoretician of the *Geisteswissenschaften*, asserted ([1910] 1970: 89). This meaning is bound symbolically and materially in expressions of the human mind which, therefore, constitute the body of primary data in the humanities. Texts embodying this are ‘expressions of human identity, the study of which teaches us what it means to be human’ (Garland, 2012: 301). They do so not directly and by no means practically. The value of the humanities may even seem elusive. For instance, Small works out the subtle connections between the impact of interpretative schemata, emotional reflection, and the ways in which these can affect

rational development (2013: 89–124). By keeping interpretative schemata alive in scholarly discourse, such schemata and what it means to being human trickle down into all spheres of society, like a stone thrown into a lake. This is why a culturally strong society—culture in the sense of art, theatre, or literature—is one with strengthened humanities scholarship (Marquard, 2020c: 243). It can even be argued, that the humanities break away from the Humboldtian *Bildungsideal* to becoming a community of dissensus in which never-ending modes of culture are to be openly debated (Readings, 1999: chapter 12).

This can be scrutinised even more critically as it is problematised within the broader societal development as a dialectic of enlightenment such that the relentlessly evolving progress bears within itself societal regress. Enlightenment turns into a rationalist myth, while it claims to free the world of myths. Critiquing this by tying what it means to be human to a mythical past and irrational present is a core value of humanities scholarship. And it would, thus, require for the humanities to claim a status of the arts—they would have to reject becoming subjugated to praxis: the ‘urge to *rescue the past as something living*, instead of using it as the material of progress, has been satisfied only in art [...]’, Horkheimer and Adorno assert ([1944] 2002: 25; emphasis added). But the humanities are not a branch of art; they are much more intertwined with institutional practice and politics.

Culture Principles of Advancement: Progress and Recursion

The issue of non-practical ideals can be illustrated with an excursus on progress, which feeds into understanding authorship and its *newness* in the humanities. Maintaining a sense of origin means dealing with history as the world progresses. But the humanities themselves do not progress; they are furthered only by differentiation (besides the historical

progress that is natural to all praxis). Difference builds upon the definition of what is new and the assumption that that which is new is not necessarily progressive.

In the sciences, that which is new indeed is progressive in the way described by Kuhn (1996): development takes place within normal science, the historical period of a particular explanation of nature. That period will eventually be overcome: the advancement of knowledge is not a cumulative process but a navigation of legitimising, refuting, and, eventually, discarding what has been legitimately known. Revolution changes what and how objects are to be known and the scientific community progresses. Weber already emphasised this as he principled the cultural foundations of scientific work on infinite progress ([1917] 2015: 17).

The humanities largely defy such grounding in progress. Its knowledge is not revolutionised; it constantly evolves by circling back on itself. Instead of delivering progress, that which is new differentiates the existing body of knowledge recursively. Philosophical thinking does not have, as Jaspers asserts, ‘the character of a progressive process’ (1953: 9; translation by the author). This is true for thought in the humanities in general. It is only because of this grounding in difference and recursion that the expansion of content—an expansion of discourse—in the humanities allowed to involve the same questions for the better part of 2,500 years. Both in the ancient Lyceum and in today’s university, philosophers ask: does the world exist? What can we know? How should we live? Philologists look at past discourses and interpret them in relation to today’s standpoints. Historians try to make sense of times past by reflecting historical artefacts, discourses, or past interpretations of these. Surely, the body of artefacts that are being investigated grows, a body that constitutes the materiality of the variety of expressions of the human mind. The material available to historians evolves with the progress of time; philologists add new branches of text as their potential horizon; philosophers also find

answers reflecting contemporary life relations and (some) theologians reflect God and their heavens from today's mystically de-mythified perspectives. Nonetheless, the questions asked centuries ago stay relevant alongside their answers. With the development of humankind the historicity and complexity of life relations grows which posits that the potential for differentiation in the hermeneutical resources grows as well. The multitude of interpretations, a culture of ambiguity, the variety of answers to the same questions—these are vital to the humanities, they are not their doom (Marquard, 2020b).

This distinctness of progress and difference may appear counter-intuitive at first since progress and history seem closely connected. Historicity is crucial for the humanities; in fact, the forming of the *Geisteswissenschaften* built on the German historical school of thought (Habermas, 1971: 141). But historicity is not progress: it is the grounding of past and present *within* the development of progress.

It is precisely upon this distinctness that the importance of historicity builds: the former contribution to discourse never becomes irrelevant in the context of a newer contribution (as is often the case in the sciences).¹⁵ Every answer remains relevant as an historical relict in the discourse to which the newer contribution adds difference recursively. Humanities scholars regularly excavate earlier paradigms, presenting their subject or writing *in the tradition of*, and even study the artefacts and paradigm shifts present in any contribution to discourse throughout history. This is reflected in the intertextuality of authorship, as Hyland suggests that 'new knowledge' in the humanities 'follows altogether more reiterative and recursive routes as writers retrace others' steps and revisit previously explored features' (1999: 353). The resulting difference to the body of

¹⁵ To be sure, I do not wish to insinuate that scientists should not know their discipline's history—I do think they should. But to know your discipline's history is not necessary in order to be, say, a biomedical researcher today.

knowledge is a variation of the hermeneutical resources which is where the matter of the human understanding is most pertinent.

The Humanities as Discourse Communities

After this short tour through foundational principles that may help understand the humanities, the question remains of what I refer to when referring to the humanities. The answer is: I refer to the humanities as a—cluster of—discourse community(ies) comprising of disciplines in an inclusive sense of their self-understanding.

I defined discourse, immaterially, as meaningful text which is always to be seen in interconnection with its practices and artefacts. Having started there, I now turn more comprehensively to Foucault, as he both rips apart the possibility of a conclusive unity of discourse while also showing how discourse is actually the fundamental category for what is commonly seen as disciplines. These are

reflexive categories, principles of classification, normative rules, institutionalized types: they [...] are facts of discourse that deserve to be analysed beside others; [...] they are not intrinsic, autochthonous, and universally recognizable characteristics (Foucault, [1982] 2007: 22).

A scholarly discipline is a disciplining and categorising reference that, both reflexively and pre-emptively, guides practices concerned with this discipline's epistemes. But no entity grasped with a disciplinary reference is *eo ipso* that category. In all unities that individuals refer to categorically inheres a dialectic that suggests it is always also something else, either today or in history, here or somewhere else, and in this or another epistemic function.

In simplest terms, philosophy can indeed be philological scholarship just as the analysis of, for instance, Benjamin's literary texts needs to be a philosophical investigation,

too. Historical scholarship can be regarded as discourse concerned with historical matters and the methodology thereof; likewise, any historical analysis innately embodies some historical textual scholarship, sociological analysis, and matters of believing (theology) or knowing (epistemology). Those categories only matter in the context in which they are used as they unite entities of otherwise chaotic nodes in a vast epistemic network. In turn, by uniting entities, they provide coherence to those concerned with a particular episteme by funnelling recursions of action, thus institutionalising them so that they appear as culturally meaningful practices. In everyday idiom: this is the way it is done (in this discipline, in this journal, or at this institution). These categories, thereby, become social facts; they influence the way subjects behave and are constituted. And other social constructions—categories likewise artificially bestowed upon disparate entities—become social facts as well. The JIF is notorious in the way it gives coherence and meaning to a unity which, in turn, shapes practices that embody this meaning as they are geared towards publishing with the highest impact-performing journal. The REF rating stands in this line, too: the clusters of stars in which all scholarly performance in the UK is to be sorted reflexively constructs a meaning that guides practices way beyond the direct reach of the REF.

Because of this social and cultural responsiveness, I see disciplines as discourse communities. The analytic category of discourse community gathers subjects through practices and artefacts concerned with specific, epistemically bounded text. Consider history: it is generally spoken of as a discipline, but it may also be defined as a broader discourse community because of the practices of communicating historical scholarship and the corresponding artefacts—journals, books, blog posts, conference proceedings, or even unofficial text. This can further be broken down into local histories, for instance, in the UK: the *Victoria County History* can be unified by its official association, or by its book series which forms artefacts with specific practices required for contributing to them.

Now, generalise from history and include other disciplines as well to arrive at a more abstract category. Looking at the whole of professional—that is, academically institutionalised—scientific and scholarly endeavours, those characteristics can be found that I discussed in the sections above as specific to particular discourses termed humanities. It is, thereby, not the academic institutionalisation, nor the official title of a scholar, and also not the keyword of a publication that unites these subjects; it is by way of epistemic unity and the practices that are engaged with for contributing to these discourses that the uniting takes place (of which keyword and title are only reflections). I, thus, propose to recognise the humanities and their disciplines from within, since discourse is their primary locus.

This is, therefore, a definition resulting in a variable field as seen, largely, from within. This *within* points to practices of legitimation. These practices, ever-more dependent on peer review today, determine what counts as legitimate discourse (as I further elaborate on in chapter 10). These practices are always performed by those with a required status *within* the field, so that a community is formed as decisions are made about legitimacy. Such practices take on the form of a ‘censorship of heretical productions’ since, from within, a few put forward ‘the definition of the publishable’ (Bourdieu, 1975: 30).

Hyland further discusses the possibility of a conception of discourse communities. He partly relates his conception to Becher and Trowler’s academic tribes which considers the community-building mechanisms of specific academic cultures (Becher and Trowler, 2001: chapter 3). As these cultures are embodied in practices, one way of determining a tribe might be to pick up specific practices through which a tribe can be identified. Disciplinary practices produce and reproduce the ways of meaning-making that provide the basis for elementary aspects such as determining what is justified knowledge, how others are to be acknowledged through references, or how to package this in formal terms for

submission to a communicative locus, the artefact termed journal. Hyland summarises that discourse communities can be seen ‘as real, relatively stable groups whose members subscribe, at least to some extent, to a consensus on certain ways of doing things and using language’ (2009: 49); a category that refers to ‘a principled way of understanding how meaning is produced *in interaction* and [which] proves useful in identifying how writers’ rhetorical choices depend on purposes, setting and audience’ (2009: 66; emphasis in original).

This may suffice to show how essential the connection between the content of discourses with discursive practices and artefacts is. In this sense, I refer to the humanities as a discourse community—a community bounded by their practices concerned with discourse, its epistemes circling around expressions of the human mind or what it means to be human. This discourse fans out into many smaller and, eventually, niche epistemes defined by disciplinary subject, region, or language—creating many intricately connected small discourse communities that overlap. And it is by means of their practices concerned with such discourse that they are geared, ideally, towards a culture of deliberation. Note that I refer to discourse non-materially here; I make the case of publishing venues—journals or publishers and their brands—being the loci as material artefacts of discourse communities in chapter 10.

All the while, the variability and dialectic of reflexive categories need to be borne in mind while using them in a manner productive for analysis. The approach I propose here highlights the interconnectedness of content and practice which is essential to evade a one-dimensional analysis. The sense of inclusivity in this approach allows consideration of both historical meaning-making, formal criteria, and the self-understanding of scholars. Particularly this, parts of a self-understanding of practices, is what I aspired to test empirically. The next sub-chapter shows the results.

7.2 Disciplinary Differences: Humanities in Authorship and Publishing

After engaging in the abstract principles of humanities scholarship, I turn to authorship and publishing concretely. Since the ways scholars engage in discourse and contribute to it are different across disciplines, the means and perceptions of doing so may be able to render the disciplines on the basis of practices. Part of my exploratory survey was to do just this. In this sub-chapter, I present findings from this survey (for the methods of obtaining and analysing the data, see chapter 3.4).

Hypotheses

I developed the hypotheses as a condensed list of common statements about authorship and publishing practices, and discourse more generally. This *commonness* is derived from engagement with the widespread literature about scholarly communication irrespective of theme. In other words, it is not necessarily literature about specific characteristics of humanities practices where such statements appear; this can be seen in the literature presented in chapter 2. These statements often appear to be only assumptions. Some such assumptions seem to be trivial and broad, such as those about preference of monographs and single authorship; others are more nuanced such as those about slower publication processes or engagement with metrics. However, they often remain implicit or accepted in the literature.

With my empirical investigation, I wished to determine the details behind those assumptions: to what degree are such statements substantiated within the humanities? Are there differences between Germany and the UK as well as in relation to the conceptually closer neighbour, the social sciences? Besides these assumptions, statements about the

publishing history of scholars contributes to determining average numbers of publications at different career stages as well as accounts about the languages and formats used.

Determining these substances and differences helps locate the idiosyncrasies of the humanities in a pragmatic, practical approach. Furthermore, these findings help to contribute to an analysis of authorship and publishing where practices are not simply descriptive in itself but point towards an ontological location of disciplines among other disciplines. The statements phrased as hypotheses are (if in relative terms, in relation to the social sciences):

- Single authorship is dominant in the humanities.
- Humanities scholars prefer monographs over articles.
- Humanities scholars are less engaged with metrics.
- Humanities scholars are more inclined towards the physical availability of monographs as opposed to digital discoverability.
- Humanities scholars prefer slower publication processes.
- Humanities scholars are rather averse towards OA.
- Humanities scholars are more reluctant in response to self-publishing services.
- Salami slicing is less common in the humanities.

This list could have been extended with similar hypotheses; it is by no means exhaustive. The ones I focus on here are a selection based on what I identify to be the most common, untested assumptions. Next to gaining profound information on the published portfolio of humanities scholars, a theory that tries to differentiate the

humanities on the basis on authorship and publishing would have to clarify the bases for such assumptions, as I do in the following sections.¹⁶

Disciplinary Self-Understanding

Before presenting the findings about statements, I show the disciplinary self-perception of scholars in the humanities and social sciences. Self-perception relates to the question of whether a scholar sees herself as a humanities or social science scholar. Respondents were asked to state their discipline as well as the disciplinary cluster. The combination of the two is represented in figure 23 for key disciplines.

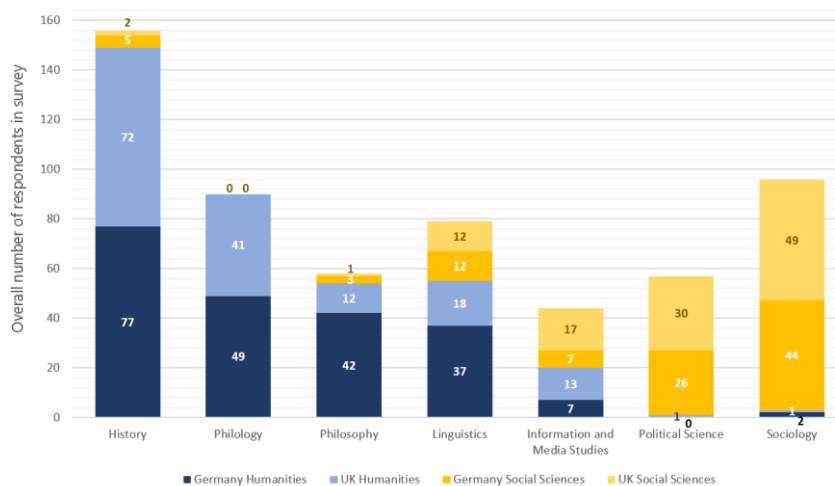


Figure 23: Disciplinary self-perception.

As can be seen, there is a quite clear separation of disciplines into clusters. Respondents with a subject focus in history, philology, or philosophy largely mark themselves as humanities scholars, while political scientists and sociologists do so as social scientists. There are only a few outliers. This is opposed by linguistics and the cluster of

¹⁶ Note on statistical significance: although I present the data as grouped in career progressive levels, I calculated the statistical significance of the hypotheses both in simple disciplinary clusters and as in the represented 12-fold grouping. See for statistical test results: appendix A5.

information and media studies. Scholars here showcase a mix of the two clusters: being a linguist does not necessarily mean being a humanities scholar or a social scientist. For these disciplines, therefore, a generalising or normative statement about the cluster a project or scholar is to be sorted into cannot be made—it always depends on the specifics of the work.

Publishing Portfolio

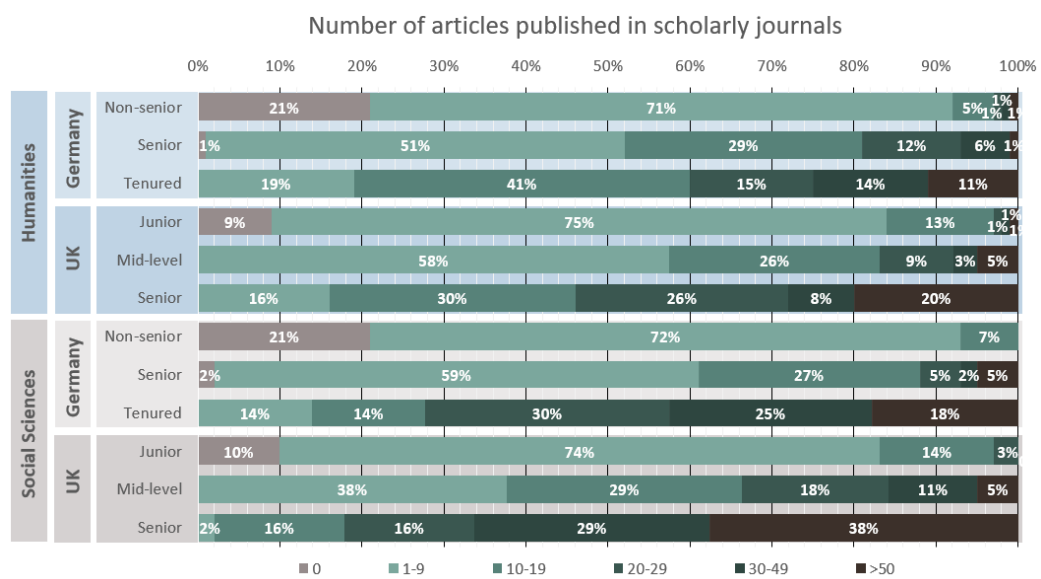


Figure 24: Average number of published articles.

Figure 24 indicates the considerable differences within average portfolios of published articles. The number of published articles rises proportionately with the academic level. In Germany, there is a clear division between senior and tenured scholars. Within both the humanities and the social sciences, there is a distinction between the number of publications of tenured (Germany) and senior (UK) scholars in that scholars based in the UK tend to have more articles published than scholars in Germany. This disciplinary difference is similar for published monographs (figure 25). However, non-senior scholars in Germany tend to have published a monograph already while junior

scholars in the UK tend to have not. This may be explained by the formal requirement to publish the PhD theses in Germany.

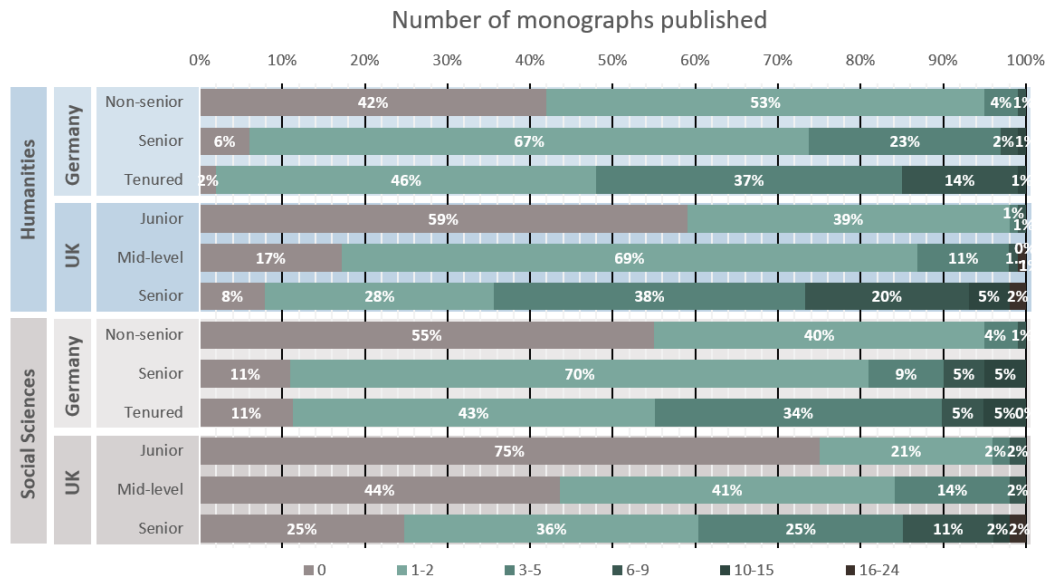


Figure 25: Average number of published monographs.

The figures 26 and 27 below show the experience of having at least one article and one monograph published OA. The difference between formats is striking. While the statements about articles are mixed, a prominent feature about monographs is that more Germans indicate they have published OA than UK scholars do.

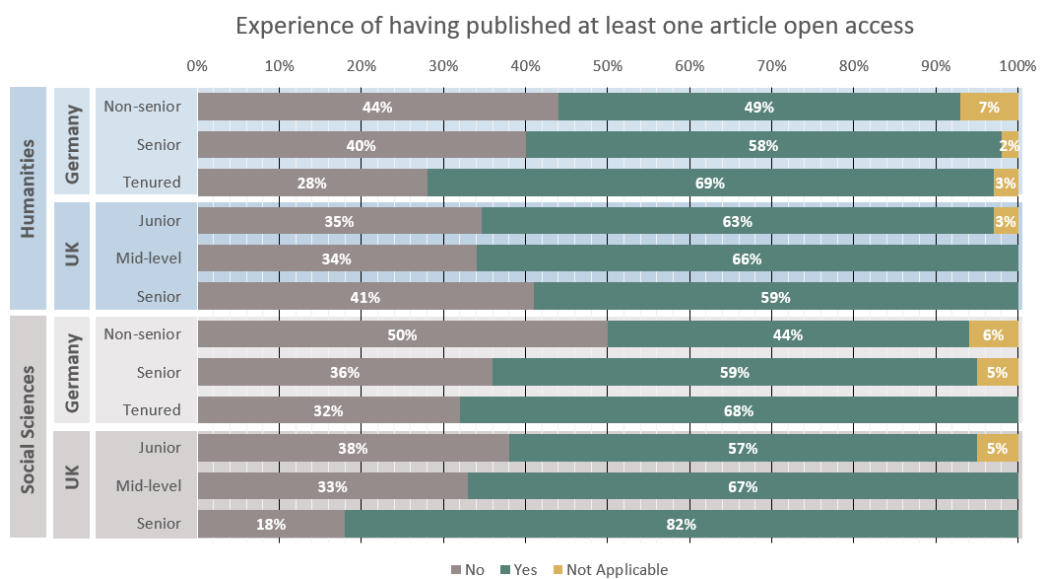


Figure 26: Experience publishing an article OA.

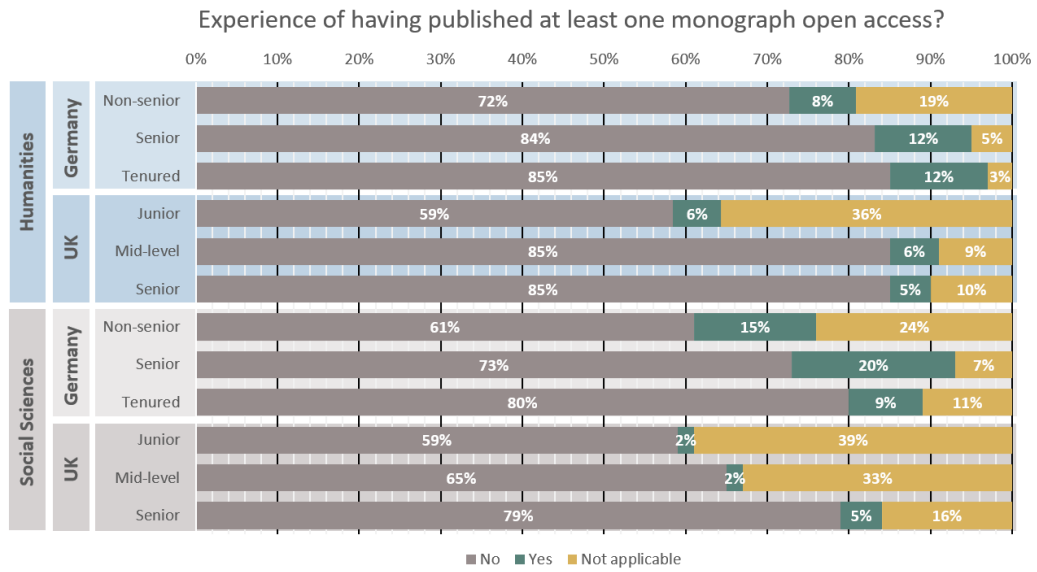


Figure 27: Experience publishing a monograph OA

Figure 28 shows information about published contributions to edited collections. These tend to be more important in the humanities than they are in the social sciences. Against the assumption of higher publications of articles and monographs in the UK, in terms of edited collections, there are more contributions published by scholars in Germany. An explanation for this may be, again, formal requirements about grants and proceedings in Germany.

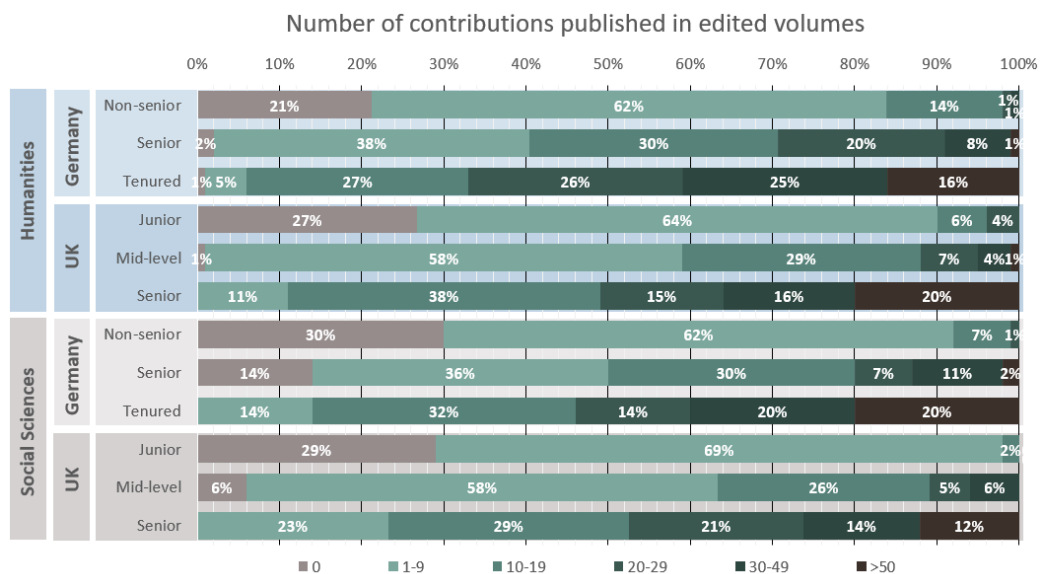


Figure 28: Average number of published contributions/chapters.

Figure 29 shows information about textbooks. The general assumption that this format is rather uncommon in the humanities and social sciences can be confirmed; there are only few senior scholars who have published 1-2 textbooks, and rarely anyone who has published 3-5.

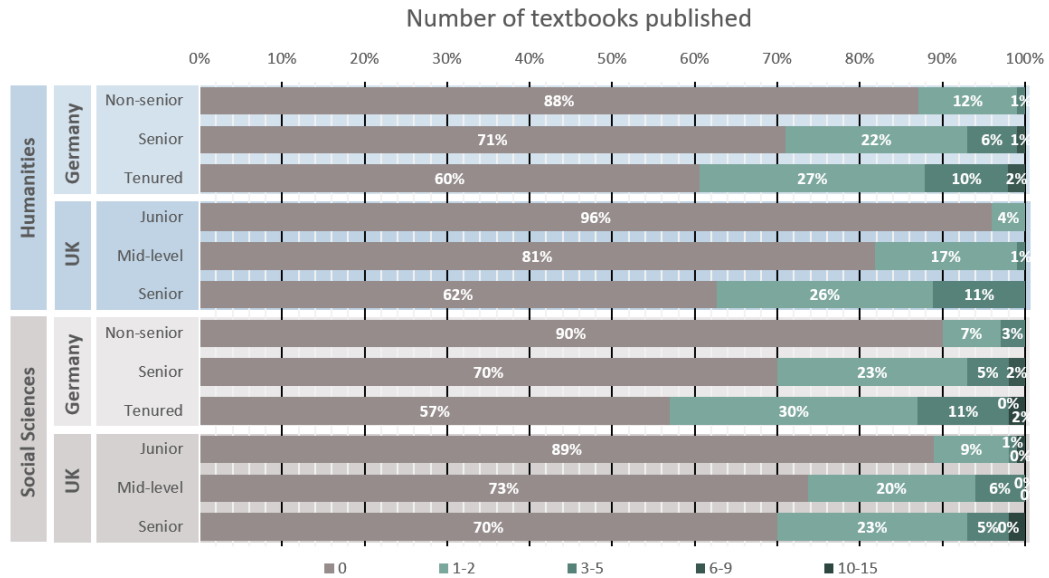


Figure 29: Average number of published textbooks.

Language

Figure 30 provides an overview of the use of languages in scholarly publications. It is evident that English is most influential in the UK. But also among German scholars, the English language seems to be an important medium, in the social sciences more so than in the humanities. In particular, a mix of English and German becomes predominant with increasing seniority. Languages other than English or German seem to be rarely used, mostly so among tenured humanists in Germany.

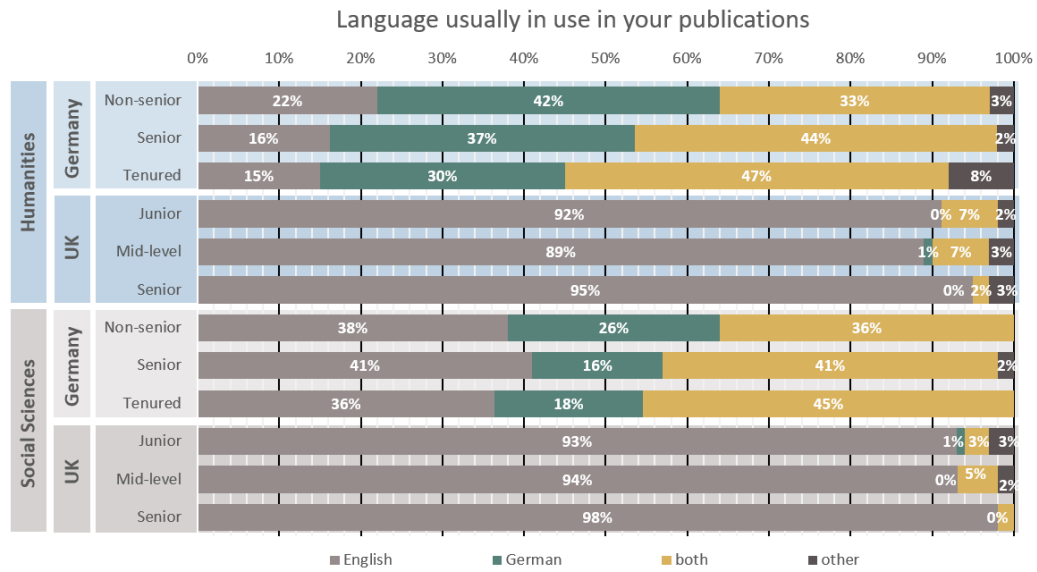


Figure 30: Language used in scholarly publications.

Figure 31 provides further data about language use in a follow up to the prior question, enquiring whether the language mostly used is a second language. The data confirms that many scholars in Germany are making use of a second language to communicate their scholarship. However, the number of scholars doing so is quite high in the UK as well, more so among junior scholars than among more senior scholars. An explanation for this may be the diversity and internationality of early career scholars gaining scholarly experience in the UK.

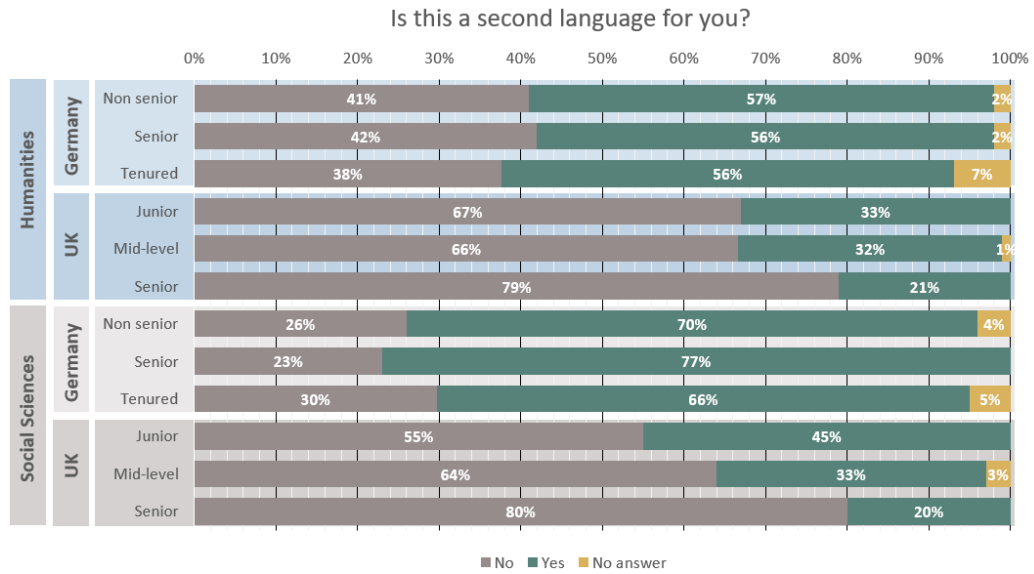


Figure 31: Second language use in scholarly publications.

Other Formats

Figure 32 shows that only a fraction of scholars has published non-scholarly books related to their scholarship. In general, the number of scholars having done so increases with academic level, but there are exceptions, for instance, among humanities scholars in the UK. Other non-traditional forms of publications are shown in table 7 (as an open comment field). Policy and grey literature publications were the most mentioned media followed by a variety of article formats for non-scholarly publishers.

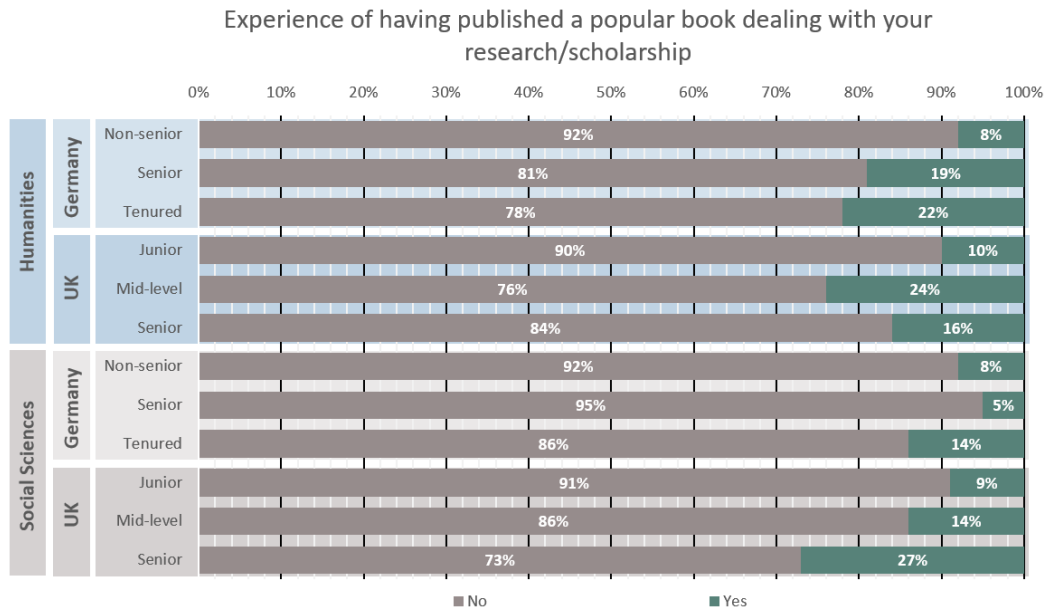


Figure 32: Non-scholarly books published.

Type of non-traditional	Mention	Type of non-traditional	Mention	Type of non-traditional	Mention
Policy report or grey lit.	31	Newsletter	3	Commentary	1
Special interest journal	30	Conference proceedings	3	Jubilee book	1
Newspaper	26	Interviews	2	Own edited volume	1
Digital resource	23	Radio	2	Pamphlet	1
Magazine	16	Schoolbook	2	Papers for my employer	1
Various online	16	Animations	1	Preprints on researchgate	1
Video content	15	Archive	1	Print on demand	1
Own or project website	14	Arts practice	1	Project atlases	1
Curatorial work	9	Audiobook	1	Self-publishing on Amazon	1
Engage in fora	5	Co-authored multigraph	1	Subtitles	1
Brochure	3	Comics	1	Translations of poetry	1
Ebook	3				

Table 7: Other non-scholarly publications in use

Figure 33 shows data about blogging practices. As with non-scholarly books, the number of scholars doing so is overall quite low. However, it tends to be more popular in the UK than it is in Germany. Moreover, blogging is not confined to younger scholars as many more senior scholars seem to publish blog posts regularly.

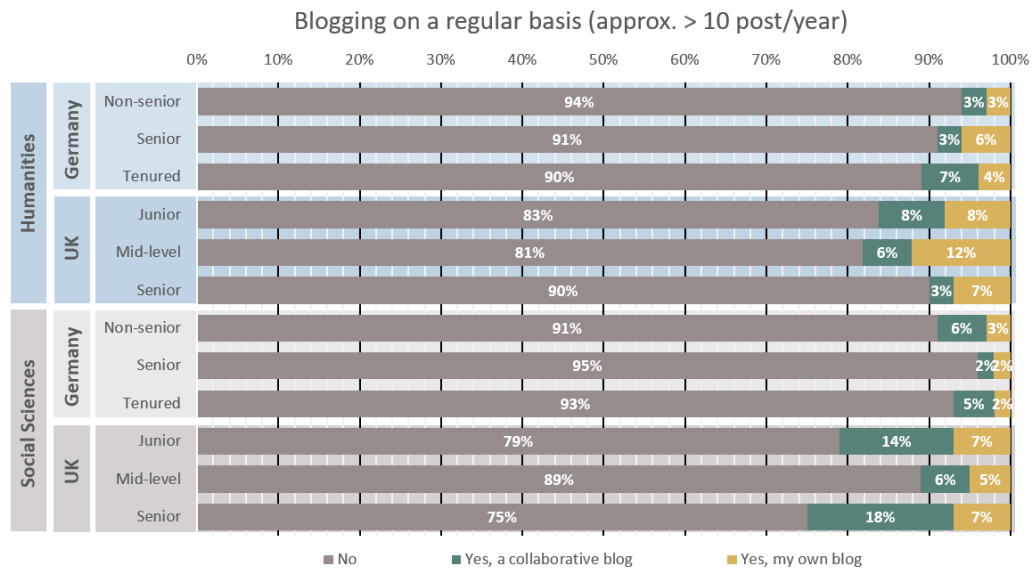


Figure 33: Number of scholars blogging on a regular basis.

General Publishing-related Perceptions

Figure 34 shows the significant difference between the humanities and the social sciences in terms of authorship: single authorship is defining in the humanities, while it is much less so in the social sciences. This confirms the common assumption that the humanities very much rely on sole authorship.

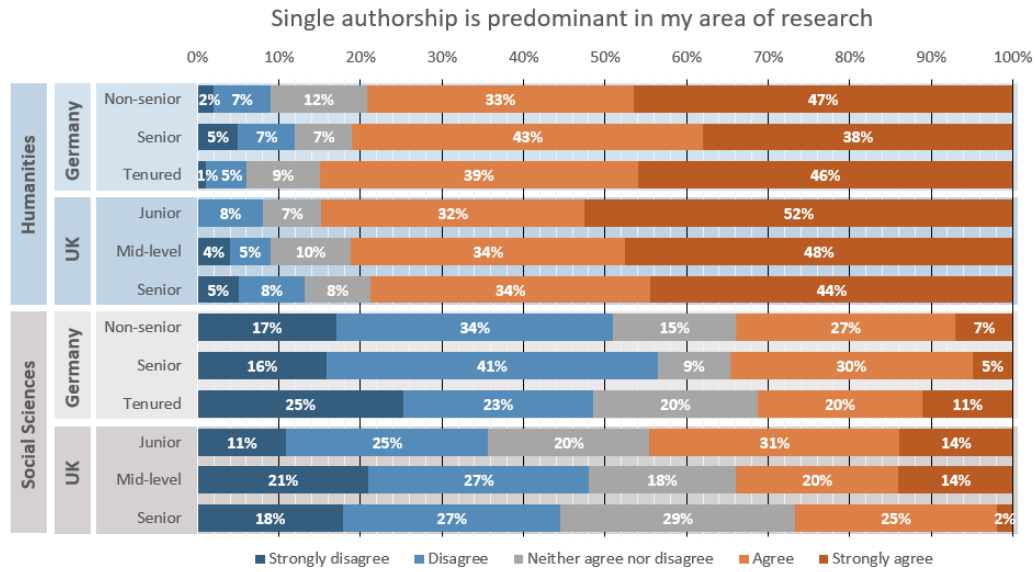


Figure 34: Predominance of single authorship.

This predominance cannot be generalised for monographs in opposition to articles, however. Though agreement with the statement that research can best be expressed with a monograph (figure 35) is generally higher in the humanities than it is in the social sciences, there are still high rates of disagreement among humanists as well. This is true in both Germany and the UK. More extreme are the results concerning the wish to focus on a monograph instead of further writing articles or contributions. Humanities scholars indeed indicate a tendency to reduce article output in order to focus on a monograph. Interestingly, there are only minor differences between career levels among UK humanists. The two figures in comparison indicate that the article as well as the contribution to an edited collection have justified places as media in the humanities, but the individual scholar is inclined to reduce these media in favour of monographs, if possible.

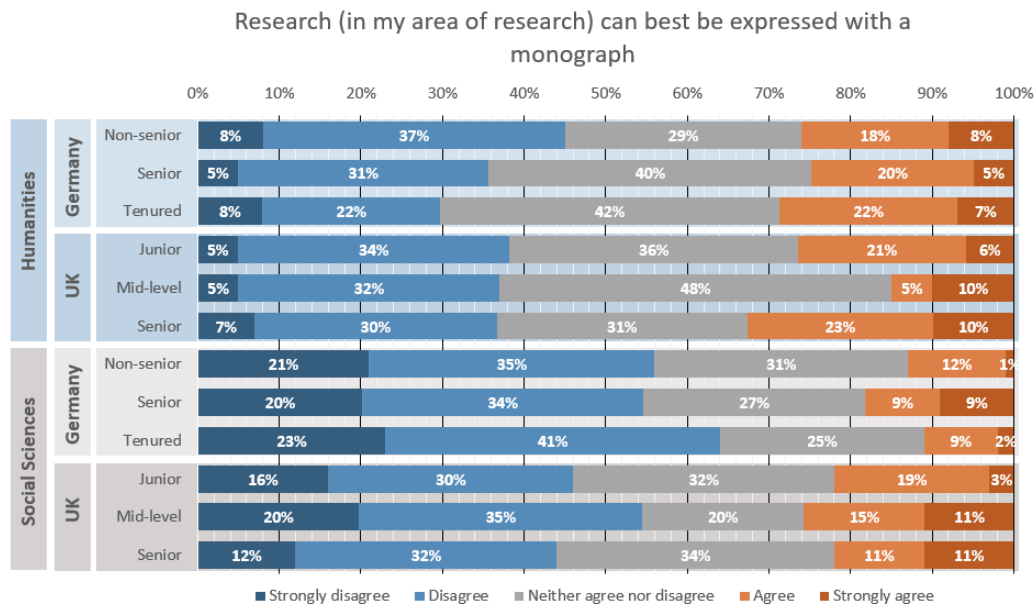


Figure 35: Monograph as the preferred format.

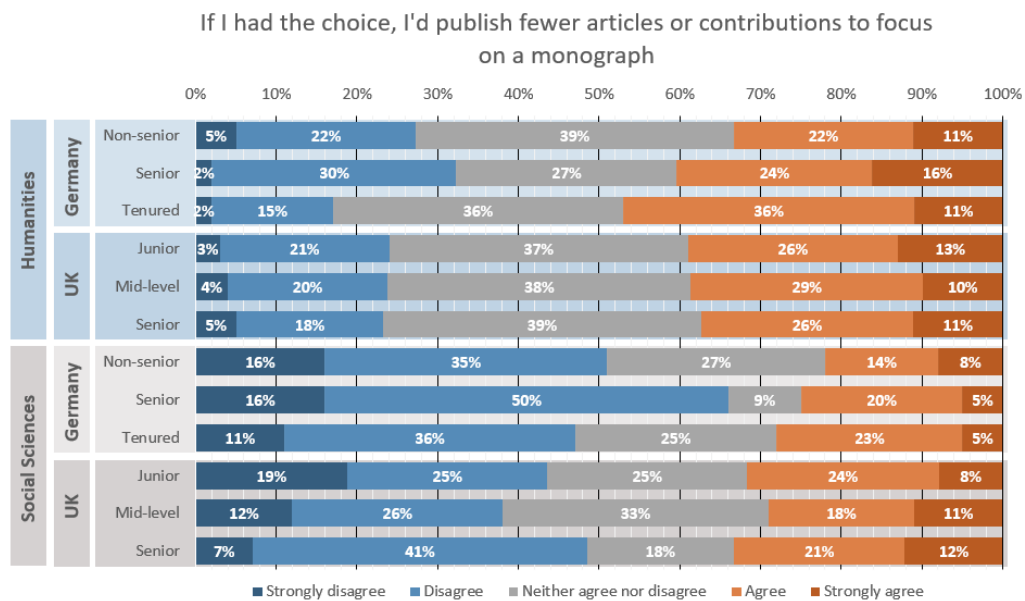


Figure 36: Preference of monograph over articles or contributions.

Figures 37 and 38 further indicate differences between disciplines: humanities scholars seem to be less knowledgeable about metrics than social scientists are. Disagreement with the statement about the knowledge of metrics is higher among humanists as well as among Germans, and also increases with seniority. In regard to format, the differences between social scientists and humanists is larger for journal articles,

especially in Germany. This points to a confirmation of the assumption that there is less engagement with metrics among the—as is often claimed—traditionalist German humanists than there is in the UK.

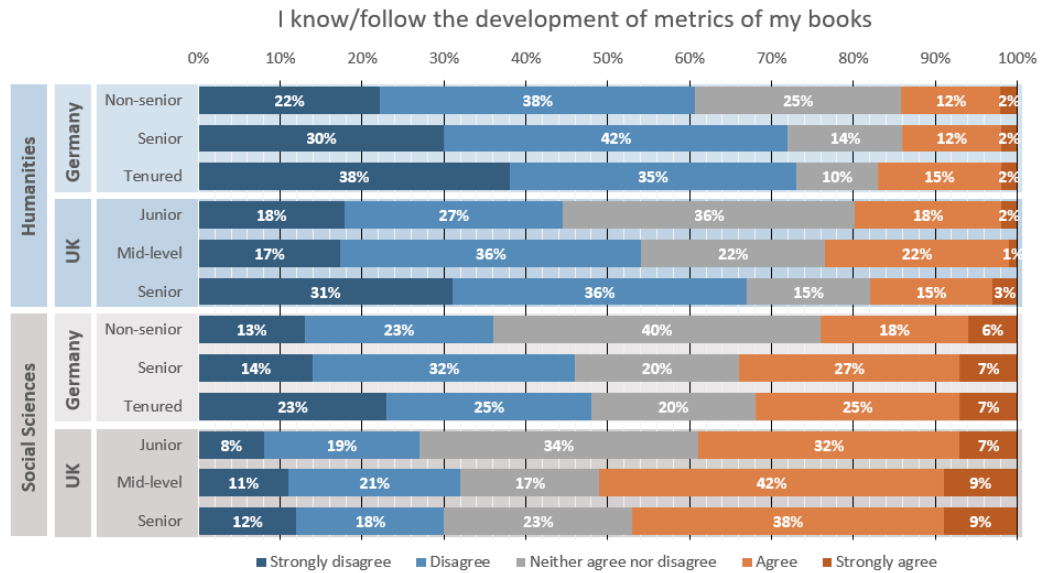


Figure 37: Knowledge about metrics of published monographs.

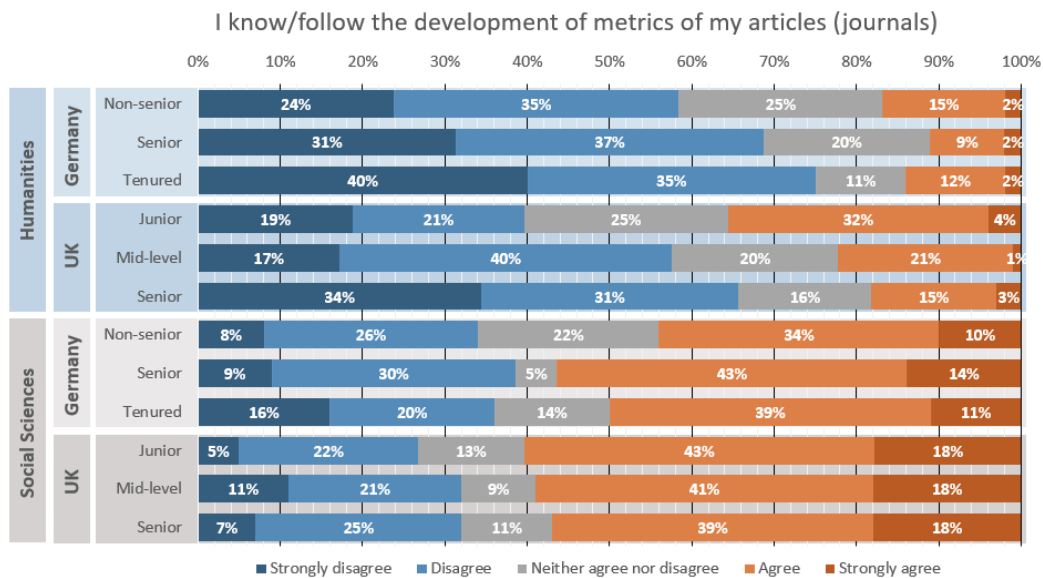


Figure 38: Knowledge about metrics of published articles.

Figure 39 further indicates assumptions about the connection between metrics and research. An overwhelming number of scholars, especially in the humanities (more than 75-90%), but also in the social sciences (50-70%), (strongly) disagrees with the statement that

their research can well be expressed in quantifiers or metrics. On the one hand, this confirms that research in the humanities seems to be ill-suited to being expressed in quantifiable terms only. On the other hand, this raises the question why, for at least a few, scholars do indeed follow their metrics (as was indicated above). Surely, there is a slight disconnection in the categories: the first statement is about articles and books, while here it is about research. Yet, since articles and books are representations of scholarship, it seems that scholars do not follow the developments of those metrics as a matter of understanding the perceived value of their scholarship by fellow scholars, but by understanding a form of use of publications. This is paradoxical and seems indicative of a disconnection of publication and scholarship.

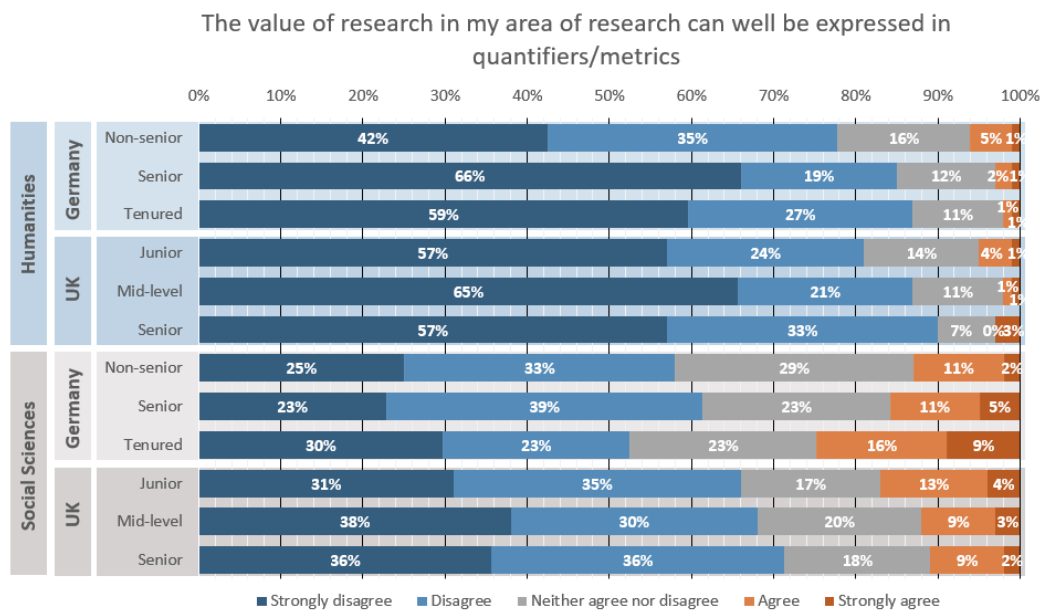


Figure 39: Expressiveness of the value of research in quantifiers/metrics.

The responses displayed in figure 40 indicate that there is a difference in the prevalence (or the awareness) of *salami slicing*: it seems to be less the case in Germany and also less so in the humanities. Especially in Germany, the awareness of such prevalence seems to be quite uncommon among tenured scholars.

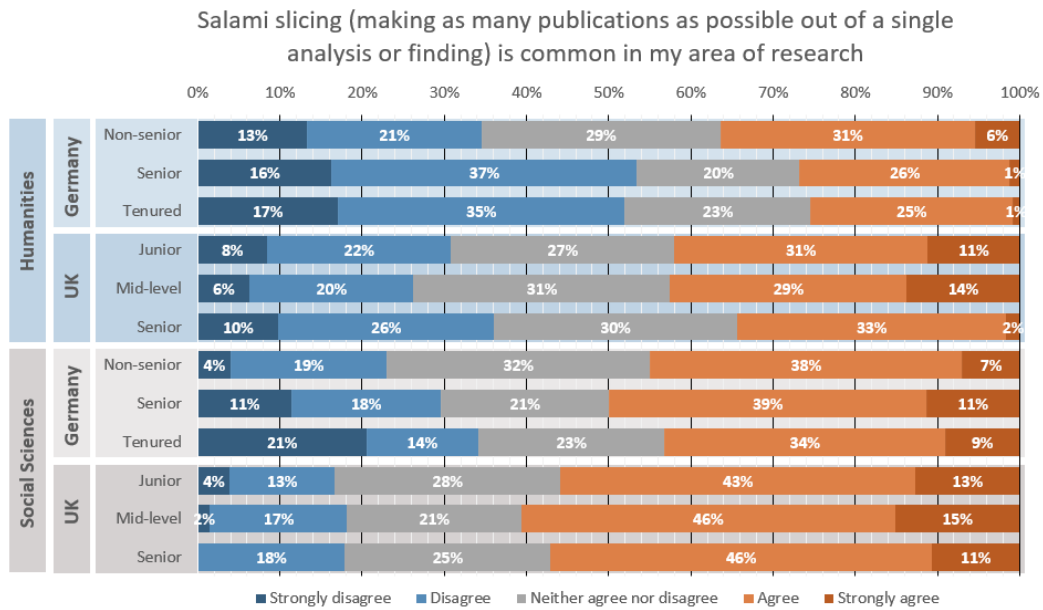


Figure 40: Pervasiveness of salami slicing.

Monograph-related Perceptions

There is a strong rejection of self-publishing services across disciplines and countries (figure 41), though the social scientists seem to be a bit less reluctant. This is accompanied by a high rate of missing information: many scholars seem either to not know about the availability of such services or unsure about engagement with it.

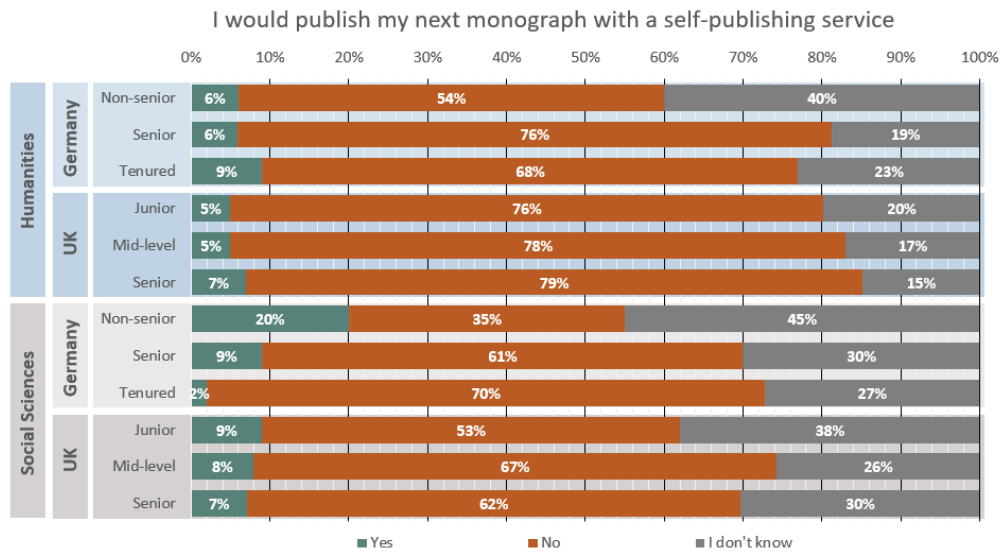


Figure 41: Optionality of self-publishing service.

As opposed to the rejection of self-publishing, there is a strong agreement with the statement that a publisher’s brand is an important indicator for quality while browsing through a bibliography (figure 42). Half of all scholars agree or strongly agree with this statement, with humanists in the UK and social scientists in Germany reaching agreement rates of up to 70%. This confirms assumptions about brands that are seen as heuristics.

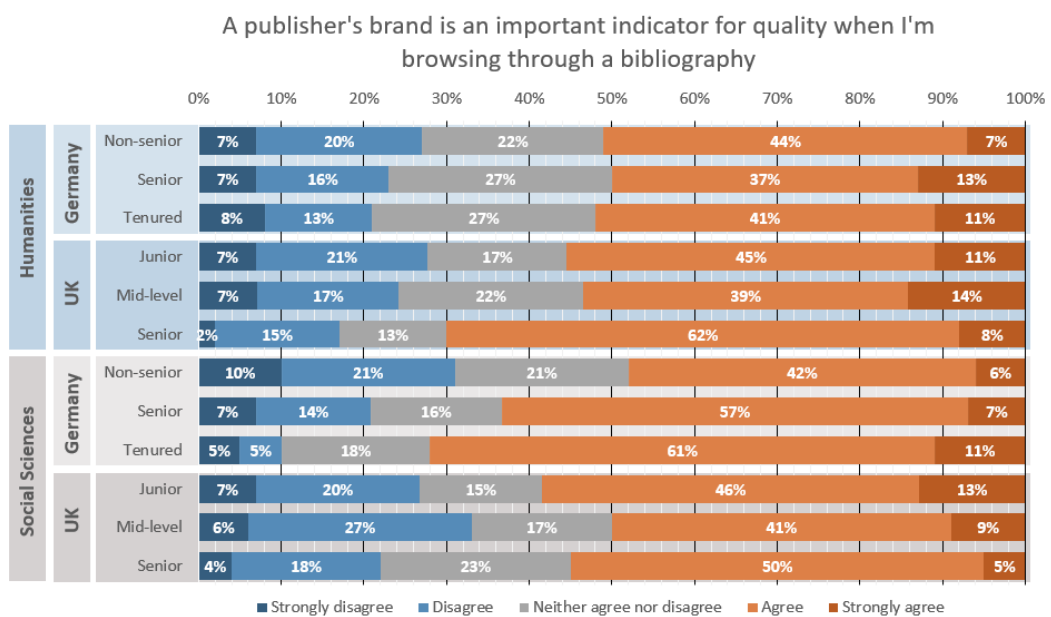


Figure 42: The publishing brand as heuristic for quality.

Figure 43 enquires about the usefulness of providing chapter downloads of monographs. This is a crucial aspect in praxis as many larger publishers offer such service, while other publishers tend to focus on the single PDF. This can have both technical reasons and reasons within the self-perception of humanities scholars who may see their monograph as a single, unified entity. The results suggest that a majority of scholars consider this to be a useful option. However, especially among established scholars in Germany, there is a tendency to rather reject this option.

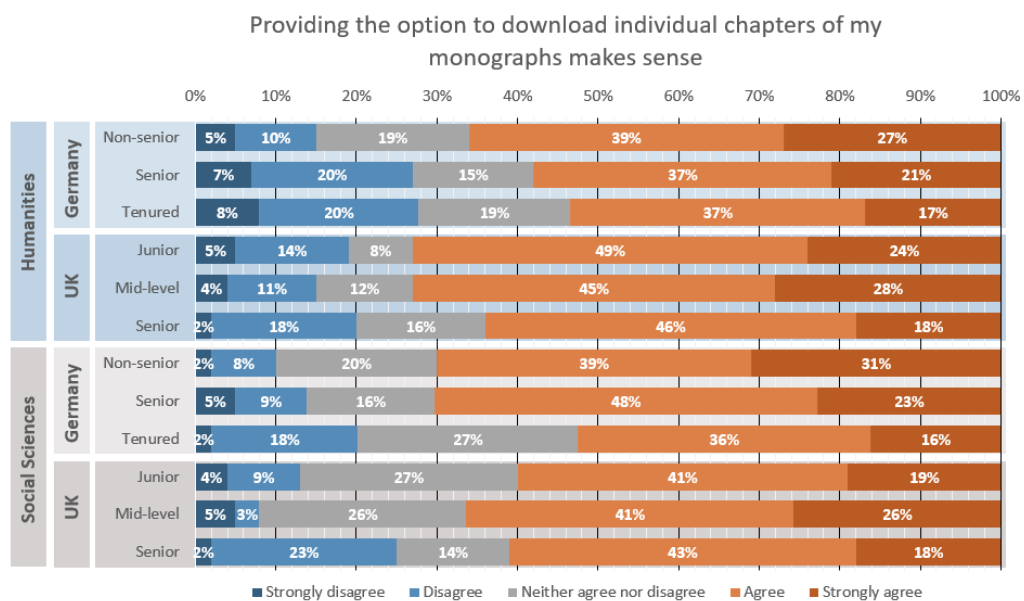


Figure 43: Sensibility of chapter downloads.

The figures 44 and 45 both relate to temporal aspects of publication processes. The importance here is given to the connection: the former statement connects a fast publication process to mere immediacy, while the latter connects an unhurried publication process to content improvement. Especially for the humanities, it becomes evident that an unhurried publication process focussing on content is more important than the rapid publication. Furthermore, there is a clear division between the humanities and social sciences. Though their tendencies are comparable, humanists reject rapid publication

processes more than social scientists do. In return, humanists show higher rates of importance for unhurried publication processes, and Germany more so than the UK.

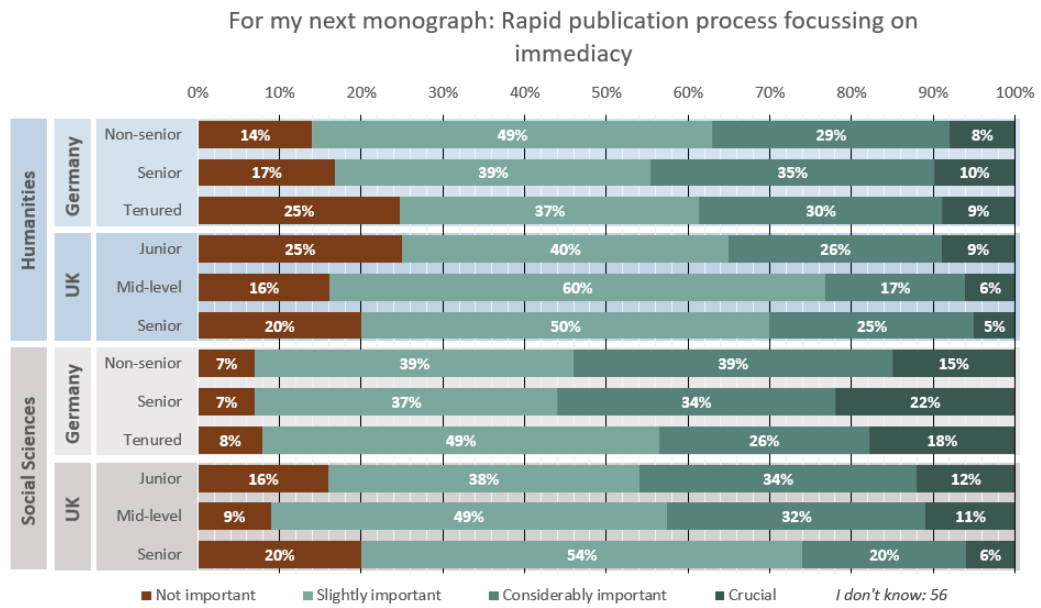


Figure 44: The importance of rapid publication processes.

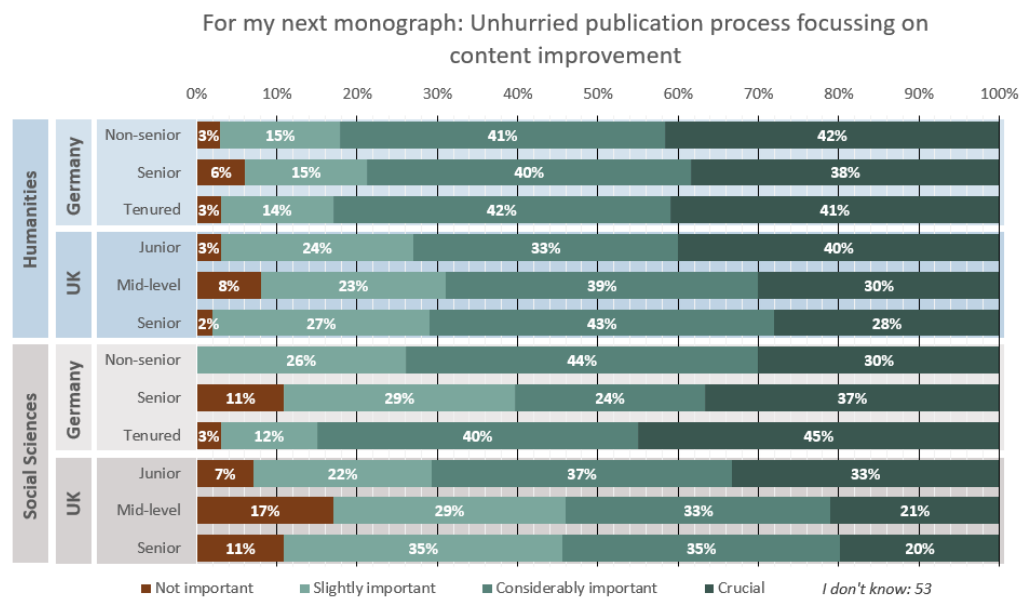


Figure 45: The importance of unhurried publication processes.

The necessity of the availability of a monograph in physical form in libraries is confirmed by the indicated agreement of—especially senior and tenured—humanities scholars (figure 46). Only a small fraction of humanists rejects a perceived importance of

the printed book on the library shelf. The social sciences show less concern here, while especially non-senior, junior, and mid-level scholars seem to see less of a need of such physical availability.

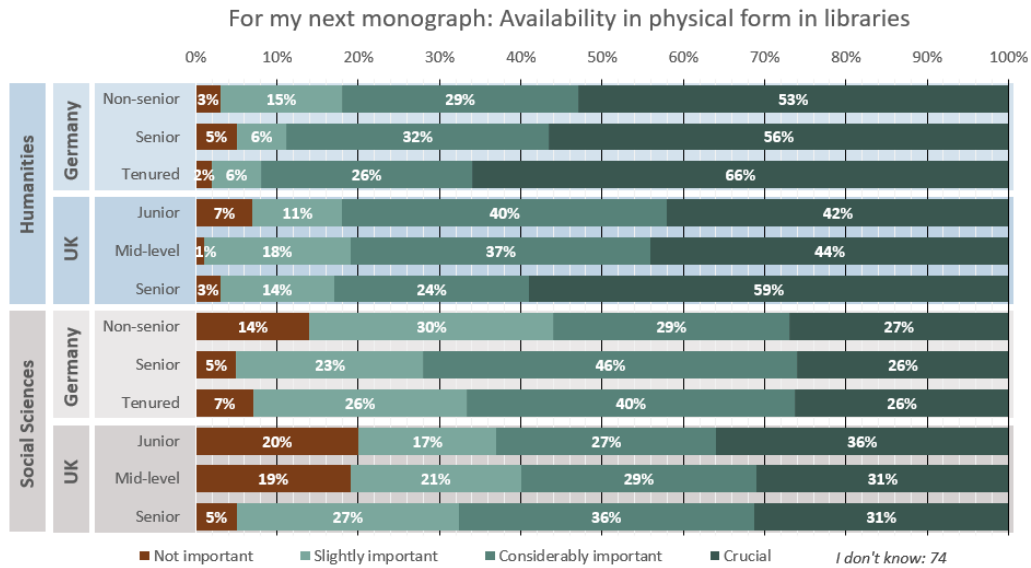


Figure 46: The importance of the availability of a monograph in physical form in libraries.

The item above is mirrored by figure 47 in that the item enquires about digital discoverability. There is wide-spread agreement that this is crucial or considerably important with the social sciences, showing even higher rates in these categories than the humanities do. The two figures compared show that, for humanists, especially in the earlier academic levels, digital discoverability is slightly more important than physical availability. For social scientists, the digital item seems to be even more important.

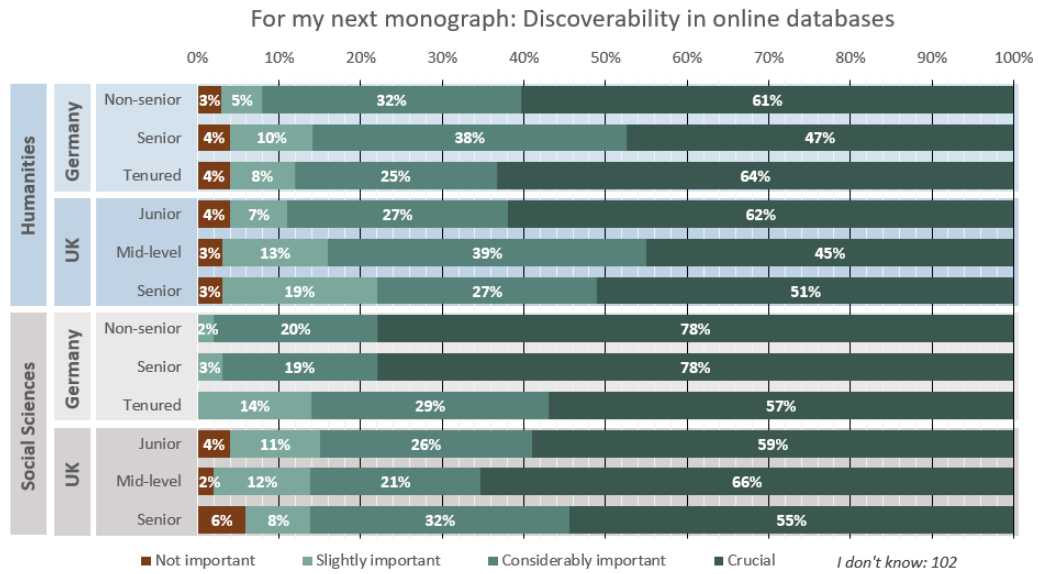


Figure 47: The importance of the discoverability of a monograph online.

Figure 48 offers a third statement in this row, enquiring about the importance of the availability of a monograph in bookshops. This item shows quite mixed results, with a tendency of slightly more agreement than disagreement among German scholars. These responses resonate that compared with online discoverability and physical availability in libraries indicated above, bookshops seem to be the least important places for circulation across disciplines.

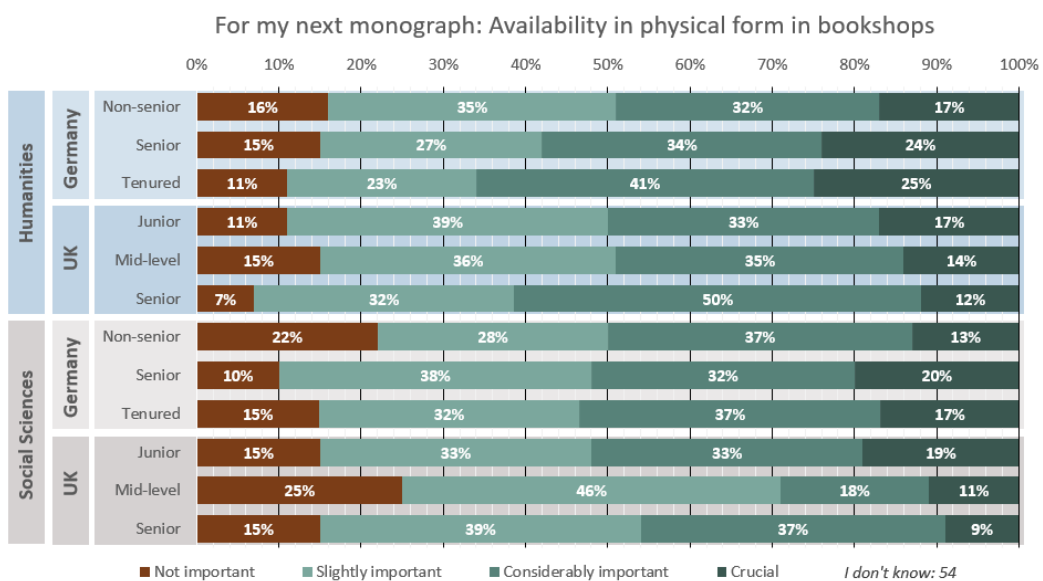


Figure 48: The importance of the availability of a monograph in bookshops.

The last item in this section enquires about the importance of publishing OA, which seems to result in strong opinions. Among humanists, there seems to be rather a rejection of OA, especially among more senior scholars. In the UK, 43% of senior humanists even claim OA to be not important at all (which is the highest rate about needs for future monographs in my survey overall). In the social sciences, the rejection of OA is also stronger than any rejection of the enquired items above; though less so in Germany among non-senior scholars. In the light of the strong pressure perceived regarding publishing OA, the differences between the two countries seem to be much more subtle as presented here in figure 49. This may indicate that the pressure perceived to publish OA may be disconnected from the general perception: the individual scholar may see value in OA, and therefore does not reject it entirely. All the while, she feels the pressure to publish OA which may be related to external factors as opposed to the innate value of openness.

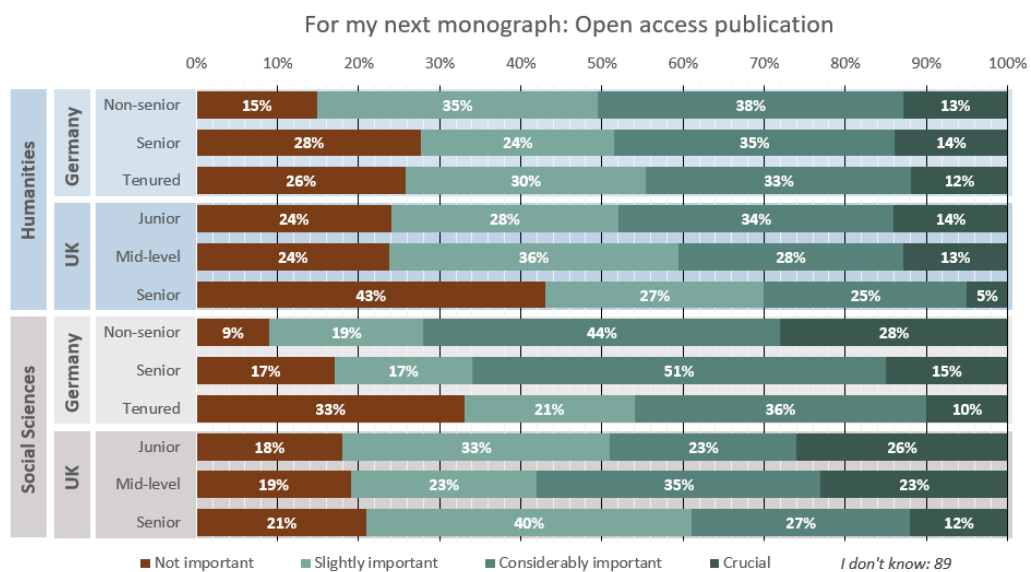


Figure 49: The importance to publish OA.

Discussion of Hypotheses

The analysis of publishing portfolios provides a fruitful basis for understanding authorship in the humanities. It provides knowledge about the question: how much has a scholar, in terms of different formats and on average, published at a relevant career stage? The portfolio exploration also indicates the languages used, which is particularly relevant to assessing German scholars, and how selected other publication modes are deployed. This knowledge is highly relevant and a contribution in itself as it gives a structured foundation for further assessing aspects of authorship and publishing practices.

The perceptions of scholars substantiate and differentiate this knowledge. As such, the hypothesis that single authorship is dominant in the humanities can be confirmed. The single author is not a long-held myth or a bibliographically undetected relict. Scholars themselves perceive this to be the basis of their work. However, the hypothesis that humanists prefer monographs over articles needs to be differentiated. The grounds for this statement cannot be provided by statistical analysis, the monograph as the towering mode of practice cannot unequivocally be confirmed. Other formats, particularly the article, are reliable modes of communication as well in the humanities—however, the way an article is constructed qualitatively may still be very different in comparison to the social sciences.

It can be confirmed that scholars in the humanities are less engaged in dealing with metrics: quantifiers of authorship play a minor role for humanists and the scholarship cannot as easily be represented in metrics. All items pertinent to this enquiry are a clear affirmation of the assumptions often found in contemporary literature and an important flag for future policy: it is not simply bibliometric nit-picking; scholarship in the humanities is not as easily represented with quantifiers.

The issue of the least publishable unit, expressed as *salami slicing*, is less clear: it seems to be less common in the humanities in relation to the social sciences, but it still is an issue. It seems relevant here to remember that this is an indication of the perception of scholars, not a clear-cut empirical statement about the pervasiveness of the practice itself. In other words, this is what scholars perceive to be true about the least publishable unit. Ultimately, however, it is a matter of more subtle characteristics to allow for a statement of how the least publishable unit looks like in the humanities concretely. What *is* the least publishable unit in philosophy, philology, or history: a sliced-up review; a minor argument; a hypothesis not further pursued?

Regarding the preferable requirements for future monographs, humanists have some indicative needs and some less characteristic ones. Firstly, humanities scholars generally reject the use of self-publishing services; they are not overly different than social scientists are in this respect, though. Secondly, major differences appear regarding the temporality of the publication process: the slower process is preferred with a focus on content improvement. Immediacy of the formal publication, therefore, seems not to be the indicative terms of choosing a publisher. And OA is not, either: it is not a crucial issue (but it is not altogether rejected either, with significant differences between Germany and the UK).

And lastly, it cannot be said that humanities scholars were more inclined towards the physical availability of monographs as opposed to digital discoverability. The physical copy in the library is more relevant for humanities scholars than it is for social scientists. Still, though, online discoverability in indices is indicated to be more important.

Overall, therefore, the data emphasise to rejection any claim that humanities scholars are to be seen as technologically ill-equipped or unintelligible. This can be seen throughout the survey and also within the perceptions and wishes for future monographs. The online

discoverability is a pertinent aspect for a future monograph as well. What can be rejected as relevant, however, is the physical availability in bookshops. To be sure, this may not hold true for the (potential) crossover book; but by and large, the bookshop is not a relevant site for the ordinary scholar's next monograph.

7.3 Conclusion

In this chapter, I proposed an inclusive definition of the humanities. I proceeded by highlighting essential characteristics of humanities scholarship which illustrate cultural aspects to which practices are bound. I also substantiated disciplinary differences with quantitative data about authorship and publishing practices. These show differences between the humanities and social sciences and provide data about statements that are often left unsubstantiated in existing literature. All of these aspects articulate characteristics of scholarship in the humanities, a culture of slowness, careful consideration, hermeneutic subjectification, and deliberateness. In short, something like a *culture of deliberation* that is underscored by the principles described above and perceived in practices of authorship and publishing. Ideally, practices are means with their ends rooted in the interest of enlightenment, substantively thick ends whose values are negotiated across longer periods of time (as opposed to the thin ends of purely instrumental practices).

The second half of the chapter aimed to account for aspects of self-understanding, especially those pertaining to authorship and publishing. The two sub-chapters in combination provide a comprehensive overview of what the humanities *can* be today: in principle and in the ways authorship and publishing are understood. Next to this dependence on categories and definitions, Marquard reminds us about something worthwhile: the humanities need neither epistemological rectification nor metaphysical justification. What the humanities need, above all, is more courage (Marquard, 2020b: 181).

And yet, the progress of rationality that instrumentalises the humanities enforces, as Horkheimer and Adorno say about philosophy, ‘a kind of intellectual Taylorism’ ([1944] 2002: 201). The courage that the humanities need may be a courage to defy such Taylorism and its inherent technocracy and scientism.

Beyond defining the community of scholars to be counted within the humanities based on discourse, it is easy to miss a fundamental function of scholarship: teaching. Teaching seems to be elusive. It is rarely inscribed in long-lasting artefacts and in the many disputes about excellence and impact, teaching always takes on the minor role. Only the genial monuments of scholarship have their teachings and lectures transcribed and published (therefore, crossing or defying the categories of teaching and research—of talking and publishing). Since the Humboldtian conception of the unity of teaching and research, however, the intricate connection the two bundles of practices (may) have is usually acknowledged. As Humboldt writes in around 1810, the scholar is not to teach the student—both are in collaboration to advance scholarship ([1810] 2017: 153). They form a discourse community which is to benefit all parties involved. In this sense, I recognise teaching to be vital for scholarship in the humanities—the discourse resulting from oral practices may be categorically more important than many a publication.

This thesis is not about teaching, but about publishing. Nevertheless, it cannot be stressed enough how the principles accounted for in this chapter—particularly the matters concerned with being human, being grounded in history, and the hermeneutics of understanding—are perhaps more profoundly, albeit less efficiently, embodied in teaching than in discursive practices of traditional, formal publishing. And this, the matter of efficiency, will be a focal point of the next chapter. From elusive ideals, I turn to culture principles explained in terms of formal rationality as efficient decision-making.

8 Culture Principles of Rationalisation: Instrumental Publishing and Alienation

In the preceding chapter, I outlined what the humanities are and referred to their culture as a culture of deliberation. This culture is in its ideal-type constituted by slow, considerate, and especially qualitative meaningful practices in regard to discourse. In this chapter, I mirror this culture with one of instrumentalized practices with thin ends, a culture to be explained by the Weberian categorisation underlying rationalisation. This rationalisation helps explain fundamental aspects of a managerialist culture of contemporary academia. A culture that enforces formal authorship as the epitome of achievement. Practices of publishing are instrumentalized to gain more (and more often) the advantage of such formal authorship.

I substantiate this argument by first explaining rationalisation. Secondly, I outline my main argument about rationalisation, publishing practices, and formal authorship in theoretic terms. Thirdly, I introduce the category of alienation which helps understand the issue of instrumentalization and its subjective perception as pressure.

This chapter provides the social theoretical explanation for the development that I showcased in chapters 4 and 5. It also lays out the conceptual and terminological fundament for the more abstract discussion of instrumentalization and publishing brands in the chapters 9 and 10.

8.1 Rationalisation, Rationalities, and Academia

Variations of Rationality

Means-end calculation is the process of dealing efficiently with scarcity; this is the essence of the common understanding of rationality. It is an efficiency in process that refutes or dismantles everything standing in its way. The history of social theoretical thought contains thick descriptions of varieties of rationality. It has been a central category for illuminating the essence of modernity and the progress of societies since at least, the industrial revolution. As the 'ability to exercise reason' (OED, 2020), rationality has been used since the seventeenth century. Most early social theorists have, implicitly or explicitly, contributed to conceptions of rationality.

Max Weber's conception is most nuanced and dominates social theory up until today. The analytic dichotomy in Weber's (pre-practice theoretical) conception is that of *substantive* and *formal* objective rationality (Weber, [1922] 1978: 85-86; Levine, 1981; Kalberg, 1980). Modernity's process of rationalisation is the development of formal rationality, a focus on formalistic procedures of instrumental means as ends, as opposed to substantive rationality with a focus on ultimate ends. The analytical dichotomy of substantive and formal allows for a methodological taxonomy of practices that are either normatively meaningful or mere factual calculation. These two modes are ideal-types in a Weberian sense: they are usually not actualised either/or; actualisations range gradually between the two.

The substance of ultimate ends has a culturally dependent normative character for rationality: actions, institutionalised as practices, are substantively rational insofar as they are judged by a set of fundamental values ascribed to them. These may be based on humanistic ideals, religion, social equality, or a similar belief system. They are ultimate

because they are negotiated over a long time so that the negotiation becomes part of culture; its practices embody the values established in all their ambiguity and contingency. Outlier actions cannot simply, reflexively change values just as a relative autonomy of culture suggests. Such values provide meaning, coherence, and a normative frame within which practices are evaluated.

Formal rationality opposes such a primary focus on ends and lacks normative character. Practices deemed formally rational are indifferent to values *per se*, they are reduced to factual instrumentalism and efficiency. Formal rational practices are explicit in their nature as ends in means-calculation. Purposes make place for further means so that a teleology of ends as means comes to life: ends become mere means themselves. Alongside such replacement, the actualisation of formal rational practices may evoke a sense of irrationality on the personal level, as a matter of personal hermeneutics where life world and system become disengaged in the disputed Habermasian sense (Habermas, 1987: 153–197). Besides this, Habermas' conception of rational discourse as well as his discussion of Weber's rationalisation more generally are noteworthy for understanding Weber in the context of other critical theorists (1984: 25; 143-272).

Douglass refers to the categorical irrationality that Weber develops on the grounds of disenchantment and structural differentiation as mindless instrumentalism in what is known as an iron cage. Increasing formalism and calculability leave society empty and disconnected from ultimate values. Eventually, mindless instrumentalism is the guiding principle of practices:

all that remained was activity for activity's sake—activity the people involved often found difficult to justify. But paradoxically, that did not seem to affect the intensity of their performance at all (Douglass, 2018: 58).

The encroaching bureaucracy in every branch of societal organisation can be—but is not necessarily—descriptively decisive for this formalism. For Weber, ‘[t]he development of modern forms of organization in all fields is nothing less than identical with the development and continual spread of bureaucratic administration’ ([1922] 1978: 223). Bureaucratic formalism means impersonal interaction, matter-of-factness, and treating decision-making based on rules. Note the sense of comparability that is characteristic to all these aspects. The development of such devoted rules leads to a discouragement of regulating a ‘matter by individual commands given for each case, but only to regulate the matter abstractly’ (Weber, [1922] 1978: 958).

Formal Rationality, Thin Ends, and Academia

Rationalisations’ focus on means and ends, and the shift from one to the other, becomes even clearer if the meaning of rational is dissected not just as being efficient, but also as legitimate (where irrational is illegitimate) belief. In the categorical mode of formal rationality, a practice is legitimated because of its formal performance: the practice as a means suffices to legitimate itself since belief in the performance precludes results. In the mode of substantive rationality, legitimation takes place *because* of those results: the purposeful substance of ultimate ends. It is this division and the force of formal and decline of substantive rationality that is often implied in accounts of rationalisation of Western society. Observe how Evans, in his analysis of discursive practices of bio ethics, repeatedly calls this shift categorically a *thinning* (2002: chapter 1).

This conception is particularly striking: rationalisation as a thinning of ends. I adapt it as a descriptive element for my discussion of alienation below. This thinness is the fading away of ends in favour of (rationalistic) means (the very shift described above)—the

purposes of institutionalised actions are thinning towards efficiency-purporting replacements (Evans, 2002: 14). This repeats high rates of predictability and calculability that become key characteristics of formal rationality in the late Weber (Collins, 1980: 927). Such efficiency-purporting replacement is picked-up again in the discussion of the teleology of instrumental practices in 5.3.

The conception of formal rationality and rationalisation has been applied to higher education and academia in numerous studies. Above all, Weber himself put forward the rationalised position of the scholar in contemporary academia, a subject that calculates and is located in an infinite progress ([1917] 2015: 19). This account is influenced by the notion of *Wissenschaft* in the sense of the sciences. The ingrained rationalisation is, thus, a technocracy building on formal rationality that, today, also encroaches within the humanities.

Among the more recent studies, Babintsev et al. decry the instrumentalization and Westernisation of higher education in Russia (2015). After the fall of the Soviet Union, administration was trapped into practices adhering to formal rational logics which counteract values inherent in the traditional institutions of higher education of Russia, the authors claim. With a similar focus on bureaucracy and drawing heavily on Habermas, Murphy traces the notion of formal rationality from Weberian times to today's academia, particularly in the UK (2009). Strong similarities appear where accountability and efficient management have deteriorated the foundations of academic values. A notable aspect of this assessment is, indeed, that capitalism and its competitive imperative are minor figures alongside bureaucracy.

Levidow likens the practice of impact assessments as a central instrument of formal rationality with its demoralised and reifying nature (1992). He questions whose rationality it is that is instrumentalised with such a tool, since it is normatively value-neutral and seems

irrespective of cultural substance. Moreover, the link between rationalisation and academia is also found in praxis, such that increasing rates of tertiary education in the wake of late modernity enforced more rationalisation in respective countries (Schofer et al., 2020). Parker and Jary make a less differentiated, though not less forceful critique of academia in the UK (1995). Managerialism focussed on returns of investments is seen as being at the forefront of progress; efficient bureaucracy is a result of capitalistic and especially neoliberal policy decisions. A trickle-down effect of devaluing meaningful practices and instrumentalising academic work has implications on subjectifications. Ultimately, unapologetic to Bourdieu's *Homo Academicus*, the authors stress that the intellectual purpose must remain at the heart of practices despite efforts to advance accountability.

Münch is among the most fervent critics of rationalisation in German academia (2007, 2008, 2011). He provides a substantial overview of the working of German academia and the formal rationalistic impact of NPM, in line with prior works on the field of academia in a national context, such as in the US (Platt and Parsons, 1973), France (Bourdieu, 1975, 1988), UK (Brink, 2018; Collini, 2012; Frank et al., 2019; Sperlinger et al., 2018), or Germany (Leendertz, 2020; Meier and Schimank, 2009). With a system theoretic approach, Franzen similarly argues that processes of democratisation inevitably lead to forms of standardisation (2014: 376) that can be explained as a development of rationalisation in the wake of NPM. These studies are key to understand how NPM produces or strengthens culture principles of formal rationality, for which Münch repeatedly also draws comparisons to the UK or the US.

As a conflict theorist, Münch's analyses are heavily drawn towards Bourdieu's field theory and arising conflicts of power and privilege based on material interests and forms of capital (particularly devised and expanded in the works: Bourdieu, 1984, 1986, 1988, 1998). Münch studies a thinning of ends in academia through a rationalisation driven by NPM.

With the conception of Meyer and Rowan (1977) explained above, Münch argues that this rationalisation is but a myth which builds upon a separation of practices within an organisation from the image a rationalised organisation presents to an outside observer (2011: 278). In other words, NPM only creates the illusion of efficiency; internal to established organisations, it actually clashes with given hierarchies as well as Germany's traditional practices of bureaucracy and scholarship. Instead of working with this notion of a rational myth, and since I am not focussed on organisations but on authors, I work with Reckwitz's notion of the hybrid subject in my analysis of Germany's rationalisation. I do, however, arrive at a similar conclusion to Münch, as shown in chapter 6.

Of particular relevance for my analysis are Münch's studies of the performance-oriented allocation of research funds (e.g.: 2008: 118). This is supposed to provide the substratum for increasing competition among scholars: a market for funding based on ideas. Along the blind processes of peer reviewing and metricised practices, allocation shifts from basic funding and trust to performative funding, and from input to output-based reward. In the sense of a neoliberal governmentality, universities become subject to an all-encompassing competition while scholars, subsequently, become employees in this competitive pursuit of the allocation of funds. This being a *free* market, hierarchies are supposed to be dismantled and the working of privileges reduced to bring about a more level playing field such that equality governs who may acquire funds. Münch dissects this as a Bourdieuan *illusio* of contemporary academia (2011: 276–287); *illusio*—the belief in a game and its righteousness, without substantive intelligibility of the working principles of this game, where a game is a bundle of practices geared towards a certain end (Bourdieu, 1998: 76). The missing intelligibility at work in the *illusio* is blind towards social closure, as Münch repeatedly excavates in the practices of German academia.

The conception of social closure is important for understanding the creation of artificial social boundaries. Social closure is widely established within sociology and first formulated by Weber in §6 of *Political Communities* ([1922] 1978: 926–939). Communities can become closed to outsiders because of a variety of issues such as prestige, status in a hierarchy, race, or material wealth. The community encloses itself by closing relationships within the community based on the marker of distinction and, thus, defining the social other as outsiders. Reproduction of hierarchy and status are, then, secured against outsiders. And such ‘stratification by status goes hand in hand with a monopolization of ideal and material goods or opportunities’ (Weber, [1922] 1978: 935). That is, in the context of NPM, obliviousness of social closure fundamentally hinders equal chances in a free market and instead enforces a gradual monopolisation.

Münch finds such a monopolisation resulting from social closure—with a tendency to overemphasise financial advantages over culture-embodying practices—at various instances in German academia where NPM sets the markers of achievement in artificial principles or mechanisms. Among such are rankings and metric-based performance measurement (Münch, 2007: 73–160), overemphasising a rhetoric of marketable reputation (such as of *world-leading* or *excellence*) (Münch, 2008: part 2), or the creation of an audit university where strategic, external management of quality replaces the autonomous conduct of scholarship, research, and teaching (Münch, 2011: 94–123). These principles and mechanisms are, in the terminology I established above, forms of a thinning of ends: practices of scholarship become means for externalised ends as required because of efficiency in institutional decision-making processes of NPM. Münch expresses this when he declares that the ‘modernisation of the tools for quality assurance’ led to ‘a shift of objectives, a replacement of academic with managerial quality assurance’ (2011: 94; translation by the author).

Though Münch's claims about social closure in relation to material conditions are valuable resources, at times, he fails to articulate the culture dimension of those practices that lead to the enclosure. Peer review and output-based performance evaluation create the image of being geared towards traditional culture principles of scholarly communities and ideational achievement. It is *as if* the force of the better scholarly argument matters, while, in fact, it is just this *as if* that is at the root of the *illusio* dissected by Münch. This is especially true for publishing practices where, of course, no manuscript will be published without content so that, abstractly, every publication retains the image of contributing to discourse, the image of existing to facilitate communication between author and reader. Surely, something is provided to be communicated, but the *as if* hides the many formal rationalistic principles shaping practices.

Thus, it is in this obliviousness to efficiency-inducing principles coming to bear on publishing practices where Münch's analysis falls short. He suggests that the problems of NPM, particularly in the humanities and social science, can be alleviated through formal publications: 'research performance would be measured by publication output per institutional equipment [size] alone' (Münch, 2007: 88; translation by the author). He, thus, neglects the possibility of strategizing or gaming publishing performance in the first place. I aim to clarify this gap by shifting the focus from mechanistic solutionism to culture principles.

The Culture of Excellence

Excellence is another key theme for the articulation of managerialism culture, particularly semantically. Excellence means surpassing the average, the good, and, in principle, also the better. It means not standing still. It is, for institutionalised practices the

ever-unfolding objective of achieving more: '[w]hatever the activity is, it must, we are constantly told, improve at a certain rate' (Collini, 2012: 109). Improving allows for interpretation, though. That which is to improve may need to become more, better, or faster. Above all, though, there needs to be a substance to be judged: that which needs to become excellent.

In the pursuit of institutionalising excellence, the objective has taken on a life on its own. Excellence could denote a category that refers to those entities that reach a certain quality, thus, being a meaningful, substantive category in terms of accepted quality criteria. Substantive excellence would be possible, so to say; it would denote that there is an excellent feature, that within a category of scholarship and judged by scholars of this area, the quality of a contribution to discourse substantively surpasses other contributions. Judgement in this respect is about the idea 'that quality is a matter of exceeding a certain bar', as Brink puts it (2018: 111). Determining such substantive excellence would be a matter of peer assessment of content, individually and qualitatively. But this is not the referred-to category in the rhetoric of excellence. Take, for instance, the issue of temporality in excellence: present excellence always needs to be surpassed so that acknowledgement of the fact of good quality immediately equals acknowledgement of the challenge to be even better. The rhetoricated excellence forces competition on the all-encompassing terms of a single market, to always outsmart the other, and to deploy all means to (appear to) surpass the other's quality. Brink puts this ambiguity in the different potential meanings of excellence in relation to quality as such:

[e]xcellence is about a relationship—typically a competitive relationship—with others. Quality is about our relationship with ourselves. We cannot all have excellence, but in principle we can all have quality (Brink, 2018: 189).

This seems to suggest that it is not substantive quality *per se* that is demanded.

Relative excellence is, and this excellence is a matter of category gaming: it is empty as such and becomes meaningful only through further attribution: ‘excellence has the singular advantage of being entirely meaningless, or to put it more precisely, non-referential’ (Readings, 1999: 22). Despite this nature, excellence is applied in manifold ways in Western academia such that it acquired a meaning on its own. A meaning that is certainly not connected to or does not emerge out of the conduct of scholarship, as Lamont shows (2009: chapter 5). It rather is a category external to scholarship, one that relates to and is fostered by managerialism. Because of this, a well-intentioned perspective on the discourse of excellence fails to differentiate where excellence as substantive quality and excellence as a means of social closure blur. Indeed, excellence does not tell much about the research and scholarship conducted, but about those who fund the quest for excellence (Stilgoe, 2014). It is, in this critical perspective, an ideologizing rhetoric (Moore et al., 2016). In other words, the discursive practices centring on excellence carry meaning into the social spheres of academia that has a tendentious, distorting impact by being geared to economic, neoliberal principles.

Quality scholarship is not enough according to those principles, excellence is the objective and universities manage their resources accordingly. Whether this substance—scholarship conducted—can be measured efficiently in a larger cross-institutional, cross-disciplinary context is not a concern in this objective. If this were the case, substantive quality would be the desired category: scholarship that is good, above all, in respect to itself. Discourse communities are very well suited to judge this. Each university and discipline, and even each scholar within her focussed area of enquiry, would strive for their own mission, their own understanding of the value of good scholarship. Instead, what is being employed is the standardisation of this understanding of value in order for it to be

efficiently comparable: not judged, but measured; and not locally where histories, cultures, and media representations of ideas under scrutiny in the humanities matter, but internationally; measured not as an achievement in its own right, but as an instance that allows for comparison; and measured not substantially, but in terms of comparable outputs, risking putting quantity above quality. This makes the matter of efficient comparison a matter of faster futures. Constant improvement, as Collini refers to, does not mean maintaining what is good, but always striving for better, for more in terms of something abstractly other.

Yet, this hardly bears a connection to those objects and their principles of exploration that laid grounds for the importance of humanities scholarship in the first place: the local histories, cultures, and media representations of ideas. Indeed, it appears to be the case that, as analysed by Readings two decades ago, contemporary excellent scholarship is intertwined more with capitalism than with rational discourse and scholarship's culture ideals. This conjures a decisive positioning for the connection of humanities scholarship and capital in the confrontational analogy expressed by Collini:

rather than saying that extending human understanding is valuable because it provides the means to prosperity we should surely say that one of the reasons prosperity is valuable is because it provides the wherewithal to extend human understanding (2012: 111).

Because scholarship in the humanities does not pay a direct dividend and human understanding is discounted in favour of short-term prosperity, the quest for excellence reduces the humanities to a workshop of productive understanding that comes in units to be counted and compared. The idea of a university has, as Readings puts it, 'now lost all content' where excellence denotes the boundaries of a field that 'requires [...] for activity to take place, and [in which] the empty notion of excellence refers to nothing other than the optimal input/output ratio in matters of information' (1999: 39). The idea of a university is

said to be no longer home to the foundations of scholarship, but ‘a point of capital’s self-knowledge, of capital’s ability not just to manage risk or diversity but to extract a surplus value’ (Readings, 1999: 40). The management of improvement lost touch with the substance that was to be improved by the quest of excellence.

Eve, though being a strong advocate for improved material structures of discourse in the humanities, discusses the ‘university as a service industry’ (2014: 63). This opposes the possibility of the *community of dissensus* as conceived by Readings. Such a community that

presupposes nothing in common, would not be dedicated either to the project of a full self-understanding (autonomy) or to a communicational consensus as to the nature of its unity. Rather, it would seek to make its heteronomy, its differences, more complex (Readings, 1999: 190).

Readings draws on Levinas’ intersubjective ontology here which provides the philosophical key for what may be ideal culture principles of scholarship in the humanities. Any strategizing is likely to disturb this communicative community as external service demands or internal career requirements affect what and how something is to be communicated, e.g. practices of publishing. Miller discusses this and argues, contra the university-as-a-service, that the humanities is not ‘an assembly of programs teaching [...] communication skills’ (1995: 132). Dissensus requires an internal openness for external otherness, an existential otherness that is reinforced where demands of democratisation demand all to be open to other voices. These are expressions of the human mind and as they are never exhaustively explored, ‘we cannot emancipate ourselves from our dependency on others’ (Readings, 1999: 190), since independency presupposes unequivocal knowledge about fixed boundaries of an us (or an I) and some otherness. Dissensus does not denote a temporal step towards eventual consensus. Dissensus presupposes that scholarly discourse practices allow for disagreement, its complexity and chaotic

incompleteness, and to make discourse ‘the locus of work toward that democracy to come which is the horizon of our calling’ (Miller, 1995: 137).

This philosophical argument optimally emphasises culture principles of humanities scholarship vis-à-vis excellence and contemporary academia. Such a community of dissensus does not, of course, mean that the humanities shall be given money without end to let them do whatever they wish. But if democracy and the paradigmatic openness to explore existential otherness in rational discourse fare as ultimate ends, academia requires substantive rationality that balances efficiency with these ends in the process of decision-making. The Weberian formal rationality claims this balancing becomes abandoned so that efficiency and the formal rule govern institutional settings. This is evident where efficient decision-making and institutional agendas impact authorship, which helps us understand why scholars publish the way they do, as I explain in the following sub-chapter.

8.2 Formal Rationality, Publishing Practices, and Formal Authorship

The modes of objective rationality explained above provide the frame for the empirical situation of my study. The rationalistic culture principles of contemporary institutional academia are articulated by publishing practices and their instrumentalization to yield formal authorship. These can be considered the formal rationalistic terms of authorship and publishing. This is the theoretic core of my argument.

Publishing Practices and Variations of Commodity and Currency

Formal authorship has become the primary means of legitimation, of proving one’s productivity. This is central for understanding why publishing practices are constituted the

way they are. Scholars publish as they require legitimate evidence for their scholarly labour—representations of their thinking. The formal rationality inherent in contemporary academia suggests that formal authorship is the optimal means for this legitimation. On the one hand, the *traditional* publication that establishes formal authorship, thus, has an added value which is not primarily bound to discourse. On the other hand, other forms of contributing to discourses such as through teaching or informal modes such as blogging, public engagement, or speaking at conferences are side-lined by the *traditional* publication.

Traditional by no means refers to *as it has been decades or centuries ago*. As I work out in the following section, *traditional* refers to the *illusio*. It is a belief in the righteousness of some forms of publications above others. The informal mode of sharing a manuscript does not count, not to speak of doing scholarship itself—practices such as thinking, making notes, or discussing arguments with colleagues. Formal authorship is superior to all other practices of communication or scholarship so that it seems to be the existentialist precondition of being a scholar. And it is because of this that publishing practices are instrumentalised to purposes external of discourse.

On the one hand, this can be explained with the terminology elaborated above. Where efficient decision-making depends on formal authorship, a thinning of ends in publishing practices can be witnessed as they are repurposed to focus more and more on formal authorship instead of on discourse. The suggested ultimate end of humanities scholarship—an interest in enlightenment, wider ideals of discourse, the culture of deliberation—cease to be guiding principles of discourse practices.

On the other hand, a critical theoretical terminology can be deployed to illustrate this shift in more radical terms. Think of the transcendence of commodification: a publication comprising of humanities scholarship in text appears to be a trivial thing. Its authorship is a reference in discourse. It has a use value in the communication of scholarship: by being

published, text is made visible to an audience which aspires to understand that scholarship. 'But, as soon as it steps forth as a commodity, it is changed into something transcendent' (Marx, [1867] 1906: 82). The publication, suddenly, is in competition with all other publications on terms of exchange in a job market, and no longer on terms of use in discourse. The dissemination of ideas from author to reader and its potential competition of ideas *after* dissemination is substituted to the competition of ways of dissemination *eo ipso*.

The commodification means a shift of the locus of competition from scholarship to its formal form as referenced in authorship. Non-discursive markers of distinction take over the role normatively assigned to scholarly discourse in flagging interest to an audience, in making visible by discussion that this is a valuable contribution. Rosa laments the 'mad run rush for more publications' (2010: 55), which opposes a Habermasian rational discourse (Habermas, 1984: 25): scholars rush for more and more publications instead of the deliberate creation of better arguments. The reason for this is just that the value of scholarship appears to be found in the exchange value, which is attributable to authorship as a commodity form. Here, it means a predominance of the symbolic over the discursive, a 'mystical character' (Marx, [1867] 1906: 82) that does not originate in use value: the exchange value of authorship as a commodity form dominates over the use value of scholarship in discourse. This exchange does not refer to the exchange of scholarship; it refers to the added value that authorship gains in the commodity market of jobs, funding, or reputation.

Existing, non-critical theory literature often refers to currency instead of to commodity. Gump traces the context and value of university presses and concludes a circular dynamic between university, publishing house, and scholars, a dynamic where the '[p]ublication became a *currency* for the new academic professionals, for whom prestige,

largely through scholarly excellence, was highly sought after' (2006: 75; emphasis added).

Cronin writes that '[a]uthorship [...] is the undisputed *coin* of the realm in academia' (Cronin: 559; emphasis added). Bennett and Taylor discuss the purpose of authorship, among which is the

[c]reation of *currency* for: academic appointment, promotion and research funding; entry to professional bodies (Bennett and Taylor, 2003: 264; emphasis added).

Currency or *coin* relate to symbolic capital here, both semantically and in its social function (I discuss this original Bourdieuan category in chapter 10). What comes through in the idiom of *currency* is the same as what I referred to as the commodity form. The publication and its corresponding authorship are valuable not just for discourse—or, to act as devil's advocate: increasingly less important for it—but are so also for external use such as in the job market, in institutional requirements of accountability, or similar situations.

Furthermore, Eve also discusses a commodity form of scholarship in his outline of an OA future in the humanities. Though he differentiates normative functions and practical appropriations of humanities scholarship and its values, as mentioned already, he unfolds a progressive agenda which may eventually see 'the university as a service industry that can provide training in methods of reading, understanding and (re-)producing such material' (Eve, 2014: 63). This surely seems to be a justification for public investment in humanities scholarship in the short run. However, it reifies an instrumental appropriation of scholarship which can usher in a whole range of issues such as public accountability and mechanistic legitimation of scholarly work. Eve's notion of OA is connected to such a service-function and its principle of the commodity form. In this respect, OA seeks to be a solution that is built on a suspicious assumption, a matter that may lead to a misappropriation of OA as I argue similarly elsewhere (Knöchelmann, forthcoming; such

misappropriations are aimed to be remedied, as is visible with the Radical Open Access projects or project *Copim*; see: Moore and Adema, 2020).

As both the commodity form and the semantic of *currency* suggest, authorship as something externally valuable has to do with competition and market which I explain below. Before this, I briefly expand on the meaning of formal authorship.

The Formal Form of Authorship

Among many other things, authorship can be a function in discourse in a Foucauldian sense (Foucault, 1979); it can be a community-constructed social reference (Ramdarshan Bold, 2018); it may denote being ‘entitled to some portion of the credit for writing the manuscript’ (Borenstein and Shamoo, 2015: 268); it may be a point of reference in the construction of corpora of knowledge (Hyland, 1999); it can also be a reference in a politicised linearity of names with indifference to contributions as is common in the *team sciences* (Falk-Krzesinski et al., 2011) of scientific disciplines (this politicisation and questionable contribution is exemplified by the discussion of what constitutes authorship in the sciences; see: Franck, 1999; Hundley et al., 2013; Siegel, 2008). Such scientific vanity fair is much less pertinent to the humanities as the ‘cogency of a substantial piece of work seems more closely bound up with the individual voice of the author’ (Collini, 2012: 75), so that the politicisation takes place within the subject formation instead of in collectives. Lastly and pragmatically, authorship can simply be the name of a person on a publication, being a material referent in terms of copyright, inventorship, or ideational ownership.

I propose a slightly different focus on authorship, one that aims to capture the issues studied in this thesis—and indeed, this focus is a result of this study, not a normative conception. This focus differentiates between discourse and subject: authorship always

denotes the connection of text to its originator *and* the formal reference of artefact to individual. The former is subject to discourse or content analysis; the latter is thematic for my analysis since it implies the political, strategic, or simply career decision relations that are important in the context of the imperative to publish. As cannot be emphasised enough, content does play a role and individuals do wish to communicate something in their texts. But my argument rests upon the notion that the systemic motivation of publishing practices shifts from discourse towards subject-focus, meaning that the latter relation becomes more important.

This latter relation rests on the *formal form* to which authorship refers. The practices of publishing centre on the transformation of informal text into formal publication—the practice of making thought—its complexity and specificity—comparable and generalisable; making it an equivalent next to others. Underlying this is the rationalistic culture which enforces principles of accountability, comparability, and manageability. Thought is an instance of information here. As a result of such principled practices, publications come closer to being comparative instead of substantive artefacts, they become more formal in their relation to others of the same class, they become estranged from the possibility of chaotic difference.

That which is formalised can be categorised, sorted into abstract classes and, thereby, appears to become more meaningful in comparison. This is opposed by the informal, characterizable only by the complexity and specificity of thought. The informal is much less efficiently comparable. It does not have a surface which can be used for immediate evaluation so that it cannot as easily be handled in classes; it is contingent, chaotic, and idiosyncratic. (Informal) text (and often also oral discourse) requires qualitative judgement; the publication establishing formal authorship, accumulated on personal CVs, can be measured efficiently. The development of formal rationalistic culture principles can be

conceived as a development of a dependency on formal forms. It means making the incomparable comparable—of reducing everything to equivalence, in the words of Horkheimer and Adorno ([1944] 2002: 4). The formal form allows for a reduction of abundancy by providing a means for efficient external calculability where there is mostly complexity and chaos internal to scholarship.

What do I mean by the formal form, the *traditional publication*? There is no specific or universal form to refer to here. It is neither print nor digital, editorial selection nor peer review, paywall nor OA—despite some obvious preferences existing in sub-communities (as indicated in my exploratory survey). In fact, it may not be a scholarly medium as such, since many texts of non-scholarly publishers or cross-sectional publications count as respected and esteemed artefacts in the humanities, too. Observe how Collini states that his

thinking and writing [has] been at least as deeply influenced by certain brilliant review-essays [he has] read in such publications as the *London Review of Books* or the *New York Review of Books* as they have by reading items which assessment procedures more easily recognize as “research publications” (2012: 78).

The *more easily* is telling: such media are *by definition* not part of the corpus of institutional assessment, but *by practice* of discourse they indeed are. Thus, they indicate fuzzy borders of the il/legitimate which may only be visible among scholarly sub-communities. It must be acknowledged that there never is a definite corpus, albeit the suggestive terminology of *scholarly* or *research publications* that aims to indicate a distinction between esoteric and exoteric discourse.

It is the publication *as such*, the notion carried in the field’s *illusio* about the practice of publishing as it is understood as well as the artefacts as they are respected among scholarly (sub-)communities that marks the distinction. Formal refers to an abstract notion that can be variously characterised by disciplinary or institutional practices. In the sense of established, orderly, and adhering to approved structures, this formalism refers to a sense

of systemness and convention. It is opposed by the potentially inaccurate, ambiguous, or indefinite, the complexity or even chaos that has to be judged qualitatively. Putting something forward formally suggests a performative aura of correctness and ceremony by evoking the impression of meticulous checking and reflection. Formalism shifts the perspective from substance to package, from idiosyncrasy to technocratic rule. And yet, the performativity of the formal form requires a belief in the correctness of formal criteria, a belief that is maintained by shared culture principles embodied in practices, a belief in the sense of a Bourdieuan *illusio*. It means carrying on with practices and, by so doing, submitting to the correctness of them and their artefactual aura. It is a matter of believed righteousness of published scholarship, of the equivalence produced by the practice of turning manuscript into publication. It is, in short, a matter such as: you have a hard time getting a job with a couple of manuscripts only, but you stand a chance applying with a proper personal list of formal publications.

The scholarship may be the same, the aura of the publication, expressed in formal authorship, adds the exchange value. Thus, this publication may be conceived as the inversion of the Benjaminian aura of works of art (Benjamin, [1935] 2010: 15): an appearance of equivalent nearness, however distant the content may be. The publication and its formal referents convey the appearance of comparability which diminishes the complexity and qualitative contextuality of text, thus, creating a distance between text and comparer (who may have not read it). An appearance that suggests the work can be reduced from complex ritual of scholarship to abstract comparison of authorship. This comparable, formal authorship is what I refer to.

The Terms of Competition

If a scholar wishes to compete, she has to submit her thinking to the terms of formalising so that others can make efficient decisions. This, of course, does not mean that every scholar publishes only for formal comparability, that the communication of scholarship is no longer an objective at all, or that no one looks at text anymore. The point of this argument is that, irrespective of the wish to communicate, the established practices of authorship and publishing require scholars to act in response to formal rationality. As explained before, the rationalistic culture is embodied *within* the practice. And to be sure, not all practices dealing with publications or authorship require efficient measurement; publications are still being read, scholarly discourses exist substantively. Scholarship and writing to varying degrees always precede publishing practices: a publication cannot be empty; except, perhaps, if the emptiness proves the point as in *The unsuccessful self-treatment of a case of "Writer's Block"* by Upper (1974).

And yet, most practices responsible for the existence of discursive artefacts are almost inevitably shaped by formal rationalistic terms directly or reflexively. Most scholars and research management staff would, intuitively, claim that, irrespective of this being culture or not, efficient measurement is a necessity. It might be lamented: how else can scholars respond to the masses of publications, applications, or reviews? One may say: information is everywhere, and you need to rationalise it in order to master it. I argue that this intuition counters the basic tenets of the humanities, since the principles of hermeneutics and historicity demand that qualitative judgement is preferred to efficient measurement. The need to handle masses of information are symptoms of the problem, rather than solutions. Efficiency based on formal authorship means dealing with a symptom instead of working on the problem, an approach that only reinforces the problem.

An essential point of my argument lies in the matter of rationalistic *terms*—not of competition itself. More competition is inevitable if more democratic access is demanded while the overall number of places does not grow disproportionately as well. Normatively, competition needs to be geared towards terms that embody, instead of disturb, the ends of practices on which competition takes place. Otherwise, it only contradicts itself by becoming an irrational rationality. Within the potential rationalities of discourse, I argue the formal rationalistic terms expose such an irrational rationality where achievement is more and more legitimated through formal authorship alone, or where such formal authorship is used as managerialist ways of institutional enhancement as in the REF. The predominance of this practice of legitimation is not normatively the only legitimate option; it is suggested as the most efficient option and, so, it is the option preferred by managerialist culture. Resulting from this, it becomes clear that the problems may not lie in competition but in its terms. In the terminology developed above, formal rationality enforces a thinning of ends within the terms of competition. This problem of terms is well-illustrated by Wood as she differentiates the opportunity of niche markets and imperative of a single market.

Wood searches the early developments of capitalism. Beginning in fifteenth-century rural England and preceding the principles of an advancing capitalist society articulated by Marx, an imperative to compete for ever-better productive techniques in farming became the primary precept of ordinary folk. There used to be many local markets; their differentiated and respective terms have been *opportunities* for farmers to engage. The market around which society and its many differentiated social systems increasingly came to be centred, however, was no longer opportunity but *imperative*, as the markets closed in on larger and, ultimately, single market structures. It was a combination of the ever-increasing inclusivity of a single dominating market and the options of consumption available to those depending on the market that shifted *opportunity* to market *imperative*. This

‘existence of market dependent tenant-producers’ created ‘competitive pressures’ (Wood, 2016: 130): ‘pressures to produce cheaply, pressures that reinforced the cost-sensitivity imposed by already existing imperatives of competition’ in recursion (Wood, 2016: 140).

By analogy, the contradiction of small market opportunities and single market imperative culminates in this: the interest in enlightenment requires myriad differences in its cultural dependency; managerialist excellence wishes to rank all scholarship to make it processable efficiently. Scholarly ideals and the thriving for a better argument could be the terms of competition. In this respect, ultimate ends of an interest of enlightenment or human understanding are negotiations that depend on a competition of the better argument in a rational discourse. Formal rationality, however, reduces complexity to rule-based processing and, thus, allows for an all-encompassing market.

This difference in the ways market and competition are conceived is essential. It helps to comprehend that the terms of competition are not universal: any market can have its own terms. Only if a single market dominates all, this market’s terms likewise do so—and they begin to dictate the terms of subsistence as well, creating pressures in recursion. Various niche markets would maintain and balance different opportunities and, thus, different means of subsistence. A single market inculcates an imperative which is the same for all. The progress of efficiency in modernity suggests that the market best suited to dominate is the one working on the most efficient means—irrespective of considering ultimate ends with which to balance individual means and abilities.

Moreover, I argue that competition in a balanced mode is favourable as it allows the strength of systems of patronage or nepotism to diminish and raises the potential for all to gain access to positions, an instance that may loosely be in the realm of a potential of democratisation. A culture shift towards valuing democratisation is evident where debates on diversification and the reduction of enclosing mechanisms related to class or

background abound. Such a cherishing of democratisation is ridiculing its principles, however, if it is based on formal rules only. The positioning of meritocracy shows the manipulative impact of this; several recent works on merit and the *demos* exemplify this discussion (Mandler, 2020; Markovits, 2020; Sandel, 2020).

A hundred years ago, Weber claimed academia to be an *aristocracy of the mind* ([1917] 2015: 10): a description that opposes more democracy in academia. Today, the discussions around merit indicate the will to enforce more democratic access. These discussions also show, however, how an *aristocracy of the mind* prevails as merit is, stealthily, still often bound to individual origin. The rhetoric of merit is ingrained in our culture as a device of blindly relating achievement with ability. Western societies aspire to allow all the education they deserve based on their *true merit*, irrespective of origin or family background, which is said to diversify elites and enable social mobility. The corresponding social construct, meritocracy, is both a lie and a placeholder for imperative competitiveness.

Firstly, originally conceived as a satiric term referring to the reflexive legitimization of the elite's position within social stratification by Young (1994), meritocracy has crept into various discourses, bringing in its wake a range of unresolved ambiguities. The culture argument in Karabel's monumental study shows the fluidity of what merit can denote—or can be made to denote. Karabel traces the admissions criteria of Harvard, Yale, and Princeton. Starting at around a century ago, the individuals running these institutions sought to retain power over who would be admitted (both in order to please rich donors and to keep the elite cohorts as closely connected as possible), while the ethos of equal opportunity arose in society. The universities' presidents were smart in their choice to resolve this impasse by inventing *character*, a 'shorthand for an entire ethos and way of being' (Karabel, 2005: 2) that was to be assessed by means of essays, recommendations of trusted sources, and interviews (Karabel, 2005: 131). And by universalising character as a

requirement for advanced attainment and elite education, society perceives character even more as a virtue of merit. All the while, elite institutions can choose who may become elite, since character ‘could only be judged by those who had it’ (Karabel, 2005: 2). In the end, you would want admission to be based on more than formal criteria of school grades, for these depend heavily on investment in learning early on; but extracurricular activities and personal traits invites opacity and ambiguity into the constitution of merit so that ‘however it is defined, it will benefit some groups while disadvantaging others’ (Karabel, 2005: 3).

Secondly, probably to the distress of progressive politics, the aspiration to meritocracy, even in benign terms, enables competition as a culture principle of aspiration. As an historian, Mandler approaches meritocracy quite differently than Karabel. His attempt to determine the actualisation of an equal opportunity principle in British education provides a positive perspective as there is more and more education as a result of which class structures became somewhat permeable. And yet, the problem of the conception is, in Mandler’s terms, that too much value is given to the matter of education in the first place in relation to what sociologists like to see in it (social mobility, diversification of elites, less hereditary reward; Mandler, 2020: 180–205). Mandler’s twist, describing the merit principle, exposes the downsides: if education is geared towards a more holistic sense of merit, this does not directly ensure a more equal society, since education is but one factor for how society and its elites work. For sure, better education correlates with higher financial and non-pecuniary rewards for the individual. But, all the while, constant discourse on the principle of merit and equal opportunity enforces a ‘greater sense of competitiveness’ resulting in the notion that the ‘problem is not poverty but poverty of aspiration’ (Mandler, 2020: 203). Sandel comes to a similar conclusion with his philosophical approach to meritocracy (2020: 34).

If these approaches to meritocracy are combined, the cultural construction of a holistic imperative appears. There is merit: all in society only have to show merit so that they can pursue a career based on achievement. And there is competition: since all are allowed to compete, competition becomes ever-increasingly more crowded. All the while, in terms of merit: the constitution of merit is pathologically defined by particular, enclosed subjects. And in terms of crowded competition: this requires merit to become less of a real distinction but an accumulation of comparable achievements because of the need for efficiency within the crowd. The result is that competition is increasingly concerned with its own terms: it suggests a reduction of contingency by making merit more efficiently processable but raises contingency for the individual by focussing competition on formalised achievements. Only the geniuses constitute exceptions since they bring in the uniqueness that repudiates claims to efficiency and gathers attention that surpasses relative terms in the first place.

This lives on in contemporary culture, albeit not directly: scholars also have to improve today in all respects as if they have to make themselves profitable (most dominantly articulated in the REF in the UK). The terms for so doing are dictated by a single market where improvement or achievement need to be efficiently comparable. This is true in manifold guises: for instance, as an imperative to establish formal authorship for competition in the job market (chapter 9) or as a means to the valorisation of research institutions in the UK (5).

8.3 Alienation: Relationlessness and the Teleology of Instrumental Practices

How can pressure be understood, an affect in practices that demands engagement, a demand that may not connect to substantive purposes? I suggest using alienation for a conception of this. Alienation ‘describes relations that are not entered into for their own

sake, as well as activities with which one cannot “identify” (Jaeggi, 2014: 4). This shortcut reference illuminates the disturbed relation of a subject to its practice: by actualising practice, the subject seems to be estranged from the purpose of so doing. And yet, what does entering a relation *for its own sake* mean? Can practices be acknowledged in a decisively non-essentialist way? I answer these questions by explaining the concept of alienation.

Jaeggi bases her critique of alienation on a conception of a ‘relation of relationlessness’ (2014: 25). She, thus, tries to transcend earlier concepts such as those of Marx or Heidegger which cannot sufficiently describe the social relations of practices and culture today. Her conception can be used to illustrate pressure working on the individual (explained in the next section) and, more abstractly, instrumental publishing (the subsequent section).

Focus on the Individual: Pressure to Publish and the Relation of Relationlessness

The paradox of the relation of relationlessness guides the conception of alienation away from essentialist or static principles. There is no role or identity authentic *eo ipso*. Not a normative meaning of practices or forms of life, but the process of appropriation is decisive for an unalienated condition: ‘the criteria for successful appropriation are located in this process itself, in the functioning of this process *as a process*’ (Jaeggi, 2014: 153; emphasis in original).

To clarify this, consider the ontology of the self; that is, its absence since ‘the self’s “being” is dissolved into something practical (the performance of an action) that has no ontologically independent entity underlying it’ (Jaeggi, 2014: 160). This theoretical foundation makes Jaeggi’s work particularly worthy for a practice theoretical study (also, see further to Jaeggi’s approach to practices: 2018). The self is not an inner, somewhat

authentic essence that may become alienated in articulation through action or discursive expression. A self is built at the intersection of internal and external, of reasoning, wants, and needs, and their articulations. Inner reflection is not transcendental action of a self without world, but a reflection of the self's world, a reflection of a *being-in-the-world*. World and self cannot be separated so that acting and the process of articulating wants and needs is always preceded by a self's history in a world; there is no *original position* but only process, so that 'we behave in an appropriative manner [...] to our own given conditions' (Jaeggi, 2014: 163).

Actualisations of practices are actualisations of a relation of an individual to practices, to artefacts, to cultures, to others—and by those intersections to herself. The alienated condition becomes articulated in the quality of this relation: it is *relationless* because of a failed appropriation. Such relations are *mute* (Rosa, 2019: 305): they have lost purpose and the individual is apathetic towards them irrespective of the result of action (such as economic benefit or social recognition).

Perceptions of pressure and external force to act, the feeling of being dominated by the practice, are decisive elements of this *relationlessness*. For sure, pressure can be positively connoted as well: the force to excel in a task based on well-intentioned motives or the short-term pressure experienced during work under the condition of an approaching deadline—these modes of pressure can motivate if applied to a stable self, an individual well positioned in her world such that the risk of failed appropriation is low. But a constant or repeated pressure becomes an alienating force if it strengthens instrumentalization and reduces chances of successful appropriation. Such pressure is the articulation of a disturbance on the *functioning of the process as a process*: it means the submission of the individual to being instrumental to a practice, instead of allowing for purposeful

engagement and providing the means for having some power over the practice and the form of engagement that may determine the feeling of purpose.

Constant pressure enforces a negativity (in a sense similar to the Weberian iron cage). Visible in this sense of the category is also a pressure of subsistence in a single market, where the ends of all practices are to be scrutinised within one regime. A market that turns diverse opportunity into holistic imperative and, therefore, a resulting alienation that explains the negativity of the pressure to publish on an individual. This is one sense of alienation pertaining to my argument.

Focus on the Subject: Instrumental Publishing and the Teleology of Instrumental Practices

Another sense of alienation is evident in the teleology of instrumental practices, an aspect of the category that is somewhat less fully elaborated in Jaeggi's conception. This sense pertains less to the individual and more to the systemic constitution of practices. It is an alienation of a practice in the sense of a thinning of ends.

If the purpose or end of a practice is scrutinised, this is not necessarily a recourse to an essentialist conception; it is a consideration of the balance of intrinsic values of practices or a practice as mere means to an end. This is crucial pertaining to the Weberian pursuit of formal rationality, its implicit focus on calculability, and the thinning of ends as discussed with Evans above. To recapitulate, the thinning of ends takes place as the ends of practices (the purpose of institutionalised doings) become more eclectic or fade away in favour of particularly efficiency-serving ones.

This thinning can be read as a teleology of instrumental practices. Jaeggi refers to a teleological circle where a practice (action, in Jaeggi's critical theoretical terminology) is merely a means to another practice. The practice ceases to function as a purposeful

instance on its own—it is instrumental in a non-meaningful way. To be sure, any practice is always related to some form of means-end calculation; it is never based on a single immanent purpose alone. That is, put differently, it can always be claimed that a practice contains different ends. To what degree, however, is a practice geared to an intrinsic primary purpose or to a meaningfulness it purports to fulfil? In this respect, Jaeggi asserts that there can be action that is meaningful despite its instrumentalization: ‘if it passes over into an action that leads to a goal that is not itself another means to an end but is pursued for its own sake’ (2014: 208). In other words, instrumental practice does not necessarily lead to an alienated condition; but a series or recursion of instrumental practices risks becoming a teleological circle.

This teleological circle can be found in the practices of authorship and publishing. Such a teleological circle of instrumental action is a thinning of ends: ends become thin as they are reduced to mere means themselves. The end as the normative essence of a practice is gradually replaced by being an instrumental end—a means—itself. This has taken place within publishing practices. Jaeggi briefly introduces the ‘academic who publishes solely with a view towards the citation index’ (Jaeggi, 2014: 4) as a phenomenon of alienation, without further elaborating on this. Despite its shortness, this statement well illustrates the alienated condition without recourse to a subjective essence but including a notion of normativity in the practice. This normativity is evident in the notion of *solely with a view towards*, or: it is supposed to be different. Publishing is not supposed to be instrumentalised as a formalistic means towards an end that is not the communication of scholarship (the citation index, in this example). This very sense of alienation as a shift of ends is also implied in Hyland’s remark about scholarly publishing:

[t]he desire to produce knowledge, to share ideas, and to make an important contribution, is just one impetus for academics to publish. It may, however, no longer be the primary one (2015: 6).

This terminology has a more psychological attitude (Hyland's disciplinary lens is linguistics), which I do not follow. But the argument is the same. Where Hyland says the impetus to publish shifts, I assert the end of the practice shifts. The former evokes the motivation of an individual, the latter pertains to the culturalised practice of a whole community. Harvie explains this as an alienation of the scholar from the products of her labour, the actual scholarship: 'the pressure on all academics [...] to *produce research output*, as opposed to *simply being engaged in research*' (2000: 113; emphases added). Likewise, Barth refers to the *perversion of authorship* as intrinsic scholarly motives vanish where all that matters is published output (2019: 13).

In this sense, publishing becomes an instrumental nexus of practices, one that is ingrained in a teleological circle of formal rationality. It loses its meaning within discourse as it shifts to focus on the formal form of authorship as a managerialist means external to discourse. Scholars engaging in practices of publishing necessarily actualise this teleological circle as this form of alienation takes place in the constitution of the practice, not in actualising it.

8.4 Conclusion

In this chapter, I drew on a number of theories that help understand practices in their contextual cultures as well as in their stipulations of subjectification. The Weberian categories of rationalities serve as the frame within which the culture principles of scholarship and contemporary academia are to be dissected. By introducing a theoretical argument of the formal form as well as by drawing on an advanced conception of alienation, I laid the foundation on which I go into details of these cultures, as they are articulated in concrete practices of authorship and publishing.

9 Strategizing Instrumental Publishing

Formal authorship is the most valuable resource in academia; it is evidence of scholarly productivity in a comparative mode. Whenever career decisions are made, authorship is the principal means to prove productivity and scholarly identity. In turn, the practices of establishing authorship—publishing—are instrumentalised for this purpose.

In this chapter, I explain the systemic focus on formal authorship in response to the job market and the way publishing is instrumentalised for this focus, particularly from the perspective of early career scholars. It is in this perspective that the matter of alienation of the individual—the pressure to publish enforcing an alienated condition—is most pressing. I explore this by analysing evidence from the qualitative interviews which contextualises this pressure. Subsequently, I discuss these findings alongside relevant literature. I pay particular attention to how practices of publishing are inherently practices for strategizing authorship and how pressure is reified in discourses. These two aspects illustrate variations of formal rationality.

9.1 ‘They Publish Much Too Early- Much Too Quickly’

Data about pressure within my exploratory survey results a bleak picture of publishing practices. The survey provides evidence about the distribution and substance of publishing pressure which allows me to identify decisive points in the established practices that demand further investigation. The following discussion of evidence from the qualitative interviews contextualises these quantitative aspects about pressure.

Reasons to Publish

I started all interviews with the widely open question of: *why do you publish?* It is revealing that the communication of scholarship or participation in discourse are the least likely reasons to be heard. In the foreground, almost always, are career and job-related aspects. Omar asserts ‘I mean so why I publish is it’s an essential part of the job [...] I’m paid to do it’; Clara says that ‘we’re obliged to do so [...] I’m paid to do so’; Tabea claims that ‘you cannot begin your career without published output’; Quintessa states that ‘it’s my job to publish research’; and Nora says that ‘why I published my research is largely in order to ensure that it’s possible for me to remain in this career’.

Relevant in such responses, first and foremost, is that the wish to communicate scholarship indeed often came only as a secondary or somewhat implicit reason. It was uttered as a wish to ‘contribute to the conversation’ (Melissa) or that ‘there’s no point in doing research unless someone knows about it’ (Richard). Quintessa made it more explicit; a hierarchy of reasons can be witnessed in her answer:

[I] kind of need to publish in order to stay in the job and get the next job- get grants. [...] But I suppose more positively it’s to share my research with the- kind of wider academic community really.

The hierarchy of reasons—first the career, second the discourse—is the summary of a systemic alienated condition such that it is not by contributing to discourse that authorship counts; performativity of formal authorship is made the primary matter, while discourse becomes meaningful posteriorly.

This posteriority seems to be weaker in Germany. Albert claims that ‘we publish our scholarship, so to say, to make it available to the knowledge community’ and Gerald accounts for both aspects: publishing as a means ‘to share scholarship with other scholars’ and as a means ‘to push ahead your career’. Herb connects the two aspects in a subtle

differentiation as he asserts that one instrumentalizes the content of scholarship, either to fulfil the ‘quantitative criterion’ or to strategically ‘position yourself in discourse’.

In all those answers, so to say right from the start of the interviews, the interviewees set the field of authorship and publishing as instrumental means. The practices the interviewees engage in are ways to negotiate external requirements of formalism and productivity-ensuring performance where the nature of scholarly discourse seems to be a by-product. This is not to say that these scholars do not seek to advance discourse. They appear to be rooted in an intrinsic desire of knowing and sharing knowledge which is evident in subtle terms throughout most interviews. But the terms of being able to remain within their discourse communities and to further knowledge are not made without managerialist principles as they are not connected to epistemological advancement, but to institutional requirements. A pertinent theme relating to this is that of strategic publishing where pressure is negotiated as a balance of quality and quantity.

The Imperative to Publish Early and Publish a Lot

In short: quality always tops quantity, but as quality is difficult or even impossible to produce *ad hoc*, quantity can make up for lack of quality. When temporal pressure to perform, such as the construction of an early publishing list, weighs on the early career scholar, she may strategically choose to focus on the number of publications on that list. Witness Ian, a reputable professor in Germany, as he summarises this balance aptly: ‘a short publication list is damaging in application and promotion procedures unless it concerns a professorial tenure’. Such quantification may be detrimental to the overall quality of discourse, but pertinent to the practices of authorship and publishing is the issue

of temporality preceding discourse. The pressure of this temporality was evident already in the quantitative survey and it was prevalent in many responses in the interviews as well.

Tabea criticises the temporal pressure impacting scholarly pursuit: ‘I think especially for early career people it would be great if people thought that they have more time to publish things of higher quality rather than have to fill up a CV very rapidly’. Likewise, Nora laments the pressure to publish precisely as a pressure to publish too quickly:

[a]s a general tendency I do believe that the pressure to publish is much too high- that it’s much too high on young researchers- they publish much too early- much too quickly- they’re much too specialized already- they don’t get any chance to broaden their scholarship.

Elizabeth talks about the hybrid strategy within publishing practices as an account of negotiating quantity and quality per se:

[i]f you get into an evaluation then you’ll have to have some high quality articles since the content will be assessed- but then not the whole publication list- only the four- five texts that you submit alongside your list [will be evaluated]- so then- yes this reflects some kind of hybrid strategy for those who work on their publication list- some for the length and some for the quality.

Similarly, Don emphasises that ‘if the five pieces are brilliant- that is as much valuable- but overall quantity really matters’. Omar further relates the issue of numbers to the intrinsic value of a few high-quality publications:

so it’s definitely the case that you have to have a solid publication record in the UK to get a job- that’s absolutely necessary. It’s not the case that you can be like some of the superstars in the US where you can get a job at very fancy places with no publications- that would be completely impossible in the UK.

And even then, as Omar says about *publish or perish*:

it’s not an exclusive disjunction. [...] you can publish a lot and still not make it. And that’s clearly the reality for a lot of junior people.

The case of Nora is particularly revealing as she refuses to be guided by an instrumental imperative in both her own practices and the advice she gives to her PhD students. Nora talks about a former PhD student of hers and says she graduated with a highly praised intellectual achievement and followed Nora's advice. Nora remembers that this person was glad about the scholarship pursued but, ultimately, she left academia because 'she needs a roof over her head' and, because of her path-breaking scholarship irrespective of adhering to managerialist principles, academia denied her getting a job to pay for that roof. By focussing on the quality of her scholarship—rejecting early, scholarly somewhat futile publications and resisting the imperative to publish—she diminished her options to pursue more of that scholarship.

In a similar vein, the experienced German professor, Clara, relates to the relentless load of material to read and the requirement to publish, so that the impact on the material essence of life is perceptible. Because as a performative scholar you have to invest a lot of time at an early stage of the career, it is the case that 'there tend to be individuals who are successful if they do not have a family', she remarks soberly. The connection between academia's way of treating achievement in performative publishing, its precariat, and the adverse impact on gender equality become blatantly evident in this account.

The imperative to publish and the ways strategic publishing practices respond to it are prevalent also in how senior scholars act in their roles as mentors or supervisors. Asked about what advice they give to—or how they experienced advice as—early career scholars, almost all interviewees implicitly articulated the negotiation of quantity and quality, particularly as an issue of early formal output standing against a necessary or desirable intellectual development. This performative strategizing, the structural pervasiveness of so doing, and even the possibility of its success as mere performance all indicate that the resulting publications are by no means driven by the desire to contribute to discourse. The

practices and their publications symbolise a reflexive effect of individualistic achievement that results from competition on formal rationalistic terms. The effect is reflexive in the way the practices reify and reinforce the terms. In the long run, thus, they change culture, honing it to the efficiency external management requires academia to run on. This is apparent in the difference between Germany and the UK, where the UK is much more advanced in working on efficient criteria of productivity.

The difference is qualitatively noticeable as German scholars seem wistful about the steadiness and slow publication culture of earlier times before the dominance of managerialism and excellence arose. John is typical in this respect as he talks about how he recognises a shift during his long existence as a philosopher. The German PhD thesis, necessarily published as a book, does not suffice anymore as a signpost for one's intellectual programme. More and more, peer reviewed articles are to be published alongside the thesis or, at best, already during the PhD. Don states in this context that 'the source of pressure surely is a sort of measurability of productivity'.

Remember also how Herb, being just in his early years as an aspirational philosopher, claimed that the intellectual work is 'instrumentalised'. He longs to focus on the argument, to make it better and, thus, position his thinking properly before externalising it through publications. These accounts do not mean to put the scholar in the ivory tower where he shall rest until eventually a good thought comes to mind. These scholars do want to be active, engage in discussions, and further their arguments. Instead, therefore, this shift is one of evaluating achievement, of what it *means to be active*: productivity counts only in the form of formal authorship. In simple terms, irrespective of having developed an intellectual programme, you have to publish—only this will allow you to get into the position to *continue thinking*.

Responses of scholars in the UK are more sober and even defeatist. In fundamental ways, they express the pressure that is prevalent in my exploratory survey as a relentless pressure to perform. The pressure here is more strongly connected to institutional regimes of formalism than it is in Germany where, as the shift above indicates, the matter of formal achievement is not as institutionally driven. The influence of the REF and accountability are emblematic. Most interviewees were conflicted in this respect: they signified a sense of gloom and discomfort about the advice they give their early career mentees alongside a sense of obligation or even burden that, since this is the way one *has* to behave in the job market, not giving this advice would be irresponsible. Witness the strategizing behind advice about publishing, for instance, in Tabea's response:

I tell them that it is the major thing that will determine if they get a career or not. And I try to get them publishing early on in their doctoral career- so I usually try to get them to send off an article after their first year of graduate study- and I aim for them to finish their doctoral study with three articles.

Melissa similarly expresses advice but makes her ambiguity explicit:

I advise them to try to publish at least one good thing in a good place before they hit the market. Yeah- and ideally two and possibly three. [...] I wish that weren't the case- but I think it would be- you know- a dereliction of duty not to encourage them in that way.

Karen even talks of having a 'really structured plan for getting some publications out'. Richard is more leaning towards quality: as a professor at a highly-ranked institution in the UK, Richard claims that 'the people who publish their PhDs as monographs too quickly tend to do less well in the job market because- precisely because they haven't taken the time and effort to improve it'. His advice, therefore, is to only 'publish a really broad-exciting- ambitious article as early as they can- which has the advantage of staking out the territory that is showing people what they're working on'.

An ambiguity to negotiate quantity and quality is observable in such statements. An ambiguity that is made explicit by others. Samuel, for instance, seems to be caught in a fight against windmills as he states that ‘well I used to give the advice not to try and publish during the PhD’. This, however, ‘changed during my career so it’s now- it would now be very foolhardy to go on the market without any publications’. Still, he tries to preserve his approach against the futility of so doing:

I’m normally in the business of trying to make sure they don’t publish stuff prematurely. And to some extent I think that can take the pressure off you know if people realise that it’s not quite the frantic dash that they might think it is. But it’s still quite a frantic dash.

Such discursively created *frantic dash* can also be an issue of conflicting advice.

Quintessa talks about how she observed such a conflict:

one of them said just publish as much as you can as early as you can- and the other one said- don’t publish anything for- like at least five years- like anything you publish now will just be wrong and you’ll have- you’ll have to live with it for the rest of your career and you don’t know anything yet and you should just wait and not publish.

Publish or Perish in Discourse

What appears to arise in such responses is the difficulty of negotiating quality and quantity of publications. This negotiation is often referenced as *publish or perish* among early career scholars, which can be seen as a reifying narrative (as I argue in 6.3 below).

Remember the notion of the *frantic dash* that Samuel referred to—engaging in the discourse on *publish or perish* may be a practice that, itself, enforces this *frantic dash*. It is a dialectic that can be observed by looking closer at the ambiguity in some interviewees’ answers, even if they do not explicitly talk about the reifying narrative.

For instance, Penelope talks about the pressure she felt during grad school and her first years as a scholar, where the encouragement to publish was persistent. All the while, there ‘was a debate- because it seems it’s actually not great for peoples’ intellectual development’ to publish early. Hoping for radical ideas and change in this respect, she proclaims that

[j]ournals should not accept articles by graduate students. I mean they were saying- look these people should be exploring they shouldn’t be trying to write all these journal articles- we shouldn’t be doing this.

Clara is very precise about how the pressure is partly evoked by discourse in that early career scholars ‘talk about this to each other’. There are conversations about what is required to be ‘internationally competitive’ and ‘what is required to get a starting grant’. All these elements, the conversations suggest, are about formal performativity.

Melissa expresses the ambiguity as a dualism where a career promotion pressure is the direct effect, while social pressure is the impact stemming from discourses with peers. Melissa centres this discourse around quantity as she says that ‘there’s a lot of social pressure to just publish as much as you can’ and that ‘there’s a real sort of- I think- bias is the word- just- I don’t know- assumption around that if you publish a lot more that you’re better in some way’. She further explicates how such discourse and the bias it evokes is considered a norm—a matter to be reflected as reification. To either publish or perish is the law-like principle, objectively cemented in academia’s culture, that early career scholars enter and reify by reproducing discursive practice.

Melissa sums this up in her own words: ‘I think- there’s just quite a lot of almost casual- well they have the best papers and such and such- or- well they don’t really publish very much’. As she is an advanced early career scholar, she faces both kinds of pressure herself but says she tries to ‘tune those voices out’ and to reserve, what she calls, an

integrity in publishing. An integrity to not participate in ‘gamey’ publishing practices (in the sense of strategic performativity):

I think that there are ways to publish and still have integrity [laughs]. [...] I just think that’s more difficult- because there’s a lot of pressure to do it in a way that I myself find compromised- you know- intellectually compromised.

Refusing to reproduce this narrative and, as a result, feeling less pressured to engage in practices seems to be a strategy among some early career scholars if they have some sort of assurance. Herb, the young philosopher in Germany, demonstrated an intellectual resilience throughout our conversation. He is knowledgeable about the discourse and the widespread anxiety but has found a strategy to focussing on his scholarly development against what he referred to as the instrumentalization of his scholarship. He expresses this in relation to the reified principle:

I think that somehow- that it takes on a life on its own and you could take the freedom to say- well I’m still doing a PhD and maybe two papers suffice and it doesn’t need to be five- interestingly only few people take this freedom during their PhD perhaps out of anxiety- an anxiety that is being instilled.

For sure, this can most easily take place if a scholar reaches a permanent position. Shutting social pressure out, therefore, is a strategy Karen also refers to. Having a permanent position as an early career scholar, she realises that she is in a lucky position since ‘it generates quite a lot of anxiety and tension if you don’t have a permanent position’. My conversation with her reflects how she knows about both the social pressure and the affect this produces to engage in strategizing practices, and that the situation changes once a scholar reaches a permanent position. She escaped the pressure and adapted her practice accordingly: ‘if I didn’t have a permanent job- I would probably be trying to publish more’.

Berta explains how such discourse arises because of end of year checks on productivity when each scholar has to report on her formal output: ‘in a way you don’t

want to be that person who, at the end of the year, has to say- well I don't have anything', she admits. The pressure arising from this is not entirely bad, though. It can be fruitful and some check on productivity has its rightful position. The problem, however, is that it is entirely based on formal results—that accountability comes about in terms of almost singular forms of output. Berta implies this when she contends that it would be

more productive and creative and you could publish in different ways if there wouldn't be given such a directionality especially at the beginning- so to say- what do you want with that publication now- this doesn't make any sense on your CV right now.

The sense of social pressure is echoed in Quintessa's account of a rather private, informal situation that provoked to think of having to fulfil some form of quota. Note how she evokes the sense of comparability by means of formalisms:

you remember writing job applications and friends sending me kind of their CV and so I could model my own CV on how they laid out stuff- I just find it really distressing looking at this enormous list of publications that they had to just feeling- kind of inadequate in relation to that. So it is kind of I guess the feeling of pressure is one of comparing yourself to other people.

Gerald exemplifies this anxiety based on comparability as in response to the discourses and artefacts of application procedures. He, likewise, refers to the casualty and simplicity of the ways pressure is imbricated with small issues that feed into the presence of pressure:

if for instance I apply and I am told- state your five most important publications and I only have five publications then this confronts me with the impression that I should have more so that I could choose from all my publications the five essential ones.

Ian, an accomplished professor in Germany, abstracts what he experiences among younger scholars as a rivalry where 'people are distressed- if they haven't published several articles before they hand in their PhD thesis'. It is illuminating to witness how he perceives

that this rivalry precedes all intellectual development which feeds into the notion of reification. The principle is law-like, it appears as an objective structure which cannot be changed or surpassed. Rather, its comparable formalism has to be dealt with in that one's scholarship is to be adapted to it: 'first of all there is an abstract pressure to publish and only then comes the question- what is actually to be published', Ian explains.

There also appears a mythical nature of this narrative. The anxiety, an affect that is passed on within this narrative or in situations of comparison with peers, mostly refers to quantity. The discourse on *publish or perish* can hardly be attributed to any form of intellectual achievement as such, to scholarship or the quality of publications (and neither on informal dissemination nor teaching). Most interviewees know about the importance of the latter and they acknowledge that without certain quality, no quantity will suffice. Samuel formulates this empirical reality as:

a few people have succeeded with- you know reasonably good stuff in large numbers. But it's much more likely that you succeed with a small number of really good things.

To act on the terms of this discourse, to simply publish for the numbers, therefore, is no solution on its own. But to publish nothing at all is no solution either. Instead, the affect that it produces turns to become the motivational aspect that leads early career scholars to engage in the strategizing publishing practices. By evoking this affect, this mythical narrative enforces alienation on an individual level.

The power of this affect and the symbolism narratively produced reduce along the development of a career. Some form of pressure remains, but it is less existential in the way that it shifts focus, most abstractly, a shift from competition to accountability. Since with a progressing career a scholar has already published material and, therefore, at least some substance on her list, the focus is more on satisfying requirements of publications following

grants and conferences, institutional requirements of the REF, or of publishing a substantial piece in Germany to get ahead with a professorship.

9.2 Strategic Publishing and Alienation

The dimensions and differences of perceived pressure became evident in the preceding sub-chapter. How can this empirical situation be understood with my argument about formal authorship, alienation, and culture principles of formal rationality? As the pressure is the highest among those with insecure futures—especially early career scholars—both senses of alienation can be found here. The publication, literally, is an existentialist means: to further the career in institutionalised academia, you have to publish relentlessly. The basis for this is the terms of competition that determine who will be awarded a place in academia. I discuss this along with strategies inherent in publishing practices that articulate their instrumentalization. I further show how the discourse on *publish or perish* reifies pressure, contributing to the shaping of practices reflexively.

Competition and Achievement

More than being just indicative, literature about employment provides strong evidence for characteristics of early career competition, both cultural and material. Witness the difficult employment situations of aspiring scholars in Germany (Agarwal, 2015; Pauli, 2016; Sander, 2017; Ullrich, 2016, 2019) or the UK (Acton et al., 2019; Calkin, 2013; n.a., 2018; Swain, 2013; White, 2015). In both countries, there are ever more PhDs accompanied by fewer long-term prospects of academic employment (Maher and Sureda Anfres, 2016). A stunning 87% of scholars in the German *Mittelbau* (the range of positions between PhD and professorship) are on fixed-term contracts (Ambrasat and Heger, 2020:

5), and only 4% of an annual doctoral cohort has the potential to reach a permanent position in academia (Wissenschaftsrat, 2014: 25). Add to this, particularly for the humanities, the tales of unemployability of PhDs or how it is considered a failure to leave academia (Kruger, 2018; Segran, 2014). And as a structural addendum, look at the mental health crisis pervading, even worsening, throughout academia for young scholars and researchers in the Western dominion (Evans et al., 2018; Levecque et al., 2017; n.a., 2019). This all describes the ‘precarious position of young scholars’ with its ‘nasty, vicious circle’ of poorly paid, short-term positions that deprive scholars of the security to develop and publish truly original scholarship (Beard, 2019: n.p.). It is even claimed that academia works on *hypercompetition* (Moore et al., 2016: 8) which is articulated by the rhetoric of excellence and, ultimately, is said to harm research quality. It is claimed there is ‘a Darwinistic competitive race among (especially young) academics—for funds, citations and publications, not for better crafted and more convincing (counter)arguments’ (Vostal, 2016: 105).

These statements can be unpacked into: the fact of abstract competition and the matter of its concrete terms. The former is a required mechanism allowing many to take part in intellectual pursuit; the latter is its manifestation as formal performative terms. Demanding less competition may just mean raising an illusionary veil, hiding the fact that there *are* many individuals for only a few positions. Personally, I know there are many aspiring cultural theorists next to me. The question is on which terms will a few of us be promoted in our (potential) careers?

What precarious competition does is it contributes to an uncertainty about the future; but only because of the culturalised terms on which competition is to be performed are publishing practices produced and reproduced as instrumental towards external ends. These terms relate to uncertainty as an allure of rationalistic alleviation. The notion of

instrumentalization is evident in the various accounts of reasons to publish in which scholarly discourse is relegated to a secondary position in my interviews. Moreover, the temporality of publishing—the need to have instances of formal authorship today rather than tomorrow—that is prevalent among early career scholars articulates how competition is based on the formal form.

Early career scholars are systemically discouraged from focusing on intellectual development so long as this stands in the way of constructing a substantive personal publication record. The characteristics of the rationalistic terms of authorship and publishing are embodied in this principle. And the precautions are so as well: surely the intellectual development can go hand in hand with the expansion of one's publication record. The engagement in discourse with one's early thinking may prove to be a fruitful endeavour even for this development. And yet, as my non-senior interviewees show, because the way publishing practices are constituted today with their inherent affectual mode of imperative, intellectual development may be prohibited more than it is supported. The reason for this is the aspect of instrumentalization bound to culture principles of formal rationality which are partially relatable to scientific or technocratic understandings of productivity.

Mediating Achievement through Culture Principles of Scientism and Technocracy

The alienation ushered in by the instrumentalization of publishing articulates the publishing practices' partial indifference to discourse. In other words, the development of an intellectual programme is geared towards a substantive rationality which is diminished by the formal rationalistic terms the competition for employment works on. As contributing to discourse moves away from *the sake of discourse*, publishing practices

incorporate ways of increasing the yield of formal authorship. This is the matter of formal rationalistic technocracy conceptualised theoretically before (5.1), which I know relate to the empirical situation.

Practices of recognising achievement and awarding reputation shift from a dependency on qualitative judgement of content to a dependency on measurement of formal authorship (Schimank, 2010: 235). It is a truism to state that as soon as comparability is quantifiable, quantity becomes a goal on its own (just as Goodhart's law suggests in crude simplicity). As shown in the literature review, there is widespread evidence for the availability of the mechanism referring to a measurement turned incentive, but it is rarely investigated in the humanities. Goodhart's law appears in manifold guises in the sciences in connection to principles of comparative productivity measurement. Edwards and Roy, for instance, point to the downward spiral that productivity measurement turned incentives created (2017). This may also include forms of misattribution of authorship and coercive citation practices (Fong and Wilhite, 2017). But the lack of quantified evidence for such mechanisms in the humanities does not mean that it is not applicable here. I suggest that the research Biagioli puts forward about scientists is relatable to the humanities.

This is the strategy of balancing quantity and quality. Biagioli states regarding Goodhart's law that some scholars 'aim high, but not too high' (2016: n.p.), deploying all means to produce more publications—but just not those high impact publications. The simple reasoning behind such strategy is that more publications look better than fewer publications, and if they do not *look too high impact* (or appear in high impact places), it is unlikely they will be scrutinised. Scrutinised means being read, and a few high-quality articles suffice for being read. Nevertheless, one can (and the interviewees suggest, one has to) go beyond those to be scrutinised since job applications with more publications are said

to fare better. As Williams poignantly puts it, '[t]here are demands of academic promotion, which can encourage one to make as many published pages as possible out of whatever modest idea one may have' (2008: 183). These demands enforce the instrumentalization so that early career scholars learn to balance quantity and quality to yield the highest reward of formal authorship. This reward is telling since it is evident that it is not effort that is rewarded; instrumentalizable (that is: publishable) results are.

The observable fraudulent practices available to *scientists* such as p-hacking, ghost authorship, or metric manipulation (Head et al., 2015; Schofferman et al., 2015) may be harder to facilitate in the humanities and, therefore, they seem to not be manifest here. Or, perhaps: they may only be harder to define since there hardly ever is a p-value in the humanities. But there are questions of significance, nonetheless. Consider the absence of null results in the body of legitimately produced knowledge in the sciences (Hubbard and Armstrong, 1997; Petty and Gross, 2019; Rosenthal, 1979). When scholarship means argumentative reasoning instead of statistically signifying, what appearances can null results have? I argue that null results of sorts are available also in the humanities: it is not statistical significance but the meaningfulness of the argument to the community that make the statement significant, albeit this being not falsifiable. A scholar exploring a new argument may find it meaningless but may still be able, or required, to turn it into a publication, just as Williams laments. There may be no such resistance against meaningless—null—arguments in the humanities as there is in the sciences (where the publication of null results even has an adverse effect on the researcher).

Moreover, in the sciences, there is the normative argument that all results *should* be published, since they have a meaning irrespective of assumed significance, may it be only to reduce redundancy of experimentation. This normativity, of course, does not mean that this is realised in praxis. In the humanities, where meaning and subjective argument are

essential for discourse, it is much more questionable whether such meaningless explorations—humanities null results, so to say—need to be contributed formally to a community. To put it in analogy: in the sciences, it is valuable to reduce redundancy of experiments. In the humanities, redundancy of thinking may not be so sought after, but this diversity of thinking not necessarily needs to result in formal publications (since oral discourse and teaching can be strong for debating ideas).

The rationalistic reality suggests the opposite, meaning that all explorations are forced formally into discourse. To achieve quantity, publishing practices allow, and demand, scholars to turn every argument into a publication. Formal authorship is gained and, all the while, the argument may be irrelevant to the community. It will simply not be read; these are the articles that are perceived to be published too much as shown in the exploratory survey (figure 47). Several of my interviewees expressed such points implicitly and even explicitly in terms of the relevance of publications or their decreasing quality. And this is an issue with articles much more than with books since the former are more easily amassed.

This can be turned to its oppositional perspective where there is indeed a significance for the community. In the humanities, this may be a powerful new argument or an insightful new exploration into archival material. The instrumentalization of publishing has shaped even this: it resulted in the practice of deriving least publishable units out of a single analysis or dataset (also known as *salami slicing*). Again, evidence of this practice in the sciences is easily accessible (Broad, 1981; Supak Smolčić, 2013). What is a least publishable unit in the humanities? One would claim that *salami slicing* is harder to facilitate in the humanities since each slicing up of an argument reduces its power. And still, the idiosyncrasies of argumentative statements in humanities disciplines allows for each representation of an argument to differ from one another—and so, to make two

publications *sui generis*. It is rather a pragmatic matter of time to author two representations of an argument, once its force has been discovered. My exploratory survey indicated that *salami slicing* does, indeed, prevail in the humanities (albeit less so than in the social sciences; figure 23).

One may argue that it is also fruitful for the *discursive* author to make more publications out of a strong argument. And this point can hardly be refuted. Being ubiquitous with one's argument—and, so, being widely visible to as many readers as possible—surely has its worth. And yet, the demand to make as many published pages as possible seems more suitable to explain the matter of *salami slicing* than intrinsic values of discourse can. In the end, the very point of communicative loci of communities—brands which provide the basis for a community's communication—can facilitate visibility very well (as I discuss in chapter 10). And yet, the brands best suited to making a contribution visible are the once the hardest to get in. Time is scarce, though, and particularly for early career scholars, quality cannot simply be produced by honing the single argument to its best version irrespective of impinging pressure. In this inheres a dialectic, which I explain in the next section.

Mythical Discourse: Publish or Perish Embodying Reification

There is a sense of *publish or perish* which I propose during the discussion of evidence from the interviews above. *Publish or perish* is a referent to a scholarly theme as shown in the literature review. Apart from this, however, it is also a referent to a non-scholarly discourse passed on among early career scholars. The two converge as the origins of the term show. Still, they can be separated where the former is a keyword for analysis and the latter is an articulation of the reification of instrumental publishing. By engaging in this non-scholarly

discourse on *publish or perish*, early career scholars reproduce the symbolic construction of a rationalist solution to avert uncertainty about the future: I only have to publish to avoid the death of my career. This reification is meaning-making per se: the reproduction of narrated culture principles.

The narrative comes about as a reflexive discourse with which individuals, especially early career scholars, negotiate the insecurity of entering the academic job market. This discourse is reflexive in that scholars, by engaging in the discourse, react to the increasing lengths of publication lists and the unknown own position in relation to others entering the job market or to those with secure positions. This narrative is not: *to think or perish*, or: *to argue or perish*, or even: *teach or perish*. It is *publish or perish*, since it is formal authorship that is said to count and, in turn, the illusionary idea that the traditional publication indeed suffices as a way forward.

A dialectic can be observed in this reification. By engaging in discourse about the solution, early career scholars induce added pressure that potentially harms their intellectual development. This is the ambiguity evident in the interviews. Scholars account for the ubiquity of informal comparison and conversations about relentless publishing. All the while, those who claim to unplug themselves from this constant stream of comparison and conversing seem to deal better with the underlying alienation: they find ways of negotiating the pressure and their intellectual development by publishing less.

The reasoning behind this is that the constant conversation on having to publish fares as a social fact: meaningful reality constructed through discourse that creates real pressure by apodictically claiming there is only one solution. The conversation reifies the rationalistic terms of authorship and publishing and, so, reflexively enforces them. Therefore, engagement in this discourse identifies a solution embedded in a structure that is part of the problem. It creates, in the words of one of my interviewees, the *frantic dash* for

publications and discourages alternatives. For sake of comparison, engaging in this discourse and acting upon the *frantic dash* it creates may be similar to frantically waving arms and legs when being caught in a mud bog. For the latter, the better strategy is to remain calm and to assess the options for *slow and deliberate* movements.

Remember the self-help guides that I presented in the literature review. They aim to help scholars to be more productive in formalistic terms. They suggest that the way out of the precariat is paved by formal publications, since the practices of counting and comparing records of formal authorship is the most fundamental way of recognising productivity. In the end, they are embodiments of this *publish or perish* discourse and are, likewise, instances of the reification of its formal rationality.

This narrated solution can be regarded as a myth, as I claimed already while discussing interview data. Seeing it in this way helps clarify the affectual structure inherent in this discourse which explains why early career scholars engage in it. It is this meaning within the mythical narrative, the symbolic outlook on a solution, that motivates engagement in this discursive practice; it is not the materiality of artefacts or job applications as such that entices this discourse. The reason for this is that materiality is mediated by culture. There is an analogy in the discussion about *risk society*. As proclaimed by Beck ([1986] 2003: 25–31), society has to cope reflexively with risks that it produced itself. It is a form of dialectic often seen in social theory where society tries to master nature or create more efficient technology which bears the sting of its own destructive force right from its invention. It is a modern dilemma, one that is particularly determined by formal rationalistic means just as Beck's *risk society* is the subsequent development beyond industrial society.

Such reflexive coping is also evident in academia. Think of the need for better literature databases and search engines where previous advances of rationality led to

exponential growth of literature; or the need for better citation managers in times when bibliographies get ever longer and, so, the work of dealing with it becomes ever more of a burden. And yet, risk should not be teleologically related as cause and effect where it may not be thus explicable. There can be the categorical mistake to take reflexivity as a fact, rather than a myth put in place to alleviate a burden of insecurity—the mistake of being a ‘prisoner of objectivism’, as Alexander and Smith call Beck (1996: 256). The authors argue that *risk society* is ‘a myth constructed by, and reflecting, the social and cultural structures of contemporary society itself’ (1996: 259). Risks have always appeared alongside rationalistic progress so that the shift from industrial productive to post-industrial risk-coping society only reflects this dialectic.

Turning back to publishing, observe how early career scholars try to cope with pressure that has always been within scholarship. You always had to prove you are better than your competitor. This competition only has grown more strenuous as the terms have been rationalised, reduced to the appearance of a formal teleology. All the while, it cannot be claimed that long publication records are *always* demanded or that submitting non-formally published works or pre-printed manuscripts in a career application process is always outright rejected. It is the culture principle that mediates appearances of competition in material phenomena so that subjects perceive the appropriate response is the one proposed by the *illusio*. Likewise, no scholar has to engage in the discourse on *publish or perish* just as no one is forced to the *frantic dash* of relentless publishing. But it takes unconventional motivation to not reproduce established practice; a motivation to resist the practices’ immanent affect.

Faced with an uncertain future, it seems to be a high-risk strategy to disregard competition on formal authorship: who is there to judge and assure me that my few brilliant ideas developed in informal manuscripts will suffice? All strive to achieve more

and, by looking at the best in their fields, usually senior scholars, try to replicate such senior publication records. But how does an early career scholar account for the instability of the first steps of scholarly thinking, where to get the reassurance of the eminence of the slow process, of the few focussed publications? How can the German *Habilitand* (still under the auspices of a professor) or the early lecturer in the UK be sure of her position in the field if all around her publish frequently? And why put so much effort in this singular publication if quantity tends, or is said to tend, to be rewarded above quality—and quality through reception will, in the humanities, be determined only in a few years' time when the contribution is really being picked up in discourse? And most of all, why put so much effort in the single publication if the single publication is hardly ever accorded the attention it may deserve? In other words, as John remarked during the interview after being asked why he publishes his scholarship: 'that's a good question since it's not being read'.

Relying on the production of formal authorship yield seems to be more secure and, thus, it developed as a dominant culture principle, albeit that this was potentially a myth. In this respect, this is a disciplining mode: the own intellectual development and a thorough scholarly agenda stand in the shadow of a formal track of productivity. This is essentially Weberian: I referred to the theme of discipline and the absence of ethics in Weber's work on rationality (chapter 8.1). Since no normative frame to evaluate the ends of practices is in place, formal rationality places mere discipline above responsibility: one need only work hard enough in accordance with given (thin) ends—effectively working through means: ticking boxes—until achievement rewards discipline. Where the rationale of ticking boxes creates anxiety in its narrative representation of *publish or perish*, it does also create an assurance that a way forward is possible. It advocates that an intellectual career can become plannable and manageable where insecurity and precarious work situations suggest the opposite.

The Academic Precariat and Women

This is a gender issue, too. The academic precariat of young scholars is known for being a hostile environment for planning and starting a family or raising children. A gender gap is reported in various instances where, by historical default, women are disadvantaged. Witness the different ways women are structurally underrepresented in the bibliometrics of authorship (Bendels et al., 2018; West et al., 2013). Such structural effects, arguably worse in STEM fields than in the humanities but nonetheless so here, also prevail in many practices in academia such as hiring, promotion, or grant peer review (Bornmann et al., 2007; Larivière et al., 2013; Laufenberg et al., 2018; Moss-Racusin et al., 2012; van Dijk et al., 2014; Weisshaar, 2017). Ultimately, underrepresentation of female scholars in high impact publications is even worse than it is for their underrepresentation in a scholarly discipline, as has been observed for the field of sociology (Akbaritabar and Squazzoni, 2020). My exploratory survey has also shown that pressure to publish tends to be perceived as higher among women (figure 34).

The connection of publication numbers and career development for young women becomes blatantly clear where sheer disciplinary workload of carving out publications stands against the tasks of the reproduction of life, foremost of raising children. Such work simply misses the recognition as a fundamental achievement (Honneth, 2003: 141). The anecdotal evidence persists of women deciding to raise a child and, so, missing achieving as long a publication record as a male competitor who therefore gets a job. 2020's lockdown in response to COVID-19 was evidence in this respect: the gender balance in journal submissions shifted towards male scholars and researchers, indicating that female scholars and scientists had other tasks to look after (Flaherty, 2020; Rusconi et al., 2020)—for society as for academia, these are likely more important tasks than publishing, though they miss their due recognition.

9.3 Conclusion

In this chapter, I traced the impact of pressure in publishing practices in relation to culture principles of competition and ways of perceiving achievement. Scientific or technocratic principles become available as efficient means in the humanities, which reduces principles of contributing to discourse. The triangulation of quantitative data, qualitative statements, and available literature works well here as the issues are complexly interwoven. And it well underscores the conception of alienation with empirical data.

The findings discussed in this chapter show in regard to the *question of why scholars publish*, reasons are shaped reflexively by a culture shift in response to, and mediation of, structural issues of the field. Both these structural issues and the culture shift have no single origin; they are enforced by various drivers. All of these seek to advance efficient means as a response to aspects of uncertainty, ubiquity, or legitimation. The next chapter will further clarify, with both a conceptual discussion and empirical findings, how the matter of efficiency is involved in the rationality of scholarly discourse today.

10 Publishing Venues: The Stratification of Discourse

Talking about matters of efficiency requires looking at media. Media, in their literal sense, transfer information. Much more than that, however, media also transform text symbolically: media are the basis for the formal form of a publication. Media are, in this sense, the material manifestations of the academia's *illusio*. And yet, where they correspond to discourse communities, media could be *mere* material manifestations of these. Only as they acquire additional symbolic value, they transcend this original materiality. The symbolism in this value is another articulation of culture principles and, in the shift from materiality to transcending symbolism, an articulation of means of efficiency. In respect of such efficiency, media are a focal point of analysing publishing practices as they, on the basis of venue selectivity and its resulting hierarchy, fundamentally shape the way publishing practices are constituted. This introduction arguably comprises of much abstract name-giving. I explain all of it in this chapter.

A note on venues: I use the term *venue* as a referent to an individually branded site of publishing (in existing literature often referred to as *publisher*, *outlet*, *imprint*, or simply *brand*), may it be a journal, a publishing house, or a distinct monograph series within a publishing house. This allows a view on the mechanisms of a *venue* across different epistemic genres from journals to books. A *brand* functions in a similar way irrespective of the epistemic genre it refers to: it is the referent of a publication *venue* to its symbolic and material value produced and reproduced in practices. For matters of understanding and legibility, I mostly used the terms publisher or brand without venues until now in this thesis.

This chapter has three distinct areas of interest. As before, I discuss findings from the interviews before going into detailed topics. Thereafter, I discuss venues firstly categorically. I start with an outline of a discourse-pragmatist conception of venues before

going into details of how this becomes stratified and gathers added symbolic value. This comprises of a discussion of Bourdieuan symbolic capital. Secondly, I discuss practices of selection. These illustrate well the culture principles in question throughout this thesis: the humanities with their subjective, qualitative mode of arguing on the one hand; managerialist principles of efficient handling of information on the other. This opposition within the practices of selectivity make this discussion worth to be included in this thesis. Thirdly, I sketch out further aspects of media in an essayistic excursus, including matters of openness and digital technology.

10.1 'I Know the Journals Which Are Good and I Read Those Things First'

Symbolic and Material Value of Publishing Venues

Among the most prevalent aspects in regard to publishing venues in the interviews was that of the consecratory role of venues; a point raised in both Germany and the UK. This role, however, is not dominantly connected to a consequent hierarchisation of venues. Rather, a sort of clustering emerges that is not necessarily a hierarchy. Certain venues have a reputation that symbolically legitimises content (after having constituted its material existence), while other venues do not achieve to gain such reputation (and whose providing of a material existence of content remains somewhat irrelevant for discourse). Evidence for the former is expressed as a form of trust.

Gerald, for instance, claims that in literature reviews, he trusts publications in known venues without reading them. Karen similarly states that the ability to rely on certain publishing venues as trusted sources without critical reading of content is helpful. However, she also outlines concern that while reviewing personal publication lists, it is important not to rely on venues alone since the choice to try to publish with certain venues

also involves ‘what kind of advice someone got or like their own confidence in themselves’. Desire to publish *and* gatekeeping both matter.

Tabea says that ‘there is so much published that if I’m looking through a long list I recognise- I know the journals which are good and I read those things first’. Berta puts forward the idea that the university press—irrespective of its position in a hierarchy—evokes the symbolic notion of quality in processes, a reputation of materially valuable effort. Penelope articulates the consecratory impact certain esteemed brands have as she talks about strategizing publishing: ‘one good publication in a good journal is worth as much as five publications in not very good journals’.

The quality of content is externalised to the venue as a marker of this quality—by practice, not by necessity. Herb implicitly confirms such consecratory role of highly esteemed publishing venues as a cluster. The well-regarded venue legitimates content so that potential readers or evaluators do not have to read the contribution; the brand itself allows them to perceive a contribution as valuable. And it is for this reason that Herb would always strive to publish with one of those highly esteemed venues, albeit knowing that they have no innate value for scholarship. Notions of the ‘gatekeeper function’ of a journal (Albert) or the value of a journal being its selection (Berta) also emerge. This, however, is also criticised, as Gerald claims that journals try to create an exclusivity rather for marketing purposes.

These notions of the consecratory function of venues connect with their material value. This value is expressed by the idea that a specific venue, by means of its active and passive selection of content, fares as a central site of communicative exchange—and therefore as a condition—for a discourse community. During the interviews, this material value often implicitly emerged to be on par with the notion of the consecratory impact of a

brand. The expression of visibility plays an important role here—a visibility that can neither be reduced to symbolic nor to material value.

Elizabeth marks the distinction of the important journals as being those where ‘the exchange within a small scholarly circle is easily facilitated’, which paraphrases my notion of the material condition of discourse communities. She further remarks that, indeed, the more obscure the publication venue, the harder it is to access the content technically. It is not simply that such venues do not facilitate much symbolic value for the author—the content is also materially invisible (a matter that is about to change technologically due to OA). In a similar vein, Tabea articulates her article publishing decision-making as a balancing of reputational merit (attached to more general journals) and specific audience (as in narrower-scoped journals)—remember this distinction in my outline of a discourse-pragmatist conception in 9.2.

Penelope sketches how ‘you having published in these visible high places’ tops all other forms of making your scholarship visible (such as by networking at conferences, for instance). Recounting from chapter 6, Herb voices his concern over how specific German-language journals in history of philosophy become lower ranked because of their bounded, national visibility. And, because of this, that these journals disappear from the high stratum of disciplinary journals.

Omar further articulates how the symbolic value of a brand can overlap with that of epistemic genre: it can be more beneficial to publish an article in one of the top-ranked general philosophy journals than to publish a book. As such, brands ‘play a signalling role’ but they ‘are not important for scholarly discourse’. Such an overlapping also happens where a contribution to a collected volume—even with highly-esteemed venues—might be detrimental for the published scholar simply because such a volume is regarded as less

exclusive: 'I've been told by senior colleagues for example not to put papers in collected volumes'. A similar point is made by Clara in a critical manner.

Omar further states, however, that the value of the venue as a quality heuristic as such might be diminishing within philosophy. This connects to other suggestions about desired change in respect to venues: Samuel openly voices his anger over brands and prestige as an indicator where such brands, especially those owned by corporate publishers, are 'entirely parasitic on the academic community' and 'if we could get rid of them that would be great'. Nora issues the concern of an overall decline of quality in submissions, which is reflected by a long tail of publications for which she cannot discern whether they are in quality venues or not—there are, in other words, too many venues to form an opinion on each venue. Richard is critical of the notion of reputation of a venue since it might be a very arbitrary or historical construct that is not necessarily mirrored by performance.

Herb voices the concern that publications in low-end journals are generally without value, so that scholars should not aspire to submit to such media in the first place. This, Herb explains, is not the case because the contribution is useless but because the discourse community 'couldn't care less' about such venues. The low end of the logic of distinction is, as I suggest in my conception, pronounced here as a material invisibility where the symbolic (either value or insignificance) is not considered at all.

Regarding assumptions of consecration, Quintessa became aware of her contradiction during the interview: she, normatively, claims that every humanities scholar should assess content critically irrespective of venue; but she, all the while, knows that she reads text differently if it is published in a 'journal that's completely unfamiliar' to her. The obscurity and, therefore, non-consecratory role of a venue produce an undesired reading bias.

Venues and Promotion Practices

Another important theme during the interviews is the venue as a heuristic during promotion practices, which, in the end, is crucial for understanding the non-discursive value of venues. In this respect, the interviewees expressed some regional differences which, therefore, cannot necessarily be generalised. In Germany, Clara, articulates that the venue is one aspect that is considered during promotion review practices. It seems to play a role as an indicator of industriousness or international visibility. But it is important only as one among other indicators. John also claims that in his discipline, philosophy, external criteria such as venues are not crucial and, in most practices, are only a secondary criterion of assessment. In a similar vein, Ian clearly states that the venue is always only a secondary criterium for evaluating personal publication lists. Don has a different opinion as he says that his recognition of a scholar is different if she has published repeatedly in highly recognised journals. Yet, he concedes that the content is still looked at and that this practice is not fundamentally different than it has been at earlier times.

For practices in the UK, Quintessa says about a brand that ‘it’s kind of a heuristic of quality but it’s just not a great one’. Nora is straightforward in her critical opinion about the value of venues for a career, expressed as: ‘I will play the game but that’s all it is- it’s a game- so it’s brands- it’s just- you pick your brand even if you think it’s nonsense’. This playing the game metaphor is also what she teaches her scholarly mentees:

I show them all the rankings. I tell them it’s nonsense [...]. But I say what you have to do for your career is you publish in the top journals.

Richard outlines how scholars screen personal publication lists during promotion procedures just as employers screen positions on a CV for prestigious institutions: ‘but unlike them [employers] we [scholars] can actually read the work’. Melissa even says that in most cases of promotion, the venues on a personal publication list is not an important

criterion. All these statements highlight the ambiguity inherent in brands in regard to promotion reviews.

Selectivity and Gatekeeping

The interviewees unequivocally confirmed the rise of peer review as the dominant active selection practice in humanities disciplines in both Germany and the UK. There is a tendency to regard this as a negative development as the positive voices are a minority.

Critical aspects are these: Don is sceptical about the tendency to value peer reviewed works higher in the humanities (in grant review or promotion practices) since scholarship is not as objectively measurable as research in the natural sciences is. In a less nuanced voice, Ian offers a strong opinion with his conclusion that ‘peer review in the humanities doesn’t add up at all’. Especially the form of peer review, its blindness, is of concern here. Nora, also highly critical about blinding, refers to the network structure of editors, reviewers, and authors who, by knowing each other from conferences or previous work, are biased. From this follows that ‘the idea that there is anonymous peer review in the areas in which I publish is more or less a myth’. And as someone external to those smaller networks, or as someone challenging their practices, peer reviewing can become a hostile practice, as Nora explains: ‘it gives a license to bully academics who do not follow the rules- who don’t go along with the crowd’.

Similar worries are voiced by others. Berta outlines that within specific scholarly communities, publication does not necessarily depend on peer review even within peer reviewed journals since editors ask her whether she wishes to contribute. Penelope claims that subfields tend to become so small that blind processes are impossible since everyone knows each other. Melissa mentions strategies of gaming to get published by a venue, for

instance, to cite other publications of this venue for this purpose. She also refers to the fact that highly-esteemed scholars will always find a way to get published irrespective of the standard selection practice. John reiterates the same point and further explains the shortcoming of the practice under time constraints on sides of the reviewers who, sometimes, only read the first page, do not like the introduction to the argument, and on the basis of this reject the paper—without having read the actual argument.

There are a few positive notions as well. Richard is positive about peer review practices in the humanities as they are less competitive than they are in STEM fields. Gerald states that peer review is positively connoted as an institution that ensures that, quite frankly, ‘there is no nonsense’ in the publication. Quintessa also stands up for peer review as she says that it can be collaborative and help improve a submission.

Berta concedes that with economically-well situated publishers, peer review seems to be more constructive and comprehensive since the reviewers tend to get paid for their work. Herb is more differentiated in regard to the mode of the practice. He elaborates that selectivity is an often-necessary task; the question only is on what terms an article is selected or not. For the discourse community to take these terms into account, the reviews should be made publicly accessible alongside the submitted text.

Conformist Biases

Also of relevance in respect to peer review is the possibility of a potential conformist bias. No unified statement can be derived from the interviews about such a conformism. But there is a clear tendency towards expression of a concern about it in regard to both form and style of the submission and to the canonisation of content.

Richard, as the most positive voice regarding peer review is the only one to reject such concerns. Since the practice depends on subjective judgement, he says, there might be reviewers who are ‘territorial’, but others are much more open minded. He discards the assumption that ‘conservatism and resistance to new ideas’ are concentrated in top journals, but that, actually, ‘the most innovative work tends to feature in the top journals’.

Samuel is specific about the changing content patterns in the reliance on peer review—which strongly reverberate the notion of an increasing formalisation. Even though, overall, Samuel sees peer review to be not terrible, he laments

the fact that reviewers tend to require so many alterations to articles. I think that the overall effect of that is a certain homogeneity of article. I lament the fact that articles have got longer- so the main length was you know I would guess- a little under 20 pages when I first started writing- and now it’s probably about 30. [...] it’s very hard now to place a piece that doesn’t have 10 pages introducing the debate- and I think that’s often just a waste.

Clara confirms a feeling of conformism, for instance, in writing where any creative approach will be stopped systematically. Berta voices her concern about ‘canonisation’ of works because of standardised selection practices. Similarly, Omar criticises the problem that ‘there are higher standards in places at the very top journals’ which makes it ‘harder to get weird stuff published in those places and sometimes the weird stuff is the interesting stuff’; as Penelope says that

I do think in order for some innovation to take place it’s important that there are journals and publishers who aren’t super prestigious- who haven’t super big names- but who publish things that are a bit on the margins.

She further outlines that

the more original you get and the more you sort of question these frameworks the harder it becomes to publish- especially to publish well. And so- people engage in these nit-picking discourses a lot of the time and it’s really unhelpful I think.

Gerald reiterates the question of whether journals still facilitate the environment so that the innovative contributions actually reach relevant audiences. And Melissa claims

strong status quo biases. So that if you have a sort of very unorthodox piece- it's very difficult to get it published in a- in its first sort of incarnation in the top end journal.

Her words sum up the ambiguity of the debate where selectivity is a 'double edged sword': there are certain threshold conditions which are good and necessary and, in addition, there are 'unfortunately other- not necessary conditions' which prevent actually good contributions from being published.

Passive Selectivity

Another theme about the scholar subject emerged in the interviews, which connects to the passive selectivity of a venue. Richard, for instance, claims that selectivity must also be seen as an issue of perseverance: publishing with less-esteemed brands is simply easier, both in terms of the perceived value of the content and the effort a scholar has to put into the submission. If a scholar's decision relies on the wish to have a book published quickly, the less-esteemed brands are certainly the better choices (which is a reminder of the temporality in the REF publishing decisions). Similarly, Melissa raises the issue of 'desperation': the urgent need to just have a contribution published somewhere without going on trying to get into one of the high impact venues.

Don articulates the potential issue that an author is simply uninformed about publishing venues and, therefore, her high-quality content is published only in an odd, niche journal or an obscure venue of a lower stratum. At last, the overall quality of the field also needs to be considered when the selectivity of a venue is in focus: Clara does so as she talks about her experience as an editor-at-large at a history journal in Germany. She

articulates her feeling that the competition for publications is not extraordinarily high; it rather is the case that if an article is qualitatively good—in a general meaning of the term—then it will be published.

10.2 Categorising Venues and the Consequences of Efficiency

A Discourse-Pragmatist Conception of Publishing Venues

I defined the humanities as a cluster of discourse communities in chapter 7. Non-material discourses—scholars' epistemic sayings—require a materiality, a communicative locus of exchange. Looking at practices of teaching, the materiality is only the space of discourse, e.g. the lecture room in which oral discourse takes place. At the end of each lecture, the materiality ceases to be a referent to this specific discourse, making teaching a materially-fragile practice that is, unless a diligent student takes notes, rather elusive in nature. This allows teaching to be more direct, though, since practices are shaped only by communicative norms and low material boundaries.¹⁷

Turning from teaching to publishing, I suggest a *discourse-pragmatist conception* of publishing venues as an analytic category. This means that a publishing venue is the central site of the discursive practices of a community. Since a scholarly community centres around discourse about specific epistemes, the publishing venues the members of such a community use for their discourse has a constitutive effect for the community. Inclusion

¹⁷ The many lamentations and grumbings about digital facilitation of teaching during the COVID-19-induced crisis very well illustrate the benefit of direct communication in lecture rooms, since the materiality of online teaching, as practical as it is during a crisis, creates hurdles for discussions or even monologic exchange.

and exclusion of venues means inclusion and exclusion of both the discourse and, hence also, the community.¹⁸

The practices of publishing leading to being published at a particular venue mark the borders of discourse: its epistemes, its formal form, methodologies, language, reference styles, etc.—in short: what is discussed, how this is discussed, and in what ways it is presented is continuously developed and reproduced through publishing, in circular reflection with practices of readership. In turn, these borders and their continual development mark the development of a community, both discursively and epistemically as well as in terms of its members.

Thus normatively conceptualised, venues have not necessarily any added reputational impact since they are artefacts as media (in their sense of transfer) and not artefacts as referents to prestige hierarchies. Discourse and subject-related values of venues are almost the same, like two circles in a Venn diagram where the common ground is nearly all there is. The discourse-related value is simply that of being a locus of discourse, it positions the venue as an optimal guide from author to community. The subject-related value is that of individual recognition. It is this value that is often referred to in Bourdieu's sense of symbolic capital. And it is this value that wreaks havoc with my pragmatist conception since the symbolic value an author may gain from a publication has become, partly, dissimilar to that of discourse. In the analogy of the Venn diagram, the two circles move further apart from each other, leaving in their midst a somewhat smaller common ground. The question is: why is this the case?

¹⁸ In praxis, a community has no definite borders just as there are no singular venues for any one discourse; venues, discourses, and communities overlap. Each scholar takes part in several discourses with increasing career development as well as through interaction with several venues. This complexity is simplified here for clarity of the argument.

The Reduction of Complexity

Confronted with abundancy, scholars seek to reduce complexity. They may not do so actively; it is ingrained in the practices: once in the information practices and again in the valuation of individuals. Broadly speaking, the former seeks to determine *what to read* while the latter results estimates of the *merit* of a scholar. Both are matters of efficiency in different guises. And though they are imbricated, circularly reinforcing each other, they have to be recognised as two aspects to really understand them.

Looking at the matter of abundancy first: there are myriads of venues and ever-increasing numbers of scholars. It is notoriously hard to determine comprehensive quantifiers about the growth of institutional positions and publications in the humanities; bibliometric analyses tend to focus on scientific disciplines or citation analyses (see the review study: Franssen and Wouters, 2019). Across disciplines, there are more than 40,000 active scholarly journals, growing annually at a rate of 3-5% (Johnson et al., 2018: 25); Web of Science's Arts & Humanities Citation Index alone counts more than 1,800 active and indexed journals in 2020 (Web of Science Group, n.d.), with thousands of non-indexed journals in their official shadows, since this index has rather strict terms; the number of book publishers and especially their branded venues such as imprints and specific series is much harder to ascertain, though it ranges in the thousands as well (Giménez-Toledo et al., 2017). Consider also how Wood finds an exponential growth of PhD theses overall in North America which is exceeded in its pace specifically by arts and humanities disciplines; these show both a sharper rise after WW2 and a sharper decline during the massification of university education in the 1970s, compared with social and natural sciences (1988).

Fohrmann summarises the development of publishing practices for German philologists in regard to an increased quantification (2009): within the 50 years since the 1970s, scholars have had to develop successful publishing lists already during the PhD,

contribute to edited collections, turn the PhD thesis into a monograph (and the *Habilitation* into a second). This increase of publications of the individual is escalated by a boost of the numbers of scholars entering the field. Jehne (2009) echoes the surge in output for the discipline of history in Germany as does Boehm (2009) for art history. Rescher also reiterates the surge of output across epistemic genres in philosophy (with a perspective skewed towards the US but addressing the German philosophy audience): ‘the output of philosophical publications has by far surpassed the increase of members of the profession’ (Rescher, 2019: 750; translation by the author). Marquard claims that, even in the smallest definition of one’s field, there are too many new publications today (2020c: 246). Both my interviews and the exploratory study (figures 47/48) further confirm these accounts of too many.

Choosing What to Read

Consider the information practices leading up to reading choices. Every choice of reading is bound to criteria—if not, choice would be pure contingency (though by referring to criteria, I do not imply agents of rational choice). Of relevance here is the rationale that underscores these criteria and which, by means of information practices, translate to authorship and publishing practices. As is evident in the interviews, and probably in most reader’s own experience, thinking about what to read means evoking a symbolic stratification of venues that helps make decisions—and this stratification shapes publishing practices, too. I aspire to be published in a way guaranteeing my text the highest visibility among the optimal readership—a readership as I know, or think to know, it from my own reading practices.

This means that scholars arguing that their field is becoming too crowded with publications (for instance: Marquard, 2020c; Quine, 1974; Rescher, 2019) require some sort of differentiator beyond epistemic categories. As in the case of Rescher, there are too many studies on Kant. Correspondingly, the discourse community, let us call it *Kant Studies*, grows in a way that their communicative locus is becoming too crowded for individuals to follow. Nevertheless, scholars have to stay up to date. As a result, discourse becomes stratified as some publications are deemed more useful than others, say *high*, *middle*, and *low impact*. This will be practically useful for the busy scholar working on another critique of one of Kant's concepts, since she will find among the many such works already produced and published immediately those with *high impact*—those works that, in her perspective, will help her own scholarship the most.

Of course, such categories can develop epistemic differences as well, or they may be hidden *as* epistemic differentiators. Culture principles of excellence and internationalisation enforce a hierarchisation of epistemic categories as scholarly work is mediated by a top-down value consensus of esteem. Furthermore, to be working on noble ideals of democratisation or classical philosophy may likely be more symbolically rewarding than to work on, say, digital phenomenology; researching in a national archive for evidence about international relations may have more reputable impact than the history of local communities in Northern England.¹⁹ This relates to the distinction that international impact matters (as made explicit by the REF categorisation) as well as to the elitist categorisation that traditional disciplines in the humanities are more esteemed than newer ones. Di Leo discusses such a disparity between two humanities: the noble and esoteric traditional disciplines (for instance, philologies or history) on the one hand and the applied

¹⁹ To be sure, these are illustrations; I do not imply these hierarchies are justified or true.

and exoteric ones on the other (for instance, media studies) in the US context (2013: 17-21). The predominantly bourgeois oriented elite institutions favour the former, while the somewhat proletarian mass colleges favour the latter—a separation that has not been in place in Germany up until the recent drive towards excellence. Embodied in these distinctions is the culturalised meaning of scholarly work. Pertinent to my analysis is that non-epistemic stratification works like an overlay raster.

The Reciprocity of Material and Symbolic Value

This raster becomes more complicated as the stratification turns into personal gain, which is where the focus turns from content to subject. The overlay raster of non-epistemic stratification is no news to anyone familiar with brands or media more generally—ubiquity becomes stratified by its users. This matter is somewhat more complex, however, as the scholarly subject gains a personal advantage by claiming a surplus of symbolic value through her personal publication record. This is best described by the Bourdieuan symbolic capital, with some theoretical reorientations.

Bourdieu developed the concept of symbolic capital and the way materiality is tied to social meaning in a variety of works. Most fundamentally, '[s]ymbolic capital is capital with a cognitive base' (Bourdieu, 1998: 85). Bourdieu draws on prior anthropological studies and the Weberian unification of the 'two opposed conceptions' of 'the objectivity of material differences and the subjectivity of representations' (2013: 294). This results in an appreciation of symbolic alongside material value, a value that is constituted by social belief and practices of consecration, not inherent in materiality but equally given because of the social embeddedness of the material. In other words, there is *always* another dimension to

the objectifiable materiality of economic capital (or of practices and artefacts). And this is the symbolic dimension.

The symbolism is a matter of consecration of content, a legitimisation based on the *illusio* of the specific scholarly community. Where Bourdieu asks ‘who is the true producer of the value of the work’ and ‘what authorizes the author’ (Bourdieu, 1980: 263), he relates to the mechanisms of consecration and legitimisation constituted by social belief. A community’s members collectively objectify scholarly work. They partly authorise venue and form—and, thereby, content—through engagement with a venue. Authorship and publishing practices, therefore, produce and reproduce the consecratory impact of venues.

Consecration involves legitimising content so that the content *becomes* what it is by means of its positioning in the strata of venues: the venue prophesies that *this is worth reading* which constitutes part of the quality itself—a self-fulfilling prophecy. The concept of self-fulfilling prophecies was introduced by Merton (1948) and has since then become a strong referent. Most basically, it refers to special cases of social performativity where a belief has ‘consequences that make reality conform to the initial belief’ (Biggs, 2009: 295). A pertinent example is that arguments put forward by well-known scholars are perceived more aptly and, because of this, the audience is more inclined to build on it or even believe it (as opposed to arguments made by more obscure scholars). This is a ‘self-confirming process, making for the greater evocative effect of publications by eminent men of science’ (Merton, 1968: 62).

An effect similar (or, rather, compounding) to the names of *eminent women and men of science* have venues: well-known publishing venues tend to create a confirmative effect for their content. In addition to the consecratory impact that a venue has for text as a legitimate contribution to discourse, the more well-known the name of such venue is, the better an argument or theory is likely to fare in a given discourse. The venue *prophesies* (or

vouches) for the immanent (non-symbolic) value of the content and, because the audience believes the venue, the content is accorded value in advance. In turn, because of this meaning-making, venues can enforce argumentative agendas.

The applicability of this concept can be seen also in other areas of scholarly publishing: Marques et al. scrutinises the tendency of favouring articles above all other forms of submissions to UK's REF to be a self-fulfilling prophecy. Underlying this tendency is the belief that articles are 'the most valuable form upon which reputation and funding are distributed' (Marques et al., 2017: 834); a belief that influences publishing practices so that, in the long term, articles fulfil their assumed performative role. Such an (un)intended consequence of institutional design is also visible in rankings where distinction is created by the ranking so that the ranked entities are perceived to perform better beyond the statistical marginality (or beyond the attributes) on which the ranking rests (Espeland and Sauder, 2007). Such a ranking is created to reduce complexity by actualising formal rationality—while most of what it actually does is create a one-dimensional fiction which becomes the new reality subsequently (Espeland and Sauder, 2016).²⁰

Reorienting Bourdieu means: this symbolism of venues *requires* its material referent as well as the social belief in this symbolism. This has important implications, but it is often

²⁰ As a matter of fact, the global health crisis resulting from the spread of the novel coronavirus (COVID-19) is perhaps the most pressing example for the inappropriateness and explanatory insufficiency of the generalising and falsely objectifying rankings. The Global Health Security Index is supposed to be the authoritative and annually renewed assessment of the world's countries' health systems. With criteria responding to scientific theory, evoking the chimera of robustness and reliability, and paired with the reputation of the issuing and supporting institutions, this index positioned the USA first and the UK second. The index's primary task of making a statement about a country's responsiveness to potential health crises such as a global pandemic, therefore, declared these two countries as optimally—best in the world—positioned to react to the spread of the coronavirus. And yet, the two countries failed to react anything but appropriate. They, in fact, 'have provided among the world's worst responses to the pandemic' according to Dalglish (2020: n.p.); see also, *NEJM Editorial*: n.a. (2020). The pre-defined categories were ill-equipped and the ranking's one-dimensional output nowhere near informative as it neglected social contingency. To issue such a ranking might not just be inadequate: its deceptively clear prediction can fare as a predictable risk.

missed in existing literature. Thompson discusses different forms of capital available to publishing houses: symbolic capital is presented as one among other capitals, as ‘the accumulated prestige, recognition and respect accorded to certain individuals or institutions’ (2005: 32). Observe also how, perplexingly, in the practices of successful literary agency, intermediaries can ‘transform social capital into symbolic capital’ while they, instead of enjoying ‘economic reward’, ‘earn legitimation, that is, symbolic capital’ (Pareschi, 2015: 412), until they are ‘recognised as a legitimate mediator’ and are, thus, more and more ‘endowed with symbolic capital’ (Pareschi, 2015: 413). Symbolic capital is presented as a standalone category next to other capitals as if symbolism is transmitted without any artefactual foundation. Likewise, Münch relates to the prestige goods of highly symbolically valuable venues as he claims that scholars seek to publish in these *because of* their symbolic value only (2011: 237). In the same chapter, Münch discusses the potential to be cited because of the significant differences in circulations of journals, hinting at material value but not explicating this materiality.

Such works insinuate that there are two separated worlds, each with their own ontology. The constitution of the value of a publishing venue—its worth (or insignificance) for published author and audience—based on its material and symbolic value is complex. It cannot be stressed enough, however, that those two relations—object/material and social/symbolic—are not two separate issues or require distinct ontologies, just as Latour claims for scientific conceptions more generally (1993: chapter 4). They are imbricated, in praxis geared towards venues just as Bourdieu originally conceptualised:

[s]ymbolic capital *is any property* (any form of capital whether physical, economic, cultural or social) when it is perceived by social agents endowed with categories of perception which cause them to know it and to recognize it, to give it value (Bourdieu, 1998: 47; emphasis added).

Ultimately, it is for these reasons that the application of symbolic capital seems to be semantically insufficient and overly generalising. Instead of capital, I propose using simpler forms such as the notion of *value*. I suggest the study of subjective hermeneutical resources informed by culture needs to look more closely at the value of the unity and relativity of material and symbolic aspects—so to say, to break the principle of two separate ontologies. Symbolic and material values of a publishing venue always need to be assessed as imbricated issues. They are mutually reinforcing in a venue's historical development. And in this sense, symbolic value is an analytical category with an 'incorporated history' (Steinmetz, 2011: 51). The esteemed venue dominates a field since it has a large audience—it has a large audience since many a scholar considers the venue esteemed and, therefore, follows its publishing development. The statements in my interviews can only be fully understood if this imbrication is acknowledged.

Starting from this imbrication, the reciprocity of symbolic and material value can vary in the short run. It can shift with technological change: the reciprocity might be *broken up* where technology and culture forces separate—or, instead, it might be *prolonged* precisely because a culture is much stronger than technology (as the Alexanderian school would correctly remind us). Consider how OA or informal dissemination processes such as through preprints allow for much wider reach of text in principle, but the information practices embedded in a scholarly culture of trust in the legitimation of the formal form may prevent this wider reach to matter. Thus, just as culture and technology are reciprocally affecting each other, they unite in an artefactual ontology that must not be separated during critical assessments. I argue that only thus can the debates about OA be fully understood (I pick-up this matter again in 9.4).

As practice is the site in which culture is produced and reproduced, through practices, the publication artefacts become mediated by the culture that values scholarly

esteem in specific, collective senses. The accumulated positive decisions of fostering the dissemination of scholarly achievement, visible in a publisher's list and programme, reinforce the value of today's list and constitute parts of the authority of scholarly legitimisation. Not to be neglected and going hand in hand with this, the visibility and discoverability a venue creates—the audience it constantly fosters—has a considerable non-symbolic material value as the communicative locus of an epistemic community: and this, as well, constitutes parts of the authority of consecration (in the sense of reflexive symbolisation). It may well be useful to have an esteemed brand on a scholar's personal CV, but this symbolic value is always accompanied by the material value of being visible (and, thus, being invited to give a talk, being discovered and cited, being invited to collaborations, etc.).

The notion of visibility is crucial as it transcends mere symbolism derived from reputation. The ambiguity of symbolic value expressed during my interviews connects to this matter of visibility. This visibility refers to the materiality of the venue without which its imbricated symbolic value cannot gain impact. Look at the JIF in this respect (albeit its general insignificance for the humanities): despite its discursive connection to reputation, the JIF nevertheless is a referent with both symbolic *and* material value. Its entire constitution is a statistical residue of material reuse and, as this statistical reduction is seen as a problem, mere symbolic value can only be a driver but not the foundation of the problem (see for extensive discussions of the reputation/JIF issue: Brembs et al., 2013; Brembs, 2018; Lariviere and Sugimoto, 2018; McKiernan et al., 2019; Vanclay, 2012).

Connecting to statistics, by now, the humanities encounter the inclusion of metrics as well—despite the principled rejection of their use, as is largely confirmed also in my exploratory survey (figures 20-22). Such metricization highlights the culture shift towards efficiency that stands in close relation to managerial principles of NPM. The increasing

reliability on ratings, metrics, and seemingly efficient reductions of complexity abounds in both modern society and academia (Mau, 2017, 2020; Muller, 2018; Münch, 2008). This reinforced efforts of analysing publishing practices in relation to such metricization, efforts that build upon a sociology of visibility and evaluation (Hesselmann and Schendzielorz, 2019; Krüger and Hesselmann, 2020; Nicolae et al., 2019). And in a similar line of argument, Franzen finds that NPM induces externally referential criteria that draw scholarly practices away from internal, epistemic references (2014: 389–393).

In the form of a hierarchisation of publishing venues, scholarly output becomes ranked and, thus, relationally rated. Even if being ‘simplistic, obscurantist, inaccurate, and subjective’ (Esposito and Stark, 2019: 11) to varying degrees, rankings and ratings provide a reference point for decision-making in a world of uncertainty. They appear as a reduction of risk and complexity by evoking a sense of trust. Prior to all rating, however, is the need to compare (Heintz, 2016). Comparison means opening categories in which entities can be brought in relation to one another.

Dialectics of Efficiency: The Direst Consequences of Rankings

This stratification—the symbolic value of venues vis-à-vis its materiality—invites ranking. In other words, an implicit raster becomes manifest in a particular form as a quantified, one-dimensional hierarchy. This is where the formal rationalistic culture principles of comparison and efficiency turn on venues most pertinently, and where, reflexively, impact on publishing practices is evident most crudely.

The construction of hierarchy anticipates a usefulness not immanent in the work, but in competitive distinction; just as the rhetoric of excellence posits quality primarily as a relative instead of as a substantive matter. In the high strata of a venue hierarchy—the high

impact brands—rejection rates can be as high as 90% (Crane, 2018; Hargens, 1988; Suber, 2005). Coming back to the example given above: if ten new studies of Kant are submitted, only one will be accepted irrespective of their potentially very similar scholarly questions (answered each in its idiosyncratic way). These categories are in place for the reproduction of hierarchy, not for epistemic differentiation.

This can be seen in what is reflected in, for instance, the *Leiter Reports*, the ranking of impact of journals in philosophy (2018). Providing a hierarchy of 30 top journals in philosophy, the report is based on a poll that accumulates the perception of 636 individuals (in 2018). Indicative are the comments to this list, where a scholar writes that there ‘is obviously a stable prestige hierarchy in the profession, which polls like this reliably reproduce’ (in: Leiter, 2018: n.p.). What such a comment refers to is that this ranking reproduces and explicates the stratification of venues that is commonly held but usually only implied in practices. Another commentator summarises the perplexity some scholars have when confronted with such prevailing structure, where prestige accorded by a community meets characteristics of personal (or scholarly) values:

I wonder whether the top five journals or so are overrated simply because they have 95% rejection rates? Do people confuse exclusivity with quality? Most certainly. Do those journals really publish better articles? Hard to tell personally. It seems to me that very good articles appear in all of these journals (Leiter, 2018: n.p.).

The value of venues is culturally ambiguous. In such a statement, and likewise in most of my interviews, the ambiguity scholars relate to venues becomes legible. They know about the hierarchy but claim to judge content by itself. All the while, they do not have time to read everything so that they require a reduction of complexity in advance. In praxis, scholars thus reproduce the principle of consecration of content based on the venue it is published in even though they may aspire to give epistemic categories priority. This, therefore, is again an alternation where differing culture principles come to bear on

unstable practices: requirements of efficiency, somewhat more indirectly here, enforce a formal rationalistic processing of information which is, traditionally, bound to culture principles of humanistic deliberation as qualitative judgement. Scholars aim to take the time to judge content and scholar qualitatively, but they feel forced to make use of shorthand practices that are available anyway. The reference of *this has been peer reviewed* symbolises achievement and claims of a justified contribution to knowledge in the shared meaning-making of contemporary academia (Heesen and Bright, 2020: 2). This culturalised symbol is likewise fundamental in the constituting of my notion of the traditional publication.

It can be taken for granted that scholars constantly and mostly implicitly compare each other by reading each other's works—similar as they relate their work in writing to others, they find others' works in their historical or epistemic context. This is a qualitative contextualisation that contributes to discipline-specific hermeneutics. But by doing so, do scholars also process a hierarchy? One might find a contribution exceptional or another piece of poor quality. But it seems unlikely that the hermeneutics of perceiving and reusing text involve a strict hierarchisation. Such a top to bottom relation is, much rather, to be seen as an external heuristic, a layer before the content that influences what is being read in the first place. The matter of trust in the symbolic form—part of the field's *illusio*—is one of choosing what to read, for it makes materially visible what is worth reading. Krüger and Hesselmann comprehensively discuss the different forms of how visibility is constructed by means of evaluations. In their formalised form, practices of evaluating contributions and subsequently signposting outcomes through visibility is a reduction of complexity for audiences. For those being evaluated—authors—the need to be visible creates the 'permanent competition *for* visibility' (Krüger and Hesselmann, 2020: 157; emphasis in original; translation by the author).

This shows a turning point from being visible in order to exhibit your ideas, to deploying ideas in order to becoming visible. It is this switch of competitive terms that ingrains in practices the matter of *visibilizing* as a means to gain symbolic value and consecration—symbolism employable as surplus value for the subject, and symbolism showing others that this is worth reading. The agonistic nexus takes place before making content visible to one's epistemic community; that is: *as part* of publishing.

The matter of visibility indicates, thus, the very shift from content to package, from substance to formal form. I argue that this is an enforcement of an alienated condition in the very sense that the symbolic form and the need to *visibilize* becomes the practice's focus. It alludes to Jaeggi's *scholar publishing for the index* as well as to Harvie who discusses the practice of perceiving publications in terms of their ranked, symbolic value as a form of alienation, too (2000: 114). This alienation can be read in my interviews in the back and forth of screening CVs and reading text, of the ambiguity between determining quality in either package or content. This is an ambiguity in the *illusio* and it particularly questions *why* scholars publish where there is neither reward of visibility nor connection to niche epistemes—a questioning of the long tail of the low end of hierarchies.

The alienation is a systemic force here to which the matter of publishing more and more contributes. This drives a dialectic inherent in efficiency: this shift is both a necessity and a problem in recursion. The necessary condition is the one explained above: too much choice and too little time to read it all require means of efficiency. These means are exemplary for formal rationalistic principles and they show the whole range of ambiguity where different principles are confronted in culture shifts. The formal forms can well be aligned with epistemic communities: the highest impact journal in the historical discipline might be just the one really benefitting readers of general history the most. That large university press might indeed be the one with the most reliable content across discourse

communities. In other words, the means of efficiency can be well bound up with epistemic communities and their co-created symbolic forms.

And yet, the ambiguity identified suggests there is a discrepancy and the notions of unread publications even reinforces the impression that a discrepancy between readership fit and overall value of venues prevails. Firstly, it can be argued that, as high impact symbolic forms are sought, conformism is systemically invited (a problem to which I turn in the following sub-chapters). Secondly, hierarchisation means that, irrespective of epistemic community, competition turns into a single—or singularising—mechanism. Corresponding discourses become eroded as their symbolic values are lined up and publishing practices become geared towards this singular line instead of to the differences of communities. It is a dialectic of efficiency that, by recursive iterations, reaches an antithetical quality where its rationale of efficiency turns into an irrational rationality—where necessity turns into the making of problems on its own.

Here, competition *for* visibility contributes to a circular reinforcement that, eventually, leads to what I propose to call a *growth/trust spiral*. Looking at the matter of growth first: in his essay on the serial crisis and academic output (in terms of journal articles), Baveye analyses the risk of growth of scholarly and scientific output over recent decades. This cannot simply be attributed to growth of populations alone. Competitive funding regimes and career developments based on quantities of output further incentivised increasing numbers of publications across disciplines and countries. Baveye himself calls attention to the problem that scholars have less and less time to read in depth (2014); he points out the trigger to this problem in an earlier text where he concludes:

[t]he direst consequences of these trends will likely be felt by researchers themselves. They will not only have to find more and more time to peer-review manuscripts, aggravating an already significant problem, but also have to stay abreast of substantially more articles than are currently published (Baveye, 2010: 204).

Crude growth turns into the need for trust (the matter of consecration) and, thereby, competition for entering these trusted venues (the competition *for* visibility). This need is ingrained in contemporary publishing practices but influenced by cultural particularities: the local historian has a different perspective on trusted sources/visibility than the cosmopolitan English philologist aiming to address a worldwide audience. Mediated by their particularities, they engage in practices that requires them to assess their own work in relation to a competitive potential. This is not simply a competition for symbolic forms or even a struggle of the better argument: a scholar needs to make her contribution visible within the masses by means of entering a trusted source—these masses, however, are not the masses of contributions (which could be sorted and categorised epistemically) but venues (which are stratified and, therefore, partly simply invisible). This invisibility is the *real* death of the author: Baveye concludes that the crude increase in productivity results in the scenario that the output that was supposed to justify the humanistic endeavour turns to be a hardly justifiable endeavour itself, as scholars write ‘a plethora of journal articles, 50 per cent of which will never be cited’ (2010: 210).

Hence, as a consequence of these *direst consequences*, the growth/trust spiral is triggered: growth of submitted contributions allows for a growth of venues that requires enforced venue stratification so that scholars can rely on a trusted set of venues for their daily scholarship. This stratification enforces mechanisms of visibility and mass publications that trigger further demand to publish: to stay visible. A side effect of this primary spiral is the ever-increasing workload accompanied by it: growth of both venues and submissions leads to a growth of the burden of peer review which cut out significant time resources—which, in turn, induces the feeling of the need to again rely on established sources for one’s daily reading since you do not have the time to read beyond those few sources.

This spiral articulates how the effects are circular unintended consequences: if there were less venues (considering the rejection rate would stay the same), there would not be as many options to publish for quantity and the focus on quality might have to increase, so that, in effect, the stratification of venues would not have to be in place in that manifest form, or constantly reinforced, in the first place. The single publication might be attributed more value again, a value in substance, not in relation. Heesen and Bright come to a similar conclusion regarding such a spiral and they argue, sympathetically, career decisions would be bound to reading and qualitative decision-making again (2020: 14).

Concluding this section, the rather bleak scenario is to be faced that the means with which academia is said to become more efficient (stratification of venues) is partly responsible for circularly enforcing the inefficiency of academia (the crude growth of output as part of the competition to being visible). The problem of growth, thus, cannot be *rationalised away*. The assessment of publishing practices in relation to venues is very much a study of unintentional consequences of formal rationality which develop a substantive irrationality. That the symbolic value of venues is given so much attention blurs the roles and purposes a differentiation of venues and bibliodiversity can have for scholarship.

10.3 Practices of Selection

In the preceding sub-chapter, I presented my understanding of venues which helps comprehend the meaning of practices vis-à-vis artefacts. As with the original Weberian understanding of social practices, they are routinisations of shared meaning. Understanding this meaning, therefore, is the basis for understanding practices dealing with publishing artefacts. In the following sub-chapter, I turn to how this meaning is negotiated in practices of selection. These practices further clarify some of the issues touched upon above in more practical terms.

Selectivity is often thought of as gatekeeping by means of editors or peer review. And yet, the selectivity of a venue cannot be reduced to its own practices of active selection: selectivity is always already embodied in the (desired) publishing decision-making of authors on the basis of their subjective and culturally objectified reflection of the venue. This was pointed out repeatedly, though mostly implicitly, during my interviews. In other words, selectivity is two-sided: scholars select, and venues select.²¹ As their inverse selections meet, symbolic value is constructed.

Author Selection

In her extensive study of *how professors think*, i.e. review other scholar's work, Lamont (2009) highlights comprehensively the culture aspects of practices of inclusion/exclusion particular to the humanities. Practices performing selection gear selectivity towards notions of scholarliness and quasi-objectifying ideas of originality and rigour (see for such notions in the humanities also Lamont's other work in this area in: Guetzkow et al., 2004). This culture dependence is the basis for claiming that what counts as scholarship is constituted within a scholarly community, just as Bourdieu has earlier asserted (1975: 24–25).

Selection determines the *within*: not only the inclusion in a certain stratum of the hierarchy of publishing venues, also the inclusion into the community of scholars as such is decided. In their first manifestation, practices of selection comprise of the routinised decision-making of authors themselves. In their second manifestation, practices of selection comprise of a mode of gatekeeping where an editor or reviewer selects (or advises to select) a contribution for publication from a pool of submissions.

²¹ The fact that scholars select does not, of course, mean that they are successful: they choose to publish somewhere but might be rejected (or invited to revise and resubmit).

An author's desire to publish with a particular venue may be called a venue's passive selection. It is passive because a venue mostly influences this indirectly by means of the historical development of reputation and audience—symbolic value and visibility. It is only active in case of active acquisition of already known authors, as was referred to during my interviews. Passive selection is often overlooked as it is, in other words, publishing *per se* from the perspective of the author: it is the non-technical practice of publishing in the form of a decision-making to wish to be published and to do so in a particular form in a particular venue.

An author's selection is essential since it is much more selective than any journal can be with its gatekeeping practices. The author's desire and the venue's gatekeeping influence each other circularly. The desire to publish in a venue held by a substantial mass of scholars in combination with this venue's selectivity creates scarcity and further attention. Neither can go without the other. As Baveye articulates:

[e]xperience suggests that a decision to be more selective, if taken unilaterally by one journal, would only draw increasing numbers of submissions to that particular journal (because of its higher selectivity) and, in the end, simply lead rejected manuscripts to be submitted (and eventually published) elsewhere (Baveye, 2010: 205).

The disintegration of discourse community and publishing venues is reflected in terms of two-sided imperfect information. As discussed earlier, scholars have to constantly compare themselves with others in the field. The resulting subjective uncertainty (or overconfidence) of a scholar seems to be a largely unaccounted of aspect of publishing within existing research: self-doubt about one's own ability on the one hand, and unintelligibility on the other. Both of these aspects of subjective uncertainty have been raised during my interviews. The former aspect is more intuitive than the latter: if, for instance, I am in doubt about the quality of my own work, I submit to a lower stratum of venues irrespective of the receptive quality in review or in readership (and irrespective of

the fact that a well-meaning review may have allowed for a highly-rewarding consecration in a higher stratum). The aspect of unintelligibility refers to the issue that, independent of content, authors might not even know about venues of the higher strata or do not know about the consecratory impact of venues altogether. Such authors simply submit to what appears to be most fit on account of imperfect information.

Imperfect information is innate to publishing practices and always a question of degree: no author (or reviewer) can apodictically presume the receptive quality of her contribution and neither does any author know about the full gamut of venues and their receptive or material values. The collective is, in those instances, more intelligible than the individual since both the stratification of venues as well as the reception of text (as expressed through the collectively created network of references) come to existence fully only through the discourse community. With many venues and publications at hand, the unity of discourse communities disbands. This is enforced by the hierarchisation of venues. Since, because of this, it becomes ingrained in publishing practices that each decision is also a political decision potentially impacting subsequent career or funding selection. Applied selectivity, thus, also articulates the shift from communicating to strategizing.

As Lamont rightly claims, ‘the “cream” does not rise naturally to the top, nor is it “dug out” in unlikely places: it is produced through expert interaction’ (2009: 241), selectivity as a desire to be published is a critical issue. If the practices of peer review indeed have an improving impact on contributions (as opposed to only being a form of gatekeeping), the question of whether a scholar dares to submit to a higher stratum of venues decides about this *expert interaction*. Since it can be more likely that reviewers are more intelligible about the scholarship in a field and that practice of reviewing can be more robust with high impact venues, the interaction of author and reviewer might be more fruitful for the contribution (even irrespective of being accepted with this particular venue).

If, because of doubt or unintelligibility on the side of the submitting scholar, she does not even dare contribute to this *impactful* venue, she will fail to benefit from claimed *expert interaction*.

This selection as the wish to be published is further bounded by active acquisition, as already mentioned. As senior scholars in both the UK and Germany articulated during the interviews, they are increasingly less involved in deciding where to publish since they are constantly asked to write for special issues or editions. These author's selectivity is, in a way, undercut by the selectivity of venues and their editors. To be sure, senior authors could still decline to contribute. Irrespective of this, it seems unlikely they have the time to choose to publish freely on their own next to all the requests, especially in regard to special issues. This is also reiterated in the literature. Jehne, for instance, expresses the skewed development that younger scholars are increasingly overrepresented in general journal literature as senior scholars become *absorbed* by requests to contribute (2009: 61), often concerning special issues.

This results in a peculiar empirical situation: on the one hand, peer review is said to be *the* practice of selection; after its establishment in the natural sciences for centuries, it is now also pervasive in the humanities. It is claimed to make selection more meritocratic as inclusion/exclusion is based on the assessment of work alone instead of on networks. On the other hand, however, peer review is undercut systematically (though not necessarily intentionally) by senior scholars, surely with variations across scholarly fields. This results inevitably (again) in the impossibility of said meritocratic ideal.

Because of this, looking at how the selectivity of publishing practices is an articulation of a rationalistic imperative to publish in a certain way, thus reinforcing publishing pressure, is likely to be a perspective that focusses on non-senior scholars. This only highlights the strong evidence for a skewed distribution of pressure among scholars in

both the UK and Germany dependent on seniority, as is visible in my exploratory survey (e.g. figure 36). Publishing pressure as the strongly knit nexus of experienced imperatives, the iron cage of required, formalised proceduralism, is emphasised here as the constant testing of one's qualities which takes place not in discourse but already in the practices of selectivity before public discourse. Whether or not an argument is being debated publicly is, as is evident with the matter of consecration, partly decided already before it had the chance to enter discourse. This reinforces the argument about strategic publishing put forward in the preceding chapters: strategizing involves accounting for matters of selectivity which result in matters of visibility and symbolic value.

The strong sense of calculability here articulates, in a more comprehensive version than before, the instrumentalization of publishing irrespective of epistemic means of discourse. And it shows again how mere necessity to handle ubiquitous amounts of information efficiently subsequently has an impact on the meaning of the practice and, thus, shapes it. To further understand this, it is worth looking in more detail at the other side of selectivity: practices of gatekeeping.

Peer Review and Editorial Decision Making

There is a growing body of research on the active selection of venues (for instance: Allen et al., 2019; Butchard et al., 2017; Caputo, 2019; Crane and Ryan, 2018; Eve et al., 2021; Fitzpatrick, 2011a; Hesselmann et al., 2020; Hirschauer, 2004; Müller, 2008; Ross-Hellauer, 2017; Tennant, 2018), but overall, it is still largely a black box, as discussed by Tennant and Ross-Hellauer (2020). Babor et al. suggest that 'the process by which a journal decides to accept or reject a given article has been mysterious' (2017: 37). A similar conclusion can be made for books, if not an even more mysterious decision-making might

be found here. The claim that a venue executes peer review can hardly be topped by ambiguity regarding the actual decision-making involved. The varieties and applicability of practices that *can* be at work in the guise of peer review is vast.

There is no *one* practice of peer review. Most abstractly, peer review may be considered the standardisation of the collegial assessing of scholarly work as a support for the informed decision-making of editors. It was historically much more important in scientific disciplines than in the humanities. The term originates in ‘debates over grant funding’ in the 1970s ‘and has since been extended to cover a variety of processes by which academics formally evaluate each other’s work’ (Fyfe et al., 2017: 11). However, practices of peer *refereeing* are much older. They can be traced back to a few scientific journals and their learned societies in the eighteenth century. They developed as

distinctive editorial practices of learned societies [which] arose from the desire to create forms of collective editorial responsibility for publications which appeared under institutional auspices (Moxham and Fyfe, 2018: 866).

The importance here lies in the plural. Peer review was not established as a singular practice, nor did it evolve linearly into a standard one. In the nineteenth century, learned societies already exhibited a variety of approaches to refereeing and this was framed as constructive criticism, major suggestion, or mere recommendation. It developed to become a key gatekeeper—still accompanied by the decisions of editors—and cultivated a number of practices such as blind refereeing or financial compensation for the increasingly heavier workload of referees. Over time, its purpose was transformed

from a public foil to set off and amplify the very best of the research received by the Society in the early nineteenth century to an instrument for ensuring the application of minimum thresholds of quality across the board while allocating space (and therefore resources and prestige) on the basis of expert assessment (Moxham and Fyfe, 2018: 888).

The institutionalisation of peer review as a necessary condition of publishing demarcates a culture shift as it has become ingrained in what it means to publish. It is intertwined with a process of appropriation in which commercial publishers hijacked a collaborative effort. Alongside the commercial uptake of publishing after WW2 and during the massification of higher education, the newly establishing corporations aimed to outsource parts of the editorial process to utilise the voluntary practices of learned societies and university presses. On the one hand, this helped them 'legitimise their journals as venues for high-quality original research' (Fyfe et al., 2017: 12) and interlinked the rapidly evolving number of venues with communities. On the other hand, this enforced the acceptance of peer review as a threshold to quality and authenticity, and, that by organising peer review, publishing venues themselves could claim to add non-technical value to the publishing process. This appropriation is also based on economic demand: growth of subscription revenue comes about via increasing attention which, as established above, is very much a matter of visibility and symbolic value.

This appropriation circularly enforced the culture shift as only the community's belief in the ideals of the practice and its necessity allow mediation of publishing through the prism of peer review. As the idea of deliberate contributions made to discourse is partly replaced by the *frantic dash* to receive legitimisation from the field—a culture shift from communication to personal constitution as well as from networked hierarchies to potential for democratisation—peer review became manifest as the material negotiation of legitimisation in terms of technocratic rule. Finch exemplifies how peer review is politicised and how the effective system is suggested as the optimal solution. Scientists and scholars are claimed to have

effective and high-quality channels through which they can publish and disseminate their findings, and that they perform to the best standards by subjecting their published findings to rigorous peer review (2012: 17).

Politics are essential in the development of practices of selection. They only *seem* to be an aspect of scholarship. The appearance that practices of selection are immanent to the scholarly community gives the hierarchisation of venues a patina of justification: it is scholars who determine who is accorded what place in the stratification by means of peer review. This is not to say that matters of selection have not been political before peer review was widely implemented (as Latour would notoriously intervene). The difference is that with the shift from solely editorial to communally-informed decision-making, and from a few to many venues alongside the adoption of scientific approaches to checking legitimate contributions, the cultural mediation of politics shifted. While in the earlier situation the sole editor marked the distinction as a bastion of the erudite bourgeoisie, it shifted to the matter of efficiency in times of mass education and national returns on investment.

In 1975 and arguably even earlier than that, it already seemed clear that publishing with esteemed venues meant inclusion/exclusion not just on the basis of an ideal such as merit, but on the grounds of some form of privilege: ‘most authors believe that they will never be published by a prominent house unless they are initially sponsored by a significant person or have some special advantage’ (Orlov, 1975: 51–52). Publishing scholarly books is, in Orlov’s words, an ‘old boy/girl network’ (1975: 53).

Peer review, it can be claimed, partly replaced such network-structures on the grounds of more democratic means. It can in this respect be argued that the appropriation has its merits. And yet, as concluded in the previous section on the basis of my interviews, this possibility of a meritocratic foundation through peer review is undermined by the fact that senior scholars seem to only partially have to submit their work to the dungeons of venue selectivity. To be sure, this varies in reality. But in the end, it might be a positive development for younger scholars even though senior scholars might still get in on the

basis of networks. Every scholar is allowed to submit their works to peer review—but this still leaves open the question: what happens during peer review?

Practices of Review

Irrespective of a political dimension, criticism of peer review persists for being unfit for purpose more generally. Most discussions of today's diversity of peer review practices seem to see its traditionalism as a fundamental problem, where degrees of openness may offer potential to alleviate the problem (Riesenweber, 2014; Ross-Hellauer, 2017). The characteristics of peer review and the ways it can be opened up varies tremendously, such as in terms of: purpose, visibility of peers, visibility of reviews, time of review, openness of the review process. It is difficult to assess which and how such variations affect which aspect of publishing. Since these variations are, in the short run, not systemic variations, it can only be assumed conceptually what happens if, for instance, all practices worked on post-publication reviews in order to mitigate problems of selection prior to stratification.

A most pressing problem in relation to the stratification is that it is mostly unknown which (and why) contributions are *excluded* and what is *changed* among those included. The existing literature well iterates what my interviewees likewise described. Transparency is not just an issue of openness of documents here, but of openness about the applicability of the practice and the selectors (Hesselmann et al., 2020). Ross-Hellauer discusses problems potentially surrounding peer review, most of which fundamentally affect aspects of quality and credibility. He accounts inconsistent and 'weak levels of agreement' among referees, questions the authority of their role as a gatekeeper, and issues the "black-box" nature of traditional peer review' as a '[l]ack of accountability and risk[...] of subversion' (Ross-Hellauer, 2017: 4). Most of all, the social component of peer reviewing is set against the

‘idealized as impartial, objective assessors’ on the basis of gender, nationality, institutional affiliation, or language (Ross-Hellauer, 2017: 4). Backed with (peer reviewed) studies as evidence, his review arrives at a devastating conclusion for this practice.

Particularly in the humanities, practices seem to vary strongly in matters of to whom peer review applies and how much editorial decision-making depends on it. While most journals tend to claim to execute peer review, for many publishers of monographs and edited volumes the pervasiveness of peer review as a standard seems to be more questionable. A systemic enquiry into this seems not to be available. Individual complaints are voiced about the insufficiency of contemporary practices of selection regarding their social or epistemic insufficiency (Crane, 2018; Fitzpatrick, 2011a, 2012). Katzav and Vaesen work out the problems of non-epistemically based decisions in practices of selection in philosophy in the UK (2017). They conclude that

[p]eer review in philosophy has been, and still is, subject to partisanship that is hard to justify on epistemic, moral and, as we have argued in the previous section, pragmatic grounds. Thus, altogether, there seems to be no compelling reason for keeping current review practice as it is (Katzav and Vaesen, 2017: 17).

The most pertinent aspect they emphasise is the fundamental role of disagreement in discourse and that ‘it leaves no answers to substantive issues within philosophy untouched’ (Katzav and Vaesen, 2017: 8). This goes along with what Hyland articulates about humanities discourse, which is ‘open to greater interpretation, findings are more frequently borrowed from neighbouring areas, and there are not the same clear-cut criteria for establishing or refuting claims’ (Hyland, 1999: 353). Selection of what is deemed worthy of inclusion into scholarly discourse is fundamental, since this inclusion is decisive for life chances in academia. And yet, there is a double selection and it seems telling for the culture principles of NPM that pre-emptive moderation (in the guise of peer reviewing) is emphasised so much more than the internal moderation of discourse itself.

Conformism Affecting Authorship and Publishing

Relevant to the connection of selectivity and being published is, therefore, the question of whether authors conform to standards, since this is deemed to reduce the hurdle of being selected. Franzen confirms developments of standardisation, following from the reputation-focussing cultural mediation of high-impact publishing venues (2014: esp. chapter 3), but this without focus on the humanities. The question remains whether this is indeed conformism or only formal standardisation.

Remember from chapter 6 in which the matter of internationalisation already touched upon the matter of venues: German venues are downgraded because of their supposed inferiority, a status that is ascribed to them as they publish scholarship in the German language. Journal steering committees, in response, focus editorial strategies on more international submissions. Thus, as a consequence of such visibility stratification—its constructed reality—discourse communities and their venues enforce an internationalisation as a form of conformism to retain a visibility status. Such a non-epistemic hierarchisation lacks appreciation of the ‘normative and philosophical disputes about reasonable developments of forms of articulation and understanding in the “Geisteswissenschaften”’ (Stekeler-Weithofer, 2009: 158).

My interviewees generally confirm this, which reinforces the impression of the culture shift towards strategizing publications. Only a few responses are ambiguous in this respect. As my thesis is not about content, the question of conformism cannot be answered in-depth. And yet, the theme highlights well the culture shift. For this reason, coming to the end of this sub-chapter, I would like to pick up this theme, in abstract terms, to illustrate the underlying issues by discussing the matter of reviewing through which principles of scientific rigour and objectivation are enforced in the humanities.

In analogy: I argue that a processual veil of ignorance is impossible in peer review in the humanities. I liken the process of peer review to that of Rawls' original position ([1971] 1999: chapter III, section 24), which posits that moral decision-making such as in politics should be indifferent to the position, abilities, strengths, or preconditions of that person making the decision. Applied to the judgement of scholarship: this original position would propose that any reviewer—in whichever process of determining the quality or strength of a discourse contribution—may determine quality or strength without taking into consideration who put that contribution forward—thus, disguising the origin of the contribution. As a result, judgement would be about content alone so that, in principle, the reviewer must accord her own contributions to the same standards she applies to other contributions. In the end, this is what blind peer review is about: judgement about a contribution without considering the contributor or one's own position. This process evokes the sense of merit-based achievement that prevails in both contemporary society and academia, particularly in the principles of neoliberal managerialism: she need only produce good scholarship and she will succeed in becoming a scholar, climbing up the institutional ladder alone by putting a good mind to work.

I suggest the principle of blind reviewing—the idea of a veil of ignorance in the process of judging content in the humanities—is a myth. Technically, blind review may surely work: all names can be deleted from the text so that both reviewer and the reviewed do not know each other's identity. The problem remains, however, that the reviewer knows *her own identity* and judges content on this basis (Katzav and Vaesen, 2017: 7–13). In the end, she is chosen as a reviewer *because* of her position. As in the communicative rationality that Habermas conceptualises as he says 'that objectivity of understanding is possible only within the role of the reflected partner' (1971: 181)—the partner is *reflected* since she cannot

extinguish her subjectivity. This is most welcome in discourse, but partly difficult in peer review.

In other words, judgement of scholarship in the humanities is always a judgement of scholarly position, of the belief in the right application of theory and argument, and a belief in the justification of the existence of the argument in the first place. This is much less a question of technicalities as in the natural science where, in the process of reviewing, a reviewer may be able to tick boxes about the applicability of theory related to phenomena, justified methods, and procedural steps of analysis.

Besides methodology, there is no such right or wrong in the humanities. The force of a better argument, the belief in a theory or conceptual basis, the style and aesthetics of text: judgement about these is always highly subjective. The same argument that claims content in the humanities can hardly be expressed by objectifying metrics implies that judgement can be universalised only to a certain degree. Scholars may be able to tell the quality of a contribution and, thus, to sort it into a stratum of the field. But by inherently judging from their position within a discourse, any judgement of quality always also implies a judgement of position: positions that may include differing schools of thought, differing styles of presentation, differing referential foundations, differing conceptual traditions, differing epistemological beliefs, and so forth. And to this, the politics of the field and the fact that time for practicing a review is scarce need to be added.

This is not to say that scholars cannot judge the quality of work that differs from their approach. But any judgement outside of one's epistemic grounds is much harder in a field where hermeneutics, position, and force of argument matter more than technicalities. Especially regarding diversity, digression of epistemic grounds, or interdisciplinary approaches, scholars tend to be more wary and show a conservative bias (Lamont, 2009: chapter 6).

This problem of subjective judgement and its non-universalizability pertains to the core of culture: humanistic scholarship is about difference; new knowledge is not added simply until a paradigm shift occurs—new knowledge differentiates the field. To differ, therefore, matters. But to differ also seems to lower the potential to being published: for the sake of rigour and clarity that underly the notion of selectivity, difference might be reduced. In respect to this, the requirement to get through peer review might pre-emptively enforce a certain conformism.

Plakias provides a persuasive perspective on publishing arguments in which the author might not believe (in philosophy, but this can be applicable to other disciplines with dependence on argumentative force as well). Consider that discourse is all there is so that different perspectives on arguments, even those one might regard as countering their own position, are worth being contributed (in the debates of null results, this might be termed a personal, epistemic negative result). In other words, there is no belief in the argument, but a belief in the worth of contributing it. What must follow is that ‘we ought to change our views about a philosopher’s relationship to her published work: to treat an author’s publications as a creation, rather than an expression of belief’ (Plakias, 2019: 646). This position can be applied to other humanistic disciplines as well. It is particularly this *without belief* that seems noteworthy since it implies a positioning of the author, a non-universality of claims (irrespective of whether the belief is revealed or not). A similar non-universality is put forward by Williams (2008).

Williams raises awareness about misconceptions and comparative evaluations of conceptions that are potentially universal (such as in physical sciences) and those that are more perspectival or locally and historically grounded (such as conceptions in the humanities). He detects a scientific allure in philosophy that submits (certain discourses in) the discipline to a style and form in discourse that ‘tries to remove in advance every

conceivable misunderstanding or misinterpretation or objection, including those that would occur only to the malicious or the clinically literal-minded' (Williams, 2008: 183).

It can, in a similar line of argument, be contended that ideals of rigour and innovativeness underscored by perceptions of peer review, stemming from the natural sciences, are favourable to this scientific drive in the humanities. Such constant scientific allure may, thus, be a danger for the self-understanding of the humanities since they, simply, are not sciences and no notions of post-positivism or data-drivenness may change this. Moreover, no further societal justification *for* the humanities can arise out of a comparison to or aligning with scientific endeavours. This connects to my discussion of Marquard in chapter 7.1.

What follows is the conceptual question of why humanistic endeavours should submit themselves to the same practices of inclusion/exclusion as the sciences do, considering they and their forms of discourse are so fundamentally different. Crane summarises some of those issues and, relating his demand for change to Williams, argues that it is 'unsurprising that the criticisms often put forward in peer review can seem uncharitable, pedantic and pointless' (2018: n.p.). It may, thus, be conceded that it is not necessarily the institution of peer review as such that may be damaging to the discipline or humanistic enquiry, but the way criticism is expressed through it and the way pre-emptive prevention of such criticism impacts writing practices. What is subsumed as a conformist (or moral or social) bias in peer review is, thus, but an articulation of a deeper bias that is only expressed most pertinently in the practices of selectivity.

Coming back to the notion of publishing without belief, it might be re-phrased as *publishing irrespective of belief* just as blind reviewing (one-sidedly) suggests a *selectivity irrespective of position*. In the end, (epistemic) position and belief are ingrained, one can often be deduced from the other. It follows that—connecting to my larger argument—the existence

of publishing without belief in the content 'is a caution against according publication too great a weight in appointment or promotion' (Plakias, 2019: 645).

Ultimately, this shows the incommensurability of formal and substantive rationality: a *most efficient structure* becomes destructive if this neglects the core working principles of the content that is to be rationalised. For it may be efficient for readers to have a good choice of reliable content; it certainly is not for authors. In respect to very low acceptance rates, it is 'the case that publishing only extremely "original" and "innovative" work has allegedly resulted in leaving out many good papers, in sensationalism' (Polonioli, 2016: 992). The prevalence of sensationalism and conformist, or positional, biases varies by discipline and country. As my interviews suggest, there is a tendency towards such biases.

Other studies advocate a similar tendency, where social aspects and questions of position affect decision-making, even though decisions regarding content are partly mitigated by scholarly culture of fairness and merit. Lamont's study of decision-making in peer assessment is compelling regarding the imbrication of culture principles. She concludes about the fairness of review systems that the desire is indeed a meritocratic ideal, but biases can arise out of a multitude of potential aspects:

[n]either the work nor the people are socially disembedded. Panelists' definitions of excellence are rooted and arise from their networks of colleagues and ideas. They aim for fairness, but the taken-for-granted aspects of social life [...] may lead them to assume that what appeals to them is simply best (2009: 241).

Potentially, in the end, there is reason to believe that a conformist bias, as far as it applies, is to a degree connected to pressure and, therefore, based on feeling or assumption. As scholars, particularly those early in their careers, are pressured to perform and depend on a number of formally published contributions, they may *feel* the need to adhere to established epistemes and styles as promoted by senior scholars. Such intellectual adherence may provide more secure grounds for achieving more publications. And just as

parts of the notorious discourse on *publish or perish* constructs pressure recursively, then, parts of a *perceived* conformism may likewise be a social construction that impacts practices pertaining to selectivity.

10.4 Excursus: Conundrums of Media—the Digital and the Humanities

Writing about venues, much could also be said about the way publishers themselves influence publishing practices. As publishers are often categorised as either within the academic field as university or society publishers, or without the academic field as capitalist institutions, the range of publishers may itself illustrate the ambiguity of divergent cultures impacting on scholars. It might be said, university presses are selfless institutions aiming to advance discourse, while corporate publishers serve the field only as long as they can gain a profit. The empirical situation shows such a dualism does not fit reality. The range of publishers is much more diverse and such ideal-type arguments do not seem to carry argumentative force for long. The many discussions on openness highlight this diversity as well as the difficulty of determining what is good service—that is, what really helps discourse—and what is mere rent-seeking. Taking into account that even that which might help discourse the most needs to be economically viable in the long run—or sustainable, as the standardised discourse proclaims—the line between mere rent-seeking and good service is not easily drawn, not to speak of defining economic sustainability in the first place.

As I defined in the introduction to this thesis, I do not wish to engage in discourse on technology. I focus on the scholar subject as an author in relation to a pressure to publish. And yet, throughout my thesis, I touched upon digital means. It might be argued that without digital publishing, the ubiquity of publishing artefacts might not have had evolved so that, in turn, an argument could be made that I am writing about a technological

problem all along. It could, likewise, be claimed that technology is in its literal sense of culture technologies an embodiment of culture principles beyond the materiality of artefacts. One may put forward writing, reading, or citing as principal technologies of Western scholarship, very much geared to community particularities and subjectifications. I would not argue against this.

To be sure, the digital, matters of openness, and publishers themselves without question have an influence on publishing practices. I am indeed very much in favour of seeing the problems of adopting more digital or more open practices as a social-cultural one, instead of a technological one. And I do not claim that the existing literature does not take account of social-cultural issues at all. As Gray exemplifies, infrastructure is very much a bearer of a nexus of social and political issues (2020: 251). And yet, more of such a social-cultural focus may allow for a repositioning of some aspects currently debated.

Throughout my research, I chose not to cover aspects of technology in detail, especially to provide the focus on social-cultural aspects. Moreover, I did not embark on this research to explain *all* culture principles, nor all aspects that *may* affect publishing practices. I chose to focus on the social in the context of a pressure to publish, and the aspects I do focus on—competition, the REF, cultural hybridity, venues—better substantiate this focus. Precisely this focus can contribute to the existing, vast discourse on matters of the digital and openness in giving it parts of this different perspective, one that does not focus on technological feasibility but on cultural intricacies. Indeed, in some articles that I worked on along my PhD, I provide contributions to this existing discourse, about open humanities (Knöchelmann, 2019), about epistemic inequalities and democracy in the context of OA (Knöchelmann, forthcoming), and about the future of libraries in the context of new big deals and enabling small-scaling OA in Germany (Knöchelmann, 2020).

Besides, I worked with others on a collaborative reference work on matters of open scholarship (Tennant et al., 2020).

These things being said, I wish to sketch out in essayistic form how culture principles might work on such practices, or how covering matters of the digital and openness could look like, potentially for a future work: firstly, a resistance against informal, digital publishing artefacts; secondly, the matter of openness. This essayistic positioning is by no means encyclopaedic, of course. But it serves two distinct tasks: firstly, it helps situate my approach in relation to specific discourses (such as that on OA); and secondly, it shows how my work can contribute to these specific discourses directly by highlighting my perspective.

This is situated within the chapter on venues as both aspects depend on where scholars choose, or are made to choose, to publish. Therefore, just as the reputation gained through a brand, there might be something to be gained from OA which affects selectivity. And just as the matter of venues is geared towards culture principles, the matter of OA is so as well. These essayistic sketches are largely based on discussions of existing literature and not as much on my empirical studies as the other arguments in my thesis are. And yet, I did include OA in my quantitative survey for exploratory means and it was also raised by some of my interviewees, which, ultimately, also contributed to my understanding of these issues.

Emphasis on the Formal Form as Resistance Against the Informal

An argument can be made that there is a cultural backlash against modern forms of media. Just as the meaning of artefacts is shaped by the reciprocity of symbolic value and materiality in response to means of efficiency and reliability, there are *established forms of*

media and intruders. Whereas previously, formal rationalistic principles explain the enforcement of the reduction of complexity that interferes with traditional scholarly cultures; in this argument about established forms of media, it is traditionalism that aims to retain coherence as digital cultures with their technological enablement induce complexity. This is not a cultural hybridity, to be sure; this is a resistance, as the established principles and routines are stronger.

Routinisation and reliability are very much ingrained in community cultures. Where there is a ubiquity of new forms, individuals either reject these for the sake of stability or seek intersubjective reassurance. Media theory since the establishment of digital technology is very much a reflection of this in largely non-sociological terms. Empirically, there is a ubiquity of semi-professional journalists, reporters, or distributors of meaning-making claims enabled by digital technology such as the web, social media, or blogging platforms. The tremendous information surplus and resulting chaos of mass communication require artefactual referents of authority that stabilises reception by separating some venues from the mass on the basis of their ability to make validity claims (Waisbord, 2018). In other words, journalists today need to 'retain their authority as sources of information and analysis' (McNair, 2018: 166). This authority, more widely prevalent in media and society, is visible in academia, too: in more advanced form in the medical and natural sciences and, in the humanities, in the form of a more culturalised resistance against the chaotic informalism of ubiquitous media, a resistance that in practice emphasises the authority of established artefacts.

Disseminating a publication could mean just putting something *out there*. To bridge the gap between author to reader can, simplistically, be thought of as setting up a website to which a PDF is uploaded, or a book that is sent to a few bookshops in anticipation that it will eventually be bought. Digital technology enables scholars to forego publishers, for it

is no longer necessary to employ a professional publisher to disseminate one's scholarship to a wider audience. Scholars can make use of all sorts of digital tools that help them with what have formerly been core publishing activities such as writing, working collaboratively, formatting, or disseminating work. Alongside these, there are a variety of tools that help scholars at the other end of the work, where they do not act as authors, for instance, while discovering, sharing, assessing, or sorting publications.²² Social media and networking sites, partly in connection with referencing tools, further help scholars share their work, in formally published or more informal modes of dissemination such as preprints, as well as to assist collaboratively assessing and discussing the worth and value of individual publications. Thompson asks polemically in this regard: 'in an age when anyone can post a text on the internet, who needs a publisher anymore?' (2010: 19). The answer is the categorical strengths of established artefacts as community-constituting sites of communicative interaction as well as the symbolic value attached to these, as I elaborated in the preceding sub-chapters.

Progressive readers may ask: what about blogs? What about social media? What, even, about preprints? These forms and modes are used, to be sure. But they are not symbolically on par with their traditional counterparts, even though materially they could be so. As my data in chapter 7.2 shows, technological means may not be entirely rejected, but humanities scholars are not the most technologically savvy subjects as well: while chapter downloads or digital indices are somewhat welcomed, metrics are not, and blogging is largely dismissed. Recent studies show that the uptake of social media in German academia, and Twitter especially, is particularly low (Ambrasat and Heger, 2020: 32).

²² For a more detailed analysis of available tools, look at the crowdsourced database of over 400 tools at Bosman and Kramer (n.d.).

On the one hand, new media produce an abundance of material on their own which again require practices of efficient reduction. The new media simply lack established means of indicating reliability such as traditional artefacts have; scholars, especially more senior scholars, may simply not find their ways to the texts of blogs or the arguments buried on social media timelines simply because it requires time and engagement to utilise these tools for oneself.

On the other hand, the very principles found in a culture of deliberation paired with academic traditionalism speak against engagement with new media in the first place. Where new media allow for the quick sharing, the ambiguous state of the unfinished text as preprint, the shouting out of an immediate thought in a blog post—humanities scholars may seek the opposite: the stable format, the reliability of a fixed time stamp provided by the printed book, the deliberate, slow reading of the comprehensive argument. This may well take place digitally so that the dualism of digital/non-digital might indeed be futile. The question is rather whether the entire process is digital or if the digital PDF is merely a facsimile of the stable printed version. All the while, an ambiguity cannot be neglected as a discussion on social media *could* resemble an oral dispute with disparate participants; the unfinished idea *could* be put forward for discussion as a preprint. Irrespective of such features, it may be precisely this artefactual ambiguity that is responsible for the cultural backlash against new media.

If we take McLuhan's premise that the medium is the message literally here—artefact irrespective of content *and* cultural disposition—we arrive at a technological determinism. Since, if 'the "message" of any medium or technology is the change of scale or pace' ([1964] 1997: 8), the message of new media in scholarly communication would be obviously progressive. There are many new media in scholarly communication—new transformative environments—that potentially allow (aspiring) scholars to disseminate their

ideas and arguments. These media have transformed the way some scholars approach scholarship: it is easier to share initial ideas, to try provocative arguments, or to test the coherence of conceptions as they are shared informally via, for instance, blogs, social media, or other such technology. These media artefacts have, so to say, created practices in their own right, with a purpose that has hitherto been hardly available to scholars. New technology has made this possible. But looking too closely at these artefacts blinds analysis to focus on how they have had a reflexive consequence on the traditional media. That is, the *message* may actually be a backlash, a solidification of the traditional instead of progressive change. The ubiquitous availability of new media may have *manifested* the requirement of the formal form of the traditional publication.

Furthermore, in regard to the *illusio*, the notion of traditional evokes a sense of authority which triggers the question of whether this mode is indeed more authoritative, or rather only routine. Although efforts for long-term archiving and preservation are enforced particularly by new governance policies or funding requirements, digital publishing remains contested among scholars, particularly in the strongly traditional *Gelehrtentum* of Germany (Groebner, 2016). Jensen talks about new modes of authority in a digital future where the challenge will

mean being open to the widening context in which scholarship is published, and it will mean that faculty members will have to take the time to learn about—and give credit for—the new authority metrics, instead of relying on scholarly publishers to establish the importance of material for them (2007: 306).

Witness earlier research on academic blogging and how it considers this practice as being motivated by the urge to being creative and having a sense of informal community sharing (Kjellberg, 2010), for building a multifaceted epistemic identity that transcends traditional modes of publishing (Kirkup, 2010), or simply sharing for a wider public besides the formal journal (Hager, 2013). For obvious reasons, blogging and social media in a

scholarly context are important aspects and objects of study in the digital humanities (Craig, 2018; Fitzpatrick, 2011b; Quan-Haase et al., 2015; Warwick et al., 2012). However, often absent in these studies is information about the weight social media have in promotion decisions beyond focussed experimentation. The dominance of these technologies in digital humanities discourses may allow for more prominent positions of blogging and social media uses for career advancement in these fields. But for the scholar in other humanities disciplines, these technologies seem to play little role for creating a career.

If a young historian had the chance to focus on publishing articles to respond to the REF or showcase her scholarship to a few hundred likeminded individuals on Twitter, how might she proceed? Of course, this is no exclusive disjunction. But a Twitter channel can hardly make up for formal publications on the CV where culture principles of formal rationality prevail. One may argue that skills such as networking and engagement may be important aspects of being a scholar today, and rightly so—scholarship is engagement with others and their arguments. And yet, use of these technologies alone is no proof. What Kirkup wrote a decade ago still holds true today: ‘the importance for career advancement and institutional research assessment of printed monographs and publications in peer-reviewed journals has been a discouragement from investing time in the activity of blogging’ (2010: 76). To be sure, there are examples where blogs indeed work in a scholarly context, as Bryant shows (2010). And yet, the fact that a substantial part of Bryant’s book is a reprint of blog posts shows the lasting value that is accorded to the former medium.

New governance may take on such new technological means, but the question is whether it is allowed the space to gain cultural authority on its own, or whether it is reduced to be a minor communicative channel for external scholarly communication. In other words, will scholar subjects be allowed the time to make use of informal

communication means as a substantial aspect of their scholarly identity *next* to the epitome formal publications? This may be an enticing question for a future analysis.

The Old News: Open Access

The matter of OA is complex, but it is not particularly new. As we show in the reference on open scholarship, its history is long and open technologies have been available for decades (Tennant et al., 2020). And yet, OA is often discussed either as a quite particularistic problem or regarding its technological availability, both often leading to technological solutionism. Indeed, many contributions to the discourse on OA are concerned with its technicalities (see, for instance: Björk, 2017; Bosman and Kramer, 2018; Brehm and Neumann, 2018; Laakso and Björk, 2013). For the humanities, OA is often discussed in a quite particularistic manner (see, for instance: Eve, 2014; Eve et al., 2017; Mandler, 2013, 2014; Moore, 2019a; Tanner, 2016); though, to be sure, with the focus on scholar-led initiatives, discourse on OA has become more concerned with the minutiae of community cultures as well (Adema and Moore, 2018; Barnes and Gatti, 2019; Moore, 2019b; Moore and Adema, 2020).

It could be argued as well that OA publications are considered to be of lesser value or authority in the humanities, so that they could be seen as another aspect of the resistance against the informal. OA publishing venues, it could be said, intruded on a traditional order. The highly valued university presses and smaller society publishers struggled with the adoption particularly in terms of monographs just as corporate publishers conferred an economic symbolism onto openness. This is underlined by a cultural identity of humanities scholars alongside their societies and the continuous claim of underfunding (see, for instance: Natale, 2019; or the discussion around the 2019 Plan-S

development in the humanities, for instance: British Academy, 2019; Eve, 2019a, 2019b; Royal Historical Society, 2019). OA arrived, slowly, in the humanities by means of community set-up journals, collaborative publishing structures, or demands for economic redistribution which, in the long run, may not be sustainable (Moore, 2019a; Tanner, 2017). Established professoriate and scholars working with established publications for quite obvious reasons perceive such new set-ups as inferior to the traditional media. The new artefacts are not accorded belief in their authority, they are excluded from the field's *illusio* and constitute a cultural outsider.

In the context of my thesis, however, another argument could be made: OA as positioned in the realm of formal rationalistic principles. Radical OA advocates usually have an opposite agenda. Most of the literature of vocal advocates are quite radical in their claims for more openness, positioning the agenda of opening communication in the context of a rather socialist allure, for instance, of small, community-owned infrastructures (Adema and Moore, 2018; Barnes and Gatti, 2019; Moore, 2019a), rejection of classical copyright (Broussard, 2007; Söderberg, 2002; Swartz and Lessig, 2016), or moral claims (Bacevic and Muellerleile, 2017; Willinsky, 2006). Indeed, the *Budapest Initiative* makes the moral argument, justifying OA, to 'share the learning of the rich with the poor and the poor with the rich' (Budapest, 2002: n.d.). But there is also the appropriation of OA by established players, an appropriation that reduces said moral force of openness to mere rule-following in the sense that OA is not primarily a mechanism to share and allow for broader participation in discourse, but to reinforce one's position within discourse. In this sense, large-scale OA enforcement can very well be explained by formal rationalistic principles such that NPM provides the policy requirements that enable a mode of governmentality: scholars shall publish openly so that their reputation, and thus their

institutions' or nations' reputation, may be fostered. This is the context in which enforcement of openness through the REF is still to be seen.

My own data from the exploratory survey show the ambiguity, that OA is not particularly rejected across academic levels of seniority, but not a primary objective either. Recall that experience with publishing OA is mixed for journals (figure 9) but German scholars in the humanities have experience publishing a monograph OA (figure 10). Add to this that more humanities scholars in the UK reject the importance of OA for a future monograph (figure 32). And most of all, UK scholars indicate much higher rates of experienced pressure to publish OA generally than German scholars do (figure 45). If we also account for the fact that OA is very much enforced in the UK on the institutional level particularly by the REF and political engagement with the topic, as is legible in the milestone reference works in this respect (Crossick, 2016; Finch, 2012), this evokes a rather bleak picture. There is a lot of discursive and political engagement with OA in the UK, but the topic seems less welcomed in praxis here than in Germany.

Striking in this respect is also a new report commissioned by the UK's Publishers Association which mourns the potential loss of revenue for UK publishers as well as additional costs for UK universities in the wake of UK's push towards more OA (FTI Consulting, 2021: 6). Such arguments could be seen as simply the capitalist opposition to radical OA advocates or commentators who regularly claim that closed access publishing is too profit-oriented (see, for instance: Buranyi, 2017; Tennant et al., 2016). In this respect, the report only seems to come in quite late to the debate, especially as larger corporate publishers have already found ways to securing revenue in an OA future by means of research intelligence infrastructures, as de Rijcke points out in the context of Elsevier's national agreement with the Netherlands (2020). In other words, corporate publishing is already prepared for a post-subscription revenue future. More striking in this report,

however, are the justifications and arguments. For instance, it can be read that, because OA will diminish the certifying role of journals, ‘readers and researchers would need to invest more of their own time to assess the quality and robustness of research’ (FTI Consulting, 2021: 12). The vital role journals are said to play for peer review and, thus, for quality is stated throughout the report. But only in a footnote (FN61) can a short note be found, that explains that researchers perform the peer review, without remuneration. The workload is already on the sides of the researchers: they write high-quality articles and they assess this quality. It is not highlighted in the report that publishing may just be a service *to* this quality and not the basis for it. These points very much refer to my conception of a *growth/trust spiral* developed above.

And yet, at the individual level, concern about OA is prevalent in both countries, though much more so in the UK, as indicated in the experienced pressure to publish OA in my exploratory survey (figure 45). Indeed, OA partly reveals the political dimension in choosing a venue for publication: supra-institutional policy-making induces a rule-based proceduralism that claims to reposition publishing practices; but this only up to a degree. Though it may be welcomed, or not rejected, this is often only true within the boundaries of gaining reputation in a non-governmentality, traditionalist sense. A recent study confirmed that OA has a subordinate role on the individual level, much behind reputation and peer review quality, in the humanities in Germany (Ambrasat and Heger, 2020: 26).

Moreover, particularly in Germany, culture is often implicitly articulated in the insistence on the freedom to publish. This freedom to publish is said to involve the freedom to publish behind a paywall if the author considers the respective venue to address readership optimally, thus, dismissing a dialectical conception of freedom. My discussion of venues in the preceding sub-chapters indicate the complexity of this issue. In this context, it would be valuable to look at this complexity more closely in relation to OA: a complexity

that considers not just the radical left agendas, but also the ones of NPM, and both in combination. This would require considering not just the feasibility of community-owned infrastructures and what such communitarian openness may mean, but to also consider the cultural ambiguity induced by the enforcement of openness as a means for reputation-gaining. The latter, it can be said, is alienating the moral force of openness; the former, it can be said, is creating an alternative to established venues altogether. In the nexus of the two, there is a fundamental contradiction and publishing practices of scholars have to bear the weight of this contradiction. This is a social-cultural issue, much rather than a technological one.

10.5 Conclusion

In this chapter, I represented my understanding of publishing venues and practices of selectivity—matters full of ambiguities and opacity. My discourse-pragmatist conception might help in differentiating aspects that are often conflated or disguised. The subsequent discussion of symbolic value and practices of selectivity help contextualise the complexity of the matter of venues. This discussion and the acknowledgement of the issues' complexity are vital for my thesis since this differentiates and showcases the variances in the answers to the question of why scholars publish *the way they publish*.

To claim that peer review is necessary and inevitable to maintain the quality of discourse in the humanities rejects the possibility that quality can be intrinsic to scholarly interest in discourse itself. To take up the economic terminology so often encountered today, think of discourse as a *free* market: publishing and reception are the fundamental means of steering the marketability of content—they are the analogous supply and demand sides. And speaking with the terminology of this analogy, any added constraint is like a tax—particularly a selectivity that goes beyond any form of epistemic sorting. A tax can be

helpful, but it is usually a guiding mechanism when there *is* already (a politically claimed) market failure such as a market intruder that needs to be moderated—undesired discourse contributions, so to say.

And yet: this is not constraint; it is part of discourse itself. This may be counter-intuitive but it is essential. The incorporation of critique, the immanence of historical positioning and discussion of prior concepts, is one of the pillars of humanistic discourse. Because of the hermeneutic foundation of reception, ongoing discourse is itself always also a practice of reviewing. By recursively falling back on itself, indicated through the qualitative variance of embedded references, any new contribution reviews (instead of just accounts for or gives proof of) prior scholarship. The selection of inclusion/exclusion is, therefore, only a selection at first instance, a selection that determines inclusion/exclusion for a community, its venue, and its hierarchical stratum. In addition to this preliminary binary selection, scholars continually determine a more nuanced selection by embedding references qualitatively. In other words, the market tries to moderate itself, but because of a reflexive cultural distrust the market is burdened with a tax—the internal moderation becomes externally moderated in a pre-emptive layer so that the internal culture of dissensus becomes harder to maintain in the first place.

Furthermore, the humanities have an institution of reviewing that the sciences largely do not possess. The book review is an addition to the initial gatekeeping—an institutionalised form of post publication review, or paratext—that has been and still is a crucial genre in the humanities (Fritz, 2019: 298–301; Hyland, 2004: 42–44). Again, the initial selection of a (book manuscript) submission for a venue is only the first instance of selection with only binary outcome. If positive, the more nuanced selection and discursive differentiation will take place in subsequently published reviews and uptake as intellectual re-use. The symbolic value of venue, author, or topic paired with the material values of

visibility and access might influence the uptake of post publication reviewers, but these are not sole determinants. The possibility of destructive criticism always remains and, therefore, the review can exert a strong influence on (and expose the arbitrariness of) objectifying the individual contribution with the symbolic value of the collection: even high impact publishers may publish intellectually disappointing books.

This only shows that stratification is both constructed and circular. If the stratification of publishing venues is to be seen as a phenomenon that signposts a cluster of quality—as evoked by the terms impact or excellence—the visibility of publications in high impact venues is also a *visibilizing* of those matters that are deemed the objectives of high quality (Krüger and Hesselmann, 2020). In return, it needs to be questioned whether the low impact venues simply follow different constructions of what counts—of what is deemed being of lower quality and, because of this, materially *invisibilized*. In the end, even materially invisible—niche—journals can be essential and of high symbolic value for a (materially small) niche discourse community. Especially in regional communities, due to language probably more common in Germany than in the UK, this can be usual praxis. Since formal rationality induces a *doing generality* in the terminology of Reckwitz (2020b: 28), stratification evokes the notion of normalisation as well. Publishing practices that align to a stratification inevitably align to a form of normalisation, too.

Ultimately, taking into account the strategic performativity of publishing—some publications for the count and some for the quality—the symbolic value needs to be regarded quite differentiated: the half-baked articles in obscure journals that are hardly ever read have—accumulated on a personal publishing list next to some high quality publications—a considerable symbolic value. They let the individual allow to appear symbolically productive irrespective of content. In confrontation with the issue that this

content may hardly ever be read, what might be most telling about rationalisation in the first place is that this symbolic value—and not the absence of it—is a social fact.

11 Conclusion: Reappreciating Text and Thought

Why do scholars publish the way they do? There is no singular answer to this question. My interviews, as well as the many critical reflections on being a scholar today indicate that powerful motives including discourse and intellectual development can still be found at the root of the interest of most scholars in regard to publishing. This interest, however, is partly buried under an alienating progress of rationalisation. As I determine and discuss in the chapters comprising of my empirical data, publishing practices embody culture principles that drive publishing away from a scholarly-intrinsic interest of contributing to discourse, enforcing strategizing and formal decision-making which make it hard to *simply communicate*.

Performing Contextuality: Dealing with Complexity

My research question, therefore, cannot be reasonably answered in a shorthand version. It requires engagement in the diversity of culture principles observable in contemporary academia, the hybridities of scholar subjects, or the ways culture is indeed actualised in practices—which is precisely what I did in this thesis. The REF best exemplifies how culture principles work on publishing practices in straightforward managerialist ways. Germany's scholar subjects, drawing on principles of a past *Gelehrtentum* within a managerially modern academia, exemplify a cultural hybridity. And the constitution of the symbolic value of venues and the construction of legitimate discourse through formal rational means mark the delicate mechanisms of how culture becomes actualised. These in-depth illustrations of cultures help us to comprehend the constitution of publishing practices which, ultimately, helps us understand why scholars publish the way they do.

With the empirical research and engagement with theories of modernity and culture in academia presented in this thesis, I provide a comprehensive account of authorship and publishing in the humanities in respect to the imperative(s) to publish. My exploratory survey offers statistically significant representations of perceptions, assumptions, and objectifiable statements about authorship, publishing, and pressures relating to them. On the one hand, this supplies a unique dataset about how scholars in the humanities publish in comparison to scholars in the social sciences, as well as UK scholars compared with colleagues in Germany, answering questions such as: how many publications? What formats, languages, or other media are in use? What is expected for future monographs?

On the other hand, across the same pools of individuals, I offer data about the pressures experienced that can be conceptualised as a form of alienation. These perceptions allow for an understanding of the social intricacies and subject formations behind the abstract notion of *publish or perish*. Most of all, the data enable us to infer consequences of the REF, notions of a fast/slow (or: alienation/resonance) dichotomy, and matters of increased pressure resting on the academic precariat. With more than 1,000 participants, the dataset gives a strong context for explaining concrete aspects of pressure and publishing within. The forming of pressure groups (e.g. figures 8/9) already provides a unique perspective on the often-discussed academic precariat through the lens of publishing pressures. It still, though, needs qualitative contextualisation to move from general explanation to in-depth understanding.

This contextual understanding is enabled in my thesis by engagement with a wide body of literature as well as via my qualitative interviews. By talking to scholars, moving quantitative instances in a dataset to lived experiences and life worlds, I gained the deeper insights required to understand subtle differences and idiosyncratic nuances. This engagement led me not only to contextualise my own quantitative data. It also allowed me

to better understand the literature and the broad gamut of approaches, personal perspectives, and ideologies involved. In other words, it allowed me to become reflective of the meaning-making not just of scholars *in situ*, but also of the meaning-making of the research about scholarship. All this is represented in the key analytic chapters of this thesis.

These chapters, embedded in a comprehensive theoretical framework, furnish a new perspective within the wide context of existing research about authorship and publishing. Though works abound on the imperative to publish, I contribute a unique combination of focal points: comparing different national environments within a focus on the humanities, building on a theoretical, philosophical understanding of their intrinsic paradigms that are ideally realised in authorship and publishing, and weaving this into a social theoretical mode of argumentation working with robust concepts of cultural sociology since Weber. This combination does not provide an exhaustive understanding of authorship and publishing, but it may serve as a helpful guide for those interested in better understanding of the humanities and their scholarly practices.

It will allow us to negotiate the contradictory developments of openness, in order to reorient scholars and editors in their understanding of the purpose and use of selection and selectivity. It will help policy makers and research management staff foster a socially and intellectually more fruitful environment for aspiring thinkers to thrive. For instance, my pragmatist conception of venues and its discussion may serve as a new grounding for thinking about metrics, brands, and community-focussed publishing.

Moreover, by making comprehensive social theoretical arguments such as alienation and rationalisation more accessible, this thesis will also provide new arguments to the more practical discourses on publishing. It will help librarians to further understand the contradictory principles scholars face, which can contribute to ideas for how to create an optimal discourse environment, involving both print and digital, reading and writing, a

dialogic future that substantively serves discourse instead of some formal terms. And most of all, it will enable scholars to understand their decision-making in regard to evaluating their own and their colleagues' publications. This research aims to be a key helpful guide in this regard. Although I work with a robust theoretical base and the rigour of a tested methodology, this research deals with humans as the object of study: rigour and robustness are the guiding lines for doing social research; but ways of interpretation and the diverse nature of social interaction always leave room for slightly different answers and representations. This is, in the words of Weber, the nature of the 'interpretive understanding of social action' ([1922] 1978: 4).

Reappreciating Substance

Publish or perish is a terminological kernel of contemporary academia which I trace within existing literature as well as empirically. Even though I do not think that *publish or perish* is the inevitable way to go for scholars (as I indicate by suggesting *publish or perish* is also a self-reinforcing discourse), it best exemplifies the sense of rationalisation that is articulated by practices. Building on this, it may not be possible to fully comprehend publishing practices without understanding competition in academia, since engagement in publishing is geared towards career advancement and not just communication—scholarly communication is full of strategy and does not constitute a Habermasian sphere of rational discourse. On the one hand, this means that discussing authorship always needs to differentiate what is denoted: the political dimension of subject-focus and career advancement; or the reference in scholarly discourse that connects argument and originator.

On the other hand, discussions of competition always need to clarify what are the terms to be discussed. Even though it may at first appear to be too abstract a discussion for understanding publishing, I think that only by engaging in a discussion on the terms of competition can publishing practices really be understood. This is where the matter of pressure to publish as an individual-focussed articulation of a systemic imperative of a single market is the helpful analytic reference. The pressure to publish *more*, to have *no time* to develop intellectually before publishing, or the matter of being *measured bibliographically* instead of judged qualitatively are all articulations of the terms of competition, not competition as such.

These terms are reinforcing an alienated condition centred on the individual. The early career scholar is often in a relation of relationlessness in respect to publishing practices—she perceives pressure in response to the requirements of pouring out reified publications. And more systemic, the abstract scholar subject embodies a form of alienation by reproducing publishing practices generally: the practices are ingrained in an instrumental teleological circle. The iron cage of formal rational terms is difficult to escape. The road to engage in discourse is paved by rationalisation so that the will to publish as a non-strategic, communicative means cannot simply be actualised. Any aspiring scholar has to gain the advantage of a secure position for which she has to establish herself, by circular reinforcement, institutionally and discursively, for which she is required to publish in the first place. She needs to utilise venues to make herself visible and she needs to prove herself valuable for institutional positions by *having* output. Certainly, all of this does not work without an idea of history, a philosophical argument, a philological theory, or otherwise constituted substance; but such substance alone does not seem to suffice. Therefore, comprehending a systemic motivation to publish cannot be uncoupled from an understanding of the constitution of the practices' environment in the first place.

Looking at the rationalities of discourse with the substantive/formal categorisation may leave the researcher with a bleak picture: how does humanities discourse proceed with its qualitative, historical-hermeneutical principles on the one hand and the counting and generalising principles of institutional academia on the other? That is, how are resonant relationships between authors, readers, and discourse possible in an environment of seemingly abundant alienation?

I proposed the term *culture of deliberation* to refer to humanities scholarship, which I ground theoretically by discussing paradigms as well as empirically with a unique set of data about authorship perceptions. An ideal communicative locus would support this *culture of deliberation* without the need to strategize beyond the interest of the better argument. With some of the strivings for community-owned infrastructures, this sense of discursive interest re-enters debates today. And yet, this needs to go beyond technology, alluding to a re-enforcement of solidarity among both local and international scholarship. The culture principles that I illustrate in the preceding chapters are critical points where this solidarity seems to be inhibited. Both my data and the interpretive understanding of it contribute to these debates to help reinvent solidarity alongside the means of new digital technology. Such solidarity as a principle of a *culture of deliberation* may serve as a North Star for guiding substantive rationality—the balancing of rational means with ultimate ends rooted in scholarship.

Alongside this positive, progressive note, and against the rather bleak picture of formal rationality alone, there are already positive aspects to account for as well. These instances of progress should not be forgotten when looking at the work that still needs to be done, and they are intertwined with some of the formal rationalistic terms. In other words, the pressures of competition *can* have their positive sides. Means of democratic involvement are much more widely implemented today. In the cracks of traditional

practices, new technology is used to amplify the dissemination of published manifestations of thinking. And at an even more basic level: more scholars are allowed to think as such—to freely engage in discourse and advance the never-ending process of enlightenment—than ever before. These instances make the perspective of rationalities a promising lens. It dismisses a generalising black and white categorisation and, instead, helps reflect where more *balancing* of substance is required and where formal means-calculation reduces authorship and publishing to an instrumental teleology.

Substantive quality may, in the context of authorship and publishing, mean reducing input in order to regain an appreciation of that which is available. This is specific to the humanities and it is, again, a dialectic: contemporary academia has come to favour formal authorship and the published output so fundamentally that this favouring risks reducing individual authorship and published artefacts to their antithetical irrelevance. If the strategizing dimensions and career intricacies of formal authorship can be diminished, the *illusio* of the formal form of traditional publication artefacts resolved, or the bureaucratic burdens and appearances of legitimation building upon publications lessened, this may help us to reappreciate the substance of text and thought—and, thus, to regain the potential for resonance in authorship and publishing, a cultural potential for solidarity that becomes articulated in the practices of contributing to scholarship. This requires a shift towards more nuanced terms of competition and towards building an environment where scholars are allowed, even encouraged, to publish less, to appreciate the aura of the original text, its qualitative content. It might enable us to appreciate each publication as a substantial piece with a communicative purpose rooted in discourse, as well as enabling teaching and oral discourse to take back their due space. Instead of strategizing appearance, it would allow us to focus on rational discourse again.

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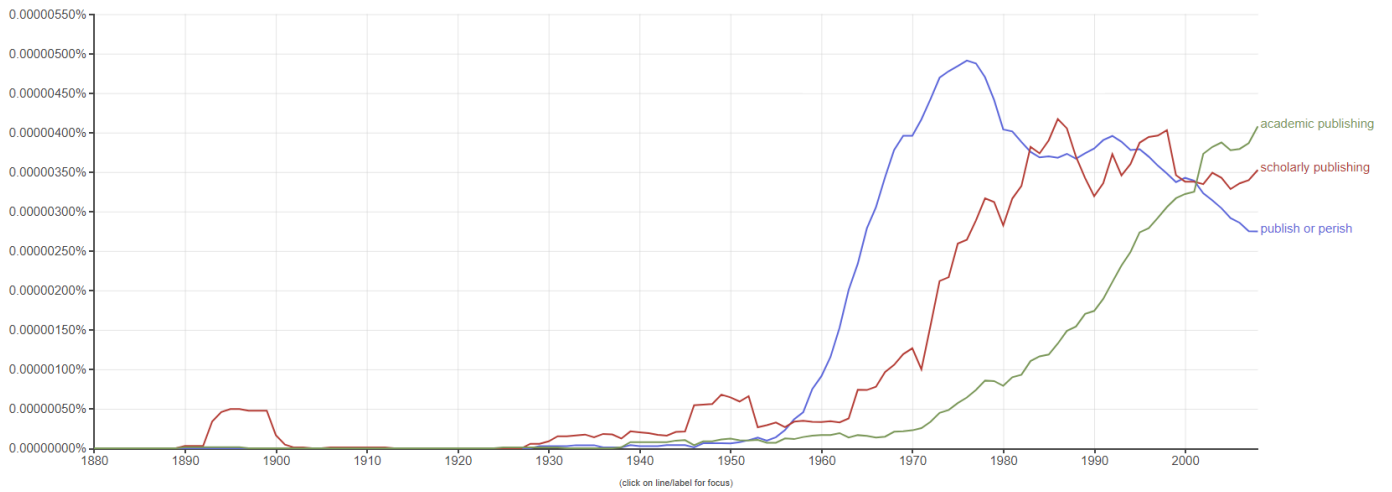
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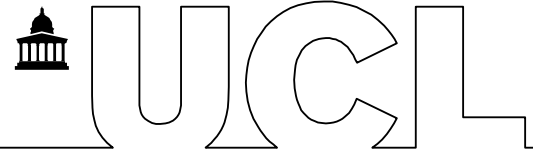
Appendix

A 1. Google Books Ngram



Google Books Ngram comparative search of the terms *publish or perish*, *scholarly publishing*, and *academic publishing* in use between 1880 and 2019.

A 2. Ethics Clearance Form for Exploratory Survey



DEPARTMENT OF INFORMATION STUDIES

RESEARCH ETHICS APPLICATION FORM

Guidance notes

UCL expects all staff and students to follow its Research Ethics regulations (<http://ethics.grad.ucl.ac.uk/>).

All staff & students* embarking on research with human participants must complete this form and submit it to the Chair of the DIS Research Ethics Committee (REC), Dr Andrew Flinn (a.flinn@ucl.ac.uk) **BEFORE** they start their research.

The REC Chair will review the form and decide whether:

- the proposed research is **exempt** from the full UCL Research Ethics Committee - the REC Chair will keep your form and no further action is required.
- the proposed research requires **further information or full approval by the UCL Research Ethics Committee** - the REC Chair will notify you (and if appropriate your supervisor) and advise on how to proceed.

Changes to exempt research project: if you are planning to change your research project or methodology, you **MUST** contact the DIS REC Chair, Dr Andrew Flinn, as soon as possible and provide relevant details as your project may now no longer be exempt.

DEPARTMENT OF INFORMATION STUDIES: RESEARCH ETHICS APPLICATION FORM

1. Personal Details

First Name	Surname	Email
Marcel	Knöchelmann	Marcel.knochelmann.15@ucl.ac.uk

2. For UCL students only

Programme of study	Principal Supervisor (MPhil/PhD students)
Information Studies PhD	Samantha Rayner

3. All applicants

Brief description of proposed & suggested research methodology (including details of topic, human participants and plans for anonymity, procedures to acquire and document informed consent from participants etc)

The proposed research will provide a theory about the requirements for modern academic publishing in the humanities, from the perspective of the author within the larger researcher communication process. This theory will help to understand the value publishers create and whether parts of their work in the modern research communication process may be obsolete. The core research question of this project is "What is the role of a publisher in the research cycle in the humanities?" This question aims at finding comparative advantages, especially in terms of reputational capital and added value for researchers, especially when they act as authors.

This dissertation will follow a qualitative mode of enquiry, studying the context and processes into which the subject matter is embedded, mainly building on a set of data gathered through an online survey.

This aim of this survey is to gather a broad set of data (n ~ 1,000) to find patterns and systemic differences, from which personas (standards of researchers) will be derived. The survey will enquire the attitudes and needs of researchers when they act as authors. The population of the study will be of equal representation from Germany and the UK as well as equally distributed among research/education-intensive, and larger/smaller universities. The questionnaire for this study will be online-based and distributed to researchers via email contacts of institutional staff directly as well as through learned societies, and networks in term 3 of the academic year 17/18. An online-based questionnaire best serves this study as it allows for gathering a large set of data (mostly of quantitative nature/closed questions) about researchers' behaviours, perceptions, and attitudes towards publishing and publishing houses. No personal data are required for this study, so that the survey will be anonymous and only broad questions about the status of the participants will be asked (i.e. type of institution (university/German FH, level of career (postdoc, junior/senior, ...), field of research).

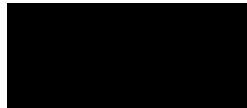
4. Date form submitted to DIS REC Chair

Date: 19.03.2018

FOR OFFICE USE ONLY (to be completed by REC Chair)

1. Proposed research is **exempt**

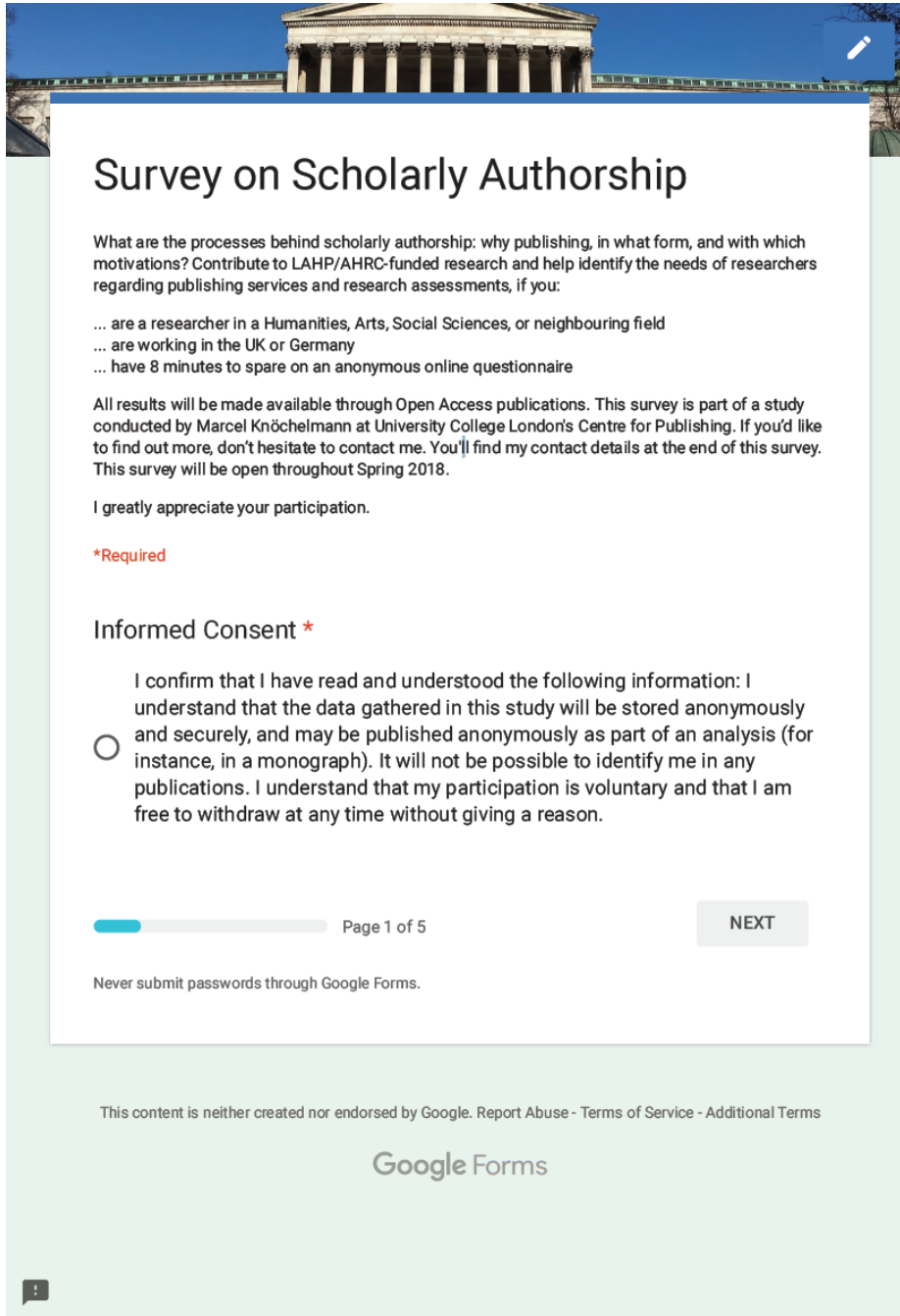
If (2) Applicant notified on:



Signature:

A 3. Exploratory Survey Online Questionnaire

Questionnaire Page 1



The screenshot shows a Google Forms questionnaire titled "Survey on Scholarly Authorship". The form is set against a background image of a classical building with columns. The text of the form includes an introduction, eligibility criteria, a consent section, and a "NEXT" button. A progress bar indicates "Page 1 of 5".

Survey on Scholarly Authorship

What are the processes behind scholarly authorship: why publishing, in what form, and with which motivations? Contribute to LAHP/AHRC-funded research and help identify the needs of researchers regarding publishing services and research assessments, if you:

- ... are a researcher in a Humanities, Arts, Social Sciences, or neighbouring field
- ... are working in the UK or Germany
- ... have 8 minutes to spare on an anonymous online questionnaire

All results will be made available through Open Access publications. This survey is part of a study conducted by Marcel Knöchelmann at University College London's Centre for Publishing. If you'd like to find out more, don't hesitate to contact me. You'll find my contact details at the end of this survey. This survey will be open throughout Spring 2018.

I greatly appreciate your participation.

***Required**

Informed Consent *

I confirm that I have read and understood the following information: I understand that the data gathered in this study will be stored anonymously and securely, and may be published anonymously as part of an analysis (for instance, in a monograph). It will not be possible to identify me in any publications. I understand that my participation is voluntary and that I am free to withdraw at any time without giving a reason.

Page 1 of 5

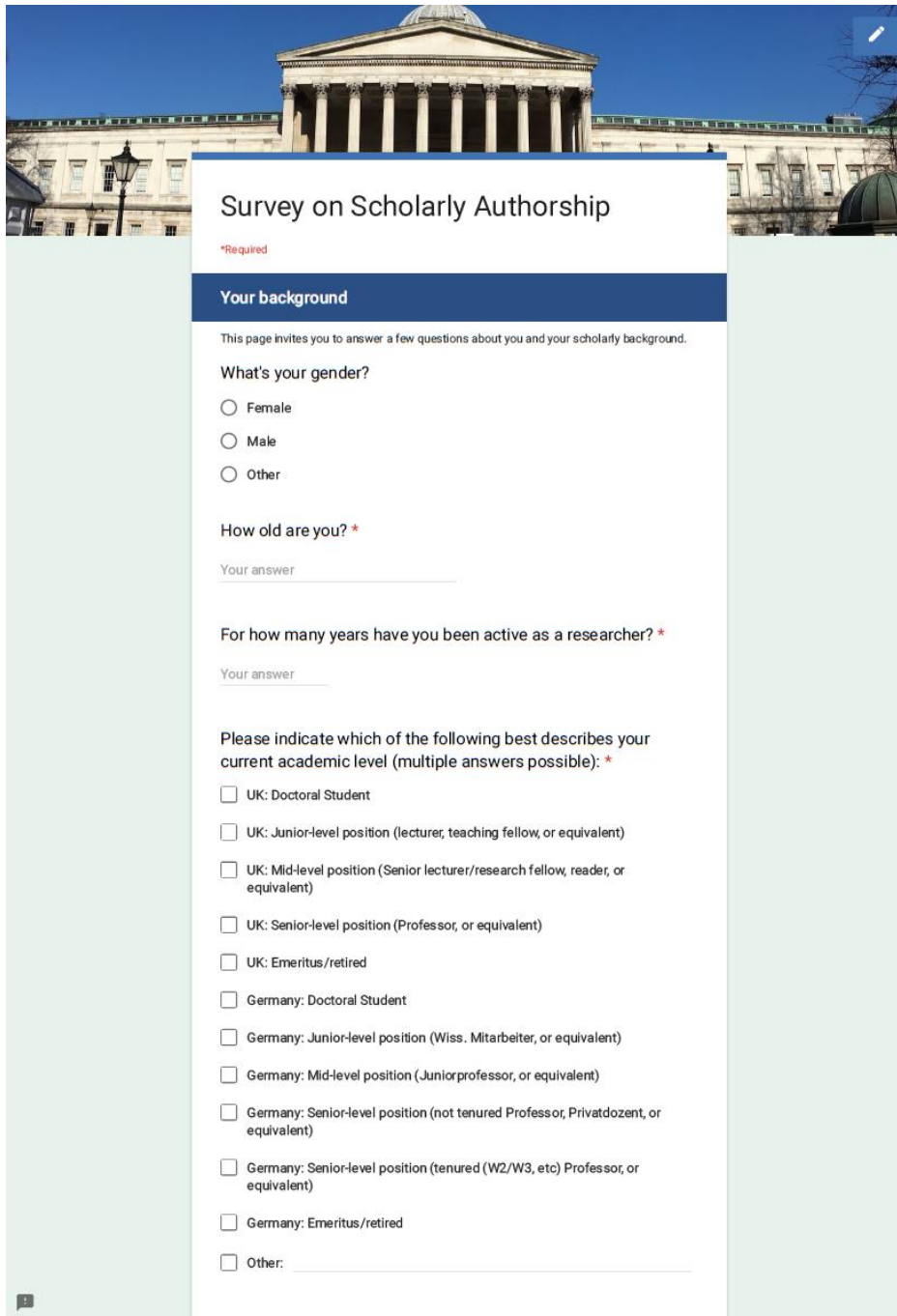
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Google Forms

Questionnaire Page 2

The image shows a screenshot of a survey titled "Survey on Scholarly Authorship". The survey is presented on a white background with a blue header and footer. The header contains the title "Survey on Scholarly Authorship" and a red asterisk indicating that the following questions are required. The main content area is titled "Your background" and contains several questions. The first question is "What's your gender?" with three radio button options: "Female", "Male", and "Other". The second question is "How old are you?*" with a text input field labeled "Your answer". The third question is "For how many years have you been active as a researcher?*" with a text input field labeled "Your answer". The fourth question is "Please indicate which of the following best describes your current academic level (multiple answers possible):*" with a list of checkboxes for various academic positions in the UK and Germany, plus an "Other:" category with a text input field. The background of the survey is a photograph of a classical building with a dome and columns.

Survey on Scholarly Authorship

*Required

Your background

This page invites you to answer a few questions about you and your scholarly background.

What's your gender?

Female

Male

Other

How old are you? *

Your answer _____

For how many years have you been active as a researcher? *

Your answer _____

Please indicate which of the following best describes your current academic level (multiple answers possible): *

UK: Doctoral Student

UK: Junior-level position (lecturer, teaching fellow, or equivalent)

UK: Mid-level position (Senior lecturer/research fellow, reader, or equivalent)

UK: Senior-level position (Professor, or equivalent)

UK: Emeritus/retired

Germany: Doctoral Student

Germany: Junior-level position (Wiss. Mitarbeiter, or equivalent)

Germany: Mid-level position (Juniorprofessor, or equivalent)

Germany: Senior-level position (not tenured Professor, Privatdozent, or equivalent)

Germany: Senior-level position (tenured (W2/W3, etc) Professor, or equivalent)

Germany: Emeritus/retired

Other: _____

Please indicate which discipline you are mainly working in (for instance, philosophy, linguistics, mathematics, history, nuclear physics, information studies, etc.): *

Your answer

Do you consider yourself a researcher in the... *

- Humanities
- Social Sciences
- Arts
- Natural Sciences
- Engineering
- Life Sciences
- Formal Sciences
- Other: _____

What kind of institution are you affiliated with? *

- UK Russell Group University
- UK New University (former Polytechnic, etc.)
- UK Other University
- UK Private Research Institution
- UK Individual Researcher
- UK other
- German University in the Excellence Cluster
- German Public University
- German Private University
- German FH
- German Private Research Institution
- German Individual Researcher
- German other

BACK

NEXT

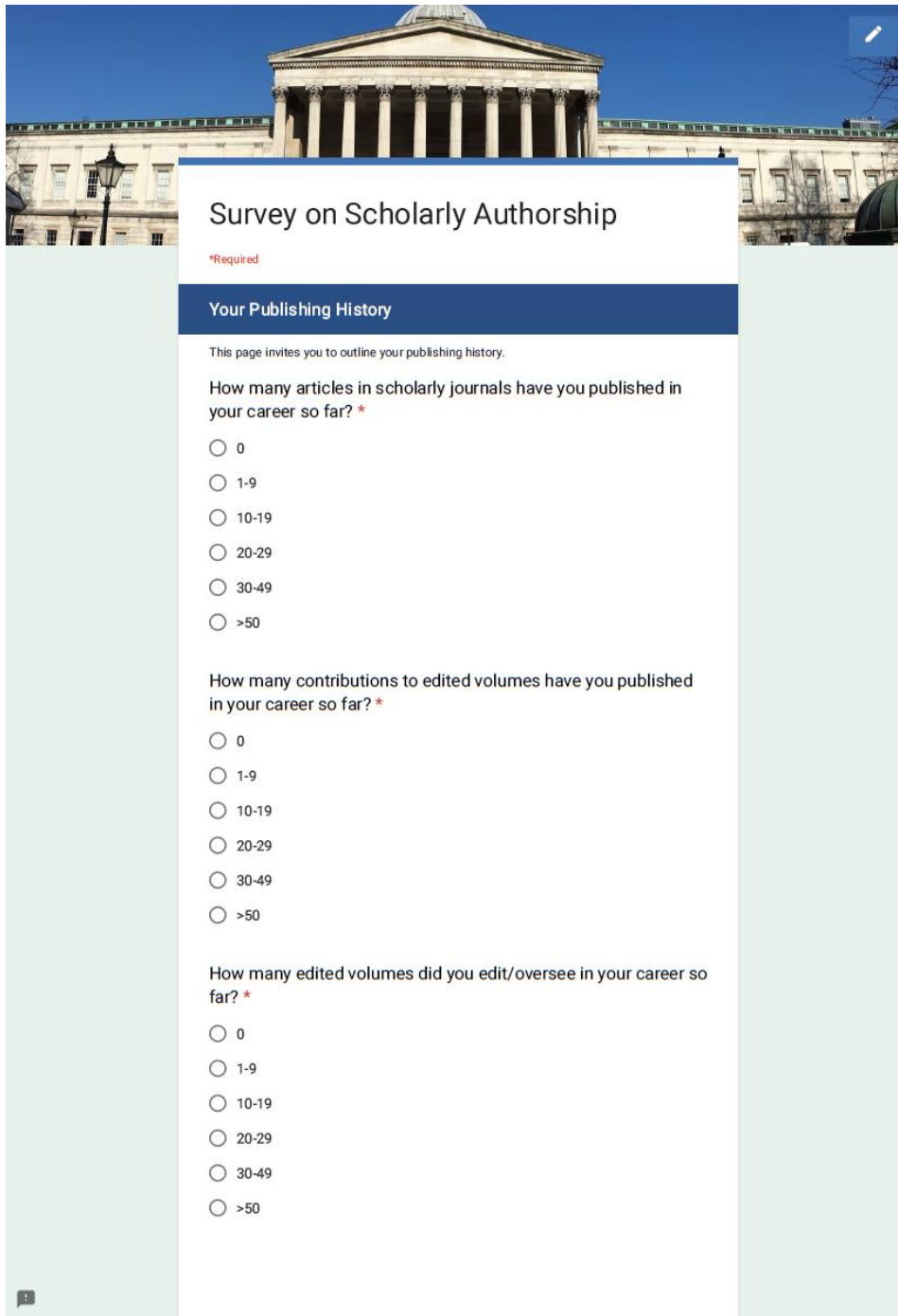
Page 2 of 5

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Google Forms

Questionnaire Page 3

The image shows a screenshot of a survey titled "Survey on Scholarly Authorship". The survey is presented on a page with a background image of a classical building with columns. The survey content is contained within a white box with a blue header. The header reads "Survey on Scholarly Authorship" and "Your Publishing History". Below the header, there is a red asterisk indicating a required question. The first question asks "How many articles in scholarly journals have you published in your career so far? *". The second question asks "How many contributions to edited volumes have you published in your career so far? *". The third question asks "How many edited volumes did you edit/oversee in your career so far? *". Each question has five radio button options: 0, 1-9, 10-19, 20-29, 30-49, and >50. There are small icons in the top right and bottom left corners of the survey area.

Survey on Scholarly Authorship

***Required**

Your Publishing History

This page invites you to outline your publishing history.

How many articles in scholarly journals have you published in your career so far? *

0

1-9

10-19

20-29

30-49

>50

How many contributions to edited volumes have you published in your career so far? *

0

1-9

10-19

20-29

30-49

>50

How many edited volumes did you edit/oversee in your career so far? *

0

1-9

10-19

20-29

30-49

>50

How many monographs have you published in your career so far? *

- 0
- 1-2
- 3-5
- 6-9
- 10-15
- 16-24
- >25

How many textbooks have you published in your career so far? *

- 0
- 1-2
- 3-5
- 6-9
- 10-15
- 16-24
- >25

Have you published any of your articles Open Access so far? *

- Yes
- No
- Not applicable

Have you published any of your monographs Open Access so far? *

- Yes
- No
- Not applicable

Have you published any non-scholarly books that deal with your research? *

- Yes
- No

Do you publish in an online blog on a regular basis (approx. more than ten posts per year)? *

- No
- Yes, my own blog
- Yes, a collaborative blog

Have you published in any other non-traditional format, where the content deals with your research? If so, please indicate:

Your answer _____

Do you usually publish in English or German language? *

- English
- German
- Both, depending on the research
- Other: _____

Is this a second language for you?

- Yes
- No

BACK

NEXT

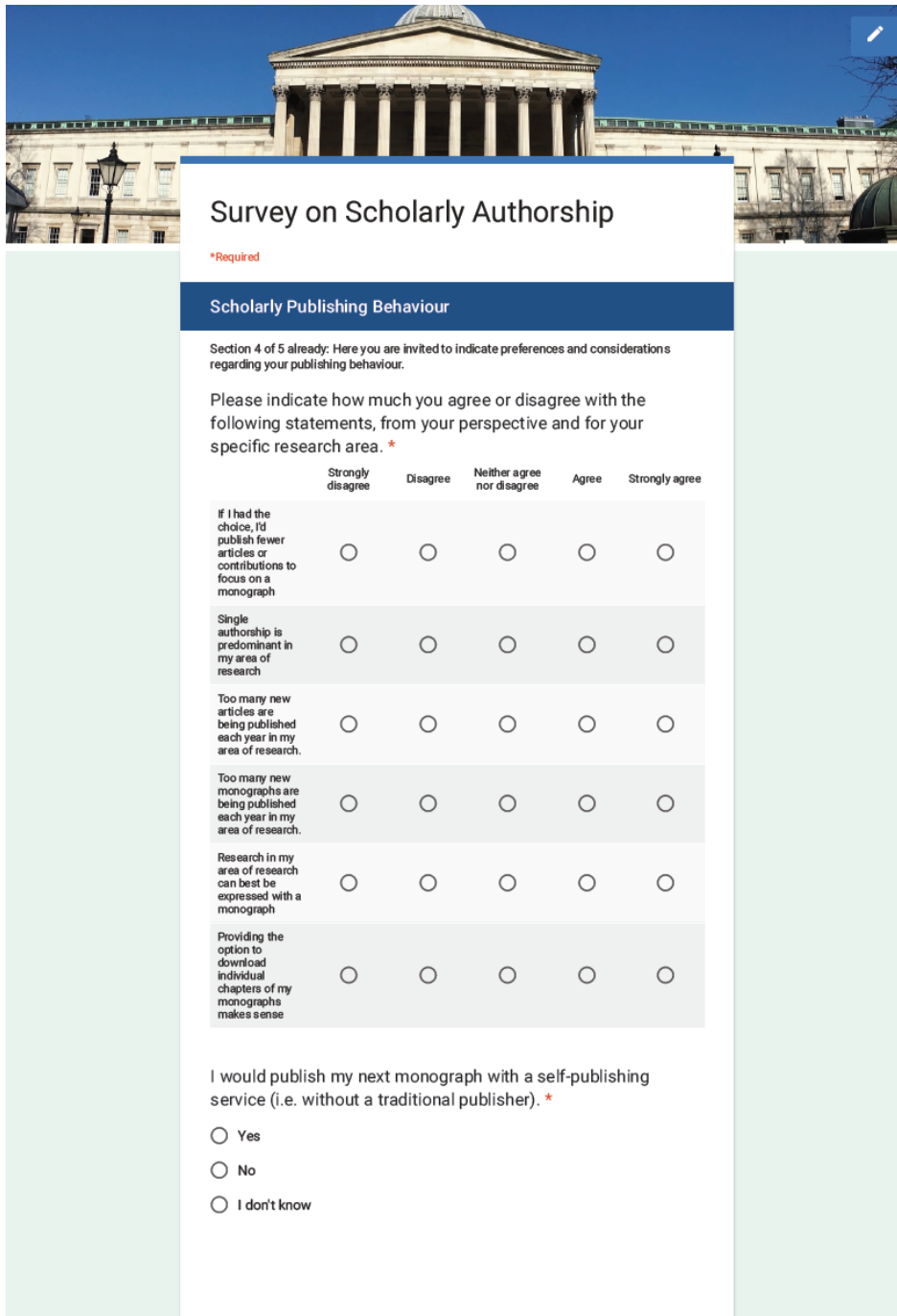
Page 3 of 5

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Google Forms

Questionnaire Page 4



Survey on Scholarly Authorship

*Required

Scholarly Publishing Behaviour

Section 4 of 5 already: Here you are invited to indicate preferences and considerations regarding your publishing behaviour.

Please indicate how much you agree or disagree with the following statements, from your perspective and for your specific research area. *

	Strongly disagree	Disagree	Neither agree nor disagree	Agree	Strongly agree
If I had the choice, I'd publish fewer articles or contributions to focus on a monograph	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Single authorship is predominant in my area of research	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Too many new articles are being published each year in my area of research.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Too many new monographs are being published each year in my area of research.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Research in my area of research can best be expressed with a monograph	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Providing the option to download individual chapters of my monographs makes sense	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>

I would publish my next monograph with a self-publishing service (i.e. without a traditional publisher). *

Yes

No

I don't know

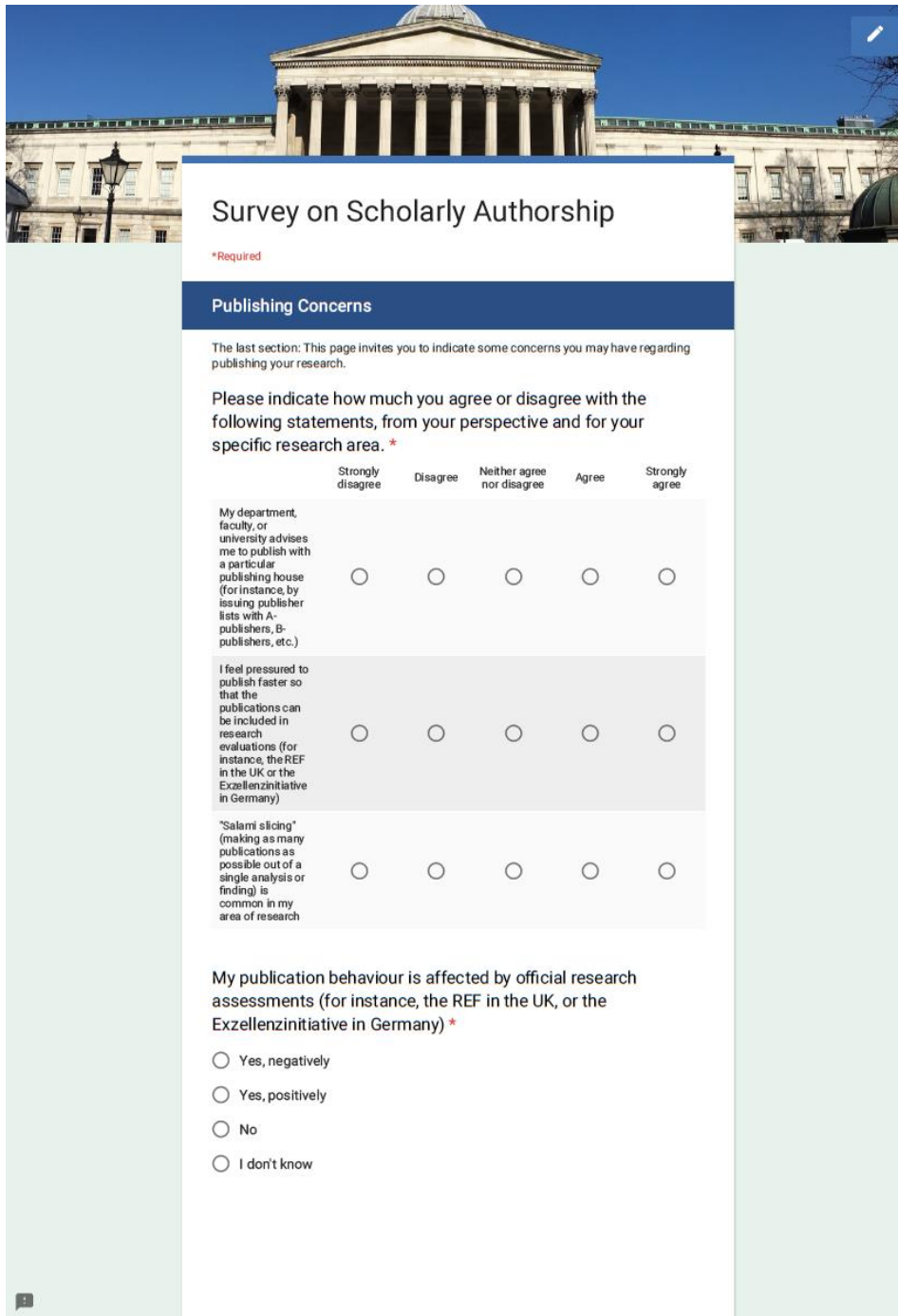
Please indicate how much you agree or disagree with the following statements, from your perspective and for your specific research area. *

	Strongly disagree	Disagree	Neither agree nor disagree	Agree	Strongly agree
My career is built on my strong publication list	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
I know/follow the development of metrics (for instance, citations or downloads) of my books	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
I know/follow the development of metrics (for instance, the Impact Factor, citations, downloads) of my articles (journals respectively)	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
The value of research in my area of research can well be expressed in quantifiers/metrics	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
A publisher's brand is an important indicator for quality when I'm browsing through a bibliography	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>

When you think about your next (potential) monograph, please indicate which criteria are crucial and which not important for you.

	Not important	Slightly important	Considerably important	Crucial	I don't know about this
Rapid publication process focussing on immediacy	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Availability in bookshops	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Unhurried publication process focussing on content improvement	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Discoverability in online databases	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Open Access publication	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Availability in physical form in libraries	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>

Questionnaire Page 5



The image shows a survey page titled "Survey on Scholarly Authorship" with a background image of a classical building. The survey is divided into sections, with the current section being "Publishing Concerns". It includes a header, an introduction, a list of statements with a Likert scale, and a final question with radio button options.

Survey on Scholarly Authorship

***Required**

Publishing Concerns

The last section: This page invites you to indicate some concerns you may have regarding publishing your research.

Please indicate how much you agree or disagree with the following statements, from your perspective and for your specific research area. *

	Strongly disagree	Disagree	Neither agree nor disagree	Agree	Strongly agree
My department, faculty, or university advises me to publish with a particular publishing house (for instance, by issuing publisher lists with A-publishers, B-publishers, etc.)	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
I feel pressured to publish faster so that the publications can be included in research evaluations (for instance, the REF in the UK or the Exzellenzinitiative in Germany)	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
"Salami slicing" (making as many publications as possible out of a single analysis or finding) is common in my area of research	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>

My publication behaviour is affected by official research assessments (for instance, the REF in the UK, or the Exzellenzinitiative in Germany) *

- Yes, negatively
- Yes, positively
- No
- I don't know

Please indicate how much you agree or disagree with the following statements, from your perspective and for your specific research area. *

	Strongly disagree	Disagree	Neither agree nor disagree	Agree	Strongly agree
I feel pressured to publish more journal articles	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
I feel pressured to publish a monograph	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
I feel pressured to publish Open Access	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
I feel pressured to publish faster so that at times the quality of the research suffers	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>

Any other comments?

Your answer

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A 4. Questionnaire Reference Guide

Ref	Name	Question	Type
A	Timestamp	-	Automatically created
B	Gender	What's your gender?	closed (female, male, other)
C	Age	How old are you? *	open
D	Active	For how many years have you been active as a researcher?	open
E	Primary	-	Working column
F	Secondary	-	Working column
G	Country	-	Working column
H	Level	-	Working column
I	Academic level	Please indicate which of the following best describes your current academic level (multiple answers possible)	closed (from junior to senior/tenured with country prefix, incl. open other option)
J	Academic level self	-	closed (from junior to senior/tenured with country prefix, incl. open other option)
K	Discipline	Please indicate which discipline you are mainly working in (for instance, philosophy, linguistics, mathematics, history, nuclear physics, information studies, etc.)	open
L	Discipline self	-	Working column
M	Cluster	Do you consider yourself a researcher in the...	closed (Humanities, Social Sciences, Arts, etc., incl. open other option)
N	Cluster self	-	Working column
O	Cluster formal	-	Working column
P	Affiliation	What kind of institution are you affiliated with?	closed (country-specific types of institutions such as Russell Group, New University in the UK or Excellence University in Germany)
Q	Articles	How many articles in scholarly journals have you published in your career so far?	closed (0, 1-9, 10-19, 20-29, 30-49, >50)
R	Contributions	How many contributions to edited volumes have you published in your career so far?	closed (0, 1-9, 10-19, 20-29, 30-49, >50)
S	Monographs	How many edited volumes did you edit/oversee in your career so far?	closed (0, 1-9, 10-19, 20-29, 30-49, >50)
T	Textbooks	How many monographs have you published in your career so far?	closed (0, 1-2, 3-5, 6-9, 10-15, 16-24, >25)
U	Editor	How many textbooks have you published in your career so far?	closed (0, 1-2, 3-5, 6-9, 10-15, 16-24, >25)
V	Articles OA	Have you published any of your articles Open Access so far?	closed (Yes, No, Not applicable)
W	Monographs OA	Have you published any of your monographs Open Access so far?	closed (Yes, No, Not applicable)
X	Popular books	Have you published any non-scholarly books that deal with your research?	closed (Yes, No)
Y	Blog	Do you publish in an online blog on a regular basis (approx. more than ten posts per year)?	closed (No, Yes, my own blog, Yes, a collaborative blog)
Z	Other non-traditional	Have you published in any other non-traditional format, where the content deals with your research? If so, please indicate:	open

AA	Language	Do you usually publish in English or German language?	closed (English, German, Both, depending on the research, incl. open other option)
AB	Second Language	Is this a second language for you?	closed (Yes, No)
AC	Affected by ref	My publication behaviour is affected by official research assessments (for instance, the REF in the UK, or the Exzellenzinitiative in Germany)	closed (Yes, negatively; Yes, positively; No; I don't know)
AD	Publish article	I feel pressured to publish more journal articles	Likert scale
AE	Publish monograph	I feel pressured to publish a monograph	Likert scale
AF	Publish OA	I feel pressured to publish Open Access	Likert scale
AG	Publish faster	I feel pressured to publish faster so that at times the quality of the research suffers	Likert scale
AH	Publish faster ref	I feel pressured to publish faster so that the publications can be included in research evaluations (for instance, the REF in the UK or the Exzellenzinitiative in Germany)	Likert scale
AI	Publish fewer	If I had the choice, I'd publish fewer articles or contributions to focus on a monograph	Likert scale
AJ	Publication advisory	My department, faculty, or university advises me to publish with a particular publishing house (for instance, by issuing publisher lists with A-publishers, B-publishers, etc.)	Likert scale
AK	Single authorship	Single authorship is predominant in my area of research	Likert scale
AL	New articles	Too many new articles are being published each year in my area of research	Likert scale
AM	New monographs	Too many new monographs are being published each year in my area of research	Likert scale
AN	Metrics for books	I know/follow the development of metrics (for instance, citations or downloads) of my books	Likert scale
AO	Metrics for articles	I know/follow the development of metrics (for instance, the Impact Factor, citations, downloads) of my articles (journals respectively)	Likert scale
AP	Quantifiable value	The value of research in my area of research can well be expressed in quantifiers/metrics	Likert scale
AQ	Salami slicing	Salami slicing (making as many publications as possible out of a single analysis or finding) is common in my area of research	Likert scale
AR	Expression monograph	Research in my area of research can best be expressed with a monograph	Likert scale
AS	Publication list	My career is built on my strong publication list	Likert scale
AT	Selfpublishing	I would publish my next monograph with a self-publishing service (i.e. without a traditional publisher).	closed (Yes, No, I don't know)
AU	Chapter download	Providing the option to download individual chapters of my monographs makes sense	Likert scale
AV	Publisher brand	A publisher's brand is an important indicator for quality when I'm browsing through a bibliography	Likert scale
AW	Rapid publication	Rapid publication process focussing on immediacy	Importance scale
AX	Bookshop	Availability in bookshops	Importance scale

AY	OA publication	Unhurried publication process focussing on content improvement	Importance scale
AZ	Unhurried publication	Discoverability in online databases	Importance scale
BA	Availability in library	Open Access publication	Importance scale
BB	Discoverability online	Availability in physical form in libraries	Importance scale
-	Comment	Any other comments?	open

A 5. Statistical References

Descriptive statistics about clusters as represented in figures 4/5:

Active years of scholars by academic level, Germany, Humanities and Social Sciences

	n	mean	sd	median	trimmed	mad	min	max	range	skew	se
Non senior, Hum	165	8.84	6.07	8	8.03	4.45	1	35	34	1.56	0.47
Non senior, SocS	100	6.79	4.72	6	6.24	4.45	0	25	25	1.13	0.47
Senior, Hum	86	19.21	8.79	18	18.63	10.38	7	44	37	0.58	0.95
Senior, SocS	44	13.95	7.3	13	13.06	5.93	4	38	34	1.17	1.1
Tenured, Hum	106	26.09	8.57	24.5	25.52	6.67	10	50	40	0.65	0.83
Tenured, SocS	44	23.11	9.99	21	21.86	7.41	10	60	50	1.49	1.51

Age of scholars by academic level, Germany, Humanities and Social Sciences

	n	mean	sd	median	trimmed	mad	min	max	range	skew	se
Non senior, Hum	165	35.76	6.6	35	35.12	5.93	25	56	31	0.9	0.51
Non senior, SocS	100	33.47	6.16	32	32.77	4.45	24	62	38	1.54	0.62
Senior, Hum	86	46.71	8.89	46	46.49	10.38	31	72	41	0.28	0.96
Senior, SocS	44	39.95	7.2	38	39.19	5.19	29	63	34	1.17	1.09
Tenured, Hum	106	52.22	7.82	50.5	52.07	8.15	38	70	32	0.22	0.76
Tenured, SocS	44	49.77	9.11	49	49.08	6.67	35	83	48	1.11	1.37

Active years of scholars by academic level, UK, Humanities and Social Sciences

	n	mean	sd	median	trimmed	mad	min	max	range	skew	se
Junior, Hum	107	9.79	5.98	9	9.23	5.93	1	32	31	1.03	0.58
Junior, SocS	102	8.94	4.68	8.5	8.57	5.19	1	30	29	1.11	0.46
Mid, Hum	80	19.04	8.2	18	18.44	8.9	5	40	35	0.6	0.92
Mid, SocS	66	17.41	8.04	16.5	16.87	8.15	3	42	39	0.69	0.99
Senior, Hum	61	33.02	7.15	32	33.06	8.9	20	50	30	0.07	0.92
Senior, SocS	56	27.43	9.98	25.5	27.15	8.15	7	52	45	0.33	1.33

Age of scholars by academic level, UK, Humanities and Social Sciences

	n	mean	sd	median	trimmed	mad	min	max	range	skew	se
Junior, Hum	107	36.49	7.77	35	35.91	7.41	24	64	40	0.8	0.75
Junior, SocS	102	36.94	7.4	35.5	36.05	6.67	24	64	40	1.29	0.73
Mid, Hum	80	46.49	8.13	45.5	46.02	9.64	33	68	35	0.47	0.91
Mid, SocS	66	45.95	8.47	44.5	45.39	8.15	30	69	39	0.61	1.04
Senior, Hum	61	56.82	6.75	56	56.98	7.41	43	70	27	-0.2	0.86
Senior, SocS	56	55.14	9.5	56	55.3	8.9	34	77	43	-0.08	1.27

Test statistics for hypothesis testing; clustering: 12 levels with 3 career levels per disciplinary cluster per country.

survey reference	test	degrees of freedom	p-value
q	Kruskal-Wallis chi-squared = 481.62	11	< 2.2e-16
r	Kruskal-Wallis chi-squared = 492.19	11	< 2.2e-16
s	Kruskal-Wallis chi-squared = 337.9	11	< 2.2e-16
t	Kruskal-Wallis chi-squared = 98.559	11	3.443E-16
u	Kruskal-Wallis chi-squared = 214.3	11	< 2.2e-16
v	Pearson's Chi-squared test = 51.688	22	0.0003452
w	Pearson's Chi-squared test = 124.6	22	2.53E-16
x	Pearson's Chi-squared test = 34.956	11	0.000252
y	Pearson's Chi-squared test = 44.667	22	0.002924
aa	Pearson's Chi-squared test = 543.3	33	< 2.2e-16
ab	Pearson's Chi-squared test = 141.42	22	< 2.2e-16
ac	Pearson's Chi-squared test = 310.84	33	< 2.2e-16
ad	Kruskal-Wallis chi-squared = 148.14	11	< 2.2e-16
ae	Kruskal-Wallis chi-squared = 164.03	11	< 2.2e-16
af	Kruskal-Wallis chi-squared = 217.23	11	< 2.2e-16
ag	Kruskal-Wallis chi-squared = 75.705	11	9.927E-12
ah	Kruskal-Wallis chi-squared = 233.34	11	< 2.2e-16
ai	Kruskal-Wallis chi-squared = 58.545	11	1.725E-08
aj	Kruskal-Wallis chi-squared = 128.04	11	< 2.2e-16
ak	Kruskal-Wallis chi-squared = 278.03	11	< 2.2e-16
al	Kruskal-Wallis chi-squared = 11.287	11	0.4195
am	Kruskal-Wallis chi-squared = 20.491	11	0.03904
an	Kruskal-Wallis chi-squared = 106.91	11	< 2.2e-16
ao	Kruskal-Wallis chi-squared = 166.06	11	< 2.2e-16
ap	Kruskal-Wallis chi-squared = 102.75	11	< 2.2e-16
aq	Kruskal-Wallis chi-squared = 78.009	11	3.576E-12
ar	Kruskal-Wallis chi-squared = 39.53	11	0.00004304
as	Kruskal-Wallis chi-squared = 83.675	11	2.861E-13
at	Pearson's Chi-squared test = 86.364	22	1.414E-09
au	Kruskal-Wallis chi-squared = 22.406	11	0.02141
av	Kruskal-Wallis chi-squared = 16.178	11	0.1347
aw	Kruskal-Wallis chi-squared = 36.711	11	0.0001288
ax	Kruskal-Wallis chi-squared = 25.345	11	0.008114
ay	Kruskal-Wallis chi-squared = 46.724	11	2.407E-06
az	Kruskal-Wallis chi-squared = 41.125	11	0.00002293
ba	Kruskal-Wallis chi-squared = 85.691	11	1.159E-13
bb	Kruskal-Wallis chi-squared = 33.597	11	0.0004208

Test statistics for hypothesis testing; clustering: 4 levels with 2 disciplinary clusters

per country.

survey reference	test	degrees of freedom	p-value
q	Kruskal-Wallis chi-squared = 21.706	3	7.51E-05
r	Kruskal-Wallis chi-squared = 23.022	3	4.00E-05
s	Kruskal-Wallis chi-squared = 44.956	3	9.45E-10
t	Kruskal-Wallis chi-squared = 5.2146	3	0.1567
u	Kruskal-Wallis chi-squared = 18.827	3	0.000297
v	Pearson's Chi-squared test = 13.505	6	0.03568
w	Pearson's Chi-squared test = 58.069	6	1.11E-10
x	Pearson's Chi-squared test = 5.9629	3	0.1134
y	Pearson's Chi-squared test = 29.273	6	5.40E-05
aa	Pearson's Chi-squared test = 517.88	9	< 2.2e-16
ab	Pearson's Chi-squared test = 115.08	6	< 2.2e-16
ac	Pearson's Chi-squared test = 258.03	9	< 2.2e-16
ad	Kruskal-Wallis chi-squared = 59.418	3	7.83E-13
ae	Kruskal-Wallis chi-squared = 98.141	3	< 2.2e-16
af	Kruskal-Wallis chi-squared = 198.18	3	< 2.2e-16
ag	Kruskal-Wallis chi-squared = 24.632	3	1.84E-05
ah	Kruskal-Wallis chi-squared = 197.72	3	< 2.2e-16
ai	Kruskal-Wallis chi-squared = 52.173	3	2.75E-11
aj	Kruskal-Wallis chi-squared = 70.847	3	2.81E-15
ak	Kruskal-Wallis chi-squared = 270.29	3	< 2.2e-16
al	Kruskal-Wallis chi-squared = 0.6916	3	0.8752
am	Kruskal-Wallis chi-squared = 6.6731	3	0.08308
an	Kruskal-Wallis chi-squared = 97.428	3	< 2.2e-16
ao	Kruskal-Wallis chi-squared = 149.49	3	< 2.2e-16
ap	Kruskal-Wallis chi-squared = 88.549	3	< 2.2e-16
aq	Kruskal-Wallis chi-squared = 64.452	3	6.57E-14
ar	Kruskal-Wallis chi-squared = 34.387	3	1.64E-07
as	Kruskal-Wallis chi-squared = 26.152	3	8.87E-06
at	Pearson's Chi-squared test = 39.434	6	5.88E-07
au	Kruskal-Wallis chi-squared = 4.5428	3	0.2085
av	Kruskal-Wallis chi-squared = 2.1235	3	0.5472
aw	Kruskal-Wallis chi-squared = 28.582	3	2.74E-06
ax	Kruskal-Wallis chi-squared = 8.449	3	0.03759
ay	Kruskal-Wallis chi-squared = 21.557	3	8.06E-05
az	Kruskal-Wallis chi-squared = 26.994	3	5.90E-06
ba	Kruskal-Wallis chi-squared = 78.169	3	< 2.2e-16
bb	Kruskal-Wallis chi-squared = 16.256	3	0.001005

Test statistics for hypothesis testing; clustering: 3 pressure groups in 2 countries.

survey reference	test	degrees of freedom	p-value
q	Kruskal-Wallis chi-squared = 96.65	5	< 2.2e-16
r	Kruskal-Wallis chi-squared = 83.964	5	< 2.2e-16
s	Kruskal-Wallis chi-squared = 80.106	5	7.974E-16
t	Kruskal-Wallis chi-squared = 39.031	5	2.341E-07
u	Kruskal-Wallis chi-squared = 26.024	5	0.00008828
aa	Pearson's Chi-squared test = 342.35	15	< 2.2e-16
ab	Pearson's Chi-squared test = 59.561	10	4.387E-09
ac	Pearson's Chi-squared test = 253.63	15	< 2.2e-16
ad	Kruskal-Wallis chi-squared = 348.91	5	< 2.2e-16
ae	Kruskal-Wallis chi-squared = 290.69	5	< 2.2e-16
af	Kruskal-Wallis chi-squared = 155.17	5	< 2.2e-16
ag	Kruskal-Wallis chi-squared = 153.73	5	< 2.2e-16
ah	Kruskal-Wallis chi-squared = 222.33	5	< 2.2e-16
ai	Kruskal-Wallis chi-squared = 10.248	5	0.0685
aj	Kruskal-Wallis chi-squared = 77.467	5	2.841E-15
al	Kruskal-Wallis chi-squared = 8.7492	5	0.1195
am	Kruskal-Wallis chi-squared = 6.5624	5	0.2553
aq	Kruskal-Wallis chi-squared = 60.6	5	9.135E-12
as	Kruskal-Wallis chi-squared = 23.783	5	0.000239

Descriptive statistics and ANOVA test for pressure groups (figures 35/36):

Comparison Germany-UK, across Humanities and Social Sciences

	n	mean	sd	median	trimmed	mad	min	max	range	skew	se
Ger, No pressure	154	21.95	11.38	21	21.55	13.34	1	60	59	0.35	0.92
UK, No pressure	92	23.66	12.28	21.5	23.3	12.6	4	52	48	0.33	1.28
Ger, Low pressure	254	13.79	9.38	12	12.84	8.9	1	45	44	0.88	0.59
UK, Low pressure	224	16.99	10.96	14.5	16.09	11.12	1	45	44	0.61	0.73
Ger, High pressure	137	9.5	6.42	8	8.76	5.93	0	35	35	1.42	0.55
UK, High pressure	156	14.1	8.96	12	13.05	8.9	1	43	42	1.1	0.72

Multiple R-squared: 0.1564, Adjusted R-squared: 0.1523 | F-statistic: 37.5 on 5 and 1011 DF, p-value: < 2.2e-16

Comparison Germany-UK, Humanities

	n	mean	sd	median	trimmed	mad	min	max	range	skew	se
Ger, No pressure	110	23.02	10.85	24	22.99	8.9	1	50	49	0	1.03
UK, No pressure	50	26.62	12.03	27	27.05	14.83	4	50	46	-0.14	1.7
Ger, Low pressure	168	15.25	9.77	13	14.33	10.38	1	45	44	0.81	0.75
UK, Low pressure	126	18.18	11.55	15	17.52	13.34	1	45	44	0.4	1.03
Ger, High pressure	79	9.9	6.45	10	9.15	5.93	1	35	34	1.43	0.73
UK, High pressure	72	13.36	7.71	12	12.66	7.41	1	40	39	1.09	0.91

Multiple R-squared: 0.1739, Adjusted R-squared: 0.167 | F-statistic: 25.22 on 5 and 599 DF, p-value: < 2.2e-16

A 6. Overview of Thematic Analysis of Open Comment Section

compound	sentiment	theme	topic
Scholar in history, UK, 52	negative	academic labour in germany	damaging hierarchy and career system in Germany
Scholar in Media and Communication, UK, 32	negative	advice	no publishing strategy yet
Scholar in Political science, UK, 66	mixed	assessment practices	the use of JIF and Google scholar
Scholar in Turkish Studies, Germany, 56	mixed	assessment practices	in support of disregarding bibliometrics at institution
Scholar in English Literature, UK, 62	mixed	assessment regime	pressure related to REF
Scholar in Media studies, UK, 41	mixed	assessment regime	pressure related to REF
Scholar in Catalan Philology, UK, 61	negative	assessment regime	assessment regimes in other countries; impact surveying in the humanities
Scholar in Criminology, Germany, 43	negative	assessment regime	the tediousness of discussing publishing venues/impact points
Scholar in Development Studies, UK, 55	negative	assessment regime	own institution protected against the worst of the REF, but this protection is fading
Scholar in economic geography, UK, 45	negative	assessment regime	REF is contributing to expectations of overpublication
Scholar in Education, UK, 61	negative	assessment regime	REF as an extremely damaging institution
Scholar in English Literature, UK, 40	negative	assessment regime	the principle of signalling REFable virtues
Scholar in geography, UK, 40	negative	assessment regime	REF as pro quicker publishing and contra quality
Scholar in History-Social Sciences, UK, 42	negative	assessment regime	REF as an emphasis on formal publications and implications on the job market
Scholar in Linguistics, UK, 57	negative	assessment regime	downside of constant monitoring practices
Scholar in Linguistics, UK, 64	negative	assessment regime	REF as "an ugly millstone for academics and universities"
Scholar in media studies, UK, 39	negative	assessment regime	damaging impact of the REF
Scholar in Media studies, UK, 47	negative	assessment regime	REFable output production as opposed to scholarship as such
Scholar in Politics, UK, 33	negative	assessment regime	on publishing too quickly; the narrowness of REF definitions at institutions
Scholar in Social Policy, UK, 49	negative	assessment regime	downside of university promotion system and metrics
Scholar in Social Sciences, UK, 40	negative	assessment regime	left professional academic because of the REF
Scholar in Urban planning, UK, 37	negative	assessment regime	pressure of the REF induces anxiety
Scholar in Linguistics, Germany, 44	mixed	career system	privilege of tenured scholar
Scholar in literary and cultural studies, Germany, 48	negative	change	monograph as "the mainstay of humanities research"

Scholar in Linguistics, Germany, 34	negative	competition and publishing	contextualising pressure to publish
Scholar in romance philology, Germany, 35	negative	costs	publications costs in Germany
Scholar in English literature, UK, 53	negative	disciplinary practices	"research in the Humanities has been led by a science-oriented model"
Scholar in Celtic studies, UK, 46	negative	disciplinary practices	science-criteria lead to too many publications
Scholar in Philosophy, Germany, 34	mixed	early career pressure	career building mechanisms and publishing
Scholar in linguistics, UK, 47	negative	early career pressure	publishing during PhD
Scholar in Philosophy, UK, 28	negative	early career pressure	publishing environment as "toxic and inconducive to good research"
Scholar in Sociology of families and relationships, UK, 30	negative	early career pressure	teaching only contracts because of lack of publications during PhD
Scholar in educational science, Germany, 26	mixed	leaving academia	less pressure as drawing out of professional academia
Scholar in Management Studies, UK, 60	negative	no impact of publications	turns papers into podcasts; low readership of papers
Scholar in Linguistics, UK, 31	mixed	open access	open access not necessarily negative pressure
Scholar in Economics, Germany, 48	mixed	open access	open access and peer review
Scholar in Near- and Middle Eastern Archaeology, Germany, 65	mixed	open access	confusion of academia.edu as open access
Scholar in South Asian Studies, UK, 50	mixed	open access	open access and pressure to publish results quickly due to funding
Scholar in Archaeology, UK, 40	negative	open access	oa as being better
Scholar in Classics, UK, 35	negative	open access	damaging effects of national open access conversations on international publishing behaviour
Scholar in English literature, UK, 58	negative	open access	required open access for REF as a disaster
Scholar in history, Germany, 48	negative	open access	open access irrespective of usefulness
Scholar in history, UK, 47	negative	open access	"Open access is toxic for history"
Scholar in History, UK, 48	negative	open access	open access disadvantage for scholars at less well-funded institutions in the UK
Scholar in History, UK, 38	negative	open access	open access as "extraction of financial and cultural value from academic workers"
Scholar in Klassische Altertumswissenschaften (v.a. Klassische Archäologie), Germany, 38	negative	open access	downsides of open access on discipline
Scholar in literature, UK, 65	negative	open access	"I am strongly opposed to open access publication."

Scholar in literature and philology, UK, 69	negative	open access	against compulsory open access without sufficient funding; suspicious of rankings
Scholar in Sociology, UK, 67	negative	open access	hostility to open access development
Scholar in Machine Learning, Germany, 27	positive	open access	open access advantages in the field
Scholar in musicology, UK, 68	positive	open access	"I WANT to publish Open Access."
Scholar in Philosophy, Germany, 36	mixed	outlier	"I do not really believe in research. I see teaching as the main part of my job."
Scholar in art history, Germany, 57	mixed	outlier	"I publish only for my private pleasure and for my personal scientific knowledge"
Scholar in archaeology, Germany, 53	negative	pod	"negativ experience with print-on-demand publisher"
Scholar in Sociology, UK, 34	mixed	potentially positive pressure	REF as a chilling effect
Scholar in linguistics, Germany, 56	mixed	precarious contracts	career progression and promotion system
Scholar in Economics, Germany, 39	negative	precarious contracts	"Publications are not the most important factor explaining pressure on Scholar."
Scholar in English and Medical Humanities, UK, 31	negative	precarious contracts	ban on publications due to promotion system and hierarchies
Scholar in Linguistics, UK, 38	mixed	pressure	natural productivity opposing pressure to publish
Scholar in English literature, UK, 30	negative	pressure	pressure to publish reduces quality of the work represented/decoupled from quality of scholarship
Scholar in German literature (NdL), UK, 39	negative	pressure	pressures from both REF and career progression
Scholar in History, UK, 27	negative	pressure	pressure increases as career advances
Scholar in Social Anthropology/Ethnologie, UK, 57	negative	pressure	on self-pressures and immediacy of research
Scholar in musicology, Germany, 38	negative	pressure	"I prefer to focus more on the content of my research rather than on moving up the career"
Scholar in history, UK, 52	positive	pressure	"pressure motivates"
Scholar in History, UK, 48	positive	pressure	no pressure due to tenured position
Scholar in German memory and identity, UK, 37	negative	pressure/female	fixed teaching contract and pressure to publish; pressure on female scholars
Scholar in Classics, UK, 40	negative	publishing general	not paid by publishers while they make a profit
Scholar in economics, UK, 63	negative	publishing general	"trade off between brand (low) and convenience (high)"
Scholar in management studies, UK, 54	negative	publishing general	general hostility to publishing structures

Scholar in Creative Writing, UK, 39	negative	publishing in creative writing	publications in creative areas do not count as much as "academic publications"
Scholar in economic geography, UK, 32	negative	publishing targets	contextualising pressure (JIF, targets, neoliberal management)
Scholar in Social sciences, Germany, 55-60	negative	quality	too much saying about nothing new because too many publications
Scholar in philosophy, Germany, 39	negative	quality	frustrated with the decreasing quality of content
Scholar in Anthropology, UK, 56	negative	quality and quantity	"The emphasis should be on quality not quantity."
Scholar in History, Germany, 34	negative	quality and quantity	"self-reinforcing reward-structures in place that push volume over quality of publication"
Scholar in linguistics, Germany, 49	negative	quality and quantity	"I feel that quality is suffering due to interest in quantity."
Scholar in literature, UK, 45	negative	quality and quantity	demand for more emphasis on quality
Scholar in media and communication sciences, Germany, 46	negative	quality and quantity	too many publications and too poor quality
Scholar in Medieval Scandinavian Studies, Germany, 36	negative	quality and quantity	"Far too often quantity seems to be more important than quality."
Scholar in Musicology, Germany, 55	negative	quality and quantity	"I would really appreciate to be evaluated based on quality and not on quantity."
Scholar in teacher education; linguistics, Germany, 27	negative	quality and quantity	"It seems like it is all about the length of your list of publications"
Scholar in English Literature, UK, 39	negative	quality assurance practices	fundamentally damaging quality assurance practices; marginalisation of "slow professors" irrespective of quality
Scholar in linguistics and foreign language teaching, Germany, 26	negative	quality assurance practices	lack of quality in publications and processes
Scholar in Theatre, UK, 42	negative	quality assurance practices	lack of collaborative approach to open access in the humanities
Scholar in ancient history / classics, UK, 55	mixed	quantity	pressure on others leads to too many publications with lots of bad stuff
Scholar in architecture, UK, 47	negative	teaching and profits of publishers	no time to publish; against publisher's profits
Scholar in Archaeology (Classical), UK, 61		x - on survey	book reviews
Scholar in History, UK, 60		x - on survey	open access
Scholar in law, UK, 47		x - on survey	contextualising answers
Scholar in Book and Publishing Studies, UK, 34		x - on survey	contextualising answers
Scholar in archaeology, Germany, 49		x - on survey	peer review
Scholar in history, Germany, 40		x - on survey	about the respondent

Scholar in church history and history, Germany, 45		x - on survey	this research
Scholar in Design, UK, 34		x - on survey	this research
Scholar in Education, UK, 70		x - on survey	this research
Scholar in English, UK, 47		x - on survey	this research
Scholar in fine art, UK, 66		x - on survey	this research
Scholar in French Literature & Culture, UK, 71		x - on survey	this research
Scholar in linguistics, Germany, 40		x - on survey	this research
Scholar in linguistics, UK, 76		x - on survey	this research
Scholar in manuscript sciences / ancient polemical literature, Germany, 53		x - on survey	this research
Scholar in Philosophy, Germany, 46		x - on survey	this research
Scholar in Philosophy, UK, 56		x - on survey	this research
Scholar in Prehistoric archaeology (European and British), UK, 73		x - on survey	this research
Scholar in psychology, Germany, 31		x - on survey	this research
Scholar in Roman philology, Germany, 75		x - on survey	this research
Scholar in social anthropology, UK, 76		x - on survey	this research
Scholar in special needs education / inclusive education, Germany, 32		x - on survey	this research
Scholar in Anthropology, Germany, 37		x - on survey	this research

A 7. Ethics Clearance Form for Qualitative Interviews



DEPARTMENT OF INFORMATION STUDIES

RESEARCH ETHICS APPLICATION FORM

Guidance notes

UCL expects all staff and students to follow its Research Ethics regulations (<http://ethics.grad.ucl.ac.uk/>).

All staff & students* embarking on research with human participants must complete this form and submit it to the Chair of the DIS Research Ethics Committee (REC), Dr Andrew Flinn (a.flinn@ucl.ac.uk) **BEFORE** they start their research.

The REC Chair will review the form and decide whether:

- the proposed research is **exempt** from the full UCL Research Ethics Committee - the REC Chair will keep your form and no further action is required.
- the proposed research requires **further information or full approval by the UCL Research Ethics Committee** - the REC Chair will notify you (and if appropriate your supervisor) and advise on how to proceed.

Changes to exempt research project: if you are planning to change your research project or methodology, you **MUST** contact the DIS REC Chair, Dr Andrew Flinn, as soon as possible and provide relevant details as your project may now no longer be exempt.

DEPARTMENT OF INFORMATION STUDIES

RESEARCH ETHICS APPLICATION FORM

5. Personal Details

First Name	Surname	Email
Marcel	Knöchelmann	Marcel.knochelmann.15@ucl.ac.uk

6. For UCL students only

Programme of study	Principal Supervisor
Information Studies PhD	Samantha Rayner

7. All applicants

Brief description of proposed & suggested research methodology (including details of topic, human participants and plans for anonymity, procedures to acquire and document informed consent from participants etc)
<p>Qualitative Interviews</p> <p>The proposed interviews will be the second empirical part of my PhD project, which seeks explanations for authorship and publishing among humanities researchers in the UK and Germany.</p> <p>The aim of the overall project is to find out about motives and reasons as to why researchers engage in formal communication, and what structural constraints may affect those motives and reasons (f.i. pressures to publish that impact academic discourses known as 'publish or perish'). To find out about the broader aspects of such pressures and the publishing behaviour in general, I conducted a pilot study in 2018, which was entirely anonymous and resulted to 1178 valid responses.</p> <p>The second empirical part builds upon these data with an in-depth enquiry into motives and reasons. For this, I want to conduct semi-structured, qualitative interviews with researchers in the humanities. Questions of those interviews will include the origins of potential pressure, the impact of potential pressure on publication behaviour as well as on scholarly discourse, intentionality of publication behaviour, and reputational power within publication behaviour. The answers to these questions will be analysed with respect to Anthony Giddens' Theory of Structuration which provides the theoretical framework for explaining social behaviour. These aspects of social behaviour within authorship and publishing are those that appeared to be a concern expressed in the quantitative survey (conducted in 2018). Together, the results of these two sets of data will be the basis for my analysis.</p> <p>The here-proposed qualitative interviews will be completely anonymised. The aim is to conduct 12-18 interviews in the UK and in Germany respectively. For the analysis, the interviewees will receive anonymous identifiers which only roughly sort them into categories such as, for instance, "Senior Researcher, Philosophy, Russell Group University" or "Early Career Researcher, History, Post-1992 University". No other criteria are required so that the interviewees cannot be identified retrospectively. In accordance with data security requirements, I will store the contact details of the interviewees in a different Excel sheet than the one in which I store their answers, and keep it securely on a separate, external hard drive.</p> <p>I aim to conduct the interviews in person during Spring and early Autumn in three rounds. Initial contact to potential interviewees will be made via email after a random selection process based on public university profiles.</p>

8. Date form submitted to DIS REC Chair


Date: 21.02.2019

FOR OFFICE USE ONLY (to be completed by REC Chair)

If (2) Applicant notified on:

Signature:Andrew Flinn 21/2/2019.....

A 8. Ethics Consent Form Example



University College London
 Department of Information Studies
 Gower Street
 WC1E 6BT London

Consent Form

PhD research project being conducted by Marcel Knöchelmann at University College London, Department of Information Studies (partly at Yale University, Department of Sociology, January 2020-May 2020).

Title: A Social Theory of Authorship and Publishing in the Humanities

Primary supervisor: Dr Samantha Rayner, UCL.

Contact Details:

Marcel Knöchelmann
 marcel.knoechelmann.15@ucl.ac.uk | [REDACTED]
 [REDACTED]

Consent form for interview respondents:

- I agree that I will be interviewed for the purposes of data collection in this project.
- I agree that the data collected can be used in the PhD thesis presented to UCL.
- I understand that the research may be published in, for example, the form of a monograph or a journal article, and that the PhD thesis may be made available by the University in its digital repository or in print, after the Examination Board has determined the outcome of the examinations.
- I understand that respondents will not be individually named (or made identifiable in any way) in these outputs. The data will be collected and stored in accordance with the Data Protection Act 1998. It will be retained for the duration of the project and for a period of up to 1 year afterwards in order to allow for re-examination of the data by the research student or his/her supervisors or examiners for purposes connected with the PhD thesis and its examination.
- I understand that participation is voluntary.

Many thanks for your participation.

Name:

Signature

Date

A 9. Exemplary Interview English: Melissa

00:01:25

Marcel: So- the broader question to start with will be- just in general- what is your idea- why do you publish your research?

00:01:30

Melissa: Oh- that is a general question.

00:01:32

Marcel: Yeah-

00:01:32

Melissa: Yeah. Maybe two things seem to me to be the central things I do. One is- I don't know about other fields and the people's motivation- but I got into philosophy to try to sort of contribute to the conversation. That's how I see it. And I think that's the sort of formal way of contributing. So that I think I want to make a contribution to the area- the philosophical inquiry and then that's the sort of formalized mechanism for doing it- you know- sort of the formal equivalent of raising my hand or something. So- I think that's the central motivation. But that is only about sort of the academic side of things. And for me- I think I care about my ideas actually connecting up to the world outside of academia and that's another reason to publish. So depending on what kind of publications- I have a few non-academic collaborations in writing and I think that stuff you know can reach a different audience that I wouldn't have access to otherwise. So that's another reason to sort of get my ideas out there in other sorts of areas where I can't actually have a literal conversation you know?

00:03:05

Marcel: Yeah. Right.

00:03:07

Melissa: Yeah. And then- so those are the two sort of like nice and real genuine reasons in

my case. But of course- you're aware of the pressure to publish for promotion reasons and things like that. So and I- people are different about this. You know- I really take that seriously as far as that pressure goes. I don't really feel the need to publish as much as possible. So I really shoot to try to write two things a year that I feel really good about. And I spend a lot of time at them. And I know that's not how everyone feels about it. But I think for me- and for me- because that's a strict sort of promotion pressure- that's the only one I try to listen to. You know- there's a lot of social pressure to just publish as much as you can. And I really try to tune those voices out myself.

00:03:56

Marvel: Yeah- interesting. Ok. So you mentioned there the- like two different forms of pressure. One is the promotion pressure and the other is more- I think- more general- or more a broader social pressure. Right?

00:04:13

Melissa: Yeah- yeah.

00:04:14

Marvel: So how does- could you elaborate on these two a bit more- what do you mean specifically with the promotion pressure?

00:04:24

Melissa: And so I think you just at least here in the UK- you know- one criteria for promoting is that you publish a certain amount- you- basically for the REF- you want to make sure that you're refable. And then when you submit for promotion you know- you have to have had a certain amount of publications within that period since the last time you went for promotion. And that's- how much that is different depending on what level you're seeking promotion for. So that pressure is pretty straightforward- I think- just as far as when I think about the next time I want to apply for promotion. Do I have enough publications to tick that box? That's what I mean and that's sort of a box- it really is sort of

a box ticking exercise that kind of pressure. And you can- at least in the UK- you know you can be promoted without ticking that box. It's just in my case- I guess- because I put a lot of time into the research at the box that I try to tick. So- yeah. So that pressure is pretty straightforward. So social pressure of course is a different thing. I think- not that I'm holding on to that view- but there's a real sort of- I think- bias is the word- just- I don't know- assumption around that if you publish a lot more that you're better in some way. Like that somehow quantity translates into quality in a way that I think it's just patently false. So and but there's quite a lot of pressure about that. So when people are applying for jobs or just trying to switch up from institution to institution and that kind of stuff. You know- I think it's very hard because that seems to be the sort of- even if people don't admit that or say it- there seems to be a kind of underlying view that- you know if you were really good- you'd be publishing quite a lot. Whereas I think that is just false and a lot of it is very gamy- you know- so and especially certain areas are niche- you know I- you kind of- it's not really blinded in a kind of way and once you sort of get into the right circle of people and journals- you know it's quite easy- I think- to have- you know what are essentially spin off papers. And a lot of people are- you know- do that. And there's a lot of pressure- I think- to play that kind of a game. And it really does seem to me a game.

00:06:48

Marcel: Yeah- ok.

00:06:48

Melissa: Yeah. So that was a long-winded answer Marcel.

00:06:54

Marcel: All right. That's all right. It's good. So regarding the social pressure- the broader or more underlying subtle pressure- how do you get to know about this? Is this like kind of an narration when you talk to other peers in your field or when you get to meet others at

conferences. So where does this kind of pressure come from- do you an idea about that?
Or is it more ever around somehow?

00:07:25

Melissa: I think people talk about it all the time. Yeah- at least in my experience. Yeah.
Yeah. I think people often explicitly talk about it. I had a friend- said to me shortly after we became friends that she checked my CV records- she would be my friend- of course I laughed out loud- I thought I have a [inaudible] CV- I thought- it was a person I met at a philosophy conference. So I think- there's just quite a lot of almost casual- well they have the best papers and such and such- or- well they don't really publish very much. People I think say that kind of stuff- just kind of all the time. At least around me- I don't know.
Yeah.

00:08:04

Marvel: Ok. And you mentioned also that it's it's gamey or is it's it's kind of a game- especially regarding the quantities and kind of performative aspect of publishing.

00:08:18

Melissa: Yeah.

00:08:18

Marvel: In what sense can people game this kind of- yeah- authorship and publishing issues?

00:08:28

Melissa: Yeah. I think people know who are on the various boards and who is likely to review in their area if they submit to it to certain places. And and that means that they will intentionally often cite those people's work somewhere in their submission. For instance- that's one way I think that the gaming occurs- that's sort of artificial citations or that the person who might review your paper will be more likely to want it to be published. And that kind of thing is- I guess- what I have in mind. You know- there's quite a lot of that

sort of I think- I wouldn't say it's exactly dishonest. It's just the intention- in the work isn't coming from the work.

00:09:22

Marcel: Right- yeah.

00:09:22

Melissa: Yeah. It's the kind of that sense of gamy.

00:09:25

Marcel: Ok. So that connects directly to a question that I also have written down. It's that- or the idea that some researchers publish rather for their publication list than for the actual discourse. Would you agree with that?

00:09:45

Melissa: Oh definitely- yeah- definitely.

00:09:48

Marcel: Ok. And connected to that.- do you have the feeling that there is or there are too many publications in your discourse or in your field?

00:09:58

Melissa: That's an interesting question. I think I would hesitate to make that kind of a judgement. I don't know how many would be too many. I think the very focus on quantity is the thing that I find misguided. So I wouldn't want to say there are too many or too few. The point is just that's not the important issue I think.

00:10:22

Marcel: Ok.

00:10:22

Melissa: Does that make sense?

00:10:24

Marcel: Yeah. Yeah- it does. So- yeah. Next to the quantity or apart from the quantity- is there then an issue of quality when people- people publish more for their publication lists-

so that they- maybe repeatedly publish about the same issue because they can just churn out about the same subjects- something that's like salami slicing in the natural sciences- so is there an issue or do you get the feeling of there is an issue of quality?

00:11:10

Melissa: Yeah- yeah. I mean- I definitely think that's right. Yeah I think that's sort of right. Yeah. And I and I- and I think everybody- if not publicly who engages in this kind of thing acknowledges that. I have quite a lot of friends who have quite impressive CVs and I think they know exactly what they're doing- why they're doing it.

00:11:30

Marvel: Yeah- ok. And so connected with impressive CVs and also the quality is often the question of brands like publishing brands- the name of a journal or the name of a publishing house. In what sense are publishing brands useful- would you say?

00:12:07

Melissa: I think that they are a genuine kind of check on quality in the sense that there are certain norms and standards that I think have to be- they are the kind of threshold condition- I think. But- I do think that's true. But I think that there are unfortunately other- not necessary conditions. There's something- nothing that I checked- but other things that usually also have to be true- that mean that you know- that there are quite a lot of good things that would have trouble getting in. So I guess I- they are kind of a double edged sword in some ways- I think. Yeah yeah.

00:13:03

Marvel: Ok. And as you mentioned- to get into certain brands- there's the notion of selection which makes these brands somehow more high impact or of more high quality. Is peer review the norm in your field?

00:13:23

Melissa: Yeah yeah- definitely.

00:13:24

Marcel: Ok. And what do you think of peer review? I suspect that's more like the supposedly blind peer review. What do you think of those processes in your field- do they make sense?

00:13:38

Melissa: Yeah yeah. I think it's- I think it's I think it's good to- I think it's good to have blind peer review and my only qualms about it is that it's not genuinely as blind as it ought to be.

00:13:50

Marcel: Ok.

00:13:52

Melissa: Yeah- I rejected a paper early on. It can- not long after I got my PhD- I was really flattered to be send a paper by a sort of top ranked journal in my field to review and I rejected the paper and I was afraid to reject it because I knew who it was by. And if they knew that the rejection was written by- could give me some trouble- you know. So I think that's just one example- but I think- you know- at least in philosophy- things are unfortunately not as blind as one wishes they were.

00:14:28

Marcel: Ok.

00:14:31

Melissa: I think if you're a leader in the field- you- there are ways to write your paper so that almost everyone will know who you are and then because it's you- almost any journal will publish it.

00:14:42

Marcel: Right- yeah. And when we think of- that brands are sort of heuristics for- also for readers to know about the content that the journal publishes- do you think that brands are also important and- or related to the promotion pressure that you mentioned?

00:15:13

Melissa: What do you mean? Not sure I'm following you.

00:15:14

Marcel: That brands when they are mentioned on publishing lists- for instance- signify a certain quality of the research in the end- so that people try to get certain brands on their CVs.

00:15:29

Melissa: Yeah- I think on the promotion side- at least in the UK not so much- because the people- at least not- the only time it matters for promotion in the UK- I think is when you go for professor- because when you go for professor- you got a sort of external review by other people in your field. So I think anytime that you're dealing in your field- that it matters quite a lot for prestige. But I think other than that- at least in the UK- it's not so important for promotion criteria.

00:15:56

Marcel: Ok. And you mentioned earlier on that people or scholars get into kind of the right circle and the earlier- an early idea of journals was that they are kind of a network or they are kind of the communication venues of certain networks of discourses. Do you think that this is still true or are they- do they have different functions now and people get to know the work of others already by other means- like social media- email lists or conferences or so on?

00:16:43

Melissa: That's an interesting question. You know- I've never thought about that. And- I think that's for the most part- that the leaders in- the leaders in any area- their work is already known before it's published and the publishing is sort of the official stamp of the ideas that they've already been sort of working out in conferences and in discussion and in conversation and in email and that kind of thing. However- I think for younger people

coming out that the established big brand journals are a way for them to become recognised. So I think it depends on where people are in their career- exactly what kind of function the journals play.

00:17:37

Marvel: Yeah. And if we think of the other side of the sort of say not high impact brands or low impact brands then- why do people or why do scholars publish in those venues and not rather try to improve their publications so then to publish in the high impact journals as well?

00:18:09

Melissa: At least in my field- I think there are a number of reasons that can happen. One reason is that there's- you know- quite strong status quo biases. So that if you have a sort of very unorthodox piece- it's very difficult to get it published in a- in its first sort of incarnation in the top end journal. So a lot of people when they have unorthodox ideas- either put them in a collection- in a volume- or they put it somewhere lower first and see if it gets a little bit of buzz. And then they publish another sort of version of the idea later to see if it gets some buzz in a higher impact journal and so on. So it can be a sort of stepping stone. That's one reason. Another reason is desperation. So I think especially younger people who really need publications when they hit the job market- when they're coming out or at the start or in a postdoc or people who are towards the beginning of their career. You know- these journals take a very long time. And so often if you try once or maybe even twice to get a high impact place- what you get for the publication- you know- you feel like- look I just can't keep cycling with this one- I'm not going to try another one up in the high impact end- I'm just gonna get to somewhere lower. I think that's a reason- you know the kind of desperation about it. And some people- I think don't care. I have talked to some people who just feel like for the reasons maybe that- we were saying it's gonna go like- since for promotion it's not so important until later on. But it is important that you

have something- I think some people- just as long as they have- especially if they have a job that's already stable- secure- they don't care about that anymore and they just send it out to have it out somewhere.

00:20:04

Marvel: Yeah- ok interesting. So what is important for you when you think about publishing your next article? How do you approach the decision? How do you make the decision?

00:20:20

Melissa: Well I write first without a journal in mind- I think I don't think everyone does that. And- but I guess- like that sort of- I kind of go since- I really- since the second year at the very beginning- I really try to silence the voices about some of the status and pressure- social pressure on this stuff. So- for me that's just not the kind of philosopher I want to be. And without judgment about people who have other goals. That's just not what I'm interested in doing. So I write first and then after I have something I'm happy with- then I start thinking about where to go. And I sort of start by thinking about the top 20 in my field and where the paper might within those top 20 might be best suited- if it's too long for that- if it's too unorthodox for that. And then at that point I will make adjustments and will make adjustments. So often it's too long or I feel like it's too empirical or I feel like it doesn't connect enough with certain kinds of ideas or so all of- I'll adjust it that way. But I don't game it in the way other people sometimes do in the way they were talking about before. I don't substantively change the content. I don't add additional citations. I don't go look at who's likely to review it. I don't keep track of that kind of stuff. Yeah- and I don't write with a particular journal in mind. I think that's a big difference. I think a lot of people- I mean that's a big difference between a kind of approach. I think a lot of people write with a journal in mind.

00:22:03

Marvel: Yeah. Right- ok. And as you mentioned the research excellence framework and like

sort of refable content- does the REF have a direct effect on your publishing decisions or publishing behavior or also the writing behavior? Or is it more like an underlying thing that you have in mind because of your position?

00:22:29

Melissa: I think for me it's an underlying thing I have in mind.

00:22:32

Marcel: Ok-

00:22:32

Melissa: Yeah- yeah. I think if I were different- if I had- if I were a less sort of- I don't know- rebellious thinker- some people are sort of more tinkerers by nature. So they're not interested in sort of working out big or controversial ideas. And so I think then the pressure from the REF might be different- but I already kind of naturally write in a more refable way- which is fortunate for me I think

00:23:00

Marcel: Ok. And related to like future REF cycles is the question of open access- which is also connected often to the funders- like green or gold open access publications. Is this an issue for you? Or do you have any kind of department guidelines or funder guidelines that you have to follow?

00:23:30

Melissa: No I don't think so. No. I don't worry about that.

00:23:34

Marcel: Ok. Good- so you- as we talked about- like or the other approach that you mentioned- that some people follow regarding their publishing- that they think about the journal or they think about the publication lists first- so to say- what do you think- what would need to change in academia- like more generally again- so that this kind of behavior stops or changes towards people really write and publish for the discourses again?

00:24:17

Melissa: Oh boy. I mean I think the incentives- the social incentives would have to change. But I don't know- what incentives actually have to change. I don't know how you would get that process going- I think. Yeah- I think it would take quite a lot- unfortunately- yeah.

00:24:52

Marcel: And one other question related to this- if you have PhD students or PhD students in the future- what is your- what do you tell them or what would you tell them regarding publishing? Should they start publish early on or should they focus on their PhD and developing an own research program first? What would you- what would be your advice?

00:25:20

Melissa: Yeah I advise them to try to publish at least one good thing in a good place before they hit the market. Yeah- and ideally two and possibly three. And I think- I think that's said- I wish that weren't the case- but I think it would be- you know- a dereliction of duty not to encourage them in that way.

00:25:43

Marcel: Yeah- right. So they have to kind of engage in this actually not favorable practice to actually be able to become an academic right?

00:25:58

Melissa: I would not- I wouldn't put it quite so negatively as that- even though I think the system as a whole is so negative as that- I think that there are ways to publish and still have integrity [laughs]. I just think- I just think that's more difficult- because there's a lot of pressure to do it in a way that I myself find compromised- you know- intellectually compromised. But I think that I don't-you know- I don't feel like I've published stuff just to publish it. And I've been able to keep up and have some semblance of status in my area. But you have to kind of plant your flag in a way and make clear that you're publishing the way that you want to- it's not that I'm trying to be this other thing and have failed. That

I'm publishing a couple of things a year in good places cause that's the kind of writer I want to be. So I think that's the kind of thing I'm encouraging the students. You know- look at the fact of the world is that you need to publish in these places if you want to get a job. Once you get a job you have more freedom- I think. At least for the UK- for the reasons we were saying before- but in order to get a job- you may have to- but you have to find a way to do that and still be true to yourself. And I think you can you know- it's just harder.

00:27:19

Marvel: Right. Ok. So more abstractly- how would you define an excellent researcher in your field?

00:27:38

Melissa: Someone who regularly contributes interesting careful ideas to the conversation.

00:27:48

Marvel: Ok. And is that different from like a productive researcher?

00:27:51

Melissa: Yeah sure. One could be productive and not be excellent.

00:27:59

Marvel: Ok- so excellent is really more- more focused on the content than productive like the-

00:28:05

Melissa: Yeah- definitely. Yeah- but I mean- I do think you should- there should be regular contribution. It's just I guess- I think that could be a lot less than some people feel they need to be.

00:28:20

Marvel: Yeah- right. Ok. And then the last question already- it's also a bit more open. It's kind of a scenario where a colleague approached you with the idea of creating a new

independent journal and would ask you if you would publish it. Would you do it and what were the requirements that the journals would fulfill so that you publish in it?

00:28:53

Melissa: I think there would need to be a reason for a new journal. So there needs to be a really clear mission.

00:29:00

Marvel: Yeah. Ok- good. That's it from my side. Unless you have any other open comments- anything about this whole issue?

00:29:14

Melissa: No- I mean- well- I say no and then I'm going to say something. I just I think it's great that your- research like yours about it is going on. It's high time for some- I think. And I think- I would guess that most people feel that way. But no one's very happy with the norms- but we all keep operating with them anyway. So it's good to- be thinking about them and addressing them and challenging them. So I'm glad you're doing work you're doing.

00:29:39

Marvel: Yeah- great. That's also what I hear some- quite often from researchers. Most people engage in the practices and would like to change them- but it's kind of requires a collective effort to change your mind and the presumptions.

00:29:53

Melissa: Yeah- yeah.

00:29:57

Marvel: Yeah. Good. Well thank you for your time. It was an excellent conversation.

00:30:06

Melissa: Of course. I hope it's helpful.

A 10. Exemplary Interview German: Clara

00:00:58

Marcel: Warum publizieren Sie Ihre Forschung?

00:01:04

Clara: Warum ich meine Forschung publiziere?

00:01:06

Marcel: Genau-

00:01:07

Clara: [laughs]- naja- weil wir dazu verpflichtet sind- und- das ist der eine Grund- weil ich dafür bezahlt werde. Und zum anderen weil ich immer schon schreiben wollte und es am Ende die Form geworden ist mit der ich an die Öffentlichkeit trete.

00:01:30

Marcel: Und inwiefern versuchen Sie- also versuchen sie das zu balancieren zwischen einer breiteren Öffentlichkeit und der professionellen Forschungsöffentlichkeit mit Publikationen?

00:01:42

Clara: Ja- also wenn Sie das auf meine andere- meine erste Aussage beziehen- das ist eine Entscheidung die ich früher mal nach Ende des Studiums getroffen habe. Zu sagen dass ich nicht freie Schriftstellerin werde und auch nicht in den Journalismus gehe sondern eben wissenschaftlich schreibe also insofern ist da schon irgendwie eine Richtungsentscheidung gefallen und sofern dieses- dieses nennt man das publizistische schreiben oder so- das ist jetzt nicht im Vordergrund bei mir. Aber je mehr eben auch an die Wissenschaftler herangetragen wird dass man sich wissenschaftskommunikationsmäßig an eine breitere Öffentlichkeit wenden soll umso mehr Gelegenheiten ergreife ich dazu und tu das- und das wollten sie jetzt ja wahrscheinlich wissen- durchaus mit den neuen Medien- gerade in einem Forschungsprojekt von der DFG tun wird das in einem Blog wo so eine freiere Form

möglich ist. Und ansonsten sind es- gibt es ja auch verschiedene jetzt eben nicht Peer Review Journale aber durchaus was man früher vielleicht mal als Mitteilungsblatt oder Informationen für etc pp bezeichnet hat als solche nicht gerade peer reviewte Literatur gibt es ja durchaus auch. Ab und zu werde ich dann auch schon mal gefragt ob ich nicht was schreiben möchte.

00:03:26

Marvel: Und bezüglich der peer reviewten Fachzeitschriften haben Sie das Gefühl dass das mehr geworden ist dass da mehr Peer Review stattfindet im Speziellen in ihrer Disziplinen?

00:03:41

Clara: Das ist schon der Fall. Wobei- ich bin ja Geschichtswissenschaftlerin und da gibt es auch- da gab es jetzt eine lange Phase wo wir sehr viel immer in Sammelbänden publiziert haben. Was nicht heißt dass die nicht auch begutachtet werden da gibt es ja durchaus auch Beiräte und Herausgeber die kritisch darauf schauen. Aber das ist nicht das klassische Peer Review. Natürlich. Ansonsten wird das aber schon mehr- also beziehungsweise es wird zumindest in Forschungsanträgen einem überall suggeriert dass die peer reviewten Journalbeiträge höherwertig sind als die aus Sammelbänden- genau.

00:04:35

Marvel: Und würden Sie zustimmen dass die Fachkommunikation- also die Kommunikation nicht zur Gesellschaft sondern eher für Fachdiskurse- ein Stück weit formaler geworden ist durch Peer Review? Weil das halt so viele formale äußere Kriterien sind nach denen beurteilt wird bisweilen?

00:04:58

Clara: Formaler- meinen sie das im Sinne von uniformer- oder?

00:05:00

Marvel: Beispielsweise auch uniformer- vom Inhalt her beispielsweise dadurch das nach

bestimmten Kriterien geschaut wird- aber auch so wie quasi entschieden wird was eine gute oder schlechte Publikation ist- ob das nach äußeren Kriterien entschieden wird?

00:05:21

Clara: Ja- gefühlt würde ich das schon so sagen weil jede Form des irgendwie mal kreativeren Schreibens wird einem gleich auch unterbunden oder gestrichen. Also wenn es auch nicht gleich abgelehnt wird- aber genau- das kann man schon sagen und deshalb schon auch weiß es nicht- vielleicht hat es auch mit der Fantasielosigkeit vieler Kollegen zu tun aber das hat das System wahrscheinlich schon so erschaffen.

00:05:56

Marvel: Ok. Und ein Teil dessen was man so oft sieht wenn man auf Peer Review schaut ist die Selektion die durch Peer Review stattfinden soll- und eng zusammenhängend mit dieser Selektion sind die Publikationsmarken beispielsweise Reihenmarken oder auch bei Fachzeitschriften die Namen der Fachzeitschriften- wie sinnvoll finden Sie solche Publikationsmarken?

00:06:28

Clara: Also Marke im Sinne von Renommee?

00:06:32

Marvel: Genau- beispielsweise- also zum Beispiel eine Selektion anhand von Fachzeitschriften- das kann man ja auch umdrehen wenn man das- als Leser hat man eine Liste von verschiedenen Beiträgen vor sich und dann funktioniert das gegebenenfalls als eine Heuristik dass man sagt- ich schaue mir erst einmal die Beiträge aus bestimmten Fachzeitschriften an weil die gegebenenfalls qualitativ besser sind. Findet sowas statt?

00:07:00

Clara: Ich verstehe. Und die Frage war jetzt nochmal ob ich das gut finde?

00:07:05

Marcel: Ob sie das oder überhaupt- ob sie sehen dass dieser Prozess so stattfindet bei ihnen und was sie- den Wert dessen finden-

00:07:27

Clara: Schwierig- das ist schwierig zu sagen. Ich kann das nur sozusagen- das ist ein völlig subjektiver Eindruck- also der hat der hat bestimmt auch ein bisschen mit meinem Alter zu tun- also dass ich eigentlich in einer Phase bin wo ich nicht mehr danach ringen muss irgendwo schreiben zu dürfen. Das ist ja mittlerweile so dass ich von sogenannten A-Journals Anfragen bekomme ob ich nicht was schreiben würde. Verstehen Sie? Und dadurch ist das Ganze ja pervertiert. Es ist ja nicht so- und ich bin selbst im Herausbergremium von drei Zeitschriften- es ist ja nicht so dass da jährlich 100 Beiträge eingehen und nur zehn drucken können. Das ist ja schon so dass wir echt mal gerade die qualitativ wirklich guten- also drucken wir halt auch alle ab. Und die maximal die Hälfte wird abgelehnt. Aber die sind dann halt auch- die sind dann auch wirklich einfach nicht gut oder müssen überarbeitet werden. Das ist der Punkt. Und deshalb- das hat was mit den vielen vielen Publikationsorganen zu tun die es gibt. Es gibt in der Geschichtswissenschaft auch gar nicht so viel Innovation im Sinne von Neugründungen von Zeitschriften. Es sind viele traditionelle die es immer noch gibt- die die es schon seit 150 Jahren gibt teilweise- und wenn mal was Neues passiert dauert es lang bis die sich einen Namen gemacht haben- etabliert sind. Das was den steilsten Aufstieg hat- das sind jetzt diese ganz neuen Formen des golden Open- Open Access wo man gar nichts- gar nicht mehr druckt.

00:09:37

Marcel: Ja- Ok. Aber sie finden- oder finden sie dennoch wenn man bei diesen Fachzeitschriften bei denen sie im Herausgeberrat sind wo Peer-Review stattfindet aber dennoch eigentlich gar nicht so viel durch Peer Review selektiert wird- ist Peer Review

dann dort sinnvoll oder wären beispielsweise redaktionelle Entscheidungsprozesse genauso sinnvoll

00:10:03

Clara: Also-

00:10:03

Marcel: Ob da quasi ein Redakteur- eine Redakteurin sitzt die das vorselektiert?

00:10:10

Clara: Ja- ja es gibt ja alle möglichen Formen je nachdem wie gut diese Herausgeberschaften ausgestattet sind. Es gibt einfach welche da machen das wirklich- ich sag jetzt mal so meine Kollegen alleine- haben vielleicht maximal noch eine studentische Hilfskraft die sich irgendwie um die Korrespondenz und Verwaltung der Beiträge kümmert. Da kann man jetzt nicht groß von Redaktion sprechen wenn das alles in einer Person liegt oder in zweien- dreien oder so. Und dann gibts ein paar wenige wo es wirklich professionelle Stellen gibt dafür- es gibt auch welche die noch- es gibt auch Zeitschriften die gehören einem Verlag. Da ist auf jeden Fall ein Profi-Layouter da- aber die machen ja keine inhaltliche Prüfung. Also eigentlich- es ist schon so in der Tendenz- je mehr Prüfungsschritte man hat- aber da würde ich jetzt die fachliche Begutachtung nie rauszählen- sondern die fände ich immer noch die wichtigste- je mehr Schritte man hat umso mehr- umso besser kann auch ein Beitrag werden. Das ist selbst bei mir so. Wenn mir noch mal im allerletzten Moment jemand über das- jemand so ein Copy-Editing macht- der findet immer noch 10 Dinge die er mir sozusagen verbessern kann und das hat einfach mit dem anderen Blick der Leute zu tun die haben anderes- man kann nicht auf hundert Dinge gleichzeitig achten bei einem Beitrag und deshalb- also ich finde es nicht schlecht je mehr Leute dabei sind. Ich würde es aber- wenn ich es- wenn es aus Personalmangel reduziert werden würde würde ich nie auf die reine Redaktion schieben die inhaltlich nichts mehr sagen kann oder das sind vielleicht auch manchmal ehemalige

Wissenschaftler- aber wenn die ja zehn Jahre aus der Wissenschaft draußen sind können die ja inhaltlich nichts mehr sagen und da passieren dann Böcke. Das sind ja- ich war ich- ich hab das nie verfolgt aber das fällt mir jetzt gerade ein es gab doch so eine Zeitschrift die wird vom Hannah Arendt-Institut in Dresden herausgegeben und da hat tatsächlich jemand mal darauf angelegt diesen ganzen Peer-Review Prozess sozusagen einen Streich zu spielen und hat einen Aufsatz über den Deutschen Schäferhund gereicht. Irgendwie wo es- ich hab ihn nie gelesen- wo es halt auch um irgendwie der Schäferhund und den Nationalsozialismus irgendwie- was aber eine reine Fake-Geschichte gewesen ist und der ist durchgegangen durch alle Etappen und der ist gedruckt worden. Und daran sieht man ja schon- also ist ein gemeiner Streich gewesen- aber daran sieht man schon dass eigentlich Fachbegutachtung schon ganz wichtig wäre.

00:13:06

Marvel: Genau- das kann ad absurdum geführt werden. Es stimmt aber wenn sie auf den gesamten- ihren Fachdiskurs schauen würden und wenn- da findet ja auf jeden Fall eine Selektion statt die ja auch in bestimmten Kreisen gut ist- würden sie sagen dass aber insgesamt mehr oder immer mehr publiziert wird? Als vor 20 Jahren beispielsweise- oder 10 Jahren?

00:13:30

Clara: Insgesamt ja also- das würd ich schon sagen. Es wird mehr publiziert und- was mir persönlich sehr aufgefallen ist- ist einfach dass sehr- dass jüngere Leute sehr viel mehr publizieren als früher- also das ist- zumindest in der Geschichtswissenschaft kann ich das sagen- das ist- also man halt selten während der Promotion schon richtig publiziert. Ich selbst hatte wirklich im allerletzten Jahr hatte ich dann mal einen Aufsatz publiziert der mehr oder weniger in dem Moment rauskam als ich meine Verteidigung hatte. Das war schon gut- das war schon eine Besonderheit in gewisser Weise und heute- also dass- meine eigenen Leute haben ja schon drei oder vier Aufsätze publiziert bevor sie abgeben. Da ist

insofern- und wenn sie dann drinbleiben bleiben in der Wissenschaft haben die sehr viel früher eine lange Liste an Publikationen.

00:14:34

Marcel: Aber- was würden Sie sagen woher kommt das- dieser Druck beispielsweise mehr oder früher- schneller publizieren zu müssen. Können Sie das sagen wenn Sie beispielsweise an ihre eigenen Doktorandinnen- Doktoranden denken?

00:14:56

Clara: Also das wird denen ja erzählt- das erzählen die sich gegenseitig. Ich sage das denen nicht. Ich sage immer nur wenn ihr einen Aufsatz publiziert hat bin ich ja schon zufrieden. Das hilft euch auch euer Thema nochmal richtig zu formulieren oder so. Den Tipp gebe ich mit- so- aber die werden ja- die gehen auf Fortbildungen. Das gibts ja jetzt überall- alles was ich selbst nie hatte- sodass Doktoranden halt sozusagen alle möglichen Softskillkurse- sind ja auch ein paar sinnvolle dabei oder was auch beim Lehren- wie bereite ich ein Seminar vor oder so etwas- da fängt es ja an- aber das geht ja auch hin zu promovieren während der Promotion oder wie publiziere ich meine- das ist ja schon auch etwas was man lernen kann. Während wir früher so was immer erst gelernt haben nach es irgendwie verteidigt war und wir uns auf dem Markt irgendwie schlau gemacht haben kriegen die das heute systematisch in Kursen beigebracht und die die sehr sozusagen- international auch competitive sind die erzählen sich natürlich alles was man für so ein Starting Grant alles schon braucht. So und daher- insofern wird es- ist es das- ist das das allgemeine System sozusagen sowohl der Forschungsanträge die darauf warten dass man so eine hohe Anzahl an Dingen schon vorweisen kann- dann aber auch- und ich denke das ist ein bisschen parallel auch gelaufen dieses System der Vergabe von Stellen insbesondere nach der Diss. Das ist ja früher- ist ein bisschen pauschal gesagt aber früher hat ja irgendwie noch jeder der mit summa irgendwie promoviert hat der hat auch automatisch eine Assistentenstelle gekriegt- der hat auch ein so- wie sie- die Professoren haben da auch ein bisschen auf diese

Karrierebildung geachtet auch wenn dann halt für 6 Jahre dann kein Platz mehr war für eine zweite Person. Aber so wurden die Karrieren gemacht und heute ist es ja alles viel offener. Auch durchaus demokratischer- also man muss nicht seinem Professor den Koffer tragen damit man eine Stelle kriegt. Es gibt sehr viel andere Möglichkeiten der Finanzierung. Aber es gibt halt auch mehr Menschen die darum ringen und insofern wird dadurch ja auch indirekt mehr Competition produziert. Und dann wird gezählt- da wird geguckt was hat er denn- wenn man jemanden einstellen will- was hat er denn schon an Publikationen publiziert. Das ist bei Postdoc stellen nicht anders wie bei Professuren dass man da mal drauf schaut und wenn jemand nichts publiziert hat würde man niemanden- heutzutage glaub ich- niemanden einstellen.

00:17:58

Marcel: Und neben der reinen Quantität- natürlich bei Berufungen wird dann auch noch auf den Inhalt geschaut- glauben Sie dass da auch die Namen der Fachzeitschriften relevant sind wenn man solche Listen überfliegt?

00:18:16

Clara: Also wir tun das eigentlich schon. Nicht nur ich- sondern- das mache ich- wir sind ja- oft trifft man die Entscheidungen ja nicht alleine sondern in Kommissionen. Gerade gestern war ich wieder in einer. Und da wird schon- immer dann wenn es auffällig wenig oder auffällig viel wird- wird dann vergleichend darüber gesprochen was die Leute publiziert haben und dann wird als Kriterium schon angewandt wo da publiziert wurde oder ob es international genug ist. Das spielt eine Rolle- also sicherlich nicht als einziges Kriterium aber es ist dann halt eines von dreien oder viere- das Projekt- das neue Projekt muss- das ist natürlich das ist das A und O- das neue Projekt muss auch überzeugend sein- was Neues versprechen. Und als nächstes schaut man dann ob jemand zielstrebig ist und ob jemand fleißig ist. So kann man es eigentlich zusammenfassen.

00:19:21

Marcel: Ok. Und dass so viel publiziert oder dass mehr publiziert wird und dass insgesamt insbesondere auch bei jungen Wissenschaftlern- Wissenschaftlerinnen so viel mehr publiziert wird- glauben Sie dass das für den fachlichen Diskurs sinnvoll ist?

00:19:40

Clara: Nee-

00:19:40

Marcel: Ok-

00:19:40

Clara: Nee das glaube ich nicht weil wir das alles gar nicht mehr lesen können.

00:19:44

Marcel: Ja- das wird schon- das so ist so eine Überhand dass teilweise schon mehr für die Autorenschaft als für die Leserschaft publiziert wird?

00:19:50

Clara: Ja- genau. Also für die Autoren selbst ist es eigentlich am wichtigsten. Und vielleicht noch dann in dem Kontext in dem sie sich befinden also dass man das dann in einem Kolloquium bespricht oder mal jemandem zum Lesen geben kann dass man sich damit bewerben gehen können. Aber im Gros hat diese Masse nicht dazu geführt dass sie jetzt irgendwie profilierter in ihrem Fach sind. Da gibt es ja auch genug Aussagen darüber wie häufig irgendwie diese ganzen Aufsätze gelesen werden- also das ist ja erschreckend erschreckend wenig wie die Dinge rezipiert werden.

00:20:31

Marcel: Das stimmt. Und wenn man mal nicht auf die hoch selektiven Fachzeitschriften schaut sondern auf die anderen Fachzeitschriften- es muss ja dann auch die Gegenseite quasi geben- wo ja auch immer mehr publiziert wird- wo es quasi ein bisschen fast schon Massenportale und Massenzeitschriften gibt- finden sie dass es wertvoll ist in solchen

Sachen zu publizieren wenn sie gar nicht erst in bestimmte Fachzeitschriften reinkommen oder sollte man dann eher probieren die noch zu verbessern und zu warten?

00:21:13

Clara: Ja also- zu warten meinen sie im Sinne von- die ausreifen lassen?

00:21:18

Marcel: Genau- also während der Promotion nicht vorschnell zu publizieren-

00:21:28

Clara: -mhm-

00:21:29

Marcel: -sondern es mehr zu überarbeiten?

00:21:29

Clara: Ja- also ich glaube- am Ende ist wahrscheinlich ein ausgewogenes Maß das richtige Rezept dafür. Das würde ich sagen- weil jetzt nur in unbekanntem Zeitschriften oder in Blogs zu publizieren das führt dann auch nicht viel weiter. Und für einen guten Aufsatz braucht man halt auch mal wirklich drei Monate oder so. Jedenfalls erzähle ich immer so- und deshalb- das muss man einkalkulieren- das kann man nicht immer- da ist manchmal einfach die Zeit nicht dazu da das zu machen- das heißt es das sollte eigentlich selten genug sein etwas qualitativ Reifes zu produzieren- das braucht seine Zeit aber so ab und zu mal- ein- zweimal im Jahr so was Gutes zu tun das ist schon sehr sinnvoll. Und dann- genau das andere finde ich aber auch wichtig. Ich selbst- gerade deshalb gefällt mir auch das Schreiben so in Blogs oder so weil ich dann tatsächlich mal in drei Tagen was produzieren kann was mir spontan einfällt und was ich einfach mit Lust mir von der Seele schreibe sozusagen. Das ist ja auch gut. Aber jetzt nur- selbst ich sollte es mir- sollte nicht auf die Idee kommen irgendwie 20 oder 30 Blogbeiträge pro Jahr zu machen dafür aber irgendwie keine seriösen Publikationen mehr. Das ist für mich nicht- am Ende ist es für mich nicht

hochdramatisch weil ich nicht mehr um Stellen kämpfen muss. Aber bei jüngeren Leuten- genau- ich denke so eine Ausgewogenheit sollte da sein

00:23:19

Marcel: Und es wird ja allgemein gesagt in der Geschichtswissenschaft ist die Monographie ja noch sehr wichtig- ist das bei Ihnen- speziell bei Ihnen im Feld auch so?

00:23:29

Clara: Ja.

00:23:31

Marcel: Und wenn sie an eine Monographieveröffentlichung denken- was ist da für sie wichtig für die Entscheidung wo sie publizieren- also welchen Verlag beispielsweise sie auswählen oder eine bestimmte Reihe?

00:23:48

Clara: Also es gibt jetzt nicht für jedes Thema die ideale Reihe. Aber das finde ich- wenn man die Kriterien zusammenbekommt dann würde ich schon sagen es sollte ein Verlag sein der ordentlich auf dem Markt dasteht- also das heißt- so praktisch es ist aber dieses einfach nur bei seiner eigenen Universitätsbibliothek- das heißt die Diss als PDF auf einem Server zu stellen- das wird nicht richtig wahrgenommen. Also das kann man dann machen wenn man tatsächlich ein wenig aus der Wissenschaft rausgeht. Da müsste zumindest in Deutschland noch mal mehr passieren- da müssten sich wieder mehr Unis durchringen eigene Universitätsverlage zu gründen- weil diese Diffusion die Präsenz auf den Messen- das Sorgen dafür dass Rezensionen geschrieben werden und das ist ja schon wichtig. Also da würde ich immer zu einem soliden Verlag raten- der vielleicht auch noch Zeit für ein kleines- ein richtiges Lektorat gibt's ja eh nicht mehr- aber wo da erfahrene Leute sitzen die den Buchmarkt kennen- dann eine Reihe empfehle ich auch immer für einen- für jüngere Leute und ich empfehle immer auch auf den Preis- also auf den Verkaufspreis zu achten.

Es hat keinen Sinn ein Buch seine Dissertation für 98 Euro auf den Markt zu werfen. Das kaufen nachher zehn Bibliotheken und dann hat man gar nichts davon.

00:25:31

Marcel: Genau-

00:25:31

Clara: Also so eine Mischung-

00:25:31

Marcel: Und daran anknüpfend und auch an das was ich vorhin schon bei den Zeitschriften gesagt haben dass- die Frage ob sie dem zustimmen würden dass bestimmte Verlage- bestimmte Reihen- bestimmte Fachzeitschriften so eine Art Netzwerkwirkung haben- dass sie quasi gemeinschaftsbildend sind indem sie Leser und Autoren zusammenbringen.

Würden Sie dem zustimmen? Wo sie ja gerade gesagt haben dass- wenn man nur das PDF hochlädt dann kommt das überhaupt gar nicht irgendwo an und bei bestimmten Reihen wird das ja gesehen. Glauben Sie dass das noch der Falls ist?

00:26:09

Clara: Ja aber das ist ein Unterschied zwischen Monographien und Zeitschriften- also die paar Zeitschriftenaufsätze die ich digital habe die haben mit Sicherheit einen höheren Verbreitungsgrad als selbst die die ich quasi mit dem- die ich sekundär digital publizieren konnte- wissen sie das ist dann das grüne Open Access glaube ich- selbst die haben dann weil man ja dann- wenn man ein bisschen Social Media oder sowas noch querschaltet- da haben die binnen kurzer Zeit Hunderte von Lesern erreicht. Das schafft man nicht zu schnell mit einem reinen Papieraufsatz und mit einem Buch halt auch nur dann wenn man jetzt- es gibt ja schon Verlage- De Gruyter macht jetzt schon dass die dann parallel zum Buch- wenn man nochmal 5.000 Euro hinlegt- schalten sie das auch schon gleich frei. Dann haben sie irgendwie ihre Kosten gedeckt oder so. Insofern also beim Buch würde ich sagen ist es nur dann sinnvoll dieses- jetzt im Moment so wie die Strukturen halt sind ist es

so dass es nur dann eine breite Rezeption erfährt wenn es auch noch bei einem guten Verlag ist der für Diffusion sorgt. So kann man es sagen- und jetzt das nur als PDF auf seinem Bibliotheksserver einzustellen- das bringt leider fast nichts.

00:28:08

Marcel: Ok. Dann zu einem angelehnten Thema- die Frage der Exzellenz. Es gibt ja nun eine sehr starke Rhetorik der Exzellenz im Moment sind sie- oder was würden sie sagen ist eine exzellente Wissenschaftlerin- ein exzellenter Wissenschaftler? Wenn Sie das beschreiben würden.

00:28:26

Clara: Also jetzt losgelöst von Publikationen oder in Bezug auf Publikationen?

00:28:33

Marcel: In ihrem Bereich-

00:28:37

Clara: In meinem Bereich- oh- das ist schwierig. Also da muss ich zuerst vorneweg was sagen. Im Grunde genommen ist dieses Wort Exzellenz finde ich schon- also sozusagen Menschen sind irgendwie für sich schon mal exzellent und wenn sie sich dann sozusagen alle korrekt verhalten und es gibt ja durchaus auch so etwas wie ein Publikationswesen- das heißt- oder im Umgang mit Daten oder im Umgang mit mit wissenschaftlichen Kollegen und so weiter- wenn da gewissermaßen ethisch und moralisch alles in Ordnung ist dann sind viele von uns exzellent. So würde ich das für mich selbst formulieren und diese andere Definition ist ja nun die ist ja nun die offizielle. Das ist ein DFG oder Wissenschaftsratsprech oder so- oder ERC oder was auch immer- das ist durch letztendlich mehrstufig hochkompetitive Verfahren sozusagen- quasi mit dem Anspruch irgendwie objektiv zu sein. In einem geleiteten strukturierten Prozess werden diese exzellenten Menschen gefunden. So- das ist die Schnelldefinition- das ist was anderes als

das was man sozusagen auf der Ebene von Menschlichkeit und guter Forschung- das muss auch nicht deckungsgleich sein. Ne?

00:30:37

Marcel: Ja-

00:30:37

Clara: -diese zwei Ebenen. Und ja- und dann kommt halt hinzu dass diese ganze Exzellenzinitiative zu einer- das hat sicherlich an bestimmten Standorten zu einer Profilbildung geführt- man hat auch sicherlich einigen Forschungszweigen was Gutes tun können aber es hat ja auch eine neue Geografie geschaffen. So eine komplette Exzellenzinitiative- das passiert- ist in Deutschland passiert wo sie ein West-Ost-Gefälle haben. Das passiert jetzt in Frankreich wo das noch anders gesteuert wird staatlich weil die einfach das Ziel gesetzt haben dass es in einem Prozess der Vereinigung von mehr Universitäten in derselben Stadt zu Konzentrationsbildungen kommen muss und nur wer das tut kann am Ende einen Exzellenzstatus erreichen und am Ende des Prozesses sollen dann nur noch zehn exzellente Standorte übrig sein. Das sind ja völlig andere Kriterien als da sind ja- das sind einfach politische Steuerungsprozesse- in Frankreich stärker politisch- in Deutschland vielleicht nach Strohschneiders Worten stärker wissenschaftsgeleitet. Aber dennoch erzeugen diese Prozesse auch noch andere Dinge-

00:32:15

---: [the skype connection was shortly interrupted]

00:32:18

Marcel: Genau bis dahin habe ich es gehört.

00:32:20

Clara: Ja ja- sehr viel mehr habe ich nicht gesagt. Ich habe ja auch nur gesagt dass es sozusagen- genau dass dann damit andere Dinge erzeugt werden- in diesen Prozessen. Das muss man immer noch sehen und das sind auch Dinge die man nicht wollte- die man

bestimmt nicht intendiert hat. Manchmal bestimmt sind sie intendiert aber die Tatsache dass halt ganz klar ausgedrückt dass es irgendwie im Osten Deutschlands nur Dresden und Berlin als Exzellenzunis gibt- das hat man bestimmt nicht so erzeugen wollen. Das kommt dann aber dabei heraus wenn man sich wenn man es sich genauer anschaut das einfach diese Ostunis teilweise einfach schlechtere Ausgangsbedingungen hatten sich an dem Prozess zu beteiligen und dann wird der Graben noch größer gemacht.

00:33:10

Marcel: Genau-

00:33:10

Clara: So-

00:33:10

Marcel: Und der dahinter stehende Wettbewerb- glauben Sie dass das eine Auswirkung über die Exzellenzinitiative hinaus hat- oder ist es sowieso schon so stark wettbewerbsgetriebener wissenschaftliche Prozess?

00:33:27

Clara: Ja- es ist glaube ich- es ist ohnehin stark Wettbewerb- das ist ja nicht nur die Exzellenzinitiative gewesen. Es gibt- das ist ja nur die Spitze des ganzen Geschehens in dem wir uns eh schon befinden und was ja auch von Brüssel letztendlich auch so gesteuert wird. Ich glaube aber es gibt halt- die Exzellenzinitiative die hat durchaus auch Skeptiker erzeugt. Ich glaube das könnte der noch der Gewinn des Ganzen sein- das wir eben nicht nur jetzt irgendwie wissen wer alles den- die Exzellenzmedaille sich um den Hals hängen darf sondern dass wir umgekehrt auch Skeptiker des Systems dazugewonnen haben.

00:34:11

Marcel: Ja.

00:34:11

Clara: Und das dann vielleicht dadurch auch mal wieder eine Besinnung und vielleicht eine Verlangsamung des Ganzen auch stattfindet.

00:34:18

Marcel: Ok.

00:34:18

Clara: Also- so- ist vielleicht ein bisschen esoterisch aber sozusagen das geht- am Ende ist es- meine ich- da ist es schon auf der Ebene was ich vorhin schon sagte was wir bestimmt auch nicht verneinen- dass man einfach- dass so viel produziert wird dass man schon gar nicht mehr lesen kann oder dass wir halt einfach dass es teilweise Leute gibt die dann erfolgreich sind wenn sie halt keine Familie haben. So können wir ja nun nicht alle werden.

00:34:54

Marcel: Ok. Das war es so weit meinerseits.