The sociolinguistics of academic publishing: A relational approach to language choice

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This study presents a sociolinguistics of academic publishing in historical as well as in contemporary times. From the perspective of Swedish academia, it unites a wide range of scholarly knowledge, including perspectives from the sociology of science, history of science and ideas, and research policy. The study focuses on publishing practices in the empirical realities of two disciplinary fields, history and psychology. Drawing on facts and figures from publishing practices as well as interviews, the study argues that English is currently making inroads into the field of history, in line with and aided by the field-external power of new regimes of research evaluation and performance-based funding impinging on the university field at large. In the field of history, unlike in psychology, the English language is thus currently a weapon since it provides access to international publishing markets where new forms of scientific authority can be obtained. This option seems to be most compelling for junior scholars seeking to enter the field. Following Bourdieu, publishing in English is here interpreted as pertaining to a social strategy, enacted in pursuit of investing differently, so as to subvert the order of the historical field.

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1 Introduction

1.1 Publishing language

In recent times the position of English in academic publishing has attracted growing interest from scholars worldwide. There has been, notes Williams (2010, 52), a tendency for English to become 'the international vector of academic visibility', which is linked to the fact that scholars 'are integrated into an international network which operate [sic] increasingly through the medium of English.' As an object of study, this phenomenon is approached both from a historical perspective (e.g., Gordin 2015; Kaplan 2001; Mortensen & Haberland 2012) as well as in studies that, with varying foci, account for its unfolding in the publishing practices of present times (e.g., Canagarajah 2002; Lee & Lee 2013; Lillis & Curry 2010; Salö 2015). In scholarship on language in society, the rationale for addressing this topic is commonly critical, aiming to highlight the hegemonic role of English in contemporary research and higher education, and the consequences of this state of affairs for other languages and communities (e.g., Ammon 2001, 2012; Carli & Ammon 2007; Durand 2006; Gnutzman 2008). Across northern Europe and beyond, a large body of scholarly work – papers, reports, edited volumes, special issues, and so forth – now exists on questions sensitive to the so-called 'Englishization' (e.g., Hultgren & Thøgersen 2014) of universities on a global scale. For example, the chapters in Ammon (2001) comprise accounts from a wide range of contexts across the world, while, more recently, the chapters in Hultgren et al. (2014) present accounts of ideology and practice in Nordic university contexts more specifically.

This study seeks to produce a better understanding of the sociolinguistics of academic publishing by exploring the case of Swedish academia. The main rationale for addressing this issue is to expand the knowledge base with respect to the predominant position of English in the realm of science and the diverse subspaces it comprises. Understanding this situation means bringing empirical specificity and an in-depth, historical gaze to the question of language in scientific publishing. Additionally, it requires producing knowledge on the constraints for publishing practices imbued in the contemporary university field and its prevalent research policies. And, finally, it entails a solid social-theoretical idea of human action to account for the logic of practices of the dwellers of university life. To accomplish this undertaking, the study subscribes to the scientific approach of Pierre Bourdieu, which has many virtues for exploring these issues. For the purposes here, it invites the analyst to take a relational approach to academic publishing. In this context, 'relational' entails investigating publishing practices at the intersection of the histories of disciplines and the dispositions of contemporary researchers – shaped by these very same histories. By this logic,

> [t]o understand the practices of the producers and their products entails understanding that they are the result of the history of the positions they occupy and the history of their dispositions. (Vanderberghe 1999, 54)

Combining historical analysis with interview accounts, this task is undertaken with a view to uncovering the historicity, the present state, and, to the extent possible, the future developments of the matter at hand. On this latter point, however, I concur with the position of Sörlin (1994, 27) in holding that historical research cannot actually foresee the future, but rather provides us with well-informed reflections on forthcoming events and development. Thus, to at least stand a chance of anticipating that which is yet to come entails understanding the past in relation to the currently transforming realm of science, and here, Bourdieu's gaze is valuable.

1.2 Rationales, objectives, and organization of study

The backdrop of this endeavor is the predominant position of English in research and higher education, which in Sweden, as well as in other nation-states, has arisen as an issue of major language political relevance. A case in point is provided by languages used in scientific publishing, where English currently dominates across most disciplines (e.g., Salö 2010). However, as this study seeks to highlight, it is noteworthy that issues of publishing language are not only – or even primarily – matters of language *per se*. Rather, publishing practices, as well as the languages used for these ends, are entangled in a web of other facts. The university field is a battleground between various – at times, conflicting – interests, including those of language. These struggles have sociolinguistic implications, and as analysts we had better make sure to record and account for these implications as a part of the bigger picture. On this point, as Paasi (2005, 772) remarks, 'the English language is the medium, not the cause.' Yet, as I shall argue here, publishing language is a window into these struggles. Trends and disciplinary differences in publishing language stand proxy, as it were, to a set of tendencies that are currently on the move within the realm of university life (e.g., Kennedy 2015).

There is ample literature to suggest that university life is currently undergoing a profound makeover globally that is changing longstanding academic values and practices. In short, what we are witnessing is a process whereby a general drift toward globalizing knowledge is gaining the upper hand, which intersects with new research political techniques as an impetus for managing academic work (e.g., Currie & Vidovich 2009; Kauppi & Erkkilä 2011; Kennedy 2015; Naidoo 2003). To be sure, in the realm of science, globalizing processes *as such* are not new; what is new, however, 'is the growth of international networks, funding initiatives, publishing and ranking systems' (Holm et al. 2015, 114). According to critics (e.g., Readings 1996; Rider et al. 2013), these transformations are supplanting the core values of the Humboldtian university as we know it, that is, as linked to the autonomous pursuit of truth (Krull 2005). In its place, the advent of a new managerial regime arises, enforced under the banner of a quality culture, and followed by the imposition of league tables reminiscent of the world of sports (e.g., Tight 2000). The driving force behind this development is an increased competitiveness between states and their universities; their demands for increased accountability and productivity come with pivotal effects on the stakes and strategies of those who take part in the research game across the contemporary globe – that is, researchers – since they are obligated to align their practices to the ideational and managerial visions of those who dictate the terms in the university field at large (Lucas 2006). Intrinsically linked to this process is the increasing importance attached to publications as the key form of symbolic capital in the fields of science (e.g., Putnam 2009), hence the phrase 'publish or perish' as an imperative of the scholarly enterprise of this day and age. However, concerns linked to publishing in general are being increasingly magnified by the drift toward globalizing knowledge and of publishing for transnational marketplaces, as indicated by Lee and Lee's (2013) thought-provoking article titled 'Publish (in international indexed journals) or perish.' While this fact reflects the crude realities of most dwellers of academia globally, the issue is a particularly sensitive one among scholars of the non-Anglophone world. This is so because it follows from contemporary transformations in academia that more importance is also attached to English, which is the language that currently predominates in the marketplace, toward which scholars, willingly or unwillingly, are ever more gearing their publishing practices (e.g., Coulmas 2007; Lillis & Curry 2010).

Understanding these transformations requires a multi-layered line of scientific inquiry. In view of this, the present study delves into the question of publishing language in Sweden by uniting strands of research from the history of science, the sociology of science, and the language of science. These seemingly vast areas of knowledge production converge in the work of Bourdieu. Intellectually, he is a product of a French tradition, and here, the separation between history and sociology is often considered epistemologically unfounded (e.g., Steinmetz 2011); hence, as Bourdieu posits, 'all sociology should be historical and all history sociological' (cited in Wacquant 1989a, 37). Bourdieu's science of science (e.g., 1975, 1988, 2004) takes as its uncompromising starting point that a scientific field is invariably defined by its own history (Broady 1991, 55). In Bourdieu's view, moreover, practices of language are seen both as arenas of social power and as instruments for implementing strategies (e.g., Bourdieu 1977; Wacquant 1989a). The language used in publishing can thus be seen as an index of the ways in which researchers make use of their discursive resources in relation to the strategic possibilities rendered significant by the struggles shaping the social fields and subfields in which they engage (cf. Hanks 2005).

Empirically, the study focuses on practices of scientific publishing in two disciplinary fields of Swedish academia: history and psychology. These two fields occupy different positions in the scientific space at large, and each field can therefore be scrutinized by using the other as an analytical contrast (Löfgren 1992). The emergence of history as an autonomous field dates back to the 19th century, whereas the field of psychology is a more recent construction. With respect to publishing, there is a general sense that the position of Swedish holds strong in history, while English predominates in psychology (e.g., Salö 2010; see also section 2.1). The study combines historical analysis of the two disciplines with interviews of professors and doctors in the same two disciplines, so as to provide an account of the ways in which field histories play out in contemporary publishing practices. According to this agenda, the two pivotal thinking-tools 'field' and 'habitus' are adopted to attain a theoretical understanding of science, disciplines, and researchers. Together with Bourdieu's meta-theoretical propositions, these thinking-tools provide a viable gaze and conceptual pathway for conceiving the logic of the spaces of engagement out of which research is yielded, both shaped by and shaping the agents who dwell therein.

The study is organized as follows. Section 2 introduces the question of language choice in publishing in Swedish academia and its disciplines, where Bourdieu's stance on human action will also be introduced. Section 3 outlines Bourdieu's conception of the dual nature of social life, which serves as a way of explicating the study's design, procedure, and dataset – including notes on methodology. Section 4 expands upon the logic of academic life as viewed through the lens of Bourdieu's relational science of science, that is, as a locus of struggle between agents with differing symbolic and material assets. This is then followed by two empirical parts. The first part (section 5) accounts for histories. First, there will be a general historical description of language use in the early days of Swedish academic life, after which follows more detailed accounts of publishing language in the histories of the two disciplinary fields: history (5.1) and psychology (5.2). The second part accounts for publishing language in current times. Section 6 outlines the currents in research policy impinging on the contemporary university field, with a view to foregrounding recent trends in managing research that direct the preconditions for publishing practices. Then follows a section (7) on habitus as fields made flesh where interview accounts from four agents in the two fields are presented, with one dominant agent (professor) and one newcomer (recent PhD graduate, henceforth *doctor*) from each field. Section 8 discusses the data presented by foregrounding two axes of relationships. Whereas the first relationship pertains to differences between the two fields, the second relationship pertains to university hierarchies as manifested within each field as a difference between dominant agents and newcomers. Section 9 presents concluding remarks.

In brief, the study here argues the following: ‘Language choice’ in publishing should be understood as a form of historical action residing in the relation between histories of scientific disciplines as fields, on the one hand, and histories as incarnated in researchers’ habitus, on the other (Bourdieu 1990a, 190). As such, the choice of publishing language is a social strategy pursued by the researcher in relation to the possibilities offered within the disciplinary field – but also in relation to the conditions of the overarching university field within which disciplinary fields are located. Contemporary transformations of the Swedish university field increasingly orient scientific practices by imposing new ways of rewarding particular forms of scientific performance. This strategy is currently setting the stage for a new form of struggle within some of the disciplinary fields it comprises – the field of history being a case in point. In one sense, the contemporary political vision of the university field, along with the tools used to enforce this vision, carries with it a severe form of misrecognition of the values long upheld in this field, since it does not recognize the value of the practices that incite the agents here. At the same time, this misrecognition opens up new options for newcomers to enter the field. In view of this, the study points to the emergence of a *new scientific habitus*, molded by the desires of challengers to attain competitive positions within the field. Contrary to its status in psychology, English is currently a weapon in the field of history since it provides access to international publishing markets where new forms of scientific authority can be obtained. While English is an avowed investment strategy for newcomers, the tendency to use it devalues the capital of the dominant agents by undermining the principles that were previously most favorable to their products (Wacquant 1989a, 40). By this logic, the study predicts that English will continue to make inroads into the field of history over the years to come – but not without having to overcome some challenges.

2 Language choice in Swedish academia

In Sweden, the balance of power between Swedish and English in Swedish academic life is a well-known topic of ongoing concern – both of scholarly and language political discourse (e.g., Kuteeva 2015 for an overview). Several mappings have shown that currently – and increasingly so – English serves as the chief language of scientific publishing across most disciplines (e.g., Gunnarsson 2001; Gunnarsson & Öhman 1997; Melander 2004; Salö 2010). From the perspective of language planning and policy, this is a problematic development, and, accordingly, Sweden has signed the legally non-committal *Declaration on a Nordic Language Policy* (2006). Among other things, this document makes a plea for so-called *parallel language use* as a way of promoting the use of Swedish (and other Scandinavian languages) along with English in the presentation of scientific results.

In the Swedish debate, the question of variation in language use in publishing practices has traditionally been cast as a matter of 'language choice'. McGrath (2014) argues that policy documents at various levels tend to depict choices as falling upon the individual, thus nourishing a view in which researchers across Swedish academia are portrayed as 'deciding' to publish in English, and in so doing, are 'choosing' to abandon Swedish (see also Kuteeva 2015, 271; Petersen & Shaw 2002, 371). While this may be so, several Swedish studies on these matters have instead highlighted structural constraints to publishing, for instance, in relation to the force of bibliometric valorization (e.g., Salö 2010) or in the context of 'diglossia'– that is, macro-social propensities against which measures should be taken to increase the qualification value of publishing in Swedish (e.g., Gunnarsson 2001). This question, thus, feeds into a well-known debate on structure–agency as a baseline for conceptualizing choice epistemologically (e.g., Paton 2007). In the context of language choice in publishing, it seems clear that commentators take a different stance on this point, and accounts accordingly range between understanding choices as externally imposed and power-laden, 'exposed to Anglo-American "imperialism"' (Gunnarsson 2001, 306), and the more liberal vision in which choices are seen as 'pragmatically determined' (McGrath 2014, 15).

It may be suggested that the most fruitful approach is to opt for a middle ground. As Coulmas (2007, 158) notes, while it can be stated that ‘behind every paper is an individual decision to publish it in one language rather than another’, there is little reason to believe that ‘decisions are made in the void or that choice is free.’ The foundation of Bourdieu’s practice theory offers some purchase in grappling with these matters, attuned as it is to emphasizing choices as relationally situated between constraints and individual agency. As Bourdieu states, ‘We can always say that individuals make choices as long as we do not forget that individuals do not chose the principles of their choices’ (cited in Wacquant 1989a: 45; also Bourdieu 2000, 149). From a sociological point of view, this dimension of ‘handed-down will’ can be specified as follows:

> [H]ow can one fail to see that decision, if decision there is, and the ‘system of preferences’ which underlies it, depend not only on all the previous choices of the decider but also on the conditions in which his ‘choices’ have been made, which includes all the choices of those who have chosen for him, in his place, pre-judging his judgements and so shaping his judgement. (Bourdieu 1990b, 49–50)

A key implication of Bourdieu’s line of reasoning is that all scientific choices made by the researcher, including those concerning publishing forum and language, depend on the agent’s location within her professional universe, with the traditions, habits, beliefs, values, and censorships embedded therein (Bourdieu 2003, 283). To a considerable extent, therefore, this position imports with it a vision of language ‘choice’ as pertaining to values and preferences in which subjectivist accounts offer limited explanatory value. Yet, in respect to social action, Bourdieu’s project comprises the intellectual means to circumvent the two pitfalls of either producing a pure internalist account that overemphasizes individuals’ free choices or an externalist account that overemphasizes the constraints of regulating forces. Instead, it allows for an understanding in which publishing practices are seen as a relational outcome that arises from the encounter between socialized and interested researchers, on the one hand, and a range of possibilities available at a given point in time where they dwell, on the other (Bourdieu 1983, 313). By this logic,

> [s]ocial agents are not ‘particles’ that are mechanically pushed about by external forces. They are, rather, bearers of capitals and, depending on the position that they occupy in the field by virtue of their endowment (volume and structure) in capital, they tend to act either toward the preservation of the distribution of capital or toward the subversion of this distribution. (Bourdieu, cited in Wacquant 1989b, 8, emphasis removed)

Researchers, then, do not follow rules but pursue strategies (see below section 4). Likewise, from the vantage point opted for here, apprehending language choice as comprising ‘choices’ about language only appears to be a gross simplification. It should be noted that this insight is not new in the Swedish debate; in fact, even by the late 1990s Hyltenstam was arguing that the use of a given publishing language should be seen as a manifestation of the self-perception of various disciplines.

> The extent that dissertations are written in Swedish or in supranational languages at a given point in time should primarily be seen as a reflection of how given branches of science perceive their discursive context, as chiefly national or international. (Hyltenstam 1999, 217, my translation)

2.1 Disciplines, difference, and language

The perspective proposed by Hyltenstam above demands us to pay attention to the fact that the scientific field contains smaller universes, each characterized by its own embedded economy of symbolic goods that define the chances of profit more locally (Bourdieu 1985, 196). We know these universes as academic disciplines, which, by Bourdieu’s logic, can be apprehended as fields or subfields (Bourdieu 2004, 64ff.). Here, the historically yielded specifics of each disciplinary field intersect with the general forces of the overarching university field, for instance, those concerning research policy. As one among many principles of division, these struggles unfold through language, an issue I will attend to presently.

It is commonplace that different things matter to different disciplines. Disciplines take different objects, pose different questions, and develop different research commitments and procedures in relation to these. Broadly, then, disciplines differ in what Habermas (1987) refers to as their ‘knowledge-interests’, in other words, their epistemological orientations to knowledge discovery, and, moreover, in their more or less hierarchical ways of structuring knowledge (e.g., Gibbons et al. 1994; Martin 2011). What is more, from an anthropological viewpoint, these differences can be said to engender cultures, which suggests a view of disciplines as being caught up in systems of beliefs and valorization with pivotal effects on the practices (and beliefs) of those who enter into them (e.g. Gerholm & Gerholm 1992; Knorr Cetina 1999). Accordingly, various cultural aspects of disciplines have been foregrounded by envisioning them in terms of ‘tribes and territories’ (Becher & Trowler 2001). Subscribing to a similar view, Geertz (1983) has said that

> [i]n the same way that Papuans or Amazonians inhabit the world they imagine, so do high energy physicists or historians of the Mediterranean in

> the age of Phillip II – or so, at least, an anthropologist imagines. (Geertz 1983, 155)

Over time, differences in interests render fields distinguishable as broad sociohistorical patterns of collective action, often held together by the support of (modern) institutional organization. As Clark has argued, this fact has distinguishable effects in regard to epistemologies, interests, and research agendas, for example, in respect to publishing channels for disseminating the knowledge that they yield.

> In physics, young scholars-to-be publish articles, often jointly authored; in history, they attempt to write books. The one is interested in scientific objectivity; the other in the power of individual interpretation. (Clark 1991, 110)

In a non-Anglophone setting such as Sweden, this does not only pertain to genres and publishing formats, as highlighted in Clark's account above, but moreover intersects with questions of publishing language. As many studies have shown, there are major disciplinary differences in regard to the prevalence of English in publishing. As a general tendency, English has been shown to dominate for the most part in the scientific publishing practices of fields situated within the natural sciences, medicine, and technology, while the position of Swedish holds fast in the humanities, with the social sciences lingering between these two poles (e.g., Gunnarsson & Öhman 1997; Salö 2010). As noted by Kuteeva and Airey (2014), this pattern of English impact in publishing corresponds to the idea of the 'hierarchical knowledge structure' characteristic of the hard sciences, as opposed to the horizontal knowledge structure of the humanities, characterized by the lesser use of English in publishing (see e.g., Martin 2011; also Gibbon et al. 1994). Thus, by this reasoning, these authors argue that the knowledge structure characteristic of the natural sciences supports the need for a common language of publishing (Kuteeva & Airey 2014). In addition, this state of affairs intersects with the literatures of science (e.g., Hicks 2004), where articles prevail in the hard sciences, whereas monographs are common in the humanities (e.g., Myrdal 2009; cf. also Clark 1991, quote above).

These differences can be discussed across a range of themes, such as external and internal boundaries, epistemological issues, etc. (Beacher & Trowler 2001, 210–211; Gibbons et al. 1994). This is so because the interlinkage between genres, disciplines, and languages does not explain difference *per se*, but rather manifests other forms of difference in which language variation emerges as a consequence (Salö & Josephson 2014, 284). Exploring this phenomenon in Swedish scholarship, Salö and Josephson (2014) adopt a disciplinary perspective to compare the contemporary publishing patterns of history, computer science, psychology, linguistics, law, and physics. This study points to a continuum in which the position of Swedish is strong in history and law, with both standing out as 'book disciplines' in the sense that monographs and edited volumes prevail. By contrast, English predominates in physics and computer science, where scholars predominantly publish journal articles and conference proceedings, respectively. In fields characterized by a greater theoretical and methodological unity, such as psychology, scholars are often inclined to publish articles, typically through co-authorship (Sörlin 1994, 225). Accordingly, articles and proceedings dominate in psychology and linguistics, and here, English predominates in publishing (Salö & Josephson 2014). On that point, however, it can be noted that journal articles are also fairly common in history and law, but here the balance between English and Swedish is fairly comparable, with even a slight tendency toward Swedish dominance (ibid.). Hence, distributing knowledge in articles does not necessarily entail publishing in English. Subsequently, other forms of difference must be brought to the fore.

For example, disciplines differ in respect to what Sörlin (1994) calls their 'context dependence'. The Swedish-dominated disciplines typically deal with national substance or source material, such as Swedish law or Swedish history (e.g., Salö & Josephson 2014). As de Swaan (2004) and others posit, using English is problematic in the humanities because they are more 'strongly bound to language' (p. 140). Correspondingly, fields such as history tend to be oriented toward a 'local disciplinary community' that typically operates through the medium of local languages (Petersen & Shaw 2002). Typically, scholars of such fields are preoccupied with producing detailed descriptive accounts of distinctive source material devoted to presenting facts that are relatively space-consuming; hence the tradition of reporting findings in book format (Jarrick 2012; Myrdal 2009, 41). By contrast, scholars in many of the hard sciences take objects with no particular foundation in the Swedish context specifically; rather, these objects appear the same irrespective of the geographical location of the researcher. Astrophysics and computer science serve to exemplify this point: there exist, so to speak, no Swedish physics or computer science (Salö & Josephson 2014, 284). Accordingly, such fields typically exhibit more internationally oriented publishing patterns, serving, as they do, international communities (Myrdal 2009, 39; Petersen & Shaw 2002; Sörlin 1994, 225). In the natural sciences, de Swaan (2004, 141) claims, 'most of what can be said in English can also be phrased in mathematics and in formal schemes.'

These observations are certainly helpful in attempts to unravel the logics of language choice in publishing practices. Yet, as this study seeks to highlight, it is pivotal not to lose sight of the fact that they do not point to essential qualities inherent in disciplines. From the outlook opted for here, they are, rather, realizations of historical struggles that have made things appear much more 'natural' than they really are (Bourdieu 1991, 23). Fields are manmade, according to the logic outlined in section 4. It follows from this position that the object of our study 'is not an evolution but a societal phenomenon whose historical shapes and societal driving forces it is our task to unravel' (Sörlin 1994, 18, my translation) Every contemporary disciplinary field, as we can perceive and examine in the here and now, is the outcome of a 'more or less overt struggle over the definition of the legitimate principles of division of the field' (Bourdieu 1985, 208).

3 Meta-theoretical entry points and procedure

This section introduces Bourdieu's idea of 'relational thinking'. It then explicates how this conception is translated into the study's design.

3.1 The two modes of existence of the social

To sum up, the plan here is to develop a better understanding of the sociolinguistics of academic publishing. To approach this matter from a distinct Bourdieusian viewpoint entails subscribing to a number of guiding propositions as part of the research agenda. Bourdieu's scientific project encompasses an intrinsically relational conception of social life. His sociological gaze adheres to a form of dualism between two realizations of historical action, namely 'the inscription of the social in things and in bodies' (Bourdieu 2000, 181; also Wacquant 1989a, 44). This insight provides the meta-theoretical entry point for considering the relationship between a scientific discipline, on the one hand, and a scientist within that discipline, on the other. In Bourdieu's framework, these represent 'two modes of existence of the social' (Bourdieu 1990a, 190–191). The discipline exists as reified history, engendered through the struggles of its formation. Apprehended sociologically, disciplines are fields in Bourdieu's sense (see below, section 4), although this study adopts the term 'disciplinary fields' interchangeably for the sake of clarity. As a corresponding thinking-tool, habitus is adopted to account for the fact that, through sustained engagement, the scientists who engage in a field come to embody the history of the field within which they have learned to think and act (Bourdieu and Wacquant 1992, 136). The scientist, thus, 'is a scientific field made flesh' (Bourdieu 2004, 41; see section 7). This meta-theoretical entry point dictates that understanding the practices and products of researchers 'entails understanding that they are the result of the meeting of two histories: the history of the positions they occupy and the history of their dispositions' (Bourdieu 1993b, 61).

3.2 Procedure – a relational design

Bourdieu's 'relational thinking' imports with it a particular methodological agenda, attuned to constructing relational attributes rather than 'looking for intrinsic properties of individuals or groups' (Swartz 2013, 22). This guiding principle prods the analyst to account for both modes of the social, that is, to account for the historically imbued values of each field as well as the incorporation of these histories in contemporary agents. The symbolic capital and stakes of each field, therefore, are empirically accessible in two ways. Firstly, as reified history, the fields can be studied through historical analysis (e.g., Gorski 2013). Secondly, as embodied history, the capital and stakes can be studied by prying into the habitus of their occupants, for example, via interviews. Because of this design, the procedure here is two-fold, which also splits the study into two broad sections: one that deals with the past, and one with the present (Bourdieu & Wacquant 1992, 234–235). As realized here, these two objects are field and habitus, which are both the topics and the tools of the investigation. Based on the material drawn from in this inquiry, habitus is empirically accessible primarily through the accounts of contemporary agents in the two fields (history and psychology, respectively). The point is not that fields are historical while habitus is a notion of synchronic constitution; on the contrary, habitus, like the fields it embodies, is a historical construct, which is to say that agents' dispositions to action are, at heart, historical. Social agents, Bourdieu posits,

> are the *product of history,* of the history of the whole social field and of the accumulated experience of a path within the specific sub-field. (cited in Wacquant 1989b, 10)

The past: First, the study presents a broad outline of language use as an enmeshed aspect of academic life in Sweden, which began in the heyday of Latin. Next, it presents histories of each field: initially the field of history, followed by psychology. This task is undertaken with an eye toward foregrounding the ways in which events and struggles in their historical developments feed into questions of publishing language. Here, however, intra-scientific struggles alone are insufficient for explaining the tendencies that are observed; rather, this outlook invites the analyst to pay attention to the ways in which currents within the sciences have harmonized or conflicted with broader societal ideologies and historical developments (Broady 1991, 525). Consequently, this first procedural part of the study draws on a wide range of sources, where 'thick constructions'[1] of the fields' historical developments are juxtaposed with facts and figures on publishing practices, as elicited from bibliographies that cover different periods. As Gouldner notes, 'publication as such is not of course theoretically interesting, but it becomes so if taken as an index of something else' (1957, 289). In this study, the language used in academic publishing is seen as an index of different forms of national and transnational values at stake within a field at a given point in time.[2]

The present: Second, the study turns to contemporary academia and its dwellers. The study first provides an overview of current research policy in Sweden and beyond. Next, it sets out to unravel the ways in which historically contingent values are rooted in people's habitus – their dispositions, representations, and experiences of the social world (Broady 1991, 193). Here, the study presents accounts obtained from interviews with contemporary agents of the two fields, both dominant agents and newcomers, with the primary motive being to account for the ways in which the aspirations held by historians and psychologists, respectively, link to publishing practices.[3] What is taken into consideration here is their academic habitus in relation to publishing strategies. These data were obtained from four hour-long interviews, which were recorded and subsequently transcribed.[4] For each field, accounts of the interviews with the professors are presented first. These are agents who are consecrated by their fields, and who embody their fields – they are 'people who are known' and, therefore, 'may speak on behalf of the whole group, represent the whole group and exercise authority in the name of the whole group' (Bourdieu 1986, 251–252). They were selected to be informants by virtue of their positions as dominant agents, that is, major holders of symbolic and material assets. Next, the study turns to the accounts of the doctors, both of whom are up-and-coming scholars in their respective fields. Both defended their theses just after the turn of the 21st century. While they have begun to advance in their respective fields, they are both still treated as newcomers, first and foremost by virtue of their capital assets, which they strive to increase in order to advance in position within the spaces of possibilities constitutive of their fields.[5] In opposition to the professors, the doctors are new entrants who have recently begun to internalize the logic of practice of their fields, and who are products of a ‘new mode of recruitment’ (Bourdieu 1988, 152) constitutive of contemporary university life. So far in their careers, they have gone through an ample, albeit differentiated, share of evaluation to eventually arrive at their current positions. It follows from this that they have a sense of what counts symbolically in their respective fields and that this practical knowledge can be drawn upon to navigate through their spaces of available possibilities as they appear in contemporary fields (Bourdieu 1983, 313).

It is appropriate to make two notes on methodology here. As for the section accounting for the past, the study uses Bourdieu’s notion of field first and foremost as a way of asking questions relevant to the vast amount of material that is used. ‘Field’ thus serves as a conceptual lynchpin, that is, as a way of making inroads into the histories of the two fields with the aim of foregrounding particular features of the historical processes of their growth: the spaces of position, the values at stake, etc., which stand out as relevant to the question of language (Hanks 2005, 73; see also note 6). As for the second part, where interviews are used, it is necessary to underscore that the analysis is deliberately geared to discovering accounts of the selected social worlds through the sense-making filter of the habitus of the interviewees. The position-takings of interviewees on their own practices are by all means legitimate, but they are ‘points of view’ in the most factual meaning of this expression. In these interviews I am interested in the positions from which interviewees express their views (Bourdieu 1996, 34). What this stance brings about is this: in respect to the inquiries of this study, the interviewees ‘do not have in their heads the scientific truth of their practice’ (Bourdieu 2003, 288), and therefore, the task of objectivizing their accounts inexorably falls upon the much-privileged analyst:

> Interviews with social agents, therefore, offer insightful discourse on a particular point of view but it is a ‘point of view’ and the social scientist must uncover the forces, which are driving the discourse (the struggles and strategies within the field(s) within which the agents are positioned). The scientist must provide a point of view on a point of view. (Lucas 2006, 69–70)

4 Bourdieu’s science of science

The principles of Bourdieu’s sociology of science are outlined in several publications (e.g., 1975, 1991, 2004), but are, empirically, most thoroughly elaborated in his book *Homo Academicus* (1988). Contained within the larger field of power, science is the constitution par excellence of a field of cultural production. As noted, Bourdieu sees these contexts as comprising peculiar universes, imbued with their own characteristic logics, values, stakes, and driving forces. These can be envisioned as social worlds embedded within each other; disciplinary fields, for example, are nested within the larger scientific field as a hierarchized space of disciplines (Bourdieu 2004, 66; see also Grenfell 2007, cp. 10; Hilgers & Mangez 2015; Maton 2005). But, as understood here, the overarching *university field* is occupied by agents other than those who teach and produce scientific knowledge; to be included here are funding agencies, research councils, agents from the publishing industry, and bureaucrats with stakes in research politics whose interests affect research practice.[6] Often, and increasingly so, these are policymakers operating from within the bureaucratic field, legitimized by the power-field of the state (e.g., Bourdieu 2014). In what follows, these premises will be expanded upon, focusing particularly on the logic of academic life. Questions of research policy driving contemporary transformations of the university field are dealt with in section 6.

4.1 Fields as sites for the struggle over resources

The scientific field, Bourdieu holds, 'is the locus of a competitive struggle, in which the specific issue at stake is the monopoly of scientific authority' (1975, 19). Here, socialized agents – viz. the species of homo academicus (Bourdieu 1988) – struggle for individual distinction. Hence, 'to exist is to differ, i.e. to occupy a distinct, distinctive position, they must assert their difference, get it known and recognized, get themselves known and recognized' (Bourdieu 1983, 338). For this 'social physics' to work, there need to be, on the one hand, stakes, profits, and desirable awards, and, on the other, people who are willing to engage in the games where such stakes, profits, and awards can be earned (Bourdieu 1993a, 72). As Bourdieu posits, 'there would be no game without belief in the game and without the wills, intentions and aspirations which actuate the agents' (Bourdieu 1981, 308). At stake in academia are various forms of academic and scientific capital, which serve as powers or currencies in the game of academic success. For example, there is capital of academic power, scientific power, as well as that of scientific prestige and intellectual renown (e.g., Bourdieu 1988, 40). These forms of capital take the shape of academic titles and positions, publishing records, committee memberships, and reputation. In typical scholarly CVs, such capital accumulations are listed as merits, commonly aligned with the demands of a given field. In academia, success breeds success. Forms of capital merge into economies of symbolic value, where accumulated success serves as a currency by means of which more success can be traded. Often, these forms of resource exchange are channeled through agents' accumulated assets of social capital, such as networks and personal contacts. For example, being published in high-prestige journals may improve the newcomer's chances of pulling rank in expert assessments for research funding and academic positions; having received research funding once is likely to improve one's chances to receive funding again, and so forth. Likewise, the academic capital of being head of the department is commonly reached as a result of having gained an ample measure of scientific prestige, which in turn can be exchanged for a higher salary. The homo academicus in this vein capitalizes on prior achievements to yield more profits. This is why, as Reay (2004, 32) aptly notes, 'Academia is full of cultural capitalists' who struggle for their specific interests (e.g., Bourdieu 1993b, 181).

As relatively autonomous social universes, what is highly valued in one disciplinary field may be less profitable in another, which is to say that *symbolic capital* differs across disciplinary fields (e.g., Broady 1991, 17). Symbolic capital is what is commonly referred to as prestige, renown, or reputation, 'which is the form in which the different forms of capital are perceived and recognized as legitimate' (Bourdieu 1985, 197). This fact explains why most investments made in a particular subfield (e.g., the field of history), are perceived as odd or 'disinterested' by agents outside of the field, (e.g., in psychology), where investments are placed according to other historically given principles (Bourdieu 1990a, 48). Hence, we can say that the scientific field

> defines itself by (among other things) defining specific stakes and interests, which are irreducible to the stakes and interests specific to other fields (you can't make a philosopher compete for the prizes that interest a geographer) and which are not perceived by someone who has not been shaped to enter that field. (Bourdieu 1993a, 72)

4.1.1 Interests and strategies

As part of the struggles outlined above, the two concepts *interest* and *strategy* stand out as particularly crucial to grasp, above all since they are envisioned in a particular sense within Bourdieu's framework that does not correspond to their usage in common language. The so-called 'logic of practice' of those who engage in scientific fields dictates that the driving forces are essentially fueled by a profound *interest* on the part of the involved agents in the subjects of their expertise. The agents of academia are in this sense driven by a form of curiosity in respect to their object of inquiry; thus, 'curiosity', to some degree, expounds agents' interests for engaging in research. The term 'interest' (also *illusio*), however, carries a two-fold meaning within Bourdieu's framework. In addition to this kind of altruistic passion, 'I am interested in history or psychology, etc.', it also taps into other forms of interests linked to the fact that science constitutes a system founded on meritocracy, competition, and hierarchism, where, to advance or even maintain one's position, people are compelled to make various sorts of *investments* that yield profit in the race. Researchers, thus, seek to maximize the value of their deeds within the field; yet, not just by adapting to the field, but by imposing authoritatively their own definition of what is and should be valued in relation to the research objects at stake (Lucas 2006, 60–66; Putnam 2009, 129). For those striving to be acceptable in terms of 'academic honor', researchers are at pains not to come across as being blatantly mercenary, since strengthening one's own position should serve to also strengthen the field. In this sense, research is viewed as a form of team sport in which individual players hold their own ranking, and here career interests can be acceptable as long as they are coupled with research interests. It follows from this same logic, moreover, that any voicing of self-interest is swept under the carpet or rephrased in terms of honor (Broady 1991, 206–207). Interest, Bourdieu holds,

> is 'to be there', to participate, to admit that the game is worth playing and that the stakes created in and through the fact of playing are worth pursuing; it is to recognize the game and to recognize its stakes. (Bourdieu 1998, 77)

In participating, agents adopt different *investment strategies*. Yet, by invoking the notion of 'strategy', Bourdieu does not subscribe to the idea of the rational agent who acts solely in accordance with the maximization of profits through conscious calculations (2000, 28). Neither, however, are agents seen as acting in fully unconscious or uncalculated ways, since human action can very well encompass 'a strategic calculation of cost of benefit' (Wacquant 1989a, 45). Strategies are envisioned as more or less conscious moves, generated by the agents' practical sense, which serve to reproduce his or her position and to generate profit in the social game (Mérand & Forget 2013, 97). To Bourdieu (1998, 81), while such moves can be conscious, for the most part, agents' strategies do not have 'a true strategic intention as a principle.' In this vein, this distinct notion of 'strategy' seeks to circumvent the opposition between the totally conscious and the totally unconscious (Kauppi 2000, 231; Lucas 2006, 63ff.; Mérand & Forget 2013).

Over time, above all, the conscious becomes unconscious 'as the same or similar situations are repeatedly encountered' (Hillier & Rooksby 2005, 22).

4.1.2 The newcomers and the dominant players

A key property of every field is that there are struggles between newcomers – that is, pretenders or *nouveaux entrants* – who aim to enter the field, and the dominant agents, who try to hold rank, defend monopoly, and, by doing so, withstand competition (Bourdieu 1993a). It is axiomatic that agents of the scientific field differ in respect to capital possession, both in respect to the volume and the composition of the capital endowed (Bourdieu & Wacquant 1992, 108ff.). Hence, in academic life in particular, accumulating capital takes time, and agents who have placed their investments successfully over a long course of time commonly hold dominant social positions. Correspondingly, the dominant players of academia tend to be more senior and established agents with permanent academic positions in their fields. By the same logic, upon entering this game, young scholars typically arrive empty-handed with respect to symbolic assets. As neophytes at the scientific game, newcomers, therefore, are obligated to conform to existing hierarchies put into place throughout the history of the field, but they can also work in pursuit of changing the symbolic order of the field, for example, by reorienting their investment strategies. As Hilgers and Mangez (2015, 11) put it, 'newcomers will tend to implement strategies aimed at subverting the symbolic order; otherwise they will tend to undergo a form of symbolic violence that leads them to recognize the legitimacy of a symbolic order that is unfavourable to them.' To defend the order, those who possess the dominant positions are subsequently at pains to produce weapons by virtue of which they can hinder the displacement of established principles of hierarchization, for example, by discrediting newly developed investment strategies (Bourdieu 1983, 322). In this vein, '[t]he history of the field arises from the struggle between the established figures and the young challengers' (ibid., 339). As Williams puts it, the struggle

> involves those whose interests are served by the status quo in the distribution of capital, and those who will benefit from change. Despite being locked in struggle, all participants will benefit from the preservation of the field, and thereby will have an interest in its reproduction. (Williams 2010, 85)

Questions pertaining to the logic outlined above will be brought to light in section 7. In what follows, the study digs deep into the historical sociolinguistics of scientific life in Sweden.

5 The sociolinguistics of science: the longue durée

In Sweden, university life began to consolidate in the 17th century, thereby breaking with the church-linked scholastic knowledge production that had prevailed before (e.g., Frängsmyr 1989a). From a European perspective, this was a late start. As a nation of knowledge production, as well as in other ways, Sweden lagged behind its culturally more developed European counterparts, where humanism had long flourished at the universities of Bologna, Paris, and elsewhere (e.g., Eriksson & Frängsmyr 1991). While Uppsala University was officially established in 1477, it was largely inoperative throughout the 16th century. Thus, during most of the 16th century, higher education had to be undertaken abroad, primarily in Germany (Tengström 1970, 40). However, during the second half of the 16th century, small-scale academies and seats of learning were set up in the mid-Sweden region. In 1593, a decision was made to re-establish the embryonic work at Uppsala University, where three chair professors in theology and four in philosophy were installed (Tengström 1970, 41). This event, arguably, marked the early rise of Sweden as a nation of science, and during the 17th century, this initiative was followed by the establishment of academies in other parts of the Swedish empire: Dorpat in 1632 (Tartu, contemporary Estonia), Åbo in 1640 (Turku contemporary Finland), and Lund in 1668 (Lindroth 1975, 47ff.).

Still, as a society isolated in the far north, Sweden was comparatively small and infinitesimal, thereby creating the need to use a language that, unlike Swedish, held currency in transnational communication (Lindberg 1984, 79). In this largely expanding university environment, Latin was adopted as *lingua eruditorum vernacula*, 'the vernacular of the learned' (Lindberg 1984), at the beginning of the 17th century, thereby marking the beginning of an important epoch of Sweden's linguistic history. Thus, in spite of the outspoken aim of Swedishizing the new, conquered provinces, and in spite of the exhortation of the Swedish king Gustav Adolf II to 'turn all the sciences onto our mother tongue' (quoted in Lindberg 1984, 34), Latin soon held the position of being the official intermediary language of scientific knowledge and dissemination (Tengström 1970, 62). Bibliographies covering this time period (Lidén 1779) show that Latin was the language used in all written dissertations between 1611 and 1718 (see also Östlund & Örneholm 2000; Tengström 1970, 65). Likewise, Latin was adopted for use in lectures, dissertation defenses, orations, and in other oral academic performances during that time (Lindberg 1984, 27; Tengström 1970, 41–42).[7]

In the 18th century, scholars such as Linné, Celsius, Bergman, Wallerius, and others brought fame to Sweden. Now, the number of publications skyrocketed, and coinciding in time as it did with utilitarian thinking, this period was important in the construction of scientific discourse (Gunnarsson 1997, 2011). The scholarly celebrities of this time, however, aimed their scientific work for an international audience, and, accordingly, Latin was used (Lindroth 1978, 584). In 1748, a national commission suggested that Swedish should be allowed as a language for dissertations in physics, mathematics, and history (Lindberg 1984, 40; Tengström 1970, 74). This proposal was rejected, but it launched a language ideological debate that would last throughout the 18th century, thus emerging in tandem with the rise of the nation-state (e.g., Lindberg 1984; Nilsson 1974). It is beyond the scope of the present work to account for this debate in detail, but, in summary, the following lines of argumentation were invoked.

The promoters of Latin argued on behalf of the many benefits of adapting the global language of science. Latin was supported by forces representing the old society: church, the nobility, the learned (Lindberg 1984, 45). These groups perceived Latin as a 'neutral' language insofar as it was nobody's mother tongue, thereby avoiding the charge of inequality among researchers. Using Swedish and other national languages, in their view, was seen as contributing to the national isolation of knowledge. The national languages, it was argued, all had their shortcomings: French was too aristocratic, English was only good for commercial matters, and German was mastered sufficiently by too few (ibid.). Latin, moreover, exhibited a sort of censorship to the market of scientific goods, as it kept 'the indocti', that is, the non-Latin literate, from engaging in this high-order knowledge exchange (ibid., 81). For example, the well-known Swedish botanist Linné, who himself never actively engaged in the debate, argued that while the use of Latin allowed his work to reach a wider scholarly audience, it also helped him to avoid comments and opinions from less cultured segments of society (ibid., 95–96).

The opponents of Latin, for their part, raised concerns related to having the elite's language as a medium of instruction and scientific publication (e.g., Lindroth 1975, 572ff.). The criticism of Latin targeted all of those aspects that the language was perceived as manifesting: the non-utilistic, the pedantic, the ostentatious, the exclusive, the non-Swedish (Lindberg 1984, 34). Swedish, on the other hand, was seen as more beneficial for the nation at large. Practical arguments concerning language skills were also invoked: the practical mastery of Latin was said to occupy too much valuable studying time, and, by the same token, knowledge in Swedish was seen as more usable in society at large (Tengström 1970, 77–78).

In the days of the Swedish empire (1611–1718), professors had lectured in Latin in all subjects and faculties (ibid., 81). However, beginning in the early 18th century, the question of teaching in Swedish was raised. While it was not yet an established policy, various forms of translingual lectures came into practice in Lund and Uppsala, where Latin and Swedish were used interchangeably in subjects such as philosophy and mathematics. There were also instances of French being employed in teaching, since many of the noble students did not understand Latin sufficiently (ibid., 82). However, for other, culturally customary functions and academic events, such as oral dissertations (defenses, see note 7), orations, commencements, and so forth, Latin remained unchallenged at Swedish universities. The same is true for scientific written production, where the dominance of Latin proved to be difficult to challenge throughout the 18th century. This was so despite the purposeful promotion of Swedish carried out by The Royal Swedish Academy of Sciences, founded in 1739, which, with its journal proceedings called 'handlingar', ventured at 'bringing science to the people' (Frängsmyr 1989a, 4; see also Frängsmyr 1989b). Although the work of the Academy contributed to greater text production in Swedish, this had a more significant impact for the growth of popular science than it had for the production of original research in Swedish (e.g., Fries 1996; Lindberg 1984, 31ff.). Nonetheless, the Academy did demonstrate that scientific matters could be dealt with in registers of Swedish (Teleman 2011, 84; cf. Sörlin 1996, 36).

Up until the mid-19th century, Latin was the dominant language of scientific publishing in Sweden, albeit with certain disciplinary restraints. Economics, for instance, was granted an exemption from the requirement of submitting dissertations in Latin in the year 1741, as it was considered to be a semi-academic subject whose scientific results were mostly for the benefit of the people (Lindberg 1984). Likewise, occasional exceptions to publish in Swedish were granted in jurisprudence (Tengström 1970, 83). In 1852, the regulating frameworks for dissertations changed. From then on, the respondent was required to have been the author of the presented work, which was a new demand (ibid., 91; see also note 7 here). At the same time, policy restrictions concerning dissertation languages were dropped. This is also the point in time in which the universities of Swedish academia slowly began to transform into the modern organizational shape still used today, with faculties that could house emerging disciplinary fields, etc. Institutionally, Swedish university formation gathered its ideals from the German tradition, which led to the order that each discipline was represented by a chair professor (e.g., Odén 1991) – agents who held ‘a volume of capital sufficient to enable them to wield power over the capital held by the other agents’ (Bourdieu 1991, 13 emphasis removed). As a consequence, the shaping of specializations in each field was largely dependent on the specific visions and research interests of the appointed professor. As noted by Suppe:

> Thus the German scientific establishment tended to break into various schools surrounding a few main figures holding professorships or directorships of institutes; and the members of these schools tended to share the specific interests and approaches of the professor or director. (Suppe 1974, 7, cited in Odén 1991, 171)

5.1 The history of history

In the field of history, it is useful to make a distinction between the writing up of history, which dates back to the ancients, and the much more recent concept of history as a systematic research practice, which is a product of the 19th century (e.g., Lindberg 2012). Between these two phases, we find a process of overt professionalization and the rise of history as a disciplinary field in its own right. Throughout both periods, the main knowledge interest was the same: Sweden and Swedish history. By the same token, in the field of history Swedish has predominated in publishing over the past 125 years.

The writing of history was important during the time of the Swedish empire, as the history of the nation was identified with its ruling kings, and therefore had to be written and preserved. For example, the creation of Riksantikvarieämbetet [The Swedish National Heritage Board] in the 17th century made Swedish ‘old-history’ into an object of patriotic study (Sörlin 1994, 54). This sort of history writing, thus, was what Nietzsche referred to as ‘monumental’, that is, backward-looking, idolatrous, and instigative – but with no articulated theory (Lindberg 2012, 19). Aiming as it did to glorify the olden days of Sweden, this sort of history writing was often addressed to a foreign readership, and accordingly, most of the early national chronicles of Nordic and Scandinavian history were written in Latin. However, well into the 18th century, after the fall of the empire, the emphasis on the grandiosity of the homeland was replaced by a form of patriotic utilitarianism (Lindberg 2012, 10–12). This moreover led to a re-orientation toward a domestic readership, a practice pioneered by Olof Rudbeck, who wrote Atlantica (1679–1702) in Swedish (S. Nordin 2008, 42). Lectures in history were delivered in Swedish in the middle of the 18th century (Lindroth 1978, 582; Tengström 1970, 81–82). Publication practices, however, were still enmeshed in a Latinist culture. In the subject of history at Uppsala University, dissertations were written in Latin almost entirely until the year of 1842, after which both Swedish and Latin were used (Markelin 1856; Tengström 1970, 91). The trend, however, worked steadily in favor of Swedish, where, increasingly, Latin was often used so long as the research dealt with source material, while the actual writing up of history was done in Swedish (Lindberg 1984, 110).

Kjørup (1996, 30) draws a dividing line between the pre-histories and the histories of the humanities around the year 1800. The 19th-century implementation of German university ideals and subsequent organization worked in favor of the historical field, not least of all due to currents of romanticism and nationalism (Odén 1991, 50). Throughout the 19th century, the field experienced an overt process of professionalization (Björk 2012). Archives were made available to historical research, and the field began to develop a new orientation toward epistemological beliefs in research results: that new knowledge could be produced by adopting particular methods, theories, and so forth (e.g., Torstendahl 1964, 1966). Attempts were made to situate history onto what was perceived to be 'the real' terrain of science, not least of all by gearing toward the positivism of Comte's sociology as an inroad to establishing laws of human behavior (Torstendahl 1966, 34–35).

In Sweden, rapid industrialization and movement toward modernization at the end of the 19th century boosted national science (Crawford 1992, 36). Disciplinary fields were increasingly established nationally, and journals were founded that would long come to dominate and define their fields: *Samlaren* in literature studies in 1880, *Ymer* in anthropology and geography in 1881, *Arkiv för nordisk filologi* in Scandinavian linguistics in 1882, etc. Concomitantly, the field of history grew rapidly at Swedish universities. Here, the journal *Historisk tidskrift* was founded in 1880, with the aim of distributing scientific work that was comprehensible also for the educated public (Odén 1991, 59). Like the above-mentioned journals, *Historisk tidskrift* consequently published work in Swedish. This period, moreover, coincided with the book becoming established as the key genre for scientific dissemination (Myrdal 2009, 93, and note 80 there).

In the humanities, in particular, national romantic currents were particularly strong from the 1890s on, which led to the blossoming of interest in Swedish culture and its distinctive characteristics (e.g., Johannisson 1989, 100ff.; Sörlin 1996). In history, programs for doctoral studies were launched in 1890, which led to an increased production of theses in Uppsala and Lund, and later in Gothenburg and Stockholm. At the history departments at all of these universities, Swedish was the main language used in writing theses (Pikwer 1980). Publishing practices were now being rapidly nationalized. Josephson's (1897) bibliography of theses covering the second half of the 19th century shows an almost complete dominance by Swedish and only occasional instances of Latin within the field. Out of 339 theses listed for 'General history' and 'The history of Sweden' during this time, 334 were written in Swedish, 2 in Latin, and 3 in undetermined languages.

Studying academic expert reviews, Larsson (2010) investigates the value of scientific ideals as attached to certain ideal types of historians throughout the first half of the 20th century. According to Larsson, the empirically driven scholars, those who invested their time in the archives, enjoyed high currency at the beginning of the century, while scholars more attuned to theorizing were privileged in the middle of the century. According to many historiographies, the 20th century was dominated by a struggle between two schools of thought within the historical field: The Hjärne school, based in Uppsala, and the Weibull school, based in Lund (see Björk 2012 and Torstendahl & Odén 2012, respectively). Hjärne was an influential field-founder who created a research environment that came to yield a substantial number of followers (Odén 1991, 153). Although he was a conservative, caught up in the romantic spirit of his time (e.g., Johannisson 1989, 101), Hjärne nevertheless believed in scientific universalism and urged historians to account for the phenomena of study against the backdrop of the world beyond Sweden (Hasselberg 2007, 100–105). Moreover, he sought to create a rupture in the field's national isolation by establishing links to other countries (Odén 1991, 154; Torstendahl 1964, 286). Nonetheless, in the 1930s Uppsala grew to become a stronghold for historical research with a strong nationalistic footing (Gunnerisson 2002). In this sense, the professionalization of the research of the field can be seen as being linked to the strategic positioning of providing the nation-state with a glorious past (Jarrick 2013, 56). This position, however, was challenged in the years to come by the more radical school in Lund, led by Lauritz Weibull, which sought to break with the hitherto prevailing ideological project by adopting a fact-oriented approach that would 'transform politics into science' (ibid.). The journal *Scandia* was founded at the school in 1928 to be a place where a challenging line of historical research could be cultivated and distributed. In line with this strategy, Weibull and followers could take advantage of the fact that, after World War II, nationalistic overtones had lost a great deal of currency in the field of history and elsewhere (Gunnerisson 2002). In the 1950s, the internal struggle between competing historical views began to lose its force in the historical field, as students of Weibull held professorships at every Swedish university – a new school of thought now dominated the field (ibid., 212, 230f.).

During the latter half of the 20th century, history found itself squeezed between two contradictory discourses: on the one hand, that of an increased emphasis on internationalization; on the other, one that remained true to the nation's history writing, thereby linked to a form of public responsibility to produce knowledge usable to wider audiences. In his report entitled *Universitetsreform* [University Reform], Myrdal (1945) lamented on the state of affairs that he saw as particularly salient in some of the social sciences and the humanities, namely that many scholars acting there were remarkably invisible internationally. Myrdal linked this observation to matters of publishing language in the following way:

> The language boundary appears to serve as a customs protection to a scientific production mainly directed to the domestic market. Rarely does it receive the tempering of the international competition. It is striking particularly within some sciences of the humanities that our national researchers do not become international authorities. (Myrdal 1945, 9, my translation)

As will be evident in the case of psychology, many disciplinary fields in Swedish academia became geared toward transnational publishing markets around this time. The field of history, however, did not shift in this direction. At any rate, the prevalent struggles of the field did not have much impact on languages of publishing. They unfolded in the field when international engagements by and large were toned down, a trait that was particularly salient in the period from 1914 to 1950 (Brissman 2010, 405). In fact, the strong empiricism in the Weibullian tradition had further contributed to nationalizing the field (Odén 1991, 166). Likewise, questions of method were privileged over questions of theory, as the latter was seen as mirroring the values of the conservative (Hasselberg 2007, 31). Accordingly, the position of Swedish as a thesis language remained unchallenged. Pikwer's (1980) bibliography covering the years 1890–1975 reveals an almost complete dominance by Swedish, with a mere six percent being written in other languages. Among the theses written in languages other than Swedish, German was the only language manifesting up until the 1940s, after which English began to acquire shares in the publishing market (Blom 1980, 9).

Throughout the post-war era, the field of history was undergoing a more profound shift in respect to what Liedman (1983) calls its usage of knowledge: away from humanistic knowledge as ideologically usable and toward an increasingly state-linked view of administrative usability, for example, concerning reform politics (Odén 1991, 328; Sörlin 2013, 23). This shift also introduced a discourse in the field on the importance of asserting itself as a mature scientific field in its own right. Torstendahl's book *Historia som vetenskap* [History as a Science] (1966) can be seen as manifesting a development whereby questions of method, primarily quantitative approaches, appeared on the field's agenda. From this point, a discussion was launched about the relationship between history and the social sciences in Sweden, which also led to an increased adoption of theory, first imported from North American structural functionalism and, later, Marxist theorizing (Åmark 2012, 145–146). Commonly, such initiatives to change the field came from doctoral candidates (Odén 1991, 174). In the 1970s, the field also saw a slight increased interest in engaging with global history, a trend reflected in the increasing number of theses posing research questions beyond the national paradigm (e.g., Eklöf Amirell 2006a, 262). Around this time, around 10 percent of the theses were written in English (Blom 1980, 9). To some extent, the establishment of *Scandinavian Journal of History* in 1976 can be seen in this light, that is, as a way of expanding the frontiers of Swedish history research through a forum publishing in English (e.g., Mörner 1985, 446).

To many historians – not least those that Larsson (2010) refers to as 'the martyrs of the archives' – these developments were seen as damaging, since they jointly shifted emphasis away from the empirical legacy of the field (Åmark 2012, 148). The fluxes in the field, additionally, were seen as screening the field's knowledge production from the educated public (ibid., 170). Related to this, moreover, Swedish history research was evaluated by a Scandinavian expert group in 1988 (Danielsen et al. 1988). Here, the field was criticized for being overtly intra-scientific and for not reporting their findings sufficiently to the general public. The committee pointed out that Swedish historical research results were often presented in a style appraised as 'lærd', 'u-folkelig', and 'kjedelig' (p. 124) – that is, scholarly lettered, anti-grass root, and boring. Five years later, Åmark (1993) reported that the field at large had already responded to this line of criticism. Pioneered by the work of Englund (e.g., 1988), arguably, many historians soon began to develop a preference for writing up history for a wider audience, thereby acquiring capital outside the field. Thus, while fields often struggle to defend their autonomy (e.g., Maton 2005), this does not merely entail isolating the field from external interest, but also of reclaiming the turf of historical knowledge. In the eyes of many historians, the field was perceived as having been left too open to groups with other knowledge interests (Åmark 1993, 273; 2012). As a field of knowledge production, history had – and still has – competition among agents from a range of other fields with an interest in producing accounts about the past, for example, journalists, popular writers, teachers, politicians (e.g., Stråth 2013). Names in the Swedish context include authors such as Herman Lindqvist and Jan Guillou from the 1990s and onwards, and, decades later, Henrik Arnstad. These are agents who have not paid their entry fee to the historical field and are, accordingly, labeled 'entertainment historians' by the field's agents (Sörlin 2013, 22). Consequently, agents of the historical field commonly criticize these authors for not adhering to the principles of 'proper' history research, that is, for not being real historians, yet producing historical accounts (see e.g., Meinander 2006).

Accordingly, pared with an increased public interest, Swedish history research saw a tendency among Swedish historians to publish more easily accessible work aimed for a broader Swedish-speaking audience (e.g., Eklöf Amirell 2006a, 274–275). This fact amplified a number of already existing traits of historical research. Firstly, it contributed to further rendering the borderline between strictly scientific work and more popular accounts much less distinct in history, compared to other disciplines (e.g., Danielsen et al. 1988, 99). Secondly, it worked in favor of a pre-existing reluctance among Swedish historians to publish in languages other than Swedish; in the period 1990–1996, the share of theses in English remained around 10 percent (Aronsson 1997, 35), rising to 12.5 percent in the period 1997–2001 (Aronsson 2003, 184). Some historians have criticized this state of affairs by framing it as a question of a general disinterest in global perspectives among Swedish historians (e.g., Eklöf Amirell 2006a, 2006b; Mörner 1985). In an overview of the field's international foci, Mörner (1985) pointed out that existing bibliographies of Swedish dissertations in history (e.g., Pikwer 1980; Blom 1980) reveal a heavy bias toward an interest in general Swedish history and Swedish local history. Particularly worrisome, argued Mörner, was that Swedish history research was becoming more nationally oriented throughout the 20th century, with the exception of the 1970s, during which internationally oriented theses peaked.[8] This state of affairs notwithstanding, Mörner argued, Swedish historical research cannot be said to be more nationally oriented than the history writing of other European countries (Mörner 1985, 441); history, thus, appears to adhere to the national paradigm also in a global perspective.

In a follow-up study of this development into the new millennium, Eklöf Amirell (2006a) shows that internationally oriented history research has continued to decrease, constituting a mere share of 16.8 percent in the first 5 years of the 21st century, which amounts to about the same levels in place in the 1940s and ’50s. Eklöf Amirell also claims that out of the country’s 48 active professors, only 5 have pursued a principally international research agenda (ibid., 266). As for the languages used in the theses, Eklöf Amirell (2007) presented a bibliography listing 606 doctoral theses defended in Sweden between 1976 and 2005. Here, it is notable that more than 80 percent of the theses were written in Swedish at all eight universities included in the study; in Stockholm, Växjö, and Örebro, the share was more than 90 percent. He shows that 13.2 of the theses defended in the whole period were in English and that a mere 2.5 percent were written in other foreign languages; hence, the vast majority was written in Swedish. Among the theses dealing with the Swedish context, only 3.6 percent were written in English. Eklöf Amirell interprets these findings as an indication that Swedish newcomers to the field, to a small extent, seek to contribute to the international scientific discussion by means of the knowledge they produce.

Doctoral theses, however, only amount to a small share of the scientific texts produced in historical research. As for other genres – thus written by researchers at all levels – publications in English are more common since 2000. Salö and Josephson (2014, 278), for instance, show a slight inclination toward the use of English in journal articles and a more significant dominance of English in conference proceedings. Congruently, Amirell (2013) shows that 58 percent of the peer-reviewed articles published in the field between 2009 and 2012 were in English. Nevertheless, Swedish dominates in the genres in which there is the greatest number of publications, viz. books and book chapters (Amirell 2013, 501; Salö & Josephson 2014, 278).

5.2 The history of psychology

Compared to history, psychology has a much shorter history in Sweden, in particular, as an autonomous disciplinary field. Nilsson (1981) dates the establishment of psychology as a field in its own right in Sweden to the post-war era, when the first chair professor was installed at Uppsala University in 1948. Compared to other European nations, including neighboring countries such as Denmark, this was a late start (Madsen 1970, 123). Also, from an international perspective, psychology constitutes an illustrative example of a field that, throughout its history of growth, has shifted in different disciplinary directions. In bygone days, questions of a psychological nature were generally dealt with within the broad realms of philosophy and, to a lesser degree, pedagogics, medicine, and physiology (Sörlin 2004, 208). Thus, while an interest in psychological reasoning can be traced back to classical antiquity, it was not until the late 19th century that psychology began to emerge as a field in its own right (Kjørup 1996, 28). At this stage, Germany stood out as the first intellectual locus out of which psychological thought and research practice was exported – not least of all to the U.S., where an experimental orientation swiftly gained popularity (Sörlin 2004, 209). Madsen (1970, 45) provides an overview of the rapid creation of psychological laboratories between the years 1875 and 1897, when a large number of labs were built across Europe and North America – after 1886, at the rate of at least two labs per year.

In Sweden, the development of psychology as an autonomous field was hindered by its ties to broader disciplinary embedding, where in particular the legacy from Boströmian idealist philosophy stalled the development of an experimental natural scientist approach (Agrell 1952; Hyden 1984; Nilsson 1978, 1981). Psychological research was carried out within the disciplinary boundaries of other fields, such as philosophy and physiology (Madsen 1970, 53; Nilsson 2008, 165; Lundh 1979; Öhman & Öhngren 1991, 21). In the first half of the 20th century, psychology developed as a branch of the broad field of pedagogy, under which influences from German phenomenological experimental psychology was subordinated (Nilsson 2008, 165).

In the premature rise of psychology in Sweden, Swedish was the main language in the few doctoral theses written. During the period 1855–1890, Josephson (1897, 284) lists 13 theses, of which 10 were written in Swedish and 1 each in German, French, and English. This tendency remained intact over the following five decades. For the category ‘Psychology and pedagogics’, Nelson (1911) lists eight theses between 1890 and 1909, all of which were written in Swedish. For the period 1910–1940, Tuneld (1945) lists 17 theses, of which all were written in Swedish, again in the category ‘Psychology and pedagogics’. Likewise, based in Uppsala, the journal *Psyke* (1906–1920) published work authored mainly in Swedish, albeit with occasional contributions in other Scandinavian languages, as well as in German. In sum, then, we can say that Swedish dominated in the publications, including the theses, written in the field of psychology so long as psychology remained a subject enclosed within the boundaries of other fields (see Brissman 2010, 399–400, note 616).

However, a number of events in the surrounding world would soon come to have an impact on Swedish psychology – in publishing practices and otherwise. World War I had severely damaged the form of scientific internationalism that had previously burgeoned and instead made researchers around the world submit to the service of their nations (Brissman 2010, 70;

Landström 1996). Around this time, the U.S. was putting significant economic and infrastructural effort into research, not least of all through the organization of research councils (Brissman 2010, 96ff.). The onset of the decline of German as a scientific language internationally is thus to be located after World War I (Ammon 2012; Gordin 2015, 7). But this development was soon furthered. The outcome of World War II was disastrous for the position of Germany as an intellectual node, which was reflected in Swedish scholarship in psychology and beyond. Throughout the launch of Hitler's regime, the U.S. enjoyed a significant immigration of European psychologists (Madsen 1970, 120). At the same time, the U.S. invested heavily into research (e.g., Sörlin 1994), and here a renewed interest in perceptional psychology gained currency (Madsen 1970, 112). Thus, at this point in time, the U.S. replaced Germany as the key geopolitical point of intellectual orientation. This can be seen, for example, in the purchases of scholarly psychological literature made by Swedish libraries. Sörlin (1994, 108–109) notes that in psychology, new German books amounted to 42 percent of the total number of foreign books purchased in 1930: 29 percent in 1938, and a mere 1.2 percent in 1946. The share of American scholarly titles by now amounted to 53.5 percent.

In 1937, the Swedish field of psychology was joined by the eminent experimental psychologist David Katz, who had come to Stockholm due to turbulence in central Europe. Katz was already a big name in German *Gestalt* psychology, and, on his way to Sweden, he had stayed for four years in England, where he established many contacts (Nilsson 1981, 2008). Even though Katz belonged to a German tradition and wrote mostly in German, he brought an international orientation and reputation to the Swedish field (Ekman 1972, 166; Hugdahl & Öhman 1987, 464–465). To his successors, namely, the generation of experimental psychologists that pursued his work, this international orientation was accompanied by a strong American influence. Swedish–U.S. academic relationships had already been established in the young social sciences, not least of all due to the connections created by Alva and Gunnar Myrdal (e.g., Myrdal & Myrdal 1941). Allied with the social-democratic dominant-party, the Myrdals are widely considered to be the social engineers of the so-called Swedish model, which, among other things, involved a salient strand of internationalism as part of a broader modernizing project for the nation (Löfgren 1992; Ruth 1984). Around this time, the Myrdals thought of America as a model of inspiration: 'the continent of the future, land of the brave where the political will to implement radical and bold solutions to social issues existed at a time when they clearly thought it did not in Sweden' (Andersson 2009, 233).

While the impact of the Myrdals should not be overstated[9], it is clear that, directly after the war, North American connections were soon customary in the psychological field. This tendency was particularly noticeable in relation to the series of international congresses that were organized, where Swedish psychologists made acquaintances with renowned American scholars such as B.F. Skinner (Sörlin 1994, 206). Likewise, at Swedish universities, scholars began to subscribe to journals published by the American Psychological Association, and Swedish psychology researchers began visiting the U.S. regularly, for example, as guest professors, conference participants, etc., which also resulted in the emergence of a tradition in which doctoral candidates went on stipends to study in the U.S. (Sörlin 1994, 206–207). In the mid-20th century, chairs in pedagogy were split into one professorship in pedagogy and one in psychology, and departments of psychology were created to house the latter group of professors at Sweden's major universities: Uppsala in 1948, Stockholm in 1953, Lund in 1955, and Gothenburg in 1956 (Husén 1972; Künnapas 1976; Öhman & Öhngren 1991, 21). Accordingly, as more programmatic psychological research began developing in Stockholm and Lund in the 1950s, North American influences were already in circulation. The so-called Stockholm school developed under the leadership of Gösta Ekman, who took office after Katz. Throughout the 1950s and onward, Ekman and his followers developed an interest and research agenda in psychophysics. Unlike as it had been under Katz, psychophysics as headed by Ekman had strong ties to North American psychology of that time, where it had developed rapidly since the 1920s, linked to names such as L.L. Thurstone and S.S. Stevens (Ekman & Künnapas 1972, 195; Hugdahl & Öhman 1987, 468). Psychophysics encompassed measuring methods that were imported from the field of physics (Ekman & Künnapas 1972, 195), which, in the Stockholm group, led to a strong emphasis on method and quantification, as well as an alignment with the American praxis of working in research groups, with co-authoring as a common trait (Nilsson 2008, 167). While the Stockholm group appears to have pioneered this development, psychological research in various areas advanced at other universities, too. Groundbreaking work was presented in dissertations defended in Stockholm and Uppsala, for example, on event perception (Johansson 1950), psychophysics (Künnapas 1959), and motion perception (Mashhour 1964) – with all of these theses being written in English (see Johansson 1972). Under the leadership of Gudmund Smith, psychological research in Lund invested heavily in psychological theory, but, as it did also in Stockholm, this research adhered to a methodological approach imported from the American tradition (Nilsson 2008, 168). Smith in Lund, as well as Ekman in Stockholm, considered psychology to be part of the natural sciences, and they both wrote their most important work in English, often in collaboration with American colleagues (ibid., 168–171).

Around 1950, Swedish psychologists began to place their work in international journals. Swedish scholars who entered the field at this time enjoyed significant international success, with several making important contributions to the international forefront. By now, the Swedish field was rapidly consolidating. Ekman played a part in the founding of *Nordisk Psykologi* [Scandinavian Psychology], the first issue of which was published in 1949. The founding of this journal was initiated by psychological associations in Denmark, Norway, Finland, and Sweden, with the aim of providing a forum for scholarly and professional exchange in the Scandinavian countries. It contained a mixture of scientific articles, country reports, bibliographies, and reviews, thus serving as a professional forum for establishing linkages between the ways psychology was evolving in neighboring countries. For example, the first few issues contained accounts on the history of the field in each country, as well as accounts on the current knowledge frontier and tendencies across a range of different areas.

Initially in *Nordisk Psykologi*, Scandinavian languages were used most commonly, with the occasional exception of articles in English. Already in 1950, however, the journal imposed a policy of English language abstracts for original research articles. In 1951, Stockholm hosted the International Congress of Psychology. Linked to this event, Issue 3 (1951) was a special issue in English dedicated to presenting psychology research in Scandinavia. Thus, the issue comprised country reports written by Scandinavian scholars, according to the editor (p. 74), 'to inform their colleagues throughout the world about the directions in which psychological research in our countries has developed.' Conversely, the journal served as a way of informing the field about the development in psychology in the U.S., as well as evaluating the Swedish field's position in relation to the international forefront. In one such report, Karsten (1952) pointed out that in the U.S., the discussion on methodology dominated the scene. Reviewing developmental tendencies in Swedish psychological research, Agrell (1953) made reference to parallel currencies in American psychology and concluded that, in general, current Swedish research foci were well aligned with contemporary advances on the international scene (p. 106). Three years later, Ahlström (1956) came to a similar conclusion in his overview, but pointed out that the Swedish field lagged behind that of others in the area of acquisition and learning. In this vein, thus, the Swedish field kept close track of cross-Atlantic developments.

In the editorial of Issue 7 of the same journal in 1955, it was pointed out that the language policy that had been upheld thus far had limited the readership significantly, and that Scandinavian psychologists who aimed for an international audience therefore had been forced to turn to non-Scandinavian journals. For this reason, it was declared that *Nordisk Psykologi* had joined a partnership with the internationally renowned *Acta Psychologica* in Amsterdam, with the aim of cross-publishing articles. As a consequence, subsequent issues comprised a mixture of work written in Scandinavian languages and work written in English, for example, in the form of papers read at international conferences. Likewise, for a period, some issues (9, 1957) were dominated by English articles, while others consisted mostly of work in Scandinavian languages (10, 1958), etc.

It soon became clear that *Nordisk Psykologi* did not suffice in providing an arena for the rapid development of Swedish psychology. In Stockholm, an English language report series launched in 1954 (Ekman 1972, 168; Hyden 1984, 18). At the end of the 1950s, Ekman and his colleagues were beginning to enjoy recognition internationally, and their work increasingly appeared in international journals (Hyden 1984, 17). To create an international forum, *Scandinavian Journal of Psychology* was established in 1960, with the aim of disseminating research results to an international readership (Hyden 1984, 18). Thereby, it was decided that *Nordisk Psykologi* was to reorient itself toward its original Scandinavian focus and mainly provide overview articles, reviews, etc. – ultimately as forums for professional, not scientific, communication (Østlyngen 1968). From this point, the vast majority of all work was written in Scandinavian languages up until 2005, after which the journal changed its name to *Nordic Psychology* and shifted its policy to publish exclusively in English. As stated in the editorial in the last issue:

> With these six papers, Nordisk Psykologi ceases herewith in distributing publications in the Scandinavian languages. The goal of the shift to English is that the distinctive Nordic psychology will be brought to bear in the globalized world, which demands greater internationalization. (Elsass 2005, 302, my translation)

At any rate, to the university-based agents of the field, the academic world had already come to be the primary audience by the 1960s, with scholars communicating to them and each other by publishing in international journals (Hyden 1984). In overviews of Swedish psychology of that time (e.g., Ekman 1974), much importance was attached to showing that Swedish psychology was firmly established internationally, which was measured in terms of publications in international journals (Hyden 1984, 12). As Ekman notes elsewhere:

> The international character assumes particular importance for a small country, especially if it has a language of limited currency that greatly restricts the size of the potential audience. (Ekman 1972, 166)

Sjöberg (1976) provided a cross-country comparison of internationally oriented psychological research in Scandinavia. 'International' here was operationalized as work published in non-Scandinavian languages, which, as he noted, meant, for the most part, work published in English (p. 209). As his results show, Swedish psychologists were significantly more internationally oriented than their Scandinavian counterparts. As one of the potential reasons for this state of affairs, Sjöberg mentioned the compilation theses – that is, article-based theses with an introduction – which by now were gaining in popularity within the Swedish doctoral programs, accompanied by a thorough training in writing articles for international journals. Likewise, all five departments of psychology in Sweden now had an English language report series of their own, thus providing important training in using written English professionally.

In sum, the grounding of the international profile of Swedish psychology can be explained by the spirit initiated by Katz but amplified by the research agenda pursued by Ekman in the 1950s and '60s (e.g., Hugdahl & Öhman 1987, 464; Sjöberg 1976). In this period, the field left its national anchorage behind. This shift in orientation was palpably reflected in publishing practices and the languages used for these ends. University of Gothenburg provides an illustrative case, where the Department of Psychology published 251 theses between 1934 and 2015: 221 in English and 30 in Swedish. Among the first 10 theses to be published (1934–1952), 8 were in Swedish. Since then, Swedish theses have been submitted only occasionally: none in the 1960s, six in the 1970s, seven in the 1980s, four in the 1990s, and four in the 2000s.[10] As for the predominance of English, we can also look at the theses published at Stockholm University. From 1947 to 2005, a total of 234 theses were published, of which 197 were written in English, and 37 in Swedish.[11] The theses in Swedish are distributed over the entire period, so there is no period of time when choosing Swedish seems to have been particularly common. Rather, work written in Swedish seems to be linked to particular applied subfields of psychology and to genres, such as reports, that are more commonly written in these areas (cf. Salö & Josephson 2014, 280). Likewise, listings of theses published at Uppsala University from 1950–1998 show that 94 percent of the theses were published in English and 6 percent in Swedish (Wallin 1998). Between 1997 and 2015, 97 percent were written at the university in English.[12] When a wider range of academic genres in the contemporary psychological field is considered, the state of affairs seems more diverse. Nationally, in the time period 2000–2012, the share of English in conference proceedings was 91.7 percent and journal articles 94.3 percent, while Swedish publications were in the majority among reports (73.3 percent) and books (63.5 percent) (Salö & Josephson 2014, 280).

6 Contemporary academia in transformation

Thus far, this study has foregrounded a view of fields as historical formations and arenas of struggles, forged by internal and external impact. That is, it has dealt with disciplines as *things*, which, following Bourdieu's relational approach, is the realization of one state of the social. Ensuing this line of thought, the study turns to the other state of the social, namely to history as laid down in the bodies of contemporary agents, and the practices in which they engage. Before doing so, it is necessary to shed light upon some of the contemporary transformations in the university field at large, in other words, the broader space within which the two disciplinary fields are located. These transformations – or so this study argues – must be accounted for in order to grasp 'the structures which orient scientific practices' (Bourdieu 2004, 32). They are changing the rules of the game, and with respect to publishing language, those within the field of history in particular.

As revealed, both of the fields that I investigated have been caught up in a tug-of-war between nationalizing and de-nationalizing struggles. Lately, however, the latter tendency seems to have triumphed, thus preparing the foundation of an emergent transnational science (Crawford et al. 1993a, 1). In turn, this prevailing trend has been translated into a political vision, enforced under the banner of *internationalization* (e.g., Paasi 2005, 776). Internationalization and its metadiscourse is thus a key facet in modern struggles over global academia, where these forces are caught up in new forms of governing academic research that reinforce 'the role of managerial tools and evaluations of various kinds in the government of knowledge' (Kauppi & Erkkilä 2011, 324). These transformations are currently happening all across the world. By many commentators, they are seen as intrinsically linked to the rise of the neo-liberal university, leaving older, established images of the university 'in ruins' (e.g., Readings 1996). These images, more specifically, stem from the very influential idea of modern university research, closely linked to the vision outlined by Humboldt in late 19th-century Germany. Part of this vision was the emphasis on solitude and freedom in the pursuit of truth, facilitated – but not controlled – by the state, which, besides allocating funding, had no direct involvement in research (Krull 2005). However, due to a number of contemporary transformations that will be expanded upon presently, this notion of academic freedom is perceived by many as being sacrificed through a range of austerity measures that, according to critics, are driven by the imposition of market mechanisms by neo-liberal ideology (Lynch 2006; Naidoo 2003). In the critical literature, terms commonly invoked to describe these changes include 'managerialism' (Currie & Vidovich 2009), 'marketization' (Lynch 2006), and 'commercialization' (Bok 2003). Bourdieu (e.g., 2004), too, takes a critical stance to these developments, as he sees them as limiting the autonomy of science in that the forces of adjacent fields increasingly dictate the terms for scientific enterprise.

In research, the drivers of development feed into broad processes of globalizing and commodifying knowledge. In contemporary academic life, competition does not merely pertain to assessing the performance of individual scholars, which is a practice as old as science *per se*. There is now a research game driven by international competitiveness between universities, and ultimately, between states, which entails that not just scholars but also universities are interconnected within a global space (Lucas 2006, 7–11). At the risk of simplifying, this complex process can be phrased in more straightforward terms: Nowadays, it is not only researchers who have research strategies, but universities and their departments do as well – but since the latter agents are inanimate objects, they can only achieve their goals by having researchers change their practices. As Putnam (2009) has argued,

> [r]eputation and image of departments, then, stem from developing and using measures of success that are related to the way faculty accumulate symbolic capital. Thus, academics struggle to accumulate forms of symbolic capital that will enhance their individual reputations, their department's images, and the prestige of their universities. This criterion, however, although developed internally is also linked to cultural and social capital *outside a field*. (Putnam 2009, 130, emphasis added)

To disciplinary fields, but also to the scientific field at large, the question of autonomy from external interests is key, and therefore constitutes an important site of struggle (e.g., Bourdieu 2004; Maton 2005). The backdrop of the development pointed to above is an increasing emphasis on a particular notion of excellence and effectiveness on the part of universities, and ultimately, those who work in them. The initiatives that are taken center on increased demands for university accountability and, linked to them, the emergence of performative demands and criteria (Lucas 2006, 72; Putnam 2009). A key instrument of this vision is the adaptation of new techniques for evaluating research and new matrices for the allocation of university resources. This development should be seen in light of the emergence of rankings of the world's best universities, the best known of which is *The Shanghai List* (e.g., Kauppi & Erkkilä 2011). Universities compete for symbolic profit – reputation – the recognition of which is offered by such global ranking lists, where universities are tiered by virtue of a range of criteria that universities are inclined to maximize in order to pull rank (e.g., Engwall 2008). Symbolic profit, in turn, may be traded for material profit, since rankings and other forms of performance measures may be used by universities to secure a larger share of their state's funding, which, in turn, is increasingly exposed to competition. This development has given way to a process attaching more and more importance to the journal article (Hicks 2004, 204; Lillis & Curry 2010, 9). However, it is not journal articles in general that are highly valued; rather, as part of the assessment criteria used by the list-makers, much weight is placed on publication in high-ranked indexed journals (e.g., Putnam 2009). Initiatives of this kind are greatly facilitated by technical development, such as the commercial database service Thomson Reuters' *Web of Science*, used by list-makers and policymakers worldwide (e.g., Nelhans 2013; Ossenblok et al. 2012; Putnam 2009). According to critics, what this enterprise leads to is the singling out of particular tokens of achievement linked to the idea of excellence (e.g., Benner 2011; Hicks 2013). As Fejes and Nylander (2014, 233) critically note on this point, '[g]overnments and university administrators across Europe are increasingly calling for their researchers to publish their work in what are commonly held as "international journals", and in several countries research excellence is *defined* along these highly standardized, and arguably reductionistic measurements.' In this sense, researchers 'who spend their time writing books, textbooks, popular versions for broad audiences and so on are not helpful' (Larsson 2009, 38).

Thus, from a sociolinguistic point of view, this development brings with it a range of ripple effects, since, one can generally add, researchers who publish in languages other than English are not perceived to be very useful either. The databases used for extrapolating the information used by the list-makers house mostly English-language journals (e.g., Fejes & Nylander 2014; Larsson 2009; Lee & Lee 2013, 217). 'International' journals therefore become intrinsically linked to English-medium journals (Lillis & Curry

2010). Indirectly, thus, publish-or-perish regimes favor English (Kauppi & Erkkilä 2011), and the result of this process, critics argue, is the homogenization of publishing markets, where tremendous weight is placed upon Anglophone journal articles (Paasi 2005; Putnam 2009). As Holm et al. (2015) summarize, in many academic environments worldwide, these transformations are commonly held to work in favor of Anglophone homogeneity along the following chain of concerns:

> [a] publishing, especially journal articles, is key to professional advancement; [b] the more prestigious the outlet, the more useful the publication will be to a scholar's career; [c] but, typically, prestigious means an international and English language journal. (Holm et al. 2015, 116)

6.1 The Swedish university field: policies, practices, debates

Academia, then, increasingly seems to function like a research game, and less like a cultural field governed by its own logics. Although these tendencies have become more apparent elsewhere, such as the UK (e.g., Lucas 2006; Tight 2000), Swedish academia is no exception to the trend sketched above (e.g., Bennich-Björkman 2004, 2013; Ek et al. 2013; Larsson 2009; Widmalm 2013). As a country with a strong international self-image, Sweden has long been an early adopter of new ideas and concepts of policy-making nature, as they emerge globally (Hasselberg et al. 2013, 205). In the 1990s, such ideas conflated with the view that Swedish academia had too long been left to its own devices and internal logic (ibid.). Performance-based research policy in Sweden, then, is an 'expression of political-administrative will' (Widmalm 2013, 49) All political parties in Sweden are in general agreement that the promoting of internationalization is of utmost importance for Sweden to remain a leading knowledge nation. Hence, the broad objectives for a national strategy for internationalization of higher education stand pat, irrespective of shifting governance. Subsequently, both at the level of the state and at universities, attempts to 'increase the activity on the international publishing market' is gaining currency (*Resurser för kvalitet* 2007, 418, my translation). In Swedish research policy, as elsewhere, there has since been a shift in orientation toward a performance-driven orientation, linked to the idea of excellence and innovation, and the striving among universities to receive more funding, pull rank on the many listings, etc. (e.g., Benner 2011; Widmalm 2013). In short, in this vision, the implemented policies seek to encourage high-quality publications and discourage low-quality publications (Hammarfelt & de Rijcke 2014, 3–4). Like elsewhere, the techniques for achieving this goal center on finding ways of managing by measuring success and of allocating resources in a way that rewards particular forms of normative performance (Benner & Sandström 2000). For these purposes, bibliometric systems have lately been introduced, and here the basic node is the citation (e.g., Hasselberg 2013, 13; also Larsson 2009). Since 2009, Swedish research policy has encompassed the element of performance-based evaluation on the basis of publication and citation measures, which in turn is used as an instrument in the distribution of research funding along a complex chain of actors: from the state to research councils, faculties, departments, and individual researchers (Nelhans & Eklund 2015).[13] Funding, in this sense, is a policy tool (Sörlin 2007). Universities and their departments are evaluated by virtue of their productivity, and so are individual scholars, the publishing records and citation rates of whom serve as indicators of their individual productivity and impact.

It should be noted that, in Sweden and beyond, there is an ongoing debate about ways of finding responsible and sustainable ways of implementing performance measures in research evaluation – not least in relation to the diversity of disciplinary fields comprising the scientific field (e.g., Hicks 2012; see also Hicks et al. 2015 on the so-called Leiden Manifesto for research metrics[14]). What is more, in the Swedish context, universities are free to adopt their own models in the allocation of financial resources further down in their respective systems, and recent surveys show that universities have implemented different policies in this regard. Most universities seem to apply bibliometric models in distributing a share of funding to their faculties and departments, while performance-related economic incitements targeted directly to individual researchers occur only occasionally (Hammarfelt & Åström 2015; Nelhans & Eklund 2015, 29ff.). The most common scenario is the implementation of publication counts, whereby researchers' written publications are assigned points according to either well-established listings, such as the widely employed *Norwegian List*, or matrices locally adopted by universities, where high-impact publications typically yield a greater profit (Nelhans & Eklund 2015). It should be noted that many of these listings and matrices do not candidly privilege publication in English as a quality in its own right, as seems to be the case in the incentive structures imposed in other countries (Hicks et al. 2015). In Sweden, the imposed matrices typically emphasize high-ranked and peer-reviewed publication channels, and while weight is commonly placed upon international journals and publishing houses, languages other than English can be used while still scoring high (Hammarfelt & de Rijcke 2014, 4; see Nelhans & Eklund 2015 for the scoring at various universities). In some universities, the economic rewards received for different sorts of publications are readily accessible on their respective webpages.[15] Many universities also publish on their webpages advice on how to achieve 'good bibliometric results.'[16] Commonly produced by full-time bibliometricians, such advice stresses the necessity for researchers to choose indexed journals with a high impact factor, as well as the importance of publishing in English (e.g., Eliasson 2011; Kronman 2011).

English, we can say, is thus a pivotal resource for engaging in the markets of globalizing knowledge where profit is currently accrued. However, questions may be posed as to whether the desires of the state and its universities orient the practices of researchers de facto. Recent studies on the effects of implemented evaluation systems on publishing patterns suggest they do (e.g., Hammarfelt & de Rijcke 2014; Ossenblok et al. 2012). Using both publishing statistics and questionnaires to explore Swedish academia, Hammarfelt and de Rijcke (2014) conducted research on the Faculty of Arts at Uppsala University, where a performance-based model for resource allocation was implemented in 2012, thus following the trend at the national level. The local model, which uses a battery of indicators that include citations, publications, external grants, and strategic considerations, reallocates 10 percent of the universities' research budget. While these scholars are careful not to make strong claims about the causality between the imposition of the new model and the change in publishing practices, they show that English is currently making inroads to fields where Swedish previously prevailed in publishing. The study points to an overall increase in English publications, a trend that is particularly salient with respect to journal articles, where, congruently, the share of Swedish publications has dropped during the same time period (ibid., 7; see also figure 2, p. 8). In 2006, almost two-thirds of all publications were in Swedish, whereas in 2013, English publications were in the majority (ibid., p. 9). The study also points to an increase in publications indexed as peer-reviewed: with respect to journals, from 33 percent in 2006 to 65 percent in 2013.

What can be deduced from the above is that in Swedish academia, publishing strategies at collective as well as individual levels are rapidly gaining importance across most, if not all, disciplinary fields; there is, as Larsson (2009) notes, 'an emerging economy of publications and citations.' Nonetheless, it should be stressed that this development does not seem to affect all disciplinary fields in the same ways. With respect to some fields, the new criteria conflict with the ways in which fields, throughout their histories, have cultivated publishing traditions that differ from the currently desired pattern. In other fields, these new ways of assessing performance seem to fit the value economies already in place to a manageable degree, not least of all in relation to publishing practices. In the field of psychology in Sweden, for example, traits of this heated discussion are also present, ultimately because the issue unfolds in the university field at large, into which the disciplinary field is embedded. In psychology, however, the question of publishing language remains largely outside the realm of discourse, and, to the extent that the topic has surfaced, commentators on the field have made a case for an expanded international approach within the behavioral sciences in Sweden, as well as economic incitements to assure that this goal is reached (Sjöberg 2003). In this discourse, the publishing patterns of psychology serve as the blueprint for other fields to mimic (Hammarfelt & de Rijcke 2014, 13, following Whitley 2007).

However, as we shall see, psychologists also hold traits of contemporary transformation to be problematic. Be that as it may, current transformations are perceived as being more palpably ill-fitted to the values and practices upheld in other fields, many of which reside within the realm of social sciences and the humanities (Ekström & Sörlin 2012, 82). Generally, as Nederhof (2006, 96) notes, scholars of such fields tender to at least three publics: the international research frontier; regional or national colleagues; and the non-scholarly public. Therefore, bibliometric methods cannot be straightforwardly applied here. This is so because, ultimately, many of the texts produced in these fields are not included in the databases drawn upon in the bibliometric evaluation of research performance (e.g., Hellqvist 2008; Hicks 2004). In the humanities, consequently, these issues are much debated as part of a discussion about the usefulness of humanistic knowledge (e.g., Boguslaw et al. 2011; Ekström & Sörlin 2012; Forser & Karlsohn 2013).[17] For example, critics have forcefully argued that gearing toward international publishing markets is not necessarily the best way to utilize research in the humanities, which first and foremost deal with human conditions in a societal context (e.g., J. Nordin 2008, 615). For this reason, bibliometric measures do not apply well to a field such as history, according to the author of the example given below:

> An adman who recommends the local grocery dealer to advertise in the New York Times rather than the local newspaper would be regarded as senseless. Within bibliometrics the corresponding purpose and target audience analyses have no place. (J. Nordin 2008, 616, my translation)

These circumstances constitute a source of irritation among agents of the humanities, in spite of the fact that studies have shown that scholars of the humanities are not generally less favored by the models most commonly employed (Piro et al. 2013). Be that as it may, in 2014 the frustration grew stronger when the Swedish Research Council announced that it no longer intended to grant financial support to national journals, since doing so did not seem to comply with its mission (e.g., Björk 2013). Scholars and representatives of the concerned journals seem inclined to perceive this decision as an index of a lack of understanding among policymakers of the particularities of humanistic research (e.g., Barkman 2013). However, an alternative, yet still not far-fetched, analysis would be that, from the perspective of policymakers, using the research budget to sustain the preconditions for national publishing markets is at odds with the aim of encouraging international publishing agendas. For instance, Kerstin Sahlin, the chief secretary of the Council, was quoted in the press saying that these forms of subsidies possibly 'conserve more than they re-new' (Barkman 2013, 4, my translation). Thus, from the perspective of scholars of the humanities and beyond, this decision can be seen as an attempt on the part of policymakers to not only introduce soft incitements for researchers to orient toward international markets of knowledge, but also to work in pursuit of actively reducing the national markets of knowledge by forfeiting their subsidies. The Research Council's decision was later postponed, not least of all since it brought up to date a heated debate in the media over the marginalization of the humanities (e.g., Fleischer 2013; Salomon 2013), and more specifically the decision's negative consequences for the possibility of practicing scientific Swedish (Eliasson 2014; Josephson 2013).

In contemporary Swedish history research, more particularly, there is a lively debate on the international orientation, or rather the lack thereof, within the field at large. This debate, which has also revolved around the role of professional historians in society, is played out in special issues of the field's journals.[18] The backdrop is that in spite of the general striving toward international cooperation and output, most historians deal with research problems that belong to the national, Swedish frame (Österberg 2012, 178–180). Since the source material of most projects is in Swedish and the phenomena studied are situated in the Swedish context, there is, according to many debaters within the field, a lack of global perspectives in historical research in Sweden (ibid., 203–205). As a result, the more well-known Swedish historians are also remarkably invisible to international audiences (Sörlin 2013, 28). A core theme of the debate has accordingly revolved around the necessity and, if so, the means of moving beyond the national paradigm (e.g., Eklöf Amirell 2009a, 2009b; see Torstendahl 2009 for a response). Eklöf Amirell (2009b, 246), for example, argues that overcoming the national paradigm is necessary in order for Swedish historical science to retain its contemporary relevance and not ‘be reduced to an antiquarian science or national romantic genre of entertainment’ (my translation). However, other participants in this debate (e.g., Gustafsson 2005) have rejected the idea that Swedish historians are less internationally oriented than their counterparts in other countries, as did Mörner (1985). Östergren, likewise, has argued that Swedish history research is in fact internationalized in the sense that it draws on theories and conceptual tools adopted from the international scene (Östergren 2012). Ågren (2005) also acknowledges this fact, but contends that the problem arises in that the Swedish results produced in the Swedish context are not reported back to that same discussion, since Swedish historians publish for the most part in Swedish.

As can be seen, this debate also feeds into questions about publishing language, since a reorientation from the unique to the general entails writing to an international audience (e.g., Eklöf Amirell 2006a). Agents of the field such as Hedin and Nordlund (1999), Ågren (2005), Eklöf Amirell (2005), and others have argued for the need among Swedish historians to engage in dialogue with the international scientific community by publishing work in internationally viable languages – for the most part English. Ågren (2005) moreover questions the widespread opinion that fields of the humanities have a special responsibility to uphold the Swedish language as a vital means of communication, since this obligation is seldom placed on other disciplines. However, Gustafsson (2005), Östling (2013), and others question the idea that scientific publications in English should automatically acquire a value as being ‘international’. Gustafsson (2005), for example, argues that the humanities are inclined to focus on particularities, and that Swedish taxpayers, at any rate, are entitled to take part of the knowledge yielded there. Similarly, in 2008, the editor of *Historisk tidskrift,* J. Nordin, noted that most contributors to the journal are Scandinavians writing about Scandinavian topics to Scandinavian audiences; hence, he argued, it seems reasonable to use a language that all participants have mastered (J. Nordin 2008, 606–607). While acknowledging the importance of using English, too, he draws attention to the risk of so-called ‘domain loss’ to the Swedish language (see Salö 2014), and the subsequent need to counteract such a process by maintaining Swedish as a scientific language in the field. According to Östling (2013), it is particularly worrisome that newcomers to the historical field abandon the Swedish language in their publishing practices:

> Particularly younger researchers with an uncertain future are anxiously contemplating how they can increase their academic capital. For a while

> now, the hard currency has seemed to consist in peer-reviewed articles in American journals with a high impact factor. (Östling 2013, 15, my translation)

The concern raised by Östling here points to an issue to which this study will return in the next section, since it points to a generation gap, which is key to Bourdieu's theory of fields, and moreover seems to correspond to the findings of studies that investigate trends in publication productivity (Hammarfelt & de Rijcke 2014; Kyvik & Aksnes 2015).

7 Habitus as fields made flesh

To say that 'a scientist is a scientific field made flesh' (Bourdieu 2004, 41) is to recognize that any given social agent embodies the history of his or her lived experience. By this logic, only scientists involved in a specific field have the means to symbolically appropriate work and assess the merits of that particular field (Bourdieu 1975, 23). Following Bourdieu, we can say that the habitus of a historian or a psychologist

> is all at once a 'craft', a collection of techniques, references, and a set of 'beliefs', such as the propensity to give as much importance to the notes as to the text. These are properties that derive from the history (national and international) of the discipline and its (intermediate) position in the hierarchy of disciplines. (Bourdieu 1993a, 72–73)

In this section, interview data from four scholars will be presented. Firstly, the study presents interview accounts from professors in history and psychology, both, *ipso facto*, consecrated by their respective fields by virtue of being major holders of the capital valued by each of these fields. Secondly, accounts from doctors in the same two fields are presented. The doctors are both early in their careers, and they navigate through the value economies of their respective fields in search of viable career paths to pursue.

7.1 The habitus of the professors

The professor of history – henceforth Prof. Hist.[19] – is a senior department member at one of the largest history departments in Sweden, and through his research interests and investments, he is affiliated with another university. Because of such engagements, he holds a great share of the forms of capital at stake in academia, in general, and in his field, more particularly. In addition, he has been the leader of large research projects and has ample experience supervising doctoral candidates. This has put him in a position of reproducing the field, by exercising a form of soft symbolic violence by virtue of imposing his system of preferences onto the practices of newcomers in disguised, taken-for-granted forms (Bourdieu 1990b, 49–50; Reay 2004)

Prof. Psych. is a highly successful senior scholar in his field, who throughout his career has accumulated a significant volume of the key forms of capital at stake within this field. His achievements, mostly in cognitive psychology, have been materialized in the form of several prizes and awards. As a capital of academic power, he holds a professorship in psychology at a comparably large department. He is also honorary doctor at several universities in Sweden and abroad, and, as additional capital of intellectual renown, he has been elected as a member of several scientific academies of sciences. As for scientific prestige, Prof. Psych. has directed several large-scale research projects and has also been a member of several editorial boards for international scientific journals, where he has also served as editor-in-chief. Like Prof. Hist., he has supervised many doctoral candidate projects.

It should be clear from the field histories presented in section 5 that there are some salient differences between the two fields that the professors embody and are therefore selected to represent and give voice to. These differences, manufactured through historical struggles, also unfold over publishing practices, where publishing language manifests difference. Prof. Hist. embodies a field in which scholars traditionally write books or book chapters, as well as review books by other scholars. For these genres, Swedish is the dominating publishing language (Salö & Josephson 2014, 278). As part of his own scholarship, Prof. Hist. has written extensively on Swedish modern history, the majority of which he has published as a single author in Swedish. It is a typical trait in history that doctoral theses are extensive, often comprising more than 300 pages (e.g., Lindegren 2004). Correspondingly, Prof. Hist.'s thesis was a weighty tome, authored in Swedish, and has since been followed up by a respectable number of Swedish monographs. Yet, historians also publish journal articles. In part, these journals are vested in an international publishing market, but as a particularity of the historical field, there are a number of journals that publish work in Swedish – *Historisk tidskrift* and *Scandia,* to name two. Consequently, in the field at large the balance between journal articles published in Swedish and English is roughly equal in history (Salö & Josephson 2014, 278). Correspondingly, Prof. Hist.'s publishing record also encompasses edited volumes, articles, and book chapters in both Swedish and English, some of which are co-authored or co-edited with colleagues. He has moreover produced textbooks aimed for the markets of pre-university education.

Other genres notwithstanding, there can be no doubt that publishing monographs is what counts scientifically in the field. This is so in spite of the fact that publishing peer-reviewed articles is currently gaining in popularity. Prof. Hist. gives voice to this state of affairs in the following way:

> *Prof. Hist.: It's still the case that having published a book of your own counts the most, so to speak. Surely. But then you have the current interest in peer reviewed articles, and there are rankings of the fine and the not quite as fine journals. I think that's beginning to affect the consciousness. Perhaps not among us elderly, but among the young I think that's made a deep impression. And you note that they're very keen to have their articles published, it should be peer reviewed, and, yeah, a lot of efforts go into that.* (Extract 1)

As noted, the two professors are seen here as being embodiments of their fields, and in this sense they also embody the tensions and conflicts of their fields. In reviewing the history of history, it was noted that the field has long been a battleground where discourses have worked to shift the field in different directions. According to Prof. Hist., there is a tension within the field of history between, on the one hand, the old economy of symbolic value in which the monograph indexed scientific credibility, and, on the other hand, the new economy of symbolic, academic value, linked to publishing in high-ranked peer-reviewed journals. This latter strategy, Prof. Hist. claims to have noticed, seems particularly tempting to younger agents of the field. This observation was similarly made by an established historian in the study by Hammarfelt and de Rijcke (2014, 11; see also Östling 2013, 15, quote provided in section 6.1). Prof. Hist. expands on this issue in the following way:

> *Prof. Hist.: In some way, I think that we're possibly in the transition between two different approaches. One was that of the lone wolf who worked on his or her life achievement. Busy in doing so, there was never any thought about career, you were happy if you were given a sandwich to go with your coffee now and then. But then, as it were, you have those who are more bent for building up their CV.* (Extract 2)

Thus, according to Prof. Hist.'s sensibilities, things are happening in the historical field that seem to be altering agents' dispositions to historical research practice, including publishing practices. As of yet, however, the value attached to writing books remains strong in the field's locally ingrained economy of symbolic value *entre-soi*, in spite of the impact from the values circulating outside the disciplinary field. Prof. Hist. reports that the releases of published books are accompanied by symbolic rites that celebrate – and thus encourage – agents who withstand the practice of authoring books.

Prof. Hist.: Well, in the workplace corridor, people are very glad to hear that you've come out with a book. You gather around at the coffee break to display the book and all of that, and it's even become common to have small release parties, we didn't have those before. But you don't do all this when you have published an article. So it could well be that there's a certain difference in valorization between the bibliometric norm on the one hand, and the colleagues' appreciation, on the other. That's probably the case. (Extract 3)

Prof. Psych, on the other hand, holds a dominant position within a field where scholars predominantly publish journal articles, and here the prevalence of English is almost total (Salö & Josephson 2014, 280). Correspondingly, this trend is saliently reflected in Prof. Psych's own academic performance output. His publishing record reveals hundreds of publications, the vast majority being articles in English. These articles are typically short (typically seven pages) and co-authored (typically more than five authors per article). He has also written a few monographs, a small share of which is in Swedish. Yet, as for scientific prestige, the journal article is valued most highly, as indicated in the account below.

Prof. Psych.: I'd reckon that you with rather great certainty can say that articles in journals carry the highest prestige. Researchers within the area place their best work there, rather than in book chapters or in books, since chances are that they don't reach very many. (Extract 4)

As the development of the psychological field shows, from the beginning of his career, Prof. Psych. entered a field where the journal article already held a central position. The central position of the journal article in psychology can also be mirrored in Prof. Psych.'s consumption of academic texts.

Linus: Could you say something about the sorts of scientific texts, genres and so forth, that you read the most?

Prof. Psych.: It's definitely journal articles. Articles in journals that are situated within the areas where I do research. I read the odd handbook and textbook too, but mostly, it's journal articles. (Extract 5)

As noted, the vast majority of Prof. Psych.'s output is in English, whereas most of Prof. Hist.'s publications are in Swedish. It is noteworthy that both professors talk about their preferred publishing languages by virtue of reaching as many readers as possible – but that the outcome of their position-taking places the two agents on opposite poles of the Swedish–English continuum. To Prof. Hist., using Swedish is linked to the possibility of receiving a form of impact that resides beyond intra-scientific recognition. By and large, this fact indexes that history has a field of consumption that is much wider than the field of production (cf. Broady 1991, 270ff.), which, as section 5.1 shows, has roots in the early rise of the field. This trait also means that Prof. Hist. perceives Swedish as being the language that will attract the most readers, since English, in his view, limits the readership to a specialized trade of history research.

> *Prof. Hist.: When you write in Swedish, you can entertain hopes of reaching the educated people, so to speak. But when you write in English it's more of a marked academic context. How can I put it, no ordinary, generally interested Brit or American will read my journal articles, I don't think so. But, if you publish something in Swedish, it could hopefully, well maybe, be mentioned in a daily, and in that way get some sort of attention.* (Extract 6)

Prof. Psych, likewise, claims to be guided by the aim of reaching as many readers as possible, as indicated in his position-taking on journal articles (extract 4). In his discourse, however, this fact renders English to be the most viable language to use. Prof. Psych says there are no forums where it is possible to publish in Swedish within the field, unless one chooses to target other audiences, such as the wider public. This position-taking also incorporates the history of the field, marked by an overt specialization in the 1960s that subdivided the field into one purely scientific branch. As a consequence, psychology researchers write more or less exclusively for other researchers; choosing Swedish, therefore, would entail engaging in dialogue with Swedish specialists only.

> *Prof. Psych.: I write for the international scientific community, that way my article will reach fairly many. If I were to write in Swedish, it would reach only a handful of people if even that many, maybe one or two, who deal with and are interested in precisely what I deal with. For this reason, it has become this way in this area that in order to reach the international community of researchers, you have to write in English.* (Extract 7)

In psychology, English has been the dominant publishing language for as long as Prof. Psych has been in the research game. The idea of using Swedish in scientific publishing has therefore never really been up for discussion. Hence, he does not make 'language choices'; rather, using English is a part of his feel for the game of scholarly exchange. He has, in other words, internalized the regularities of the game, and therefore 'does what he must do at the moment it is necessary, without needing to ask explicitly what is to be done' (Bourdieu 1998, 98). Using Swedish for the sake of protecting the language, likewise, carries little, if any, value for Prof. Psych. Yet, he reports having noted certain national differences in this matter, that is, that the discourse of protecting the national language seems present in psychology – but mostly in other countries. At least in part, he sees this stance as a proxy argument, that is, as a statement that disguises its underlying motifs.

> *Prof. Psych.: I think you see differences between the Nordic countries, I think Sweden and Finland are the two countries who have most clearly become English writing when it comes to scientific texts. […] I have often been struck by the way things are in Denmark and Norway where you write in Danish and Norwegian respectively. And then, we […] pose questions such as 'why don't you want to reach out to researchers in the whole world with these results, they are exciting, you should tell the world about your results'. 'No', they say, 'we want to cultivate and protect our language', they say. Very often in Denmark, that's what they say. […] They often use that about safeguarding Danish. If it is honest, the thought is good, but I think you lose something in doing so as a researcher within a given discipline.* (Extract 8)

As illustrated by this extract, Prof. Psych. associates publishing in national languages as being at odds with long-established preconditions of the national enclaves of the global psychological field that reside outside the Anglophone world. In so doing, he echoes the stance taken by early field-founders, namely that psychological knowledge is inherently international, and that international languages must therefore be used as a requisite for maximizing a potential audience (e.g., Ekman 1972, 166). Within his field, he sees this struggle as somewhat passé.

This situation is different in the field of history. It was noted in section 5.1 that the field of history has been impacted by the co-existence of contradictory discourses on the need to maintain the national obligations of the field while at the same time align its publishing practices with current developments linked to internationalization and other contemporary fluxes of the university field. These discourses unfold in Prof. Hist.'s position-taking as a salient ambivalence. He is well aware of the discussion on the lack of international perspectives in his field. Partly, he seems to agree with much of this criticism, while still acknowledging some of the advantages linked to using Swedish. He also points to some of the difficulties linked to using English, which partly feed into matters of practical language competence, and partly pertain to the defense of Swedish. This fact corresponds to findings in other studies, where senior historians in Sweden were shown to be less positive to writing in English (Kuteeva & McGrath 2014; see also Lee & Lee 2013 for similar accounts from Korean history professors). Prof. Hist. gives voice to this issue as follows.

> *Prof. Hist.: On the one hand, I consider it to be extremely important that we carry out our research in an international context, and I do think that I belong to a generation that has been somewhat poor in doing so, and a little bit too self-absorbed and confined in a Swedish milieu. And it follows from that that you publish more and more in English. At the same time I consider that we have to stand up for Swedish, partly because of some sort of linguistic nationalism. I also think that the language often becomes a bit colorless. You have a smaller repertoire when you write in English, and even if you obtain help with that, it won't be the same.* (Extract 9)

As can be seen in this extract, publishing in English is viewed by Prof. Hist. as indexing a form of strategy associated with both positive and negative valorization. On the one hand, publishing in English is seen as necessary and unavoidable, but on the other hand, as potentially negative both to research quality and for the position of the Swedish language. Even though there are journals in history that publish articles in Swedish, Prof. Hist. seems to see publishing in journals as being linked to English. What is more, publishing in journals indexes the dispositions of the striver who has strategically incorporated the value system imposed upon the field. This position-taking surfaces when Prof. Hist. is asked to comment upon the status of being internationally oriented within the field.

> *Linus: Is it considered high status to be internationally oriented in the discipline?*
>
> *Prof. Hist.: I don't know if I really think so. Of course you think that it's good. And then, this is all tied to the striving of publishing in journals, and then it becomes linked to the bibliometric trend. So it can perhaps be, well indulgently and with a faint smile, that you think that hehe, 'he knows what it's all about', so to speak, do you see what I mean? There might be some sort of conflict here between doing what you yourself really consider to be good, and of conforming to norms from the above or external norms. It's of course great to be published in a fancy journal, but on the other hand, you can find yourself smirking when people are too eager, as it were.* (Extract 10)

As can be seen, Prof. Hist. speaks about an emergent form of conformism among a group of eager colleagues, who in some overtly strategic sense seek to re-orient their strategies to values that reside beyond the symbolic order of the historical field. 'Conformism', however, is a slippery notion to apply here, since adhering to the established order of the field is also a way of conforming to a form of 'logical conformism' rendered legitimate by the field, so that nationally embedded publishing practices come to be seen as the natural way of conducting one's self (Emirbayer & Schneiderhan 2013, 142). As similarly reasoned by Hammarfelt and de Rijcke,

> it would be naïve to think that 'publication strategies' did not exist before the implementation of various bibliometric measures and evaluations. Rather, the criticism against 'publication strategies' could be seen as a conflict between older mostly disciplinary and national traditions of publishing, and new more internationally oriented practices. (Hammarfelt & de Rijcke 2014, 13)

Hence, practices of publishing in Swedish 'appear "disinterested" only in reference to different interests' (Bourdieu 1991, 8) – namely, publishing in English. There are, in other words, two symbolic markets, and in this sense, there is no way in which agents can be non-conforming with regard to publishing language. In academic life, there are no no-choices, or better, no line of action that is not linked to strategies in Bourdieu's non-strategical sense of the concept (Bourdieu 1991, 9–10). This means that agents who stick to writing monographs in Swedish also pursue an investment strategy, one which seems profitable given the censorship historically exercised by the field in its ways of rewarding performance (Eklöf Amirell 2007, 37). The strivers that Prof. Hist. refers to, therefore, are first and foremost 'obedient to the conformism of anti-conformism' (Bourdieu & Passeron 1979, 46).

One salient shift in practice that Prof. Psych. reports to have noted throughout his career is an increased focus on cooperation, and the ensuing rise of research groups in which each person contributes with his or her area of specialized knowledge. As noted, most of Prof. Psych.'s publications are co-authored, and co-authorship is common in the psychological field. A bibliometric analysis of the publications in psychology from 2012 to 2013 shows that the average number of authors (median) of a text is three.[20] As Prof. Psych. notes, this fact results in extensive publication lists among the most successful players in the field, and here the impact factor of the journals that one publishes in carries a great deal of distinction. Prof. Psych. states that there is a salient awareness about the performance assessment imposed in the field.

> *Prof. Psych.: People keep very close track. I notice among doctoral candidates and others how keen they are to learn these things. Already in writing up articles during the course of doctoral studies they struggle for being able to send it to a good journal, with a high impact then.*
>
> *Linus: Is it so that that people typically know what index they have?*
>
> *Prof. Psych.: Eh, I think everyone knows this. Everyone knows their h-index.* (Extract 11)

According to Prof. Psych., most agents of the psychological field know their h-index[21] as a form of knowledge internalized by engaging in the field. This is not so in history: Prof. Hist., at least, reports not knowing his index. In history, moreover, co-authorship has long been fairly uncommon, as is typically the case in the humanities at large (e.g., Piro et al. 2013). While Prof. Hist. has in fact experience in co-authoring and co-editing, his overtly empirical field belongs to a set of fields that house an ideal of the independent, academic loner, where individual contributions are highly valued (Sörlin 1994, 225). Yet, as already indicated above, Prof. Hist. reports noting the emergence of a new set of dispositions to publishing practices in his field, particularly among junior department members. These changes do not pertain merely to co-authorship, but are rather seen by Prof. Hist. as an index of a new form of CV-thinking, making scholars prone to only engaging in scientific practices that yield materialized profit in the competition.

> *Prof. Hist.: When I was younger you could very well partake at a conference, give your talk and then go home, pleased and satisfied. But now, like, 'it'll result in a publication afterwards, right?' So I reckon there are higher demands that everything that you do will lead to a publication. There's a stronger instrumentality in the thinking, everything must lead up to something which shows clearly in the CV.* (Extract 12)

In this account, Prof. Hist. points to a new line of instrumental thinking as a facet of a newly developed scientific habitus of agents within the historical field – most palpably manifested among its newcomers. This matter, which is linked to changing dispositions to historical research practice, will be dealt with below, where extracts from interviews with the doctors are presented.

7.2 The habitus of the doctors

In this section, data is presented from interviews with one doctor from each of the two fields, both of whom are currently at the beginning of their respective career paths. As noted earlier, however, they have already begun to pursue their careers, which means they have experience in being evaluated in different contexts. As one of the informants, Dr. Psych., puts it, 'it is evaluation all the time.' It follows from this that the doctors have a form of practical knowledge or sense of the ways in which the field ascribes value to academic achievements and, in relation to that, a sense of their own deeds.

The recipient of external project financing, Dr. Hist. currently works at one of Sweden's larger history departments. His thesis was a monograph published in Swedish that exceeds 300 pages. Since his defense, he has been able to support himself by being part of various research projects, combined with temporary positions at research institutes elsewhere. Dr. Psych., for his part, works at one of the larger departments of psychology in Sweden, but he earned his doctoral degree in another Swedish department. His thesis is a compilation type, which corresponds to the predominant – if not close to exclusive – praxis of the field. For a time period spanning several years, Dr. Psych. has managed to get along by means of holding various temporary academic positions, which have followed what he refers to as 'the standard route' in the psychological field: post-doctoral position – research fellow – substitute and associate senior lecturer, etc. He has also had research funding for a period of time. Currently, however, he holds a lectureship and is thus a permanent faculty member. Moreover, he was recently appointed 'docent', the academic title in Sweden roughly equivalent to associate professor in the U.S. or reader in the UK.

Neither of the two doctors report having reflected much on the language of publishing upon entering their fields. Publishing in Swedish was conveyed as being the default choice to Dr. Hist., and by the same token, publishing in English was without doubt the way to proceed for Dr. Psych. This fact appears to exemplify Bourdieu's point that '[a]gents to some extent *fall into* the practice that is theirs rather than freely choosing it or being impelled into it by mechanical constraints' (Bourdieu 1990a, 90). The two doctors comment on their publishing strategies in the following way.

> *Dr. Hist.: When I wrote my PhD thesis, for example, not a single question came up if I was to write in Swedish or English. It wasn't that they told or encouraged you to write in English, it was more like, you did it in Swedish. Most in my group of doctoral candidates did it in Swedish.* (Extract 13)

> *Dr. Psych.: I have never published anything in a peer-reviewed journal in any other language than English. English is the language that my subdiscipline uses as default. […] But there are nuances; it differs between sub-disciplines. In my case it is only English, you could say, in principal.* (Extract 14)

However, as their careers have continued, Dr. Hist. has published increasingly in English, whereas Dr. Psych. has obviously stuck to English. Dr. Psych. points out, as did Prof. Psych., that the regularity revealed in his publishing record is linked to the fact that there is no market for publishing scientific texts in Swedish within his subfield.

> *Dr. Psych.: I have no good Swedish outlet. […] It would be a matter of going to Natur och kultur [a Swedish publishing house] and write up a book in Swedish, but I'd get no cred for that, it would end up primarily in my pedagogical deed, it becomes course literature.* (Extract 15)

Indeed, then, Dr. Psych. subtly acknowledges some potential possibilities for publishing in Swedish, but adds to this statement that the field would not reward such efforts to the extent that doing so seems worthwhile. This entails that, indeed, there are options in the psychological field to author texts in Swedish, but that such undertakings are largely incompatible with the ways in which scientific profit is achieved in the field. In this sense, thus, we can say that publish or perish ideology imbued in his field exercises a form of censorship that orients his scientific practices. Nonetheless, this fact pertains mostly to scientific merits, since the production of course literature can carry weight, for instance, in the competition for academic positions.

> *Dr. Psych.: If I write, for example, course literature within my field I might get a bit of bonus points on the scientific side, but it doesn't carry much weight, it's articles, articles, articles, articles. On the other hand, I could emphasize it more clearly as a part of my pedagogical deed. And the importance of that is dependent on what is evaluated in a given situation.* (Extract 16)

Clearly, the same logic seems difficult to apply to the field of history, where there is in fact a field of options for scholars who wish to publish academic texts in Swedish. As a matter of fact, there is a journal market as well as a book market to meet these ends. This fact notwithstanding, it can be noted that Dr. Hist.'s investment strategies have differed somewhat as compared to those of Prof. Hist. Dr. Hist.'s publishing record displays a diverse set of journal articles, book chapters, and extensive studies published in report series. Here, although work in Swedish also amounts to a fair share, there is an overabundance of work published in English, as well as occasional publications in other languages. It is noteworthy that similar publishing patterns are reflected among the young Swedish historians studied by McGrath (2014), where the two junior informants who were studied had only published in English (p. 10). In history, authoring books in Swedish has long been positively sanctioned by the field. However, linked to current developments, viz., in particular, the 'bibliometric trend' in Prof. Hist.'s denotation, Dr. Hist. gives voice to a type of pressure in the system that works in an unfavorable way for this form of publishing trajectory. On this point, Dr. Hist. makes the following remark:

> *Dr. Hist.: At least in the discipline of history, there's some kind of break in time concerning the language issue that those who are currently professors have not had to confront in the same way. There hasn't existed a demand or an expectation that they should publish in other languages. It has probably been nice if they have, but I don't think that there's been this pressure in the system that this is what it takes to go upwards or on-*

> *wards in your career. So there, I think there's quite a big difference really.* (Extract 17)

As can be seen above, Dr. Hist. refers to a 'pressure in the system' that is changing the values and practices of historical research. To a great extent, this can be explained by virtue of the fact that recently graduated doctors of today face a labor market fierce in competition in a way that was not the case for previous generations. For example, according to Ekström and Sörlin (2011), the number of active historians today is 10 times greater than the number in 1950. Dr. Hist. makes the following remark on this topic:

> *Dr. Hist.: Those who are professors today, they could become historians, and then, they could in some sense lurk about in the system in full assurance that, sooner or later, they'd end up somewhere. This isn't the case for my generation in the same sense, since now, you can actually be pushed out.* (Extract 18)

Thus, in Dr. Hist.'s account, increased competition in the field is seen as an important driver for newcomers to reorient their strategies. What is particularly noteworthy here is that Dr. Hist. sees the adaptation of English as a chief means of investing differently and so gain a profitable position in a field that has long been nationally oriented.

> *Dr. Hist.: Well, for that matter I think that the discipline of history has been, and is, a little bit more protected than certain other disciplines. It's big enough to carry its own weight in some way. Plus the fact there's an audience outside of the university who wishes to read in their mother tongue. So there's also some sort of unspoken mission of managing the national history, and that's easiest done in the own language. In that sense, I think it [the discipline] has been a bit protected or that it maybe has not been exposed to the same pressure for change in comparison to other disciplines that are more intra-disciplinary. But on the other hand, in recent years I think that it has grown into the discipline of history too. It has opened up an opportunity to qualify oneself through that course, you can compete by publishing mainly in international languages, chiefly in English, then.* (Extract 19)

In other words, according to Dr. Hist., using English is currently part and parcel of another way of competing in the field of history. In this sense, English as a form of linguistic capital can be converted into scientific authority – yet, only 'under certain conditions' (Bourdieu 1975). These conditions are currently provided by the political-administrative will that dictates the terms in the contemporary university field at large and that currently encourages international research agendas.

Dr. Psych. was asked whether he sees any differences between generations in their orientations to publishing. In respect to publishing language, his response was *no*; however, there are other ways in which newcomers can aim at reaching a profitable position within the field by asserting difference.

> *Dr. Psych.: No, and it absolutely might not exist locally at a department or even within the country, but you can observe a trend coming in internationally, and if you're an early adopter you'll probably not be countered initially, but most likely later on when you have a career. [...] International trends come all the time, and in psychology Sweden commonly follows the U.S.A. It might not be this way in 20 years, but it's this way today. Research topic wise, what happens there will happen here a few years later, and if you stay alert to the international trends as a young researcher, you can slip into something that almost surely will end up here later, irrespective of what parts of your surroundings say.* (Extract 20)

From the accounts of both doctors, it is clear that different investment strategies pay off for different forms of evaluation, as indicated in Dr. Psych.'s earlier account (extract 16). In both cases, this is due to the fact that different merits carry weight in applications for positions and funding, respectively. For example, from the viewpoint of his field, Dr. Psych. makes the following remark.

> *Dr. Psych.: In order to be attractive to research funding, you have to produce loads of articles. [...] To receive financing for research, you need to work on the scientific production. To be appointed a lectureship, well, then you also need teaching merits.* (Extract 21)

This account may come across as rather self-evident, since, in principle, it applies to the scientific field at large. It is therefore also valid in the field of history. Here, however, there are other field-specific values at stake attached to scientific production, since a key difference between applications for research grants and academic positions pertains to the position in academic space of those who assess merits. Dr. Hist. believes that his publishing record has been fairly competitive in applications for research funding, but less so in applications for academic positions. According to Dr. Hist., this is largely due to the fact that he has not aligned his investment strategies to those traditionally valorized in the field, which means that applications assessed by dominant historians see his merits as being limited in a way that does not seem to be the case when he competes for funding. By no means, then, does an international publishing agenda entail a way of becoming competitive in the field on all fronts.

> *Dr. Hist.: I must admit I haven't applied for many positions. I have applied for some where I was ranked high but where I was taken off the*

> *short list with the argument that my main publications were on [modern history]. That's a kind of derogatory assessment which resulted in them moving up someone else who had done something else. [...]I've the feeling that when you as a historian apply for money from the research councils, you are shunted into this publishing pressure, attention is put to your publishing record, and so the scale ends up being the same as for other disciplines. But when you apply for positions in history, you get evaluated by other historians, and there, it can probably linger about in the system that you don't really count unless you have done your years in the archive. Among historians, it's still met with respect if people see that you have done the dirty work.* (Extract 22)

Here, Dr. Hist. points to an emergent facet of the historical field that has already been hinted upon, namely that the field comprises two partly overlapping economies of symbolic value, where one set of field-specific values points back to the history of the field. Dr. Hist. may have written several texts in English, but he has not done a sufficient amount of 'dirty work', which, arguably, means time in the archives with the other 'archive martyrs' (Larsson 2010). This fact is manifested in what is seen as a lack of extensive work in his publishing record, as perceived through the prism of the field's internal economy of symbolic value.

As for the value attached to scientific texts, both doctors by and large attest to the accounts presented by the professors: monographs are valorized in history, while journal articles carry scientific prestige in psychology.

> *Dr. Psych.: Peer-reviewed articles, not other articles, but peer-reviewed articles are highly valued. Book chapters are very low valued as they are not seen as peer-reviewed work, even though they often or sometimes are. For example, I've published a few book chapters, and if I were to apply for a professor position today I would get very little cred for that. They would look at the peer-reviewed articles, even though I sit here knowing that I've put much more effort into the book chapters, since there you take a holistic view in some way, you often cover an entire field in a book chapter in a sense that you don't do in a very narrow article. So I often find articles to be easier to write.* (Extract 23)

It can be noted that in Dr. Psych.'s field, to a considerable extent, texts derive their value from the forums in which they are published. At the top of this hierarchy, the position of the journal article is unchallenged (e.g., Hicks 2004). In the field of psychology, this also means that other genres, such as book chapters, do not really yield profit in the competition; this seems to be so irrespective of the authority provided by the publisher or of the language used. Hence, neither do peer-reviewed book chapters in English carry much weight in relation to publication in journals.

This matter is more distinctively linked to publishing language in Dr. Hist.'s account. According to him, it is difficult to fit historians' overtly empirical interests into the format offered by the article genre.

> *Dr. Hist.: Among historians, it's not considered impressive to have written 10 articles on [the same subject matter]. I don't think it really matters where you have published them, it's rather seen as you trying to cash in on what you have done. [...] After all, the main publishing form for historians is the monograph. There're so much that historians do that cannot be done in these brief articles. It's a more extensive form of account, it's a science which isn't oriented towards theoretical approaches to a problem. If you publish internationally, rather few journals accept empirical accounts or descriptions of courses of events. You delve into theoretical debates, and there, empirical data is what you use to exemplify theoretical arguments.* (Extract 24)

As shown, Dr. Hist. notes that work aimed for the international journal market needs to engage primarily with theoretical debates in which empirical data are used to support and exemplify the arguments that are made. In contrast, the national market demands that its producers engage thoroughly with the empirical material as a task in its own right, and here it carries no value to present a narrow argument in brief.[22] As can be noted, in the historical field, there is a strong conviction that historians deal with big issues that cannot be fitted into small formats. According to Dr. Hist., in the local value economy of the field, publishing several articles on the same topic carries limited weight. He here points to an interesting trait of his field, namely that, to a limited extent, the forum *per se* sets the price of scientific products. At any rate, this is so in relation to psychology, where there seems to be in place a more institutionalized belief in the prestige of journals, measured, for example, by virtue of their impact factor. Dr. Psych. was asked about the extent to which such measures orient his publishing practices.

> *Linus: How much would you say that you take, or have taken, into account things like a journal's impact factor and so forth when you plan where to send it?*
>
> *Dr. Psych.: Well, imaginably significantly less than many of my colleagues. I'm here since I enjoy it, but I'm aware of these types of things, and I don't publish in too poor of journals, with the word poor in quotation marks, then, without having some sort of ... it should be an internationally renowned journal, if it is, it will have a bit higher impact. [...] I should perhaps add some nuance when I say that I haven't been particularly calculating. I'm very well aware of how the system in which I am located functions. I'm also aware that I might not always have chosen the most strategic things. For example, I have no self-authored articles. I have consciously chosen to have a more inclusive position. I'm fully*

> *aware that I'll need to produce a few extra articles in order to be able to apply to becoming a professor compared to if I wouldn't have had such an inclusive approach.* (Extract 25)

In this extract, it may be noted that Dr. Psych. initially produces a discourse in which strategic thinking linked to career interests are downplayed. He says that he is first and foremost in the game because he enjoys it. This shows that being overtly strategic in psychology is also seen as gauche, and by virtue of this fact, 'the field will award those who appear to be without interest' (Lucas 2006, 70). Yet, Dr. Psych. later clarifies this self-image by adding that he does take into account the formal valorization of journals, as well as other indicators of scientific valorization, such as that offered by being the single author of articles. Co-authorship, asserts Dr. Psych., is generally highly valued in the field, but with regard to being an independent researcher, being the single author certainly carries weight within his field as well. This is so because its value economy also encompasses a trait of Humboldtian thought that celebrates solitude and independence as part of the scientific craft (e.g., Krull 2005). In evaluations, therefore, having a record both as co-author and single author is profitable. Dr. Psych., thus, is well aware of the symbolic economy of his field, or, put another way, the rules of the game he plays. However, he also testifies to the assertion that the voicing of self-interest and strategic thinking goes against the logic of research practice, where crude career interest ought not to be separated from research interest (Broady 1991, 206–207). That position can be seen below.

> *Dr. Psych.: People are differently good at adapting to life in an environment. I won't be naming anyone since it's not interesting, but of course, if you're located in an environment, understand the environment in which you are located, then you see people who are very calculating, and you sometimes wonders if it's driven by research interest or if it's out of career interest. They can go hand in hand, but …* (Extract 26)

Time pressure is a central theme that surfaces in both doctors' interviews. With increasing demands of productivity, time is clearly an issue to most inhabitants of academia, who in their publishing practices must take into account the demands of the markets. Both doctors testify to the fact that this sort of adaptation is particularly salient to newcomers, who are at pains to align their practices with the possibilities offered by a field at a given point in time. This fact is particularly pertinent to doctoral candidates, that is, those who lurk just below the two doctors in the hierarchies of their fields. Toward the end of the 1990s, the Swedish system of doctoral studies was subject to profound reforms, which put an end to a discussion dating back to the 1960s (e.g., Lindberg 2004; Peixoto 2014). For many years, doctoral candidates had experienced poor working conditions, often financed by education grants. From the perspective of the state, this circumstance was seen as ineffective, and initiatives were therefore taken to ascertain that doctoral candidates were fully financed at the beginning of their doctoral studies. As a causal implication, doctoral candidates are now expected to write up their theses over a period of four years of full-time employment, a timeframe during which they are also obliged to complete their coursework. Among other things, the reform meant a 'turning of the economic thumbscrews which makes doctoral candidates hurry up and finish in the four stipulated years' (Lindberg 2004, 188, my translation). Recent studies on this matter have shown that doctoral candidates nowadays struggle to complete their thesis within the allotted time (e.g., Peixoto 2014). This fact is linked to the emotional stress of having to perform within the given timeframe (Hasselgård 2012, 154; Peixoto 2014). For example, doctoral candidates in history enrolled after the reform have expressed concerns about their chances of producing theses that can compete with pre-reform work, which had taken a much longer time to complete (Högskoleverket 2003, 35).[23]

As noted, doctoral candidates in psychology have long written up article-based compilation theses (e.g., Sjöberg 1976). This tendency stands pat within the field at large, where few theses are monographs.

> *Dr. Psych.: In principal, we don't write monographs anymore, doctoral candidates are allowed to, but we've had one in the last few years, so things are moving towards compilation theses. I suspect that this is a consequence of articles being more highly valued.* (Extract 27)

In history, the format of compilation theses began to appear in the 1960s, but has since not been common (Blom 1980, 7; Eklöf Amirell 2007). Dr. Hist. senses that this state of affairs is currently shifting, and the reason for this shift, he asserts, pertains to the fact that in relation to the field they wish to enter, doctoral candidates see it as being unfeasible to publish monographs.

> *Dr. Hist.: I've met doctoral candidates that evidentially write compilation theses. I think that's got to do with the fact that they don't have time to write monographs anymore in the same way. In these four years, it's difficult to come up with a historical monograph that's completive in the sense that it could match with those that were written in the old system. So I believe that a compilation thesis becomes a very good alternative to a bad monograph. You get a good compilation thesis meaning three published articles, which is better than a semi lousy monograph.* (Extract 28)

Indeed, Dr. Hist. may well be right in this prediction. According to representatives of universities where doctoral studies in history are offered, doctoral candidates currently seem to write up their theses according to a pattern that indicates change – both in relation to the choice between monographs and compilation theses, and between Swedish and English. It was noted in the section accounting for the history of the historical field that the vast majority of all doctoral theses have been monographs published in Swedish. For example, in the most recent bibliography (Eklöf Amirell 2007), which lists 606 theses published between 1976 and 2005, 13.2 percent were written in English, 2.5 percent in other foreign languages, and the rest in Swedish. In the material, six theses were identified as compilation theses, and, out of these, only one was written entirely in English. Set against this backdrop, a survey was distributed to university departments that are entitled to award a qualification at the level of doctoral studies, in which respondents were asked to state (or estimate) the format and language of the theses that are currently in production.[24] The survey revealed that some 138 theses in history are currently being written, out of which approximately 9 are compilation theses. While nine theses amount to a fairly small share of the total number of theses written (6.5 percent), in absolute numbers it means that more theses are currently being produced in this format than were written during the entire period from 1976–2005 (Eklöf Amirell 2007). It may also be noted that six of the compilation theses currently being produced contain studies and summarizing chapters in English, whereas three seem to comprise work published in Swedish. As for the general distribution between languages of publication, the survey shows that 34.8 percent of those currently being produced are in English, which suggests a remarkable increase in the adoption of English. On all of these points, however, there are major differences between the universities: for example, at the University of Gothenburg, the majority of the theses are in English; whereas at Lund University, no theses are written in English. Such local differences aside, it seems clear that English is currently making unprecedented inroads into the historical field, at least with respect to doctoral theses. This finding seems particularly interesting in light of Dr. Hist.'s previous statement about the new opportunities offered by English in his field (extract 19).

In regard to this reform of 1998, as well as in other ways, both doctors view themselves as belonging to a generation that entered academia at the intersection between the new and the old systems. Dr. Psych. formulates this in the following way:

> *Dr. Psych.: Round about the time when I began as a doctoral candidate, there were these old farts who had been going for 10 years. […]The system doesn't allow for that anymore, and that forces doctoral candidates today to be more operative and to think about how they use their time. […] It's much to do with the fact that we have these four years today, you cannot spend ten years thinking about the meaning of life. In some cases it led to nowhere, and in some cases it was very good that you could do it. I can see a value in the old system too, if the right person wonders about the meaning of life he might reach something great, if only he is given enough time. But then you get all these who do not use the time, so it's all difficult. But the way things are today, we cannot decide how much time we want […] so thereby, the system causes a performance hype.* (Extract 29)

Thus, Dr. Psych. holds that in some sense, the psychological field is characterized by performance hype, which, arguably, is particularly salient among doctoral candidates. It is, however, in place among other groups of staff, too, and while Dr. Psych. partly sees it as being a part of the game, he is also critical of some elements of the new managerial techniques being used to evaluate performance.

> *Dr. Psych.: I don't like it at all. I think that the quantification that is going on has nothing to do with good research. Take for example the impact indexes we use to evaluate individual researchers. They weren't even developed for this purpose, they were developed as an instrument for libraries to decide which journals they should pay big money for. And then somewhere along the way they began being used to assess individual researchers. And I think that this leads to, and this debate is huge, an unfortunate quantification of research that everyone appears to be doing, since, finally, you have this easier way of assessing differences between individuals. We've been doing this for ages within the university world, like […] this person is skilled, this person isn't, this person is a second-rater, whatever. All of a sudden we can quantify this. The teaching part is remarkably difficult to quantify, it was always easier to quantify the scientific part, that is, you can count articles, but now we can quantify this further.* (Extract 30)

Notably, Dr. Psych. sees the quantification of research performance at the individual level as running counter to his conception of good research. This indicates that psychologists, even though they publish in accordance with the will and aspirations of performance-based policy, do not have happy-go-lucky sentiments toward contemporary changes linked to performance-based funding in the field. Even so, the quantification of research is a much more recent development than the prevalence of English as a publishing language in the field, and, therefore, they are not seen as being intrinsically linked in Dr. Psych.'s account. The generation gap is evident in history, too, but here the link between quantification of research performance and English is more saliently manifested. Here, as Dr. Hist. holds, agents can compete by publishing chiefly in English (extract 19). To newcomers here, publishing in English thereby creates a rupture with the principles of the old scientific order of the historical field.

> *Dr. Hist.: Those who defended their theses from the mid-90s to 2005, 2006, 2007 or something like that, I think we belong to an in-between generation. We're somewhat fostered in the old spirit, you were a doctoral student for many years, the thick book was all that counted, there were nationally formulated research questions and you were primarily oriented toward the national research scene. But I think of those coming after us, who have been accepted to the new doctoral education program where the thesis must be finished in four years. Competition is fierce already in getting accepted for the program, and where candidates are immediately brought under the pressure of being competitive internationally. There's some difference there. As for me, I was fostered in the old spirit but landed in the new world.* (Extract 31)

As illustrated by this extract, Dr. Hist. reports having been socialized in a social world that does not quite match the state of the field as synchronically perceived, that is, where he 'landed.' As for his dispositions to historical research, he reveals what Bourdieu (2000, 160) refers to as a 'destabilized habitus, torn by contradiction and internal division, generating suffering.'

> *Dr. Hist.: You feel some kind of pressure, you don't really know where it comes from, who is behind it, what the purpose is. And you feel that you're being uprooted somewhat.* (Extract 32)

Dr. Hist. sees no other option than to adapt to the forces that are currently dictating the terms of his field, in spite of the fact that these forces are engendered field-externally. Hammarfelt and de Rijcke's (2014) study on publication strategies in Swedish academia similarly reveals young researchers being particularly sensitive to outside pressures as they compete for positions and resources. While several respondents in their study held epistemic and disciplinary factors to be the most important in their choice of publication channel, their position-takings also revealed an ostensible 'generation gap': 'The younger scholars seemed slightly more inclined to follow externally driven publishing strategies (i.e. focus on international peer-reviewed journals)' (p. 11). Investigating the increase in publication productivity in Norwegian academia, Kyvik and Aksnes (2015) show that, while scholars of all generations seem to have increased their publishing activity over time, new generations of academic staff perform at a higher level. These authors point to a set of partial explanations to account for this fact, including increased research collaboration and the introduction of incentive and reward systems. Dr. Hist.'s viewpoint seems to support this explanation.

> *Dr. Hist.: I'd say it is an absolute effect of the bibliometric. At least the way things are currently set up, you almost get a distinction: that which is written in Swedish is per definition popular science and that which is in English is science. That will be the effect if a Swedish monograph at a Swedish publisher results in 0 points, which is the case at the moment, and an English article of varying quality results in 1 or 2 points or whatever it might be. The long-term effect of that will be that Swedish becomes a mediating language, while English becomes the scientific language. If the system conforms perfectly to these new guidelines, I can see no other consequences.* (Extract 33)

Thus, in the historical field, as elsewhere, the pursuit of scientific authority is currently working in favor of English. It should be noted that English is not adopted into Dr. Hist.'s publishing practices in order to maximize share profit, but rather as a way of staying relevant in a changing field where there is currently a risk of being 'pushed out' (extract 18). Clearly, then, this is a matter of bread and butter; after all, people need an income by means of which other forms of capital can be traded, and this is so irrespective of other concerns of valorization. Hence, as Bertolt Brecht has pinpointed in an oft-quoted phrase, 'Erst kommt das Fressen, dann kommt die Moral' – first the food, then the morals. Relatedly, Dr. Hist. does not think highly of the investment strategy he partly pursues, not least since the bibliometric trend that facilitates his entering of the field undermines the position of Swedish in the field.

8 Discussion: two axes of comparisons

As this study has sought to show, fields are historical, and understanding their present appearances therefore entails understanding their past. In light of the interview accounts presented in the previous section, comparisons across different axes hold relevance for this study. In what follows, I shall discuss these in respect to lines of convergence and divergence between the fields as well as between positions within each field.

Comparisons between the fields: First, there are disciplinary differences, where scholars of the two fields seem endowed with differing views of the stakes and obligations of their fields in relation to their social *milieux*. In history, to begin, the line between an intra-academic and extra-academic audience is often thin. This trait of the field can be understood as being closely interlinked to the field's perception of the use of the knowledge it produces (e.g., Liedman 1983). As revealed in the historical analysis of the field, one outcome of its struggles for professionalization was to abandon the task of producing ideologically usable knowledge in favor of other forms of national obligations, more fact-based and attuned to the needs of the state, as such needs appeared from the late 19th century and onward (Jarrick 2012). In time, this fact-centered approach came to serve as the key weapon in defending the autonomy of the field in respect to the occurrence of non-professionals who have sought to produce historical knowledge, and in relation to whom the field was – and is – forced to position itself as a social authority (Bourdieu 1991, 7). As a consequence, the audience of the field, its field of consumption, was expanded so as to also encompass the general public at the society pole, more interested in reading work in Swedish than journal articles in English. This fact has cemented a tradition already in place, namely, to adopt national publishing strategies, often aimed toward book markets available to a broad readership. The problem thereof, as Åmark (1993, 273–274) recognized some two decades ago, is that this form of knowledge dissemination is generally devalued in the university field at large.

Clearly, psychology differs in this respect. The contemporary psychological field exhibits a higher degree of unity 'founded on successful investment in the activity of research alone' (Bourdieu 1988, 74). At stake here is the capital of strictly scientific authority, where researchers, as knowledge producers, 'have their rivals as their only possible consumers' (Bourdieu 1991, 15), and, linked to that, well-established conventions of publishing (e.g., Sörlin 1994). In this field, scholars typically publish short co-authored journal articles in English. However, the historicist approach opted for in this study brings to the fore that this unity does not pertain to the nature of psychological knowledge production; rather, it has come into place by virtue of historical labor and struggle. Up until the middle of the 20th century, Swedish psychological research was to a great extent nationally embedded, after which a specialized research-centered approach geared toward transnational publishing gained the upper hand. This latter development can be seen as the result of scholars with a particular habitus gaining prominent positions in the founding of the field, which occurred in tune with the emerging modern welfare state and its social engineering (e.g., Nilsson & Öhngren 1991, 20). In the mid-20th century, field-founders began orienting their practices to the new global hub of psychological knowledge production: the U.S. In so doing, the field also managed to assert difference and so break loose from its previous organizational embedding, which had stalled the development of psychology as an autonomous field.

Following Gouldner (1957), we may refer to this type of emergent psychology habitus as 'cosmopolitan', that is, scholars who seek to engage in intellectual dialogue with colleagues located elsewhere, with a similar type of 'outer reference group orientation' (p. 290). In history, by contrast, scholars have engaged in struggles over other forms of value, and, because of a strong sense of having various forms of obligations to Sweden, cosmopolitan dispositions have remained largely aloof. The field has instead housed *docile* dispositions, inclined toward *strategies of succession* (Bourdieu 1991, 17), where orientations to the national paradigm have offered safe investments and predictable careers. This is so in spite of the fact that representatives of the marginalized international orientation to historical research in Sweden have recurrently pointed to a general lack of interest among Swedish historians to engage in historical questions of global concern (e.g., Eklöf Amirell 2006a). Instead, as Österberg (1993, 281, my translation) has argued, Swedish historians in the main are – and have long been – 'truffle seekers rather than parachutists.' Using this image, Österberg points to a number of traits in historical research, such as the strong preoccupation with detailed empirical accounts firmly situated within the national context, at the expense of broad accounts that seek to synthesize global issues across national boundaries.

It may be argued, then, that the field of history is currently undergoing changes that in some respects impacted the field of psychology some 60 years ago. However, as a parallel across time, this comparison does not hold entirely. The reorientation to transnational publishing in psychology arose from struggles within the field, whereas the current transition of history toward transnational marketplaces is caught up in discourses of internationalization where the state has stakes and intervenes more actively through the adaption of various policies (Lucas 2006). Here, increased competitiveness between states and their universities yields demand for accountability and productivity that seem to be at odds with the values within the field where a profoundly different notion of research usability has long prevailed. In different ways, thus, the state's visions intersect with the values and practices already in place in the fields. In the field of psychology, the powers that are in force are 'in line with external power' (Bourdieu 1991, 17) – viz. contemporary currents of performance-based policy. Accordingly, the historical field is facing an obstacle course riddled by contemporary ways of conceptualizing accountability, productivity, and research usability within the university field at large.

One outcome of this process – or so this study argues – is that the field of history demonstrates a discrepancy between what is formally valued and a kind of local economy of valorization of scientific action. For this reason, it is a complicated matter to assess the extent to which the use of English and the international profile it bargains for is a form of symbolic capital in the historical field, since it is mostly in relation to one symbolic economy that it is perceived and recognized as legitimate (Bourdieu 1989, 17). In psychology, by contrast, this discrepancy is nowhere to be found; it seems rather to be the case that the formal value aligned with an economy of symbolic value is already in place. This is to say that the psychological field does not seem to embody much tension that can be explained by reference to external pressures challenging the value economy previously in place, that is, prior to the launch of performance-driven demands. In fact, one could argue that the imperatives to publishing that are currently imposed upon other fields use psychology and similar fields as their blueprint. Arguably, then, they are imposed to 'encourage the less prestigious fields to imitate the procedures and approaches – at least superficially – of the more central ones' (Whitley 2007, 23). By this logic, psychologists behave academically, as it were, in a way that matches the criteria currently celebrated within the university field at large. Stating this is not to suggest that agents of the psychological field are altogether pleased with the strategies currently impinging the research political landscape. For example, the gearing toward individual measurement of success, and the performance hype it brings with it, is often seen as damaging to research practice (cf. extract 30). What it does suggest, however, is that present currents in the university field have a much more profound impact on the publishing practices of history than on those of psychology.

Comparisons within the fields: Second, there are differences within the two fields, in particular between dominant agents and newcomers. In relation to the framework employed here, it was noted that the initiative to change in fields falls to the newcomers, who are least endowed with capital (Bourdieu 1983, 338). In acting, they stand at the junction of either investing in goods that are currently highly valued, or, in goods whose value is likely to increase in the future. The latter strategy raises the stakes, because it pertains to subversion strategies – 'infinitely more costly and more hazardous investments which will not bring them profits accruing to the holders of the monopoly of scientific legitimacy unless they can achieve a complete redefinition of the principles legitimating domination' (Bourdieu 1975, 30). In the analysis, this difference generally appears to be less salient compared to interdisciplinary differences. In psychology, in particular, the study was not able to identify any clearly viable pathways through which newcomers can currently develop subversive strategies. To the extent these do exist, which is quite likely the case, they unfold over issues such as methodological or theoretical inventions; at any rate, they do not currently seem to unfold over publishing practices and the languages used in these. This observation seems to relate to the historical argument invoked above, that is, that English began serving as a resource for transnational communication in psychology publishing about half a century ago. While the options rendered significant in fields tend to change over time, since then, no other language has managed to challenge the privilege of English in the global marketplace of scholarly exchange relevant to this field. Consequently, Dr. Psych.'s investment strategies in publishing do not deviate much from those of Prof. Psych.

As for the field of history, however, this study argues that dispositions to scientific practice are currently on the move, in a direction that is altering the practical sense of historians linked to what they do – and what they should do. Both Prof. Hist. and Dr. Hist. appear to be in agreement that the field is currently in transition between two different approaches to scientific practice and that this transformation unfolds over publishing and the languages used there. This transition is expressed as a sense that the value economy traditionally upheld in the field is being challenged by the forces currently in place in the overarching university field, which pressures and divides the historical field further. Scholars in the historical field nowadays have the possibility of reorienting their strategies toward transnational publishing markets as a new way of competing in the field. 'The new scientific regime', as it were, 'redistributes the meanings and values associated with the various scientific choices by imposing new norms of interpretation and new categories of perception and of appreciation of importance' (Bourdieu 1991, 14). Newcomers seem particularly sensitive to these transformations, since it is in their interest to assert their difference, so as to yield distinction and recognition (Bourdieu 1989, 338). These are agents who benefit from change and who have the possibility of making use of the broader discourses of internationalization that currently loom in the field at large. To them, English serves as a resource for advancing in the field along a relatively untried route, which also encompasses the abandoning of well-established investment strategies, most significantly, the Swedish-language monograph. As such, publishing in English is also '*a strategy of subversion* aimed against the established scientific order of the field, and through it, against the social order with which this scientific order is bound up' (Bourdieu 1991, 17). On the one hand, the chances of yielding a greater profit increase by investing differently. On the other hand, subversion strategies come with a substantial risk, particularly in cases where newcomers are assessed by dominant agents who seek to maintain the symbolic order and are disposed to hinder attempts to "'leap-frog' over career paths by offering a different form of "production", more suited to the new "scientificity"' (Grenfell 2007, 126). Writing monographs in the historical field still engenders an ample measure of symbolic capital within the field's more local value economy. This fact, one that all newcomers must relate to, seems particularly pertinent when agents are evaluated by other historians, and when the values of the field set the benchmark.

9 Concluding remarks

Academia – or the university field – can be grasped as a site of struggle between different ideational and managerial structures (Lucas 2006, 75). As this study has served to illustrate, one of the stakes of such struggles pertains to the question of language in publishing. It may be argued that the practice of publishing is where the backwashes of the profound processes of denationalization of science (Crawford et al. 1993b) are manifested most tangibly. As the historical analysis has shown, the term 'denationalization' seems apt to invoke here, accentuating as it does that historically, science has also undergone overt processes of nationalization (e.g., Jordanova 1996; Sörlin 1996). Science, thus, has long been an object of struggle caught between currents of nationalism and internationalism (e.g., Crawford 1992; Schroeder-Gudehus 1990), and this struggle seems to have increased in speed, force, and intensity through modern-day transitions of universities into global marketplaces (e.g., Bok 2003; Kauppi & Erkkilä 2011; Kennedy 2015). Using publishing language as the focal point, we see that the Swedish state seems ambivalent to these developments. The state, holds Bourdieu (2014, 345), grants power over other kinds of capital, and so determines their relative hierarchy of legitimization. In respect to the university field, interests vested in the politics of research clash with interests vested in the politics of language. On the one hand, the state appears to be well on the way to achieving its explicit goal to 'increase the activity on the international publishing market' (*Resurser för kvalitet* 2007, 418, my translation). On the other hand, however, these goals seem to be at odds with other interests anchored in the state, such as the commitment to reward the presentation of scientific results in Swedish, and thereby uphold the parallel use of Swedish and English in publishing (*Declaration on a Nordic Language Policy* 2006).

This study has sought to account for language choice in publishing in relational terms, that is, as a form of practice that is borne out of a relationship between disciplines and researchers, fields and habitus. Drawing on these concepts, the study has pried into the publishing practices of history and psychology. If we are to believe Bourdieu, stakes and interests are specific to fields, and therefore, you cannot make a historian compete for the prizes that interest a psychologist (Bourdieu 1993a, 72). Yet, it seems as though the state and its policymakers are willing to give it a try, and, as a consequence, the historical field in particular seems currently to be undergoing change that is altering agents' dispositions and practices. In the two fields, there are different stakes attached to the transformation of academia *in relation to* language. As a disciplinary field, history exhibits a form of autonomy that is sensitive to contemporary transformations of the university field, not least of all insofar as its field of consumption is much wider than the field of production (cf. Broady 1991, 270ff.). In all of its complexity, language plays a part here, and, therefore, the disconnect between publishing practices and habitually refined language practices signifies a transition that poses a significant hardship to overcome. In history, it lies within the core objective of the field to vivify its object of study through language. Consequently, Swedish, and the field-specific styles and devices it comprises, signifies a value in its own right, which agents of the field are reluctant to abandon. Pursuing an international research agenda is to move into a space where, bluntly put, Swedish is worth noting (e.g., Bourdieu 1977). Some historians perceive English as an instrument for the production of theoretical accounts, often brief and dressed in colorless prose, which reduces the width of the field's readership (cf. extracts 6, 9, and 24). As linguistic capital, Swedish is a pivotal asset for achieving the objectives at stake – without Swedish, historians are dispossessed from the instrument necessary for the reproduction of the market in relation to which their value as cultural producers is set (Bourdieu 1977, 651f.). By contrast, the encounter with the neoliberal vision of recent research policy has not perturbed the psychological field in any comparable way – much owing to the form of autonomy forged here. In this field, prior to recent transformations in research policy, an external outlook was already in place, and so was the ensuing praxis of producing objects of knowledge *fashioned* as being to a lesser extent context dependent. These objects – *texts* – thus travel lightly, as it were, across national boundaries and thereby align easily with the linguistic market conditions of the contemporary academic space where research results are marketed.

In a given field, the struggle between agents – above all, amid newcomers and the dominant – will always be present, but the means that can be utilized in these struggles are at variance both between and within fields at different points in time. Fields are thereby constantly being restructured. In the development of the psychological field since the post-war era, the means for achieving distinction and recognition appear to have remained fairly unchanged, at least on the battlefield of language. In this study, the field of history stands out in this regard, in that English has come to be a weapon in the struggles of the disciplinary field coterminous with the marketization and internationalization of the university field at large. Some agents, mostly, although not exclusively, new entrants to the field, buy in to this change, that is, they import its principles into their own investment strategies. In other words, it is a case of changing dispositions and practices (Lucas 2006). Young historians seem to hold that producing a Swedish-language monograph within the timeframe that is now offered is a poor strategy for gaining a profitable position in the field they seek to enter. Instead, the compilation thesis or monographs in English stand out as more compelling investments. In opting for such strategies, they can draw support from contemporary transformations of the field, and in so doing, English becomes tangled up in 'the strategies whereby the occupants of these positions seek, individually or collectively to safeguard or improve their position, and to impose the principle of hierarchization most favorable to their own products' (Wacquant 1989a, 40). Here, the study points to the emergence of a *new scientific habitus* – new as in newly developed in relation to the time-honored values and logic of practice of the field. Other agents, mostly but not exclusively holders of dominant positions, implement strategies to defend the 'hierarchies of importances' that have been traditionally upheld (Bourdieu 2004, 64). Unlike the newcomers, who benefit from change, the interests of dominant agents are served by maintaining the status quo in the distribution of capital (Williams 2010, 85). In so doing, they often find themselves defending a market in which they have invested their products (e.g., Bourdieu 1977).

In closing, what this study has served to highlight is that publishing practices and the languages to pursue them cannot be grasped by focusing only on the choices of individuals. Neither, however, can these dynamics be grasped by focusing only on the regulation of external forces. Following Bourdieu (1990, 64), we can then say that publishing strategies are the products, 'not of obedience to a rule but a feel for the game which leads people to "choose" the best match possible given the game they have at their disposal.' In this sense, the choice of publishing language is to be seen as a social strategy, ultimately because

> [t]here is no scientific choice – choice of area of research, choice of methods, choice of publication outlet […] – that does not constitute, in

> one or other of its aspects, a social strategy of investment aimed at maximizing the specific profit, inseparably political and scientific, provided by the field, and that could not be understood as a product of the relation between a position in the field and the dispositions (habitus) of its occupant. (Bourdieu 1991, 9–10)

From this premise, it follows that a comprehensive understanding of language-in-publishing entails taking into consideration the specific spaces of engagement in which researchers act. Yet, these spaces – fields – must be treated as having been shaped by history; the appearance of a field at a given point of time is in fact the result of the outcome of previous struggles, hard-fought confrontation, and showdown (cf. Broady 1991, 425). Disciplinary differences as synchronically perceived are thus not inherent qualities but the outcome of historical struggles. Recognizing this fact pinpoints that these universes of options are subject to change, which is to say that fields unfold over time as spaces of possibilities that are rendered significant by struggles in and beyond their confines (e.g., Bourdieu 1983, 344). Fields, after all, are sites for the struggle over resources and the value of these resources. Fields are social spaces 'within which agents confront each other, with differentiated means and ends according to their position in the structure of the field of forces, thus contributing to conserving or transforming its structure' (Bourdieu 1998, 32). This fact, I would argue, is the key insight that a relational approach brings to the fore. In academia, agents 'do not just hold fixed positions but have to defend and negotiate the terms of their positions against an ever-changing and fluid landscape' (Lucas 2006, 71). This fact, I argue, explains why the discourse of negative appraisal is commonly projected onto newcomers who invest differently. The capital of disciplinary fields is a capital of recognition, and as fields change, so do the principles of such recognition (Hilgers & Mangez 2015, 6). As this study has shown, this relational fact also applies to publishing language, as old field histories and new scientific habitus meet.

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Notes

[1] I take this term from Loïc Wacquant, as used in the workshop *Thick Construction: Engaging Bourdieu's Theories in Research Practice*, Uppsala University, June 11, 2015. The term is meant to signal kinship to the Geertzian notion of 'thick description' but with an eye toward emphasizing that the 'objects of knowledge are *constructed*, and not passively recorded' (Wacquant 1989a, 42; also Bourdieu 1990b, 52).

[2] Doctoral theses are interesting in this respect, since, *per se* they are the products of agents who seek to enter the field. In respect to publishing language, as well as other domains, newcomers must remain sensitive to currents and options offered by their fields. In addition, they have thesis advisors who are typically, albeit not always, dominant agents of the field and, by virtue of this fact, act as gatekeepers of their fields. It can be noted, though, that the languages used in theses often reflect prevailing trends and traditions with some five years delay, i.e., the time it takes from the beginning to the end of a doctoral project (Brissman 2010, 400).

[3] The selection of informants was made on the basis of their positions in academia: both in respect to disciplinary position (historians and psychologists, respectively) and in relation to their capital possession or position in the hierarchy of each disciplinary field (professors and doctors, respectively). Agents of both genders were asked to participate; yet, as it turned out, the informants who agreed to be interviewed from the fields reported on in this study were all male academics. Interviews with female researchers were carried out, but either as representatives of fields that were eventually not included in this study, or on topics reported on elsewhere (e.g., Salö 2015). In respect to the findings of this study, I do not know what this bias entails concerning the positionality of the informants in relation to the accounts they produce. However, in light of the discussion on equality issues in academia, this circumstance could be seen as problematic in that it reproduces gendered academic hierarchies (see Reay 2004).

[4] These interviews were carried out in Swedish. All data extracts presented in section 7 were translated to English by the author of this study.

[5] It follows from this fact that there exist degrees of dominance as well as of 'newcomerness'; for example, doctoral candidates are more junior than doctors, etc., at least in respect to their formal rank in the hierarchies. In point of fact, the interviewees treated as newcomers here are fairly established agents from the point of view of doctoral candidates and students.

[6] The task of mapping the university field empirically is beyond the scope of this study. For future analysts engaging with these questions, it might be fruitful to thoroughly account for the impact on research practices from commercial interests of publishing houses and the research evaluation industry. Consequently, it must be stated that this study does not comprise field studies in any rigorous sense; such an undertaking would call for a different methodological approach (see e.g., Bourdieu 1993a, 72–77). For example, the demographics of the two disciplinary fields would require more attention, since, for example, the two fields studied are likely to exhibit different structures of available career paths in and out of academia.

[7] It should be noted that the academic performances of this era were quite different from those currently exercised. For example, 'dissertation' refers both to short academic texts and to the oral practices similar to the present-day thesis defense, demonstrating oral eloquence (Lindberg 2004). Moreover, scholars were expected to engage in this practice on a regular basis, meaning that a single individual is often the author of several dissertations. Around the mid-19th century, these procedures were changed into a system more comparable to that presently in place; the doctoral thesis in the present-day sense, for example, is an invention of this time (see e.g., Lindberg 1984; Tengström 1970).

[8] 'Internationally oriented' is here to be understood as theses dealing with topics residing beyond the national context, and is thus not merely a question of publishing language.

[9] Notably, there were, in fact, strong trans-Atlantic connections in place elsewhere, for example, in engineering (Brissman 2010, 59).

[10] http://psy.gu.se/forskning/avhandlingar (accessed Jan. 26, 16).
[11] Searches were made in the database PsychLib, hosted by the Department of Psychology, Stockholm University: URL: http://fmp.psychology.su.se/FMPro?-db=biblioteket&-lay=webb&-format=uppssok.html&-view.
[12] URL: http://www.psyk.uu.se/forskning/avhandlingar/ (accessed Jan. 26, 16).
[13] This policy is currently under reevaluation, so as to also encompass qualitative assessment of expert panels (*Forskning och innovation* 2012; see Romnus 2015). Noteworthy, the model that is currently being developed by the Swedish Research Council seems to be inspired by the British *Research Assessment Exercises*, RAE (Nelhans & Eklund 2015, 19; see Lucas 2006 for a scrutiny of RAE).
[14] The Ledien Manifesto is an initiative to support researchers and policymakers in the implementation of procedures of research evaluation in a way that increases research quality without damaging the scientific system See URL: http://www.leidenmanifesto.org/. Among other things, the manifesto stresses the need to combine quantitative measures with expert assessments, and to protect locally relevant research not published in English-language journals (Hicks et al. 2015, 430).
[15] At Luleå University of Technology, for instance, departments receive SEK 70 000 [i.e., around 8 000 U.S dollars] as monetary compensation for each journal article published in level 2 journals indexed according to the Norwegian list or, alternatively, in a journal or book series indexed in the Web of Science. Lower-ranked publications are awarded less. URL: http://www.ltu.se/ltu/lib/Publicering/Publiceringsstod?l=en. Accessed January 15, 2016.
[16] Phrasing as used in the advice given by Stockholm University. URL: http://www.sub.-su.se/home/publish/bibliometrics/advice-on-how-to-achieve-good-bibliometric-results/. Accessed Jan. 15, 16. See also Kronman (2011) and Eliasson (2011).
[17] Issues pertaining to research policy and the future of the humanities are recurrently debated in a wide range of forums (see Sandström 2006 for an overview). See the special issues of the periodicals *Kulturella Perspektiv* 2008 (2) and 2013 (1); *Axess* 2006 (7); and *Glänta* 2005(1-2). The debate was particularly heated throughout 2005 as it unfolded in Sweden's major newspapers, such as *Dagens Nyheter*, *Svenska Dagbladet,* and *Sydsvenskan* (see e.g., Frängsmyr 2005; Sörlin 2005; also URL http://www.dn.se/stories/stories-kultur/humaniorans-framtid (assessed Jan. 19, 16).
[18] See *Historisk tidskrift* 2007-127(4), 'Beyond national history', and *Scandia* 2013-2, 'That is why we are historians' (my translation).
[19] Throughout the study, this abbreviation convention will be used to denote all interviewees: Prof. Psych. (professor of psychology), Dr. Hist. (doctor of history), and Dr. Psych. (doctor of psychology).
[20] Data were retained from Web of Science on Jan. 1, 14. The analysis focuses on collaboration in the research area of psychology 2012–2013, where at least one author is based in Sweden. The median was chosen as a measure to avoid the effects of extreme values from participating authors. The analysis is accredited to Per Ahlgren, bibliometrician at Stockholm University Library.
[21] The h-index is a measurement for the productivity and citation-based impact of individual researchers. In some fields and higher education systems, it is widely employed in promotion decisions, grant applications, etc. (see e.g., Hicks et al. 2015, 429f.).
[22] This account brings new light to the reported issue of language difficulties among historians (e.g., Kuteeva & McGrath 2014; also extract 9). What it points to is that such difficulties do not pertain to language skills only, but are rather intertwined with other forms of discursive practice where theorizing as a means of generalization holds a prominent position.
[23] It may be debated whether doctoral candidates in recent years have less effective time for finishing their thesis. What seems clear, however, is that they are expected to do so within a

more compressed time period than was the case before. Indeed, certain differences between disciplinary fields set aside, the graduation rate seems to have moved in a desirable direction on this point, as the completion rate has generally been higher in doctoral programs since the launch of the reform (Pettersson 2013).

[24] Using an online survey tool, a questionnaire was distributed to the directors of studies of eight Swedish departments housing research programs in history: Linköping University, Linnaeus University, Lund University, Örebro University, Stockholm University, Umeå University, University of Gothenburg, and Uppsala University. The survey, then, does not include theses in history produced within the contexts of other fields, subfields, or universities. Linköping University did not have any doctoral candidates, and therefore did not leave a response. The person responding to the survey at each university was asked to state, or estimate in cases of uncertainty, the formats and languages that doctoral theses were currently being written in. Most of the responses that were obtained were expressed in terms of absolute numbers. However, some of the responses were expressed in terms of spans (i.e., '2–4'), and one in terms of shares, which is why the numbers reported here should be seen as estimates.

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