Conflicting virtues of scholarship : moral economies in late nineteenth-century German Academia

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Conflicting Virtues of Scholarship

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Contents

Introduction

In 1883, a young student called Hugo Münsterberg attended a lecture by the Leipzig philosopher and experimental psychologist Wilhelm Wundt. He was deeply impressed and decided to continue his studies in his laboratory. Only two years later, the 22-year-old Münsterberg finished his doctorate under Wundt's supervision. His subsequent meteoric career would bring him to Harvard in the early 1890s. By the end of the decade he had even been elected president of the American Psychological Association.[1] Wundt had good reason to be very pleased with this ambitious and successful pupil.

In their correspondence, however, Wundt hardly expressed any satisfaction about Münsterberg's accomplishments. Instead, in 1890, their relationship seemed to be on the verge of breaking when the latter wrote his former teacher a letter full of heartfelt complaints.[2] He was particularly hurt by the many accounts he had received of conversations in which Wundt had called him 'ungrateful.' Even though he claimed that he could live with the idea that his former teacher had hardly any praise for his scholarly accomplishments, he said that he would 'lose [his] self-respect, when [Wundt's] accusation of ungratefulness would be warranted'. He drew attention to the many ways in which he continued to express his gratitude: 'I ostentatiously present myself as your student toward all your detractors; your framed picture is the only decoration on the walls of my laboratory; I sent you the first copy of all my books'. The letter did not, however, have its intended effect.

Although Wundt indignantly denied that he had ever accused his former student of ingratitude, his attitude was not conciliatory. He wrote that some of Münsterberg's recent publications had been 'rushed and not sufficiently matured'.[3] He then stated that when 'somebody wants to show his gratitude by his own will', he can only do this by working 'reliably, diligently and meticulously without caring about authorities or career'. By listing these requirements, Wundt implied that Münsterberg's attempts to express his gratitude had fallen flat; his rushed and immature work suggested a lack of reliability, diligence and meticulousness. As Wundt also added that he had shared his unfavourable judgements with mutual acquaintances, it is very unlikely that his words reassured Münsterberg.

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[1] On Münsterberg's life and career, see Keller, Phyllis, *States of belonging: German-American intellectuals and the First World War*, Harvard University Press, Cambridge (MA), 1979. 5–118.
[2] Hugo Münsterberg to Wilhelm Wundt, 10 November 1890, Universitätsachiv Leipzig (hereafter UAL), NA Wundt/III/701-800/764b/415-426. Unless otherwise stated all translations are my own.
[3] Wilhelm Wundt to Hugo Münsterberg, 12 November 1890, UAL, NA Wundt/III/701-800/765/427-438.

Over the following decades this awkward exchange of letters would haunt their relationship. Six years later, Münsterberg mentioned the 'rushed production' of his early career in a long and humble letter to his doctoral advisor.[4] Almost 10 years later, he would still bring up this 'rushed immaturity'.[5] He would also continue to assure Wundt of the sincerity of his gratitude. An 1896 letter was concluded with an apology for all the ways in which he had 'knowingly or unknowingly hurt or wronged' him as well as for all the things for which he had 'not sufficiently expressed his sincere gratitude'.[6] A few months later, he even asked Wundt to acknowledge him as his 'most grateful student'.[7] Almost a decade after that — at the opening of his brand new laboratory at Harvard — he stressed his thankfulness again: 'I just wanted to say […] that today I am profoundly aware of my dependence on and my gratitude for you and that I am […] guided by the desire that this workplace will be imbued with your spirit'.[8]

For the purposes of this study there is no reason to delve deeper into the merit of Münsterberg's early work or Wundt's teaching.[9] What is important to note, however, is the language of virtue they used to talk about both their relationship and their scholarship. It is repeatedly made clear that the relationship between a researcher and his teacher should be grounded in the virtue of gratitude and that good scholarship is the result of virtues, such as reliability, diligence and meticulousness. The way in which these different virtues of scholarship touch on each other is even more striking. Wundt's observations in particular suggest that gratitude cannot be considered in isolation from reliability, diligence and meticulousness. This study takes a closer look at the virtues that shaped scholarship in late 19th and early 20th century Germany with an emphasis on the continuous attempts to maintain a balance between the requirements of potentially conflicting virtues.

Before turning towards Wilhelmine Germany, this introduction first reflects on the way in which virtues are discussed in recent studies on the history of scholarship, in order to draw attention to what is still conspicuously lacking in this body of literature. It points out that insufficient attention has been paid to how virtues relate to and interact with each other. Next, attention is directed to the notion of a 'moral economy' of scholarship, an analytical framework that can be traced back to the work of Robert Kohler and Lorraine Daston, in the 1990s. I argue that it is highly suitable to

[4] Hugo Münsterberg to Wilhelm Wundt, 31 March 1896, UAL, NA Wundt/III/701-800/765f/501-512.
[5] Hugo Münsterberg to Wilhelm Wundt, 5 November 1905, UAL, NA Wundt/III/701-800/768a/607-622.
[6] Hugo Münsterberg to Wilhelm Wundt, 7 April 1896, UAL, NA Wundt/III/701-800/765g/513-528.
[7] Hugo Münsterberg to Wilhelm Wundt, 14 April 1896, UAL, NA Wundt/III/701-800/765h/529-532.
[8] Hugo Münsterberg to Wilhelm Wundt, 5 November 1905, UAL, NA Wundt/III/701-800/768a/607-622.
[9] For more details on Wundt's career, see Bringmann, Wolfgang G. and Ryan D. Tweney (eds.), *Wundt Studies: A Centennial Collection*, C.J. Hogrefe, Inc., Toronto, 1980; Rieber, Robert W. and David K. Robinson (eds.), *Wilhelm Wundt in History: The Making of a Scientific Psychology*, Springer Science + Business Media, New York (NY), 2001. Wundt has also written a very readable autobiography: Wundt, Wilhelm, *Erlebtes und Erkanntes*, Alfred Kröner, Stuttgart, 1920.

shed light on the intricate interplay between virtues — on the frictions between them as well as on the balance that scholars assumed to exist between virtues. I subsequently reflect on the opportunities offered by a cross-disciplinary approach to the history of scholarship. Next, I introduce the professional networks of scholars from three disciplines in late 19th and early 20th century Germany from which my case studies have been selected. These introductions are followed by a discussion of my primary sources and methodology. Here, I discuss the merits of a cultural history approach to the history of scholarship. This is especially relevant because this study draws heavily on the anthropological tradition of thick description of ideals, practices and everyday life. Finally, I outline the structure of this study, which is built around the various ways in which scholars can relate both to each other and to each other's work.

Virtues and scholarship

In a recent study, Steven Shapin observes that 20th century scholars tend to present the history of scholarship as an impersonal process that is propelled by 'rationally organised and regulated institutions'.[10] He regrets that such depictions conceal the fact that 'at least since the seventeenth century, familiar people and their virtues have *always* been pertinent to the making, maintenance, transmission and authority of knowledge'.[11] In recent years, however, an increasing number of researchers have paid attention to questions of virtue and vice in the history of scholarship. The growing interest in these issues can be observed in very different research programmes that deal with scholarly virtues for a variety of reasons.

A first group of scholars that should be mentioned here consists of those who are primarily interested in understanding scholarly trustworthiness and reliability. In the 1980s, Shapin was one of the first historians to emphasise the importance of attributions of virtue in settling questions of trust in scientific findings. Only someone who was widely known to be virtuous would be able to convince his peers of the truth and significance of his work. He would have to have a reputation for possessing both the virtues of accuracy and love of truth to convince others of the veracity of his new discoveries or novel insights.[12] Questions of trust not only arose among the early-modern gentlemen of science described by Shapin. Kasper Eskildsen has made similar observations about a very different group of scholars: those of 19th century German historians. He found that historical accounts that exhibited the typical properties of epistemic virtues, such as accuracy,

[10] Shapin, Steven, *The Scientific Life: A Moral History of a Late Modern Vocation*, University of Chicago Press, Chicago (IL), 2008. 3.
[11] Ibid., 4. Shapin's italics.
[12] Shapin, Steven, 'The House of Experiment in Seventeenth-Century England,' *Isis*, 79(3), 1988, 373–404. 397–398.

honesty and impartiality, were generally considered to be trustworthy. The virtues of these texts then testified to the virtues of the historians who explicitly and carefully referred to them.[13]

Conceptions of virtue have also played an important role in cultural approaches to the history of scholarship. Herman Paul, for example, has drawn attention to the relationship between scholarly virtues and middle-class values. He has argued that German historical studies in the 19th century were shaped by the fact that 'loyalty was a cardinal virtue' among their bourgeois practitioners.[14] Others have focused on virtues recognised among scholars rather than on the way in which the values of society at large have shaped conceptions of scholarly virtue. Jessica Wang has recently drawn attention to solitude and austerity as scientific virtues.[15] Gerald Holton has passionately praised the virtuousness and vital role of imagination in science.[16] Jo Tollebeek's study of the working life of the Belgian historian Paul Fredericq is one of the most detailed of the recent cultural histories of scholarship. Despite the fact that his book primarily deals with the everyday conduct of scholarly life, Tollebeek also acknowledges the pivotal role of epistemological, ethical and aesthetic considerations, as well as the shaping influence of ideology and emotions.[17]

A third group of people with an interest in questions of virtue and vice consists of scholars examining the historical development of the relationship between religion and science. Peter Harrison has, for example, investigated how the intellectual vice of curiosity was transformed into a virtue over time. He describes how curiosity had acquired a poor standing in the writings of the church fathers and how this reputation improved in the course of the 17th century.[18] Michael Heyd has looked into medical and theological critiques of religiously inspired enthusiasm during the same period.[19] Even though Sari Kivistö's study of the many vices of learning recognised at early-modern universities is not limited to an analysis of the relationship between religion and scholarship, she is interested in the way in which a common appeal to 'the importance of traditional moral and religious values' contributed to 'conflicting notions of knowledge and scholarly ethics'.[20]

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[13] Eskildsen, Kasper Risbjerg, 'Inventing the archive: Testimony and virtue in modern historiography,' *History of the Human Sciences*, 26(4), 2013, 8–26. 11.
[14] Paul, Herman, 'Germanic Loyalty in Nineteenth-Century Historical Studies: A Multi-Layered Virtue,' *História da Historiografia*, forthcoming.
[15] Wang, Jessica, 'Broken Symmetry': Physics, Aesthetics, and Moral Virtue in Nuclear Age America,' in: Jeroen van Dongen and Herman Paul (eds.), *Epistemic Virtues in the Sciences and the Humanities*, Springer, Cham, 2017, 27–47. 38.
[16] Holton, Gerald, *Einstein, history, and Other Passions: The Rebellion against Science at the End of the Twentieth Century*, Addison-Wesley, Reading (MA), 1996. Chapter 4.
[17] Tollebeek, Jo, *Fredericq & Zonen: Een antropologie van de moderne geschiedwetenschap*, Bert Bakker, Amsterdam, 2008. 24.
[18] Harrison, Peter, 'Curiosity, Forbidden Knowledge, and the Reformation of Natural Philosophy in Early Modern England,' *Isis*, 92(2), 2001, 265–290. 267 and 283.
[19] Heyd, Michael, *"Be Sober and Reasonable": The Critique of Enthusiasm in the Seventeenth and Early Eighteenth Centuries*, Brill, Leiden, 1995.
[20] Kivistö, Sari, *The Vices of Learning: Morality and Knowledge at Early Modern Universities*, Brill, Leiden, 2014. 13.

Virtue and vice are also discussed by historians of scholarship who aim to make cross-disciplinary comparisons.[21] In their introduction to a volume about epistemic virtues in scholarship Jeroen van Dongen and Herman Paul argue that a focus on epistemic virtues is promising, because it potentially contributes to a history of knowledge that goes beyond customary disciplinary horizons.[22] Their approach builds on a broader development in the study of scholarship in which cross-disciplinary comparisons have become increasingly common. Recently the argument in favour of comparative studies of the humanities and the sciences has also been convincingly made by Rens Bod.[23]

Finally, it is worth mentioning that the interest in scholarly virtues and vices has not been limited to historians. At least two philosophical approaches to these issues have been pursued in recent years. On the one hand some philosophers have worked on what is commonly called 'virtue epistemology'. Robert C. Roberts and W. Jay Wood have, for example, argued that 'in one way or another all virtues have a cognitive aspect'.[24] Linda Zagzebski's analyses start from the assumption that intellectual virtues are forms of moral virtue.[25] The other group of philosophically inclined authors who discuss scholarly virtues are primarily concerned with scientific research ethics. Some of them approach the theme historically, by showing how scientific research ethics emerged from a tradition of ethical reflection on the central virtues. Albert Jonsen's and Robert Baker's studies on the history of medical ethics in the United States are key examples of this approach.[26] Others argue more constructively in favour of a virtue ethical approach to scientific research ethics. Inspired by Alasdair MacIntyre, whose *After Virtue* shaped a wide range of subsequent debates, they often present virtue ethics as a remedy to the perceived limitations of protocolised types of ethics, such as those institutionalised in codes of conduct and ethical review boards.[27]

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[21] Paul, Herman, 'The Scholarly Self: Ideals of Intellectual Virtue in Nineteenth-Century Leiden, in: Bod, Rens, Jaap Maat and Thijs Weststeijn (eds.), *The Making of the Humanities, vol. II: From Early Modern to Modern Disciplines*, Amsterdam University Press, Amsterdam, 2012, 397–411. 397.

[22] Dongen, Jeroen van and Herman Paul, 'Introduction: Epistemic Virtues in the Sciences and the Humanities,' in: Dongen, Jeroen van and Herman Paul (eds.), *Epistemic Virtues in the Sciences and the Humanities*, Springer, Cham, 2017, 1–10. 5.

[23] Bod, Rens, 'A Comparative Framework for Studying the Histories of the Humanities and Science,' *Isis*, 106(2), 2015, 367–377.

[24] Roberts, Robert C. and W. Jay Wood, *Intellectual Virtues: An Essay in Regulative Epistemology*, Clarendon Press, Oxford, 2007. 59.

[25] Zagzebski, Linda Trinkaus, *Virtues of the mind: An inquiry into the nature of virtue and the ethical foundations of knowledge*, Cambridge University Press, Cambridge, 1996. xiv.

[26] Jonsen, Albert R., *The Birth of Bioethics*, Oxford University Press, Oxford, 1998; Baker, Robert, Before Bioethics: *A History of American Medical Ethics from the Colonial Period to the Bioethics Revolution*, Oxford University Press, Oxford, 2013.

[27] For example, see Pennock, Robert T. and Michael O'Rourke, 'Developing a Scientific Virtue-Based Approach to Science Ethics Training, *Science and Engineering Ethics*, 23(1), 2017, 243–262.

Conflicting virtues and moral economies

The wide range of studies on scholarly virtues allows us to draw up an extensive inventory of virtues that have been associated with good scholarship, at different times and places. Such a list not only underlines the wide variety in studies about this topic, but also draws attention to a significant common feature — all these studies highlight one virtue at a time. Sometimes, this is the obvious result of the author's deliberate decision to focus on just one virtue, such as Harrison's analysis of curiosity or Paul's study of loyalty.[28] The extensive discussion on objectivity by Lorraine Daston and Peter Galison and the volume on impartiality edited by Kathryn Murphy and Anita Traninger fit this mould, as well.[29] Some authors, on the other hand, discuss multiple virtues and vices. They usually only address them one by one, however, without examining whether someone's assessment of or compliance with one virtue touches on his or her judgement of other virtues.

This widespread emphasis on separate virtues fails to shed light on the way in which these individual virtues relate to each other. These relationships can be intricate; there is no reason to assume that there is some kind of natural harmony among all virtues. The example in the opening paragraphs already illustrates that this relationship is more complex. Münsterberg could only defend the sincerity of his gratitude by separating this virtue from the equally important virtues of reliability, diligence and meticulousness. Wundt, on the other hand, called Münsterberg's gratitude into question because he believed that it depended on these other virtues. This is not the only conceivable complex relationship between virtues. Virtues can also be experienced as being in conflict with each other. It is easy, for instance, to imagine a clash between the commitments to loyalty and to scholarly solitude, or a conflict between untamed imagination and careful dedication to thoroughness and accuracy. What is more, disagreement about the relationship between various virtues of scholarship might also be a reflection of the often-complex relationships between individual scholars. This is tellingly exemplified by Münsterberg's cumbersome relationship with his *Doktorvater*.

Historians of science have recognised the complications arising from the variety in the ways in which virtues can relate to each other. Few, however, have tried to develop conceptual tools to address this. In recognition of the complexity of a plurality of virtues, Herman Paul proposes to look at scholarly personae as templates of scholarship that can be characterized as 'constellations

[28] See footnotes 12 and 16.
[29] Daston, Lorraine and Peter Galison, *Objectivity*, Zone Books, New York (NY), 2007; Murphy, Kathryn and Anita Traninger (eds.), *The Emergence of Impartiality*, Brill, Leiden, 2014.

of commitments to specific goods'.[30] This notion of virtues as part of a constellation allows for an evaluation of the significance of individual virtues in the light of others. Moreover, this conception also suggests that the relative importance of individual virtues is not set in stone and can therefore change, over time.[31] However, this precisely reveals a limitation of the persona approach as well. As Paul himself admits, 'the prism of scholarly personae encourages historians to acknowledge *synchronic variety'* in how scholars define standards of virtue.[32] Yet, to what extent was this variety made possible by shared horizons of expectation or what one might call shared rules of the game? If one ignores for a moment the sometimes heated debates over a scholar's 'first' or 'most important' virtue and looks at ordinary scholarly practices, such as collaborating with colleagues on a text edition, reviewing a dissertation turned into a monograph, or running a scholarly journal, to what extent were these practices regulated by standards of virtue? And how can we understand how virtues interact at this practical level, quite apart from how virtues were attributed to scholarly personae?

The questions this study seeks to answer, therefore, are: How did scholarly virtues relate to each other on the 'practical' level of day-to-day scholarly work? To what extent did these virtues correspond to unwritten rules or tacit assumptions on how to engage in scholarly work? To what extent and in what ways did these virtues come into conflict with the expectations raised by these rules and assumptions? How did scholars react to the possibility that virtues and the expectations they raised might come into conflict? This study aims to shed light on the complex relationships between various virtues in scholars' everyday working lives by portraying them as part of what modern-day authors, such as Robert Kohler and Lorraine Daston, described as a *moral economy of scholarship*.[33]

The popularity of this term can be traced back to E.P. Thompson's 1971 article 'The moral economy of the English crowd in the eighteenth century'.[34] According to Thompson, a moral economy is basically a 'consistent traditional view of social norms and obligations, of the proper economic functions of several parties within the community'.[35] In the ideological debates of the

[30] Paul, Herman, 'What is a Scholarly Persona? Ten Theses on Virtues, Skills, and Desires,' *History and Theory*, 53, 2014, 348–371. 364.
[31] These issues are also discussed in: Engberts, Christiaan and Herman Paul (eds.), *Scholarly Personae in the History of Orientalism, 1870–1930*, Brill, Leiden, 2019.
[32] Paul, Herman, 'Introduction: Scholarly Personae in the History of Orientalism, 1870–1930,' in: Engberts and Paul, *Scholarly Personae*, 1–16. 14. Paul's emphasis.
[33] See especially: Daston, Lorraine, 'The Moral Economy of Science,' *Osiris*, 10, 1995, 2–24 and Kohler, Robert E., *Lords of the Fly: Drosophila Genetics and the Experimental Life*, University of Chicago Press, Chicago (IL), 1994.
[34] Götz, Norbert, "Moral economy': its conceptual history and analytical prospects,' *Journal of Global Ethics*, 11(2), 2015, 147–162. 152.
[35] Thompson, Edward P., 'The moral economy of the English crowd in the eighteenth century,' *Past & Present*, 50(1), 1971, 76–136. 79.

Cold War era, the term was widely used by political and social scientists and historians who were attracted to its emphasis on 'pre- or non-market arrangements' and its assumed applicability to peasant and non-Western societies.[36] Not until from the late 1980s, the term also started to appear in studies about the history of science.

Although he did not use the term, Steven Shapin's studies on the culture of experimentalism in early modern England are among the earliest works on the moral economy of scholarship.[37] He describes how 'access to experimental venues' was shaped by a 'tacit system of recognitions, rights and expectations that operated in the wider society of gentlemen.[38] As mentioned above, the virtues most commonly associated with true gentlemen — especially a disposition to tell the truth and natural civility — were seen as warrants of trustworthiness.[39] The virtue of civility might have been even more important than expectations of truthfulness. Shapin goes as far as claiming that sociability, pliancy and politeness were the virtues that were the 'condition for the production of reliable knowledge'.[40] Thus, the moral economy of the early modern English experimentalist, above all, is pictured as rooted in long-standing gentlemanly values.

In the 1990s, Robert Kohler and Lorraine Daston reflected more explicitly on the moral economy of scholarship. Kohler's study of early 20th century fruit fly geneticists provides a detailed analysis of the virtues that shaped the collaborative efforts of this well-defined group of researchers.[41] This group was not only defined by its shared interests but also by temporal and spatial features. Kohler describes the assessments of virtue shared by a limited number of scholars at a limited number of interconnected laboratories developing specific forms of collaboration and a shared identity during a relatively short period of time. By thus limiting his scope, he is able to give an elaborate description of what he refers to as 'a moral ethos of cooperation and communality'.[42] Kohler's detailed and contextualising approach to moral economies is promising. His work, nonetheless, does not provide a sufficiently developed starting point for an investigation into the relationships between various virtues, as he did not look into in the relationship between potentially conflicting values, nor did he make an effort to define the term *moral economy*.

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[36] Götz, Norbert, "Moral economy'," 155. Probably the most famous application of the term to a community that was both peasant and non-Western is: Scott, James C., *The Moral Economy of the Peasant: Rebellion and Subsistence in Southeast Asia*, Yale University Press, New Haven (CT), 1976.
[37] Shapin, Steven, 'The House of Experiment'; Shapin, Steven, "'A Scholar and a Gentleman'": The Problematic Identity of the Scientific Practitioner in Early Modern England, *History of Science*, 29, 1991, 279–327.
[38] Shapin, Steven, 'The House of Experiment,' 389.
[39] Ibid., 397–398.
[40] Shapin, Steven, "'A Scholar and a Gentleman'," 297.
[41] Kohler, *Lords of the Fly*.
[42] Ibid., 92–93.

In this respect, Lorraine Daston's work is more promising because it actually provides a definition. In a pioneering article, she emphasises that, in studies of the history of scholarship, the word 'economy' does not refer to the 'money, markets, labor, production, and distribution of material resources, but rather to an organized system that displays certain regularities'.[43] She then goes on to define a moral economy as 'a web of affect-saturated values that stand and function in well-defined relationship to one another'.[44] She adds that this web is a 'balanced system of emotional forces, with equilibrium points and constraints'.[45] Following Daston, the term 'moral economy', as used in this study, does not refer to norms and regulation of the marketplace, but rather to the balance between various assessments of scholarly virtue.

This study, thus, explores the way in which the ever-changing relationship between various virtues produces a balanced system of equilibrium points and constraints. It is intended to present a valuable contribution to the existing literature on scholarly virtues and vices, as it goes beyond the common single-minded focus on individual virtues, by acknowledging and outlining the complex relationships between a variety of virtues. This dynamic understanding of the relationship between virtues was inspired by Paul's conception of constellations of commitments, though this study focuses more on shared moral horizons than on distinct scholarly personae. The study demonstrates that these constellations of commitments to various virtues amount to a moral economy of scholarship. This moral economy can be conceived as balanced, because it revolves around the assessments of the relationships between a limited number of virtues. However, a balanced system does not necessarily amount to an entirely static environment. Virtue assessments are subject to change, over time, and — more importantly in this study — different judgements can be made by scholars who work in different disciplines as well as by those who perform different roles, such as editor, reviewer, government adviser or amiable colleague.

A cross-disciplinary history of scholarship

This study takes a comparative approach to the history of scholarship. As mentioned above, one attractive aspect of an emphasis on virtue is the fact that it might contribute to a history of scholarship that transcends disciplinary boundaries.[46] This does not in any way imply that assessments of scholarly virtue are similar across disciplines. On the contrary, over the course of this study I took into account the influence of a variety of ideals, traditions, political influences and

[43] Daston, 'The Moral Economy of Science,' 4.
[44] Ibid.
[45] Ibid.
[46] See footnote 21.

economic incentives on various disciplines. I would even argue that the most characteristic qualities of virtue assessments in these disciplines only become visible in the light of such differences.

Until the late 20th century, such cross-disciplinary approaches to the history of scholarship were uncommon. In 1984, Richard Whitley observed that contemporary science studies had produced 'empirical studies of the emergence of new fields, of scientific controversies, and of the construction of scientific knowledge in particular circumstances as social phenomena'. He added, however, that these studies had not generated 'much comparative understanding of how different disciplines become established and develop in different ways in different circumstances'.[47] The lack of attempts to make a comparative analysis of fields of scholarship is especially striking when considering the extent to which the humanities have been integrated into the history of science. Rens Bod observed that the humanities are underrepresented in at least two ways.[48] On the one hand, they are largely neglected in wide-ranging histories of science, such as George Sarton's *Introduction to the History of Science* and Hans-Joachim Störig's *Kleine Weltgeschichte der Wissenschaft*. On the other hand, almost all histories of the humanities lack a comparative perspective, because they only deal with individual disciplines.

This does not mean that comprehensive or comparative approaches to the history of scholarship have never been pursued. Sarton, for example, explicitly stated his interest in philological, historiographical, juridical and sociological scholarship.[49] However, most humanities disciplines only received limited attention in his study.[50] The call for an integrated history of the sciences continued to be voiced after Sarton published his *Introduction*. Ziman, for instance, argued that '[...] to maintain […] an impassable divide between Science and the Humanities is to perpetrate a gross misunderstanding' and stated that, in many ways, the study of the latter 'is perfectly akin to the scientific study of electrons, molecules, cells, organisms or social systems'.[51] But, even though the acknowledgment of such similarities calls for comprehensive and comparative treatments of the sciences and the humanities, such studies remained rare, throughout the 20th century.

In the 21st century, the call for a cross-disciplinary — or even post-disciplinary — history of scholarship became louder. Lorraine Daston and Glenn Most make a passionate plea for 'broadening the subject matter of the history of science to include at least some of the history of

[47] Whitley, Richard, *The Intellectual and Social Organization of the Sciences*, Clarendon Press, Oxford, 1984. 5.
[48] Bod, Rens, *A New History of the Humanities: The Search for Principles and Patterns from Antiquity to the Present*, Oxford University Press, Oxford, 2013. 3–4.
[49] Sarton, George, *Introduction to the History of Science*, Volume 1, Robert E. Krieger, Williams and Wilkins, Baltimore (MD), 1931. 7–8.
[50] Bod, Rens, *A New History of the Humanities*, 3.
[51] Ziman, John M., *Public Knowledge: An Essay Concerning the Social Dimensions of Science*, Cambridge University Press, Cambridge, 1968. 20.

some of the humanities'.[52] They argue that especially philologists' efforts to 'minimize errors through systematic methods' allow for insightful comparison with the sciences.[53] In a similar vein, Bod states that a comparative framework will enable us to write a *longue durée* history of scholarship that will do justice to the many transfers of method that have taken place between disciplines.[54] He illustrates this by examining how grammar formalisms have shaped computer science and how philological stemmatic rules have been adopted by hereditary biologists. Finally, as mentioned above, Jeroen van Dongen and Herman Paul argue that one of the great promises of the cultural turn in the history of scholarship is its potential contribution to a history of knowledge that transcends traditional disciplinary boundaries.[55]

These advocates of a cross-disciplinary approach to the history of knowledge all emphasise its usefulness for the study of different features of scholarship. Bod argues that a comparison at the level of what he calls formalisms and rule systems is the most promising approach, because, only at this level, true equivalencies between disciplines might be found rather than mere analogies.[56] Daston and Most argue that 'a genuinely comparative framework that would examine the history of diverse intellectual traditions on an equal footing' should primarily look at practices.[57] Van Dongen and Paul, finally, emphasise the promise of looking at epistemic virtues, because they are 'often shared, transferred, traded, and borrowed across disciplinary boundaries'.[58]

These three different emphases are, however, not mutually exclusive. Daston and Most, for instance, explore 'key practices like error analysis'.[59] These practices also have formalistic and moral dimensions. They point out that there are formalistic similarities between astronomy and philology in the way in which errors are classified in both disciplines.[60] The moral dimension of these practices becomes apparent in their acknowledgement of the 'unwavering attentiveness and painstaking care' that was expected of both astronomers and philologists.[61] This recognition of the importance of care and attentiveness can be understood as an acknowledgement of the significance of epistemic virtues. Moreover, their acknowledgement of the interplay between virtue and

[52] Daston, Lorraine and Glenn W. Most, 'History of Science and History of Philologies,' *Isis*, 106(2), 2015, 378–390. 383.
[53] Ibid., 380.
[54] Bod, Rens, 'A Comparative Framework,' 367–377.
[55] See page 5 and Van Dongen and Paul, 'Introduction,' 5.
[56] Bod, Rens, 'A Comparative Framework,' 369.
[57] Daston, Lorraine and Glenn W. Most, 'History of Science and History of Philologies,' 389–390.
[58] Dongen, Jeroen van and Herman Paul, 'Introduction,' 5.
[59] Ibid., 381.
[60] Ibid., 380–381.
[61] Ibid. ,379.

practices is consistent with this study's attempts to look at scholarly virtues through the prism of practices that encourage judgement and sometimes — as a result — conflict, as well.

Three disciplines

To enable a cross-disciplinary approach, this study takes a closer look at scholars working in the disciplines of orientalism, experimental psychology and bacteriology. Today, these fields are considered part of the humanities, the social sciences and the sciences, respectively.[62] Modern-day classifications are not, however, always consistent with the way in which these disciplines were seen in the past. Even if 19th century orientalism, to some extent, could be characterised as the philology of ancient oriental languages, the study of Semitic languages stayed closely linked to Old Testament studies, which were taught at theological faculties.[63] At the same time, the term 'social sciences' was not used, as yet. Experimental psychology was still firmly rooted in the post-Kantian epistemological debates that shaped 19th century German philosophy.[64] One of experimental psychologists' major claims on philosophical innovation was the introduction of methods borrowed from physiology, a sub-field of medicine.[65] Bacteriology, finally, was a medical sub-discipline mostly focused on unresolved questions about the ultimate causes of disease.[66] Its development, however, owed a great deal, not only to the earlier efforts by experimental pathologists but also to the insights of 19th century botanists, such as Ernst Hallier and Ferdinand Cohn.[67]

The three disciplines discussed in this study were selected not only because they allow for an interesting cross-disciplinary overview of shared scholarly virtues. By the end of the 19th century, each discipline also showed various features that gave rise to debate about good scholarship. This

[62] A recent forum section in *History of Humanities* further explores the ever-shifting distinction between the sciences and the humanities; see Krämer, Fabian, 'Shifting Demarcations: An Introduction,' *History of Humanities*, 3(1), 2018, 5–14 and Bod, Rens, 'Has There Even Been a Divide? A *Longue Durée* Perspective, *History of Humanities*, 3(1), 2018, 15–25.
[63] Marchand, Suzanne L., *German Orientalism in the Age of Empire: Religion, Race, and Scholarship*, Cambridge University Press, Cambridge, 2009. 86. Mangold, Sabine, *Eine "weltbürgerliche" Wissenschaft — Die deutsche Orientalistik im 19. Jahrhundert*, Franz Steiner, Stuttgart, 2004. 59–64.
[64] Boring, Edwin G., *A History of Experimental Psychology*, second edition, Prentice-Hall, Englewood Cliffs (NJ), 1950. 246–250; Robinson, Daniel N., *An Intellectual History of Psychology*, revised edition, Macmillan, New York (NY), 1981. 325–326.
[65] Boring, Edwin G., *A History of Experimental Psychology*, 420–426.
[66] Hardy, Anne I. and Mikael Hård, 'Common Cause: Public Health and Bacteriology in Germany, 1870–1895,' *East Central Europe*, 40(3), 2013, 319–340. 320.
[67] Berger, Silvia, *Bakterien in Krieg und Frieden: Eine Geschichte der medizinische Bakteriologie in Deutschland 1890–1933*, Wallstein, Göttingen, 2009. 33–35.

ample room for disagreement encouraged scholars to reflect on their assessments of scholarly virtue, in different ways.

Orientalists who studied ancient languages realised that they often were building on unstable intellectual foundations that allowed for sharp differences of opinion. One critic describes them as 'often ruthless in denouncing each other's translations and editing decisions' and adds that '[...] rivalry and rancour have been powerful driving forces in the story of Orientalism'.[68] In addition, it was hard to find common ground between the proponents of a secularised approach to orientalist scholarship and the theologically inspired students of Semitic languages.[69] By the end of the 19th century, a new debate about research priorities emerged. Traditional orientalists, who largely limited themselves to the study of ancient languages and texts, were challenged by a new generation of scholars who showed an increasing interest in modern languages and contemporary culture. The *Seminar für Orientalisch Sprachen* (institute for oriental languages) was established in Berlin in 1887 to teach contemporary Asian languages.[70] In the early 20th century, two journals devoted to the modern Orient were founded, named *Der Islam* and *Die Welt des Islams*.[71] Because these new fields of research and teaching were inextricably linked to simultaneous developments in European colonialism, they challenged the disinterested self-image of the philologically inclined orientalists.[72]

The room for disagreement in experimental psychology and bacteriology was mostly the result of the fact that both were relatively new fields of research. One issue that caused friction among psychologists was related to the question of which mental processes could be investigated using experimental means.[73] Another pressing question dealt with the appropriate institutional environment for experimental psychology. Some psychologists argued that their work should remain within the walls of the philosophy department. This point of view was criticised, however, by both their colleagues at various faculties of philosophy and by psychologists working at other departments. The philosophers resented the fact that they had to compete with the psychologists for a limited number of university appointments. The psychologists considered themselves to be working in a completely new field of research and therefore argued that they deserved their own institutional structures.[74]

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[68] Irwin, Robert, *For Lust of Knowing: The Orientalists and their Enemies*, Penguin, London, 2007. 7.
[69] Marchand, *German Orientalism in the Age of Empire*, xxxiii.
[70] For more on the Institute for Oriental Languages, see Chapter 4, 131–134.
[71] Marchand, *German Orientalism in the Age of Empire*, 349.
[72] Paret, Rudi, *Arabistik und Islamkunde an deutschen Universitäten: Deutsche Orientalisten seit Theodor Nöldeke*, Franz Steiner, Wiesbaden, 1966. 18.
[73] An example is provided by the heated discussion about the measurement of higher mental processes: Ogden, Robert M., 'Oswald Külpe and the Würzburg School,' *The American Journal of Psychology*, 64(1), 1951, 4–19. 10–12.
[74] Ash, Mitchell G., 'Academic Politics in the History of Science: Experimental Psychology in Germany, 1879–1941,' *Central European History*, 13(3), 1980, 255–286. 278–282.

Bacteriologists faced a very different challenge. The safety and efficacy of their innovative new cures often met with scepticism from the general public as well as from medical practitioners. Because the sceptics' suspicions proved to be justified in at least one widely covered case, newly developed cures continued to face critical scrutiny well into the 20th century.[75] The distrust about bacteriological findings was further reinforced by the fact that large amounts of money could be made through the sale of new drugs. Against this background, well-researched claims about effective new cures could be represented in the media as mere self-serving advertising.[76]

The protagonists

In order to provide detailed descriptions of scholarly life, I looked at the everyday working life of individual scholars rather than at disciplines as a whole. However, because disagreement about the assessment of virtue requires more than one person, this study is not limited to individuals. It looks not merely at the words and actions of individual scholars, but also at the networks in which they participated. The different chapters put emphasis on different network relationships of the protagonists. Some of these networks were collegial communities of academics working at the same faculty or laboratory. However, collegial networks could also take shape around collaborations between researchers who did not share a physical work environment. Influential professional networks emerged around leading journals and scholarly societies. Prussian government officials also cultivated their own networks of academic advisors. Finally, most scholars maintained an extensive personal network through private correspondence. Studying the protagonists' membership of more than one type of network provided a close look at the role of assessments of virtue in a variety of environments.

This study's examination of conceptions of virtue in orientalism is largely based on the networks of Theodor Nöldeke. Nöldeke was an active member of the *Deutsche Morgenländische Gesellschaft* (*DMG*), the national association of German orientalists. He was also an active contributor to the *Literarische Centralblatt*, one of the best-read review journals of late 19th century Germany. In addition, he was a member of the international consortium assembled by his Leiden colleague Michael Jan de Goeje for his 30-year project of editing the *Annals* of al-Ṭabarī. Last but not least, Nöldeke maintained close personal relationships with colleagues from different generations, such

[75] Gradmann, Christoph, 'Robert Koch und das Tuberkulin — Anatomie eines Fehlschlags,' *Deutsche Medizinische Wochenschrift*, 124(42), 1999, 1253–1256.
[76] For example, see Mildenberger, Florian, 'Auf verlorenem Posten — der einsame Kampf des Heinrich Dreuw gegen Syphilis und Salvarsan,' *Würzburger medizinhistorische Mitteilungen*, 30, 2011, 163–203. 171.

as the senior Leipzig orientalist Heinrich Leberecht Fleischer, the Hungarian Arabist Ignaz Goldziher and the versatile young Semitist Carl Heinrich Becker.

I based my examination of assessments of scholarly virtue in experimental psychology on the networks of Wilhelm Wundt, introduced above as Hugo Münsterberg's critical *Doktorvater*. Like Nöldeke, Wundt was a frequent contributor to the *Literarische Centralblatt*. In addition, he also edited his own journal, the *Philosophische Studien*. Although he cultivated a personal relationship with his older colleague Gustav Theodor Fechner, a major part of his network can be retraced to the laboratory for experimental psychology, which he established in Leipzig in 1879. This laboratory was the first of its kind, and a significant number of leading psychologists in late 19th and early 20th century Germany spent some formative months or years at this laboratory. Some of the better-known alumni from the Leipzig laboratory with whom Wundt had stayed in touch after they left, were the psychiatrist Emil Kraepelin, the psychologist Oswald Külpe and the paedagogical scholar Ernst Meumann.

The investigation into the moral economy of bacteriology, finally, was largely based on researchers associated with the early years of the *Institut für Infectionskrankheiten* (institute for infectious diseases) in Berlin, in the 1890s. For this study, I looked at its first director Robert Koch as well as at its staff members Emil Behring and Paul Ehrlich. This collaborative network was disbanded in the 1890s, when Behring and Ehrlich left the institute to pursue independent careers. Notwithstanding that the relationship between Behring and his peers became increasingly tense, he would stay in contact with both Koch and Ehrlich. All three men also maintained a close relationship with Friedrich Althoff at the Prussian Ministry of Education, who had a strong interest in advancing medical research. Finally, the need to test newly developed drugs forced these bacteriologists to develop a working relationship with clinicians who could perform such tests.

As the above introduction of protagonists demonstrates, my study's primary focus was on networks of scholars within a national context. A first reason for this choice is the comparative character of the study. It would have been difficult to account for the particularities of various disciplines while assessing the differences between national scholarly cultures. A second reason is that, at least from the late 18th century onwards, the national state provided the framework within which the careers of most scholars took shape.[77] Even the features of scholarly life that were not explicitly guided by state policies often developed in a national context. Scholars were more likely to be involved in national associations, such as the *DMG,* than in international organisations. The editors of most

[77] Jessen, Ralph and Jakob Vogel, 'Die Naturwissenschaften und die Nation: Perspektiven einer Wechselbeziehung in der europäische Geschichte,' in: Jessen, Ralph and Jakob Vogel (eds.), *Wissenschaft und Nation in der europäische Geschichte*, Campus, Frankfurt, 2002, 7–40. 22.

scholarly journals preferred to publish contributions by their compatriots in the national language. And, even though some scholars attracted a high number of foreign pupils, most students chose to study and pursue an academic career in their country of birth.[78]

I should note that this emphasis on the national context of scholarship does not imply the absence or insignificance of international academic networks. The development of psychology as an academic discipline in the United States, for instance, was decisively shaped by former students of Wundt. In the late 20th century, half of America's psychologists could still claim to be descendants of Wundt's teachings.[79] Another example of the international character of scholarship is provided by Behring's cultivation of strong ties with researchers at the Parisian *Institut Pasteur*.[80] Compared to the other disciplines discussed in this study, 19th century orientalism had a very strong international orientation.[81] The description of the collaboration on De Goeje's al-Ṭabarī edition by an international consortium of scholars, mentioned in Chapter 1, further illustrates this quality.

Of course, assessments of scholarly virtues were not made in only one location; this study could have focused on any European country to contribute to our understanding of them. The emphasis on Germany is attractive, however, for a couple of reasons. In the first place, all of the main 19th century developments in the organisation of scholarship can also be found in German academia. These developments included, but were not limited to, the founding of professional societies, a strong growth in the number of scholarly journals, the ever more frequent collaboration between industry and university, a continuous increase in the number of students and teachers, and the growing importance of new academic spaces, such as seminars and laboratories.[82]

In addition, German universities, research institutes and scholars were held in high regard, around the world. German Arabists, for example, were the single largest group of scholars participating in De Goeje's al-Ṭabarī consortium. The international appreciation of Wundt's work is illustrated by the large number of international students who flocked to Leipzig. The influence of Koch's work

[78] Examples of university teachers who attracted a large number of foreign students include the Paris orientalist Antoine Isaac Sylvestre de Sacy and Wilhelm Wundt. Sylvestre de Sacy influenced a generation of German arabists, among whom we find Heinrich Leberecht Fleischer. Wundt taught a large number of students from the United States. See Marchand, Suzanne L., *German Orientalism in the Age of Empire*, 121 and Boring, Edwin G., *A History of Experimental Psychology*, Chapters 18 and 21.
[79] Hillix, William A. and James W. Broyles, 'The Family Tree of American Psychologists,' in: Bringmann, Wolfgang G. and Ryan D. Tweney (eds.), *Wundt Studies: A Centennial Collection*, Hogrefe, Toronto, 1980, 422–434. 433.
[80] Linton, Derek s., *Emil von Behring: Infectious Disease, Immunology, Serum Therapy*, American Philosophical Society, Philadelphia, 2005. 190.
[81] The case for the international clout of German orientalism is most explicitly made in: Mangold, Sabine, *Eine "weltbürgerliche" Wissenschaft*, 296–298. The book's title translates as 'A Cosmopolitan Discipline.'
[82] For example, see Charle, Christophe, 'Patterns,' in: Rüegg, Walter (ed.), *A History of the University in Europe*, Volume III, Universities in the Nineteenth and Early Twentieth Centuries (1800–1945), Cambridge University Press, Cambridge, 2004, 33–80.

is illustrated by the fact that he is often mentioned in the same breath as foreign luminaries, such as Joseph Lister and Louis Pasteur.[83]

Finally, the well-maintained and accessible archives and university libraries in Germany made it highly attractive to work on the history of German scholarship. The archives of the Prussian Ministry of Education deserve a special mention here. The fact that, for a long time, this influential ministry was managed by a single official — the aforementioned Friedrich Althoff — whose papers have been well-preserved, was highly instrumental in studying the relationship between state and academia. As the following section shows, the accessibility of the well-maintained collections at a number of universities, libraries and archives was equally indispensable for this study.

Primary sources

The case studies in this book are largely based on the examination of a large number of primary sources. Correspondence proved to be indispensable for a better understanding of the often privately shared assessments of virtue. In the case of Nöldeke, some of his correspondence has already been published.[84] Most of his letters, however, are preserved in various archives. At Leiden University Libraries, I consulted his correspondence with Michael Jan de Goeje and Christiaan Snouck Hurgronje, who was a student of both De Goeje and Nöldeke. Most of Nöldeke's correspondence with De Goeje is being preserved in neatly tagged folders.[85] A significant number of Nöldeke's letters, however, is stored in the folders that contain De Goeje's communications about his al-Ṭabarī edition.[86] Fleischer's letters to Nöldeke have been retrieved from his papers at the university library in Tübingen.[87] I also examined the papers of Ignaz Goldziher, which contain letters from Nöldeke as well as from other colleagues, such as Becker and De Goeje.[88] In addition, I consulted the *DMG* archives in Halle, which allowed for a closer look at both the society's inner workings and Nöldeke's role in it.[89] And, last but not least, I analysed all of Nöldeke's more than 100 book reviews in the *Literarische Centralblatt* between 1871 and 1880.

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[83] Gradmann, Christoph, *Krankheit im Labor: Robert Koch und die medizinische Bakteriologie*, Wallstein, Göttingen, 2005. 15.
[84] Koningsveld, Pieter S. van (ed.), *Orientalism and Islam: the letters of C. Snouck Hurgronje to Th. Nöldeke from the Tübingen University Library*, Documentatiebureau Islam-Christendom, Leiden, 1985; Maier, Bernhard (ed.), *Gründerzeit der Orientalistik: Theodor Nöldekes Leben und Werk im Spiegel seiner Briefe*, Ergon, Würzburg, 2013.
[85] The correspondence with De Goeje can be found at: Leiden University Library (hereafter LUL): BPL: 2389. Nöldeke's letters to Snouck Hurgronje can be found at: LUL: Or. 8952 A: 754–772.
[86] LUL: Or. 5585e and LUL: Or. 5585f.
[87] Universitätsbibliothek Tübingen (hereafter UBT): Md 782 A 68.
[88] Magyar Tudományos Akadémia Könyvtár Információs Központ (Library and Information Centre of the Hungarian Academy of Sciences, hereafter MTAK), https://konyvtar.mta.hu/.
[89] Universitätsarchiv Halle-Wittenberg (hereafter UAHW) Rep. 90:70 and 90:67.

The only part of Wundt's correspondence that has been published consists of his letters to and from Kraepelin.[90] However, scans of a large part of his correspondence kept at the university archives in Leipzig have been made available online.[91] I transcribed additional material that has not been made available in this way, at the archive itself.[92] This material contains letters exchanged between Wundt, his publisher and his former students. I also carefully read Wundt's more than 100 book reviews in the *Literarische Centralblatt* in the 1870s. The chapter that discusses these reviews is preceded by one that, among other things, takes a closer look at the editorial practices at the *Centralblatt*. This section is largely based on the correspondence of the *Centralblatt*'s long-time editor Friedrich Zarncke, which I consulted at the university library in Leipzig.[93] These papers contain Zarncke's correspondence with a large number of reviewers as well as with his publisher Eduard Avenarius.

The bacteriological case studies are based on extensive archival research, as well. A large number of letters to and from Koch have been preserved at his old research institute, today known as the *Robert Koch-Institut*.[94] This material was complemented with letters of Koch, Behring and Ehrlich, kept in the Ludwig Darmstaedter collection at the *Staatsbibliothek* in Berlin.[95] The *Staatsbibliothek* also stores the correspondence of Behring's collaborator Erich Wernicke.[96] These provide a valuable addition to the other material, because Behring developed a personal relationship with Wernicke that was closer than with any of his other colleagues. An even larger number of Behring-related material has been scanned and made available online by the University of Marburg.[97] This source has been especially important for consulting the correspondence between Behring and the paediatrician Otto Heubner, who tested his diphtheria blood serum in the 1890s.

Because of the Prussian government's far-reaching interest in medical research, a large amount of information about the careers of Koch, Behring and Ehrlich can be found among the Althoff papers at the *Geheimes Staatsarchiv Preußischer Kulturbesitz* (Prussian Secret State Archives). A large number of folders contains either correspondence between these men or documents in which their

[90] Steinberg, Holger (ed.), *Der Briefwechsel zwischen Wilhelm Wundt und Emil Kraepelin: Zeugnis einer jahrzehntelangen Freundschaft*, Hans Huber, Bern, 2002.
[91] https://home.uni-leipzig.de/wundtbriefe/viewer.htm.
[92] Universitätsarchiv Leipzig (hereafter UAL): Nachlass Wilhelm Wundt: Signatur: NA Wundt.
[93] Universitätsbibliothek Leipzig (hereafter UBL): Nachlass 249.
[94] An overview of Koch's papers in Berlin can be found in: Münch, Ragnhild, *Robert Koch und sein Nachlaß in Berlin*, De Gruyter, Berlin, 2003.
[95] For Koch, see Sammlung Darmstaedter in der Staatsbibliothek zu Berlin (hereafter Slg. Darmstaedter), 3b 1882. For Behring, see Slg. Darmstaedter, 3a 1890 (5). For Ehrlich, see Slg. Darmstaedter 3a 1875 (4).
[96] Staatsbibliothek zu Berlin (hereafter StaBi), Nachlass 156.
[97] https://www.uni-marburg.de/fb20/evbb/behring-digital.

research institutes are discussed.[98] Althoff was not, however, exclusively interested in medical research. His papers also include reports about the Seminar for Oriental Languages in Berlin and evaluations of the work of university appointees in various other disciplines.[99] His papers contain a large number of informal reports about the day-to-day operation of faculties and research institutes, complemented with private discussions about these findings between Althoff and his most trusted advisors. These materials provide both a glimpse of scholars' everyday life in 19th century Germany and a closer look at the entanglement of virtue assessments and appointment policies.

The reliance on such a wide range of archival material raises the question if any generalisable conclusions can be drawn from such a variety of sources. If I would have limited myself to listing scattered references to scholarly virtue, I would most likely have ended up with nothing more than a disjointed inventory of value judgements, anecdotes and more or less widespread ways of assessing the challenges of an academic career. Such findings might not have supported any generalisable conclusion about the assessment of virtue among 19th century scholars. The only conclusion that could have been drawn from such a list would have been that there are many significant differences between all the individuals, disciplines and institutions included in this study. Its aim, however, was more ambitious and actually benefited from this wide variety of sources.

As mentioned above, I was primarily interested in assessments of the way in which different virtues relate to each other. However, bringing the relationships between a variety of virtues to light was no easy feat; when people have no objections to each other's conduct, they rarely make explicit references to virtue or vice. Disagreements about such issues only become visible in scenarios of actual or potential conflict.[100] And conflict may arise whenever scholars find themselves in a position of having to pass judgement on each other's work or character. This study, therefore, focuses on situations in which judgement is encouraged or even required; for example, in the

[98] For these pieces of correspondence, see Geheimes Staatsarchiv Preußischer Kulturbesitz (hereafter GStA PK): VI. HA, Nl Althoff, F. T., No. 668, Behring-Bellermann; VI. HA, Nl Althoff, F. T., No. 709, Ehrenberg-Ehrlich; VI. HA, Nl Althoff, F. T., No. 801, Koban-Kögel. For documents about these men's research institutes, see GStA PK: VI. HA, Nl Althoff, F. T., No. 325, v. Behring. Heilseruminstitut. Briefe v. Behrings; VI. HA, Nl Althoff, F. T., No. 326, v. Behring. Heilseruminstitut. Allgemeine Briefe; VI. HA, Nl Althoff, F. T., No. 313, Das Kochsche Tuberkulin; VI. HA, Nl Althoff, F. T., No. 343, Kochsches Institut für Infektionskrankheiten; VI. HA, Nl Althoff, F. T., No. 346, Diphterie-Kontrollstation Berlin. Institut für Serumforschung Steglitz. Institut für experimentelle Therapie Frankfurt/Main.

[99] See GStA PK: VI. HA, Nl Althoff, F. T., No. 156, Orientalisches Seminar, Lehrer, Lehraufträge; VI. HA, Nl Althoff, F. T., No. 159, Orientalisches Seminar: Diversa; VI. HA, Nl Althoff, F. T., No. 112, Mediziner (Beurteilungen); VI. HA, Nl Althoff, F. T., No. 118, Philosophen (Beurteilungen); VI. HA, Nl Althoff, F. T., No. 124, Semitisten (Beurteilungen).

[100] Herman Paul has also suggested that it might be 'worthwhile to study clashes and conflicts between epistemic virtues', in: Paul, Herman, 'Performing history: how historical scholarship is shaped by epistemic virtues,' *History and Theory*, 50(1), 2011, 1–19. 13.

editorial decision-making process and the practice of writing letters of recommendation for vacant professorial chairs. Private correspondence offers yet another opportunity for criticising the work of a colleague — as well as for sharing some juicy scholarly gossip.[101] It is therefore important to look at a wide variety of sources in order to identify the many tensions that shed light on different assessments of virtue. Exactly this divergence of assessments allows me to identify moral economies that can be described as balanced systems shaped by the continuous interplay between potentially conflicting virtues.

Cultures of scholarship

I used a cultural history approach in my assessment of the delicate balance of scholarly virtues. Although the term 'culture' is often used by historians and social scientists, its meaning is not self-evident. William Sewell points out that we can distinguish at least 'two fundamentally different meanings' of the term.[102] Culture can be understood as 'a theoretically defined category […] that must be abstracted out from the complex realities of human existence'. In this meaning, the word culture belongs to 'a particular academic discipline or subdiscipline' and invites an emphasis on culture as an analytical concept instead of a focus on a variety of cultures. The second meaning refers to culture as 'a concrete and bounded world of beliefs and practices'. The latter assumes the existence of different cultures characterised by their own specific set of beliefs and practices. This study follows the example of sociologists and historians of scholarship who have worked within frameworks that fit this second meaning, with some qualifications.

The first qualification relates to Sewell's description of cultures as 'concrete and bounded worlds'. The cultures examined in this study are primarily shaped by networks of scholars working in specific disciplines. This, however, does not imply that their world is so bounded that they are isolated from their colleagues working in other disciplines and the world outside the university. Still, large parts of this study focus on the disciplinary networks that facilitated the development of shared conceptions of scholarly virtues. Some chapters, however, pay attention to the way in which most scholars were involved with a wide variety of people and institutions.

The second qualification concerns Sewell's use of the word 'belief'. Even though this word can refer to all kinds of ideas and convictions, I interpret it as primarily referring to shared conceptions

[101] On the function of gossip among scholars, see Engberts, Christiaan, 'Gossiping about the Buddha of Göttingen: Heinrich Ewald as an unscholarly persona,' *History of Humanities*, 1(2), 2016, 371–385.
[102] Sewell, William H., Jr., 'The Concept(s) of Culture,' in: Bonnell, Victoria E. and Lynn Hunt (eds.), *Beyond the Cultural Turn: New Directions in the Study of Society and Culture*, University of California Press, Berkeley (CA), 1999, 35–61. 39.

of virtue. In the context of this study, Sewell's description of cultures as 'concrete and bounded worlds of beliefs and practices' is therefore the starting point of a cultural history of scholarship that examines disciplinary networks of scholars characterised by attempts to assess widely recognised virtues.

Sociologists of knowledge were the first to study the history of scholarship along these lines. In the late 1970s and early 1980s, they discovered the laboratory as a distinctive environment in which researchers developed common conceptions of virtue. Bruno Latour and Steven Woolgar's famous study of the Salk Institute for Biological Studies and Karin Knorr Cetina's analysis of the workings of a large research centre in Berkeley were early and influential examples of this approach.[103] Historians of science soon followed suit. Steven Shapin's and Simon Schaffer's work on Robert Boyle's air-pump experiments is probably the most famous example.[104] Other early studies of the culture of experimental science include Peter Galison's *How Experiments End* and a wide-ranging volume edited by David Gooding, Trevor Pinch and Simon Schaffer.[105] During the subsequent decades, many more studies appeared that claimed to build on the cultural approach to understanding scholarship pioneered by scholars, such as Latour, Woolgar, Shapin and Schaffer.[106]

Initially, historians of science limited their efforts to understand cultures of scholarship to the study of the experimental sciences. In recent years, however, more and more studies have been published that offer a cultural account of the history of the humanities. Historians, in particular, have self-consciously responded to the challenges and opportunities offered by the application of the questions and methods of the cultural history of the experimental sciences to their own discipline. Paul, for example, refers to Knorr Cetina's concept of 'epistemic cultures' when making his case for the study of the virtues and practices of the historians of the past.[107] Kasper Eskildsen and Phillip Müller draw attention to the way in which the archives and the seminar at the professor's home shaped the historical discipline in the 19th century.[108] Jo Tollebeek's attempt to write an

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[103] Latour, Bruno and Steven Woolgar, *Laboratory Life: The Construction of Scientific Facts*, Sage, Beverly Hills (CA), 1979 and Knorr Cetina, Karin, *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science*, Pergamon, Oxford, 1981.

[104] Shapin, Steven and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life*, Princeton University Press, Princeton (NJ), 1985.

[105] Galison, Peter, *How Experiments End*, University of Chicago Press, Chicago (IL), 1987 and Gooding, David, Trevor Pinch and Simon Schaffer (eds.), *The Uses of Experiment: Studies in the Natural Sciences*, Cambridge University Press, Cambridge, 1989.

[106] Examples of influential studies by social scientists include: Pickering, Andrew (ed.), *Science as practice and culture*, Chicago University Press, Chicago (IL), 1992 and Knorr Cetina, Karin, *Epistemic Cultures: How the Sciences Make Knowledge*, Harvard University Press, Cambridge (MA), 1999. Other examples of influential historical studies include: Kohler, *Lords of the Fly* and Daston and Galison, *Objectivity*.

[107] Paul, Herman, 'Performing history,' 11.

[108] Eskildsen, Kasper Risbjerg, 'Inventing the archive,' 19; Müller, Philipp, 'Geschichte machen. Überlegungen zu lokal-spezifischen Praktiken in der Geschichtswissenschaft und ihrer epistemischen Bedeutung im 19. Jahrhundert. Ein Literaturbericht,' *Historische Anthropologie*, 12(3), 2004, 415–433. 421–427.

Alltagsgeschichte based on the papers of the Belgian historian Paul Fredericq is one of the most ambitious attempts to understand the culture of 19th century historical scholarship.[109] So, even if a strong interest in a cultural history of the humanities is a relatively recent development, it is a steadily growing field.

The studies listed above describe different disciplines in different eras in different countries. Rather than summarising their myriad of findings, the following pages take a closer look at two concepts that are often used by cultural historians. These concepts are not only widely employed in cultural histories of scholarship, they are also at the analytical heart of this study. First, I reflect on the anthropological approach to the history of science that draws its inspiration from Latour, Woolgar and Knorr Cetina, followed by a closer look at the role of practices in such anthropology-inspired accounts of scholarship.

Anthropology and thick description

Most early works on the culture of scholarship by social scientists explicitly relied on the anthropological method of participant observation. In his introduction to Latour and Woolgar's *Laboratory Life,* Jonas Salk, the founder of the investigated laboratory, describes their approach as 'a kind of anthropological probe to study a scientific culture'.[110] In a similar vein, Knorr Cetina stated that she aimed to expose herself to 'the savage meaning of the scientists' laboratory action' through 'direct observation and participation'.[111] Of course, the anthropological method of participant observation is not a viable option for investigating the past. However, one scholar's reflections on the appropriate aims of anthropological research have been particularly instrumental in forging a connection between historiography and anthropology.

In a highly influential 1973 essay, Clifford Geertz argued that anthropology is essentially 'a venture in […] thick description'.[112] Culture, in Geertz's phrasing 'is not a power, something to which social events, behaviors, institutions, or processes can be causally attributed: it is a context, something within which they can be intelligibly – that is, thickly – described'.[113] The 'grand realities' of culture are understood not through abstract analyses, but through 'exceedingly extended acquaintances with extremely small matters'.[114] Geertz's call for thick description is one for attention needing to

[109] Tollebeek, *Fredericq & Zonen*.
[110] Salk, Jonas, 'Introduction,' in: Latour, Bruno and Steven Woolgar, *Laboratory Life*, 11–13. 11.
[111] Knorr Cetina, Karin, *The Manufacture of Knowledge*, 23.
[112] Geertz, Clifford, *The Interpretation of Cultures: Selected Essays*, Basic Books, New York (NY), 1973. 6.
[113] Ibid., 14.
[114] Ibid., 21.

be paid to a wide variety of everyday details in order to obtain enough contextual knowledge to make seemingly unintelligible values and practices intelligible. The appeal to collect and scrutinise large amounts of detailed information to acquire a better understanding of the past offered historians of scholarship a viable anthropology-based methodological starting point. What is more, the attention to detail and context must have felt very familiar to those historians who sympathised with the philological ethos that has long influenced historical scholarship.[115]

Historians have embraced Geertz's framework with more enthusiasm than his anthropologist peers.[116] Paul, for example, argues that his proposal to investigate epistemic virtues 'encourages thick description and careful contextualization, so as to take into account the peculiarities of practices and epistemic cultures'.[117] Galison and Warwick's reference to an ethnographer's account of a Balinese cockfight is an obvious nod to Geertz's work.[118] Nicholas Jardine and Emma Spary likewise emphasise the importance of Geertz's work to the history of scholarship.[119] Others do not mention Geertz or thick description, but nonetheless and unequivocally place themselves in the anthropological tradition. Shapin and Schaffer state that they approach the 'culture of experiment as […] a stranger approaches an alien society'.[120] In his book about Fredericq — with the telling subtitle *An anthropology of modern historiography* — Tollebeek explicitly mentions the work by Latour and Woolgar as well as Shapin and Schaffer's *Leviathan and the Air-Pump* as inspirational examples.[121] The thick descriptions in Kohler's study of the moral economy of fruit fly genetics, finally, are also indebted to Geertz's example.

One criticism that is often aimed at this type of study concerns its tendency to focus narrowly on synchronic accounts of the past at the expense of diachronic narrative. One author argues that Geertz's conception of culture as 'interlaced and mutually sustaining systems of meaning' encourages analyses 'in which time is *suspended* or *abolished* analytically, so that things that actually occur in the flow of time are treated as part of a uniform moment'.[122] The anthropology-inspired work by historians of scholarship, generally, turns out to display a synchronic character, as well.

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[115] For example, see Paul, Herman, 'Weber, Wöhler, and Waitz: Virtue Language in Late Nineteenth-Century Physics, Chemistry, and History,' in: Dongen, Jeroen van and Herman Paul (eds.), *Epistemic Virtues in the Sciences and the Humanities*, Springer, Cham, 2017, 91–107. 94–95.
[116] Sewell Jr., William H., 'Geertz, Cultural Systems, and History: From Synchrony to Transformation,' in: Ortner, Sherry B. (ed.), *The Fate of "Culture": Geertz and Beyond*, University of California Press, Berkeley, 1999, 35–55. 36.
[117] Paul, Herman, 'Performing history,' 15.
[118] Galison, Peter and Andrew Warwick, 'Introduction: Cultures of Theory,' *Studies in History and Philosophy of Modern Physics*, 29(3), 287–294. 290.
[119] Jardine, Nicholas and Emma Spary, 'The natures of cultural history,' in: Nicholas Jardine, James A. Secord and Emma C. Spary (eds.), *Cultures of Natural History*, Cambridge University Press, Cambridge, 1996, 3–13. 7.
[120] Shapin, Steven and Simon Schaffer, *Leviathan and the Air-Pump*, 6.
[121] Tollebeek, Jo, *Fredericq & Zonen*, 25–27.
[122] Sewell Jr., William H., 'Geertz, Cultural Systems,' 40.

This study largely fits that same mould. The overview of assessments of scholarly virtue provides the building blocks for a largely synchronic cross-disciplinary comparison. This perspective is warranted by the fact that the virtues and associated practices analysed in this study continued to shape scholarly culture throughout the examined period. A more diachronic perspective would be an interesting subject for follow-up research. However, the epilogue already shares some tentative reflections on questions of stability and change, over time.

Finally, I would like to emphasise once more that the common references to Geertz and thick description do not amount to a carefully defined and broadly shared historical anthropological approach. This study is nevertheless strongly influenced by one feature shared by all the authors mentioned above, namely that of an emphasis on a contextualising approach to historical research in order to draw attention to the everyday activities of scholarship. This approach is most clearly exemplified in Tollebeek's careful investigation of Fredericq's working life, which is based on the latter's record of his daily routines in his diary. This study also pays ample attention to the everyday contexts that shaped the shared — and sometimes contested — conceptions of virtue that moulded scholars' working lives.

Practices of scholarship

A second recurring concept in cultural studies of scholarship is that of an emphasis on practices. Even if these studies primarily seek to uncover intellectual, moral and social values, they often try to understand these norms through an analysis of what people do. Latour and Woolgar claim to use 'the notion of anthropological strangeness' to 'depict the activities of the laboratory as those of a remote culture'.[123] Knorr Cetina argues that she uses culture to refer 'to the aggregate patterns and dynamics that are on display in expert practice […]. Culture […] foregrounds the machineries of knowing composed of practices'.[124] Few people are as adamant in their conviction of the importance of a practice turn in science studies as the sociologist Andrew Pickering, who states that '[...] all of the stock appreciations of scientific knowledge — as objective […], as relative to culture […], as relative to interests […] — can be translated into particular understandings of scientific practice'.[125]

[123] Latour, Bruno and Steven Woolgar, *Laboratory Life*, 36.
[124] Knorr Cetina, Karin, *Epistemic Cultures*, 8, 10.
[125] Pickering, Andrew, 'From Science as Knowledge to Science as Practice,' in: Pickering, Andrew (ed.), *Science as practice and culture*, University of Chicago Press, Chicago, 1992, 1–26. 7.

Historians of scholarship have also adopted this emphasis on practices. Shapin and Schaffer claim that they 'want to understand the nature and status of experimental practices and their intellectual products'.[126] Jardine and Spary set out 'to portray natural history as the product of conglomerates of people, natural objects, institutions, collections and finances, all linked by a range of practices'.[127] Sita Steckel is another enthusiastic proponent of a practice-based history of science, arguing that the approach promises a better understanding of academic cultures through more appreciation of the various aspects and contexts of the conduct by historical agents.[128] The interest in practices also spread to the history of the humanities. Müller aims to explore epistemic practices among 19th century historians, in the light of local perspectives.[129] Paul argues that the epistemic virtues in which he is interested 'are taught, learned and exercised in practices rather than disciplines'.[130] Tollebeek, finally, argues that the study of daily practices promises unique insights into the epistemological, ethical, aesthetic, ideological and emotional commitments of scholars.[131]

A similar focus on practices has also been advanced by scholars interested in the gendered character of scholarship. Ludmilla Jordanova, for example, describes gender as 'a cultural product […] assigned […] through social and cultural practices'.[132] With an explicit nod to Geertz, she argues that a major advantage of using the concept of gender is that it allows for the drawing of 'big pictures [that] come from a rich sense of context, from an appreciation of how science, in its broadest senses, inhabits and is produced by its milieux'.[133] In the introduction to a volume on gender and science, Marina Benjamin also emphasises the importance of paying close attention to context and 'historically specific [sets] of relationships between women and science'.[134] Bonnie Smith combines gender and practices even more explicitly in a paper on the centrality of seminars and archival research to 19th century academic historians. She states that 'a rigidly adhered-to set of practices' was foundational to the historical profession and argues that these practices 'yielded distinctive ways of imagining historical work, ways that included highly gendered fantasies, that

[126] Shapin, Steven and Simon Schaffer, *Leviathan and the Air-Pump*, 3.
[127] Jardine, Nicholas and Emma Spary, 'The natures of cultural history,' 8.
[128] Steckel, Sita, 'Einleitung: Akademische Wissenkulturen zwischen Mittelalter und Moderne,' in: Kintzinger, Martin and Sita Steckel (eds.), *Akademische Wissenskulturen: Praktiken des Lehrens und Forschens vom Mittelalter bis zur Moderne*, Schwabe, Basel, 2015, 1–5. 2.
[129] Müller, 'Geschichte machen,' 419.
[130] Paul, Herman, 'Performing history,' 11.
[131] Tollebeek, Jo, 'L'historien quotidien: pour une anthropologie de la science historique moderne,' *Schweizerische Zeitschrift für Geschichte*, 61(2), 2011, 143–167. 153.
[132] Jordanova, Ludmilla, 'Gender and the historiography of science,' *British Journal for the History of Science*, 26, 1993, 469–483. 482.
[133] Ibid., 472.
[134] Benjamin, Marina, 'Introduction,' in: Benjamin, Marina (ed.), *Science and Sensibility: Gender and Scientific Enquiry*, 1780–1945, Basil Blackwell, Oxford, 1991, 1–23. 9.

also enticed people into the profession and shaped its nature'.[135] In these gendered fantasies, common practices of scholarship were conceptualised as manifestations of quintessentially masculine virtue. Archival research was, for example, presented as a challenge involving the sort of suffering and torture that only a strong and courageous man could handle.[136]

Most of these authors assume that the meaning of the word 'practice' is somehow self-evident. However, Victoria Bonnell and Lynn Hunt notice that '[...] "practice" can be as ambiguous as culture'.[137] Most of the uses of the term by historians of scholarship fit Joseph Rouse's tentative characterisation of practices as 'patterns of activity in response to a situation'.[138] This description suggests that studies of scholarly practices should not deal with everything that their protagonists do, but only with those acts that conform to some sort of pattern. The practices investigated in this study were selected on the basis of their contribution to patterns of scholarly evaluation. This patterned character also suggests another quality of practices; namely that they 'exist only through being continually reproduced'.[139] Because every instance of reproduction allows for some deviation from the established pattern, practices are subject to gradual change. This susceptibility to change through ever-changing reproduction suggests that the values of individual agents can shape existing practices.

An understanding of practices as patterns of activity, therefore, combines two perspectives on agency. In the first place, this characterisation underlines that the scope of action by individual agents is shaped by existing patterns. This is reflected in this study's exploration of acts that fit well-established means of assessing scholarship, such as editorial decision-making and writing letters of recommendation. Secondly, the characterisation of practices as patterns of activity draws attention to the extent to which agents can shape these patterns. This is reflected in this study's emphasis on the examination of the way in which virtues relate to each other on the level of day-to-day scholarly work. Since constellations of commitment are subject to change, individual agents are in a position to continuously reassess the relative weight of different virtues. Against this background, all the case studies below illustrate the agency of scholars whose assessments of virtue

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[135] Smith, Bonnie G., 'Gender and the Practices of Scientific History: The Seminar and Archival Research in the Nineteenth Century,' *American Historical Review*, 100(4), 1995, 1150–1176. 1150.
[136] Ibid., 1173.
[137] Bonnell, Victoria E. and Lynn Hunt, 'Introduction,' in: Bonnell, Victoria E. and Lynn Hunt (eds.), *Beyond the Cultural Turn: New Directions in the Study of Society and Culture*, University of California Press, Berkeley (CA), 1999, 1–32. 12.
[138] Rouse, Joseph, *Engaging Science: How to Understand Its Practices Philosophically*, Cornell University Press, Ithaca (NY), 1996. 26.
[139] Ibid.

can be understood as efforts to shape their own careers, the careers of their colleagues, and their institutional surroundings.

The structure of this study

The first four chapters of this study each build on the examination of one particular practice. Chapter 1 explores personal correspondence between scholars, Chapter 2 looks at considerations of editors of scholarly journals, followed by Chapter 3 that looks at the evaluative content of published book reviews. Finally, Chapter 4 deals with letters of recommendation for professorial appointments. Admittedly, these are not the only practices that would have invited 19th century German scholars to assess their peers' virtues. An investigation of peer interaction at conferences or a closer look at the founding and functioning of learned societies would have fitted in.[140] I chose, however, to focus on those practices that are most likely to convey the voice of individual scholars. In this light, a focus on conferences or learned societies would be questionable. The elements of these environments that would be of most interest to this study would be the informal exchanges between attendees and members. However, unlike correspondence, these conversations were not preserved for posterity. Therefore, both conferences and learned societies are addressed only when they are discussed in any of the main sources used in this study.

Chapter 1 explores how scholars have commented on each other's work before that work would be shared with a broader audience. This entails very disparate practices, such as the pre-publication reviewing of books and journal contributions and the testing of newly developed medical drugs. It shows how a relationship of trust between individual scholars created the conditions that allowed for informal criticism. However, it also shows that there are significant differences in how philologically inclined orientalists, experimental psychologists and bacteriologists deal with their colleagues' work. These differences can be understood as the result of specific characteristics of these disciplines. A very salient characteristic of orientalist philology, for example, is its reliance on solitary work routines. Experimental psychology, on the other hand, is typically the product of the

[140] Some interesting studies about these issues are already available. For a closer look at international conferences in the 19th century, see Everett-Lane, Debra Adrienne, 'International Scientific Congresses, 1878–1913 : Community and Conflict in the Pursuit of Knowledge' (diss., Columbia University, 2004); Feuerhahn, Wolf and Pascale Rabault-Feuerhahn (eds.), *La Fabrique internationale de la science. Les congrès scientifiques internationaux de 1865 à 1945. Revue germanique internationale*, 12, 2010. Examples of studies of very different German learned societies include: Hoffmann, Dieter, Birgit Kolboske and Jürgen Renn (eds.), *"Dem Anwenden muss das Erkennen vorausgehen" Auf dem Weg zu einer Geschichte der Kaiser-Wilhelm-/Max-Planck-Gesellschaft*, Edition Open Access, Berlin, 2015; Preissler, Holger, 'Die Anfänge der Deutschen Morgenländischen Gesellschaft,' *Zeitschrift der Deutschen Morgenländischen Gesellschaft*, 145(2), 1995, 241–327.

social setting of the laboratory. Bacteriologists also face a typical challenge; more than anyone else, they have to deal with high expectations about the applicability of their findings.

Chapter 2 takes a closer look at the relationship between researchers and the editors of scholarly journals. The case studies draw attention to the fact that the editors were not simply making sure that only the best scholarship was published. Editorial decisions were shaped by the interests and expectations of at least three groups of stakeholders, namely those of publishers, audiences and contributors. The journals analysed in this chapter (*Philosophische Studien*, *Zeitschrift der Deutschen Morgenländischen Gesellschaft* and *Literarische Centralblatt)* also differ in some notable aspects that cannot be reduced to disciplinary differences. The *Philosophische Studien* is characterised by the fact that the journal was the brain child of one omnipotent editor, Wilhelm Wundt, who did not have to worry about his journal's profitability. The *Zeitschrift* was shaped by the fact that it was the official organ of the *DMG*, a society that aimed to represent all German orientalists. The *Centralblatt*, finally, was characterised by its continuous struggle for commercial success.

Chapter 3 investigates published book reviews. At least one modern-day author stated that a close look at such reviews would be of interest to anyone trying to understand assessments of good scholarship, arguing that '[...] readers of published book reviews are given various kinds of judgements that they do not find in scholarly articles that critique other people's work. How good is the writing style? Is the prose lucid? Is the work well organised? Are there typos?'[141] This chapter analyses the book reviews that Nöldeke and Wundt wrote for the *Literarische Centralblatt,* in the 1870s. This offers an overview of the qualities of individual scholars and their works that merited praise and criticism. Because Nöldeke wrote about both theology and philology, while Wundt reviewed both medical and philosophical works, it is possible to make a complex comparison between disciplines. This comparison suggests that reviews in different disciplines could be shaped by a variety of assessments of virtue.

Chapter 4 takes a closer look at the letters of recommendation that were sent to the Ministry of Education, in order to influence its appointment decisions. The case studies in this chapter are largely based on the correspondence of Friedrich Althoff. Behring's troubled appointment in Marburg is one of the cases examined, in addition to discussions about the sometimes controversial appointment of members of the Wundtian school of psychology to philosophical chairs. The discussion of these and other cases shows how hiring decisions were based, only to a limited extent, on the ideals of academic excellence that could be demonstrated in scholarly publications. In addition, these case studies show that, even if ideals of scholarly excellence and sociability were

[141] Shatz, David, *Peer Review: A Critical Inquiry*, Rowman & Littlefield, Lanham (MD), 2004. 111.

shared across disciplines, individual faculties could value very specific virtues. An appointee at a medical faculty was expected to be a good manager of the university clinic, whereas an appointee at a philosophical institute could expect some concerns about the supposed morality of his publications and personal character.

Chapter 5 differs from the preceding ones in that it does not examine yet another type of practice. Instead, it sets out the central argument of this study by bringing together the assessments of scholarly virtue discussed in the previous chapters and presenting these as the constituent parts of one moral economy. I argue that this moral economy shapes a wide variety of relationships between individual scholars, academic institutions and the outside world.

The study concludes with an epilogue. This epilogue does not draw further conclusions about the moral economy of late 19th and early 20th century German scholarship. Instead, it offers reflections on how this moral economy translates to a modern-day setting. After all, contemporary academics still recognise most 19th century virtues of scholarship and most of the practices described in this study still exist today, in some form or other; the majority of researchers still engage in private correspondence with their peers; many of today's academics continue to be involved with journals and their editors; book reviews still provide ample opportunities for praise and criticism; and it is still rare for scholars to be appointed to any prestigious academic position without the support of their peers. Therefore, a better understanding of the moral economy of scholarship of the past might help us to make more sense of the workings of modern-day academia, too.

1. A Helping Hand

Support and Criticism in Scholarly Correspondence

The use of private correspondence

This chapter looks at the ways in which scholars in various disciplines have shaped each other's work before it would be made public. It focuses on the way in which scholars contributed to each other's work in their capacity as friend, colleague or acquaintance, rather than on collaboration within institutional structures, such as faculties or laboratories. This emphasis is advisable because details of interactions at the workplace are almost impossible to uncover, while peer contact has often been preserved in correspondence in which the minutiae of everyday research tend to be discussed at length. These discussions illustrate shared virtues, expectations and moral horizons.

At first sight, the discussion to look at letters might seem to be more relevant to our understanding of the early modern Republic of Letters than to an analysis of scholarship in Wilhelmine Germany.[1] After all, the emergence of scholarly journals like the *Journal des Sçavans* and *Philosophical Transactions* is often presented as the moment at which private communications between scholars lost most of their significance.[2] This narrative, however, underestimates the continuing importance of scholarly correspondence into the 20th century. Peter Burke, for example, argues that the 'horse-drawn commonwealth' of the Republic of Letters, which lasted until approximately 1850, was succeeded by an 'age of steam' during which the 'practice of letter writing continued to be important' and even became 'faster, cheaper and more reliable'.[3] The functions of letter writing did change, however, when the age of steam succeeded the age of horsepower.

During the early modern period, letters were the medium of choice to announce new discoveries. One author argues that when journals took over this role, the function of letters changed. Correspondence was now increasingly used to discuss private issues and already published scholarly work instead.[4] These topics are discussed at length in the correspondence of the protagonists in

[1] On the importance of scholarly correspondence during the 17th and 18th century in Germany, for example, see Herbst, Klaus-Dieter and Stefan Kratochwil (eds.), *Kommunikation in der Frühen Neuzeit*, Peter Lang, Frankfurt am Main, 2009; Schneider, Ulrich Johannes, (ed.), *Kulturen des Wissens im 18. Jahrhundert*, Walter de Gruyter, Berlin, 2008.
[2] For example, see Dülmen, Richard van, *Die Entdeckung des Individuums*, 1500–1800, Fischer, Frankfurt am Main, 1997. 106 and Joachim Kirchner, *Das deutsche Zeitschriftwesen: seine Geschichte und seine Probleme*, Teil 1, 2. neu bearbeitete und erweiterte Auflage, Harrasowitz, Wiesbaden, 1958. 14.
[3] Burke, Peter, 'The Republic of Letters as a communication system: An essay in periodization,' *Media History*, 18(3–4), 2012, 395–407. 397–398.
[4] Krauße, Erika, 'Vorbemerkung: Der Brief als wissenschaftshistorische Quelle,' in: Erika Krauße (ed.), *Der Brief als wissenschaftshistorische Quelle*, Verlag für Wissenschaft und Bildung, Berlin, 2005, 1–28. 6.

this chapter, as well. Especially the correspondence between Theodor Nöldeke and Michael Jan De Goeje can be characterised as a conversation between friends as well as peers. The discussion of published work is an important topic in the correspondence between Wilhelm Wundt and his older Leipzig colleague Gustav Theodor Fechner as well. A significant part of their correspondence engages with the legacy of their older peer, Ernst Heinrich Weber. Their exchange of ideas was not, however, limited to the discussion of the published work of others. It also provided an excellent opportunity to evaluate each other's work before sharing it with a larger audience.

Scattered references to the pre-publication sharing of manuscripts can be found frequently in introductory essays to the collected pieces of correspondence of late 19th and early 20th-century scholars. The introduction of a volume containing the letters exchanged by the classical philologists Ulrich von Wilamowitz-Moellendorff and Eduard Norden mentions that the latter never 'published anything which he had not first given to a friend to read'.[5] The quantum physicist Wolfgang Pauli likewise refused to submit a groundbreaking article to the *Zeitschrift für Physik* without first privately consulting his colleagues Niels Bohr and Werner Heisenberg.[6] The letters exchanged between the philosophers Alfred Schütz and Eric Voegelin are another example. The editor of this correspondence argues that 'both thinkers needed the other's participation and critique, and it is hardly possible to understand their works without taking their correspondence into account'.[7] The sharing of manuscripts between trusting peers is one of the evaluative practices discussed in this chapter. It was very common among orientalists and this chapter will show that researchers in other disciplines developed their own practices of epistolary evaluation.

Before discussing these other ways of evaluation, the first case study offers a detailed look at the correspondence of Nöldeke and De Goeje about the latter's edition of the *Annals* of the Persian historian al-Ṭabarī (838–923). Their letters discuss this text edition in such detail that Nöldeke almost assumes the role of a co-editor. The next case study focuses not on the assessment of a text but on the critical evaluation of a newly developed medical cure. This section deals with the tentative early attempts to perform reliable clinical tests before the introduction of Emil Behring's diphtheria blood serum. The final section of this chapter reflects on the observation that the

[5] Calder III, William M. and Bernhard Huss, 'Introduction,' in: William M. Calder III and Bernhard Huss (eds.), *"Sed serviendum officio …" The Correspondence between Ulrich von Wilamowitz-Moellendorf and Eduard Norden (1892–1931)*, Weidmann, Hildesheim, 1997, xi–xvii. xii.

[6] Hermann, Armin, 'Die Funktion von Briefen in der Entwicklung der Physik,' *Berichte zur Wissenschaftsgeschichte*, 3(1/2), 1980, 55–64. 63.

[7] Wagner, Gerhard and Gilbert Weiss, 'Editors' Introduction,' in: Gerhard Wagner and Gilbert Weiss (eds.*), A Friendship That Lasted a Lifetime: The Correspondence Between Alfred Schütz and Eric Voegelin*, translated by William Petropulos, University of Missouri Press, Columbia and London, 2011, 1–8. 3.

correspondence between Wundt and his Leipzig collaborators contains fewer evaluations of to-be-published work than the letters of his orientalist and bacteriological peers.

The friendship between Nöldeke and De Goeje

The editing of foreign language text editions, one of the primary products of 19th-century oriental studies, lent itself extraordinarily well to peer evaluation. After a first draft of a text edition had been made by a scholar working on a manuscript for months or even years, another scholar could painstakingly go through the whole provisional result. Nobody could be expected to do such a demanding and time-consuming job for all his peers. However, Nöldeke and De Goeje were close friends who held each other's judgement in high regard. Their evaluation of each other's not yet published work provides a particularly good example of just how thorough and time-consuming such reviewing practices could be. Before turning to these practices, however, I this section first looks at their lives and friendship.

Theodor Nöldeke was born in 1836 in Harburg, a small town near Hamburg. In 1849, the family moved to Lingen, a village close to the Dutch border. At a young age, his father, a high school teacher, taught him Latin and Greek, as well as the basics of French and English.[8] At the age of 15, he suffered a severe haemorrhage, which kept him at home for three months. Here, he read Gesenius' *Hebräische Grammatik* and, by the time he went back to school, his Hebrew was better than that of his teacher. In 1853, he began his studies in Oriental languages with the leading Old Testament scholar Heinrich Ewald, whom his father knew from his own student days. Ewald was known as a demanding and uncompromising man, and he turned out to be an unstructured educator.[9] He could also inspire his students, however, and succeeded in challenging Nöldeke to live up to his high expectations. In 1856 he obtained his doctorate with an essay about the history of the Quran.[10]

In 1857 Nöldeke travelled to the Netherlands to study the *Legatum Warnerianum*, the Leiden Oriental manuscript collection. Here, he met Michael Jan de Goeje, who was finishing his study of Oriental

[8] This biographical sketch draws on the introduction in: Maier, Bernhard, *Gründerzeit der Orientalistik: Theodor Nöldekes Leben und Werk im Spiegel seiner Briefe*, Ergon Verlag, Würzburg, 2013.
[9] Engberts, Christiaan, 'Gossiping about the Buddha of Göttingen: Heinrich Ewald as an Unscholarly Persona,' *History of Humanities*, 1(2), 2016, 371–385; Theodor Nöldeke to Eduard Meyer, May 11, 1925, in *Der Briefwechsel zwischen Theodor Nöldeke und Eduard Meyer (1884–1929)*, ed. Gert Audring, http://www.kohring-digital.de/noeldeke-meyer.html.
[10] Nöldeke, Theodorus, *De origine et compositione Surarum qoranicarum ipsiusque Qorani*, Verlag der Dieterichschen Buchhandlung, Göttingen, 1856.

languages. Half a century later, De Goeje would still remember the strong impression that Nöldeke left: '[…] Every day I felt the contrast between your brightness, your ingenuity, your maturity, with my own schoolboy daftness, my slow thinking, my clumsiness'.[11] De Goeje's self-perceived daftness notwithstanding, the two young men got along very well. The De Goeje family even functioned as some kind of foster family for Nöldeke. More than 60 years later, he would still reminisce about Leiden family life: 'With a certain reverence I still remember the evenings spent with De Goeje's mother, who lived under very modest conditions, and who had been born in Lingen, by the way […].'[12]

In 1858, Nöldeke left Leiden to rewrite his dissertation for a competition sponsored by the Parisian *Académie des Inscriptions,* on the basis of manuscripts in the Berlin Royal Library. He left Leiden reluctantly, but his hard work paid off. The next year, he was the youngest of the three winners of the competition, the other beings Aloys Sprenger and Michele Amari. The German translation of this work was published one year later, under the title *Geschichte des Qorāns*. This was 'the first masterpiece of his career'.[13] In 1860, Nöldeke returned to Göttingen, first as a librarian, after finishing his *Habilitation* as a *Privatdozent* in Semitic languages. In 1864, he accepted an a position as *Extraordinarius* in Kiel, which would be turned into a full professorship four years later. In 1874, he was appointed to the Chair of Semitic Languages at the newly established *Reichsuniversität Strassburg.* Although other prestigious universities, such as those of Vienna, Berlin and Leipzig, tried to lure him, he stayed in Strasbourg for the remainder of his career.[14] He retired in 1906, at the age of 70, but continued publishing for 20 more years. Throughout his career, his striving for rational scholarship was so central to his work that, later, authors referred to him as a positivist.[15] He was a 'firm believer in facts', who cared about 'precision in reading, editing and translating 'oriental' texts', which caused him to supply his friends with 'corrections to their manuscripts […] and queries about particular details, which might be better known to them'.[16]

[11] UBL: BPL 2389, Michael Jan de Goeje to Theodor Nöldeke, no date, written as a reply to Nöldeke's letter of 14 October 1907.
[12] UBL: Or: 8952 A: 770, Theodor Nöldeke to Christiaan Snouck Hurgronje, 3 March 1923.
[13] Nöldeke, Theodor, *Geschichte des Qorāns*, Verlag der Dieterichschen Buchhandlung, Göttingen, 1860; Marchand, *German Orientalism*, 175.
[14] For Berlin see: UBL: BPL 2389, Theodor Nöldeke to Michael Jan de Goeje, 28 February 1875. For Berlin see UBL: BPL 2389, Theodor Nöldeke to Michael Jan de Goeje, 20 June 1888. For Vienna see UBL: BPL 2389, Theodor Nöldeke to Michael Jan de Goeje, 15 December 1879.
[15] Paret, Rudi, *Arabistik und Islamkunde*, 14.
[16] Marchand, *German Orientalism*, 177.

One of the main beneficiaries of these efforts was his Leiden friend Michael Jan de Goeje, Jan to his friends and close colleagues.[17] De Goeje was born in the Frisian village of Dronrijp as the son of a Protestant minister.[18] His father taught him both classical and modern languages. De Goeje's father died when he was eighteen, but his family raised enough money to send him to Leiden to study theology. As a future theologian he had to obtain a basic knowledge of some Semitic languages. Because he enjoyed this so much, he decided to quit his studies in theology and become a Semitist instead. T.W.J. Juynboll, Professor of Eastern Languages, referred him to the renowned Arabist and Professor of Modern History, Reinhart Dozy. Dozy would be a shining example for De Goeje, throughout his career: 'To collect and critically rework Arabic texts that had to serve as sources of a certain part of the history of civilisation, and then publish the results from these studies in a tasteful fashion, just like Dozy […] which became and continued to be his scholarly ideal'.[19] In 1860, Juynboll awarded him his doctorate for an Arabic text edition and Latin translation of excerpts of an important Arabic geographical work, al-Yaqubi's *Kitab al-Buldan*.[20]

Shortly before he had obtained his degree, De Goeje was appointed assistant curator of the *Legatum Warnerianum*. Because this seemed to be a dead-end job, he started looking for jobs outside academia, as well. In 1864 he wrote Nöldeke that he might have to give up his scholarly ambitions.[21] In 1866, however, Leiden University finally offered him an assistant professorship in eastern languages. Three years later he was appointed as *Interpres Legati Warneriani*, head curator of the manuscript collection, as well as full professor. Unlike Nöldeke, who published on a myriad of Semitic languages, De Goeje was primarily interested in one language: Arabic. His preferred type of publication was the text edition, though he sometimes also published essays based on his editions. From 1870 onwards, he published the *Bibliotheca geographorum Arabicorum*, an eight-volume series of works by Arabic geographers. His most memorable achievement, however, was his edition of the *Annals* of al-Ṭabarī, published between 1879 and 1901. This fifteen-volume and almost 10,000-page accomplishment was made possible by the help of an international consortium of co-editors, copyists and manuscript hunters. Nöldeke was one of the most helpful evaluative voices in this endeavour.

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[17] After Nöldeke had left Leiden, he apparently did not even know that De Goeje's first name was Michael. De Goeje had to explain to him that he did not sign his letters with an 'M' for '*Monsieur*', but that the 'M' stood for 'Michael' instead. UBL: BPL 2389: Michael Jan de Goeje to Theodor Nöldeke, 19 October 1862.

[18] This biographical sketch draws on the portrait in: Vrolijk, Arnoud and Richard van Leeuwen, *Arabic Studies in the Netherlands: A Short History in Portraits, 1580–1950*, Brill, Leiden, 2014.

[19] Snouck Hurgronje, Christiaan, 'Michaël Jan de Goeje', in: *Jaarboek der Koninklijke Akademie van Wetenschappen*, Amsterdam, 1909, 107–146. 117.

[20] Goeje, Michael Jan de, *Specimen literarium inaugurale exhibens descriptionem al-Magribi suntam e libro regionum al-Jaqubii versione et annotatione illustratam*, Brill, Leiden, 1860.

[21] UBL: BPL 2389, Michael Jan de Goeje to Theodor Nöldeke, 13 August 1864.

After their first meeting in the late 1850s, Nöldeke and De Goeje remained close friends until the latter's death in 1909. Two years earlier, Nöldeke had written De Goeje about their 50-year friendship: 'My dear [friend], 50 years […] have passed since we first met. There was an immediate connection between us, even if we did not yet know how close and lasting our friendship would become. […] I still often think with exceptional fondness of your lovely mother and her cosy home. It is a long, long time ago, but it often feels as if it was only yesterday'.[22] In the half century before this letter, the scholars had been sharing not only scholarly insights but intimate details about their private lives, as well, such as a young De Goeje did about his love life:

> I would love to introduce you to a couple of happy people, who cannot think about sad things, for whom war does not exist, who cannot even be saddened by the cholera. One of the two is probably completely unknown to you. It is a charming, lovely girl, with sensible and sweet eyes, with beautiful hair, with a musical voice. […] The other might be better known to you, maybe you even have his portrait in your album. […] he is as happy as he never dreamed he would ever be. Indeed, this happy young man is none other than your old friend, Jan de Goeje. At the moment our happiness is like an oasis in the desert. The cholera besieges our city and all the people are dreary and sad. […] And now people throng together from all directions to lavish themselves with the sight of a few blissful people, who don't know gloom and dreariness and who are not capable to believe in sadness.[23]

He would marry this girl, Wilhelmina Leembruggen, the next year.

They not only discussed happy events like this. When De Goeje reached the age of retirement, he shared his worries about his imminent mental and physical decline: 'Yes, the time of retirement is coming into view. How I think about it now, I hope that life will draw to an end as well. I do at least hope that I will not have to subsist as a caricature of myself. I wish you the same, my old loyal friend'.[24] They also discussed the lives and health of mutual friends, such as the Cambridge orientalist William Wright: 'If Wright […] would die! Terrible! Then *both of us* will have to promise each other to stay alive for a really long time'.[25] Portraits of both Nöldeke and Wright were also framed on De Goeje's wall.[26] Meanwhile the lighter sight of things was not neglected either.

[22] UBL: BPL 2389, Theodor Nöldeke to Michael Jan de Goeje, 14 October 1907.
[23] UBL: BPL 2389, Michael Jan de Goeje to Theodor Nöldeke, 12 June 1866.
[24] UBL: BPL 2389, Michael Jan de Goeje to Theodor Nöldeke, 2 February 1904.
[25] UBL: BPL 2389, Theodor Nöldeke to Michael Jan de Goeje, 20 June 1888. Nöldeke's emphasis.
[26] UBL; BPL 2389, Michael Jan de Goeje to Theodor Nöldeke, 14 November 1896.

Nöldeke sent German stamps to Leiden for the collection of De Goeje's younger brother, while De Goeje sent stamps from the Netherlands and the Dutch Indies to Nöldeke and his family.[27]

As a trusted friend Nöldeke was the right man to critically assess De Goeje's scholarly work. But even among friends it was not always obvious what should be shared in private and what could be discussed in public. In the 1860s, for example, Nöldeke sent his friend long lists of comments, such as notes on his al-Balādhūrī edition and his *Historia khalifatus Omari II Jazídi iI et Hischámi*.[28] To the great disappointment of De Goeje, Nöldeke also published some of these remarks in a book review.[29] De Goeje, however, insisted that these observations should have only been shared in private. In later years, Nöldeke would not just support De Goeje through private letters, he would also provide this service to some of his friend's most promising students, such as Gerlof van Vloten: 'I have not made any text corrections in my announcement; that I have sent [Van] Vl[oten] himself a list, he will have told you'.[30] Nöldeke's most time-consuming evaluative support, however, was his proofreading of long sections of the al-Ṭabarī edition.

Collaborating on the *Annals* of al-Ṭabarī

The History of Prophets and Kings by Muhammad ibn Jarir al-Ṭabarī, commonly referred to as the *Annals* of al-Ṭabarī was a revolutionary work of historiography. Completed in the 10th century, it was the first universal history that covered the whole period between the creation and the author's own time.[31] Between the 17th and the early 19th century fragments of the *Annals* had already been published.[32] The most comprehensive edition available during De Goeje's student days was Johann Ludwig Kosegarten's three-volume edition of a Berlin al-Ṭabarī manuscript.[33] The fact that De Goeje's edition would eventually consist of three series of books instead of three volumes shows just how daunting a task he had undertaken. The first series counted six books, the second one three, and the fourth series consisted of four books. In total they contained over 8000 pages of

[27] UBL: BPL 2389, Michael Jan de Goeje to Theodor Nöldeke, 9 December 1863, Theodor Nöldeke to Michael Jan de Goeje, 4 February 1864 and 1 Mai 1864.
[28] UBL: BPL 2389, Michael Jan de Goeje to Theodor Nöldeke, 13 December 1865.
[29] Nöldeke, Th., 'Historia khalifatus Omari II Jazídi II et Hischámi,' *Göttingische gelehrte Anzeigen*, 44, 1865, 1747–1753.
[30] UBL: BPL 2389, Michael Jan de Goeje to Theodor Nöldeke, 14 November 1900.
[31] Osman, Ghada, 'Oral vs. Written transmission: The case of Tabarī and Ibn Sa'd,' *Arabica*, 48(1), 2001, 66–80. 66.
[32] Muth, Franz-Christoph, *Die Annalen von at-Ṭabarī im Spiegel der europäischen Bearbeitungen*, Peter Lang, Frankfurt, 1983. 1–3.
[33] Kosegarten, Joannes Godefredus Ludovicus (ed.), *Annales regum atque legatorum dei ex codice manuscript berolinensi*, Ernst Mauritius, Greifswald, 1831–1853.

text. De Goeje also assembled two additional volumes with indexes, *addenda et emandanda* and a glossary.

Kosegarten's edition had been based on only one manuscript that was neither of a particularly high quality nor remarkably authentic. Still, such editions seemed to be the best that could be achieved at the time because no complete copies of the *Annals* were known. In 1858 De Goeje first discussed the possibility to nevertheless publish a complete edition.[34] Shortly after obtaining his doctorate he published a short note in the *Zeitschrift der Deutschen Morgenländischen Gesellschaft* about some fragments of the *Annals* that he had discovered in the Bodleian Library. He concluded that this Oxford manuscript would 'be of great service to the future editor of Tabari'.[35] Almost three years later he starting dreaming out aloud of one day doing this himself: '[…] it is a shame that Tabari is still unpublished. Maybe I will take it upon me one day'.[36] At this moment, the task was still too ambitious for him. After all, he was only 29 years old and not yet sure of the path that his career would take.

Though he never lost his interest in the *Annals*, he did not seriously plan an edition of the full work before December 1872. A few days before Christmas he received a letter from the Basler orientalist Albert Socin. Socin's former teacher, the theologian Johann Jakob Stähelin, was willing to spend 'a considerable sum' on the publication of the *Annals*.[37] Socin could not undertake this project himself, but his colleague Otto Loth had told him that De Goeje had a vivid interested in al-Ṭabarī's work. It was not difficult to persuade him and in early 1873 he enthusiastically discussed his plans with a number of scholars.[38] One year later the first outlines of what was to become an international al-Ṭabarī consortium started to take shape. Nöldeke reluctantly agreed to edit a section and Loth enthusiastically joined the enterprise.[39] By this time, Nöldeke's former student Eduard Sachau and the German orientalist and diplomat Andreas David Mordtmann had started hunting for manuscripts in Constantinople.[40] Over the following couple of years, a large number of German, Dutch, French, Austrian, Italian and other scholars would join the consortium.

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[34] Snouck Hurgronje, 'Michaël Jan de Goeje,' 129.
[35] Goeje, J. de, 'Literarische Notiz,' *Zeitschrift der Deutschen Morgenländischen Gesellschaft* (hereafter *ZDMG*), 16, 1862, 759–762.
[36] UBL: BPL 2389, Michael Jan de Goeje to Theodor Nöldeke, 25 August 1865.
[37] UBL: Or. 5585e, Albert Socin to Michael Jan de Goeje, 22 December 1872.
[38] For example, see UBL: BPL 2389, Michael Jan de Goeje to Theodor Nöldeke, 17 March 1873; UBL: Or. 5585e Theodor Nöldeke to Michael Jan de Goeje, Arist Aristovich Kunik to Michael Jan de Goeje, 27 February 1873, and Eduard Sachau to Michael Jan de Goeje, 10 May 1873.
[39] UBL: BPL 2389, Theodor Nöldeke to Michael Jan de Goeje, 7 April 1874 and UBL: Or. 5585e Otto Loth to Michael Jan de Goeje, 22 February 1874.
[40] UBL: Or. 5585e, Eduard Sachau to Michael Jan de Goeje, 23 October 1873, Andreas David Mordtmann to Michael Jan de Goeje, 6 March 1874.

Nöldeke ended up doing much more than he had foreseen. Not only did he edit the section of the *Annals* about Sassanid history, he also published a German translation of it.[41] During the more than twenty years that De Goeje coordinated the endeavour Nöldeke contributed in other ways as well. Before anything had been published, he had already been involved with drawing up the editorial guidelines.[42] Since there was no generally accepted template for publishing Arabic texts, De Goeje had to create his own guidelines on issues such as the use of diacritical points, the way to refer to Quranic and other quoted and paraphrased texts, the information to be provided in footnotes, and the criteria for the collection of words and phrases for the glossary. Though most of Nöldeke's suggestions were accepted by De Goeje, he did not adopt all of them. Nöldeke was, for example, strongly opposed to the idea that editors would be asked to collect words and phrases for the glossary. He argued that this would be almost impossible for him, since he did not have any proper Arabic lexicons at his disposal. Still, De Goeje asked all editors to record proper names, place names and proverbs. He only mitigated his request for the collection of not previously recorded words. He asked his collaborator to collects such words only if this would not cause too much trouble, and he postponed the final decision about the form and shape of the indexes and glossary.[43]

Evaluating the minutiae of Arabic texts

Nöldeke's involvement with the actual editing was even more time-consuming than his contributions to the editorial guidelines. Not only did he edit hundreds of pages himself, he also worked as an unofficial, unpaid proofreader for texts edited by others. A big messy folder with correspondence about the *Annals* in De Goeje's papers contains Nöldeke's comments on the contributions of, among others, Ignazio Guidi and Pieter de Jong.[44] Their work was of course not only evaluated by Nöldeke, who later recounted how De Goeje himself was closely involved with the work of all his collaborators: '[During the making of Tabari], De Goeje, who indeed is the foremost living Arabist, supervised every single thing, and because four eyes always see more than two, much was improved even with the best collaborators'.[45] Long after De Goeje's death, Nöldeke

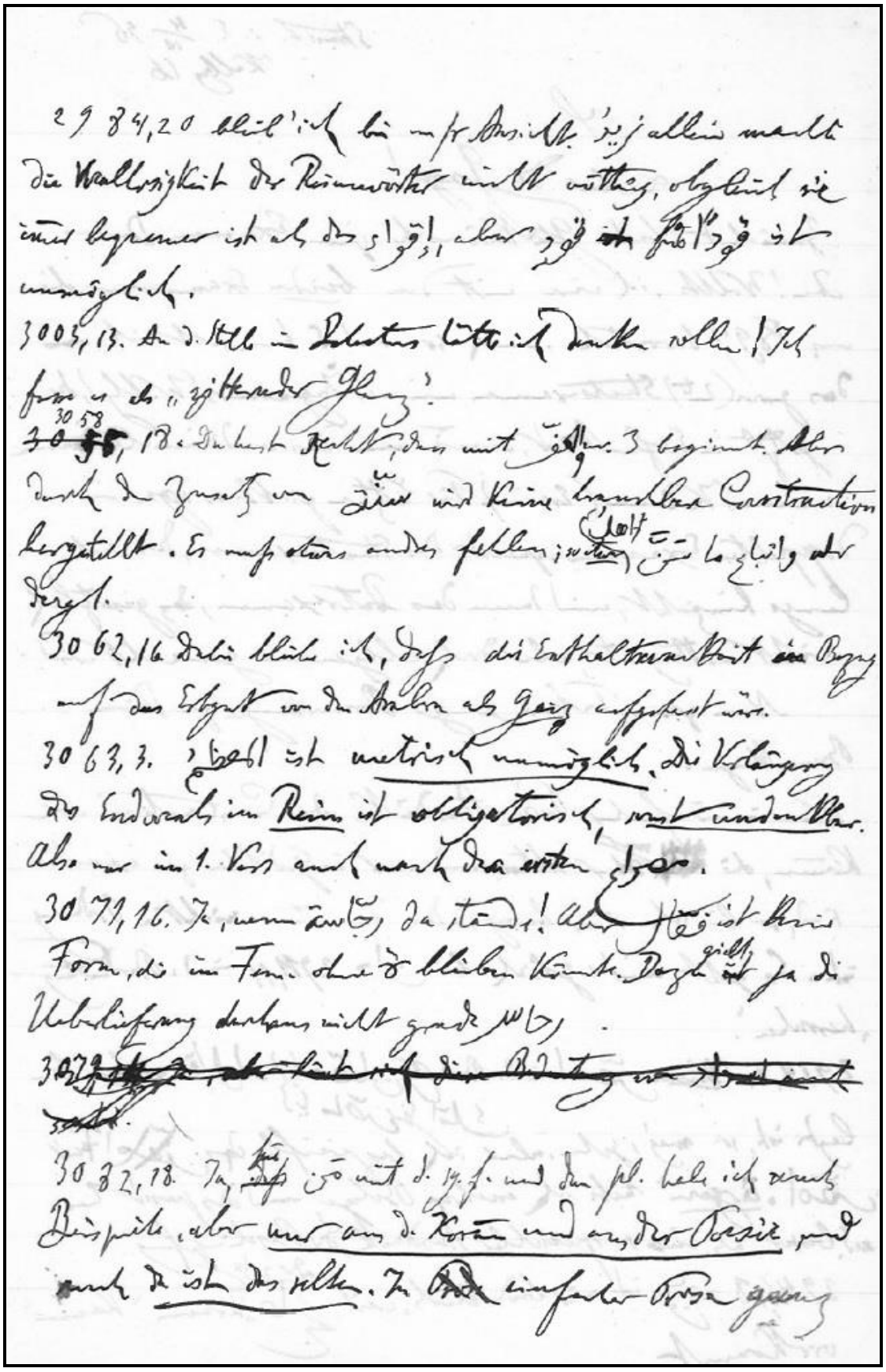
[41] Nöldeke, Theodor, *Geschichte der Perser und Araber zur Zeit der Sasaniden: aus der Arabischen Chronik des Tabari übersetzt und mit ausführlichen Erläuterungen und Ergänzungen versehn*, Leiden, Brill, 1879.
[42] UBL: Or. 5585f, Theodor Nöldeke to Michael Jan de Goeje, 21 June 1876.
[43] As shows in a comparison between: UBL: Or. 5585f, Allgemeine Bestimmungen für die Herausgabe des Tabarî and UBL: Or. 5585f, Theodor Nöldeke to Michael Jan de Goeje, 21 June 1876.
[44] On Ignazio Guidi: UBL: 5585f, undated notes in Theodor Nöldeke's handwriting about the second series of al-Ṭabarī's *Annals*, 978–1275; On Pieter de Jong: UBL 5585f, Theodor Nöldeke to Michael Jan de Goeje, 26 September 1876.
[45] Theodor Nöldeke to Eduard Meyer, 27 July 1907, in: Audring, G., *Der Briefwechsel zwischen Theodor Nöldeke und Eduard Meyer*.

would still compare his editorial merit favourably against the editing done by Sachau of the works of Ibn Sa'd: 'That De Goeje deserves more credit for Tabari than Sachau for Ibn Sa'd is obvious, especially to someone who has examined both great works, meticulously'.[46] Following Nöldeke's reasoning, De Goeje could benefit from a second pair of eyes for the large parts of the *Annals* that he had also edited himself. Nöldeke took this upon himself and went through hundreds, if not thousands, of pages of al-Ṭabarī's text that had been edited by De Goeje.[47] His commentary was formulated in the way that De Goeje liked best: long lists of detailed comments.

These lists all shared the same format. At the top of each list, Nöldeke would indicate the part of the series on which he commented. The rest of such a list would show page and line numbers in the left margin, with the related commentary to the right. Sometimes, he only corrected a minor printing error. Often, however, his comments merited more elaboration (see Figure 1). This was largely caused by the fact that most manuscripts were not as clear as the editors may have wished. Sometimes this was due to the bad quality of the available copies, at other times it was caused by the idiosyncrasies of the Arabic script. Most manuscripts lacked vowel points and other diacritical marks. These were often missed, even in the manuscripts that De Goeje and Nöldeke judged to be of high quality, such as an *Annals* fragment kept in Leiden, which Nöldeke described as 'a copy of a *very good* codex made by a completely ignorant copier, which, however, has only few diacritical points and vocals'.[48] Yet, these points and vocals would have been very useful, especially because all the *Annals* manuscripts contained a large number of hitherto unknown words and proper names.

In some cases, the challenge was not only to reconstruct the original text without the benefit of diacritical marks, but also to judge the authenticity of the diacritical marks in the available manuscripts. In the same letter as the one quoted above Nöldeke complained that it was '[...] inconvenient that various later hands have added diacritical points to the manuscript, which cannot always easily be distinguished from the original hand.' The lack of diacritical marks and reasonable doubts about their authenticity left ample room for doubts about the correct transcription. Therefore, one recurring point of discussion between Nöldeke and De Goeje concerns linguistic details, such as the requirement to either delete or add diacritical marks like the *sukūn* and the *tanwīn*.[49]

[46] UBL: Or. 5982 A:768, Theodor Nöldeke to Christiaan Snouck Hurgronje, 20 June 1922.
[47] Not all of his lists of comments are dated. Some of the ones that have been dated are: UBL: Or. 5585f, Theodor Nöldeke to Michael Jan de Goeje, 1 April 1885 and 4 October 1896.
[48] UBL: Or. 5585f, Theodor Nöldeke to Michael Jan de Goeje, 20 February 1876. Nöldeke's emphasis.
[49] For comments on the *sukūn*, see: UBL: Or. 5585f, undated list of comments on series III, pages 1923–2278; for comments on the *tanwīn*, see: UBL: Or. 5585f, undated list of comments on series II, pages 978–1275.



List of comments by Theodor Nöldeke about De Goeje's Annals of al-Ṭabarī. Source: UBL: Or. 5585f, Theodor Nöldeke to Michael Jan de Goeje, 4 October 1896.

The most common way to resolve vagueness and ambiguity in manuscripts was comparison with other texts. The *Annals* contain so many quotations that part of al-Ṭabarī's work is best described as that of a compiler or editor.[50] He compiled material from different genres. Some of it is drawn from earlier historiographers.[51] In other places, al-Ṭabarī extensively quotes ancient poetry, which often contained historical narrative, as well.[52] Other works that the editors could fall back on were those by later Arabic scholars who quoted al-Ṭabarī. Nöldeke could, for example, finish parts of his work without having all of al-Ṭabarī's text at his disposal because these fragments were copied in the works of ʿAlī Ibn al-Athīr.[53] He also consulted the work of other Arabic chronicles and geographical works, some of them preceding al-Ṭabarī, such as al-Balādhurī, others partially based on his *Annals*, such as those by Ibn Khaldūn and al-Masʿūdī.[54] He proposed further changes to al-Ṭabarī's postscript on the basis of his reading of Ibn Hisham and recommended corrections of Guidi's text after a comparison with, among other texts, the works of Yāqūt and Ibn Zubayr.[55] Probably the best-known source that al-Ṭabarī used, was the Quran; doubts about the meaning and orthography of words that could also be found there, were resolved easily.[56]

Not all Nöldeke's proposed corrections were based on a one-on-one comparison between al-Ṭabarī's text and those of other authors. Often, his comments were grounded in a general understanding of Arabic writing styles. Such observations had to take the fact into account that al-Ṭabarī had used the work of many authors writing in varying genres. The *Annals* contain short single sentence reports and medium-sized reports of a few dozen lines, as well as more extensive longer reports.[57] Many of these are written in prose, but the work also contains vast quantities of historical poetry.[58] These sections are all written in the literary style, specific to their own genre. Ancient pre-Islamic epics and odes, for instance, are known for their highly stylised language: 'The number and complexity of the measures which they use, their established laws of quantity and rhyme, and the uniform manner in which they introduce the subject of their poems […] all point

[50] Shoshan, Boaz, *Poetics of Islamic Historiography: Deconstructing Ṭabarī's History*, Brill, Leiden, 2004. xxxi.
[51] Osman, Ghada, 'Oral vs. Written Transmission,' 66–67.
[52] Beeston, A.F.L. and Lawrence I. Conrad, 'On Some Umayyad Poetry in the History of al-Ṭabarī,' *Journal of the Royal Asiatic Society*, Series 3, 3(2), 1993, 191–206. 191.
[53] UBL: Or. 5585f, Theodor Nöldeke to Michael Jan de Goeje, 13 October 1875.
[54] UBL: Or. 5585f, Theodor Nöldeke to Michael Jan de Goeje, 9 February 1876.
[55] UBL: Or. 5585f, Theodor Nöldeke to Michael Jan de Goeje, 17 March 1891; UBL: Or, 5585f, undated notes in Theodor Nöldeke's handwriting about the second series of al-Ṭabarī's *Annals*, 978–1275.
[56] UBL: Or. 5585f, Theodor Nöldeke to Michael Jan de Goeje, 1 April 1885.
[57] Shoshan, Boaz., *Poetics of Islamic Historiography*, xxix.
[58] Beeston, A.F.L. and Lawrence I. Conrad, 'On Some Umayyad Poetry,' 191.

to a long previous study and cultivation of the art of expression and the capacities of their language'.[59]

Even when the aesthetic demands of poetry did not apply, Nöldeke would sometimes reject some of the other authors' proposals because their suggested wording was simply 'barbaric' and could therefore not be authentic.[60] Other words and grammatical constructions were rejected by Nöldeke because they were too stilted to suit the plain prose texts in which they appeared.[61] Sometimes his sense of language suggested that certain words and constructions had to be changed, not because they were stylistically improper, but because they were uncommon and a rather obvious alternative existed.[62] If this obvious alternative was clearly different from the text in the available copies of the manuscript, he assessed their authority as less trustworthy than his own critical judgement: 'How limited is the authority of codices in these matters!'[63] However, even with his well-developed sense of language Nöldeke had to admit that quite some excerpts remained incomprehensible.[64] This is hardly surprising, since much of the poetry in the *Annals* still conjures up varying interpretations among modern-day scholars.[65]

The attempts to reconstruct ancient poetry were further aided by rhyme and metre. Nöldeke corrected De Goeje several times after he had taken a close look at the rhyme.[66] He more often referred to metre, however, because this was one of De Goeje's main weaknesses. Shortly after having finished his studies, he already acknowledged the incompleteness of Dozy's teaching: 'Dozy read a lot with us, but teaching grammar was not to his taste. By now I have learned it the hard way, but not sufficiently yet. I will amend it, however, just like my knowledge of metrics, which could have been more comprehensive, too'.[67] At the end of his career, metrics was still one of his least favourite subjects: 'I hate didactic poetry and books about metrics'.[68] Almost half a century later, Nöldeke had nothing but praise for De Goeje's grammatical advances, although he also noted that De Goeje had never fully caught up with the knowledge on metrics: '[…] it should be highly

[59] Sir Charles Lyall in his *Ancient Arabian Poetry* as quoted in: Nicholson, Reynold A., *A Literary History of the Arabs*, Second Impression, London, T. Fischer Unwin, 1914. 75.
[60] UBL: Or. 5585f, Theodor Nöldeke to Michael Jan de Goeje, 26 September 1876.
[61] For example, see UBL: Or. 5585f, Theodor Nöldeke to Michael Jan de Goeje, 26 September 1876 and 4 October 1896.
[62] For example, see UBL: Or. 5585f, undated notes in Theodor Nöldeke's handwriting about the second series of al-Ṭabarī's *Annals*, 978–1275 and undated notes in Theodor Nöldeke's handwriting about the second series of al-Ṭabarī's *Annals*, 606–933.
[63] UBL: Or. 5585f, Theodor Nöldeke to Michael Jan de Goeje, 1 April 1885.
[64] For example, see UBL: Or. 5585f, Theodor Nöldeke to Michael Jan de Goeje, 26 September 1876.
[65] Beeston, A.F.L. and Lawrence I. Conrad, 'On Some Umayyad Poetry,' 192.
[66] For example, see UBL: 5585f, Theodor Nöldeke to Michael Jan de Goeje, 26 September 1876 and 4 October 1896.
[67] UBL: BPL 2389, Michael Jan de Goeje to Theodor Nöldeke, 25 August 1865.
[68] UBL: BPL 2389, Michael Jan de Goeje to Theodor Nöldeke, 11 February 1903.

respected, how De Goeje later found his way around grammatical refinement. Only in one respect he never became confident, in metrics, because he apparently never had an ear for music at all. Even in his last text editions […] some disruptions of the metre occur'.[69]

Nöldeke still commented on De Goeje's inadequate sense of metre nine years after his death to his student and successor Christiaan Snouck Hurgronje. But even during De Goeje's lifetime Nöldeke shared this one point of criticism with some trusted colleagues. To the Budapest orientalist Ignaz Goldziher he wrote: 'I have given [De Goeje] some text corrections, some of which he rightfully showed me to be false. It is strange, however, that De Goeje has so little sense of rhythm that violations of the metre still happen to him'.[70] In this light it is not surprising that remarks about metre are a recurring theme in Nöldeke's comments on excerpts of the *Annals* that had been approved by De Goeje.[71] Nöldeke's lists of comments often contained added remarks by De Goeje, as well. But, although these remarks show that he did not accept all of Nöldeke's corrections, such disagreement is not displayed in his handwritten comments in reaction to any metric proposal.

A quick look at Nöldeke's listed comments shows the extent to which his private evaluation shaped his colleagues' end product. The scholarly end product of most orientalists' endeavours was a text, whether it was a grammar, a chrestomathy, a textbook, or a text edition. These texts lend themselves pre-eminently to precise and exhaustive evaluation. The knowledge of colleagues could easily and immediately be incorporated. In this respect the evaluative practices of the orientalists proved to be quite different from those of the bacteriologists around Koch.

The need for clinical testing

The fact that the correspondence of Koch and his colleagues does not contain a large amount of pre-publication discussion of their findings does not mean that they had no interest in having their work evaluated before it was made public. On the contrary, they usually had better reason to have it meticulously checked than the Orientalists. The results of their scholarly efforts were often not texts, but new drugs and treatment regimens that could either cure or kill people. An important step in assuring the efficacy and safety of these new cures, was to test them not only on animals but on people, as well. Such tests are known as clinical trials. Overviews of the history of the clinical

[69] UBL: Or. 8952 A: 763, Theodor Nöldeke to Christiaan Snouck Hurgronje, 13 January 1918.
[70] Magyar Tudományos Akadémia Könyvtár Információs Központ (Library and Information Centre of the Hungarian Academy of Sciences, hereafter MTAK), GIL/32/01/156, Theodor Nöldeke to Ignaz Goldziher, 4 April 1904.
[71] For example, see UBL: Or. 5585f, Theodor Nöldeke to Michael Jan de Goeje, 26 September 1876 and 4 October 1896.

trial often start with James Lind's testing of potential cures for scurvy in 1747.[72] However, by the end of the 19th century, there was still no general consensus about the requirements of clinical testing. It took a widely discussed affair involving Germany's most famous and influential bacteriologist, Robert Koch, to convince his colleagues of the importance of extensive and meticulous clinical trials.

In 1890 the tenth International Medical Congress met in Berlin. The highlight of this meeting was Koch's announcement that he had found a cure for tuberculosis, one of the most widespread deadly diseases of 19th-century Europe. Later that year he published his findings about the active substance, which he had named *Tuberkulin*, in the *Deutsche Medizinische Wochenschrift*.[73] The news was received with great enthusiasm: German newspapers reported Koch's accomplishment with patriotic pride and people suffering from tuberculosis from all over the country gathered in Berlin hoping to get their hands on this new miraculous cure.[74] Soon, however, the enthusiasm started to wear off. The Berlin pathologist Rudolf Virchow argued that *Tuberkulin* accelerated rather than terminated the pathological process and the Breslau clinician Ottomar Rosenbach showed that the drug might have dangerous side effects.[75] The fact that Koch could neither produce the guinea pigs that he had supposedly cured nor the exact composition of *Tuberkulin* did not help his cause either.[76] The Prussian state authorities, who were keenly interested in the new cure, repeatedly emphasised the unfinished nature of the *Tuberkulin* research programme in their internal communication.[77]

In the end *Tuberkulin* failed to live up to the initial expectations of the state, the press, tuberculosis patients and the medical community. Still the initial enthusiasm had helped Koch to take a next step in his career: he had secured state support for his own research institute, the *Institut für Infektionskrankheiten* (Institute for Infectious Diseases). The reputation of the relatively young discipline of bacteriology, however, was badly tarnished. Therefore, the first people to make a new, potentially revolutionary, discovery while working in Koch's new research institute knew that they

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[72] For example, see Kleist, P. and C. Zerobin Kleist, 'Eine kurze Geschichte der klinischen Studie,' *Schweizerische Ärztezeitung*, 86(44), 2005, 2475–2482. 2477; Lilienfeld, Abraham M., 'Ceteris paribus: the evolution of the clinical trial,' *Bulletin of the History of Medicin*, 56(1), 1982, 1–18. 4–5; Timmer, Antje, 'Kontrollierte klinische Studien vor Archie Cochrane,' *Zeitschrift für Evidenz, Fortbildung und Qualität im Gesundheitswesen*, 102, 2008, 473–481. 474–475.

[73] Koch, Robert, 'Weitere Mittheilungen über ein Heilmittel gegen Tuberkulose,' *Deutsche Medizinische Wochenschrift*, 16, 1890, 1029–1032.

[74] Elkeles, Barbara, 'Der »Tuberkulinrausch« von 1890,' *Deutsche Medizinische Wochenschrift*, 115 (45), 1990, 1729–1732. 1729.

[75] Gradmann, Christoph, 'A harmony of illusions: clinical and experimental testing of Robert Koch's tuberculin 1890–1900,' *Studies in History and Philosophy of Biological and Biomedical Sciences*, 35, 2004, 465–481. 474.

[76] Gradmann, Christoph, 'Robert Koch and the Pressures of Scientific Research: Tuberculosis and Tuberculin,' *Medical History*, 45, 2001, 1–32. 24–25.

[77] For example, see the letter of 27 October 1890 von Gustav von Goßler to the Kaiser and the opinion of chancellor Leo von Caprivi of 25 December1890, both in: GStA PK, VI. HA, Nl Althoff, No.313: Das Kochsche Tuberculin.

faced an uphill battle to convince their peers and the public of the truth and importance of these new findings.

This first new discovery was a blood serum that could be used both as a cure and as a prophylactic against diphtheria. The inventors were both assistants at Koch's institute: Erich Wernicke and, most importantly, Emil Behring. In a letter to a sympathetic paediatrician, Behring wrote that he hoped 'to make use of the experiences of *Koch* in the tuberculosis treatment and to be spared similar setbacks'. He finished his letter with the statement that he would '[...] *rather wait some more years with further publications, than present something doubtful now*.'[78] Looking back on these days, Wernicke also underlined the importance of Koch's fiasco for their serum research. He remembered how 'a major medical authority' dismissed their findings with a condescending comment: 'The serum is a slippery substance, on which its discoverers will slip'. Others recalled the fate of *Tuberkulin* and considered the diphtheria serum to be a 'similar bacteriological scam'.[79] It was obvious to Behring and Wernicke that it would not be easy to convince their peers of the merit of their discovery. Behring wrote in his diary that one should 'work on the emotions, not on reason, when one wants to carry away the crowd'.[80] Both men knew, however, that reason should not be overlooked: they needed compelling proof of the efficacy of their serum. Carefully conducted clinical trials seemed to be the most promising way to work on both the emotions and reason.

Testing the diphtheria blood serum

Emil Behring was born in 1854 in Hansdorf, a village in modern-day Poland. After receiving his medical doctorate at the University of Berlin in 1878 and his license to practice medicine in 1880, he worked as an army doctor in eastern Prussia.[81] Between 1887 and 1889 he was employed at Karl Binz's Pharmacological Institute in Bonn, after which he was sent to the Hygienic Institute at the University of Berlin, where he worked under Koch. When Koch moved to the Institute for Infectious Diseases in 1891 Behring was appointed as one of his assistants. At the Hygienic

[78] Emil Behring to Otto Heubner, 8 May 1893, Behring-Nachlass digital (hereafter BNd) (http://www.uni-marburg.de/fb20/evbb/behring-digital), EvB/B1/54. All emphases by Behring.
[79] Erich Wernicke to Thorwald Madsen, [no date], BNd, EvB/F5/1. There is no date mentioned on this letter, but it must have been written between 1921 and 1924. At the end of the letter Wernicke mentions his work in Landsberg, which he started in 1921. The letter has been attached to a letter to Bernhard Möllers sent in 1924: Erich Wernicke to Bernhard Möllers, 29 August 1924, BNd, EvB/F5. Linton states that it was 'apparently written in the early 1930s'. However, not only does this not fit with the date of Wernicke's letter to Möllers, it also does not fit in with the fact that Wernicke died in 1928: Linton, Derek S., *Emil von Behring: Infectious Disease, Immunology, Serum Therapy*, American Philosophical Society, Philadelphia, 2005.
[80] Zeiss, H. and R. Bieling, *Behring: Gestalt und Werk*, Bruno Schultz, Berlin-Grunewald, 1940. 103.
[81] This short overview of Behring's life draws from: Linton, *Emil von Behring: Infectious Disease, Immunology, Serum Therapy* and Zeiss and Bieling, *Behring: Gestalt und Werk*.

Institute he had already met Erich Wernicke. Wernicke was five years his junior and was an army doctor as well.[82] In 1891 he was also appointed at the Institute for Infectious Diseases. Here Wernicke found himself 'caught by Behring's towering idiosyncratic character,' and spent a large amount of time on Behring's antiserum studies.[83] This support was badly needed because, although Behring was the driving force of the research programme, his poor health kept him from performing crucial animal testing. Although the results of these tests were promising, they did not convince the medical establishment of the serum's merit. Therefore, clinical tests were necessary. The first tests were carried out in the clinic of the Berlin paediatrician Ernst von Bergmann, in December 1891.[84] The tested children were not cured, however, and even if they would have recovered, their number would have been too low to be statistically relevant. The first clinical trials on a larger scale would take place one year later under the supervision of the Leipzig paediatrician Otto Heubner.

Otto Heubner was born in 1843 in a village in the south of Saxony.[85] Educated as an internist, the large number of children visiting the District Policlinic in Leipzig, where he worked from 1876 onwards, pushed him in the direction of paediatrics. In 1886, he was appointed as Professor of Paediatrics in Leipzig and, two years later, he founded a children's hospital with the donations of wealthy patients and other sponsors. Since diphtheria mostly effects children and Heubner's management of the hospital was widely praised by his contemporaries, he was a very suitable collaborator on the first large-scale clinical trials of the new serum. Some other doctors were involved in the trials, as well; Behring mentioned the Berlin paediatrician Eduard Heinrich Henoch in one of his letters.[86] Heubner, however, made most of the observations of the effect that the early versions of the serum had on people.[87] Especially in 1892, Behring flooded him with requests to test new versions of his serum and to answer a myriad of questions about their effects.

[82] More about Wernicke can be found in: Schulte, Erika, *Der Anteil Erich Wernickes an der Entwicklung des Diphtherieantitoxins*, Mensch & Buch, Berlin, 2001.
[83] Erich Wernicke to Thorwald Madsen, [no date], BNd: EvB/F5/1.
[84] These first tests are mentioned in: Erich Wernicke to Thorwald Madsen, [no date], BNd: EvB/F5/1. Zeiss and Bieling say they are 'a legend': Zeiss, H. and R. Bieling, *Behring: Gestalt und Werk*, 85. Based on the letter to Madsen and some notes Wernicke added to a picture of Behring and himself Oedingen and Staerk support December 1890 as the moment of the first test of the serum on people: Oedingen, Christina and Joseph W. Staerk, 'First Cure for Diphtheria by Antitoxin as Early as 1891,' *Annals of Science*, 54, 1997, 607–610. Linton is inconclusive: Linton, *Emil von Behring: Infectious Disease, Immunology, Serum Therapy*, 112–116.
[85] Heubner's biographical details have been drawn from: Goerke, Heinz, *Berliner Ärzte: Virchow, Graefe, Koch, Leyden, Bergmann, Bier, Heubner, Moll, Stoeckel*, zweite Auflage, Berlin Verlag, Berlin, 1984. 192–198.
[86] Emil Behring to Otto Heubner, 20 July 1892, BNd: EvB/B1/57.
[87] August Laubenheimer to Aufsichtsrat der Farbwerken Vormals Meister, Lucius & Brünning, 20 April 1894, BNd: EvB/B196/13.

One question that Behring repeatedly asked, was if Heubner could figure out if the antiserum provided a specific cure against diphtheria.[88] Even if the test results on animals strongly supported this conclusion, the results of the clinical tests were not as straightforward.[89] One reason for this could be that some of the tested children suffered from other diseases, as well; diphtheria infections were often accompanied by streptococcus infections.[90] Another reason was that the serum could have a different effect on different groups of patients. Henoch, for example, did not test the serum on seemingly mild or beginning cases of diphtheria.[91] The fact that a relatively large number of the people he tested — who all had a negative prognosis to begin with — were not cured, not necessarily proved anything about the efficacy of the serum on milder and more recent infections. Behring therefore asked Heubner to make sure that he would test the serum on children with mild, medium and severe cases of diphtheria and that he would make a clear distinction between the results in these three categories.[92]

It was not enough for Behring to know whether his serum was a specific cure for diphtheria. He also asked Heubner to establish the appropriate dosage. Because it was unlikely that there would be one dose that would cure diphtheria in both its earlier and later stages, he repeatedly asked to look for both the 'curative minimal dosage' and what increase in this minimal dosage would be effective in fighting the more advanced stages of the disease.[93] Behring was also interested in the serum's side effects.[94] Nevertheless, in the spring of 1893, he already happily concluded that the serum was 'absolutely safe' for human use.[95] Finally, Behring asked for statistical data about every circumstance that could be relevant in determining just how effective his serum was. Even if the test did not include a control group, as required in most modern clinical trials, Behring asked Heubner to also collect data on children that he had treated for diphtheria before the blood serum was available to him.[96]

By the end of 1892, the results from Heubner's tests were still not decisive. Although they showed that the serum was safe, they did not provide clear indications for the optimal dosage. Behring

[88] Behring asked about this issues, for example, on 11 November 1892, 23 December 1892, and 12 April 1893: BNn: EvB/B1/41, EvB/B1/46, and EvB/B1/52.
[89] August Laubenheimer to Aufsichtsrat der Farbwerken Vormals Meister, Lucius & Brünning, 20 April 1894, BNd: EvB/B196/13.
[90] For example, see Emil Behring to Otto Heubner, 9 April 1893, Sammlung Darmstaedter in der Staatsbibliothek Berlin (hereafter Slg. Darmstaedter): 3a 1890 (5) and Emil Behring to Otto Heubner, 9 May 1893, BNd: EvB/B1/56.
[91] Emil Behring to Otto Heubner, 4 May 1893, BNd: EvB/B1/53.
[92] Emil Behring to Otto Heubner, 8 May 1893, BNd: EvB/B1/54.
[93] Emil Behring to Otto Heubner, 5 December 1892 ,23 December 1892, and 4 May 1893. BNd: EvB/B1/42, EvB/B1/46, and EvB/B1/53.
[94] Emil Behring to Otto Heubner, 5 December 1892, BNd: EvB/B1/42.
[95] Emil Behring to Otto Heubner, 12 April 1893, BNd: EvB/B1/52.
[96] Emil Behring to Otto Heubner, 8 May 1893, BNd: EvB/B1/55.

could not even rule out the possibility that the serum was not effective at all! Still, the provisional results from Heubner's testing were promising enough for Behring and Wernicke to find outside support. First of all, the *Hoechster Farbwerken*, one of Germany's major chemical manufacturers, showed an interest in producing the serum. The *Farbwerken* committed themselves to funding further, large-scale research in 1893. If the results of this research would be promising enough, they pledged additional investments in the serum's development and marketing.[97] At the same time Paul Ehrlich, another member of the *Institute for Infectious Diseases*, teamed up with Behring and Wernicke to investigate ways to determine the effectiveness of the serum.[98] He was indeed able to improve on Behring's and Wernicke's earlier efforts. While he was reluctant to give him too much credit, twenty-five years later, even Wernicke had to admit that 'nobody will question [Ehrlich's] epochal genius […] in relation to establishing the impact (*Wertbestimmung*) of serums'.[99]

The combination of the findings from new clinical trials in Berlin and the efforts of Paul Ehrlich, convinced August Laubenheimer, member of the board of the *Farbwerken*, to deliver a positive verdict on the serum's efficacy and commercial viability.[100] The commercial viability of the serum production was further confirmed in a discussion of the trials at the Imperial Health Office in Berlin. About fifteen doctors, among whom Behring, Ehrlich and Koch, convened at a meeting chaired by the director of the office, Dr Karl Köhler and attended by *Ministerialdirektor* Friedrich Althoff. Only after all attendees had lavishly praised the efficacy and safety of the serum, it was decided that it should be made available at pharmacies as a prescription drug.[101] *Hoechster Farbwerke* quickly followed up on Laubenheimer's advice from earlier that year. From November 1894 onwards the company would ensure the serum's availability to the public. The festive opening of their brand-new productions facility was attended by, among others, Behring, Ehrlich, Koch, Köhler and Althoff.[102]

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[97] Report from August Laubenheimer, copied in: Carl Ludwig Lautenschläger to das Behring-Archiv, Marburg, Alexander von Engelhardt, 14 February 1941, BNd, EvB/B 196/7.
[98] Hüntelmann, Axel C., 'Diphtheria serum and serotherapy. Development, Production and regulation in *fin de siècle* Germany,' *Dynamis*, 27, 2007, 107–131. 113–114.
[99] Erich Wernicke to Bernhard Möllers, 29 August 1924, BNd, EvB/F5.
[100] August Laubenheimer to Aufsichtsrat der Farbwerken Vormals Meister, Lucius & Brünning, 20 April 1894, BNd: EvB/B196/13. For more on Ehrlich's contribution to the diphtheria serum, see: Bäumler, Ernst, *Paul Ehrlich: Forscher für das Leben*, 3., durchgesehene Auflage, Edition Wötzel, Frankfurt am Main, 1997. 92–93 and Hüntelmann, Axel C., *Paul Ehrlich: Leben, Forschung, Ökonomien, Netzwerke*, Wallstein Verlag, Göttingen, 2011. 91–108.
[101] Beratung betreffend das Diphterieserum, [o. Datum; sicher aber am 3.11.1894), BNd, EvB/B196/5.
[102] Linton, *Emil von Behring*, 179.

Evaluating texts and serums: a comparison

A comparison between Heubner's evaluation of Behring's serum and Nöldeke's evaluation of De Goeje's texts allows for a better understanding of the characteristics — or even idiosyncrasies — of both evaluative processes. The processes share some striking features. Both Nöldeke and Heubner dealt intensively with the work of a colleague over a period of several years. Both were tasked with pinpointing shortcomings and confirming the strengths of the results of many years of research before they were shared with the public at large. Both did this for free without clearly defined expectations of personal benefit. The differences between what Nöldeke and what Heubner did, however, are at least as striking as the similarities. It could, for example, be argued that their motivations were very different; Nöldeke was at least partly motivated by his longstanding personal friendship with De Goeje, while Heubner was probably motivated by his frequent exposure to the suffering caused by diphtheria. In addition to the possibly different motivations of Nöldeke and Heubner, there are also three other interrelated differences..

The first of these is the fact that the objects of evaluation are very dissimilar. De Goeje and his collaborators produced thousands of pages of text and Nöldeke took it upon himself to closely evaluate them. Behring and his collaborators produced reports of their findings, too, and these texts were duly published in relevant journals or as independent volumes.[103] Behring, however, did not ask Heubner or Ehrlich to carefully read any of these texts. Instead, he asked Heubner over and over again to evaluate the efficacy and side effects of new versions of his serum. Likewise, he asked Ehrlich only to help him find ways to produce the serum more efficiently and to establish the strength of individual batches.[104] This emphasis on the evaluation of a material instead of a textual object required an effort that was very different from the task to which Nöldeke was committed.

The second difference between Nöldeke's and Heubner evaluations has to do with the extent to which they repeated the intellectual process through which the creator of the original work had gone. Nöldeke's evaluative efforts consisted of continually asking questions that were similar to those posed by De Goeje and his collaborators, namely 'Is this sentence grammatically correct?' 'Do the metre and rhyme in these lines follow an established pattern?' 'Could these words be be the same as differently spelled words in other texts?' His intellectual effort was, in fact, a reiteration

[103] For example, Behring, Emil, *Die Blutserumtherapie I: Die praktischen Ziele der Blutserumtherapie und die Immunisierungsmethoden zum Zweck der Gewinnung von Heilserum*, Georg Thieme, Leipzig, 1892 and Behring, Emil and Erich Wernicke, 'Ueber Immunisirung und Heilung von Versuchsthieren bei der Diphtherie,' *Zeitschrift für Hygiene*, 12, 1892, 45–57.
[104] Bäumler, Ernst, *Paul Ehrlich*, 93 and Hüntelmann, Axel C., 'The Dynamics of *Wertbestimming*,' *Science in Context*, 21(2), 2008, 229–252.

of the efforts of the editors of the text. Heubner, on the other hand, was not asked to repeat Behring's thought processes, at all. If Nöldeke was invited as a commentator because he was deemed to possess the same type of expertise as those whose work he was to review. Heubner's evaluation was important because he possessed additional expertise and resources. His task was not to assess the process that had led to the production of the serum, but to find out if the serum somehow did what its creators hoped it would do. The skills and resources he could provide were a long experience in observing and diagnosing diphtheria in people, the ability to manage large-scale clinical operations and a statistically significant number of diphtheria patients.

Finally, there are major differences in the social conditions that shaped both evaluative efforts. Text editing is a rather solitary endeavour. De Goeje's collaborators worked alone in their workrooms. They sent their work to the editor-in-chief, who would proofread it in the solitary confines of his own office. He would then send these manuscripts to Nöldeke who would also spend long days alone with them. It is exactly this chain of solitary work that makes it possible for reviewers and editors to more or less completely reiterate the intellectual processes of those who came before them. Behring's serum, however, was created in a laboratory setting, in close personal collaboration with colleagues such as Wernicke and Ehrlich and with the help of a large number of paid and unpaid assistants.[105] The testing of the serum took place in a hospital setting, which again required the cooperation of a large number of people. The social environment in which laboratory and clinical research took place shaped the way in which the scholarly work from such places could be evaluated. The following pages take a closer look at the evaluative practices in another laboratory setting, namely those at Wilhelm Wundt's Institute for Experimental Psychology.

Working with Wundt

Wilhelm Wundt was born in Neckerau, just outside of Mannheim.[106] When he was four years old, his parents moved to the small town of Heidelsheim. During his school years, he did not stand out; in primary school he was an absent-minded daydreamer and his high school teachers at the

[105] One of these unpaid assistants was Wernicke's fiancée, Meta Füth, who, while living in Friedeberg, not only fed some sheep bought by Wernicke, but also injected them with diphtheria. Much of the serum that Heubner received was produced under her supervision. Erich Wernicke to Thorwald Madsen, [no date], BNd: EvB/F5/1.
[106] The overview of Wundt's early years primarily draws from: Bringmann, Wolfgang G., Norma J. Bringmann and William D.G. Balance, 'Wilhelm Maximilian Wundt 1832–1874: The Formative Years,' in: Bringmann, Wolfgang G. and Ryan D. Tweney (eds.), *Wundt Studies: A Centennial Collection*, C.J. Hogrefe, Inc., Toronto, 1980, 13–32 and Diamond, Solomon, 'Wundt before Leipzig,' in: Rieber, Robert W. and David K. Robinson (eds.), *Wilhelm Wundt in History: The Making of a Scientific Psychology*, Springer Science + Business Media, New York (NY), 2001, 1–68.

Gymnasium thought he was dull-witted and lazy. Only after he was sent to the Lyceum in Heidelberg did he prove himself to be an above-average student. He spent his first year as a student enjoying cultural life at Tübingen, after which he went to Heidelberg to pursue his medical degree with more vigour.[107] After the successful completion of his medical studies, Wundt held an assistantship in Heidelberg with Ewald Hasse. After a short stay in the physiological laboratories of Johannes Müller and Emil Du-Bois Reymond in Berlin, he returned to Heidelberg to become the assistant of Hermann von Helmholtz. Shortly after leaving the latter's laboratory in 1863, Wundt was appointed as *Extraordinarius* at his alma mater. This job still did not provide him with a regular salary; however, he made a fairly comfortable living by writing textbooks.[108]

During these years his background in physiology and growing interest in philosophy started to converge. His main interests turned towards the cutting edge of the physiology of perception and the philosophy of mind. At this moment his future academic career was still very uncertain. In 1872 he wrote to his fiancée Sophie Mau: 'My actual scientific pursuits, specifically those that are done for science's sake and not just to make a living, mostly occupy a fringe area between physiology and philosophy, suspect among respectable scientists, where not much prestige can be gained at the moment'.[109] However, with his *Grundzüge der Physiologischen Psychologie* (Principles of Physiological Psychology), Wundt delivered a well-received exposition of his early psychological programme in 1874.[110] In the same year, the desired career switch from physiologist to philosopher finally occurred; he was appointed to the Chair of Inductive Philosophy at the University of Zürich.

He would leave Switzerland soon after that. Only one year after his arrival, he was called to Leipzig, where he was appointed at the Faculty of Philosophy of Leipzig University to study the relationship between philosophy and natural sciences. Here, he met two of the founding fathers of physiological psychology: Ernst Heinrich Weber and Gustav Theodor Fechner. Wundt and Fechner soon developed a cordial relationship. A few years after his arrival in Leipzig, Wundt established what is now seen as the first laboratory for experimental psychology, the *Institut für experimentelle Psychologie* (institute for experimental psychology). In the autumn of 1879 a growing number of students, such as Friedrich Tischer, Emil Kraepelin, G. Stanley Hall and James McKeen Cattell, spent more and more time on experimental projects under Wundt's supervision. In the light of this development

[107] Lamberti, Georg, *Wilhelm Maximilian Wundt (1832–1920): Leben, Werk und Persönlichkeit in Bildern und Texten*, Deutscher Psychologen Verlag, Bonn, 1995. 23–39.
[108] Bringmann, Wolfgang G., Norma J. Bringmann and William D.G. Balance, 'Wilhelm Maximilian Wundt', 27.
[109] UAL, Nl. Wilhelm Wundt, Wilhelm Wundt to Sophie Mau, 15 June 1872. (transcript accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[110] Wundt, Wilhelm, *Grundzüge der Physiologischen Psychologie*, Engelmann, Leipzig, 1874.

Wundt argued that the *de facto* establishment of the laboratory can be traced back to this autumn.[111] Though modern-day scholars disagree about the extent to which these developments can be perceived as the birth of psychology as an independent discipline, few would dispute the importance of this laboratory to the history of experimental psychological research.[112]

Though Wundt exchanged letters with many people, his correspondence hardly contains any in-depth discussion of the work done in his laboratory with people from outside his institute. His correspondence with Fechner is the main exception; between 1879 and Fechner's death in 1887, a lively exchange of ideas took place between the two main representatives of physiological psychology in Leipzig. The earliest preserved correspondence between them is about an issue on which they strongly disagreed, namely Henry Slade's spiritual abilities. Slade was a self-proclaimed medium, who arrived in Leipzig in 1877, after his claim of being able to mysteriously move objects through the channelling of unknown forces had been debunked in New York and London. He nevertheless caught the attention of the Leipzig astrophysicist Friedrich Zöllner, who organised sessions with Slade at his house.[113] Wundt and Fechner both attended at least once.[114] Wundt did not believe in the veracity of Slade's performances and attacked his trustworthiness in a scathing booklet.[115] Fechner, however, did not agree with Wundt's harsh judgement and defended Slade in a long private letter.[116] After a short to and fro, both men agreed to disagree. 'Why would we keep on arguing, because I would rather not quarrel with you about this issue, now that we have convinced each other that we cannot lecture each other about those things about which we disagree,' Fechner wrote.[117]

Now that the Slade discussion was out of the way, their correspondence would mostly be about the most famous legacy of Ernst Heinrich Weber, a principle known as Weber's law. This principle

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[111] Wundt, Wilhelm, 'Das Institut für experimentelle Psychologie,' in: Rektor und Senat der Universität Leipzig (eds.), *Festschrift zur Feier des 500 jährigen Bestehens der Universität Leipzig*, 4. Band, I. Teil, 1909, 118–133. 118.
[112] The argument for the establishment of Wundt's Leipzig laboratory as central to the establishment of psychology as an autonomous science is made in: Ben-David, Joseph and Randall Collins, 'Social factors in the origins of a new science: the case of psychology,' *American Sociological Review*, 31(4), 1966, 451–465; Bringmann, Wolfgang G. and Gustav A. Ungerer, 'The Foundation of the Institute for Experimental Psychology at Leipzig University,' *Psychological Research*, 42 (Wundt Centennial Issue), 1980, 5–18. A critical counter-argument has been made in: Métraux, Alexandre, 'Wilhelm Wundt und die Institutionalisierung der Psychologie: Ein Beitrag zu einem kontroversen Kapitel der Psychologiegeschichte,' *Psychologische Rundschau*, Band XXXI, 1980, 84–98.
[113] Staubermann, Klaus B., 'Tying the knot; skill, judgement and authority in the 1870s Leipzig spiritistic experiments,' *The British Journal for the History of Science*, 34(1), 2001, 67–79. 73–74.
[114] Ibid., 75.
[115] Wundt, W., *Der Spiritismus: eine sogenannte wissenschaftliche Frage: offener Brief an Herrn Prof. Dr. Hermann Ulrici in Halle*, Wilhelm Engelmann, Leipzig, 1879.
[116] Universitätsarchiv Leipzig (hereafter UAL), Nl. Wilhelm Wundt, Gustav Theodor Fechner to Wilhelm Wundt, 18 June 1879. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm).
[117] UAL, Nl. Wilhelm Wundt, Gustav Theodor Fechner to Wilhelm Wundt, 25 June 1879. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm).

states that 'the increase in any stimulus necessary to make a noticeable difference is a constant proportion of that stimulus'.[118] Because of his contributions to the further examination and dissemination of the principle, a modified version of Weber's law is nowadays known as Fechner's law.[119] Fechner's first discussions with Wundt about this subject occurred after a young Georg Elias Müller had sharply criticised the account of Weber's law given by Fechner in his *Elemente der Psychophysik*.[120] Further discussion, however, would mostly focus on the work done in Wundt's institute, where Weber's law provided the interprative framework for investigations into almost all forms of perception.

Not only did Fechner have a close relationship with Wundt, he also discussed Weber's law with Wundt's collaborators at his laboratory.[121] They did not, however, use these short lines of communication with Fechner to have their work evaluated by him before sharing it with a wider audience. Instead, it was Fechner who used his good relationship with Wundt and his students to make sure that his commentaries on what he perceived to be their misunderstanding of Weber's law would be appropriate. He shared his thoughts with the people he was criticising for at least two reasons. On the one hand, he realised that a good personal relationship could be damaged by unexpected harsh criticism. By sharing this criticism in advance, Fechner not only prepared his peers for the blow, he also gave them the chance to correct some of their mistakes before his comments would be publicised. If they decided not to correct anything, he would at least have given them the opportunity to prepare a well-thought-out response.[122] On the other hand, Fechner realised that such a pre-publication discussion would allow him to improve his own argumentation. He, for example, did send Wundt a draft paper in which he criticised Wundt's collaborators Volkmar Estel and Gustav Lorenz with the question of whether Wundt would be so kind as to

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[118] Blumenthal, Arthur L., 'Shaping a Tradition: Experimentalism Begins,' in: Buxton, Claude E. (ed.), *Points of View in the Modern History of Psychology*, Academic Press, Orlando, 1985, 51–83. 55.

[119] The search result for 'Weber's law' at the online version of the Oxford Dictionary of Biomedicine simply states 'see Fechner's law.' Lackie, John, Oxford Dictionary of Biomedicine, 2010, DOI: 10.1093/acref/9780199549351.001.00. For a more detailed account of Fechner's contributions to Weber's law, see: Heidelberger, Michael, Nature from within: *Gustav Theodor Fechner and his psychophysical worldview*, translated by Cynthia Klohr, University of Pittsburgh Press, Pittsburgh (PA), 2004. 200–207.

[120] Fechner, Gustav Theodor, *Elemente der Psychophysik*, Breitkopf und Härtel, Leipzig, 1860. Müller most famously published his criticisms in: Müller, Georg Elias, *Zur Grundlegung der Psychophysik: Kritische Beiträge*, Griebel, Berlin, 1878. Fechner's discussion of Elias's criticism with Wundt can be found in: UAL, Nl. Wilhelm Wundt, Gustav Theodor Fechner to Wilhelm Wundt, 27 October 1880 and 28 October 1880. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)

[121] Fechner did, for example, exchange views with Gustav Lorenz and Max Mehner: UAL, Nl. Wilhelm Wundt, Gustav Theodor Fechner to Wilhelm Wundt, 12 July 1885 and 13 April 1886. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)

[122] See, for example, UAL, Nl. Wilhelm Wundt, Gustav Theodor Fechner to Wilhelm Wundt, 24 March 1885 and 12 July 1885. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)

comment on his objections and counter-arguments.[123] When he was preparing a critical paper about the work by another Wundt associate, Max Mehner, Fechner shared an early version of this paper with both Wundt and Mehner.[124]

The bacteriological and the psychological laboratory: a comparison

Wundt's correspondence hardly shows any in-depth pre-publication evaluation of work by outsiders, done in his laboratory.[125] In this respect, the extensive correspondence with Fechner is quite exceptional. The lack of such review practices can be attributed to a combination of circumstances.

One of the circumstances is that Wundt's research resembles the endeavours of Koch and Behring. Both consisted of series of experiments and both were the product of collaboration in a laboratory setting. In both cases it was therefore very unlikely that any individual would be able to evaluate the end product through reiteration of the full intellectual process that had led to it. Empirical data acquired through a long series of collaborative experiments do not lend themselves to easy replication. No set of observations can easily be characterised as correct or incorrect in the same decisive way as violations of the rules of grammar, rhyme, or metre in an Arabic manuscript could be determined. The fact that the observations at the Institute for Experimental Psychology were usually described as self-observation (*Selbstbeobachtung*), inner observation (*innere Beobachtung*) and inner experience (*innere Erfahrung*) made it even more difficult for outside reviewers to evaluate the raw data.[126]

There is also a reason why the findings from Wundt's laboratory were more difficult to evaluate than Behring's serums. Although other scholars could not easily replicate the data and production processes that had led to Behring's diphtheria serum, this was not a major concern in their evaluation. This has to do with the fact that Heubner only evaluated the efficacy of the end product rather than the data and production processes that had made its creation possible. Because the output of Wundt's laboratory could not be expected to have a similar kind of easily testable

[123] UAL, Nl. Wilhelm Wundt, Gustav Theodor Fechner to Wilhelm Wundt, 7 April 1885. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[124] UAL, Nl. Wilhelm Wundt, Gustav Theodor Fechner to Wilhelm Wundt, 13 April 1886. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[125] Part of his correspondence with his Leipzig collaborators will be discussed in the next chapter: in these discussions Wundt figured as the editor of the *Philosophische Studien* who commented on the submitted papers of his own collaborators.
[126] Robinson, David Kent, 'Wilhelm Wundt and the establishment of experimental psychology, 1875–1914: The context of a new field of scientific research,' (PhD diss., University of California, Berkeley, 1987). 100.

applicability, the template of the clinical trial could not be used as the basis of any viable practice of evaluation.

In one way, however, the data from Wundt's laboratory were easier to evaluate than Koch's and Behring's discoveries. Koch's *Tuberkulin* and Behring's diphtheria serum were both presented as revolutionary new findings, the manufacturing process and efficacy of which could not be traced back to that of earlier curative agents.[127] Much of the work in Wundt's laboratory, on the other hand, was built on earlier findings of physiologists and psychophysicists; a reviewer in the *Literarische Centralblatt* praised Wundt's *Grundzüge der physiologische Psychology* — not for its groundbreaking new findings, but for how it corresponded 'exactly to the need created by recent developments in physiology and psychology'.[128] One way to evaluate the findings from Wundt's laboratory was therefore to assess if they corresponded with what could be predicted on the basis of other people's findings. This was exactly what Fechner did. In his correspondence with Wundt and his associates, he not only compared their findings to what he thought that Weber's law predicted, he also reflected on the likely distribution of errors of measurement in their data sets and compared this to what he called Gauss's law.[129] When the distribution of errors seemed to deviate from what, today, is known as the 'Gaussian' or 'normal' distribution, Fechner asked for further clarification.[130]

What is striking in these discussions is not that Fechner repeatedly commented on work conducted in Wundt's laboratory in private correspondence, but that he was the only person who extensively and privately discussed such issues with various researchers at this laboratory. A number of explanations can be put forward to explain both why the members of the Institute for Experimental Psychology were not very eager to privately solicit outside commentary and why outsiders would not have been very eager to present themselves as supportive collaborators on Wundt's psychophysical project.

One of these explanations is rooted in the character of laboratory collaboration. The fact that laboratory research is teamwork, means that any researcher can expect a fair amount of criticism

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[127] Even though it can be argued that the 'bacteriological scorched-earth strategy' that Koch assumed to explain the efficacy of Tuberkulin followed from pre-existing ideas about the relationship between disease and pathogen, it was a new and unique theory of the efficacy of a curative agent: Gradmann, Christoph, 'A harmony of illusions,' 370–371. On the innovativeness of Behring's serum, see Linton, Derek S., *Emil von Behring*, 3–6.

[128] Quoted in: Diamond, Solomon, 'Wundt before Leipzig,' 59.

[129] See, for example, UAL, Nl. Wilhelm Wundt, Theodor Gustav Fechner to Wilhelm Wundt, 24 March 1885 and 11 April 1885. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)

[130] On the messy history of this terminology, see: Stigler, Stephen M. and William H. Kruskal, 'Normative Terminology,' in: Stigler, Stephen M., *Statistics on the Table: The History of Statistical Concepts and Methods*, Harvard University Press, Cambridge (MA), 1999, 403–430.

and evaluation from his day-to-day colleagues. This can then lower the perceived need for outside evaluation. When he discussed Weber's law with Fechner, Wundt underlined the trustworthiness of the findings published by his collaborators by emphasising the fact that other members of his institute had repeated the experiments on which these findings were based: Julius Merkel and Gustav Lorenz, for example, had replicated experiments conducted by Ernst Tischer, while Volkmar Estel built on experiments done by Julius Kollert.[131] Some years later, Fechner mentioned another scholar of Wundt's institute — he did not remember his name — who in his turn conducted experiments to verify Estel's work.[132]

This laboratory cooperation not only contributed to lessening the need for further outside evaluation, it also fostered an environment in which the experimental findings were seen as a shared accomplishment. Wundt fiercely defended his collaborators against Fechner's criticism. In most of his replies his polite rhetorical strategy was not to claim that Fechner was wrong, but to clear up apparent misunderstandings.[133] Meanwhile Fechner continuously emphasised that he kept harbouring doubts and could therefore not agree with Wundt's objections.[134] As in their earlier discussion of Slade's spiritism, both men generally agreed to disagree.

Although the above considerations of loyalty to one's everyday colleagues was shared by researchers in all types of laboratories, the philosophy behind the experiments at the Institute for Experimental Psychology provided an additional reason to be suspicious of outside commentators. Experiments in Wundt's laboratory required people to take on three different roles, namely those of experimenter, observer and subject. The people in these roles were all supposed to have a clear understanding of the experiment.[135] In this setting, the role of subject was considered to require 'more psychological sophistication' than that of the experimenter or observer.[136] This requirement arose from the type of questions Wundt and his students would ask, as these were often aimed at finding the minimal perceivable difference between two impulses or the minimal reaction time in response to a stimulus. In order to perceive this minimal difference or to keep reaction times as short as possible, subjects had to be well-trained to be able to provide meaningful and stable data.[137]

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[131] UAL, Nl. Wilhelm Wundt, Wilhelm Wundt to Gustav Theodor Fechner, 27 October 1882 and 10 November 1882. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[132] UAL, Nl. Wilhelm Wundt, Gustav Theodor Fechner to Wilhelm Wundt, 14 April 1885. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[133] For example, see UAL, Nl. Willhelm Wundt, Wilhelm Wundt to Gustav Theodor Fechner, 19 October 1882 and 27 October 1882. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[134] For example, see UAL, Nl. Wilhelm Wundt, Gustav Theodor Fechner to Wilhelm Wundt, 11 November 82 and 11 April 1885. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[135] Robinson, David Kent, 'Wilhelm Wundt and the establishment of experimental psychology,' 101.
[136] Danziger, Kurt, *Constructing the subject: Historical origins of psychological research*, Cambridge University Press, Cambridge, 1990. 51.
[137] Robinson, David Kent, 'Wilhelm Wundt and the establishment of experimental psychology,' 101.

This task was so critical to the success of the experiment that Wundt considered it to be compatible to his position as head of the institute.[138] Most members of the Institute would vary between playing the role of experimenter, observer and subject. Indeed, an efficiently run laboratory for experimental psychology could not afford to use well-trained people in one role only. This experimental set-up was not very likely, however, to foster trust in the judgement of outsiders. Even if outside commentators understood what it took to be an experimenter or observer, they could hardly be expected to have access to subjects that had received the necessary training to be a source of meaningful data.

In addition to the characteristics of laboratory cooperation, there were other social circumstances that prevented Wundt from benefiting from the supportive evaluation of his not-yet-published work. One observation that can be made is that the willingness of peers to pay close attention to each other's not-yet-published works depends to a large extent on personal relationships. De Goeje and Nöldeke had been close friends since their early twenties. Likewise, Emil Behring and Paul Ehrlich had met as early career researchers at Koch's Hygienic Institute. Wundt, however, had not developed intimate and lasting relationships with the future medical doctors and physiologists with whom he studied in Tübingen and Heidelberg. His correspondence lacks letters from former fellow students and the section about his student days in his autobiography does not mention lasting friendships.[139]

This was not because he was somehow unable to engage in personal relationships. In the same book, he gratefully recounts the relationship with his high school friends Heinrich Holtzmann and Adolf Hausrath. Over the decades, the three men would share memories and witticisms. Holtzmann, for example, remembered Wundt of their time at an 'obscure ale-house in Karlsruhe' which he called 'the true university and everything your heart desires'.[140] Hausrath joked about Wundt's style of thought and his election to the *Académie française* in 1904: 'I would like to warmly congratulate you with your acceptance among the French immortals. May you succeed in completely reconciliating this great nation with us [Germans]. Without a doubt you will construct an apparatus that will reduce the […] French and German yearning for revenge to a pure mathematical formula'.[141] Like Wundt, Holzmann and Hausrath had successful academic careers.

[138] Danziger, Kurt, *Constructing the subject*, 51.
[139] The memories of his student days in Tübingen and Heidelberg can be found at: Wundt, Wilhelm, *Erlebtes und Erkanntes*, Alfred Kröner, Stuttgart, 1920. 60–102.
[140] UAL, Nl. Wilhelm Wundt, Heinrich Holtzmann to Wilhelm Wundt, 28 December 1866. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[141] UAL, Nl. Wilhelm Wundt, Adolf Hausrath to Wilhelm Wundt, 6 November 1904. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)

However, because they both became theologians, they were unable to contribute to their friend's work in a meaningful way.

Meanwhile, Wundt and his collaborators did not receive much supportive feedback from the members of other philosophy departments, either. The membership of philosophical faculties in late 19th-century Germany consisted primarily of Kantians and Hegelians, who looked at Wundt's efforts with suspicion. Some of them thought that the attempt to practice laboratory philosophy as a means to bridge the gap between the natural sciences and philosophy could only lead to materialist, unchristian and un-German conclusions. Richard Avenarius, the editor of a journal to which Wundt regularly contributed, was considered unfit for a Prussian professorship because he was described to Althoff as the representative of 'a very extreme school of thought'.[142] Others saw Wundt's experimental contributions as so new and underdeveloped that they could not yet offer a template for further research. Even if Wundt received personal praise for his accomplishments, few expected that others would be able to succeed in a similar fashion.[143] In addition, Wundt was not very good at maintaining cordial relationships with the few people who would have been able to provide meaningful evaluations of his work. The Halle university librarian Otto Hartwig reported to Althoff that Wundt had fallen out with all 'the greatest' scholars in Berlin, especially with his former employer Helmholtz.[144]

All in all, the lack of evaluation of works from the Institute for Experimental Psychology can be explained by a number of factors. The social dynamics that Wundt's laboratory shared with other laboratories are one part of the explanation. The special character of the research methods at the this laboratory are another part. In addition, Wundt's personal relationships with other scholars and his relative intellectual isolation in late 19th-century German philosophy provide another clue to the relative lack of evaluative comments in his correspondence. Wundt had cordial relationships with scholars who were unable to give useful feedback on his work and maintained uneasy relationships with people whose pre-publication evaluations could have been useful. From outside of Wundt's own institute, only Fechner regularly discussed issues of shared academic interest with him, and these discussions tended to lead to little more than an ever-recurring agreement to disagree.

[142] GStA PK, VI. HA, Nl Althoff, No. 118, Otto Liebmann to Friedrich Althoff, 23 January 1884. See also: Chapter 4, 148-150.
[143] GStA PK, VI. HA, Nl Althoff, No. 118, Carl Stumpf to Friedrich Althoff, 5 October 1893.
[144] GStA PK, VI. HA, Nl Althoff, No. 118, Otto Hartwig to Friedrich Althoff, 27 October 1893.

Conclusion: a range of evaluative practices

The above case studies share one very characteristic feature — they all focus on the evaluation of scholarly work by other scholars before sharing it with a broader audience. These evaluative practices can be considered as institutionalised if we think of institutions as 'patterned behaviour', as relatively stable, valued sets of formal and informal rules, norms and practices that constrain but also enable […] behaviour'.[145] Even if there were few formal rules or regulations, such as modern-day procedures of double-blind peer review, there were various practices that constrained and enabled scholars to ensure that their work would undergo thorough quality control before it would eventually find its way to a broader audience.

One important constraint on evaluation was its informal character. No scholar had to feel obliged to evaluate someone else's work, although, it was not uncommon for scholars to feel obliged to review the work of friends, acquaintances, admired colleagues or former students. However, there were no compelling incentives to invest large amounts of time and effort into the support of strangers. This meant that a number of respectable yet isolated scholars made a career without the advantages of peer evaluation. Wilhelm Ahlwardt, orientalist in Greifswald, was one of them. Nöldeke and De Goeje often criticised his catalogues and text editions. Nöldeke blamed the ever-decreasing quality of Ahlwardt's work on his isolation in Greifswald: 'Ahlwardt is a curious, *lonely* fellow. If he would have had a closer relationship with his peers, he would have done things differently. […] Forty years ago, Ahlwardt certainly was the best expert on Arabic poetry, but he has hardly learned anything new since that time and there is probably also much that he has forgotten. (Oh, how erudite would we be, if we could remember everything we ever knew!!!)'[146]

A related constraint was the fact that mutual evaluation is very time-consuming. For many years Nöldeke thoroughly checked not only thousands of pages of Arabic texts edited by De Goeje, he did this favour to others, as well. Meanwhile De Goeje did the same for Nöldeke and other peers. The same is true for Heubner. The first letters mentioning his tests for Behring were sent in 1892 and four years later he was still performing them.[147] And he was not the only one engaged in such tests; the Berlin doctors Eduard Henoch and Ernst von Bergmann as well as the Münchener

[145] Badie, Bertrand, Dirk Berg-Schlosser and Leonardo Morlino, *International Encyclopedia of Political Science*, Volume 1, SAGE, Thousand Oaks (CA), 2011. 1200.
[146] UBL: BPL: 2389, Theodor Nöldeke to Michael Jan de Goeje, 7 December 1904. Nöldeke's emphasis. For earlier discussions between Nöldeke and De Goeje about Ahlwardt's work, see Engberts, Christiaan, 'The Scholar as Judge: A Contested Persona In Nineteenth-Century Orientalism,' *BMGN – Low Countries Historical Review*, 131 (4), 2016, 93–111. 108.
[147] Emil Behring to Otto Heubner, 29 June 1896, BNd: EvB/B 1/58.

physician Max Joseph Oertel spent considerable time on the testing of Behring's serums too.[148] Finally, Fechner's extensive correspondence about the correctness and interpretation of the work carried out in Wundt's laboratory must have been very time-consuming. His letters often counted dozens of pages, the longest of which as much as 121 pages.[149] If we also take the fact into account that none of these people were paid for their evaluative efforts, the constraining quality of the amount of work needed to write a useful evaluation becomes even more evident.

A final constraint on mutual evaluation follows from the character of the scholarly output. Texts are eminently suitable for exhaustive mutual evaluation. This is why Nöldeke and De Goeje were able to thoroughly review each other's work. Research results, however, not always consist of text. In the case of Behring and Wernicke, the most important result from their endeavours was a serum. In the case of Wundt and his cooperators, the primary result from their experiments was a series of measurements. These results did not lend themselves to peer evaluation as easily as texts by De Goeje and Nöldeke, not in the least because they were the result of collaboration. The complexity of such a collaborative effort could not be replicated as easily as the thoughts and considerations of an individual scholar working on the intricacies of an Arabic text. Especially, the findings from Wundt's laboratory suffered from replication problems because they were considered to be highly depended on the intensive training of not only the experimenter but also of the observer and the subject. These constraints created a laboratory culture in Leipzig, where mutual evaluation largely took place within the walls of the institute while criticism from outside was — willingly or unwillingly — kept at a distance.

Some of these practices also enabled evaluative practices. Though the informality of mutual evaluation could work as a constraint for those scholars who only had limited access to networks of qualified peers, it was an enabling factor for those who were well-connected. De Goeje made long lists of suggestions for numerous people, such as Ferdinand Wüstenfeld, Albert Socin, Louis Cheikho and Carl Brockelmann.[150] Apart from Socin, with whom he had been in touch during the early days of the al-Ṭabarī project, none of these people was particularly close to him. The fact that De Goeje did not even refuse his time and energy to these rather distant acquaintances, suggests that it would have been unacceptable to refuse to support Nöldeke. In a similar fashion the extensive and detailed correspondence between Fechner and Wundt cannot be explained by simply pointing at the fact that both men shared certain intellectual interests. Though only one letter from

[148] Emil von Behring to Otto Heubner, 8 November 1905, BNd: EvB/B 1/59.
[149] Gustav Theodor Fechner to Wilhelm Wundt, 13 April 1886. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[150] UBL: BPL 2389, Michael Jan de Goeje to Theodor Nöldeke, 27 February 1870, 11 October 1902, 20 October 1905, 15 May 1906.

Fechner to Wundt survived from before his call to Leipzig, by far the largest part of their correspondence took place during Wundt's Leipzig years. The fact that the two men had become friends in real life stimulated their willingness to extensively discuss shared interests.

Another enabling factor was the perceived usefulness of critical evaluation. It could be useful to the evaluator himself. This was the case with Otto Heubner, who, as a paediatrician, had the experience of being unable to cure children suffering from diphtheria. If he could help to advance the development of the new serum, this could both benefit the sick children and himself. After all, he would have done an outstanding job as a paediatrician if he proved to be able to contribute to the eradication of this deadly disease. Heubner's work, however, was useful not only to himself—it was also extremely useful to Behring. Heubner contributed both the necessary knowledge and the indispensable resources that Behring lacked, which consisted of a long experience in observing and diagnosing diphtheria in people, the competence to manage large-scale clinical research and a statistically significant number of diphtheria patients.

This chapter has shown that the way in which scholarly work was evaluated in late 19th-century Germany depended both on disciplinary and personal factors. The production of text editions in the field of Arabic studies lent itself extraordinarily well to mutual proofreading. Laboratory sciences like bacteriology and experimental psychology, however, did not lend themselves easily to this evaluative practice. In bacteriology evaluation took the shape of testing the efficacy of newly developed substances, while experimental psychologists compared new findings to expectations derived from prior experiences and existing theories. Evaluation was facilitated by access to networks of supportive and qualified peers. Nöldeke and De Goeje were lucky to have each other as expert commentators, while Wundt was less lucky to have theologians instead of psychologists as his most trusted peers. Still, in the end, philologists, psychologists and bacteriologists all benefited in some way from the critical support of their peers. Mutual trust and a sense of loyalty created an environment in which critical evaluative practices could thrive, as illustrated by the extensive correspondence between Nöldeke and De Goeje, Wundt and Fechner, and Behring and Heubner.

2. The Editorial Experience

Balancing between Editors, Authors, Publishers and Audiences

The importance of editors

After the founding of the Empire in 1871, the German market for academic journals boomed. The increasing interconnectedness of the German states, propelled by the continuous growth of postal and railway networks, encouraged publishers to invest in new journals.[1] This chapter looks at the editorial practices at some of these new periodicals. Looking back at the history of medical journals, one modern-day author argues that their story 'is very much bound up with the characters and personalities of the many editors who, through the years, have contributed so much.'[2] Another paper on the history of scholarly publishing simply states that '[...] reviews were almost wholly performed by journal editors through most of the 19th century.'[3] One author even notes that 'as recently as the 1960s the then editor of *Nature* is said to have relied mainly on expert opinions within the editorial office, taking the occasional article with him to ask a colleague's opinion over lunch at his London club.'[4]

Such statements suggest that the editors of scholarly journals played a very important role in evaluating the submitted work. Without consulting others, they decided whose research articles and book reviews would be published. This decision was, of course, influenced by the perceived quality of the submitted papers. However, for an outsider looking in, the full rationale behind such decisions is almost impossible to uncover—especially, in the absence of any related correspondence between author and reviewer/editor. Although many letters between editors, representatives of publishing houses and authors have been preserved, it is uncommon for these pieces of correspondence to contain detailed discussions about the scholarly merit of submitted papers. They do, however, shed some light on how the relationship between publisher, editor, author and reader shaped scholarly journals in the 19th century. This chapter's analysis of these letters, therefore, can be considered an elaboration on Lynn Nyhart's observation that '[...]

[1] Kirchner, Joachim, *Das deutsche Zeitschriftenwesen*, 238–240.
[2] Booth, Christopher C., 'The Origin and Growth of Medical Journals,' *Annals of Internal Medicine*, 113(5), 1990, 398–402. 402.
[3] Mack, Chris, '350 Years of Scientific Journals,' *Journal of Micro/Nanolithography, MEMS, and MOEMS*, 14(1), 2015, 1–3. 3.
[4] Lock, Stephen, *A Difficult Balance: Editorial Peer Review in Medicine*, London, The Nuffield Provincial Hospitals Trust, 1985. 3.

historical discussions suggest that […] journals have adopted quite a variety of practices expressing different relationships between editors, contributors and readers.'[5]

Of course, there were major differences between the various journals and their editors. One factor to take into account is their institutional embedding. Many 19th-century journals were established in the wake of the founding of new scientific societies, who wanted to publish their own periodicals.[6]Not all new journals, however, were associated with such societies. The founding of some of Germany's most influential chemistry journals, for instance, completely relied on the initiative and commitment of one prominent editor, as illustrated by the examples of Justus von Liebig at the *Annalen der Chemie* and Wilhelm Ostwald at the *Zeitschrift für physikalische Chemie*.[7] The management of their own journals enabled both men to present a new sub-discipline to a broader academic audience. At the same time, it also provided them a place to publish the findings of their own and associated research groups.[8]

This chapter deals with both individually administered and society managed journals. I will first focus on Wilhelm Wundt's editorial career. After a short look at his experience with Richard Avenarius' *Vierteljahrsschrift für die wissenschaftliche Philosophie*, I will turn to his own journal, the *Philosophische Studien*. Next the focus will shift to the *Zeitschrift der Deutschen Morgenländische Gesellschaft* (*ZDMG*), which was published by the *DMG*. Finally, one more distinction is taken into account, namely that of the difference between specialised and general journals. While the editor of a specialised journal can be expected to be at least somewhat acquainted with the subjects discussed in his journal, this cannot be expected of the editor of a journal dealing with a wide range of issues. Therefore, the final part of this chapter is dedicated to Friedrich Zarncke's editorial work at the *Literarische Centralblatt für Deutschland*.

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[5] Nyhart, Lynn K., 'Writing Zoologically: The Zeitschrift für wissenschaftliche Zoologie and the Zoological Community in Late Nineteenth-Century Germany,' in: Dear, Peter (ed.), *The Literary Structure of Scientific Argument: Historical Studies*, University of Pennsylvania Press, Philadelphia (PA), 1991, 43–71. 44.

[6] Cook, Alan, 'Academic Publications before 1940,' in: Fredriksson, Einar H. (ed.), *A Century of Science Publishing: A Collection of Essays*, IOS Press, Amsterdam, 2001, 15–24. 19; Houghton, Bernard, *Scientific periodicals: their historical development, characteristics and control*, Clive Binley, London, 1975. 31.

[7] Klooster, H.S. van, 'The Story of Liebig's Annalen der Chemie,' *Journal of Chemical Education*, 34(1), 1957, 27–30. 27–28; Hapke, Thomas, *Die Zeitschrift für Physikalische Chemie: Hundert Jahre Wechselwirkung zwischen Fachwissenschaft, Kommunikationsmedium und Gesellschaft*, Traugott Bautz, Herzberg, 1990. 22–47.

[8] Morrell, J.B., 'The chemist breeders: the research schools of Liebig and Thomas Thomson,' *Ambix*, 19(1), 1972, 1–46. 5–6.

Wilhelm Wundt as editor

From its establishment in the late 1870s until his retirement in 1917 Wilhelm Wundt's operational base was his laboratory. The founding of his own journal, the *Philosophische Studien* was related to its establishment. The growth of his institute encouraged him to delve deeper into his empirical psychological and psychophysical studies and therefore created a continuing influx of prospective experimentalists. The subsequent increase of experimental findings and papers called for a new platform to publish this rather coherent body of work. Soon after the founding of his institute, Wundt already complained to his former pupil Emil Kraepelin: 'At the moment I have some works […] that have been completed in my psycho-physical laboratory […] I don't really know, where they can be published. I would really like a periodical that offers a place where such works can be brought together'.[9] The *Philosophische Studien* would soon offer such a place. The founding of the *Studien* was not, however, Wundt's first editorial experience.

Wundt's earliest experiences with journal publishing were in the 1870s, when he supported an initiative of the young philosopher Richard Avenarius. He had met him at Leipzig's *Akademisch-Philosophische Verein* (academic-philosophical society) in 1875.[10] Two years later, Avenarius, who shared Wundt's commitment to bringing together philosophy and the scientific method, accepted Wundt´s former Chair of Inductive Philosophy in Zürich. That same year, he asked Wundt to cooperate with him on his newly established journal, the *Vierteljahrsschrift für wissenschaftlichen Philosophie* (Quarterly for Scientific Philosophy). Even if Wundt did not have any more editorial experience than Avenarius, the fact that he was eleven years older put him in the role of senior advisor. In the early years of the *Vierteljahrsschrift*'s existence Avenarius often asked for Wundt's advice, for example on how to deal with pushy contributors.[11] During these years, Wundt learned how difficult it was to manage a new journal. Avenarius continuously shared his frustrations. '[...] It is truly embarrassing, how little success the invested effort and costs have earned us,' he complained in 1878.[12] Two years later, the financial viability of the journal was still not guaranteed: '[...] as little as our journal lacks in recognition and efficacy, so much does it still lack in a sufficient number of subscribers'.[13] The difficulty to assess the success of the *Vierteljahrsschrift* is also reflected

[9] Wilhelm Wundt to Emil Kraepelin, 14 October 1880, in: Steinberg, *Der Briefwechsel zwischen Wilhelm Wundt und Emil Kraepelin*, 39–40.
[10] Russo Kraus, Chiara, 'Back to the origins of the repudiation of Wundt: Oswald Külpe and Richard Avenarius,' *Journal of the History of the Behavioral Sciences*, 53(1), 2017, 28–47. 30.
[11] For example, see UAL, Nl. Wilhelm Wundt, Richard Avenarius to Wilhelm Wundt, 5 May 1877 and 21 September 1879. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[12] UAL, Nl. Wilhelm Wundt, Richard Avenarius to Wilhelm Wundt, 31 December 1878. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[13] UAL, Nl. Wilhelm Wundt, Richard Avenarius to Wilhelm Wundt, 19 December 1880. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)

in the assessments of Avenarius' editorial work that his colleagues shared with the ministry of education. While one referent praised him for being 'particularly well-known as the editor of a philosophical quarterly', another emphasised that these efforts met 'with little success'.[14] Wundt did not worry about this apparent lack of success and even shortly considered the possibility to publish the *Philosophische Studien* as a supplement to the *Vierteljahrsschrift*.[15]

Wundt planned to fill this supplement with doctoral dissertations of his students. When this plan faltered, Wundt's idea of creating his own platform started to take a more concrete shape. In the same letter to Kraepelin in which he had discussed the need for a gathering point for the experimental papers from his laboratory, he also complained that the *Vierteljahrsschrift* did not offer enough space for such publications.[16] One year later he again complained about Avenarius' journal, lamenting the fact that it had become 'somewhat too abstract and dry'.[17] When he mentioned the work completed in his laboratory, Wundt not only referred to his own writing. Most of the work in his institute was done by his students. This increasing student activity soon translated into a fast growing number of dissertations on experimental subjects, the first of which was finished by the mathematician Max Friedrich in 1881.[18] An overview published by Leipzig University lists 186 dissertations supervised by Wundt during his Leipzig years, a large part of which were either the results from laboratory research or reflections on methodological issues arising in a laboratory environment.[19] Even if it was easy for Wundt to get his own work published, it was still difficult for his students to find an audience.

Before the 19th-century serial publications of dissertations were not uncommon in the German lands.[20] At its founding in 1795 the *Archiv für die Physiologie* consisted, for example, largely of reprints of dissertations.[21] By the time Wundt had established his laboratory, though, this genre of academic publishing had become a thing of the past. In 1880 Wundt therefore decided to discuss the acceptance of dissertations into the *Vierteljahrsschrift* with Avenarius. The latter had strong

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[14] GStA PK, VI. HA, Nl Althoff, No. 118, Liebmann to Althoff, 23 January 1884 and Müller to Althoff, 25 January 1884.
[15] UAL, Nl. Wilhelm Wundt, Richard Avenarius to Wilhelm Wundt, 31 December 1880. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[16] Wilhelm Wundt to Emil Kraepelin, 14 October 1880, in: Steinberg, *Der Briefwechsel zwischen Wilhelm Wundt und Emil Kraepelin*, 40.
[17] Wilhelm Wundt to Emil Kraepelin, 4 August 1881, in: Steinberg, *Der Briefwechsel zwischen Wilhelm Wundt und Emil Kraepelin*, 58.
[18] For more on Friedrich, see: Domanski, Cezary W., 'A biographical not on Max Friedrich (1856–1887), Wundt's first PhD student in experimental psychology,' *Journal of the History of the Behavioral Sciences*, 40(3), 2004, 311–317.
[19] Alle Dissertationen bei Wundt, chronologisch geordnet: http://home.uni-leipzig.de/wundtbriefe/wwcd/chronos/chronos.htm.
[20] Kronick, David A., *A History of Scientific and Technical Periodicals: The Origins and Development of the Scientific and Technological Press, 1665–1790*, The Scarecrow Press, New York (NY), 1962. 193–200.
[21] Broman, Thomas H., 'J.C. Reil and the "Journalization" of Physiology,' in: Dear, Peter (ed.), *The Literary Structure of Scientific Argument: Historical Studies*, University of Pennsylvania Press, Philadelphia (PA), 1991, 13–42. 23–24.

reservations. He was not convinced that they would be a valuable addition to his journal because they were published as books or brochures as well. He also pointed out that the newly promoted doctors usually asked for the immediate publication of their dissertations, which was something to which he did not want to commit. Only if their authors would refrain from their honoraria and show patience with regard to publication dates, Avenarius was willing to consider printing some of the best dissertations.[22] Two weeks later, however, he returned to his earlier doubts: 'At the moment, we have the experience that 'philosophical' essays in general, and philos. 'doctoral dissertations' in particular, hardly cover the printing costs'. It did not help, he added, that such essays dealt with subjects 'for which there is — alas! — not much demand anyway'.[23] This strengthened Wundt's conviction that he should found his own journal.

Therefore, he contacted the publishing house Wilhelm Engelmann, which had been publishing his books since the early 1870s. Although Engelmann accepted the idea of a journal that would include dissertations, he did have some reservations: 'I think that young people, students, should renounce all *material* benefit from their intellectual efforts; I believe that the ideal state of mind, which we are obliged to maintain especially in our time, will not be encouraged in that way.'[24] The publisher also came up with some ideas for the new journal. He proposed to publish not only long research papers, but also to add short announcements and reviews of new German and foreign psychological literature: 'I mean that, to a certain degree, we can give the journal the character of a *revue*'.[25] He also emphasised that the readership would most likely be larger if Wundt—in addition to experimental reports—would also publish theoretical essays.[26] Though the *Studien* would indeed include some theoretical papers, usually written by Wundt himself, he convinced Engelmann to abandon the idea of a *revue*. The journal would largely take the shape Wundt described to Kraepelin in 1880: 'I think it is best, at least for now […], to only publish standalone papers, no reading reports, reviews, etcetera.'[27]

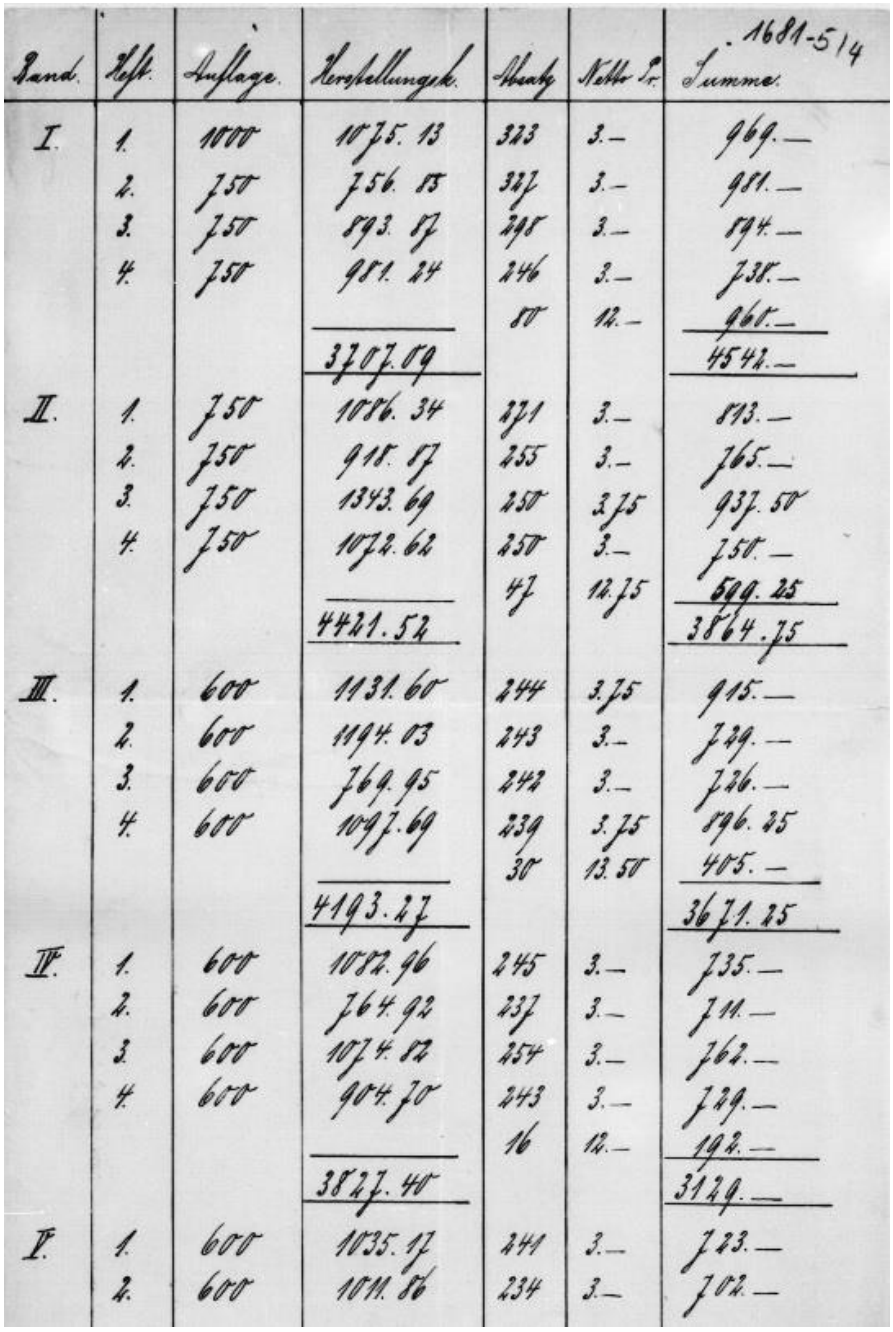
Wundt's editorial independence is further illustrated by his publisher's lack of interest in the financial returns of the *Philosophische Studien*. In 1882, Engelmann mentioned the 'currently not very

[22] Richard Avenarius to Wilhelm Wundt, 19 December 1880. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[23] Richard Avenarius to Wilhelm Wundt, 31 December 1880. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[24] Rudolf Engelmann to Wilhelm Wundt, 6 June 1881. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm) Engelmann's emphasis.
[25] Ibid. Engelmann's emphasis.
[26] Rudolf Engelmann to Wilhelm Wundt, 8 November 1882. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[27] Wilhelm Wundt to Emil Kraepelin, 17 December 1880, in: Steinberg, *Der Briefwechsel zwischen Wilhelm Wundt und Emil Kraepelin*, 40.

favourable results' and, in the following year, he expressed the hope that 'the sales of the *Studien* will slowly but steadily increase, so that the still existing disparity between income and costs will gradually improve and become more balanced.'[28] An overview of the revenues and expenditures that Emanuel Reinicke, managing partner of the publishing house after Rudolf Engelmann's death in 1888, sent to Wundt in 1890 shows that the journal was far from profitable (see Figure 1). Even if the first volume had made a profit after having been available for more than seven years, none of the following four volumes had yielded a return that outweighed the production costs. Although he hoped that the other volumes would eventually break even, Reinicke proposed to either raise the sales price or limit the number of pages of the journal.[29]

Despite the fact that some later issues eventually broke even, sales would never be impressive. It would take until 1912 before a modest reprint of a hundred copies of one 1894 issue of the *Studien* was required and, only in 1915, another similar reprint was needed of an 1891 issue.[30] Still, Engelmann did not worry about this lack of commercial success. This is probably best explained by the closing paragraph of Rudolf Engelmann's extensive 1881 letter: 'Finally, you will surely allow me to ask that you first turn to us when you are planning to publish any other more comprehensive work […]. In the light of our pleasant personal relationship, I would greatly appreciate it, if we would have a similar and enduring author–publishing house relationship.'[31] Until shortly before the First World War, Wundt indeed stayed with Engelmann. The highly profitable books he authored during the thirty years between 1880 and 1910 provided him with the freedom to manage his unprofitable journal without complaints or interventions from his publisher.

[28] Rudolf Engelmann to Wilhelm Wundt, 8 November 1882 and 3 February 1883. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[29] Emanuel Reinicke to Wilhelm Wundt, 7 February 1890. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[30] Universitätsarchiv Leipzig (hereafter UAL), NA Wundt/III/1681/9, Wilhelm Engelmann to Wilhelm Wundt, 29 July 1912 and UAL, NA Wundt/III/1681/10, Wilhelm Engelmann to Wilhelm Wundt, 15 January 1915.
[31] Rudolf Engelmann to Wilhelm Wundt, 6 June 1881. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)

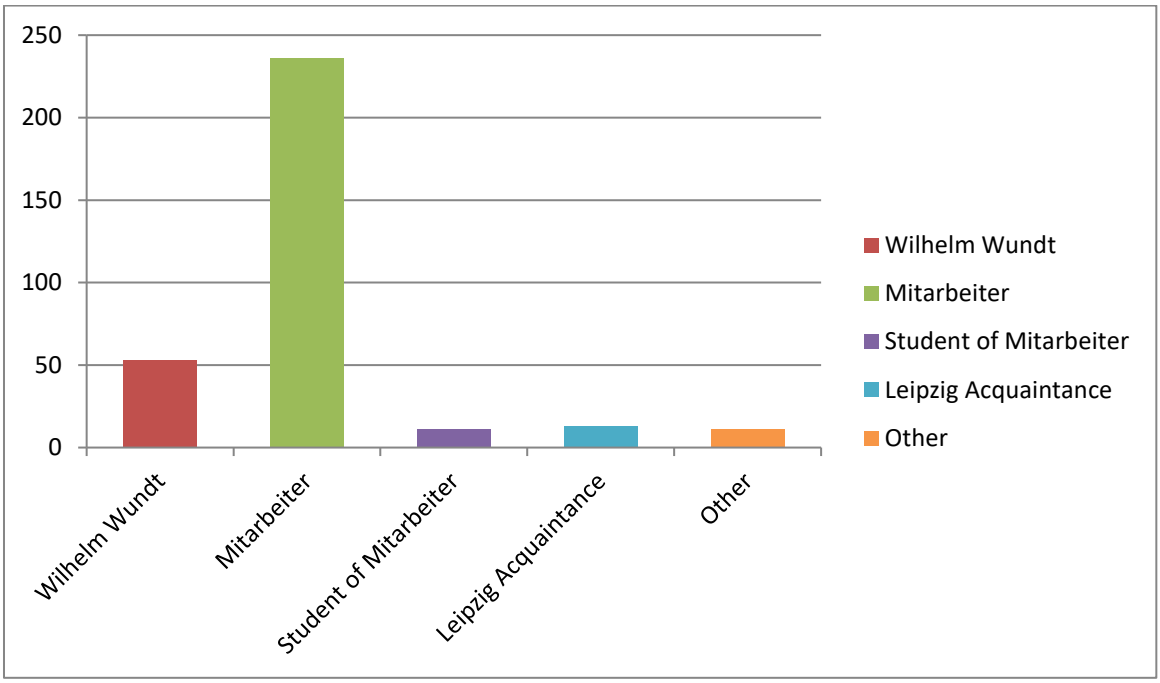


Band.	Heft.	Auflage.	Herstellungsk.	Absatz	Netto Pr.	Summe. 1681-514
I.	1.	1000	1075. 13	323	3.–	969.–
	2.	750	756. 15	317	3.–	951.–
	3.	750	893. 87	298	3.–	894.–
	4.	750	951. 24	246	3.–	738.–
				80	12.–	960.–
			3707.09			4542.–
II.	1.	750	1086. 34	271	3.–	813.–
	2.	750	918. 87	255	3.–	765.–
	3.	750	1343. 69	250	3.75	937. 50
	4.	750	1072. 62	250	3.–	750.–
				47	12.75	599. 25
			4421. 54			3864. 75
III.	1.	600	1131. 60	244	3.75	915.–
	2.	600	1194. 03	243	3.–	729.–
	3.	600	769. 95	242	3.–	726.–
	4.	600	1097. 69	239	3.75	896. 25
				30	13. 50	405.–
			4193. 27			3671. 25
IV.	1.	600	1082. 96	245	3.–	735.–
	2.	600	764. 92	237	3.–	711.–
	3	600	1074. 82	254	3.–	762.–
	4.	600	904. 70	243	3.–	729.–
				16	12.–	192.–
			3827. 40			3129.–
V.	1.	600	1035. 17	241	3.–	723.–
	2.	600	1011. 86	234	3.–	702.–

Balance sheet of the *Philosophische Studien*. Attachment to the letter by Emanuel Reinecke to Wilhelm Wundt, 7 February 1890. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)

Wundt used this independence to turn the *Studien* into the unofficial organ of his laboratory. (see Figure 2) The twenty volumes published between 1881 and 1903 contained 324 contributions, 53 of which had been written by Wundt himself. By far the largest number of contributions, however, was written by people close to him. The great majority of this group consisted of people who had worked in his laboratory, either as doctoral candidates or as assistants. The 236 papers falling under

this heading (*Mitarbeiter*) contain both dissertations and other contributions. In addition, some papers written by the pupils of Wundt's former students Kraepelin and Oswald Külpe (student of *Mitarbeiter*) were published in the *Studien*. Some acquaintances from Leipzig, such as Fechner, also contributed. Finally, there are eleven contributions written by people that are not easily categorised, such as Harald Höffding, the Copenhagen mentor of Wundt's Danish pupil Alfred Lehmann.[32] All in all, it can be concluded that the lack of pressure to turn the *Philosophische Studien* into a commercially viable endeavour allowed Wundt to turn it into a platform for himself, his students and a few other associates.



Contributors to the *Philosophische Studien*

Peripheral scholars in the *Philosophische Studien*

The *Studien* contained many contributions by Wundt's most successful students, such as Emil Kraepelin, Oswald Külpe and Ernst Meumann. More remarkable, however, is the high number of contributions by Wundt students who found themselves at the periphery of German academic life, such as Ludwig Lange, Julius Merkel, Gottlob Friedrich Lipps and Friedrich Kiesow. After they finished their dissertation with Wundt, these men struggled to have a successful academic career

[32] Höffding, Harald, 'Zur Theorie des Wiedererkennens,' *Philosophische Studien*, VIII, 1893, 86–96.

and their continued publication in the *Studien* guaranteed at least some sustained interest in their stubbornly continued experimental endeavours.

Lange's story was especially tragic. His father died while he was studying in Leipzig. Because this posed a financial threat to his academic career, Wundt hired the talented young man as his assistant.[33] Wundt could already have known about the instability of his new employee. In an earlier letter he had mentioned that he had 'reason to doubt the health of his mental state' and that he had often suffered from 'agonising passive fantasies [and] obsessive thoughts'.[34] Still, Lange seemed to function quite well in Wundt's laboratory during the next couple of years. Apart from his dissertation he published four more papers in the *Studien* between 1885 and 1888.[35] In 1887, however, he suffered his first bout of mania, which would be alternated with periods of severe depression in the next decades.[36] This forced him to quit academia. Looking back on his 'chronic suffering of several years' he admitted that 'considering the severe illness I could not do otherwise than to fail to do my duty'.[37] Lange might be the distinguished member of Wundt's institute who Kiesow later described as having become 'mentally deranged', which 'was interpreted unfavourably for the new methods of psychological observation'.[38] Still, Wundt continued to support him. He published the papers that, in his ever-shortening bright moments, Lange wrote in the *Studien* and supported his application for the position of university librarian in Leipzig in 1919.[39] His papers did not elicit much of a reaction from his peers, however, and his Leipzig application was turned down.

Though not all people mentioned above had a successful academic career, none of them suffered as sad a fate as Lange. Julius Merkel finished his dissertation with Wundt in 1883. It was immediately published in the *Philosophische Studien*.[40] During the following ten years, Wundt continued to publish his work. In addition, he also extensively commented on Merkel's manuscripts.[41] In some of his letters Wundt urged Merkel to present his findings in the light of

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[33] Laue, M. v., 'Dr. Ludwig Lange. 1863–1936. (Ein zu Unrecht Vergessener.),' *Die Naturwissenschaften*, 35(7), 1948, 193–196. 194.
[34] Ludwig Lange to Wilhelm Wundt, 9 June 1885. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[35] The dissertation was published in two parts: Lange, Ludwig, 'Die geschichtliche Entwicklung des Bewegungsbegriffes und ihr voraussichtliches Endergebnis. I.,' *Philosophische Studien*, 3(3), 1886, 337–419 and Lange, Ludwig, 'Die geschichtliche Entwicklung des Bewegungsbegriffes und ihr voraussichtliches Endergebnis. II. (Schluss),' *Philosophische Studien*, 3(4), 1886, 643–691.
[36] Laue, M. v., 'Dr. Ludwig Lange,' 194–195.
[37] Ludwig Lange to Wilhelm Wundt, 30 December 1887. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[38] Kiesow, F., 'F. Kiesow,' in: Murchison, Carl (ed.), *A history of psychology in autobiography*, volume I, Russell & Russell, New York (NY), 1961, 163–190. 172.
[39] Laue, M. v., 'Dr. Ludwig Lange,' 194.
[40] Merkel, Julius, 'Die zeitlichen Verhältnisse der Willenstätigkeit,' *Philosophische Studien*, 2(1), 1883. 73–127.
[41] For example, see Wilhelm Wundt to Julius Merkel, 28 March 1886, 5 October 1887 and 26 November 1891. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)

other research carried out in his laboratory, such as that by Karl Adolf Lorenz and Paul Adolf Starke. In other letters he expressed doubts about Merkel's methodology. He, for example, questioned the practicality of experiments on the measurement of the perception of certain sounds being twice as loud as others.[42] Meanwhile Merkel's academic career had come to a standstill. A few years after obtaining his doctorate, he found a job as a mathematics teacher at a high school in Zittau, a small-town in the south-east of Saxony.[43] He taught at this school until at least 1915.[44] So, even if Wundt's efforts to support Merkel's scholarly efforts succeeded in getting him a substantive number of publications in the *Studien*, they did not thus further his academic career.

Although it took him a long time, Friedrich Kiesow eventually succeeded in acquiring a professorship. Kiesow was already in his thirties when he started his study with Wundt, in 1891. He received his doctoral degree three years later.[45] After this accomplishment he was not, however, allowed to start working on his *Habiliation*, the requirement for an academic teaching position in Germany. This was because as a young man illness had kept him from receiving his *Abitur*, the high school qualification for university entrance without which it was hard to enter a university and impossible to qualify for submitting a *Habilitation*.[46] He therefore went to Turin to work with the Italian psychologist Angelo Mosso.[47] Meanwhile his relationship with Wundt ensured his continued visibility in Germany. Kiesow kept sending his manuscripts to Leipzig and Wundt usually published them immediately.[48] He even encouraged Kiesow to publish a paper that had already been printed in Mosso's *Archives Italiennes de Biologie* in the *Studien* as well, arguing that 'the circle of readers of [the *Studien*] and Mosso's *Archiv* don't overlap anyway'.[49] Wundt continued to publish Kiesow's

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[42] Wilhelm Wundt to Julius Merkel, 5 October 1886. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)

[43] Merkel is mentioned as Wissenschaftl. Lehrer für Mathematik in the annual report of the school in 1889: Schütze, Johannes, *Jahresbericht des Königl. Realgymnasium mit höherer Handelsschule zu Zittau für das Schuljahr 1888/89*, Zittau, 1889. 32. (accessed at http://digital.ub.uni-duesseldorf.de/ulbdsp/periodical/structure/7514853) This is the oldest annual report of the school that I have been able to find. However, already in 1887 Merkel sent a letter to Wundt from Zittau, suggesting that he already worked there in that year: Julius Merkel to Wilhelm Wundt, 8 October 1887. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)

[44] Merkel is mentioned as Professor für Mathematik und Physik in the 1915 annual report. This is the most recent annual report of the school that I have been able to find, so it is not unlikely that Merkel kept working here after this year: Korselt, Ernst, *Jahresbericht des Königlichen Realgymnasiums mit Höherer Handelsschule in Zittau für das Schuljahr Ostern 1914 bis Ostern 1915*, Zittau, 1915. 22.

[45] Kiesow is not included in the overview of dissertations written under the supervision of Wundt at http://home.uni-leipzig.de/wundtbriefe/wwcd/chronos/chronos.htm. However, in his autobiographical essay Kiesow mentions that he completed his doctoral work in Wundt's institute and that it was judged by both the zootomist Rudolf Leuckart and Wundt. See: Kiesow, F., 'F. Kiesow,' 172, 176.

[46] Robinson, 'Wilhelm Wundt and the Establishment of Experimental Psychology, 137.

[47] Kiesow, F., 'F. Kiesow,' 177.

[48] For example, see Wilhelm Wundt to Friedrich Kiesow, 7 June 1896 and 8 November 1898. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)

[49] Wilhelm Wundt to Friedrich Kiesow, 19 November 1897. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm) Kiesow, Friedrich, 'Un appareil simple pour determiner la sensibilité des points de temperature,' *Archives Italiennes de Biologie*, 30, 1898, 375–376; Kiesow, Friedrich, 'Ein einfacher Apparat zur Bestimmung der Empfindlichkeit von Temperaturpunkten,' *Philosophische Studien*, 14, 1898, 589–590.

work until he acquired the *libera docenza* title, the Italian equivalent of *Privatdozent*, in 1899.[50] Three years later he was appointed as associate professor. This delighted Wundt, who wrote Kiesow that among his most enjoyable recent experiences was the fact that 'two of my most diligent young assistants — you and Kirschmann — to whom the psychological teaching profession in Germany was barred for external reasons, have found an suitable position abroad'.[51] From this moment on, Kiesow would publish less regularly in Wundt's journals. The dissertations written by his own doctoral candidates after his promotion to full professor in 1906 could not be published in the successor of the *Philosophische Studien*, the *Archiv für die gesamte Psychologie* because most of them were written in Italian.[52] Ten years later Kiesow followed his teacher's example and founded his own journal, the *Archivio italiano di psicologia* for exactly this purpose.[53]

A final example of a pupil who spent most of his academic career in the periphery of German academia, to whom Wundt continued to give access to the pages of the *Studien* is Gottlob Friedrich Lipps. In 1888 Lipps received his doctorate after writing his dissertation under Wundt's supervision. Determined to write a *Habilitation* but not financially independent, he looked for a job in the vicinity of a German university. In this way he could both make money and stay in touch with academic life. He spent twelve long years in the vicinity of Strasbourg, first as a high school teacher in Hagenau, then in a similar position in the city itself. His teaching position did not, however, allow him to focus on his *Habilitation*.[54] After spending ten years in the Alsace without finishing his *Habilitation*, Lipps contacted Wundt to ask if he could help him land a job in Leipzig, which would allow him to write his *Habilitationschrift* under his supervision.[55] In 1902 he finally found a job at a Leipzig *Gymnasium*.[56] Two years later, sixteen years after receiving his doctorate, his *Habilitation* was approved. After teaching in Leipzig for some more years he was even appointed at Wundt's old chair in Zürich, in 1911. During all his years on the academic periphery, Wundt had

[50] Kiesow published one more essay in the *Philosophische Studien* in volume 19 in 1902, but volume 19 and 20 should not be counted as regular instalments because they were published as *Festschrifte* for Wundt's 70th birthday.
[51] Wilhelm Wundt to Friedrich Kiesow, 15 February 1902. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm) August Kirschmann was a student of Wundt as well. Like Kiesow he had not received his Abitur, so instead of writing a *Habilitation* he went to Toronto, where he would be appointed as full professor in 1902. See: Robinson, 'Wilhelm Wundt and the Establishment of Experimental Psychology,' 131. He would in the end be allowed to write a *Habilitation* in 1919, after the First World War had forced him back to Leipzig. See: https://portal.hogrefe.com/dorsch/kirschmann-august/.
[52] Wilhelm Wundt to Friedrich Kiesow, 6 September 1909. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[53] Friedrich Kiesow to Wilhelm Wundt, 31 December 1919. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[54] Wilhelm Wundt to Gottlob Friedrich Lipps, 26 December 1894. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[55] Wilhelm Wundt to Gottlob Friedrich Lipps, 4 February 1900. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[56] Wilhelm Wundt to Gottlob Friedrich Lipps, 15 February 1902. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)

not only published his work, he had also taken care of its immediate publication without even reading each individual paper.[57] Wundt's support of Lipps, which also consisted of getting him a job as the editor of a manuscript found in Fechner's estate, proved that his assistance of his less immediately successful students through continued exposure in the *Studien* occasionally paid off.[58] Unlike Lange and Merkel, Lipps ended up being a full professor and unlike Kiesow he accomplished this at a German language university.

Free from the need to make a profit and in close collaboration with his loyal students and co-workers, Wundt published a journal with a clear voice and focus. Looking back at his first year as a journal editor, Wundt remarked with some sarcasm 'I just don't have to tell the reader, who has informed himself of the content of the preceding papers, that it has not in the least been my intention to provide an open podium where the advocates of all possible and impossible philosophical points of view can raise their voices to their heart's content'.[59] Contrary to other philosophical journals, he had decided not to focus on subjects he caustically summed up as 'immanence and transcendence', 'understanding of being' and 'a typo in Kant'. Instead, he argued that his modest aim had been 'to publish a number of papers about philosophical issues, the treatment of which seems to be promising to me'. Meanwhile Wundt, of course, also realised that the legitimation and propagation of experimental methods in philosophy had been another, less modest, intention. On a more practical level, providing a platform for the research carried out under his supervision by his most promising students and other associates was another consciously pursued goal.

The omnipotence of Heinrich Leberecht Fleischer

The *Zeitschrift der Deutschen Mörgenländischen Gesellschaft* was founded in 1846 as the internal organ of the newly established *Deutsche Morgenländische Gesellschaft* and as the successor of Christian Lassen's *Zeitschrift für die Kunde des Morgenlandes*. In its first preface the editorial board emphasised the broad audience it intended to reach, calling upon 'all those who are somehow affected by the current upswing in oriental studies in Germany or who are themselves participating' for support.[60] The fact

[57] For example, see Wilhelm Wundt to Gottlob Friedrich Lipps, 15 February 1894 and 1 December 1898. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[58] The invitation to work on Fechner's manuscript can be found in: Wilhelm Wundt to Gottlob Friedrich Lipps, 26 December 1894. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[59] Wundt, Wilhelm, 'Schlusswort zum ersten Bande,' *Philosophische Studien*, I, 1883, 615–617.
[60] Rödiger, Emil, August Pott, Heinrich Leberecht Fleischer and Hermann Brockhaus, 'Vorwort,' *ZDMG*, 1, 1847, III–VI. VI.

that all paying members of the *DMG* received the *Zeitschrift* assured the journal's commercial viability. In addition, the close relationships between the *DMG* and the publisher of its journal further promoted editorial independence. Hermann Brockhaus, sanskritist and co-founder of the *DMG* and its journal, was the son of Friedrich Arnold Brockhaus, the founder of the famous Leipzig publishing house F.A. Brockhaus. The Brockhaus family was happy to support a financially risk-free publishing initiative by one of its own members. The relationship between the *DMG* and the publisher of its journal was so close that from 1862 onwards the firm F.A. Brockhaus, through its representative O. Matthesius, officially held the position of treasurer of the society.[61] However, even if Hermann Brockhaus was important in establishing a good working relationship with his family's publishing house, the most influential early editor of the *Zeitschrift* would be Heinrich Leberecht Fleischer, who wholeheartedly embraced his editorial independence.

Between the 1840s and 1880s Fleischer's influence on the *Zeitschrift* cannot be underestimated. As the correspondence of Nöldeke and De Goeje shows, Fleischer often made major changes to submitted manuscripts. Nöldeke described his efforts as follows: 'By and by, apart from the language, the shape of the essay will still change, tremendously; I know how ruthless Fleischer is in deleting and correcting mistakes, how he often translates passages anew. […] I can assure you that many articles in the *Zeitschrift* deserve Fleischer's name rather than that of the original author'.[62] De Goeje was acutely aware of Fleischer's thorough editorial practices as well. After noticing some changes in a paper he had submitted to the *Zeitschrift*, he wryly noted that 'Fleischer [was] responsible for all those strange German words, which I would never have come up with myself.'[63]

De Goeje and Nöldeke never complained about these amendments, but they did criticise another editorial habit of Fleischer: his tendency to publish excerpts of private correspondence without asking for prior consent. De Goeje experienced this in 1862, when he suddenly noticed excerpts of a private letter about his research findings and plans in the *Zeitschrift*.[64] In previous centuries, this would not have been extraordinary; it was expected that editors with a 'particularly active and large correspondence' shared what they deemed to be of broad general interest.[65] In the 1860s, however, this was not standard practice and De Goeje desperately asked Nöldeke: 'What can be done about this? If you write to [Fleischer], do you write 'confidential' above the letters that are not intended

[61] Pischel, Richard, Franz Praetorius, Ludolf Krehl and Ernst Windisch, *Die Deutsche Morgenländische Gesellschaft 1845–1895. Ein Ueberblick*, Leipzig, F.A. Brockhaus, 1895. 41.
[62] UBL: BPL 2389, Theodor Nöldeke to Michael Jan de Goeje, 25 September 1858.
[63] UBL: BPL 2389, Michael Jan de Goeje to Theodor Nöldeke, 21 October 1864.
[64] Goeje, Michael Jan de, 'Von Herrn Dr. de Goeje,' *ZDMG*, 17, 1863, 393–394.
[65] Yale, Elizabeth, *Sociable Knowledge: Natural History and the Nation in Early Modern Britain*, University of Pennsylvania Press, Philadelphia (PA), 2016. 65.

for a wider audience?'[66] In his defence, Fleischer pointed out that De Goeje's letter was of general interest and emphasised that the publication of his plans might convince others to refrain from 'competing' with him.[67] De Goeje grudgingly accepted Fleischer's ways, which were a common practice at the *Zeitschrift,* after all. The issue of the *Zeitschrift* in which his correspondence with Fleischer had been published also contained excerpts of letters to Fleischer's co-editors Hermann Brockhaus and Emil Rödiger.[68] All that De Goeje could do was to privately complain that his Leiden colleague J.P.N. Land had suffered the same fate and that his English friend and colleague William Wright had been livid.[69]

Fleischer used his far-reaching editorial influence not only to ensure that all papers in the *Zeitschrift* would live up to his standards of scholarly excellence, but also — because the aim of the *Zeitschrift* was to represent all German orientalists — to make sure that no member of the society would feel excluded or marginalised. Especially when a member's particular piece of work merited a harsh rebuke, it was difficult to strike a balance between honest scholarly criticism and collegial loyalty. The difficulty of this balancing act repeatedly shows itself in his correspondence with Nöldeke. In 1879, for example, Fleischer received a review copy of Friedrich Heinrich Dieterici's text edition of the fairy tale collection *Thier und Mensch*, which was full of mistakes.[70] Realising that he could not publish a favourable review, Fleischer tried at least to soften the blow. First, he wrote Dieterici a letter to prepare him for some heavy criticism. Then, he wrote Nöldeke to ask if he could write a serious and rigorous review in which he would avoid any inclination to ridicule Dieterici, something that he could not expect from younger, less restrained reviewers.[71] This review by Nöldeke was never published in the *Zeitschrift*, however.[72] Later that year, Nöldeke published a favourable review in the *Literarisches Centralblatt*, in which he emphasised the importance of the text edition and praised Dieterici's decision to add a list of corrections based on Fleischer's earlier privately voiced criticism.[73] In the end, Fleischer had succeeded in ensuring a friendly reception of Dieterici's text edition without explicitly going against his initial harsh judgement.

Another example of Fleischer's balancing act is that of his handling of an affair that took place in the 1870s as a result of strong disagreements about the authenticity of newly found Moabite

[66] UBL: BPL: 2389, Michael Jan de Goeje to Theodor Nöldeke, 9 November 1862.
[67] UBL: BPL: 2389, Heinrich Leberecht Fleischer to Michael Jan de Goeje, 17 March 1863.
[68] See: 'Aus Briefen an Prof. Brockhaus' and 'Aus Briefen an Herrn Prof. Rödiger,' ZDMG, 17, 1863, 382–390 and 395–397.
[69] UBL: BPL: 2389, Michael Jan de Goeje to Theodor Nöldeke, 9 November 1862.
[70] Dieterici, Friedrich, *Thier und Mensch vor dem König der Genien*, Leipz, Hinrichs, 1879.
[71] Universitätsbibliothek Tübingen (hereafter UBT) Md 782 A 68, Heinrich Leberecht Fleischer to Theodor Nöldeke, 29 April 1879.
[72] UBT: Md 782 A 68, Heinrich Leberecht Fleischer to Theodor Nöldeke, 4 August 1879.
[73] Nöldeke, Theodor, 'Thier und Mensch vor dem König der Genien,' *Literarisches Centralblatt*, 1879, 1259–1260.

artefacts. The discovery of the contentious artefacts was preceded by that of the Mesha inscription in the late 1860s. Even today, this discovery is still described as 'the greatest Biblical discovery of modern times'.[74] After this inscription had stirred up European interest in so-called Moabitica, the Jerusalem antiques shop owner Moses Shapira showed a large collection of earthenware and figurines with Moabite inscriptions to the German pastor Hermann Weser. Weser immediately wrote Konstantin Schlottmann, member of the board of the *DMG* and co-editor of its *Zeitschrift*.[75] Schlottmann published a glowing report in the *Zeitschrift*.[76] He was the only expert in this field on the board of the *DMG* and he convinced its general assembly to both promise the fast and complete publication of a description of the artefacts and to advice the Prussian Ministry of Education to purchase the whole collection.[77] The ministry did not waste any time. Not only did they immediately buy the collection, but, one year later, they also purchased a similar one from the same seller.[78] Within a couple of months, however, Albert Socin published the first critical assessment of the authenticity of the collection in the *Zeitschrift*.[79] Fleischer feared that this would be the beginning of a painful polemic between the society's members. The fact that its general assembly had followed Schlottmann in providing an advice that had convinced the Berlin government to waste a large amount of money, made the situation even more delicate.

After Socin had published his first criticism of Shapira's wares, more people expressed doubts about their authenticity. Explicitly referring to the government involvement Fleischer tried to persuade Socin to postpone further critical assessments until Schlottmann would have published all his findings.[80] This call for caution was initially quite successful among all German colleagues. Most early critical evaluations of the collections bought by the ministry were written by foreign scholars and published abroad.[81] The discussion in Germany did not continue until after an article by Schlottmann in the *Zeitschrift,* in which he defended the collection's authenticity against the allegations of his French detractor Charles Clermont-Ganneau.[82] Realising that he could no longer

[74] Quoted in: Graham, M. Patrick, 'The Discovery and Reconstruction of the Mesha' Inscription,' in: Dearman, Andrew (ed.), *Studies in the Mesha inscription and Moab*, Scholars Press, Atlanta (GA), 1989, 41–92. 42.
[75] Heide, Martin, 'The Moabitica and Their Aftermath: How to Handle a Forgery Affair with an International Impact,' in: Lubetski, Meir and Edith Lubetski (eds.), *New inscriptions and seals relating to the biblical world*, Society of Biblical Literature, Atlanta (GA), 2012, 193–242. 194–195.
[76] Schlottmann, Konstantin, 'Neue Moabitische Funde und Räthsel: Erster Bericht,' *ZDMG*, 26, 1872, 393–416.
[77] 'Protokollarischer Bericht über die in Halle a/S. am 28. Sept. 1872 abgehaltene Generalversammlung d. D.M.G.,' *ZDMG*, 27, 1973, IV–VI. VI.
[78] Heide, Martin, 'The Moabitica and Their Aftermath,' 198.
[79] Socin, Albert, 'Ueber Inschriftfälschungen,' *ZDMG*, 27, 1873, 133–135.
[80] UBT: Md 782 A 68, Heinrich Leberecht Fleischer to Albert Socin, 22 October1873. Since this letter was found between the letters that Fleischer had sent to Nöldeke, it is probably a copy of his letter to Socin.
[81] Heide, Martin, 'The Moabitica and Their Aftermath,' 200.
[82] Schlottmann, Konstantin, 'Ueber die Aechtheit der Moabitischen Alterthümer,' *ZDMG*, 28, 1874, 171–184.

suppress the debate in Germany, Fleischer urged Nöldeke to use all his influence to make sure that Schlottmann's critics would at least not attack him outside the pages of the *Zeitschrift*.[83]

Although the editors of the *Zeitschrift* were averse to controversy in its content, Fleischer had good reasons for giving Schlottmann's critics some leeway. The first reason was that, as the representative organ of a whole profession, the *DMG* was not supposed to openly side with one of its members. Fleischer could not risk the impression that he was trying to silence Schlottmann's critics. The second reason was that, if the discussion could be contained to the *Zeitschrift*, he would be able to ensure that it would be polite and professional. This ambition, however, was infeasible. In 1875 and 1876, the authenticity of the Moabitica was discussed in a myriad of German books and journals.[84] Nöldeke added to the discussion with a warning against Shapira's business, in his review of Socin's Baedeker travel guide, a long essay in the *Deutsche Rundschau* and a book review in the *Literarische Centralblatt*.[85] Though Fleischer could not confine the debate to the pages of the *Zeitschrift*, he was successful in another respect — his repeated admonitions to refrain from offensive remarks and personal attacks paid off: '[…] with God's help, the case […] has solved itself. The 'God' that helped us with this, is mainly the spirit of moderation in thought and expression of thought, for which I have to praise the opponents Schlottmann and Kautzsch. […] the opponents focused on an honest dispute, which is unimaginable without mutual respect and the avoidance of all personal remarks and offensive insinuations.'[86]

The struggles of August Fischer

From 1903 onwards August Fischer, who had been appointed to Fleischer's old chair in Leipzig in 1900, would be the editor-in-chief of the *Zeitschrift*. In this capacity he faced the same challenges as Fleischer. On the one hand, a journal aiming to represent the whole community of orientalists had good reason to stay away from controversy. This provided an incentive to limit disagreement and debate on its pages. On the other hand, this same representative function obliged him to refrain from openly taking sides in debates between disagreeing scholars. This forced him to allow at least

[83] UBT: Md 782 A 68, Heinrich Leberecht Fleischer to Theodor Nöldeke, 28 June 1875.
[84] For example, see Diestel, Ludwig, 'Die moabitischen Alterthümer,' *Jahrbücher für deutsche Theologie*, 21, 1876, 451–473; Kautzsch, Emil and Albert Socin, *Die Aechtheit der moabitischen Altertümer geprüft*, Strasbourg, Trübner, 1876; Koch, Adolf, *Moabitisch oder Selimisch? Die Frage der moabitischen Altertümer*, Stuttgart, Schweizerbart, 1876.
[85] Nöldeke, Theodor, '"Bädeker" im Morgenlande,' *Im neuen Reich. Wochenschrift für das Leben des deutschen Volkes in Staat, Wissenschaft und Kunst*, 5. Jahrgang, 2. Buch, 1875, 15–20. 19; Nöldeke, Theodor, 'Die moabitischen Fälschungen,' *Deutsche Rundschau*, 6, 1876, 447–451; Nöldeke, Theodor, 'Koch. Ad, Moabitisch oder Selimisch?,' *LC*, 1976 (13), 425–428.
[86] UBT: Md 782 A 68, Heinrich Leberecht Fleischer to Theodor Nöldeke, 21 July 1875.

some room for disagreement and debate. Lacking Fleischer's fatherly authority — De Goeje sometimes talked about 'Papa Fleischer' and Nöldeke about 'der gute Alte' — Fischer's task proved to be even more daunting than his predecessor's.[87]

In editing manuscripts, Fischer took liberties that were similar to those of Fleischer; he deleted inappropriate expressions and corrected any mistakes he encountered.[88] However, unlike Fleischer's amendments, Fischer's changes drew public criticism. In 1905, the executive board of the *DMG* received a complaint from Gustav Jahn, an Emeritus Professor of Semitic Languages in Königsberg.[89] He protested the fact that the text of his manuscript about the Mesha inscription had been changed by Fischer without prior consultation and he angrily asked if the charter of the *DMG* even allowed this.[90] The members of the board were not very sympathetic to Jahn's plight. Nobody disputed Franz Praetorius' harsh opinion that 'it is generally known that Jahn's intellectual powers haven't been normal for quite some time' and that 'his scholarly works have slipped more and more into the domain of the ridiculous'.[91] One board member noted that even though his impression of him was 'unpleasant', Jahn still deserved a well-motivated reply; after all, members of the *DMG* did have the right to turn to the executive board, in cases of disagreement.[92] Fischer explained that most of his deletions had been necessary because Jahn had personally attacked scholars he disagreed with by dismissing them as being 'dull-witted orthodox' and accusing them of lack of honesty and moral courage.[93] The other deletions, Fischer argued, had been 'so minor that only a troublemaker or a lunatic' would bother to protest them.[94]

Ernst Windisch, a long-time co-editor of the *Zeitschrift*, defended Fischer's decisions. It was the task of the editor-in-chief, he argued, to ensure that no scholar would be provoked without good reason. After all, this would only lead to the sort of debate for which the pages of the *Zeitschrift* were not intended.[95] The only alternative to editing Jahn's polemic submission, would have been to reject it altogether. This, however, had not been an option, since it had been a reaction to a recently

[87] UBL: BPL 2389, Michael Jan de Goeje to Theodor Nöldeke, 19 November 1876, 7 January 1878 and 4 December 1882; UBL: Or. 5585e, Theodor Nöldeke to Michael Jan de Goeje, 11 October 1876.
[88] See, for example, his comments in: Universitätsarchiv Halle-Wittenberg (hereafter UAHW): Rep. 90:67, August Fischer to the executive board of the *DMG*, 9 November 1905; UAHW: Rep. 90:70, August Fischer to the executive board of the *DMG*, 4 April 1908.
[89] Mangold, Sabine, *Eine „weltbürgerliche Wissenschaft" – Die deutsche Orientalistik im 19. Jahrhundert*, Franz Steiner, Stuttgart, 2004. 94–95.
[90] UAHW: Rep. 90:67, Gustav Jahn to the executive board of the *DMG*, 8 November 1905.
[91] UAHW: Rep. 90:67, Franz Praetorius to the executive board of the *DMG*, 9 November 1905.
[92] UAHW: Rep. 90:67, Ernst Windisch to the executive board of the *DMG*, 12 November 1905.
[93] UAHW: Rep. 90:67, August Fischer to the executive board of the DMG, 10 November 1905.
[94] UAHW: Rep. 90:67, August Fischer to the executive board of the *DMG*, 12 November 1905.
[95] UAHW: Rep. 90:67, Ernst Windisch to the executive board of the *DMG*, 12 November 1905.

published article by Eduard König and the editor had to allow this diversity in viewpoints.[96] While Jahn was annoyed by Fischer's changes to his manuscript, Fischer would have preferred not publishing anything by him at all: 'I have […] regretted the inclusion of König's essay for a long time, because it means that I have to let [Jahn], who does not produce anything that is not complete or half rubbish, have his say in the *ZDMG,* as well.'[97] To terminate the debate, Fischer even added a footnote to König's reply to Jahn's criticism, stating that the discussion should not be continued in the *Zeitschrift* and that he had only allowed the criticism and the reply 'for reasons of fairness'.[98]

Fischer's lack of the paternal authority was not the only reason he found himself in more profound editorial quarrels than Fleischer. He also took more liberties than his predecessor. In the same year that Jahn filed his complaint with the executive board, Fischer drew criticism for his own writing as well. He had started a new section in the *Zeitschrift*, the editorial glosses, in which he published 'short remarks with critical or complementary content in a casual way, as they came to me while reading the essays and announcements that I received for the *Zeitschrift*'.[99] He stated that he hoped that nobody would attribute polemic motivations to him and that nobody would be offended. This proved to be wishful thinking. Jakob Barth, who Fischer had especially singled out for criticism, entered into a sharp and prolonged debate with him both in private as well as on the pages of the *Zeitschrift*.[100] Barth's indignation became widely known, when he shared his anger in his private correspondence with some colleagues, such as Nöldeke, with whom he had studied, and De Goeje, on whose al-Ṭabarī edition he had worked.

In those private circles sharp criticism of Fischer circulated. Nöldeke argued that Fischer did not have the right to place himself above his authors in such a pedantic way in letters to De Goeje and Goldziher.[101] De Goeje agreed that Fischer must have acted on bad advice when he published his glosses.[102] A worried Fischer, who had heard of the commotion his words had caused, wrote to De

[96] The published contributions in the *ZDMG* on this debate were: König, Eduard, 'Ist die Mesa-Inschrift ein Falsifikat?,' *ZDMG*, 59, 1905, 233–251; Jahn, Gustav, 'Die Mesha-Inschrift und ihr neuester Vertheidiger. In Verbindung mit einer Textkritik mehrerer Bibelstellen,' *ZDMG*, 59, 1905, 723–742; König, Eduard, 'Mesa-Inschrift, Sprachgeschichte und Textkritik,' *ZDMG*, 743–756.
[97] UAHW: Rep. 90:67, Franz Praetorius to the executive board of the *DMG*, 9 November 1905.
[98] Footnote to: König, Eduard, 'Mesa-Inschrift, Sprachgeschichte und Textkritik,' 756.
[99] Fischer, August, 'Redakteurglossen', *ZDMG*, 59, 1905, 442–456. 442. See also: Engberts, Christiaan, 'The Scholar as Judge,' 105–106.
[100] The writings by Fischer and Barth printed in the Zeitschrift were: Barth, Jakob, 'Ursemit. *e*, zum Demonstrativ *ḏ*, *tī* und Verwandtes,' *ZDMG*, 59, 1905, 633–643; Fischer, August, 'II.,' *ZDMG*, 59, 1905, 644–671. Fischer mentions his unfriendly private correspondence with Barth during this period in: UAHW: Rep. 90:70, August Fischer to the executive board of the *DMG*, 4 April 1908.
[101] UBL: BPL 2389, Theodor Nöldeke to Michael Jan de Goeje, 8 July 1905; MTAK: GIL/32/01, Theodor Nöldeke to Ignaz Goldziher, 10 July 1905.
[102] UBL: BPL 2389, Michael Jan de Goeje to Theodor Nöldeke, 17 July 1905.

Goeje to ask him if he thought his glosses had damaged the cause of the *DMG*.[103] De Goeje's reply must have been unexpectedly harsh, judging by the rather upset tone of Fischer's next letter: 'To be honest, the extent to which I would have aimed with my glosses to give a 'final criticism' or to function as 'chief justice' in front of whom 'no appeal is possible' is incomprehensible to me.[104] Although the executive board of the *DMG* supported Fischer, he announced the discontinuation of his glosses, at the society's general assembly of 1905.[105] This, however, would not be the end of the public hostilities between Barth and Fischer.

The resolution of their dispute had not satisfied Barth. He decided to continue the debate in his 1907 book *Sprachwissenschaftliche Untersuchungen zum Semitischen*. He lambasted Fischer for his 'intemperate outbursts that are otherwise not common in scholarly communication' and even asserted that Fischer had 'concealed scientific facts'.[106] An angry Fischer used the *Zeitschrift* to put Barth back in his place with snide remarks, such as 'With excessive confidence alone, one cannot make it in scholarship in the long run, at least not in Arabic studies'.[107] After reading Fischer's diatribes, Barth submitted a reply for publication in the *Zeitschrift*. The vicious tone of this reply led to a drawn-out discussion about the appropriateness of its publication. Praetorius argued that this 'sad history would only become sadder with the publication of an upset, likewise personally targeted reply by Barth'.[108] The affronted Fischer was even clearer: 'Everywhere in modern society an inflicted injustice or insult is amended either by taking it back, or by calling the offender to order, but not by giving the offended the right to insult the offender now to the best of his abilities or, if possible, to outdo him'.[109] Most members of the board agreed that Barth's reply should not be printed and that the publication of a short apology by Fischer would suffice. The only disagreeing member of the board was Nöldeke. He argued that Barth was denied his right to reply to Fischer's allegations and suggested Fischer should resign from his position as editor-in-chief.[110] One day after stating his opinion to the board, Nöldeke told De Goeje that 'no matter how the struggle between Fischer and Barth may turn out, it is certain that Fischer will surrender his editorship. And that is good!'[111]

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[103] UBL: BPL 2389, August Fischer to Michael Jan de Goeje, 19 July 1905.
[104] UBL: BPL 2389, August Fischer to Michael Jan de Goeje, 18 September 1905.
[105] 'Protokollarischer Bericht über die am 2. und 3. Oktober 1905 zu Hamburg abgehaltene allgemeine Versammlung der D.M.G.,' *ZDMG*, 1905, LXXI–LXXIII. LXXII.
[106] Barth, Jakob, *Sprachwissenschaftliche Untersuchungen zum Semitischen*, J.C. Hinrichs'sche Buchhandlung, Leipzig, 1907. 30.
[107] Fischer, August, 'Allerlei von J. Barth „verbesserte" arabische Dichterstellen,' *ZDMG*, 61, 1907, 926–938. 938.
[108] UAHW: Rep. 90:70, Franz Praetorius to the full board of the *DMG*, no date [spring 1908].
[109] UAHW: Rep. 90:70, August Fischer to the full board of the *DMG*, 4 April 1908.
[110] UAHW: Rep. 90:70, Theodor Nöldeke to the full board of the *DMG*, 10 April 1908.
[111] UBL: BPL 2389, Theodor Nöldeke to Michael Jan de Goeje, 11 April 1908.

Nöldeke did not get his way. Barth's reply was not printed. Fischer published something close to an apology, in which he stated that if people thought he should apologise, he would be willing to express his regrets about the harshness of his words.[112] Nöldeke disappointedly announced that he would no longer contribute to the *Zeitschrift* as long as Fischer would remain its editor.[113] Barth revoked his membership of the *DMG*.[114] Other members of the *DMG* also took offense with the settlement. At the general assembly, Carl Heinrich Becker presented an open letter in which he objected to the bad manners that Fischer and the board had promoted through their treatment of Barth. The letter was co-signed by twenty-five colleagues, among whom we find influential scholars like Nöldeke, Goldziher, Carl Bezold and Christiaan Snouck Hurgronje.[115] Although the signatories did not accomplish more than the inclusion of their open letter in the report of the 1908 general assembly, Fischer's days at the *Zeitschrift* were numbered. In the aftermath of the first Barth affair, he had already considered resigning.[116] When Praetorius, his most staunch defender, left the board in 1910, Fischer announced his departure as well.[117] That same year Barth rejoined the *DMG* and Nöldeke contributed to the *Zeitschrift* again.[118]

If we compare the extent to which the editors of the *Zeitschrift* could shape their journal to the influence that Wundt had on his *Studien*, we find similarities as well as differences. Two remarkable similarities are that both periodicals did not have to worry about their financial viability and that the editors of both journals had an aversion to prolonged disputes.

The most striking difference is rooted in the relationship with their contributors and intended audiences. Wundt's journal aimed to provide a platform for people who were intellectually close to him — often his own students or co-workers — without having to worry about appealing to a diverse readership. This explains two characteristic of editorial policies of the *Studien*. In the first place there was often hardly any need for the critical evaluation of submitted papers, because they had been written either under Wundt's supervision or by long-time associates. Secondly, it was easy to keep prolonged debate and controversy off the journal's pages; almost all contributors were dedicated to the same Wundtian approach to experimental psychology.

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[112] Fischer, August, 'Erklärung,' *ZDMG*, 62, 1908, 203.
[113] UBL: BPL 2389, Theodor Nöldeke to Michael Jan de Goeje, 10 May 1908.
[114] 'Personalnachrichten,' *ZDMG*, 62, 1908, XL.
[115] 'Protokollarischer Bericht über die Mittwoch den 14. Okt. 1908 zu Leipzig abgehaltene Allgemeine Versammlung der D.M.G.,' *ZDMG*, 62, 1908, XLV–XLIX. XLVI–XLVII.
[116] UAHW: Rep. 90:70, August Fischer to the full board of the *DMG*, 4 April 1908.
[117] 'Redaktionswechsel,' *ZDMG*, 64, 1910, XXXVII.
[118] 'Mitgliedernachrichten,' ZDMG, 64, 1910, XLV; MTAK: GIL/32/01/241, Theodor Nöldeke to Ignaz Goldziher, 12 May 1910; Nöldeke, Theodor, 'Zum „Buch der Gesetze der Länder,' *ZDMG*, 64, 1910, 555–560.

Unlike the *Studien*, the *Zeitschrift* was published by a society that claimed to represent a broad group of scholars. Although this society did not want to offend any member of its constituency, this could not be accomplished by keeping all disagreement out of its journal. Such censorship would have been more offensive than the printing of politely worded scholarly criticism. When Fleischer, the well-respected *eminence grise* of the Arabic studies in Germany, was in charge, he exerted a strong influence on potentially explosive disagreements. When he was succeeded by Fischer, a less widely respected scholar, it became increasingly clear how hard it was to strike a balance between admissible criticism and polite disagreement. His decisions were contested and became major points of discussion both on the board of the *DMG* and at the meetings of its general assembly. These issues did not, however, figure prominently in the *Zeitschrift*, but were mostly limited to private correspondence and the reports of the general assembly.

The birth of a review journal

Even if their journals were published by commercial publishers, Wundt, Fleischer and Fischer could neglect financial considerations. However, unlike Wundt, most editors could not rely on of the publication of profitable books, or, like the *DMG*, fall back on a co-editor with family ties to a publishing house. The *Literarische Centralblatt für Deutschland* was one of the many scholarly journals that had to turn a profit to survive. It was founded as a weekly review journal in 1850 in the wake of the closedown of similar journals — like the *Literarische Zeitung*, discontinued in 1849, and the *Jenaische Allgemeine Literatur-Zeitung*, shut down in 1848.[119] The new journal was created by the publisher Georg Wigand and classical scholar Otto Jahn and legal scholar and historian Theodor Mommsen, both associated with Leipzig University.[120] The people most commonly associated with the journal, however, would be its subsequent publisher Eduard Avenarius, the father of Richard Avenarius, and its long-time editor, Friedrich Zarncke. Zarncke had been involved with the *Centralblatt* from the beginning and he was already mentioned as its editor in the first issue. Jahn and Mommsen left Leipzig after they were fired from their university positions in the aftermath of the revolts of 1848 and 1849.[121] From now on the responsibility for the journal would be Zarncke's alone. The departure of Jahn and Mommsen is probably one of the reasons why Wigand, who moved in the same liberal circles, decided to get rid of the *Centralblatt*. From 1852 onwards it was

[119] Kirchner, Joachim, *Das deutsche Zeitschriftenwesen*, 70–71, 242.
[120] Lick, Thomas, *Friedrich Zarncke und das „Literarische Centralblatt für Deutschland": Eine buchgeschichtliche Untersuchung*, Harrasowitz, Wiesbaden, 1993. 13–14.
[121] Rebenich, Stefan, *Theodor Mommsen: Eine Biographie*, C.H. Beck, München, 2002. 70–71.

published by the firm Avenarius & Mendelssohn. From 1855 onwards Eduard Avenarius would be its sole publisher.[122]

Avenarius' son Ludwig later described his father as a publisher with little regard for the journal's profitability: '[...] for him it was less about lavish monetary profit than about serving the public good'.[123] But even if Avenarius may not have been the most calculating entrepreneur, he still wanted his publishing house to be profitable. His letters to Zarncke show how both men tried to meet the demands of the marketplace. They repeatedly discussed ideas for new sections that could attract a larger readership. In 1862 they discussed the pros and cons of including overviews of the courses offered at various German universities. An increase in the number of subscriptions could not realistically be expected because a large part of the target audience for such announcements already read the journal. However, because Avenarius thought that the inclusion of such overviews could potentially increase their income from advertisements, they decided in favour of it.[124]

To cement the relationship between the *Centralblatt* and the German universities, Avenarius also repeatedly suggested to add a section on academic news. Initially he proposed to simply report 'promotions and deaths'.[125] Two years later he suggested to print reports on what 'from certain sides could be interpreted as gossip', arguing that sometimes throwing 'a pike in the carp pool' — stirring things up a little — might not hurt.[126] Two weeks later Zarncke added the first *Personalnachrichten* to the *Centralblatt*. Their matter-of-fact tone was more in line with Avenarius 1862 proposal than with his more sensationalist later suggestion.[127] Not all of Avenarius' suggestions were implemented, though. His idea to add a section called 'Questions to the scholarly world' never materialised, nor did his desire to also publish reviews of 'the most outstanding publications in the belletristic literature'.[128]

Until the early 1870s, Avenarius and Zarncke did not have to worry about the viability of their journal. In 1874, however, a new journal that largely covered the same ground and aimed for the same audience was established, the *Jenaer Literaturzeitung*. The *Literaturzeitung*, as it was known, was founded as the successor to the similarly named *Jenaische Allgemeine Literatur-Zeitung* and was edited by the Jena university librarian Anton Klette. Working in Bonn in the 1860s, Klette had earlier co-

122 Lick, Thomas, *Friedrich Zarncke*, 48–51.
123 Avenarius, Ludwig, *Avenarianische Chronik: Blätter aus drei Jahrhunderten einer deutschen Bürgerfamilie*, O.R. Reisland, Leipzig, 1912. 86.
124 Universitätsbibliothek Leipzig (hereafter UBLE), NL 249/1/A/570, Eduard Avenarius to Friedrich Zarncke, 20 March 1862.
125 UBLE: NL 249/1/A/569, Eduard Avenarius to Friedrich Zarncke, 15 May 1862.
126 UBLE: NL 249/1/A/565, Eduard Avenarius to Friedrich Zarncke, 28 May 1864.
127 See also: Lick, Thomas, *Friedrich Zarncke*, 70–71.
128 UBLE: NL 249/1/A/565, Eduard Avenarius to Friedrich Zarncke, 28 May 1864.

edited the *Rheinische Museum für Philologie* and he enthusiastically embraced the opportunity to edit a more ambitious journal as part of his new job.[129] Like the *Centralblatt*, the *Literaturzeitung* appeared on a weekly basis, and Avenarius was quick to point out the threat that it posed to his journal. In March 1874, he explicitly called the *Literaturzeitung* 'our rival' and underlined the importance of keeping track of 'the competition from Jena'.

With some complacency, Avenarius pointed out that, in the first eleven issues of 1874, the *Centralblatt* had reviewed 235 books, while the *Literaturzeitung* had only covered 166 works. He realised, though, that it would be risky to advertise with this feat. Their competitors could then argue that they indeed reviewed 'fewer books, but of course all the important ones, and these more extensively'.[130] Although Avenarius and Zarncke decided against openly advertising the larger number of works reviewed in their journal, they kept a close eye on these numbers. Two years later, Avenarius pointed out that the *Centralblatt* had reviewed 1131 works in 1874 and 1199 works in 1875. For the *Literaturzeitung*, this was 789 and 815, respectively.[131] The fact that the *Centralblatt* published so many reviews was in itself a good thing, but Avenarius did not show too much enthusiasm; the increased number of reviews was partly caused by a decrease in the number of advertisements. All in all, a comparison between the numbers of works reviewed by the two journals did not provide the *Literaturzeitung* any straightforward clues for dealing with their rival.

Another way of responding to the competition was that Avenarius and Zarncke personally addressed the people who contributed to both journals. Avenarius calculated that ninety-five contributors to the *Centralblatt* had also contributed to the *Literaturzeitung*. He wrote Zarncke that he worried that if they had already reviewed a work in the *Literaturzeitung*, they might refuse to also review it in the *Centralblatt*.[132] A letter to Wundt shows that Zarncke indeed raised the issue in his correspondence with regular contributors. Before he founded the *Studien,* Wundt frequently contributed to the *Centralblatt*. In 1874, he had answered positively to a request from Jena to one day contribute to their journal. Not until in the winter of 1875, however, he was asked to review a large number of books. These reviews were immediately noticed by Zarncke. Wundt was able to reassure him; he told Zarncke that he had indeed received the same books form Leipzig and Jena, recently, and had chosen to only review these books for the *Centralblatt*.[133] Wundt would prove his loyalty to the *Centralblatt* in the following years. In 1876 and 1877, he would write a total of 38 book

[129] Stössel, Waldemar, 'Anton Klette: 1870–1878,' in: [Arbeitsgemeinschaft wissenschaftlicher Bibliothekare der Universitätsbibliothek Jena], *Geschichte der Universitätsbibliothek Jena*, Hermann Böhlhaus Nachfolger, Weimar, 1958, 485–515. 490, 492.
[130] UBLE: NL 249/1/A/559, Eduard Avenarius to Friedrich Zarncke, 22 March 1874.
[131] UBLE: NL 249/1/A/537, Eduard Avenarius to Friedrich Zarncke, 11 February 1876.
[132] UBLE: NL 249/1/A/535, Eduard Avenarius to Friedrich Zarncke, 1 March 1875.
[133] UAL: NA Wundt/III/966, Wilhelm Wundt to Friedrich Zarncke, 3 January 1876.

reviews for Zarncke, while his rival from Jena would only receive five contributions. After 1877 he stopped contributing to the *Literaturzeitung* altogether.[134]

Another way of dealing with the new competition discussed between Avenarius and Zarncke concerned the anonymity of their reviews. During the first twenty-five years of its existence, the *Centralblatt* either published its reviews anonymously or had them signed with a short cipher. During the 1860s, for example, Nöldeke often signed with a cross resembling the letter 'X'.[135] This was in line with common practices abroad. In Britain reviews were usually published anonymously as well, thus creating 'a sense of the author as a neutral, all-seeing guide, free from human subjectivity'.[136] Zarncke's justification of the practice emphasised similar considerations. In a retrospective he pointed out that anonymous reviews encouraged substantive scholarly debate instead of personal recriminations and quarrels. He added that mentioning all the big names writing for the *Centralblatt* could have come across as distasteful self-advertisement.[137]

The *Literaturzeitung* broke with this tradition of anonymity and only published reviews that were signed with the reviewer's full name. This was also in line with the latest British developments, where the 'first completely signed periodical of the century […] with the names of the famous writers printed right on the cover' was founded in 1877.[138] Avenarius was shocked to learn that the *Literaturzeitung* was widely praised for this decision.[139] He could have foreseen this reception, however, because some authors who had been reviewed in the *Centralblatt* had already complained about the anonymisation earlier. In 1851, Nöldeke's teacher Ewald had privately criticised Zarncke's policy: 'If only the judging reviewer makes himself known, everyone can know how much credit he wants to give to his judgement'.[140] In the light of the *Literaturzeitung*'s early successes, Zarncke could not ignore such complaints anymore. He agreed with Zarncke's proposal to encourage their reviewers to sign with a cipher that would be easily recognisable for all insiders.[141]

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[134] These numbers are based on the overview made by Wundt's daughter: Wundt, Eleonore, *Wilhelm Wundts Werk: Ein Verzeichnis seiner sämtlichen Schriften*, C.H. Beck, München, 1927. 12–15.
[135] UBL: BPL 2389, Theodor Nöldeke to Michael Jan de Goeje, 30 November 1867.
[136] Secord, James A., *Victorian Sensation: The Extraordinary Publication, Reception, and Secret Authorship of Vestiges of the Natural History of Creation*, University of Chicago Press, Chicago (IL), 2000. 367.
[137] Zarncke, Friedrich, 'An unsere Leser,' *Literarische Centralblatt für Deutschland* (hereafter *LC*), 1874, 52, 1721–1726. 1722–1723.
[138] Hiller, Mary Ruth, 'The Identification of Authors: The Great Victorian Enigma,' in: Vann, J. Don and Rosemary T. van Arsdel (eds.), *Victorian Periodicals: A Guide to Research*, The Modern Language Association of America, New York (NY), 1978, 123–148. 126.
[139] UBLE: NL 249/1/A/556, Eduard Avenarius to Friedrich Zarncke, 24 September 1874.
[140] UBLE: NL 249/1/E/1051, Heinrich Ewald to Friedrich Zarncke, 3 September 1851. It should also be mentioned that Ewald himself was not at all opposed to personal attacks. He did not hide in anonymity when he attacked his peers in public or private. See: Engberts, Christiaan, 'Gossiping about the Buddha of Göttingen,' 374.
[141] UBLE: NL 249/A/555, Eduard Avenarius to Friedrich Zarncke, 1 October 1874.

During the following years, Nöldeke would sign almost all his reviews with 'Th.N.' and Wundt would temporarily sign his previously anonymous contributions with an easily recognisable 'W.W.'.

In 1874, the *Centralblatt* celebrated its twenty-five-year existence. Zarncke opened the last issue of that year with an essay looking back at these years. Because this essay was shaped by the perceived necessity to relate to the competition in Jena, it was informed by forward looking considerations at least as much as by past experiences. Commenting on Zarncke's first draft Avenarius warned him that the proposed retrospective offered their rival a justification for 'open or covert attacks'.[142] He therefore sent him a long list with suggestions to make it more suitable for publication. Zarncke had, for example, included harsh words in his closing remarks that could easily be interpreted as jabs at the *Literaturzeitung*. These remarks, which included the words 'detrimental ambition' were deleted in the final version.[143] Avenarius was also able to convince Zarncke to be somewhat less combative. His reference to 'hack writers of the lowest rank' was replaced by the slightly less provocative 'hack writers'.[144] His premise that authors who had not produced anything 'virtuous' did not deserve the right to reply to criticism in his journal was replaced with a somewhat less offensive remark about works that were not 'solid' enough to merit a defence.[145] After Zarncke's draft had thus been sanitised, Avenarius printed extra copies of the issue in which it was published. These review copies were sent to 'the most important newspapers' in Germany in the hope that this well-mannered anniversary edition of the *Centralblatt* could generate some free publicity.[146]

In the end the *Centralblatt* won the competition with the *Literaturzeitung*. It is, however, not self-evident that this was achieved by the initiatives of Avenarius and Zarncke. The unfortunate career of Anton Klette may have been the main reason for the *Literaturzeitung*'s eventual demise. By the end of the 1870s Klette had severely neglected his responsibilities as a librarian in Jena. The senate of the university forced him to resign in June 1878.[147] He then moved to Magdeburg, from where he continued to work on the *Literaturzeitung* for one more year. In 1879, however, his publishers printed a notice in what would be the last issue of the *Literaturzeitung*, in which they informed their readers that the journal could not be continued because its editor had suddenly disappeared.[148] Later recaps of Klette's career hardly mention anything about his life after the *Literaturzeitung*. He

142 UBLE: NL 249/A/553, Eduard Avenarius to Friedrich Zarncke, 10 December 1874.
143 UBLE: NL 249/A/557, Eduard Avenarius to Friedrich Zarncke, not dated, probably 10 December 1874; Zarncke, Friedrich, 'An unsere Leser,' 1726.
144 The words used by Avenarius and Zarncke were 'Lohnschreiber untersten Ranges' and 'Lohnschreiber'.
145 The words used by Avenarius and Zarncke were 'Rechtschaffenes' and 'Gediegenes'.
146 UBLE: NL 249/A/545, Eduard Avenarius to Friedrich Zarncke, 18 December 1874.
147 Stössel, Waldemar, 'Anton Klette: 1870–1878,' 514.
148 Veit & Comp., 'Zur Nachricht,' *Jenaer Literaturzeitung*, 1879, 39, 536–537.

is reported to have disappeared without a trace in the United States after 1879 or 1896.[149] Regardless of the extent to which their own initiatives were responsible for the continued success of the *Centralblatt*, Zarncke and Avenarius confidently faced the founding of a new review journal in 1880, the *Deutsche Literaturzeitung,* stating: 'we hold on to the belief that we are completely at ease with this new competitor'.[150]

Zarncke's continuous balancing act

His publisher was not the only stakeholder whose interest in the *Centralblatt* shaped Zarncke's editorial decisions. The publishers of the reviewed books also had a well-defined interest in favourable reviews as advertisements for their wares. In the 1860s, Avenarius already warned Zarncke that some publishers might be less likely to send review copies to the *Centralblatt* if they expected to receive only negative reviews.[151] Zarncke's correspondence with F.A. Brockhaus, shows that it was not uncommon for the publisher to contact him when his books were criticised in the *Centralblatt*. Sometimes Zarncke would give in to such pressure, for example when he allowed Camillo Kellner a reply to a critical treatment of his *Kurze Elementargrammatik der Sanskrit-Sprache*.[152] At other times, however, Zarncke stood his ground and refused a reply or retraction. When Brockhaus, for instance, stood up for the work of the recently deceased C.E. Hergt, Zarncke contacted its highly critical reviewer. The reviewer sent Zarncke an elaboration on his unfavourable opinion, which Zarncke then forwarded to Brockhaus. In the light of this explanation Brockhaus admitted that they had to 'acquiesce to what had been said about his publication in the *Centralblatt*'.[153]

The balance of power between the *Centralblatt* and book publishers was delicate. On the one hand, the business model of the *Centralblatt* assumed the cooperation of the publishers. If they would not

[149] The year 1879 is mentioned in: Mehl, Ernst, 'Deutsche Bibliotheksgeschichte,' in: Stammler, Wolfgang (ed.), *Deutsche Philologie im Aufriss*, Band I, Erich Schmitt, Berlin/Bielefeld, 1952, 315–378. 358. The year 1896 is mentioned in: Storost, Jürgen, 'Zu einigen germanistischen Zeitschriften in der zweiten Hälfte des 19. Jahrhunderts,' *Zeitschrift für Germanistik*, Neue Folge II, 1992, 341–354. 351.
[150] UBLE: NL 249/A/543, Eduard Avenarius to Friedrich Zarncke, 24 September 1880.
[151] UBLE: NL 249/A/571, Eduard Avenarius to Friedrich Zarncke, 10 March 1862.
[152] UBLE: NL 249/1/B/2070, F.A. Brockhaus to Friedrich Zarncke, 22 August 1868. (The references to 'F.A. Brockhaus' in this chapter are to the firm F.A. Brockhaus, not to the publisher Friedrich Arnold Brockhaus himself, since he passed away in 1823. However, representatives of the firm that carried his name continued to sign their correspondence with 'F.A. Brockhaus'.) The critical review was: D.lbr.ck. [Berthold Delbrück], Kellner, Dr. Camillo, 'kurze Elementargrammatik der Sanskrit-Sprache,' *LC*, 1868, 30, 814–815. The reply was: Kellner, Camillo, 'Entgegnung,' *LC*, 1868, 42, 1147–1148.
[153] UBLE: NL 249/1/B/2077, F.A. Brockhaus to Friedrich Zarncke, 6 November 1871; UBLE: NL 249/1/B/2078, F.A. Brockhaus to Friedrich Zarncke, 9 December 1871. The critical review was: F., 'Hergt, Dr. C.E., Geographie des Gelobten Landes,', *LC*, 1871, 36, 907–908.

send their books for review, the costs of having to purchase more than a thousand new books every year would have threatened the financial viability of the journal. Zarncke, therefore, had good reason for not alienating the publishers by publishing too many unfavourable reviews. At the same time, he was not reduced to a powerless pawn of the publishers either. One reason for this is the fact that, although publishers may have feared unfavourable criticism, they also hoped to benefit from complimentary reviews. A second reason was that he was not fully dependent on their cooperation. Frequent contributors to the *Centralblatt* often offered Zarncke reviews of books that they had acquired without his mediation.[154] Spontaneous offers to review books were often followed by a favourable review because these books were usually either a gift from a friend or bought for good reason. However, such offers still limited the extent to which Zarncke depended on the cooperation of publishing houses.

Finally, as the editor of a general review journal Zarncke knew specialists in almost all disciplines. Publishers, such as Brockhaus, recognised the usefulness of good relationships with such well-connected people. In 1869, Brockhaus asked Zarncke if he could solicit an expert opinion on a manuscript they had recently received from August Knötel about the 'Lycian Trojans'.[155] Two weeks later, Brockhaus wrote with some relief that they were happy that Zarncke had confirmed their doubts.[156] The book was never published. In the following years Brockhaus would also ask for Zarncke's mediation to decide on the continuation of the publication of the second edition of Georg August Pritzel's *Thesaurus literaturae botanicae* as well as for his advice on a possible contributor to the publisher's famous *Konversationslexicon*.[157] Zarncke's relationship with publishers was therefore complicated; they encouraged him to mainly publish favourable reviews, but because he could also be very helpful to them, in some ways, he retained the freedom to sometimes publish critical assessments, as well.

The authors of the books reviewed in the *Centralblatt* did, of course, have an interest in a favourable treatment of their work, too. Unlike their publishers, they did not have the financial clout to pressure Zarncke into refraining from harsh criticism. After all, the journal's business model did not depend on the cooperation of reviewed authors. From the early 1850s onwards, however, Zarncke saw it as 'a commandment of duty and honour' to allow authors who considered

[154] See, for example, the correspondence between Zarncke and the Leipzig theologian Franz Delitzsch: UBLE: NL 249/1/D/279, Franz Delitzsch to Friedrich Zarncke, 1 February 1872. This letter accompanied the unsolicited review for: F[ranz] D[elitzsch], 'Ebers, Dr. Georg, Prof., Durch Gosen zum Sinai,' *LC*, 1873, 9, 257–260.
[155] UBLE: NL 249/1/B/2072, F.A. Brockhaus to Friedrich Zarncke, 2 March 1869.
[156] UBLE: NL 249/1/B/2075, F.A. Brockhaus to Friedrich Zarncke, 17 March 1869.
[157] UBLE: NL 249/1/B/2081, F.A. Brockhaus to Friedrich Zarncke, 30 November 1872; UBLE: NL 249/1/B/2082, F.A. Brockhaus to Friedrich Zarncke, 8 April 1880.

themselves to have been treated unfairly the right to reply.[158] At the same time, he was worried that the inclusion of such reactions could inspire a reaction from the reviewer. He feared that an extended discussion would be 'of no use for scholarship' and 'could only be unpleasant for the reader'. He therefore announced an editorial policy that aimed to limit debate. This policy proved to be hard to enforce. Between 1850 and 1891 — the year of Zarncke's death — 179 replies to reviews in the *Centralblatt* were published and 151 of these were published together with a reaction of the reviewer.[159] Although Zarncke could not prevent reviewers from responding to replies, he was able to limit the ensuing debate. The reviewers' responses would always close the argument. The fact that the reviewers got the last word in discussions in the *Centralblatt* suggests that they did have more clout than the authors.

The reason for this clout is quite obvious. Zarncke needed the cooperation of many scholars to fill fifty-two issues of the *Centralblatt* every year. He could not allow himself to affront frequent contributors like Nöldeke and Wundt. Nöldeke, for example, contributed 96 reviews between 1871 and 1880.[160] During the same period Wundt published 123 reviews in the *Centralblatt*.[161] Their high productivity earned them certain liberties. In his relationship with Nöldeke, he decided to ignore his personal antipathy. After a fall-out with Zarncke in 1865, Nöldeke told De Goeje that he would probably stop contributing to the *Centralblatt*.[162] Half a year later, a letter from Nöldeke's colleague, frequent *Centralblatt* contributor Alfred Gutschmid, confirmed Zarncke's dislike of him: 'Once you get to know him personally, you will notice that he can be rude and you will find him informal'. However, in the same letter, Gutschmid also praised Nöldeke's lack of 'scholarly obscurity' (*Gelehrtendunkel*) and expressed his relief about the fact that Zarncke had just made peace with Nöldeke.[163] The need for hardworking contributors to the *Centralblatt* outweighed personal dislikes.

The leeway Zarncke gave his contributors was not limited to who could publish in the *Centralblatt*; it also extended to what could be published. Regular contributors could sometimes convince him to publish anonymous reviews of their own work. Franz Delitzsch, for example, sent Zarncke a review of a text edition he had just published with his colleague Seligman Baer.[164] Zarncke not only accepted the review, he even invited Delitzsch to contribute another anonymous review of his own work two years later.[165] At other times Delitzsch did not write the review himself, but suggested an

[158] Die Redaction des Literarischen Centralblattes [Friedrich Zarncke], 'Literarische Anzeigen,' *LC*, 1853, 5, 91–92.
[159] Lick, Thomas, *Friedrich Zarncke*, 313.
[160] Based on the listing in: Maier, Bernhard, *Gründerzeit der Orientalistik*, 430–438.
[161] Based on the listing in: Wundt, Eleonore, *Wilhelm Wundts Werk*, 7–16.
[162] UBL: BPL 2389, Theodor Nöldeke to Michael Jan de Goeje, 29 October 1865.
[163] UBLE: NL 249/1/G/1379, Alfred Gutschmid to Friedrich Zarncke, 4 April 1866.
[164] UBLE: NL 249/1/D/280, Franz Delitzsch to Friedrich Zarncke, 16 June 1872.
[165] UBLE: NL 249/1/D/282, Franz Delitzsch to Friedrich Zarncke, 16 February 1874.

appropriate reviewer instead. When he published another text edition with Baer in 1882, he contacted Nöldeke to discuss the most suitable reviewer. They seriously considered Nöldeke's Strasbourg colleague Samuel Landauer, but in the end the review was written by Nöldeke himself.[166] If we are to believe Wundt, this practice was very common. In reaction to a favourable review in the *Centralblatt* he noted that this was clearly a 'literary token of friendship, as they occur so frequently in the field of criticism'.[167] However, even if such friendly favours were common, not all favourable reviews by reviewers who took the initiative to write the review themselves should be judged as tokens of friendship. Some reviewers used this as a means to obtain books of interest for free. Richard Avenarius, who was a regular contributor to the *Centralblatt* before founding the *Vierteljahrsschrift*, submitted a long list of books he hoped to receive for review.[168] The notes above this list show that Zarncke willingly agreed with his requests. All in all, regular contributors had a strong influence on who would review which books, both in regard to their own and other authors' works.

Authors also had a strong influence on the shape of their contributions. Zarncke preferred to publish short reviews. Most were not much longer than one page and many were even shorter than that. Eduard Avenarius pointed out that this allowed them to review a larger number of books.[169] He assumed that the shorter reviews of the *Centralblatt* would be appreciated as much as the longer ones in the *Jenaer Literaturzeitung*.[170] It was not always easy, however, to convince all contributors to submit such short contributions. When a frequent contributor submitted a long review, Zarncke often set aside his preferred policy and published the lengthy essay anyway. Richard Avenarius' four-page essay about Steinthal's *Einleitung in die Psychologie der Sprachwissenschaft* was almost immediately printed.[171] Nöldeke's former student Georg Hoffmann also saw his four-page review of the new Syriac grammar of his one-time teacher published within a month.[172] All in all, it was not uncommon for reviews to end up being much longer than Zarncke preferred.

The correspondence between Zarncke and another close acquaintance of Nöldeke, the aforementioned Gutschmid, shows that Zarncke not only allowed his contributors liberties in the length of their review but also in its contents. In 1873, Zarncke sent him Hermann Vámbéry's

166 UBLE: NL 249/1/D/297, Franz Delitzsch to Friedrich Zarncke, 21 June 1882; Th. N. [Theodor Nöldeke], 'Libri Danielis Ezrae et Nehemine,' *LC*, 1882, 34, 1137–1140.
167 UAL: NA Wundt/III/956, Wilhelm Wundt to Friedrich Zarncke, 26 June 1872.
168 UBLE: NL 249/1/A/602, Richard Avenarius to Friedrich Zarncke, 26 May 1871.
169 UBLE: NL 249/1/A/559, Eduard Avenarius to Friedrich Zarncke, 22 March 1874.
170 UBLE: NL 249/1/A/537, Eduard Avenarius to Friedrich Zarncke, 11 February 1876.
171 UBLE: NL 249/1/A/606, Richard Avenarius to Friedrich Zarncke, 23 May 1872; [Richard Avenarius], 'Steinthal, Dr. H., Prof., Einleitung in die Psychologie der Sprachwissenschaft,' *LC*, 1872, 23, 599–603.
172 ULBE: NL 249/1/H/2161, Georg Hoffmann to Friedrich Zarncke, 15 February 1882; G.H. [Georg Hoffmann], 'Nöldeke, Theod., kurzgefasste syrische Grammatik,' *LC*, 1882, 10, 318–322.

Geschichte Bochara's oder Transoxanien for review. Vámbéry taught at the University of Budapest and his work was not taken very seriously by his German colleagues.[173] His most successful student, Goldziher, remembered his teaching as full of 'self-praise, bragging, presumptuous appraisal of his own achievements' and 'harmful dilettantism'.[174] Because his new book had received favourable reviews in what he called 'half-scholarly journals', Gutschmid argued that it was important that Vámbéry would be put into his place.[175] Gutschmid's letter to Zarncke not only criticised Vámbéry's scholarship, it was also filled with anti-Semitic insinuations. He referred to him as 'Hirsch Bamberger' — an easily recognisable Jewish name. He added some very explicit slurs as well: 'From all sides, it is perceived as an urgent need to shut the Jew boy's mouth'. 'At 24 broadsides is outlined,' Gutschmid continued, 'what could have been summarised with the words 'puffery of an impertinent Jew boy". Although he realised that the *Zeitschrift* of the *DMG* would be the most appropriate place for such a long review, Gutschmid argued — probably correctly — that this was not an option. He referred to the 'idyllic still life that Fleischer has established among German orientalists'. The polite manners that Fleischer had imposed on the *Zeitschrift* made it impossible for Gutschmid to publish his harsh review of Vámbéry's book there.

Two weeks after Zarncke had received Gutschmid's letter and review, the whole essay, which took up thirteen full pages in the *Centralblatt*, was published.[176] It was a diatribe full of harsh reproaches. Gutschmid scolded Vámbéry's 'total lack of knowledge', argued that he wrote 'like a blind man about colour', criticised his 'circular reasoning and other offences against logic', scolded his work as 'abysmal fibbing' and wondered 'what forced the author to undertake something for which he lacked no less than both the intellectual and external conditions'. He also criticised his writing style, which he characterised as bombastic and 'making the fateful step from the lofty to the ridiculous'. The last half page hinted, if not at his Jewishness, at least at the fact that Vámbéry was not German by criticising the crudeness of his language and pointing out incorrect grammatical constructions.

Of course Vámbéry sent Zarncke a reply, in which he vehemently protested Gutschmid's reproaches, though he admitted that as a Hungarian speaking a large number of western and eastern languages it was hard to avoid making some mistakes in German.[177] In his answer to Vámbéry Gutschmid used this discussion about language proficiency to subtly mention his Jewish name: 'the only thing I knew thus far about the author of *Bochara*, is that he is actually called Bamberger […]

[173] Marchand, Suzanne L., *German Orientalism*, 148.
[174] Scheiber, Alexander (ed.), *Ignaz Goldziher: Tagebuch*, Leiden, Brill, 1978. 26.
[175] UBLE: NL 249/1/G/1413, Alfred von Gutschmid to Friedrich Zarncke, 23 April 1873.
[176] A. v. G. [Alfred von Gutschmid], 'Vámbéry, Herm., Geschichte Bochara's oder Transoxanien,' *LC*, 1873, 19, 577–590.
[177] Vámbéry, Hemann and Alfred von Gutschmid, 'Erwiderungen,' *LC*, 1873, 24, 763–768.

no wonder that I could hereafter not believe anything else, but that German, or at least not Hungarian, was his mother tongue'. However, even if Zarncke's publication of both Gutschmid's overlong offensive review and his dog-whistle reply illustrate how far he went to please his regular contributors, the incident also shows that he did not give in to all their pressure. Gutschmid had asked him not to correct some misspellings in the manuscript of Vámbéry's reply, so that his lack of German roots would even be more clearly exposed.[178] However, Zarncke made sure that misspelled words like '*Erwiederung*', '*pracktischen*' and '*Kritick*' were correctly printed as '*Erwiderung*', '*praktischen*' and '*Kritik*'.

Editors, publishers, authors and audiences

This chapter looks at various considerations that shaped the editorial decisions made at scholarly journals. Even if the common modern-day characterisation of editors as guardians of good scholarship is not completely inappropriate for late 19th-century German editors, their private correspondence mostly underlines other concerns. Very different scholarly journals were decisively shaped by how editors related to three groups of stakeholders: publishers, audiences and contributors. From the point of view of the journal editor, the continuous effort to balance loyal collaboration and independent criticism was not entirely — or even primarily — an attempt to relate to the findings that his peers asked him to publish. Instead, it was an ongoing struggle to balance the expectations of all these stakeholders. The different strategies that editors developed to maintain this precious balance explain the differences between journals more convincingly than references to the very different disciplines that they covered.

Publishers had good reason to be involved in the shaping of journals because they carried the financial risk of the endeavour. For some publishers this was an incentive to get involved in discussions about the content of their journals, while others left this to the editors. Eduard Avenarius continuously pitched ideas to Zarncke with varying degrees of success. Engelmann tried to influence the character of Wundt's *Studien* with very little success. F.A. Brockhaus, finally, did not try to shape the *Zeitschrift* of the *DMG*. These differences in pressure from publishers can be related to the extent to which they had an economic incentive to push the sales of their titles. Since the *Zeitschrift* was the house organ of Germany's largest professional organisation for orientalists, almost every potential reader was a subscriber already. In combination with the personal relationship between F.A. Brockhaus and the *DMG*, there was no strong inducement for the

[178] ULBE: NL 249/1/G/1414, Alfred von Gutschmid to Friedrich Zarncke, 2 June 1873.

publisher to aim for even more subscribers. For Engelmann, it was also easy to accept Wundt's editorial independence in an early stadium. The publisher's revenue model was not to sell a large number of copies of the *Studien*, but to accommodate one of his best-selling authors.

Avenarius had more reason to be constantly worried about the profitability of his journal. The *Centralblatt* did not have a readership as reliable as the membership of the *DMG* and Avenarius did not have any special reason to accommodate Zarncke. When the *Jenaer Literaturzeitung* started to compete for the same readership Avenarius felt justified to push for changes, such as the introduction of the *Personalnachrichten* and the restriction of anonymous reviewing. The profitability of the *Centralblatt* also depended on the willingness of book publishers to provide free review copies. Though Zarncke could allow himself some liberties towards them, both Avenarius and F.A. Brockhaus remembered him from time to time of the importance of the maintenance of a friendly relationship.

Different audiences also presented different challenges. Wundt never seems to have bothered too much about the readership of the *Studien*. His journal's main *raison d'être* was to provide a publication platform for his friends, former students and co-workers. The editors of the *Zeitschrift*, on the other hand, had to take their audience into account at all times. Representing a majority of the German orientalists, its editors had to walk a fine line between doing justice to different points of view and preventing potentially offensive prolonged debate. Authors and readers allowed a widely respected senior editor like Fleischer to take quite some liberties. When Fischer, a younger and less widely admired scholar, took his place, however, disagreements about editorial choices soon reached the board of the *DMG*. When the board could not appease all complainants, the *Zeitschrift* could face the very undesirable withdrawal of some of its most valued contributors. The discussions about Fischer's functioning soon led to the discontinuation of his editorial glosses and ultimately contributed to his resignation. For Avenarius and Zarncke, finally, the perceived needs of their readership were guiding as well. The aforementioned *Personalnachrichten* and the limits to anonymous reviewing were inspired by the perceived preferences of their audience. Avenarius also successfully militated against the inclusion of overviews of technical and agricultural periodicals; because he couldn't imagine people to read such periodicals as well as the *Centralblatt*.[179]

Finally, the relationship between the editor and the journal's contributors also decisively shaped these journals. The possibility to easily publish dissertations and other products from his Leipzig laboratory was Wundt's main reason for founding the *Studien*. This practice was quite common; in

[179] UBLE: NL 249/1/A/571, Eduard Avenarius to Friedrich Zarncke, 10 March 1862.

the late 19th century, the chemists August Wilhelm von Hofmann and Wilhelm Ostwald also used their editorial positions to ensure the publication of the work of their doctoral students.[180] This objective also explains two of the *Studien*'s most salient features. In the first place, it had a very narrow focus on one type of study, namely on the perception and reaction-time measurement, as pioneered in Wundt's laboratory. Secondly, the list of authors publishing in the *Studien* only provided a very limited overview of who's who in late 19th-century experimental psychology. Rather than publishing papers from influential scholars working on different yet related issues, Wundt published the work of often peripheral scholars who were personally and intellectually close to him. Sometimes, as the cases of Kiesow and Lipps suggest, this could contribute to their eventually successful academic careers. At other times, as the examples of Lange and Merkel show, the exposure in the *Studien* did very little for their academic advancement.

The relationship between the editor and the authors of the *Zeitschrift* of the *DMG* was different. It was characterised by the same considerations that shaped the relationship between the *Zeitschrift* and its readership. After all, its authors and readership were largely the same. The editor had to continuously guard the right of his authors to voice their disagreement with others while ensuring that this would not devolve into prolonged debate. Zarncke, however, was the editor who was most dependent on his authors, because he had to deliver a new issue of the *Centralblatt* every week. To ensure their cooperation, he had to allow his frequent contributors a large degree of liberty, which included the right to anonymously publish reviews of their own work, the opportunity to handpick their own reviewers, the possibility to write overlong reviews, and even the chance to publish the sort of venomous polemics from which he and Avenarius usually tried to refrain.

Notwithstanding the differences in their relationships with publishers, audiences and authors, the *Studien*, *Zeitschrift* and *Centralblatt* share one distinctive feature. For various reasons, they all actively limited the opportunities for debate. The uniformity of the *Studien* all but precluded the sort of disagreement that could be the starting point for a prolonged discussion. Each paper published in the journal was rooted in Wundt's ideas about experimental psychology and he could easily ensure that the contributions did not contain fundamental disagreements. Though the *Zeitschrift* could not exclude all disagreement from its pages, the editors and the executive board of the *DMG* realised that they had to limit prolonged debate in order to satisfy all their members. The fact that the Moabitica were largely discussed in a myriad of other periodicals and monographs as well as the fate of Fischer as an editor illustrate the success of this policy. Finally, Zarncke's policy of allowing

[180] Johnson, Jeffrey A., 'Hierarchy and Creativity in Chemistry, 1871–1914,' in: Olesko, Kathryn M., *Science in Germany, The Intersection of Institutional and Intellectual Issues*, Osiris, 2nd Series, 5, 1989. 214–240. 225–27.

only one reply by disgruntled reviewed authors and a final answer by the reviewer proved to be successful. Even if frequent contributors sometimes took the liberty to write overly harsh reviews, this never turned into a drawn-out debate on the pages of the *Centralblatt*.

The observation that there was a widely shared aversion of prolonged debate among the editors of scholarly periodicals in 19th-century Germany is in line with the findings of modern-day authors who analysed individual journals. One scholar has noticed that 'address and reply' were rare in the *Historische Zeitschrift*'.[181] Another likewise noticed that Carl Theodor von Siebold, the editor of *Zeitschrift für wissenschaftliche Zoologie*, would not tolerate polemic, 'which was to be shunned as an effort that did not contribute anything of substance to science and as conduct unbecoming a gentleman and a scientist'.[182]

This widely shared similarity suggests that, in spite of their many differences, most German scholarly journals in the late 19th century saw themselves as platforms for the announcement of developments in scholarship, rather than as a gathering place for critical discussion. The *Studien* announced the outcomes of the latest experiments of Wundt and his co-workers; the *Zeitschrift* announced the advancement of Oriental studies in Germany, and the *Centralblatt* announced new publications in law, medicine, the natural sciences and the humanities. This attitude is perhaps best captured by Nyhart's summary of Siebold's conception of his *Zeitschrift;* he considered it to be 'a public repository of scientific research — fact and theories, but especially facts'.[183] These conclusions also provide a better understanding of the role of journal editors. Their primary task was not the to secure the correctness and excellence of each and every published paper. The editor's main responsibility was rather to balance the needs and demands of contributors, publishers and audiences. A self-portret of an institution primarily interested in being a trustworthy repository of research was less likely to upset any of these stakeholders than presenting their journal as a platform for critical debate and potentially fruitful disagreement.

[181] Fahrmeir, Andreas, 'Ort des Konsenses oder Historische Streitschrift? Zur Geschichte des 19. und 20. Jahrhunderts in der Historischen Zeitschrift,' *Historische Zeitschrift*, 289, 2009, 199–222. 201.
[182] Nyhart, Lynn K., 'Writing Zoologically,' 54.
[183] Ibid., 45.

3. A Review of Book Reviews

Criticism and Community Formation in Book Reviews

The genre of the book review

The genre of the book review developed hand in hand with the scholarly journal. Early scholarly periodicals already printed overviews of newly published literature. From the second half of the 18th century onwards the review journal gained increasing prominence.[1] Its growing popularity was related to broader developments in scholarly communication. It had become more difficult for scholars to earn a good reputation by erudition alone. Innovative research became an increasingly indispensable requirement. The only way to ensure that your peers would be aware of the originality of your work was to publish it. This caused a strong growth of book publications.[2] This process was further accelerated by changes in the book printing industry. Between 1700 and 1770 European book production tripled in size.[3] During these years of growth, book reviews, bibliographies and book fair catalogues provided scholars an overview of the enormous amount of newly published literature.

The reviews in these early journals are somewhat different from modern-day ones. Their function of providing an overview of the most important new publications shaped them decisively. They usually summarised the contents of the work under review without judging its merits.[4] Soon, however, new reviewing styles became more common. The 18th-century theologian Johann Christoph Greiling recognised three ways of reviewing, stating 'Reviewing can be seen in a historical and a philosophical meaning. In the first one it would mean: to state the contents of a book: reporting. In the philosophical meaning, however, reviewing must mean: to examine the spirit (*Geist*) of a book on the basis of the principles of the discipline (*Wissenschaft*) to which it […] belongs. The first type of review is called announcements, the second reviews in the narrow sense. Reviewing in its wider meaning brings together both types'.[5] Greiling first published his essay in

[1] Schneider, Ute, 'Die Funktion wissenschaftlicher Rezensionszeitschriften im Kommunikationsprozess der Gelehrten,' in: Schneider, Ulrich Johannes Schneider (ed.), *Kultur der Kommunikation: Die europäische Gelehrtenrepublik im Zeitalter von Leibniz und Lessing*, Harrassowitz, Wiesbaden, 2005, 279–291. 283.
[2] Ibid. 281–282.
[3] Nicoli, Miriam, 'Faced with the flood: scholarly working practices and editorial transformations at the highpoint of scientific publication,' in: Holenstein, Andre, Huberts Steinke, Martin Stuber and Philippe Rogger (eds.) *Scholars in action: the practice of knowledge and the figure of the savant in the 18th century*, Brill, Leiden, 2013, 609–629. 610.
[4] Habel, Thomas, *Gelehrte Journale und Zeitungen der Aufklärung: Zur Entstehung, Entwicklung und Erschließung deutschsprachiger Rezensionszeitschriften des 18. Jahrhunderts*, Edition Lumiere, Bremen, 2007. 222.
[5] Greiling, Johann Christoph, 'Einige allgemeine Grundsätze zu einer Theorie der Recensionen,' *Archiv für die Physiologie*, Dritter Band, 1799, 349–385. 353.

1799 and his emphasis on the more evaluative character of the 'philosophical review' and the 'review in its wider meaning' illustrates a change in reviewing practices. Even if discussion about the preferred character of reviews — should they be informative reports or critical evaluations — continued throughout the 18th century, the latter view had already become widely accepted by the time Greiling published his analysis.[6]

By the 19th century, a third type of review had also become increasingly common. More and more reviewers refused to limit themselves to simply discussing a book and used their reviews to present their own thoughts and findings instead. The French sociologist Émile Durkheim, for example, 'often used reviews as a platform for the elucidation of his own theories and for rebuttal of the attacks of his critics'.[7] Late 19th-century German historians, likewise, used their book reviews to present their 'own points of view, concepts and current research'.[8] By the end of the 19th century, the book review had developed into a highly diversified genre that could contain elements of summarising, evaluation and the presentation of one's own findings and convictions.

In the aftermath of the political turmoil of 1848–1849 Germany's leading review journals had closed down.[9] Zarncke and his collaborators jumped at this opportunity and published the first issue of the *Literarische Centralblatt* in 1850. The opening words of this issue reflected the early ideal of the review as a summary: 'The journal […] has given itself the task to provide a complete […] overview of the full literary activity in Germany. To this effect, it will announce every book published in Germany […] and it will provide explanatory notes and short reports of all important books […]'.[10] Twenty-five years later, however, Zarncke looked back at his journal as a platform for evaluation as well. He argued that the summaries had been aimed at achieving 'a wider and higher purpose; to carry the sense for correct and exact methods of research into the widest circles and let them be established as commonly as possible'.[11] A quick glance at the pages of the *Centralblatt* shows that it indeed presented a mix of what Greiling would have called announcements, and reviews in both a narrow and wider sense.

One modern-day commentator has argued that '[...] the acknowledgement or non-acknowledgement of the scholarly accomplishment in the journal decides to a large extent about the reputation of the individual scholar and at the same time defines the scholars as a group, whose

[6] Habel, *Gelehrte Journale und Zeitungen der Aufklärung*, 224.
[7] Giddens, Anthony, 'Durkheim as a Review Critic,' *The Sociological Review*, New Series, 18(2), 1970, 171–196. 171.
[8] Müller, 'Geschichte machen,' 430.
[9] Lick, *Friedrich Zarncke*, 11–12.
[10] [Zarncke, Friedrich], untitled editorial introduction, *LC*, 1 October 1850. 1.
[11] Zarncke, Friedrich, 'An unsere Leser,' *LC*, 26 December 1874. 1–2.

norms are to be observed'.[12] In this chapter I investigate how scholars assessed their peers in their capacity of reviewer. First, I will focus on the content of book reviews. What were the most common reasons to criticise a book? What were the most frequent reasons to judge an author? What qualities were reason for praise? This analysis will be based on the way in which Nöldeke and Wundt discussed the works of authors who can be categorised on the basis of very different criteria. The first section looks at authors from different disciplines. Next, the chapter pays attention to non-protestant and non-German scholars. The subsequent section takes a closer look at differences in the assessment of authors with and without academic affiliations. Initially, I also planned to consider female authors. This, however, turned out to be impracticable, because, among Nöldeke's and Wundt's more than 200 reviews, only one deals with a book written by a woman.[13] Following these analyses, the final section deals with the way in which the language of book reviews has contributed to processes of group formation.

In the first half of this chapter the evaluation of the reviewers' attitudes towards different groups will largely be based on their most critical reviews. This analysis has both quantitative and qualitative elements. The quantitative side is based on a distinction between positive and negative reviews. After reading all of Nöldeke's 96 reviews I have concluded that 12 of them were unambiguously negative, while 19 of Wundt's 123 reviews fit into this category.[14] After counting the number of reviews of, for example, Jewish authors or authors without university affiliation I can then determine if they are more or less likely than others to be reviewed favourably. The main focus of my analysis, however, will be qualitative. I will collect the many criticisms of a variety of works of different types of authors. The resulting wide range of comments will provide an outline of the qualities Nöldeke and Wundt seized on to criticise scholarly works as well as their authors.

I have not limited myself, however, to an analysis of negative reviews. This chapter also provides an overview of the most common reasons for praise. It pays attention both to reasons to applaud a book and to the personal qualities of the authors that often merited praise. The combination of Nöldeke's and Wundt's criticisms of various groups of authors and the overview of reasons for

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[12] Schneider, 'Die Funktion wissenschaftlicher Rezensionszeitschriften,' 290.
[13] Wundt, Wilhelm, 'Rubinstein, Dr. Sus., die sensoriellen und sensitiven Sinne,' *LC*, 1875, 22.
[14] In making such distinctions I follow the example of Claudia Profos Fick in her analysis of Albrecht von Haller's reviews for the *Göttingische Gelehrte Anzeigen*. She distinguishes between very negative, negative, undecided, positive, and very positive reviews. I have chosen not to make a judgement on the sometime subtle difference between very negative and negative reviews: both are categorised as negative. I have categorised all others as positive. See: Profos Frick, Claudia, *Gelehrte Kritik: Albrecht von Hallers literarisch-wissenschaftliche Rezensionen in den Göttingischen Gelehrten Anzeigen*, Schwabe, Basel, 2009. 286–287, 299. A similar subdivision in critical and uncritical reviews can be found in: Salager-Meyer, Françoise, María Ángeles Alcaraz Ariza and Maryelis Pabón Berbesí, 'Collegiality, critique and the construction of scientific argumentation in medical book reviews: a diachronic approach,' *Journal of Pragmatics*, 39, 2007, 1758–1174, 1752. I have been able to retrieve the sometimes anonymous reviews after consulting Maier, Bernhard, *Gründerzeit der Orientalistik*, 430–438 and Wundt, Eleonore, *Wilhelm Wundts Werk*, 7–16.

praise illustrates the way in which review journals allowed 19th-century German scholars not only to list and evaluate relevant new publications, but also to draw the outlines of a group of scholars with shared norms and practices. The final section of this chapter draws on linguistic analyses of politeness to further illustrate the way in which book reviews contributed to the shaping of a scholarly community.[15] After all, book reviews can be a medium through which processes of both inclusion and exclusion can be facilitated.

Nöldeke on theology and linguistics

The *Centralblatt* presented its reviews in sixteen thematic sections, covering all the major disciplines taught at German universities. There was also a section for reviews of works that did not fit under any of the main headings.[16] The expertise of Nöldeke and Wundt allowed them to write reviews for different sections. Because of the traditionally close relationship between Old Testament studies and Semitic languages, Nöldeke contributed reviews in both fields. Half of his 96 reviews featured under *Theology*, while forty were published in the *Linguistics* section. Most others were published under *History*. After all, his extensive knowledge of old Semitic texts had turned him into an expert of the early history of the Middle East as well. Wundt started writing for the *Centralblatt* before he had turned from a physiologist into a philosopher. Since he continued to review medical books after accepting his Chair of Philosophy in 1874, most of his reviews, 73 in total, were printed in the *Medicine* section. After 1874 he would, however, diversify his output. He contributed 32 reviews to the *Philosophy* and fifteen to the *Natural Sciences* section.

Nöldeke was most critical in his theological reviews: eight of his twelve negative reviews were printed in this section. In addition, one was printed in the *Linguistics* and three in the *History* section. Because he published only seven historical reviews in the 1870s, it is hard to draw any conclusions about his severity in this field. The difference between his theological and linguistic reviews, however, is noteworthy. While 8 of his 48 reviews on theological subjects were negative, this was only the case for 1 out of his 40 linguistic reviews. The one negative review on linguistics discussed a booklet by the Italian attorney Giuseppe Barzilai.[17] Nöldeke admitted that 'each dilettante has the full liberty to play with scholarly issues and to create a building without a steady fundament with

[15] I will mostly draw on: Brown, Penelope and Stephen C. Levinson, *Politeness, Some universals in language usage*, Cambridge University Press, Cambridge, 1987; Hyland, Ken, *Disciplinary Discourses: Social Interactions in Academic Writing*, University of Michigan Press, Ann Arbor (MI), 2004 and Myers, Greg, 'The pragmatics of politeness in scientific articles,' *Applied Linguistics*, 10(1), 1989, 1–35.
[16] Lick, *Friedrich Zarncke*, 33–35.
[17] Nöldeke, Theodor, 'Barzilai, Dr. G., le lettere dell'alfabeto fenicio,' *LC*, 1876, 30.

some effort of phantasy and ingenuity'. Barzilai, however, should not have published his sloppy work — 'the reader will agree with our advice that Mr Barzilai would from now on deploy his 'nourishment — consolation — energy' more purposefully than on […] printing […] such a work'.

Why then, was Nöldeke so much more critical of theological works? One historian's characterisation of him as a positivist might provide a clue.[18] Nöldeke repeatedly argues that theologists improperly neglect the essential distinction between scriptural authority and church dogma on the one hand and independent thinking and the use of modern critical methods of textual analysis on the other. In one review, he complained that '[...] the times when a catholic clergyman could, […] without apostatising his church, examine the Bible with true criticism have long passed. Mr Zschokke invariably chooses the official views held by the church'.[19] In another review he mockingly paraphrases a section from a book about the authenticity of the Pentateuch, in which the author argued that he knew of 'no other authority than that of the church, which leaves every examination aiming for truth the freest manoeuvring room'.[20] Nöldeke sneered that it indeed 'requires much less subjection of reason, to believe in the 'authenticity' of the Pentateuch […] than to believe in the infallibility of the pope'. Another book received similar criticism; 'Indeed, for Mr Böhl, scholarly criticism no longer has any value when it is in contradiction with his religious views'.[21] Nöldeke finally concludes that 'after all what has been said, the final verdict of this book cannot be positive'. Even people whose lack of religious dogmatism he wholeheartedly admitted were not free from Nöldeke's strict surveillance of the thin line between religion and scholarship. In his review of a history of biblical literature, he complains that even though the author 'is free from religious-dogmatic prejudices,' his attempts to paint a vivid picture make him 'clamp down too heavily on the accepted tradition, often even to its smallest features'.[22]

Wundt on philosophy and medicine

A similar distinction in the treatment of works in different fields can be observed in Wundt's case. Of his 73 reviews in the *Medicine* section, only 3 were strongly dismissive. In the *Natural Sciences* section, he also published three negative reviews. Of the 32 reviews he contributed to the *Philosophy* section, however, 13 were highly critical. A closer reading of his negative reviews in the *Natural*

[18] Paret, *Arabistik und Islamkunde*, 14.
[19] Nöldeke, Theodor, 'Zschokke, Dr. Herm., Prof., historia sacra Antiqui Testamenti,' *LC*, 1873, 1.
[20] Nöldeke, Theodor, 'Neteler, B., Studien über die Echtheit des Pentateuchs,' *LC*, 1873, 1.
[21] Nöldeke, Theodor, 'Böhl, Ed., Forschungen nach einer Volksbibel zur Zeit Jesu,' *LC*, 1873, 37.
[22] Nöldeke, Theodor, 'Fürst, Jul., Geschichte der biblischen Literatur und des jüdisch-hellenistischen Schriftthums, *LC*, 1871, 14.

Sciences section shows that they mostly dealt with investigations in one of his own primary fields of interest, the physiology of perception. He dismisses Susanna Rubinstein's dissertation by stating that it would have been better if 'the faculty in question would have added to their gift the advice that this treatise should not be printed'.[23] A work on Weber's law was even more harshly evaluated: 'The reading of this work could be recommended as a good exercise for future natural scientists and especially physiologists. They could learn some very striking examples from this of what they should *not* do, when they aim to engage in research'.[24]

The three dismissive reviews published in the *Medicine* section are all about works that would have fit in the philosophical section as well. Though Wundt makes some comments about the lack of originality in Ludwig Büchner's *Physiologische Bilder*, he is mostly bothered with the fact that most of the book deals with philosophical questions rather than physiological issues.[25] The book contained theories about the nature of the soul, consciousness and the character of thoughts. Wundt disapprovingly paraphrased Büchner's analysis of consciousness, as follows: 'The author makes it easy for himself with the problem of consciousness. Consciousness has to lie dormant in matter, we don't have the right to ask how and why'. Another medical work is criticised for its acceptance of vitalistic theories about 'life energy'.[26] The final book negatively reviewed in the *Medicine* section is even more harshly criticised.[27] The author tries to show that 'the biblical story about the descent of all people from one couple, should not just be discarded to the realm of fairy tales'. Wundt concludes that the author should not have dealt with this question because he lacked the necessary knowledge of Darwin's theory of evolution.

Most of the books critically dismissed by Wundt, however, share one characteristic: they try to understand the world through philosophies that he considered to be obsolete and speculative. Authors influenced by the tradition of *Naturphilosophie* are among his favourite foes. Johannes Volkelt's conception of dreams as 'the miraculous and the mystical, the opposite of the laws of the awake consciousness' sadly reminds him of the 'idolisation of dreams practiced in the earlier *naturphilosophische* mysticism'.[28] Other books are likewise dismissed for their reliance on *Naturphilosophie*.[29] This tradition is not the only one dismissed as old-fashioned and obsolete. One book is criticised for the way in which it compares the assumption that 'air, water, certain chemical

[23] Wundt, Wilhelm, 'Rubinstein, Dr. Sus., die sensoriellen und sensitiven Sinne', *LC*, 1875, 22. The 'gift' in question would be her doctorate.
[24] Wundt, Wilhelm, 'Preyer, W., das myophysische Gesetz,' *LC*, 1874, 32. Wundt's emphasis.
[25] Wundt, Wilhelm, 'Büchner, Dr. Ludw., physiologische Bilder,' *LC*, 1875, 49.
[26] Wundt, Wilhelm, 'Ranke, Dr. Joh., Prof., Grundzüge der Physiologie des Menschen,' *LC*, 1873, 21.
[27] Wundt, Wilhelm, 'Rauch, P.M., die Einheit des Menschengeschlechtes,' *LC*, 1873, 12.
[28] Wundt, Wilhelm, 'Volkelt, Dr. Joh., 'Die Traum-Phantasie,' *LC*, 1876, 31.
[29] Wundt, Wilhelm, Schellwien, Rob., 'das Gesetz der Causalität in der Natur,' *LC*, 1877, 33; Wilhelm Wundt, 'Entleutner, A.F., Naturwissenschaft, Naturphilosophie und Philosophie der Liebe,' *LC*, 1877, 52.

primordial matter and heat' are the 'external elementary conditions of life' to the classical theory of the four elements.[30] Other books are brushed off as too dependent on Fichtean idealism or dismissed as a product of Schopenhauerian idealism.[31] An avid follower of Hegel was harshly reviewed, as well: 'In this volume, we basically have only an account of the Hegelian logic, which distinguishes itself form the master's dry tone only somewhat by the fact that it has been abundantly spiced up with more or less fitting poetic quotes'.[32] After mockingly citing some of these quotes, Wundt concluded: 'These examples should suffice to show how the author has not failed to bestrew the thorny road of dialectics with manifold flowers'. A similar dismissive attitude was shown towards a book on phrenology.[33]

In the light of his discussions with Fechner it is not surprising that Wundt also disapproved of any works supporting a notion of spiritism.[34] Since his review of Owen's and Aksakov's spiritist publications was written a full year before Zöllner's publications on spiritism and two years before Wundt's debates with Fechner and other supporters of spiritist theories, he was still convinced that it would not catch on in Germany: 'In Germany, we can only find two scholarly so-called authorities that are known to be held in high regard in spiritist circles, namely Prof. of Zoology Max Perty in Bern and Prof. of Philosophy Franz Hofmann in Würzburg, and even these men have affiliated themselves with the spiritist efforts with some reservations'.

Even though Wundt used his reviews to discredit specific philosophical traditions, he did not dismiss every author with whom he disagreed. He praised one Hegelian for his efforts to bring together Hegelian speculation and modern scientific psychology: 'Even if one cannot agree with the author on all his views, nobody will put down the lucidly and appealingly written little book without feeling very inspired'.[35] A book by an orthodox Herbartian was also praised. Even if Wundt was rather critical of Herbart's philosophy, he welcomed the book 'with honest pleasure, and partly exactly because it provides an understandable exposition which is as faithful as possible and which is also suitable for a wider circle, for whom Herbart's own works are hardly palatable'.[36] Even Wundt's favourite antagonists, the *Naturphilosophen*, could sometimes get a benevolent review. Though he negatively compares one author's 'fanciful combinations' with Darwin's bold but 'sober and careful' studies, his final judgement is mild; he wholeheartedly recommends the book to anyone

[30] Wundt, Wilhelm, 'Preyer, Wilh., über die Erforschung des Lebens,' *LC*, 1873, 25.
[31] Wundt, Wilhelm, 'Schmitz-Dumont, Zeit und Raum,' *LC*, 1876, 30; Wundt, Wilhelm, 'Meynert, Th., Prof., zur Mechanik des Gehirnbanes,' *LC*, 1875, 5.
[32] Wundt, Wilhelm, 'Michelet, C.L., das System der Philosophie als exacter Wissenschaft,' *LC*, 1877, 9.
[33] Wundt, Wilhelm, 'Noel, R.R., die materielle Grundlage des Seelenlebens,' *LC*, 1874, 41.
[34] Wundt, Wilhelm, ' Owen, R.D., 1) das streitige Land 2) Psychische Studien,' *LC*, 1877, 21. For a short account of the discussion about spiritism between Wundt and Fechner, see: Chapter 2, 52.
[35] Wundt, Wilhelm, 'Carneri, B., Gefühl, Bewußtsein, Wille. Eine psychologische Studie,' *LC*, 1877, 9.
[36] Wundt, Wilhelm, 'Volkmann v. Volkmar, Dr. Wilh., Prof., Lehrbuch der Psychologie,' *LC*, 1876, 2.

interested in investigating the similarities and differences between Darwin's theory of evolution and certain ideas of growth and change that can be found in *Naturphilosophie*.[37]

The above case studies suggest that different disciplines were shaped by different ideals of good scholarship. Researchers in linguistics and medicine had a shared understanding of these ideals. Therefore, book reviews in these disciplines tended to either take the shape of announcements or to be mostly positive. In theology and philosophy, however, there was no consensus. Both Nöldeke and Wundt defended an ideal of scholarship informed by positivism and empiricism, against what they saw as the dogmatism of grand ideas and idle speculation. In Nöldeke's theological reviews this was articulated through a recurring criticism of work marred by dogmatic religious thinking. In Wundt's philosophical writing this was expressed through a series of dismissive reviews of books influenced by equally dogmatic speculative philosophies, such as *Naturphilosophie*, phrenology, vitalism, spiritism and idealism. Before jumping to further conclusions, however, it is worthwhile to take a look at other ways in which Nöldeke's and Wundt's reviews can be categorised.

Nöldeke on nationality and religion

Especially after the Franco-German War fervent nationalism was very common among German academics. Many thought that German scholarship was superior to foreign scholarship and intimately connected to a specifically German way of thinking.[38] The existence of such beliefs suggests that non-German publications might be reviewed more critically than German ones. German nationalism could also take the shape of anti-Catholicism. Because a large number of German scholars were Catholic and many foreign scholars were Protestant, this religious divide did not exactly coincide with the national divide. Therefore, it is worth looking at the reception of works by both non-German and non-Protestant authors. The latter group includes not only Catholics but also Jews. After all, as one commentator argues, in the 19th century 'the place of Jews and Judaism in society and theology was a perennial question'.[39]

In one of Nöldeke's main fields of specialisation, Old Testament studies, the question of the position of Jews is especially relevant. Nöldeke's aversion of religious — and therefore, in his eyes, uncritical — approaches to this field raises the question to what extent he might be more critical

[37] Wundt, Wilhelm, 'Baumgärtner, Heinr., die Weltzellen,' *LC*, 1876, 18.
[38] For example, see Goschler, Constantin, 'Deutsche Naturwissenschaft und naturwissenschaftliche Deutsche: Rudolf Virchow und die »deutsche Wissenschaft«,' in: Jessen, Ralph and Jakob Vogel (eds.), *Wissenschaft und Nation in der Europäischen Geschichte*, Campus, Frankfurt/New York, 2002, 97–114. 110–111.
[39] Gerdmar, Anders, *Roots of Theological Anti-Semitism: German Biblical Interpretation and the Jews, From Herder and Semler to Kittel and Bultmann*, Brill, Leiden, 2009. 3.

of Jewish authors than of others. If we take Robert Irwin's description of him as a 'racial bigot' at face value, at least some antisemitism can be suspected of him.[40] After all, a tacit dislike of Jewish scholars was not uncommon in German academia.[41] In addition the ideology of *Kulturprotestantismus* was widely shared among a majority of German academics and its appeal to secular teaching and research methods encouraged the portrayal of religiously inspired scholars as blatantly unscientific.[42]

Nonetheless Jewish scholars were quite well-represented in 19th-century German Oriental and Old Testament studies. They were sometimes seen as Orientals whose customs and thoughts were closer to Biblical peoples than to Christian Germans. Therefore they were assumed to be able to mediate between East and West as well as between Biblical times and the present.[43] There was also a tacit assumption that their Hebrew might be better than that of others.[44] One contemporary author even started his analysis of the role of Jews in Semitic and Old Testament studies with the observation that hardly any anti-Semitic statement could be found in the scholarly literature of late 19th-century *Altorientalistik*.[45] He admitted that some scholars harboured such sentiments, but argued that 'no evidence can be found for the exclusion of Jews in this field'.[46] Even if this may be overly optimistic, Nöldeke's reflections on his Jewish students support the idea that his anti-Semitic prejudice did not run very deep: 'I now have two more Jews in my audience […]. It seems as if through time my lectures change into an actual seminar for higher Judaism. Well, if people are this industrious, it is fine with me!'[47]

Still, some of Nöldeke's reviews were quite critical of Jews and their religion. This is especially obvious when he discussed books about modern-day Judaism. His review of the first volume of Abraham Geiger's posthumously published essays mixed praise and criticism.[48] He called him 'an educated, brilliant, erudite, humane, yet spirited man', recognising the mentality of *Kulturprotestantismus*: 'He knows as an erudite researcher how so much of that which is unchangeable

[40] Irwin, *For Lust of Knowing*, 198.
[41] Pawliczek, Aleksandra, 'Kontinuität des informellen Konsens': Die Berufungspolitik der Universität Berlin und ihre Dozenten im Kaiserreich und in der Weimarer Republik,' in: Bruch, Rüdiger von, Uta Gerhardt and Aleksandra Pawliczek (eds.), *Kontinuitäten und Diskontinuitäten in der Wissenschaftsgeschichte des 20. Jahrhunderts*, Franz Steiner, Stuttgart, 2006, 69–92. 70.
[42] Marchand, *German Orientalism*, 77.
[43] Joskowicz, Ari, *The Modernity of Others: Jewish Anti-Catholicism in Germany and France*, Stanford University Press, Stanford (CA), 2014. 5.
[44] Marchand, *German Orientalism*, 77.
[45] Renger, Johannes, 'Altorientalistik und jüdische Gelehrte in Deutschland – Deutsche und österreichische Altorientalisten in Exil,' in: Barner, Wilfried and Christoph König (eds.), *Jüdische Intellektuelle und die Philologien in Deutschland 1871–1933*, Wallstein, Göttingen, 2001, 247–262. 249.
[46] Ibid., 251.
[47] UBL: BPL 2389, Theodor Nöldeke to Michael Jan de Goeje, 9 November 1874.
[48] Nöldeke, Theodor, 'Geiger's, Abrah., nachgelassene Schriften,' *LC*, 1875, 32.

and holy to the rigid old believers has been formed, over the course of time, and how Judaism, too, has found itself in continual development, albeit not always in a progressive way'. But Nöldeke also had some major complaints. He accused Geiger of incorrectly retracing too many features of contemporary life to Jewish traditions. He also criticised the Jewish religion, arguing that three of its practices were incompatible with modern society: the dietary laws, the 'unreasonable strictness' of the Sabbath and the practice of circumcision. Nöldeke finally characterised Geiger's positive qualities as inconsistent with his attachment to Judaism, even though his general verdict of Geiger's book was positive.

Nöldeke's review of Seligmann Meyer's critical reply to a series of anti-Semitic articles published by Hermann Messner was ambivalent, as well.[49] He called Messner's writing 'a judgement of modern Judaism that is as loveless as it is ignorant' and stated that it is easy for Meyer to counter these 'superficial and hateful allegations'. A few sentences later, however, the criticism starts again: '[he] lapses back into an apologetic style, which tends to praise everything Jewish as such and does not want to acknowledge that the tragic histories of the Jews are largely based on their own faults'. Referring to his earlier Geiger review he again mentioned dietary laws, the strictness of the Sabbath and circumcision. He even added a few lines in which he repeated the old anti-Semitic trope about the Jewish commercial spirit! Notwithstanding these elaborations, his final verdict of Meyer's booklet was again quite positive.

When reviewing books by Jewish authors that do not touch on contemporary religious practices Nöldeke's rarely mentioned their Jewishness. Still, some of his reviews would draw attention to two major weaknesses that he presented as typically Jewish. One criticism was that some Jewish authors would stick too close to 'the authority of the old Jewish tradition'.[50] I have already discussed this type of accusation in the above section on Nöldeke's most common criticism of books in the *Theology* section. The second weakness Nöldeke mentioned was one of style rather than content: 'A flaw that is, alas, common among Jewish writers, which can be found in the work of this author as well, is a too flowery account and a tendency to embellishment in the depiction of Jewish events'.[51] This observation recurs repeatedly. One author was encouraged to use a more 'prosaic' style in his follow-up study.[52] Another received the criticism that, first and foremost, he should have 'cut out some pompous expressions'.[53]

[49] Nöldeke, Theodor, 'Meyer, S., ein Wort an Herrn Hermann Messner,' *LC*, 1877, 52.
[50] Nöldeke, Theodor, 'Bloch, J.H., Ursprung und Entstehungszeit der Buches Kohelet,' *LC*, 1873, 12.
[51] Nöldeke, Theodor, 'Gross, Dr. Siegm, Menahem ben Saruk,' *LC*, 1872, 28.
[52] Nöldeke, Theodor, 'Cassel, Dr. David, Geschichte der jüdischen Literatur,' *LC*, 1873, 12.
[53] Nöldeke, Theodor, 'Masechet Soferim. Der talmudische Tractat der Schreiber,' *LC*, 1879, 16.

Even if Nöldeke thought that he had pointed out some typical Jewish weaknesses, we cannot conclude that he was more critical of Jews than of others. In total, he wrote 27 reviews of books by Jewish authors and four of these reviews were highly critical. These numbers do not allow us to infer that Nöldeke was strongly biased in this respect. It should also be noted that 15 out of 27 of the reviewed works written by Jewish authors were reviewed in the *Theology* section. So, although Nöldeke may have been somewhat more likely to provide a critical review of books by Jewish authors, this can also be explained by the fact that Jewish authors seemed to be more likely to publish on exactly those theological issues about which he tended to be more critical to begin with.

Nöldeke's opinion of Catholicism was not more favourable than his thoughts on Judaism. When he was asked to be an expert witness in an Austrian blood libel court case, he sarcastically commented on it to De Goeje by quoting from a Heinrich Heine poem: 'But it simply seems to me / That the rabbi and the monk / That both of them they stink'.[54] Some of his earlier critical discussions of dogmatism were also directed against Catholic scholars rather than Jews. His criticism is clearly summarised in the summary evaluation of one Catholic author's book on the Old Testament: 'The book may not contain many independent judgements'.[55] It is noticeable, however, that he usually did not discuss works by vocal Catholic authors this dismissively.

A large number of these works were text editions and translations of the Syriac Church Fathers and their contemporaries. Nöldeke's main complaints were not that these editions were lacklustre, but rather that the editor or translator could have selected texts that were more worthy of publication. An important reason for Catholic scholars to decide to publish a certain text, he noted, was the orthodoxy of their faith. He for example strongly disagreed with the grouping of texts in Gustav Bickell's *Conspectus Rei Syrorum* which was based on whether the Catholic church considered these texts to be 'orthodox' or 'heretic'.[56] Nöldeke also criticised Bickell's selection of texts in his *Ausgewählte Schriften der syrischen Kirchenväter*, which showed a 'restriction to such older Church writers, who he deems to be strictly orthodox'.[57] He also dismissed a commentary on a selection of Syriac texts from archives in Rome, as follows: 'The content of most pieces contained in this

[54] UBL: BPL 2389, Theodor Nöldeke to Michael Jan de Goeje, 8 March 1885. In German, it reads: 'doch es will mich schier bedünken daß der Rabbi und der Mönch daß sie alle beide stinken'. The English translation is taken from: Schlesier, Renate, 'Homeric Laughter by the Rivers of Babylon: Heinrich Heine and Karl Marx,' in: Gelber, Mark H. (ed.), *The Jewish Reception of Heinrich Heine*, Niemeyer, Tübingen, 1992, 21–44. 36. Nöldeke ended up defending the Jewish defendant against the accusations.
[55] Nöldeke, Theodor, 'Zschokke, Dr. Herm., Prof., historia sacra Antiqui Testamenti,' *LC*, 1873, 1.
[56] Nöldeke, Theodor, 'Bickell, Gustavus, conspectus rei Syrorum,' *LC*, 1871, 30.
[57] Nöldeke, Theodor, 'Ausgewählte Schriften der syrischen Kirchenväter übersetzt von Prof. Dr. Gust. Bickell,' *LC*, 1877, 12.

volume is, alas, once more quite insignificant, at least in comparison to the many important things that the editor could have found among the Roman manuscripts'.[58]

All these boring and insignificant pieces, however, still had one point of interest; they provided an insightful picture of early Christian thought. Even if Nöldeke initially denied the value of the writings of Isaac of Antioch, he admitted that 'they are still important as a document of the views, feelings and desires of the Christian Syrians at a time, when these played a very significant role in the development of the church and its dogmas'.[59] And since he also considered many of these editions to be competently edited, he repeatedly urged his Protestant compatriots to buy these inconsequential products of Catholic scholarship anyway.[60] It should be noted, however, that this verdict cannot unequivocally be interpreted as approval of the proficiency of all Catholic scholarship; more than half of the reviewed works by vocally Catholic authors were either by Gustav Bickell or by Antonio Maria Ceriani. Still, based on his praise for these men, Nöldeke was about as likely to be critical of the work by a Catholic author as of that by a Protestant one.

Finally, there is the question of Nöldeke's verdict on foreign works. In his private correspondence, he repeatedly criticised French scholarship: 'It's a sad situation with the Arabists in Paris, anyway. Who holds the chairs of De Sacy and Quatremêre? Ever since Guyard died, Derenbourg junior has the whole field for himself. Zotenberg, who's superior to all of them, is pushed completely into the background'.[61] In addition, Nöldeke also repeatedly stressed that vanity was a typical French character trait.[62] This criticism did not, however, translate into highly critical reviews of French scholarship. He was dismissive of Sédillot's *Histoire générale des Arabes*, but not remarkably critical of other French books.[63] The other reviewed foreign books are from various countries, such as the United Kingdom, the Netherlands, Italy and Russia. In total, there are 36 reviews of non-German books, 6 of which were dismissively reviewed. He may have been somewhat more critical of non-German books, but the difference is hardly significant.

Wundt on nationality and religion

Wundt's attitude towards scholars who did not fit the mould provided by the liberal nationalism of *Kulturprotestantismus* is even harder to define than Nöldeke's stance. For a start, there was less of

[58] Nöldeke, Theodor, 'Monumenta syriaca ex Romanis codicibus collecta,' *LC*, 1878, 29.
[59] Nöldeke, Theodor, 'J. Isaaci Antiocheni,' *LC*, 1873, 8.
[60] For example, see Nöldeke, Theodor, 'Ausgewählte Gedichte der syrischen Kirchenväter,' *LC*, 1872, 28.
[61] UBL: BPL 2389, Theodor Nöldeke to Michael Jan de Goeje, 3 August 1886.
[62] UBL: BPL 2389, Theodor Nöldeke to Michael Jan de Goeje, 27 November 1858 and 15 September 1859.
[63] Nöldeke, Theodor, 'Sédillot, L.-A., Prof., histoire générale des Arabes,' *LC*, 1877, 35.

a Jewish presence in both the medical and the philosophical community than in Old Testament studies and Semitic linguistics. Alexandra Pawliczek's study of Jews appointed at the University of Berlin suggests that the faculty of law was the most welcoming to Jews; more than a quarter of the appointees were from Jewish families.[64] Almost 10% of the people of Jewish descent who finished their *Habilitation*, would end up as full professors. Although the number of Jews at the medical faculty was higher than at the law faculty, only 3% of the Jews with a medical *Habilitation* would become full professors. The faculty of philosophy, finally, offered even fewer career opportunities to Jewish scholars. And, since Jewish scholars were not uncommon in Semitic studies, the share of people with a Jewish background in other disciplines within this faculty, such as philosophy, must have been strikingly low.

The people of Jewish decent whose books Wundt reviewed are a religiously diffuse group. Many of the Jewish orientalists either taught as *Privatdozenten* at the periphery of the university system or worked at Jewish religious and educational institutions.[65] Staying at the periphery of the academic hierarchy, they did not have to compromise their religious convictions and could maintain a close relationship with their faith communities. The medical authors discussed by Wundt, however, were more dependent on institutions. Physiological and anatomical investigations required workspaces, research materials and tools that were only available at the well-endowed research institutes of German academia. Within these institutions there was a high pressure to convert to Christianity. Of all German professors of Jewish descent about 13% had not been baptised.[66] Most people with a Jewish background reviewed by Wundt had made their career as mainstream supporters of *Kulturprotestantismus*. Authors like Julius Bernstein, Jacob Henle and Rudolf Heidenhain came from Jewish families but presented themselves as Protestants. And since one's religious background is not as self-evidently relevant to medical research as to Old Testament studies, it is hardly surprising that it is not possible to discern a distinct attitude towards the work of Jewish authors in Wundt's reviews.

Wundt did not have any religiously motivated reason to be sceptical of the medical literature written by Catholics either. Many books by Catholic authors at the German market were written by Austrians. During the second half of the 19th-century Austrian medical research was highly regarded across Europe. The re-emergence of the Vienna Medical School heralded what has been

[64] Pawliczek, 'Kontinuität des informellen Konsens,' 73–75.
[65] On Jewish research and educational institutions, see: Trautmann-Waller, Céline, 'Selbstorganisation jüdischer Gelehrsamkeit und die Universität seit der ›Wissenschaft des Judentums‹,' in: Barner, Wilfried and Christoph König (eds.), *Jüdische Intellektuelle und die Philologien in Deutschland 1871–1933*, Wallstein, Göttingen, 2001, 77–86.
[66] Hammerstein, Notker, 'Universitäten in Kaiserreich und Weimarer Republik und der Antisemitismus,' in: Barner, Wilfried and Christoph König (eds.), *Jüdische Intellektuelle und die Philologien in Deutschland 1871–1933*, Wallstein, Göttingen, 2001, 25–34. 30.

called a 'Golden Age of medicine in Vienna'.[67] The Vienna General Hospital led the way in pathological anatomy and clinicopathological correlations.[68] Physiology was well represented by Ernst Brücke.[69] Wundt recognised the merit of the leading figures of the Vienna School and reviewed their works positively. In one review he even explicitly mentioned the merit of Vienna's physiological institute.[70] His review of Brücke's *Vorlesungen über Physiologie* was also concluded with praise: 'Neither the professional physiologist nor the student will indeed put these lectures aside without having derived a large amount of instruction from it'.[71] In the same issue of the *Centralblatt,* he also praised a study by Brücke's colleague, Joseph Hyrtl: 'We have no doubts that [this research method] will receive more attention than it has received so far, thanks to this work by the famous Viennese anatomist'.[72]

All in all, there's no compelling reason to assume that Wundt was more critical of either non-Germans or non-Protestants. Even if I have not been able to establish the nationality of all authors whose work he reviewed, it is likely that he is as critical of works by German authors as of those by others. At least 39 of his reviews discuss books by non-German authors, so a maximum of 84 reviews are of books by Germans. I have not been able to identify the nationality of four of the authors whose books were harshly reviewed, though they all have German-sounding names.[73] If we assume that of these people Jos. Raith is Austrian, based on the fact that his book has been published in Vienna, it seems permissible to assume that the other three authors, whose books have been published in Germany, are German.[74] If this is the case that would mean that thirteen out of a maximum of 84 German books have been reviewed dismissively. This amounts to more or less the same ratio of critical reviews as can be found for the total corpus.

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[67] Vogl, Alfred, 'Six hundred years of medicine in Vienna: A History of the Vienna School of Medicine,' *Bulletin of the New York Academy of Medicine*, 1967, 43(4), 292–299. 290–291.
[68] Schagen, Udo, 'Germany, Austria, and Switzerland,' in: *The Oxford Companion to Medicine*, Oxford University Press, Oxford, 2001. (accessed 25 October 2017 at: http://www.oxfordreference.com/view/10.1093/acref/9780192629500.001.0001/acref-9780192629500-e-208)
[69] Vogl, 'Six hundred years of medicine in Vienna,' 293.
[70] Wundt, Wilhelm, 'Exner, Dr. Sigm., Privatdoc., Leitfaden bei der mikroskopischen Untersuchung thierischer Gewebe,' *LC*, 1873, 26.
[71] Wundt, Wilhelm, 'Brücke, Ernst, Vorlesungen über Physiologie,' *LC*, 1873, 39.
[72] Wundt, Wilhelm, 'Hyrtl, Dr. Jos., Prof., die Corrosions-Anatomie,' *LC*, 1873, 39.
[73] The reviews in question are: Wilhelm Wundt, 'Schmitz-Dumont, Zeit und Raum, *LC*, 1876, 30; 'Raith, Jos., Entdeckungen im Gebiete der geistigen Verrichtungen des Centralnervensystems,' *LC*, 1877, 10; 'Krause, Alb., die Gesetze des menschlichen Herzens,' *LC*, 1877, 14; 'Entleutner, A.F., Naturwissenschaft, Naturphilosophie und Philosophie der Liebe,' *LC*, 1877, 52.
[74] Schmitz-Dumont is published in Leipzig, Widemann in Schoss-Chemnitz, Krause in Schauenburg, and Entleutner in München.

Nöldeke on academic insiders

The authors whose books have been reviewed can be categorised in a different way, as well. Most works have been written by people with a university affiliation. Some books, however, were published by unaffiliated authors. Because I have not been able to collect sufficient biographical details about all authors, I have defined this group of unaffiliated authors as consisting of two sub-groups. The first contains all people whose non-academic career could be established. This group largely consists of clergymen, high school teachers and publicists. The second sub-group contains people about whose career I could not find any information. Because German biographical dictionaries usually include lesser-known publishing *Privatdozenten,* and since authors with an academic affiliation often mention this on the title pages of theirs books, I have assumed that these people also worked outside of the academic establishment.[75]

One of the tasks that reviewers set themselves was to clearly distinguish true academic accomplishments from the work of dilettantes. Some expressions in Zarncke's 1874 retrospective essay also emphasise this ambition: 'Serious scholarly criticism has retreated to specialised journals, the wider public and even the scholar with an interest outside his own field are clueless. It was necessary to create an organ that envisioned a comprehensive overview of scholarly literature, aiming for the most exhaustive completeness while still not renouncing the strictest standards of our scholarship in any way. […] In this way, one can hope to accomplish another, higher goal — that of carrying the sense of correct and exact research methods into wider circles'.[76]

The scholars with an academic affiliation form quite a diverse group. There is a significant difference in status between someone who just finished his dissertation and a full professor. Before I discuss the evaluation of the work of established scholars and academic outsiders, I will therefore look at reviews of dissertations. Their authors found themselves in an intermediate category between the establishment and the scholarly periphery. They did not have the academic experience of even the most recently appointed *Privatdozent*, but they were affiliated to universities where their work was held up to the scholarly standards of examination committees and *Doktorvaters*. It is noticeable that Wundt hardly reviewed any dissertations for the *Centralblatt*. The only one was Susanna Rubinstein's study.[77] Not only does this one, highly critical review tell us little about Wundt's attitude towards young scholars, the author is also untypical as the only woman to have her book reviewed by Wundt or Nöldeke in the *Centralblatt,* in the 1870s.

[75] References to a large number of biographical dictionaries and lexica can be found at: http://www.zeno.org/.
[76] Zarncke, Friedrich, 'An unsere Leser,' *LC*, 26 December 1874. 1.
[77] Wundt, Wilhelm, 'Rubinstein, Dr. Sus., die sensoriellen und sensitiven Sinne,' *LC*, 1875, 22.

More than 10% of Nöldeke's reviews, however, concerned dissertations. Only one of these reviews was highly critical.[78] This dissertation argued that Muhammed used to be a faithful Christian until pride and lewdness made him stray from the path of righteousness. Nöldeke concluded that, 'it will not be easy to convert this reviewer to such curious views as those presented here, even if they would have been put forward in a less dilettantish fashion'. This stern judgement of a dissertation was not typical. Though Nöldeke often found something to criticise in the work of the new doctors, he usually praised them, as well. He then brushed his initial criticisms aside as trivialities and concluded with a variation on the same welcoming words. 'We hope that we can meet the author again as a contributor to the field of Oriental studies,' were the closing words of one of his reviews.[79] 'We expect quite some contributions to scholarship of this young scholar, who can already present such a competent accomplishment and we express the hope that he will not limit himself to the Arabic grammar,' was the last sentence of another review.[80] Martin Houtsma's dissertation merited a review which closed as follows: 'We are looking forward with high expectations to Houtsma's promised investigations about the further development of the Islamic dogmatics'.[81] Dissertations were evaluated by Nöldeke not only on their content, but also on the promise they showed.

The ultimate insiders of the academic community were the professors. Of the books reviewed by Nöldeke, however, only about one third had been published by someone who was either an *Ordinarius* or an *Extraordinarius*. These professors received dismissive reviews as often as others did. Most of these critical reviews were published in the *Theology* section. Although Nöldeke admitted that he was free from 'religious-dogmatic prejudice' he argued that a Leipzig professor still clung too much to 'tradition' in order to paint a 'colourful picture'.[82] The work by a Professor of Old Testament Studies in Vienna was casually dismissed with the observation that he 'invariably chooses for the official views held in the church'.[83] About a German Protestant teaching in Vienna, Nöldeke concluded that, for him, 'scholarly criticism no longer has any value, when it is in contradiction with his religious views'.[84] A professor at the *Collège de France* was criticised for a lack of criticism. Nöldeke argued that he did not seriously make use of the critical investigations by

[78] Nöldeke, Theodor, 'Bethge, Frideric., Raḥmân et Aḥmad,' *LC*, 1872, 26.
[79] Nöldeke, Theodor, 'Sasse, C.J. Franz, Prolegomena in Aphrastis Sapientis Persae sermones homilecticos,' *LC*, 1879, 13.
[80] Nöldeke, Theodor, 'Fünf Streitfragen der Basrenser und Kufenser […] herausgegeben und bearbeitet von Dr. Jaromir Košut,' *LC*, 1878, 38.
[81] Nöldeke, Theodor, 'Houtsma, M.Th., de strijd over het dogma,' *LC*, 1875, 34.
[82] Nöldeke, Theodor, 'Fürst, Dr. Jul., Prof., Geschichte der biblischen Literatur und des jüdisch-hellenistischen Schriftthums,' *LC*, 1871, 14.
[83] Nöldeke, Theodor, 'Zschokke, Dr. Herm., Prof., historia sacra Antiqui Testamenti,' *LC*, 1873, 1.
[84] Nöldeke, Theodor, 'Böhl, Ed., Forschungen nach einer Volksbibel zur Zeit Jesu,' *LC*, 1873, 37.

others, did not show the ability to be critical himself, and was too dependent on secondary sources.[85] All in all, someone's position as a professor hardly seems to have influenced the severity of Nöldeke's reviews. Most of his dismissive reviews of professorial production can be traced back to his earlier observed tendency to be highly critical of works influenced by religious dogma or tradition, which is discernible in his reviews of scholars of all denominations.

Wundt on academic insiders

The difference between the evaluation of work by professors and that by others is more clear in the reviews by Wundt. Of his 79 reviews of works by *Ordinarien* and *Extraordinarien,* only 5 were explicitly dismissive. This number is very low, compared to his total number of 19 out of 123 negative reviews. The above sections already mention that a remarkably high number of Wundt's most critical reviews can be found in the *Philosophy* section. Even his negative reviews in other sections tend to deal with philosophical issues.

Of Wundt's dismissive reviews of professorial publications, only one was printed in the medical section and one other in the section dedicated to the natural sciences. Two of the dismissively reviewed professorial books are by one author: Wilhelm Preyer. The first is his *Über die Erforschung des Lebens* in which he expounds his theory of the four elements. The other is *Das myophysische Gesetz*, which Wundt criticised for containing mistaken interpretations of physiological experiments that invalidate the books attempts to understand Weber's Law.[86] The other negative reviews — all published in the *Philosophy* section — criticise the outmoded idealistic character of the discussed works. Even if Schopenhauerian idealism and Hegelian dialectics were perfectly respectable in late 19th-century German academia, we have already seen that Wundt was highly critical of these traditions, which he considered to be irremediably old-fashioned an unscientific.[87] All in all, however, Wundt seems to have either highly valued the work of his professorial peers or to have been careful not to be too dismissive of those people who had proved to be able to make an academic career. This inclination is particularly noticeable in his reviews of books about medical and the natural sciences. Works on philosophical topics could still expect critical scrutiny.

A quick comparison of Nöldeke and Wundt shows that in both cases an author's academic affiliation could have high predictive value for his chance to receive a positive review. However, in

[85] Nöldeke, Theodor, 'Sédillot, L.-A., Prof., histoire générale des Arabes,' *LC*, 1877, 35.
[86] Wundt, Wilhelm, 'Preyer, W., das myophysische Gesetz,' *LC*, 1874, 32.
[87] Wundt, Wilhelm, 'Meynert, Th., Prof., zur Mechanik des Gehirnbanes,' *LC*, 1875, 5; 'Michelet, C.L., das System der Philosophie als exacter Wissenschaft,' *LC*, 1877, 9.

Nöldeke's case this mostly shows in his treatment of doctoral dissertations. These were judged not only on the value of their contents but also on whether they promised valuable future contributions by their authors. This benevolent interest in doctoral dissertations cannot be recognised in Wundt's reviews because he hardly reviewed any. What is noticeable, however, is that authors with a professorial appointment were more likely to be positively reviewed than authors without such a position. Professorial authors could not, however, get away with everything. If they discussed philosophical issues, they still risked critical scrutiny.

Nöldeke on academic outsiders

Academic outsiders also stand out. Seven of Nöldeke's negative reviews are of books by academic outsiders. Bernard Neteler was the vicar of Loburg Castle, Joseph Samuel Bloch was still two year away from his doctorate in Zürich, Adolf Brühl was a teacher at the *Philanthropin* in Frankfurt, Adolf Koch was a gymnasium professor in Schaffhausen, Giuseppe Barzilai was an attorney in Triest, and John Mühleisen-Arnold was the rector of St. Mary's Church in Capetown. I could hardly find any information about Georg Janichs. The title page of the 1871 edition of his *Animadversiones criticae* shows that he held a doctorate in philosophy and a licentiate in theology.[88] Nöldeke seems not to have known Janichs; he did not recognise the book as a reprint of his licentiate's thesis.[89] Nöldeke's main criticism of him was that his knowledge was simply insufficient. He described the small book as 'merely preparatory work', arguing a point that would 'not be doubted by any modern-day expert', based on unsatisfactory knowledge of Syriac.[90] Nöldeke criticised some of the other outsider authors for their lack of basic academic skills, as well. Giuseppe Barzilai is put aside as a 'dilettante'.[91] Mühleisen-Arnold is said to miss 'the necessary knowledge for scholarly judgement of the Islam'.

Apart from the accusation of dilettantism, we also find the well-known accusation of a religiously inspired lack of criticism. This comes as no surprise because Arnold-Mühleisen and Bernard Neteler were clergymen, while Bloch would choose for the rabbinate after finishing his doctorate. On Mühleisen-Arnold, Nöldeke commented that 'his theological point of view and his theological bias do not allow him an unprejudiced judgement about a non-Christian religion at all'. Neteler

[88] Janichs, Georgius, *Animadversiones Criticae in versionem Syriacam Peschitthonianam Librorum Koheleth et Ruth*, Schletter'sche Buchhandlung, Breslau, 1871.
[89] This becomes apparent from the title page of the earlier edition: Janichs, Georgius, *Animadversiones Criticae in versionem Syriacam Peschitthonianam Librorum Koheleth et Ruth*, Teubner, Leipzig, 1869.
[90] Nöldeke, Theodor, 'Janichs, Georg, animadversiones criticae in versionem Syriacam Peschitthonianam,' *LC*, 1871, 49.
[91] Nöldeke, Theodor, 'Barzilai, Dr. G., le lettere dell'alfabeto fenicio,' *LC*, 1876, 30.

'knows no other authority then that of the church'.[92] Bloch, finally, allows 'the authority of the old Jewish tradition' to inform an uncritical relationship with his source material.[93] All in all, people from outside the university system, often, were more likely to be critically evaluated by Nöldeke than others, because among them he found not only a large contingent of dilettantes but also an above-average number of religiously informed authors. And in his eyes both dilettantism and religious dogmatism were among the biggest threats of good scholarship.

Wundt on academic outsiders

Wundt's critical attitude towards non-academic authors is even more pronounced than Nöldeke's. Though he wrote fewer reviews of works of such outsiders, he wrote a higher number of critical ones. Out of a total of 24 reviews of outsiders 13 reviews were dismissive. Seven of these outsiders worked as either a surgeon, doctor or assistant doctor, and only one of Wundt's reviews of these medical professionals was negative. That means that almost two thirds of the authors who were affiliated to neither a university nor a hospital were reviewed critically. Among these people we find the rector of a Catholic Gymnasium, an attorney, a philosophically inclined politician and a theologian.

Almost all the authors dismissively reviewed by Wundt wrote books that he reviewed in the *Philosophy* section. This discipline clearly attracted the highest number of unaffiliated authors. However, two books about both medicine and the natural sciences received Wundt's disapproval as well. One of these was the book that aimed to show that 'the biblical story about the descent of all people from one couple, should not simply be discarded to the realm of fairy tales'.[94] The other critically evaluated medical treatise was the book in which the author did not allow the reader to ask why consciousness had the character he ascribed to it.[95]

Even though many negatively reviewed philosophical authors lacked a university affiliation, this did not necessarily imply their lack of academic education. Two of them, for example, were closely associated with Friedrich Nietzsche. Paul Widemann was a former student of Nietzsche, while Paul Rée was one of the philosopher's best friends.[96] Wundt, however, was very critical of Nietzsche's philosophy. In a 1877 contribution to *Mind,* he summarised his position as follows: 'In the writings

[92] Nöldeke, Theodor, 'Neteler, B., Studien über die Echtheit des Pentateuchs,' *LC*, 1873, 1.
[93] Nöldeke, Theodor, 'Bloch, J.H., Ursprung und Entstehungszeit der Buches Kohelet,' *LC*, 1873, 12.
[94] Wundt, Wilhelm, 'Rauch, P.M., die Einheit des Menschengeschlechtes,' *LC*, 1873, 12.
[95] Wundt, Wilhelm, 'Büchner, Dr. Ludw., physiologische Bilder,' *LC*, 1875, 49.
[96] Small, Robin, *Nietzsche and Rée: A star friendship*, Oxford University Press, Oxford, 2005. Though the emphasis of the book is on the relationship between Nietzsche and Rée, Widemann is mentioned on page 4.

of Nietzsche and others of his stamp, the pessimistic mood is combined in a very peculiar way with an enthusiastic devotion to certain ideas closely related to religious mysticism'.[97] This did not bode well for his reviews of Nietzsche's associates and indeed the sour conclusion to his evaluation of Rée's book states that though it is 'not without interest as a peculiar product of the ethics of modern-day pessimism,' this interest, was 'psycho-pathological' rather than intellectual.[98]

The other highly critical reviews of authors without academic affiliation can be divided into three categories: people advocating new but unpromising fads, authors propagating outmoded philosophies and those who misrepresented Kant. The first group largely consisted of the advocates of spiritism. The only redeeming value of Johannes Volkelt's work on dreams was that it showed that '[...] where superstition has gained such a regrettable pervasion as is the case with contemporary spiritism, the philosophical expression of such intellectual currents cannot be absent either'.[99] He was also glad to notice in another review that spiritism's reception in Germany convincingly showed it to be 'an exotic growth in Germany […] that does not truly prosper among us'.[100] Meanwhile, a critical discussion of spiritism merited praise: 'Hopefully [this booklet] contributes to this purpose, which is that the unhealthy fusion of alleged natural science with spiritist mysticism […] will soon have played its part'.[101]

Examples of negative reviews of works propagating outdated philosophies have already been mentioned in this chapter. These were the books grounded in Schopenhauerian idealism, Fichtean idealism, phrenology, vitalism, and, most importantly, *Naturphilosophie*.[102]

Finally, interpreters of Kant received strong criticism. Wundt was not a supporter of the Neo-Kantianism that had gained popularity at Germany universities in the late 19th century. Since he believed that 'the philosophy of a time is a mirror image of the spirit of that time,' he did not believe that a century-old philosophy would be the most promising intellectual starting point to understand modern-day questions.[103] He valued Kant's work, however, both as an important barrier against the high-minded claims of idealism and as a counterweight to the crude positivism of the late 19th century.[104] A strong indebtedness to Kant was therefore on itself not a reason to single someone out for criticism. A misrepresentation of his thought, however, was a sure-fire way to find yourself

[97] Young, Julian, *Friedrich Nietzsche: A Philosophical Biography*, Cambridge University Press, Cambridge, 2010. 238.
[98] Wundt, Wilhelm, 'Rée, Dr. Paul, der Ursprung der moralischen Empfindungen,' *LC*, 18777, 52.
[99] Wundt, Wilhelm, 'Volkelt, Dr. Joh., 'Die Traum-Phantasie,' *LC*, 1876, 31.
[100] Wundt, Wilhelm, '1) Owen, H.D., das streitige Land 2) Psychische Studien,' *LC*, 1877, 21.
[101] Wundt, Wilhelm, Schmidt, Osc., Prof., die naturwissenschaftlichen Grundlagen der Philosophie des Unbewussten,' *LC*, 1877, 30.
[102] See, 101–102.
[103] Wundt, Wilhelm, *Erlebtes und Erkanntes*, 124–125.
[104] Ibid., 56.

critically reviewed by Wundt. An attempt to explain the nature of feeling through Kantian categories of understanding is dismissed in a short review. Even if Wundt acknowledges the author's 'ingenuity and great labour', he states that he has 'to deny that the result corresponds with these efforts of ingenuity and labour'.[105] In his eyes, the proposed relationships between Kantian categories and feeling were highly implausible. Another discussion of Kant was dismissed for a very different reason. Wundt argued that the book was about its author's own interpretations rather than about Kant.[106]

Nöldeke's praise

Even if the criticism that Nöldeke and Wundt levelled against those publications that they did not like provides rather clear outlines of the sort of scholarship they appreciated, their positive commentary merits a closer look as well. An investigation of this yields a more clearly defined picture of what these liberal *Kulturprotestanten* considered to be praiseworthy in scholarly works. A first look at their positive reviews shows that their praise can be divided into two broad categories: praise of the work under review and praise of its author.

Many of Nöldeke's reviews contain rather similar observations. '[These texts] may not be very valuable, poetically, but they are interesting as attempts at Christian epic poetry and they have some importance for grammar and lexicon,' was his comment on a chrestomathy.[107] His review of another book concluded with the following encouraging words: 'May it be granted to him that he will bring his life's work to an end with serene energy, a work that will be of great use for scholarship for a very long time'.[108] De Goeje's edition of al-Mokadassi's geographical work was praised as 'a book that increases our knowledge of the Orient in an excellent way' while an edition of an old dictionary was praised as 'a most rich reference book about the localities found in the ancient Arabic poetry and in a part of the traditional literature'.[109] In these and other reviews certain terms appear over and over again. These include important (*wichtig*), significant (*bedeutend*), rich (*reich*), valuable (*werthvoll*) and useful (*nützlich*). These words suggest a view of the production of new knowledge as adding pieces to a jigsaw puzzle. The more pieces we collect, the better we will be able to see the full picture.

[105] Wundt, Wilhelm, 'Krause, Albr., die Gesetze des menschlichen Herzens,' *LC*, 1877, 14.
[106] Wundt, Wilhelm, 'Kirchmann, J.H. v., Erläuterungen zu Kant's Schriften zur Naturphilosophie,' *LC*, 1878, 24.
[107] Nöldeke, Theodor, 'Zingerle, P. Pius, Chrestomathia Syriaca,' *LC*, 1872, 4.
[108] Nöldeke, Theodor, 'Frensdorff, Dr. S., Prof., die Massora Magna,' *LC*, 1876, 16.
[109] Nöldeke, Theodor, 'Descriptio imperii Moslemici,' *LC*, 1877, 28; 'Das geographische Wörterbuch,' *LC*, 1877, 52.

All new text editions and analyses could be important and useful, from this point of view. These new pieces of scholarly production could also be seen as belonging to a number of different puzzles. An old edition of ancient Arabic poetry would not only add to a more complete picture of a poetic tradition, but it would also contribute to our knowledge of the Arabic language and to the understanding of the culture and history of its era. Because of the many fields to which any new piece of knowledge could contribute, it was not a problem if a text was not relevant to all fields. Bad poetry could still be linguistically valuable and editions of dogmatic theological treatises by minor Church Fathers could still be historically significant.

Another form of praise dealt with the aesthetic qualities of a text. 'We do not only find here a history but also an aesthetic evaluation of the poetic parts [of the Old Testament] with numerous tasteful translations, which are well-suited to give the educated lay person a notion of this literature,' is Nöldeke's opinion on a history of Jewish literature.[110] He also states that the second volume of Geiger's posthumously published writings is 'fresh and warm' and emphasises 'the humane undertone of [Geiger's] being' that characterises all his work.[111] Dozy received similar praise: 'That the book commends itself through its brilliant conception and glowing exposition, goes in Dozy's case without saying'.[112] A good writing style was highly appreciated. Complimentary words like clear (*klar*), attractive (*anziehend*), tasteful (*geschmackvoll*), stimulating (*anregend*), or a more modest very readable (*recht lesbar*) recurred often in Nöldeke's reviews. These compliments, however, were not as weighty as those about significance and usefulness. Nöldeke did not bother to compliment otherwise useless books with engaging writing styles, while badly written books could still be praised for their importance.

A final common type of praise consisted of compliments that referred to an authors's supposed character traits. Nöldeke's review of a catalogue of the Oriental manuscript collection of the University Library in Leiden states that '[...] the work will forever stay a monument to the industriousness, the scholarship and the ingenuity of several generations of Leiden orientalists'.[113] Reviewing a text edition by his former student, Barth, he emphasised that '[...] the editor was very well prepared for such a task, which requires painful diligence, even in details'.[114] Another work was described as 'a worthy monument to the industriousness and the cautiousness of the editor'.[115] The most common words of praise were industriousness (*Fleiss*), diligence (*Sorgsamkeit*) and

[110] Nöldeke, Theodor, 'Cassel, Dr. David, Geschichte der jüdischen Literatur,' *LC*, 1873, 12.
[111] Nöldeke, Theodor, 'Geiger's, Abraham, nachgelassene Schriften,' *LC*, 1876, 7.
[112] Nöldeke, Theodor, 'Dozy, R., Essai sur l'histoire de l'Islamisme,' *LC*, 1879, 13.
[113] Nöldeke, Theodor, 'Catalogus codicum orientalium,' *LC*, 1874, 15.
[114] Nöldeke, Theodor, 'Ta'lab's Kitâb al-Faṣiḥ,' *LC*, 1876, 1.
[115] Nöldeke, Theodor, 'Berliner, Dr. A., die Massorah zum Targum Onkelos,' *LC*, 1877, 10.

cautiousness (*Umsicht*). These could be complemented with terms referring to the intellectual prowess of the author, such as ingenuity (*Scharfsinn*), scholarship (*Gelehrsamkeit*) and the ability and willingness to think critically. A praiseworthy book, then, was written by an author showing both industriousness and intellectual prowess and added another piece of the puzzle to at least one field of knowledge. Only if the book met both of these demands, it could also be praised for its engaging presentation.

Wundt's praise

The first section of this chapter notes that reviews can take different shapes; some are primarily announcements while other have a highly evaluative character. Nöldeke's reviews were highly evaluative, but not exclusively so. Many of his reviews were long and contained some of his own findings, too. These long reviews contrasted with Wundt's usually shorter reviews. A good deal were basically announcements in which he provided a short description of the work under review without explicitly judging its merit. This means that, of the books that he did not dismiss, many did not receive a large amount of explicit praise, either.

He did, however, also give some compliments. One recurring reason for praise concerned the relevance of the reviewed book. A book on microscopic research was praised as follows: 'As a result of its inclusion of many completely new ways of experimenting, it will be a welcome addition for those who own one of the major works on microscopic technology by Frey, Harting, and others'.[116] A study on bone growth also received an honourable mention: 'In the work at hand, Kölliker has extensively outlined one of the most important parts of his research about the growth and development of bones. The presented facts are particularly relevant to [a] recently much discussed question […]'[117] Another book was praised because 'our literature does not yet possess a work that, in a similar way, provides a generally understandable […] exposition of the brain's anatomy'.[118] The continued emphasis on newness suggests another view of the growth of knowledge than that of Nöldeke. Nöldeke's jigsaw model of knowledge production did not emphasise the newness of findings, but the way in which these findings would fit in with existing ideas about scripture, history, or language. Wundt, however, was enthralled by the possibilities of new insights as more than just enrichments of received knowledge. New intellectual developments

[116] Wundt, Wilhelm, 'Exner, Dr. Sigm. Privatdoc., Leitfaden bei der mikroskopischen Untersuchungen thierischer Gewebe,' *LC*, 1873, 26.
[117] Wundt, Wilhelm, 'Kölliker, Alb., Prof., die normale Resorption des Knochengewebes,' *LC*, 1874, 22.
[118] Wundt, Wilhelm, 'Luys, J., Arzt, das Gehirn, sein Bau und seine Verrichtungen,' *LC*, 1878, 1.

and new research methods had the power to refute and replace older ideas.[119] In Wundt's eyes, the most promising scholarship built on such new insights and tools. Contribution to new developments, therefore, was the most sure-fire way to get Wundt's praise.

Like Nöldeke, Wundt also discussed matters of presentation. Some of his praise sounds similar to that by Nöldeke: 'everywhere, the elegant form has been made to fit the brilliant content, which often sparkles with wit and passion'.[120] Most of his praise, however, was reserved for effective and beautiful illustrations. A work on the larynx was praised because 'it was illustrated with numerous and excellent woodcuts in such a way that it can indeed not be difficult even for the anatomically and physiologically uneducated to obtain a rather extensive knowledge of the important organ'.[121] Sometimes Wundt also shared some more general reflections on the usefulness of new methods of illustration. One of these was *Lichtdruck*, a collotype process developed in the late 1890s by Max Gemoser. Collotype was 'the first viable commercial printing process capable of translating the continuous tones of photography into […] printer's ink'.[122] Wundt was very enthusiastic about it, in his following review: 'The attached four plates in collotype have turned out excellently and give a renewed proof of the beautiful enrichment, which the anatomic exposition has gained with the adaption of photography'.[123]

In another review, however, he expressed some doubts about the technique: 'Several of the lithographed plates have been taken from the author's *Anatomie de Gehirnnerven*. The others contain cross-sections of the head in Gemoserian collotype. […] The delicate proportions of the construction of [the brain] can clearly still be reproduced more faithfully through copperplate and even through woodcut […] than by means of collotype'.[124] Wundt's ambiguous attitude towards photography is typical of his time. From the 1870s onwards, collotype was successfully used for the 'illustration of some grosser abnormalities and pathologies' while the depiction of more delicate phenomena 'suffered from an absence of clear spatial and colouristic differentiation'.[125] Even if presentation and illustration obviously mattered to Wundt, a book did not receive a positive review

[119] The theory of evolution is a constantly recurring theme in Wundt's reviews. For example, see Wundt, Wilhelm, 'His, Wilh., unsere Körperform,' *LC*, 1875, 32; 'Lamarck, Jean, 'zoologische Philosophie,' *LC*, 1876, 18; 'Sterne, Carus, Werden und Vergehen,' *LC*, 1876, 31. On the benefits of microscopy, for example, see Wundt, Wilhelm, 'Henle, Dr. J., Prof., Handbuch der Nervenlehre des Menschen,' *LC*, 1872, 39; 'Boll, Dr. Franz, Privatdoc., die Histologie und Histiogenese der nervösen Centralorgane,' *LC*, 1873, 51; 'Strassburger, Dr. Ed. Prof., über Zellbildungen u. Zelltheilung,' *LC*, 1876, 2.
[120] Wundt, Wilhelm, 'Henle, J., anthropologische Vorträge,' *LC*, 1877, 27.
[121] Wundt, Wilhelm, 'Merkel, Dr. C. Ludw., der Kehlkopf,' *LC*, 1873, 32.
[122] Ward, Gerald W.R. (ed.), *The Grove Encyclopedia of Materials and Techniques in Art*, Oxford, Oxford University Press, 2008, 113.
[123] Wundt, Wilhelm, 'Braune, Wilh. Und A. Trüdiger, die Venen der menschlichen Hand,' *LC*, 1874, 19.
[124] Wundt, Wilhelm, 'Rüdinger, Dr. u. Prof., topographisch-chirurgische Anatomie des Menschen,' *LC*, 1874, 37.
[125] Kemp, Martin, 'Medicine in View: Art and Visual Representation,' in: Loudon, Irvine, *Western Medicine: An Illustrated History*', Oxford University Press, Oxford, 1997, 1–22. 6.

on the strengths of its illustrations alone. This mirrors Nöldeke's unwillingness to praise a book based only on its engaging presentation.

Wundt's praise of authors also shows similarities to Nöldeke's. Words like 'industrious' and 'cautious' pop up repeatedly. More striking, however, is his continuous emphasis on methods of data collection. Time and time again, he favourably singles out experiments and observations made by the author himself. An edited volume is described as containing 'important papers' mainly because 'the experiments of these researchers seem to have been conducted with great caution and partly with the use of a very ingenious technique'.[126] One author's experiments are characterised as 'ingeniously thought out and cautiously conducted' and even if Wundt did not agree with his inferences the 'physiological significance' of the study was 'not compromised' by it.[127] In one review, Wundt reflected explicitly on the importance of experiment and personal observation: 'Especially the caution in the study and conclusion prevail [in this work]. The author does not deduce final pictures of the structural coherence from his observations; we can be all the more sure that the trustworthiness of the latter does not suffer from the influence of hypotheses made in advance'.[128]

In Wundt's eyes experiment and personal observation could serve as a counterweight to the bold hypotheses and speculative metaphysics. Furthermore, this emphasis on experiment and observation was not just an impersonal evaluation of the merit of an individual's studies, but a morally charged evaluation of this person as well. As one modern-day scholar stated: 'Lab venues and practices, such as experiment and precise measurement, exemplify moral values of objectivity […]. It is in labs that cultural boundaries — for example, between the realms of nature and of religion and politics — are made visible'.[129] For Wundt, laboratory experiments and exact measurements exemplified a freedom from and renunciation of the mysticism and unwarranted speculation of earlier philosophical traditions.

Community and the language of book reviews

This chapter's introduction claims that book reviews contribute to the shaping of an academic community. The most obvious way in which this occurs is through the exclusion of some scholars by dismissing their work and the welcoming of others by praising their accomplishments. One

[126] Wundt, Wilhelm, 'Arbeiten aus der physiologischen Anstalt zu Leipzig,' *LC*, 1873, 12.
[127] Wundt, Wilhelm, 'Mach, Dr. E., Prof., Grundlinien der Lehre von den Bewegungsempfindungen,' *LC*, 1875, 15.
[128] Wundt, Wilhelm, 'Stieda, Dr. Ludw., Prof., über den Bau des centralen Nervensystems der Amphibien und Reptilien,' *LC*, 1876, 2.
[129] Kohler, Robert E., 'Lab History: Reflections,' *Isis*, 99(4), 2008, 761–768. 765.

modern-day author, however, argued for a detailed linguistic approach to book reviews because they play not only 'an important role in supporting […] the manufacture of knowledge' but also in 'the social cohesiveness of disciplinary communities'.[130] In a similar vein, another modern-day linguist characterised various rhetorical strategies often found in scholarly publications as a means of 'indicating the writer's deference before the scientific community'.[131] This social component of book reviewing 'involves charting a perilous course between critique and collegiality'.[132] The importance of book reviews for the expression of critical independence and loyal collegiality is underlined in another recent paper as well. Its authors argue that the continuous 'calls for a polite realisation of critical remarks' serve the establishment of a 'proper balance between collegiality and critique'.[133] In the book review genre, they add, hedges help 'maintain social harmony and solidarity'.

All of the abovementioned authors underline hedges as important elements of maintaining solidarity. This interest in hedges can be retraced to an influential study on politeness by Penelope Brown and Stephen Levinson in which they argue that hedges 'modify the force of a speech act'.[134] We found that such modifications are indeed often used in book reviews to tone down fierce criticism. This is an example of what Brown and Levinson call 'negative politeness'.[135] Before I turn to a closer investigation of negative politeness, however, I will take a look at what they describe as 'positive politeness'. This consists in satisfying one's audience's desire for recognition 'by communicating that one's own wants (or some of them) are in some respects similar to the addressee's wants'.[136] This can be accomplished in a myriad of ways, such as through markers of a common identity, explicitly pointing out shared commitments, or even joking.

The book review is characterised by its twofold audience; it is directed at both the reviewee and a wider peer group. These two audiences might require a different tone of voice. One modern-day scholar even argued that the relationship between reviewer and reviewee requires only 'little deference, while one researcher must always humble himself or herself before the community as a whole'.[137] Although Nöldeke and Wundt did not put much effort into appearing humble, they involved the wider peer group by explicitly addressing their readership. In one review, Nöldeke, for example, calls upon '[...] all friends of sound interpretation of the OT'.[138] In another, he states

[130] Hyland, *Disciplinary Discourses*, 43.
[131] Myers, 'The pragmatics of politeness,' 18.
[132] Hyland, *Disciplinary Discourses*, 41.
[133] Salager-Meyer et al., 'Collegiality, critique and the construction of scientific argumentation,' 1771.
[134] Brown and Levinson, *Politeness*, 145.
[135] Brown and Levinson, *Politeness*, 129.
[136] Brown and Levinson, *Politeness*, 101.
[137] Myers, 'The pragmatics of politeness', 4.
[138] Nöldeke, Theodor, 'Tuch, Weil. Dr. Fr., Commentar über die Genesis,' *LC*, 1871, 24.

that '[...] there will not be many readers, however, who will be bothered by the mentioned shortcomings'.[139] In a similar fashion, Wundt involves his readers by writing that '[...] nobody will hesitate to acknowledge that a certain advance is possible and desirable in this direction, as well'.[140] In another review, he concludes that he does not have any doubts that 'this minor work will be used with benefit by those readers who have some educational background in the natural sciences […]'.[141]

Though the above turns of phrase are exclusively directed at a wide audience of peers, Nöldeke and Wundt also include expressions aimed at underlining solidarity between themselves, the broader audience and the reviewees. These expressions can usually be characterised as claiming common ground or indicating that they all 'belong to some set of persons who share specific wants, including goals and values'.[142] In one review, Nöldeke stated that the publication of a previously unpublished old poem is 'very desirable'.[143] He also characterises elements of a new study of Semitic church history as important and 'instructive'.[144] Wundt praises a new anatomical compendium in a similar manner: 'Indeed we have always lacked a guidebook of this kind until now'.[145] Another work is praised because its editors 'have acquired a true merit for science' and the book fills up certain recently recognised gaps in physiological knowledge.[146]

Two other forms of positive politeness emphasised by Brown and Livingston are gift giving and joking.[147] The gifts most commonly granted in book reviews are compliments and the recommendation of the reviewed book to potential readers. The wide range of frequently used compliments has already been outlined in the above section. Jokes are rarer, however, although not completely absent. The few humorous expressions tend to be cases of somewhat mean-spirited irony inviting the reader to join the reviewer in making fun of a reviewed work or its author. Nöldeke's mocking remarks about the authenticity of the Pentateuch and the infallibility of the pope fall into this category.[148] One very short review by Wundt shows a similar biting irony:

> The conclusion of this work is contained in the proposition that logic is an *a posteriori* science. If the arguments of the author would have been as substantial as this proposition,

[139] Nöldeke, Theodor, 'Fürst, Jul., Geschichte der biblischen Literatur und des jüdisch-hellenistischen Schriftthums,' *LC*, 1871, 14.
[140] Wundt, Wilhelm, 'Aeby, Chr., der Bau des menschlichen Körpers,' *LC*, 1872, 8.
[141] Wundt, Wilhelm, 'Huxley, Thom. H., Grundzüge der Physiologie,' *LC*, 1872, 33.
[142] Brown and Levinson, *Politeness*, 103.
[143] Nöldeke, Theodor, 'S. Jacobi Sarugensis […] Editus a Josepho Zingerle,' *LC*, 1871, 39.
[144] Nöldeke, Theodor, 'Baudissin, Wolf Wilhelm, Studien zur semitischen Religionsgeschichte,' *LC*, 1879, 12.
[145] Wundt, Wilhelm, 'Henle, J., anatomischer Hand-Atlas,' *LC*, 1872, 5.
[146] Wundt, Wilhelm, 'Jahresbericht über die Fortschritte der Anatomie und Physiologie,' *LC*, 1874, 26.
[147] Brown and Levinson, *Politeness*, 124, 129.
[148] See, this chapter, 100.

> which he calmly expresses, the opus at hand would be an epoch-making publication. But because these arguments are basically limited to the well-known possibility of the geometrical conception of logical relationships, we can suffice with this short note.[149]

Neither Nöldeke nor Wundt, however, were as skilled humorists as Fechner. Under the moniker of Dr Mises, he had earlier published satirical essays in which he had proved, among other things, that the moon was made of iodine and engaged in a comparative anatomy of angels.[150] As such he was eminently suited to poke fun at a poetically framed analysis of the relationship between the soul and the body:

> We would like to ask the author only this one question, […], why, while otherwise […] paying attention to all details in his depiction of the human form and especially the human face, he has overlooked the nose, which, in our opinion, hardly deserves this poetic neglect; a flaw that will by the way easily be mended in a second edition […].The author should just imagine it himself; a face without a nose![151]

In his accompanying letter to Zarncke he claimed that he 'has constructed the review in such a way that […], at best, a light wholesome doubt might arise in him, whether he is being treated ironically'. He added that he would, however, not mind if Zarncke would eventually delete his jokey comments about the noseless face.[152]

In addition to these instances of positive politeness the reviews of Nöldeke and Wundt also contain many instances of negative politeness, which 'performs the function of minimising the particular imposition that [a face threatening act] unavoidably effects'.[153] Since criticism in a book review typically threatens the reputation of the reviewee, examples of negative politeness are very common in this genre. This form of politeness usually consists of different types of hedges that soften the impact of otherwise serious critiques.

One way in which both Nöldeke and Wundt tone down their criticism is by insisting that they are highlighting relatively minor issues found in otherwise important and well-executed works. In one review, Nöldeke, for example, stated he 'only has to deviate from [the author] on a few trivialities'.[154] Wundt, likewise, wrote that he could think of numerous criticism about specific

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[149] Wundt, Wilhelm, 'Widemann, Paul, über die Bedingungen der Uebereinstimmung des discursiven Erkennens mit dem intuitiven,' *LC*, 1877, 10. This is the full review.
[150] Arendt, Hans-Jürgen, 'Gustav Theodor Fechner (1801–1887) und die Leipziger bürgerliche Gesellschaft im 19. Jahrhundert,' *N.T.M. Zeitschrift für Geschichte der Wissenschaften, Technik und Medizin*, 9, 2001, 2–14. 5.
[151] [Fechner, Gustav Theodor], 'Hauschild, Ed. Ferd., Psyche, oder der Becher Giamschid's,' *LC*, 1853, 26.
[152] Gustav Theodor Fechner to Friedrich Zarncke, no date, UBLE: NL 249/1/F/61.
[153] Brown and Levinson, *Politeness*, 129.
[154] Nöldeke, Theodor, 'Smend, Rud., Moses apud prophetas,' *LC*, 1875, 42.

details of a reviewed booklet, but instead concluded that even if he could not agree with the author 'about all of his views, nobody will put down the lucidly and appealingly written little book without feeling very inspired'.[155]

Another way in which both reviewers hedged their criticisms is by pointing out that the reviewees had undertaken a very ambitious task that simply could not have been completed without at least some minor shortcomings. In the opening sentence of an otherwise highly critical review, Nöldeke remarked that the author's research was 'extremely difficult, in part'.[156] In his review of a book that attempts to distinguish certain knowledge from mere opinion in physiology, Wundt admitted that this was especially challenging in 'a science like physiology which finds itself in continuous transformations'.[157] Nevertheless he had no doubt that the book would be useful to those scholars who had some basic knowledge of the latest developments in the field.

A final recurring hedging strategy is the admittance of one's own lack of relevant specialist knowledge. After all, even a harsh review loses some of its sting when it is written by a reviewer who admits to potentially misunderstanding or overlooking elements of the work in question. Wundt rarely reverts to this strategy, but Nöldeke repeatedly confesses gaps in his knowledge. In his review of a book on Christian Syriac texts, he admits that he is 'alas, not well-versed […] in liturgical issues' which means that he 'has to declare himself incompetent to judge exactly those excerpts, to which the author attaches the most importance'.[158] Commenting on a text edition based on Coptic sources, he likewise admits that the 'final judgement of their critical value' should be left to 'the experts of the Coptic language'.[159]

All in all, Nöldeke and Wundt use a wide range of the positive and negative politeness strategies that Brown and Levinson and others have recognised. Their continuous performances of politeness in book reviews contributes to the shaping of scholarly communities in at least two ways. The negative politeness expressed through hedging allows reviewers to welcome reviewees into the peer group, while preserving the opportunity to be critical of their work. The expressions of positive politeness are generally expressed through an emphasis of shared commitments among reviewer, reviewee and readership, and therefore contribute to the shaping of a sense of common purpose and values among the peer group as a whole.

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[155] Wundt, Wilhelm, 'Carneri, B., Gefühl, Bewußtsein, Wille. Eine psychologische Studie,' *LC*, 1877, 9.
[156] Nöldeke, Theodor, 'Janichs, Georg, animadversiones criticae in versionem Syriacam Peschitthonianam,' *LC*, 1871, 49.
[157] Wundt, Wilhelm, 'Huxley, Thom. H., Grundzüge der Physiologie,' *LC*, 1872, 33.
[158] Nöldeke, Theodor, 'Bickell, Gustavus, conspectus rei Syrorum,' *LC*, 1871, 30.
[159] Nöldeke, Theodor, 'Psalterium. Job. Proverbia arabice. Paulus de Lagarde edidit.,' *LC*, 1879, 2.

What do book reviews do?

This chapter starts with some remarks on the genre of the book review, drawing attention to the two main functions of reviews acknowledged in modern-day literature: the announcement and the evaluation. A closer look at the reviews by Nöldeke and Wundt shows another type of non-evaluative review, as well: the review as a minor contribution to scholarship, providing lists of comments that could be of use to the author whose work was being reviewed. Even if reviews as announcements and those as addition to the shared body of knowledge were not uncommon, most reviews did contain evaluative content. Furthermore, these evaluations show certain patterns that teach us something about the demands and expectations of a 19th-century member of the German academic world.

The scholarly values emanating from Nöldeke's and Wundt's reviews can be compared to Fritz Ringer's famous description of the late 19th-century academic self-image. Ringer observed a general agreement among late 19th-century intellectuals that 'the modern German idea of the university and of learning was irrevocably tied to its intellectual origins in German Idealism and neohumanism' and argues that '[...] the decades around 1800 came to seem a period of primitive purity'.[160] The ideals of *Kulturprotestantismus* seem to partially fit Ringers description. The ideal of the educated man as an 'autonomous personality' can be traced back to the tenets of idealism and the classical *Bildung* of the German gymnasiums is indebted to the neo-humanism of the late 1700s. This emphasis on autonomy and individuality contributed to a religiousness that went hand in hand with a strong interest in science and scholarship as well as 'a laicist persuasion'.[161]

Nöldeke and Wundt expressed this persuasion through a critical attitude towards religious dogmatism and speculative philosophies. Even if they did not conceive of such speculation and dogmatism as particularly threatening to linguistics or medicine, they considered these to be very real dangers to the study of theology and philosophy. Nöldeke repeatedly puts orthodox Protestant, Catholic and Jewish authors back in their place. Wundt argues, over and over, against the influence of *Naturphilosophie*, idealism and other intellectual frameworks that he considered to be obsolete. Such reviews not only evaluate the work in question, they also emphasise ideals of good scholarship characterised by a healthy distrust of dogma and speculation. Noteworthy is also that national or religious affiliations have only a limited predictive value for the severity of the reviews.

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[160] Ringer, Fritz K., *The Decline of the German Mandarins: The German Academic Community, 1890–1933*, Harvard University Press, Cambridge (MA), 1969. 103–104.
[161] Hübinger, Gangolf, *Kulturprotestantismus und Politik: Zum Verhältniss von Liberalismus und Protestantismus im wilhelmischen Deutschland*, Mohr, Tübingen, 1994. 15.

A second defence of autonomous scholarship and a detached attitude towards dogma and mysticism is illustrated by the reasons Nöldeke and Wundt find to praise authors. Religiously informed certitudes are to be replaced by caution, industriousness, diligence and ingenuity. Important as these virtues are for all scholars, they gain even more significance when they are applied to a laboratory setting. The ingeniously conducted experiments, careful measurements and diligent series of personal observations are the perfect means to draw a line between the realm of scholarship and the realm of religion and superstitious speculation. Only studies undertaken with diligence, caution and ingenuity can add our understanding of the world, either by filling in gaps in existing knowledge or by opening up whole new fields of investigation. It might not come as a surprise that especially this type of study was characterised as 'significant', important, valuable, or useful.

To some extent this guarding of the distinction between scholarship and speculation takes the shape of emphasising the distinction between academic insiders and outsiders. Fields like theology and philosophy attracted more non-academic authors than linguistics and medicine. Though university professors are criticised for dogmatism and mysticism, a strikingly large share of the authors receiving this reproach were not affiliated with a university. The religiously and mystically inclined academics were to some extent able to compensate for these views by drawing on their other academic skills. A Catholic bias was not too much of a problem if it resulted in a skilfully edited text edition of a Church Father who happened to be highly valued by the Catholic church. A speculative work of philosophy could be valuable if it was based on rigorously conducted experiments and exact measurements. However, since most academic outsiders lacked either the skills or resources to do such things, Nöldeke's and Wundt's reviews — deliberately or indeliberately — drew a line between academic insiders and outsiders.

If, and only if, the scholar and the work under review would live up to the requirements of caution, ingenuity, diligence and industriousness, their work could be considered for further praise. This praise had to do with the lucidity and clarity of the exposition. In Nöldeke's reviews, lucidity and clarity were accomplished through a well-developed writing style. In Wundt's reviews, especially those of medical works, the quality of the illustrations was at least as important. Even if good illustrations were not sufficient reason for a positive review of a study, they were wholeheartedly acknowledged as contributing to its value and usefulness.

As the above reasons for praise illustrate, book reviews not only provided a medium through which reviewers could assert their own independence by subjecting their peers to critical evaluation, but also contributed to the creation and perpetuation of a community of scholars. Because nobody

could possibly read all new literature that was published in his discipline, the more easily accessible book review ensured a shared awareness of new research. The community was further strengthened by the language used in book reviews. By directly addressing a broad scholarly audience, by complimenting deserving authors and by hedging their criticism of less convincing scholarship reviewers acknowledged and strengthened scholars' self-image as, for example, scientific theologians, meticulous philologists, modern medical men, or critical and level-headed philosophers.

Nöldeke and Wundt contributed to an understanding of scholarship as a cautious and industrious endeavour that was primarily produced in institutions of higher learning by independently thinking men. As the other chapters illustrate, this notion of independence in academia could be quite ambiguous. However, Wundt, Nöldeke and their associates could agree that independence defined as freedom from religious dogma and speculative mysticism should be the starting point of every scholarly effort.

4. State and Scholarship

Recommendations and Appointments under the 'Althoff System'

The Althoff system

Reflection on the virtue of scholars and their work can take place in many different ways. The preceding chapters pay attention to informal evaluation and the role of scholarly journals. This chapter takes a closer look at yet another element of scholarly life shaped by continuous evaluation of each other's merit: professorial appointments. On the one hand, discussions about appointments invited sharp criticism of all candidates. An explicit exposé of a person's shortcomings could be enough to slow down someone's career. At the same time, however, letters of recommendation allowed for a display of loyal collegiality towards one's closest peers in a way that could actually benefit their careers. Because a professorial appointment is more substantial than a book review, criticism and praise might even be more consequential in letters of recommendation than in book reviews.

In 19th-century imperial Germany, the decision about appointments was made by the governments of its constituent states. Professorial appointments at Leipzig University had to be approved by the Saxon government in Dresden, those in Heidelberg by the Baden authorities in Karlsruhe, and those at Prussian universities by the Ministry of Education in Berlin. The decision-making process in Berlin is especially interesting as a subject for research for two reasons. One reason is that the Prussian relationship between state and university provided the blueprint for those in the whole of imperial Germany.[1] The other reason is that, for a quarter of a century, this process was largely controlled by one man, whose correspondence has been very well preserved. This correspondence allows us a close look into Prussian appointment policies during an extensive period.

The one man in charge of appointments in Prussia was Friedrich Theodor Althoff. His official position in the ministerial hierarchy was modest in comparison to his actual influence. Between 1882 and 1897 he was one of the 33 *vortragende Räte* (executive officials) and between 1897 and 1907 he was one of the four *Ministerialdirektoren* (ministerial directors) at the Ministry of Education.[2] His

[1] Brocke, Bernhard vom, 'Friedrich Althoff: A Great Figure in Higher Education Policy in Germany,' *Minerva*, 29(3), 1991, 269-293. 280.

[2] Schilfert, Sabine, 'Friedrich Althoff – ein preußischer Geheimrat von Format? Bemerkungen zu einem wissenschafts-historischen Kolloquium anläßlich seines 150. Geburtstag,' *Zentralblatt für das Bibliothekswesen*, 103, 1989, 546–552. 546. I will refer to the Prussian '*Ministerium der geistlichen, Unterrichts- und Medizinalangelegenheiten*' with the shorter term 'Ministry of Education', which seems appropriate in the context of dealing with university appointments.

far-reaching influence, however, is clearly illustrated by the way he was described in the contemporary news media. He was referred to as 'the almighty ruler of the Prussian universities', 'the secret Prussian minister of education' and 'the Bismarck of the university system'.[3] It has become common to refer to his legacy as the Althoff system. The word 'system', however, says more about the perceived inescapability and decisiveness of his influence than about the coherence of his vision. One leading modern-day scholar argues that above all he 'was a practical man, not at all doctrinaire' and emphasises that 'the 'Althoff system' slowly emerged in the course of decades of educational policy-making'.[4]

Although Althoff was not at all doctrinaire, his legacy shows a number of common threads. One salient feature of his policies was his unwillingness to discriminate against minorities.[5] He proudly claimed never to have 'participated in any rabble-rousing propaganda, not against Catholics and not against Jews'.[6] A second characteristic was an emphasis on research institutes that functioned outside of the faculty structure of the universities.[7] His support of the establishment of Koch's Institute for Infectious Diseases is a typical example of this. Another feature of Althoff's rule was his ability to find new ways to fund research. The budget of his ministry was insufficient to cover the costs of all new research institutes, but Althoff successfully forged bonds between academia and private and corporate investors.[8] Finally, if we insist on describing his legacy as a system, it is best characterised as a system for gathering information.[9] He made sure that he had at least one informant at every Prussian faculty to keep him updated him about the accomplishments, ambitions and character traits of scholars. This allowed him to make well-informed decisions about professorial appointments and to influence decision-making processes at faculty meetings.

Althoff's decision-making process was not popular among all his contemporaries. Most complaints were a response to his inclination to sidestep the existing power structures manifested in the universities' faculty structure. The professorial appointment procedure was traditionally a collaborative effort of the faculties and the state government. The faculties usually sent a proposal to the ministry in which they would express their preference for three ranked candidates. Though the ministry was not obliged to follow this proposal, it would often appoint one of the preferred

[3] Ibid., 446; Brocke, 'Friedrich Althoff,' 289.
[4] Brocke, 'Friedrich Althoff,' 278.
[5] Pawliczek, 'Kontinuität des informellen Konsens', 79.
[6] Brocke, 'Friedrich Althoff,' 278.
[7] Schilfert, 'Friedrich Althoff – ein preußischer Geheimrat von Format?,' 546.
[8] Peirce, William.S. and Peter. Krüger, 'Entrepreneurship in a Bureaucracy: The Case of Friedrich Althoff,' *Journal of Economic Studies*, 20(4,5), 1993, 52–70. 68.
[9] Zott, Regine, 'Hochgeehrter Herr Geheimer Rath! … Friedrich Theodor Althoffs Beziehungen zur Chemie,' *Chemie in unserer Zeit*, 42(5), 2008, 322–328. 323.

candidates.[10] In the eyes of his critics, Althoff's policy was not in line with this traditional practice. Both his emphasis on the establishment of new independent research institutes and his tapping into new sources of funding threatened the traditional powers of the faculty boards. In addition, his welcoming attitude towards Catholics and Jews was not always appreciated by the faculties, whose members often held more conservative views.[11] Looking back at Althoff's hold on German university life, the philosopher Max Dessoir even suggested a relationship between his rule and the powerlessness of German academics towards the Nazis, a quarter of a century after his death: 'We forgot how to speak and to act like free men; even after Althoff's death, we continued to live in 'fear of the Lord' and readily changed to another line of policy when National Socialism began to 'coordinate' us'.[12]

Althoff was sometimes criticised during and immediately after his years at the ministry, but hardly any traces of criticism of his informants have survived. One author, however, argued that critics viewed these *Vertrauensleute* 'with great suspicion, and some thought of them as unscrupulous informants, practically akin to academic spies'.[13] Although this observation is hardly surprising, my sources will not allow me to elaborate on this issue. The correspondence between Althoff and his informants contains some reports by scholars failing to obtain the desired consensus, but lack reflection on the informants' power and status among their peers.

It should also be noted that Althoff's critics may have painted too unfavourable a picture of him. An analysis of the number of forced appointments shows that there were relatively few during his years of tenure.[14] Since he cultivated warm relationships with many influential scholars, he was often able to shape the opinions within the faculty without resorting to his administrative powers. Rather than forcing his critics into accepting his views, he outwitted them.[15] One case study shows that his power was not unlimited. The faculty of philosophy of Greifswald resisted his pressure and successfully advocated their own candidate for the Chair of Classical Philology, the young classicist Eduard Norden. Because its members were able to present themselves as a united front, the faculty was able to appoint this talented young scholar instead of a mediocre older man.[16] Finally it should be emphasised that Althoff's preferences were based on a very extensive exchange of ideas with a large number of well-respected scholars. He was therefore usually well able to support

[10] Paulsen, Friedrich, *Die deutschen Universitäten und das Universitätsstudium*, A. Asher & Co., Berlin, 1902. 101.
[11] Backhaus, Jürgen G., 'The University as an Economic Institution: The Political Economy of the Althoff System,' *Journal of Economic Studies*, 20(4,5), 1993, 8–29. 13.
[12] Quoted in: Brocke, 'Friedrich Althoff,' 270.
[13] Rowe, David E., *A Richer Picture of Mathematics: The Göttingen Tradition and Beyond*, Springer, Cham, 2018. 12.
[14] Backhaus, 'The University as an Economic Institution,' 13.
[15] Brocke, 'Friedrich Althoff,' 284.
[16] Leppin, Hartmut, 'Eduard Nordens Berufung nach Greifswald: Handlungsspielräume im „System Althoff",' *Philologus*, 142(1), 1998, 162–172. 167–169.

his decisions with both good arguments and the back-up of well-respected members of the academic community.

Since I am more interested in the assessments that scholars make in their capacity as authors of letters of recommendation than in actual governmental decision-making practices this is not the place to look into the exact relationship between these letters and eventual appointments. Unlike most literature about Althoff, this chapter will rather deal with the way in which scholars used this correspondence to confidentially evaluate their peers with full knowledge of the fact that their evaluation could decisively shape their careers. The first section of this chapter illustrates the in-depth level of knowledge Althoff acquired, not only of the scholarly merit of the people he appointed but especially of their personalities and the character of their cooperation, by looking at his intimate knowledge of the events and relationships at the Institute for Oriental Languages. The following sections present case studies of how scholars tried to use their connection to Althoff to influence appointments to medical and philosophical professorial chairs. These case studies further illustrate the work performance and character traits praised and condemned by the 19th-century German professoriate. In addition, they give an overview of the way in which letters of recommendation gave scholars the opportunity to loyally support some of their closest peers, as well as provide a chance to critically distance themselves from colleagues with whom they shared less.

Althoff's intimate knowledge

The Institute for Oriental Languages was established in 1887, shortly after the young sinologist Wilhelm Grube had informed the Ministry of Education about the lack of knowledge of the local language at the German embassy in China. He pointed out that in France and the United Kingdom institutions had been founded to educate future diplomats and businessmen in Asian vernaculars, but that Germany lacked such institutions.[17] Grube suggested that an institute should be established on the model of the French *École des langues orientales vivant*, where European professors were supported by native speakers to teach Asian languages. Only a few days after the minister of education, Gustav von Goßler, had brought this idea to Bismarck's attention, he received the latter's fiat.[18] Althoff was entrusted with the preparations and two years later the Institute for Oriental Languages opened its doors under the direction of Eduard Sachau, a former student of Nöldeke and Professor of Arabic at the University of Berlin. Even if the short time it took to

[17] Gustav von Goßler to Reichskanzler von Bismarck, 8 April 1885, GStA PK, VI. HA, Nl Althoff, No. 156.
[18] Reichskanzler von Bismarck to Gustav von Goßler, 11 April 1885, GStA PK, VI. HA, Nl Althoff, No. 156.

establish the institute suggests a smooth course of events, relationships between the Institute's staff members were strained. Very few people knew more about these simmering tensions than Friedrich Althoff.

Althoff extensively corresponded with several Institute staff members, such as Sachau and the Arabist Martin Hartmann. One of his most important confidants was Professor of Chinese Language Carl Arendt. In 1889, Althoff asked Arendt for a detailed confidential report about working atmosphere at the Institute. If we are to believe the writer of one of Arendt's obituaries, Althoff had picked the wrong person. He argued that one of his most praiseworthy qualities was the fact that he had 'absolutely no talent for gossip'.[19] But, even though Arendt repeatedly emphasised his strong dislike of discussing his colleagues in such a frank way –calling it a 'painful and probably questionable assignment' — his depiction of the relationships at the Institute was almost juicy and certainly worrying.[20] Though he praised some of his colleagues, such as the Chinese lectors working under his direction, many staff members were harshly criticised. And, although he repeatedly claimed to value his great working relationship with the director of the Institute, he saved some of his strongest criticism for Eduard Sachau.

Arendt's main reproach of his leadership was the distance he maintained between himself and the teaching staff. He was seldom present at the Institute and almost all communication between him and the staff was through written missives. His office hour at the Institute was scheduled at a time when none of the teaching staff had good reason to be there and requests for meetings were usually turned down with a short note. The fact that staff and director were more or less strangers to each other led to a neglect of day-to-day classroom experiences in Sachau's planning. People who complained about this state of affairs were rebuffed; their comments were interpreted as 'revolt against his authority' and 'impermissible criticism of his practices'. To add insult to injury Sachau did not treat all member of the teaching staff equally. A request for a translation was very politely worded to most professors, but one of them received the same request 'in the shape of a decree bordering between business-like and almost rude'. Especially the Arabist Martin Hartmann and the Persianist Friedrich Rosen suffered from Sachau's antics. His dislike of them was so obvious that even their students picked up on it.

Arendt also criticised Sachau's confidants, Arabic lecturer Bernhard Moritz and institute attendant (*Seminardiener*) Heyde. If Sachau decided not to announce his new ordinances in writing, they were

[19] Foy, Karl, 'Zur Persönlichkeit Carl Arendt's: Einige Erinnerungen,' *Ostasiatische Studien. Mittheilungen des Seminars für Orientalische Sprachen zu Berlin*, 1, 1902, 177–182. 181.
[20] Professor Arendt über die Zustände am Seminar für Orientalische Sprachen. 4.VII 1889, GStA PK, VI. HA, Nl Althoff, No. 156.

usually passed on by these men. Arendt claimed not to care about Moritz's derogative remarks on his teaching and his unfriendliness that went 'as far as the neglect of the most common forms of courtesy'. He argued, however, that the general feeling of uneasiness at the Institute was largely to blame on 'the behaviour of Dr Moritz, who behaves towards us as the mouthpiece of the director and occasionally even as our superior'. The institute attendant also appeared to have a closer relationship with Sachau than the teaching staff. The eventual discharge of Friedrich Rosen and Carl Friedrich Andreas, the Professor of Turkish and Persian Languages, was first made public by Heyde. Through his indiscretion students were also able to figure out the pecking order at the Institute. Finally, he would occasionally be rude towards the professors. When one of them did something Sachau would not have approved of, Heyde was reported to have answered with an ominous: 'It is getting windy'.[21] With all these personal frictions Arendt concluded that the Institute had an 'unpleasant general mood'.

Althoff did not exclusively rely on Arendt's extensive exposition and corresponded with other members of the Institute as well. In the subsequent years the picture of the poisoned atmosphere at the Institute grew more detailed. Hartmann defended Heyde.[22] He argued that the mistakes he had made were caused by the inappropriately confidential attitude towards him taken by Sachau and Moritz. When Heyde had tried to refuse some orders that he had deemed improper, his superiors had rebuffed him and told him that 'he was cowardly' and that 'he was too good'. Hartmann therefore stated that 'the improprieties and ineptitudes of attendant Heyde can be fully traced back to improprieties and ineptitudes of director Sachau'. Even worse incriminations of Sachau and Moritz reached Althoff's office soon. The Prussian consul in Damascus, Johann Gottfried Wetzstein and Carl Friedrich Andreas accused Sachau of plagiarism.[23] A former co-student of Moritz at the Berlin Faculty of Theology accused him — without being able to provide proof — of having stolen his lecture notes of an introductory course to the Old Testament, which was why he had been forced to make his career in New Testament studies instead.[24] Althoff carefully filed these incriminations.

Sachau's correspondence with Althoff confirms the partisan character of his management. He advised Althoff to keep Rosen at a distance because 'he deserves no special entitlement to your time'.[25] One year later he stated that because of his 'character and current state of mind' Rosen was

[21] 'Es wird windig". Ibid.
[22] Martin Hartmann to 'Sehr geehrter Herr Kreisschulinspektor', 26 September 1889, GStA PK, VI. HA, Nl Althoff, No. 156.
[23] Georg Humbert to Friedrich Althoff, 20 January 1890 and 16 February 1890, GStA PK, VI. HA, Nl Althoff, No. 156.
[24] Ernst Kühl to Friedrich Althoff, 16 October 1890, GStA PK, VI. HA, Nl Althoff, No. 156.
[25] Eduard Sachau to Friedrich Althoff, 22 February 1888, GStA PK, VI. HA, Nl Althoff, No. 912.

not qualified to be an examiner and asked Althoff to dismiss him before the end of the month.[26] He also accused Hartmann of being the only staff member who did not show up to attend a holiday course; a failure that was all the more serious because his status as one of the best-paid staff members came with the responsibility of being a role model to others.[27] He also spoke up for Moritz, who seemed to be 'somewhat sullen and dispirited because of the hostilities of Hartmann and Arendt'.[28] A year later he again put in a good word for Moritz, emphasising that 'his position towards the unpeaceful elements of the Institute is very difficult and unpleasant'.[29] In the same letter, he also underlined that Moritz was not the only one to suffer from his hostile colleagues; the lector Amin Maarbes was also 'pursued with secret and public hostility by Hartmann'. Another year later, he even asked if Althoff could put Hartmann on administrative leave, so that Moritz could take over his course.[30]

It is not traceable what use Althoff made of the information he received about the job performance, character and relationships of the Institute staff. The careers of the infighting scholars don't show a clear preference for the representatives of either side. Sachau was never relieved of his responsibilities; Arendt would teach at the Institute until his death in 1902; Hartmann stayed until his death in 1918; Rosen continued his career at the diplomatic service; Andreas was hired in by the university in Göttingen after spending two decades as a freelance language teacher in Berlin; Moritz finally left the Institute for a job at the Khedivial library in Cairo in 1896, to return fifteen years later as the head of the Institute's library.

This short look into the infighting at the Institute for Oriental Languages shows two things. First, it shows the way and the degree to which Althoff was able to stay in touch with strongly opinionated opposing parties. Second, it shows the information he was able to get out of these contacts. This did not only, or even primarily, concern the professional and scholarly merit of the appointees, but included detailed accounts of personal relationships, working atmosphere, character appraisals and even vicious gossip.

A medical mess in Breslau

In some respects, the Institute for Oriental Languages was a special case in the German academic landscape. Though it was affiliated with the university of Berlin and professorships were awarded

[26] Eduard Sachau to Friedrich Althoff, 16 July 1889, GStA PK, VI. HA, Nl Althoff, No. 912.
[27] Eduard Sachau to Friedrich Althoff, 18 September 1888, GStA PK, VI. HA, Nl Althoff, No. 912..
[28] Eduard Sachau to Friedrich Althoff, 9 October 1890, GStA PK, VI. HA, Nl Althoff, No. 156.
[29] Eduard Sachau to Friedrich Althoff, 16 July 1889, GStA PK, VI. HA, Nl Althoff, No. 912.
[30] Eduard Sachau to Friedrich Althoff, 4 January 1890, GStA PK, VI. HA, Nl Althoff, No. 912.

to its most prominent teachers, it was situated outside of the university's faculty structure and did not have a strong interest in research. The evaluations of most scholars discussed in Althoff's correspondence therefore differ from those described above. Academics were usually judged on a broader range of qualities. In addition, most discussions were influenced by faculty boards who had the power to shape discussions about hiring decisions by drawing up ranked lists of candidates. The following sections illustrate the complexity of these discussions by taking a close look at Althoff's correspondence with some of his most trusted informants at medical faculties: the Breslau gynaecologist Heinrich Fritsch and the Marburg physiologist Eduard Külz.

The correspondence with Fritsch is of interest because it emphasises the extent to which discussions about appointments could be informed by the state of affairs at individual universities at least as much as by the merits of individual scholars. Fritsch was a full Professor of Obstetrics, the leader of the Breslau obstetrical clinic and Althoff's unofficial representative at the medical faculty in Breslau. He was also the administrative director of this faculty. This accumulation of tasks during a troublesome time for the Breslau medical faculty continuously threatened to overburden him as illustrated by his complaints to Althoff: 'I can basically give up on this year. If you have to argue every day about shirts, washing machines, meat deliveries, bickering by officials, etc., then where is there room for scholarship? […] But I am not going to spend one word on it, because I hope that I will not be staying in Breslau for very much longer'.[31] If we are to believe Fritsch's report of the state of affairs in Breslau his desire to leave was very understandable indeed.

The most urgent problem was the lack of patients in the university clinics. This was the legacy of an earlier time, when Anton Biermer still managed both the medical clinic and the polyclinic. His rude treatment of patients discouraged them to visit these clinics. Biermer, however, was content to work with the patients admitted to the *Allerheiligen* hospital instead.[32] In the final years of his tenure, Biermer fell ill and his clinical responsibilities were taken over by Otto Soltmann, who replaced him at the medical clinic, and Friedrich Müller, who took his place at the polyclinic.[33] During this period, the two clinics grew apart. Soltmann, a man of independent means, hardly put any effort into managing the medical clinic; he enjoyed a copious life style without bothering too much about teaching, research and managerial tasks.[34] Müller, on the other hand, proved to be an effective director and inadvertently lured patients away from Soltmann's to his own clinic.[35] In

[31] Heinrich Fritsch to Friedrich Althoff, 2 March 1891, GStA PK, VI. HA, Nl Althoff, No. 112.
[32] Heinrich Fritsch to Friedrich Althoff, 22 June 1892, GStA PK, VI. HA, Nl Althoff, No. 112.
[33] [Rector und Senat of the University of Breslau], *Chronik der Königlichen Universität zu Breslau für das Jahr vom 1. April 1890 bis zum 31. März*, Breslau, 1891. 44.
[34] Heinrich Fritsch to Friedrich Althoff, 9 August 1893, GStA PK, VI. HA, Nl Althoff, No. 112
[35] Heinrich Fritsch to Friedrich Althoff, 1 May 1892, GStA PK, VI. HA, Nl Althoff, No. 112.

1892, after Müller had been appointed in Marburg, a successor of Biermer was finally appointed. From this moment on Alfred Kast would manage both clinics. To provide patients for Kast, Fritsch made an agreement with the Breslau municipal authorities to transfer some of the poorer patients in the municipal facilities to the university clinics: 'Without this, we embarrass ourselves terribly with the beautiful medical clinic — without patients. We are not saving any money, because every patient is now hospitalised for free, so that we at least have *something*'.[36]

Another problem was a lack of students. Fritsch anxiously compared the number of students in his obstetrical clinic with the numbers at other German universities. The only clinic with a lower attendance was located in Giessen, where only 35 students showed up. Breslau's 50 students compared poorly to the 88 attendants in Kiel, the 140 in Halle and the 150 who showed up at the Würzburg clinic.[37] Fritsch argued that the declining number of students was due to the teaching and examination methods by the anatomist Carl Hasse. He called him 'a good, consistent and honest man,' who is 'full of diligence and conscientiousness'. His teaching, however, mostly dealt with animal rather than human anatomy and he was an unnecessarily harsh examiner. The students even summarised his major shortcomings in a song:

Bummellied	Strolling song
[…]	[…]
Schon 14 Tage vor Beginn	Already 14 days before the opening
Begann er sein College,	He started his lecture series
Da schimpfte er, als ich mich einst	There he ranted when I
Verspätet auf dem Wege.	Was once delayed on my way.
Von Fröschen du Batrachiern war	Almost always the only things discussed
Fast immer nur die Rede.	were frogs and batrachians.
Am Ende kam er etwas dann	In the end he would then come
Zum Menschen – ziemlich späte	To people – rather late.
[…]	[…]
Nie nannte er uns Herr und Sie	He never called us gentlemen
Nur Er und Ihr und Leute,	Only he and you and people
Im Mittelalter ging sowas,	That was fine in the Middle Ages

[36] Heinrich Fritsch to Friedrich Althoff, 22 June 1892, GStA PK, VI. HA, Nl Althoff, No. 112. Fritsch's emphasis.
[37] Heinrich Fritsch to Friedrich Althoff, 25 December 1892, GStA PK, VI. HA, Nl Althoff, No. 112.

Doch warlich nicht mehr heute.	But really no longer today
Da müsst‘ ich doch ein Esel sein	I would really be a jackass
Ein Kerl als wie ein Rinde	A guy like a cow
Wenn ich nicht schleunig fahren sollt‘	If I would not immediately go
Hinweg mit gutem Winde[38]	Away with a good wind

The song unambiguously pointed out how Hasse’s teaching and manners hurt the Breslau student population. Many of Breslau students decided to take their exams elsewhere. Especially Leipzig was a popular destination; many of those who could afford it, attended lectures there, rather than in Breslau. This left the Breslau faculty with fewer and poorer students: ‘*There is no doubt that the attendance here will decline quickly*. The sons of *respectable* families already say: we cannot expose ourselves to Hasse’s treatment and go away. After that, only the rubbish stays with us!’[39]

Fritsch’s recommendations were based on the poor state of affairs at the medical faculty in Breslau rather than on any individual’s scholarly merit. A first example is his proposal that no replacement for Friedrich Müller should be appointed. This could only lead to an unhealthy competition for patients with Kast, who was already struggling to attract enough patients to keep both clinics running.[40] Since this was an easy request to accommodate, no successor of Müller was appointed indeed. Fritsch’s recommendations for his own succession provide another example of considerations primarily based on the challenging Breslau environment. He recommended the hiring of Otto Küstner because he was ‘a prosperous, almost rich man’ and explained that ‘an independent man is necessary here. If you appoint a praxis-hungry professor from a small university, nothing will change, which is so disastrous, especially for Breslau’.[41] The challenges of leading an obstetrical clinic in Breslau were such that they demanded the full-time attendance of its director. A director who would feel the financial need to also hold private practice — something not uncommon among the staff of medical faculties — would not be able to deal with these demands. A further advantage of appointing a wealthy man would be that he would be likely to agree to start his work before he would actually get paid and that he would also be likely to waive his right to receive a travel and moving reimbursement. Less than half a year later Althoff would announce that he had indeed appointed Küstner.[42]

[38] Note captioned ‘von Fritsch erhalten. A 27/11 93. Zu Hasse,’ GStA PK, VI. HA, Nl Althoff, No. 112.
[39] Heinrich Fritsch to Friedrich Althoff, 25 December 1892, GStA PK, VI. HA, Nl Althoff, No. 112. Fritsch’s emphases.
[40] Heinrich Fritsch to Friedrich Althoff, 22 June 1892, GStA PK, VI. HA, Nl Althoff, No. 112.
[41] Heinrich Fritsch to Friedrich Althoff, 10 May 1893, GStA PK, VI. HA, Nl Althoff, No. 112.
[42] Friedrich Althoff to Heinrich Fritsch, 4 August 1893, GStA PK, VI. HA, Nl Althoff, No. 112

Fritsch's correspondence with Althoff reveals at least two things. In the first place, it shows that Althoff not only was kept well-informed about the scholarly accomplishments and character of a large number of scholars, but that he was also kept up to date on the state of affairs at universities, faculties and other constituent parts of the academic system. Secondly, it illustrates how he allowed discussions and decisions about appointments to be informed on these states of affairs as much as by evaluations of the merits of individual scholars. Even if there may have been scholars with all the required qualities to succeed Müller, the vulnerable state of the Breslau clinics convinced him to decide against such an appointment. And even if other scholars might have been as qualified or even more qualified than Küstner to succeed Fritsch, the appeal for a financially independent director of the ever-vulnerable obstetrical clinic was answered, as well.

A Saxon paediatrician

The above example of Althoff's correspondence with Fritsch shows an obvious willingness to follow the latter's advice. The decision to follow this advice was made easier by the fact that other confidants in Althoff's network supported it as well; Hermann Kuhnt and Berhnhard Schultze confirmed Fritsch's praise for Küstner.[43] It was not uncommon, however, for Althoff to receive contradictory evaluations. This was the case when he was looking for a successor for the Berlin paediatrician Eduard Henoch. One of the principal candidates was Otto Heubner, who we have encountered earlier as Behring's collaborator in testing early versions of his diphtheria serum.[44] A number of scholars had praised Heubner's work in Leipzig, but Eduard Külz — a 'medical authority' once described as one of Althoff's 'spies' or 'lackeys' — was one of the people who doubted Heubner's eligibility.[45]

When Althoff initially asked Fritsch to comment on Henoch's succession, Heubner was not even mentioned. Fritsch only dedicated a few short sentences to suitable candidates and used most of his letter to argue that Soltmann was unfit for the position.[46] Fritsch not only referred to Soltmann's presumed unwillingness to abandon his copious life style, he also pictured him as an antisemitic hypocrite. On Althoff's question whether Soltmann might be Jewish, he answered: 'Soltmann is not Jewish, whether his father was Jewish, I do not know. However that may be, [Soltmann] looks very Jewish. This makes his anti-Semitism all the more ridiculous. He owns a house in the most

[43] For example, see Hermann Kuhnt to Friedrich Althoff, 10 July 1893 and Bernhard Schultze to Friedrich Althoff, 11 July 1893, GStA PK, VI. HA, Nl Althoff, No. 112.
[44] See Chapter 1, 46–48.
[45] Brocke, 'Friedrich Althoff,' 283.
[46] Heinrich Fritsch to Friedrich Althoff, 9 August 1893, GStA PK, VI. HA, Nl Althoff, No. 112.

expensive neighbourhood of Breslau. Until today, he leaves two floors empty for over 1000 Thaler, because he does not want Jews in the house'.

Hugo Falkenheim, clinician and *Privatdozent* of paediatrics in Königsberg commented more extensively on Henoch's succession. He stated that Heubner was an 'outstandingly diligent and solid acknowledged scholar whose works […] brought significant expansion of our knowledge'.[47] Heubner would be a prudent choice because he was 'the most generally distinguished of the German paediatricians; a man who qualifies himself to be Henoch's successor through his eminent expertise, his warm interest for paediatrics and through his qualities as a teacher and a person'. Ludolf Krehl also praised Heubner in a long letter. He emphasised the quality and popularity of his lectures as well as his excellent management of a polyclinic and a paediatric clinic.[48] He also praised his character by sharing his appreciation of the fact that Heubner was 'full of zest for life and not embittered' even though he had been passed over for promotion in Leipzig so often that he would have been justified in feeling slighted by the university administrators.

Külz also underlined these career setbacks. In his eyes, they served as a red flag: 'I would like to strongly emphasise the fact that [the Leipzig medical faculty] has passed him over for the occupancy of the polyclinical professorship. Some people may say that his appointment did not have a chance in Dresden, but of course that cannot be the true and only reason'.[49] Külz's suspicion was that these setbacks must have been the result of Heubner's typically Saxon personality: 'His really too pronounced Saxon dialect may be the reason why his lectures, which are not bad, have always made me feel somewhat funny. […] I cannot advise in favour of the transplantation of this typical Saxon to Berlin'. Külz thought Ludolf Krehl's otherwise approving depiction of Heubner supported his view. This is not completely unreasonable, since Krehl wrote that 'Heubner is a very lively typically Saxon figure; he speaks a strong Saxon dialect which sometimes comes across comically. […] His lectures are harmed by [his] Saxon dialect; his way of talking is clumsy […]'.[50] Külz was not the only confidant of Althoff to be critical of the possible appointment of Heubner; the Berlin medical faculty, and especially the famous pathologist Rudolf Virchow, shared his doubts.[51] Külz therefore had good reason to believe that his advice would be followed: 'Notwithstanding all the appreciation

[47] Hugo Falkenheim to Friedrich Althoff, received at 14 November 1893, GStA PK, VI. HA, Nl Althoff, No. 112.
[48] Ludolf Krehl to Friedrich Althoff, 21 November 1893, GStA PK, VI. HA, Nl Althoff, No. 112.
[49] Eduard Külz to Friedrich Althoff, 13 November 1893, GStA PK, VI. HA, Nl Althoff, No. 112. The mentioning of Dresden refers to the Saxon government that had its seat there.
[50] Ludolf Krehl to Friedrich Althoff, 21 November 1893, GStA PK, VI. HA, Nl Althoff, No. 112.
[51] Hesse, Volker, 'Leben und Werk Otto Heubners: Eine Würdigung des ersten Lehrstuhlinhabers der Kinderheilkunde in Deutschland anlässlich seines 90. Todestages,' *Monatschrift Kinderheilkunde*, 12, 2016, 1116–1123. 1116.

of some of his works, Heubner is a chatterbox (*Faselhans*). His comical personality seems ridiculous to his audience. It would be a great mistake to appoint him'.[52]

For Althoff, the decision was far from easy. Some of his correspondents wholeheartedly recommended Heubner as the foremost German paediatrician. Others argued that Heubner's typical Saxon disposition made him unfit for an appointment in the Prussian capital. Most of the other candidates, however, seemed to have weaknesses, as well. Fritsch had recommended Adolf Baginsky, Oswald Kohts and Carl von Noorden.[53] Still, his praise of Baginksy was ambiguous at best: 'Baginsky is a Jew who forces himself to the foreground, but he certainly is not stupid'. Appointing Baginsky became even more unfeasible after the queen mother informed the ministry that she would prefer the main Berlin paediatrician — who might one day be asked to treat her children or grandchildren — to be a Christian.[54] Fritsch did not elaborate on his preference for Kohts and while nobody had strong objections against him, no confidant showed much enthusiasm either. Külz argued that Kohts would have been a better candidate if he would have shown more diligence and that he could have put more effort into deepening his knowledge.[55] Falkenhein did not fully dismiss his candidacy but explicitly stated that both Heubner and Baginsky were better qualified.[56] Van Noorden's disadvantage was that he was not actually a paediatrician, which also applied to Falkenheim's favourite, Oswald Vierordt. Theodor Escherich was also mentioned a couple of times, but Falkenheim listed him among the people that should not be considered. Külz was even more explicit in his rejection. He advised against his appointment because he considered him to be 'an overachiever' (*Streber*) who 'still has to mature'.[57]

There was no way in which Althoff could follow up on the recommendations of all his confidants. On one side of the isle there were people like Falkenheim and Krehl who were convinced that Heubner was a self-evident choice. On the other side there were people like Virchow and Külz who advised strongly against him. The position of the Berlin medical faculty is not quite clear. Some sources mention that its members followed Virchow's lead and kept Heubner off their shortlist.[58] Others state that both Henoch and the faculty supported Heubner's candidacy.[59] Heubner's supporters could make a good case because hardly any other scholar suggested by his

[52] Eduard Külz to Friedrich Althoff, 3 December 1893, GStA PK, VI. HA, Nl Althoff, No. 112.
[53] Heinrich Fritsch to Friedrich Althoff, 9 August 1893, GStA PK, VI. HA, Nl Althoff, No. 112.
[54] Pawliczek, 'Kontinuität des informellen Konsens,' 86. This may also explain Althoff's interest in the question whether Soltmann was Jewish or not.
[55] Eduard Külz to Friedrich Althoff, 13 November 1893, GStA PK, VI. HA, Nl Althoff, No. 112.
[56] Hugo Falkenheim to Friedrich Althoff, received at 14 November 1893, GStA PK, VI. HA, Nl Althoff, No. 112.
[57] Eduard Külz to Friedrich Althoff, 3 December 1893, GStA PK, VI. HA, Nl Althoff, No. 112.
[58] For example, see Hesse, 'Leben und Werk Otto Heubners,' 1116; Tomasevic, Klaudia, 'Die medizinische Versorgung von Kindern Mitte des 19. Jahrhunderts am Beispiel von Würzburg,' (diss. Bayerischen Julius-Maximilians-Universität zu Würzburg, 2002). 28.
[59] Pawliczek, 'Kontinuität des informellen Konsens', 87.

opponents could count on much enthusiasm either. The conclusion that Külz drew from his case against Heubner was exemplary. He argued that the only other viable option would be to appoint Kohts, but he showed very little enthusiasm for him: 'There are favourable and unfavourable assesmments of Kohts. Without further ado, I am more inclined to trust the unfavourable judgements. In regard to the decision, it should not be neglected that, if you don't take the reliable and experienced Henoch into account, all the representatives of the discipline are inferior without exception'.[60]

After every candidate had been criticised by some of Althoff's correspondents, he asked another confidant, the administrative director of the Charité hospital Bernhard Spinola, to meet with Heubner. Spinola's opinion was unambiguous: 'Last Thursday, Professor Heubner was with me for quite a long time, I really liked him and he seems to be willing to accept the position. I have promised him the best possible consideration of his wishes on the side of the Charité'.[61] Early next year Heubner was appointed as the head of the paediatric clinic of the Charité and one year later he also received his full professorship.

The example of the discussion about Heubner's appointment shows at least two things. First, it shows the limits of the influence a scholar could exert through letters of recommendation. If the evaluations by different confidants turned out univocal, Althoff would often follow their advice. Strong disagreements among his confidants, however, forced him to disregard some recommendations and trust either his own judgement or that of his closest confidants. The second thing illustrated by the extensive correspondence about Henoch's succession is that there was no general agreement about the requirements for holding one of the most prestigious positions in German academia — a Berlin professorship. The fact that even Külz praises Heubner for the publication of significant works and the effective management of his paediatric hospital suggests that these basic requirements were generally acknowledged.[62] There was no agreement, however, on the importance of an upper-class and metropolitan demeanour. Krehl extensively described the Saxon mannerisms of Heubner without believing that these would make him less eligible for a professorship in Berlin. Külz, however, was convinced that his mannerisms were incommensurable with the dignity attached to an *Ordinariat* at Germany's most prestigious university.

The first section of the chapter already mentions that Althoff was often at odds with the conservative forces at the universities. This was most visible in his insistence on appointing Jewish and Catholic professors. The Heubner case suggests that he also opposed a different kind of social

[60] Eduard Külz to Friedrich Althoff, 3 December 1893, GStA PK, VI. HA, Nl Althoff, No. 112.
[61] Bernhard Spinola to Friedrich Althoff, 26 December 1893. GStA PK, VI. HA, Nl Althoff, No. 112.
[62] Eduard Külz to Friedrich Althoff, 13 November 1893, GStA PK, VI. HA, Nl Althoff, No. 112.

conservatism by making decisions on the assumption that scholarly and clinical accomplishments were more important than a funny accent and a somewhat provincial demeanour. The fact that he was also able to find both supporters and opponents on this issue further testifies to the lack of consensus about the exact requirements of representability for a successful scholar.

Althoff's full force

The Heubner case showed the limits of the influence that could be gained from an extensive correspondence with Althoff; if recommendations contradicted each other, he had to make the final decision himself. In some cases, however, Althoff even decided to ignore unanimous recommendations. These decisions may have created his reputation as the 'Bismarck of the university system'. One of the appointments that evoked incomprehension and disappointment was that of Behring at the Marburg Chair of Hygiene. The widely shared doubts about Behring's suitability further add to our understanding of the expectations of a professor at a Prussian university, while they also illustrate Althoff's preference for unorthodox academics with recognisable scholarly accomplishments over scholars whose background and abilities reflected a more traditional template of Prussian professorship.

After the success of his diphtheria serum Behring had grown increasingly dissatisfied with his subordinate position at Koch's institute. For the winter of 1894 Althoff was able to find him a temporary professorial position in Halle, but this was not a permanent solution.[63] By the end of 1894 Behring's relationship with Koch had further deteriorated and he argued that it was unlikely that he would be able to fruitfully continue his work at the Institute for Infectious Diseases.[64] He had set his mind on a full Professorship of Hygiene at a Prussian university and put increasing pressure on Althoff by listing the other attractive options available to him; he was asked to become the leader of a soon to-be-established serum institute in Budapest, he could be appointed as the director of research at the *Höchster Farbwerke*, the facility that produced his diphtheria serum, and he had also been invited to continue his research in Petersburg.[65] He preferred, however, to be appointed in Marburg — the Prussian university closest to Höchst.[66] Althoff was convinced that the successful Behring should be preserved for German academia, but, because it proved to be difficult to find him a chair on short notice, he could only give one advice: 'If you really do not

[63] Linton, Derek S., *Emil von Behring*, 102, 192.
[64] Emil Behring to Friedrich Althoff, 24 December 1894, GStA PK, VI. HA, Nl Althoff, No. 325.
[65] Emil Behring to Friedrich Althoff, 2 January 1895, GStA PK, VI. HA, Nl Althoff, No. 325.
[66] Emil Behring to Friedrich Althoff, 8 February 1895, GStA PK, VI. HA, Nl Althoff, No. 325.

want to want to wait, I would advise you to decide for Höchst, because there you will still be approachable for us'.[67]

Behind the scenes Althoff looked for an appropriate position for Behring. He did not, however, look exclusively at hygienic professorships, but also explored the possibility to create a special 'serum professorship' for him.[68] His confidant at the medical faculty in Bonn, the pharmacologist Carl Binz, expressed his willingness to advocate this solution at his university.[69] At the same time Althoff also tried to secure Behring's desired professorial chair in Marburg. One circumstance that seemed to work in Behring's favour was that this chair had finally become available. In the winter of 1894, the Marburg hygienist Carl Fraenkel agreed to take over the Chair of Hygiene in Halle, the following year. Though the availability of the Marburg chair was good news for Behring, the fact that Fraenkel moved to Halle also showed his vulnerability at the academic job market. Behring had just spent a semester teaching in Halle, but his job performance had not convinced anyone in Halle to hire him instead. And, indeed, the reports about Behring's teaching activities in Halle were far from glowing.

This may have surprised Althoff because earlier evaluations of Behring's teaching had been rather positive. Since he was appointed as a military doctor and because he had never written a *Habilitation* his early teaching experience was limited to teaching courses to other military doctors. Hermann Schaper, the medical director of the *Charité*, testified that '[Behring] took great pains with his course, so that the chief staff doctors and the staff doctors have always attended it with the greatest interest'.[70] Another referent remembered the bacteriological courses that Behring had taught together with Bernhard Nocht. The latter would give the lectures after which both men would supervise the practical component of the course. He recalled that 'Behring was very detailed, exact and clear in his instruction' and he 'found that he had a great skill to explain something to the students, some of whom were completely inexperienced'.[71] Heinrich Bonhoff, a colleague at the Institute for Infectious Diseases, was even more complimentary: '[His] lecture was steady, calm and strictly factual, easy to understand for everyone, with exact consideration of the understanding of the audience […] From this and other lectures of Behring I have obtained the conviction that [he] has outstanding teaching skills'.[72] At the same time, however, the first rumours of Behring's unfitness started circulating. Eduard Hitzig, who had earlier recommended Behring for his Halle

[67] Friedrich Althoff to Emil Behring, 30 December 1894, GStA PK, VI. HA, Nl Althoff, No. 326.
[68] Friedrich Althoff to Emil Behring, 25 February 1895, GStA PK, VI. H, Nl Althoff, No. 326.
[69] Carl Binz to Friedrich Althoff, 29 January 1895, GStA PK, VI. HA, Nl Althoff, No. 326.
[70] Hermann Schaper to Friedrich Althoff, 8 August 1894, GStA PK, VI. HA, Nl Althoff, No. 326.
[71] Testimony of an unknown former student of Behring, GStA PK, VI. HA, Nl Althoff, No. 326.
[72] Heinrich Bonhoff to Friedrich Althoff, 25 August 1894, GStA PK, VI. HA, Nl Althoff, No. 326.

position, wrote to Althoff that he had heard from two different sources that Behring was 'somewhat meshuga'.[73]

The rumours of Behring being somewhat meshuga would soon be accompanied by attacks on his teaching. His Halle colleague Josef von Mering was very critical: 'Professor Behring is an outstanding bacteriologist, of whom it seems to be very doubtful, whether he is knowledgeable about the other fields of hygiene. As a teacher, [Behring] can only claim modest success, which might partly be attributed to the fact that he has not lectured before, because he has not been habilitated and partly to the fact that he does not yet master the subject completely'.[74] Von Mering argued that it would be better if Bering's teaching would be limited to the supervision of practical courses. His observations were corroborated by the pharmacologist Erich Harnack who stated that he believed that Behring 'will only be a successful teacher for those who specifically work according to his intentions under his leadership'.[75] Of course, the tidings of Behring's disappointing teaching accomplishments also reached Marburg, where they were interpreted as 'extraordinarily unfavourable'.[76] And these unfavourable evaluations were not the only argument against Behring's appointment.

Behring's other vulnerability was his polemic disposition. Even if the usefulness of his blood serum was widely recognized, the reasons and conditions of its efficacy were widely discussed. One major critic of Behring's analysis of his serum's efficacy was Rudolf Virchow. In itself, Virchow's disagreement was not a reason to worry; in October 1894, he informed Althoff that he was glad to be able to use Behring's highly effective serum in the children's hospital, the *Kaiser- und Kaiserin-Friedrich-Kinderkrankenhaus*.[77] When a Berlin newspaper reported that Virchow had claimed that the first successful experiments with favourable results had been carried out by one of his students, however, Behring defended his priority claim forcefully in an article in *Die Zukunft*.[78] In defence of his claim, he bluntly argued that his blood serum could never have been developed under Virchow's supervision.[79] He called Virchow a 'medical doctrinaire' who had to be opposed. He characterised his ideas about the origin and cure of disease as 'heresies' and argued that the 'dogmatism' of his 'belief system' had led to an 'inquisition'. Even if Behring's criticism on issues like the locality of disease in the body and the specificity of cures fell within the accepted norms of scholarly debate and his argumentation about the incommensurability of Virchow's theories of disease and the

[73] Eduard Hitzig to Friedrich Althoff, 10 August 1894, GStA PK, VI. HA, Nl Althoff, No. 326.
[74] Josef von Mering to Friedrich Althoff, 10 December 1894, GStA PK, VI. HA, Nl Althoff, No. 326.
[75] Erich Harnack to Friedrich Althoff, 13 December 1894, GStA PK, VI. HA, Nl Althoff, No. 326.
[76] Carl Fraenkel to Friedrich Althoff, 18 December 1894, GStA PK, VI. HA, Nl Althoff, No. 326.
[77] Rudolf Virchow to Friedrich Althoff, 17 October 1894, GStA PK, VI. HA, Nl Althoff, No. 326.
[78] Linton, Derek S., *Emil von Behring*, 177.
[79] Behring, Emil, 'Das neue Diphtheriemittel,' *Die Zukunft*, neunter Band, 1894, 97–109 and 249–264.

development of the blood serum may have been convincing to many, his name calling did not win him any sympathy. Behring also implicitly admitted to Althoff that he might have given the impression that his article in *Die Zukunft* was motivated by resentment.[80] This is how the medical faculty in Marburg must have interpreted it as well, since Fraenkel argued that it was this specific publication that had damaged the willingness of his colleagues to consider him for a professorship.[81]

The list of recommendations of the Marburg faculty therefore consisted of Albrecht Kossel and the former Koch associates Erwin von Esmarch and Moritz Eduard Cramer.[82] Althoff had instructed Fraenkel to make sure that Behring would also be included, but this was unfeasible: 'To get Behring on the list was simply impossible; except for me, nobody stood up for him'.[83] Since Althoff was still considering to create a special serum professorship for Behring, Fraenkel did not push the case and concurred with the faculty's preferences.[84] One month later, however, Althoff's confidant and Professor of Physiology in Marburg, Eduard Külz, died. Kossel took over Külz's position and the Marburg chair was available again.[85] Emphasising the overtures from Budapest and Petersburg, Behring did not waste any time to once more explicitly point out that he had set his mind on the Marburg professorship.[86] Althoff gave Fraenkel the same instruction as the month before, to make sure that Behring would end up on the faculty's list of preferred candidates. Once again, Fraenkel could not deliver: 'The candidacy of [Behring] seemed pointless to me […]. A sudden […] resort to Behring was really hardly possible for me, because I could not put forward any argument for it at all'.[87] The new list was the same as the old one, except that Kossel had been replaced by Walther Kruse.[88] The negotiations had reached a stalemate.

Althoff used his personal relationship with the minister to slow down the decision-making process until Behring had returned from his long vacation in France.[89] When he returned, the ministry decided to ignore the preferences from Marburg and to appoint Behring instead. The bad news reached the faculty in April. The decision was justified, as follows: '[...] your suggestions could not be considered mainly because it was particularly important to retain a distinguished man like Professor Behring in the service of a Prussian university and there was no other option available at

[80] Emil Behring to Friedrich Althoff, 6 December 1894, GStA PK, VI. HA, Nl Althoff, No. 325.
[81] Carl Fraenkel to Friedrich Althoff, 18 December 1894, GStA PK, VI. HA, Nl Althoff, No. 326.
[82] Carl Fraenkel to Friedrich Althoff, 25 February 1895, GStA PK, VI. HA, Nl Althoff, No. 326.
[83] Carl Fraenkel to Friedrich Althoff, 18 December 1894, GStA PK, VI. HA, Nl Althoff, No. 326.
[84] Friedrich Althoff to Emil Behring, 30 December 1894, GStA PK, VI. HA, Nl Althoff, No. 326.
[85] Friedrich Althoff to Emil Behring, 31 January 1895, GStA PK, VI. HA, Nl Althoff, No. 326.
[86] Emil Behring to Friedrich Althoff, 8 February 1895, GStA PK, VI. HA, Nl Althoff, No. 325.
[87] Carl Fraenkel to Friedrich Althoff, 9 February 1895, GStA PK, VI. HA, Nl Althoff, No. 326.
[88] Friedrich Althoff to Emil Behring, 25 February 1895, GStA PK, VI. HA, Nl Althoff, No. 326.
[89] Ibid.

the time'.[90] The medical faculty had lost its battle against the professorial appointment of a man they considered to be unfit and Althoff had been able to keep the inventor of the diphtheria blood serum in Prussia. And though it soon became clear that the faculty's distrust of Behring's teaching prowess was justified, this problem was soon resolved; Behring convinced Althoff to appoint his Berlin associate Erich Wernicke to take over most of his teaching duties.[91]

The case of Behring allows for a better understanding of the conclusions drawn from Fritsch's efforts in Breslau and the rocky road to Heubner's appointment in Berlin. While Fritsch's case shows that the interest of the institution could trump the interest of the individual scholar, Behring's case provides an example of the interest of the individual scholar being more significant than institutional concerns. This raises the question what the letters of recommendation collected at the Prussian ministry of education actually deal with. Do they recommend the best course to take to promote the interests of Prussian academic institutions or do they give advice about the professorial fitness of individual scholars? Though some letters emphasise the one side and others mostly focus on the other side, most recommendations look for a middle ground. Those that deal primarily with institutional interests try to serve these by recommending suitable individual scholars. Those that mostly focus on the merit of individual scholars, also touch on the question whether these merits suit the institution in question.

Secondly, the Behring case shows how institutional and individual interests could clash. All parties agreed that he was a brilliant researcher who therefore deserved a professorial appointment, preferably a special blood serum professorship with limited teaching responsibilities. All parties also agreed that Behring's wish to hold a Chair of Hygiene at a Prussian university was problematic, because he was not the right person to fulfil the teaching obligations that were part of that job. When Behring refused to settle for a special serum professorship, a stalemate was reached that could only be broken by the ministry. In cases like this it was up to Althoff to decide on a case by case basis whether institutional or individual considerations would be decisive and Behring's brilliance as a researcher as well as his pressure on Althoff proved to be crucial.

[90] Der Königliche Kurator der Universität Marburg to die Medizinische Fakultät Marburg, 11 April 1895, BNd: EvB/L271/5.
[91] Less than a year after arriving in Marburg Behring already informed Wernicke that he was working on getting him to Marburg to take over his teaching responsibilities: Emil Behring to Erich Wernicke, 29 January 1896, Staatsbibliothek Berlin, Preußischer Kulturbesitz (hereafter Stabi), Nl. 156, Erich Wernicke. In April 1896 Wernicke would arrive in Marburg: Emil Behring to Erich Wernicke, 2 April 1896, Stabi, Nl. 156.

Philosophy and morality

Some qualities were expected of prospective professors in all disciplines taught at Prussian universities. It was, for example, important to have carried out independent research and to have at least some basic teaching skills. Other qualities however, were only relevant to individual disciplines. As the worries of Fritsch and the discussions about Heubner's transfer to Berlin demonstrate, it was important for a professor at the medical faculty to be able to manage a hospital department and to maintain a good relationship with patients. Such management and communication skills were not expected from orientalists and experimental psychologists. But Wundt and his peers were judged by discipline specific criteria as well. Because early experimental psychology developed within the philosophical faculty, psychologists were evaluated on criteria that were considered to be especially relevant to philosophers. More than other scholars — except maybe theologians — philosophers were expected to be exemplary both in their teaching and the pursuit of the of their lives.

A comparison of two disciplinary overviews among Althoff's papers exemplifies the importance of such moral and religious demands. One is an overview of German psychiatrists and neurologists written by the Berlin *Privatdozent* Ernst Siemerling in 1889. The other is an overview of German philosophers by the Halle philosopher Hans Vaihinger in 1893. Siemerling mentions the religious affiliations of the twenty-six scholars on his list, but does not draw any conclusions from this information. His most striking observations concern the often deplorable character traits of his peers. Otto Binswanger, for example, is described as follows: 'Thinks very highly of himself; talks a lot, his statements are not very trustworthy, he is very secretive and always thinks of his own interest. Not a candid character'. Eduard Hitzig was harshly judged as well: 'Has a very brusque, unsociable character, an egoist and autocrat through and through'. The Breslau psychiatrist Carl Wernicke had 'made himself unpopular because of his brusque behaviour' and was said to be of 'dubious character'.[92] In addition, he listed everyone's main publications, sometimes with short comments, such as 'nothing new', 'not very important', 'Good work with new points of view', or simply 'good'.

Religion is more central to Vaihinger's overview of German philosophers. This document suggests that what is important about religion is not one's affiliation, but rather how religion influences one's outlook on life. He distinguished four main religious attitudes among his peers, namely those that 'have a positive religious interest,' 'support religious liberalism', 'are radical in religious

[92] Overview of psychiatrists and neurologists by Ernst Siemerling, 1 March 1889, GStA PK, VI. HA, Nl Althoff, No. 92.

questions, all the way up to hostility against religion' and those who are 'religiously indifferent'.[93] After these distinctions were made, he categorised the more than 100 philosophers under evaluation according to schools of thought. These schools of thought were then connected to the most common attitude towards religion associated with them. The two main categories neatly fit Vaihinger's background as a Kant scholar: apriorism (*Apriorismus*) and empiricism (*Empirismus*). He described the adherents of the different aprioristic schools of thought, such as Kantianism and Hegelianism, as 'having a positive religious interest' even if some individuals are described as liberal or radical. Empiricists are described as liberal, radical, or indifferent. Wundt, who is categorised as an adherent of 'critical empiricism' is assumed to be either liberal or indifferent. Richard Avenarius, the Zürich editor of the *Vierteljahrsschrift* and Wundt associate, is described as belonging to the school of 'positivist empiricism' and religiously radical.

Avenarius' perceived radicalism was probably the reason why he never obtained a position at a Prussian university. His problem was not that his Prussian peers were not aware of his existence. In 1884 he was one of the candidates shortlisted by the university of Kiel to succeed Bruno Erdmann, who had moved to Breslau. Althoff collected detailed information about all the shortlisted candidates. In addition to Avenarius these were the Halle *Extraordinarius* Gustav Glogau, the Bonn *Privatdozent* Theodor Lipps, and Hans Vaihinger, who was at that moment *Extraordinarius* in Strasbourg.[94] The ensuing correspondence shows that Avenarius' supposed lack of piety was not the only argument used against him. At least two of Althoff's correspondents emphasised another criticism as well. The Jena Professor Otto Liebmann advised against his appointment because, although he was already 40 years old, Avenarius had 'only published two slim volumes as books'.[95] The Strasbourg philosopher Wilhelm Windelband was less negative about a possible appointment of Avenarius, but emphasised the same shortcoming: 'Because the journal requires a large amount of work, he has, alas, not accomplished anything for years, but I consider him to be very able and industrious; something will come out of it eventually'.[96] The morally and religiously charged reproaches, however, were probably more damaging.

The importance of a positive attitude towards religion becomes apparent when we look at some of the remarks that indirectly referred to Avenarius. After casually arguing against Avenarius' candidacy the Marburg philosopher Julius Bergmann emphasised the importance of religion in his

[93] Übersicht über die philosophischen Universitätsdocente Deutschlands (mit Einschluss Österreichs u. d. Schweiz) nach ihren Richtungen (nebst einigen Notizen über deren Hauptwerke, Geburtsjahr, Heimat, Konfession u.s.w.) by Hans Vaihinger, October 1893, GStA PK, VI. HA, Nl Althoff, No. 118.
[94] Bruno Erdmann to Friedrich Althoff, 19 January 1884, GStA PK, VI. HA, Nl Althoff, No. 118.
[95] Otto Liebmann to Friedrich Althoff, 23 January 1884, GStA PK, VI. HA, Nl Althoff, No. 118.
[96] Wilhelm Windelband to Friedrich Althoff, 24 January 1884, GStA PK, VI. HA, Nl Althoff, No. 118.

rejection of the candidacy of Theobald Ziegler for another position. He argued that Ziegler was a follower of David Friedrich Strauss. Strauss's investigations into the historical Jesus were seen by many peoople as damaging to the Christian faith when they were published in the 1830s.[97] Half a century later, Bergmann still argued that 'the representation of this school of thought at a university by a full professor must pose a great danger to the students'.[98] Like Bergmann, the conservative curator of the university of Halle, Wilhelm Schrader, did not explicitly mention Avenarius. He did, however, point out that his rival, Glogau, held 'ethical-religious (*sittlich-religöse*) views' and he approvingly added that these views informed his day-to-day life as demonstrated by the fact that he had seen him attending a church service.[99]

Avenarius himself was convinced that there was a prejudice against philosophers of what he called the Wundtian school of thought.[100] Althoff seemed to be open to criticisms grounded in such prejudice. Therefore, the only wholeheartedly enthusiastic recommendation of Avenarius was presented as an argument against Althoff's worries about Avenarius moral uprightness. A letter by the Berlin philosopher Friedrich Paulsen shows that Avenarius faced an uphill struggle: 'It seems to me that you fear or at least suspect from Avenarius just about any indiscretions or provocations that are capable of disturbing the peace in public education'.[101] Paulsen tried to convince Althoff of Avenarius' moral uprightness: 'I don't doubt […] that [Professor Avenarius] is too honest a man not to express his thoughts in the way that is most appropriate to him. On the other hand, I am convinced that whatever he says will be said with the earnestness and objectivity (*Sachlichkeit*) that should be demanded from a lecture dealing with the final and highest things. I think he is as incapable of defamation as of hypocrisy'. Others, however, were less kind. Even if Windelband was rather positive — in his eyes, only Glogau was a better candidate — he subtly underlined that he himself represented a very different intellectual tradition.[102] Otto Liebmann simply stated that Avenarius represented 'a very extreme school of thought'.[103] Bergmann argued that he had started to read Avenarius' work but that he had never finished it because he immediately realised that Avenarius advocated a one-sided empiricism that he considered to be a regrettable reduction of Kant's thought at best.

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[97] For example, see Linstrum, Erik, 'Strauss's Life of Jesus: Publication and the Politics of the German Public Sphere,' *Journal of the History of Ideas*, 71(4), 2010, 593–616. 605–606.
[98] Julius Bergmann to Friedrich Althoff, 25 January 1884, GStA PK, VI. HA, Nl Althoff, No. 118.
[99] Wilhelm Schrader to Friedrich Althoff, 1 February 1884, GStA PK, VI. HA, Nl Althoff, No. 118.
[100] Richard Avenarius to Wilhelm Wundt, 2 February 1883, UAL, Nl. Wilhelm Wundt. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm) The extent to which it is useful to speak of Avenarius as being part of a Wundtian school of thought is debatable. The close relationship between the two man, however, is clear: see Chapter 2, 64–65 and: Russo Kraus, 'Back to the origins of the repudiation of Wundt,' 30–32.
[101] Friedrich Paulsen to Friedrich Althoff, 22 January 1884, GStA PK, VI. HA, Nl Althoff, No. 118.
[102] Wilhelm Windelband to Friedrich Althoff, 24 January 1884, GStA PK, VI. HA, Nl Althoff, No. 118.
[103] Otto Liebmann to Friedrich Althoff, 23 January 1884, GStA PK, VI. HA, Nl Althoff, No. 118.

The ethical and religious objections against Avenarius were not overturned by Paulsen's later recommendations. Even Avenarius' supporters were unable to advocate his cause without distancing themselves from his thought. His opponents did not have to go out of their way to point at his assumed shortcomings; a short reference to religion, morality, or the supposed extremeness of his thought combined with a reference to his small scholarly output was enough. Glogau would eventually be appointed in Kiel, Vaihinger would take over his position in Halle, and Avenarius would stay in Zürich for the remainder of his career. More than a decade later, in August 1896, he was put at the top of the list of candidates for a Chair of Philosophy in Freiburg.[104] Finally a move back to Germany seemed to be a realistic possibility. But on the 18th of the same month, shortly after he had received the promising news, Avenarius passed away in Zürich.[105]

Schools of thought

Another background against which appointment decisions were taken was the difference between the schools of thought represented by the candidates. Often, the choice for a specific candidate was also that for a specific approach to research and teaching. When Robert Koch left his position as Chair of Hygiene in Berlin for the Institute for Infectious Diseases, he was succeeded by Max Rubner. It was clear to everyone involved that this entailed a change from an emphasis on bacteriology to a focus on physiology.[106] In Orientalism, the generation coming of age by the end of the 19th century advocated to 'open the doors to a wider, deeper and more powerful Orient' as a response to what they considered to be the narrowly positivist philology of the older generation.[107] This translated into a growing interest in contemporary Oriental societies, cultural practices, living languages and a willingness to advance grand-scale hypotheses.

Philosophy was especially susceptible to divisions between competing schools of thought (*Richtungen*). We have already seen how Avenarius and his views were criticised for being radically anti-religious, but his 'positivist empiricism' was only one of the nine principal schools of thought recognised in Vaihinger's overview. He listed four principal schools of aprioristic philosophies grounded in the thought of four influential German philosophers: Hegel, Herbart, Lotze and Kant. He also recognised four schools of empiricism: critical empiricism, psychological empiricism,

[104] Hugo Münsterberg to Wilhelm Wundt, 12 August 1896, UAL, Nl. Wilhelm Wundt. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[105] Hugo Münsterberg to Wilhelm Wundt, 21 August 1896, UAL, Nl. Wilhelm Wundt. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[106] Rössner, Stephan, 'Max Rubner (1854–1932),' *Obesity Reviews*, 14, 2013, 432–433. 432.
[107] Marchand, Suzanne L., *German Orientalism*, 212–216.

idealistic empiricism and positivist empiricism. In addition he recognised a category of 'ultramontane Catholic philosophers' and classified a number of younger scholars whose views had not yet fully crystalised as philosophers and whose school of thought could therefore not yet be determined.[108]

Of course, not everyone would have agreed with all of Vaihinger's categorisations. After all, there are many different ways to distinguish philosophers from each other. Vaihinger's overview does, however, nicely illustrate the huge differences that German philosophers perceived among themselves. Because of such perceptions, philosophers were often hired not because they were assumed to somehow be better at their job than others, but rather because their school of thought suited the sensibilities of other faculty members. The discussions about the Chair of Philosophy in Berlin, in 1894, illustrate the importance of such preferences. Even if the faculty agreed that they were looking for someone with both experimental and psychological credentials, the eventual choice for Carl Stumpf was largely based on the ways in which his work differed from both Wundt's and Georg Elias Müller's.

Stumpf's distance from Wundt was widely recognised in the early 1890s. In the first years of the decade they had been involved in a controversy that had started as a debate on Weber's law, but which quickly turned into an exchange of personal reproaches. The starting point of this discussion was the dissertation of Wundt's student Carl Lorenz, which was published in the *Philosophische Studien* in 1890.[109] Stumpf analysed Lorenz's findings in a long and critical paper, after which Wundt wrote a long reply to defend the work carried out in his laboratory.[110] Even if he claimed to write '*sine ira et studio*', he explicitly commented on Stumpf's personality and closed his polemic with the observation that Stumpf would benefit from his harsh words because he would 'learn to value, not only as the best but also as the most useful virtue for a scientific researcher, this: to be just towards others, to be strict towards himself'.[111] In his replies, Stumpf would become as personal and hostile as Wundt; he accused him of a 'mixture of untrue assertions, of confusions, of mutilations of the

[108] See footnote 92.
[109] Lorenz, Carl, 'Untersuchungen über die Auffassung von Tondistanzen,' *Philosophische Studien*, 6, 1890, 26–103. Carl Lorenz should not be confused with Gustav Lorenz, worked on his dissertation in Wundt's laboratory half a decade before Carl Lorenz and who was mentioned in Chapter 1, 53 and who worked on similar issues.
[110] Stumpf, Carl, 'C. Lorenz: Untersuchungen über die Auffassung von Tondistanzen,' *Zeitschrift für Psychologie und Physiologie der Sinnesorgane*, 1, 1890, 26–103; Wundt, Wilhelm, 'Ueber Vergleichungen von Tondistanzen,' *Philosophische Studien*, 6, 1891, 605–640.
[111] Wundt, 'Ueber Vergleichungen von Tondistanzen,' 640; the English translation has been taken from: Boring, Edwin G., 'The Psychology of Controversy,' *The Psychological Review*, 36(2), 1929, 97–121. 109.

course of my thought, of obscure imputations and negligences, of infirm evasions, of fallacies of every kind, and of frequent assurances of the incapacity and ignorance of his adversary'.[112]

The confrontation with Wundt probably made Stumpf a more attractive candidate in the capital. Though the rumours that Helmholtz had fired Wundt in Heidelberg because of his lacking mathematical prowess have been challenged, contemporary sources suggest that Wundt was not highly regarded in Berlin, Helmholtz's new home city.[113] The Halle university librarian Otto Hartwig reminded Althoff of the controversy and added that 'because the greatest men in Berlin, Helmholtz in particular, have fallen out with Wundt, his former pupil from Heidelberg, Stumpf is already for that reason better liked by them'.[114]

The polemic between Stumpf and Wundt not only shows that scholars could be tempted to make unpleasant personal comments, it also illustrates that different schools of thought existed among scholars whose interests and research methods were closely related. Lorenz and Stumpf had both explored the ability to judge the middle tone between two tones sounded in series.[115] Lorenz's analysis suggested that listeners would pick out the arithmetic mean instead of a harmonious musical interval. Stumpf, however, argued that a well-trained listener would choose the geometric or musical mean as the middle tone instead. The most likely explanation for these different findings was that Lorenz and Wundt relied on a large dataset obtained from musically untrained subjects, while Stumpf based his findings on the aesthetically refined perceptive skills of a smaller number of trained music listeners.[116] This implies that Stumpf's psychology explicitly allowed for an appreciation of aesthetic judgement. In the eyes of some of his peers this appreciation made Stumpf a more attractive colleague than a supporter of what they considered to be Wundt's positivist reductionism. Stumpf also emphasised his aesthetic interests in a letter to the Berlin philosopher Wilhelm Dilthey: 'In regard to the lectures, I will try to read aesthetics alongside psychology and for both I hope to find a growing audience that is not motivated by worries about examinations'.[117] Althoff's most active correspondent about the occupation of the Berlin chair was the Breslau

[112] Stumpf, Carl, 'Mein Schlußwort gegen Wundt,' *Zeitschrift für Psychologie und Physiologie der Sinnesorgane*, 1891, 438–443. 438–440. the English translation has been taken from: Boring, Edwin G., 'The Psychology of Controversy,' 110.
[113] The story about Wundt's dismissal is most famously brought up in: Hall, G. Stanley, *Founders of modern psychology*, Appleton and Company, Ney York, 1912. 311. Wundt himself denies this course of events in his memoires: Wundt, Wilhelm, *Erlebtes und Erkanntes*, 154.
[114] Otto Hartwig to Friedrich Althoff, 27 July 1893, GStA PK, VI. HA, Nl Althoff, No. 118.
[115] Hui, Alexandra E., 'The bias of "music-infected consciousness": the aesthetics of listening in the laboratory and on the city streets of fin-de-siècle Berlin and Vienna,' *Journal of the History of the Behavioral Sciences*, 48(3), 2012, 236–250. 241. My description of the differences between Lorenz and Stumpf is based on this article.
[116] In Chapter 1, 56, I also referred to the subjects in Wundt's laboratory as well-trained. Being trained in introspection, however, is not the same as being trained in interpreting sound within the framework of musical theory.
[117] Carl Stumpf to Wilhelm Dilthey, 5 October 1893, GStA PK, VI. HA, Nl Althoff, No. 118.

Professor Theodor Lipps. He recommended Stumpf wholeheartedly for the Berlin chair: 'Above all, it appears important to me that, thanks to Stumpf, justice will also be done to aesthetics'.[118]

It was clear that most of Althoff's confidants agreed that the appointee to the Berlin chair should not be a Wundtian, but because the Berlin faculty was set on appointing a philosopher who had proven himself to be an apt experimental psychologist the choices were limited. The *Extraordinarius* and founder of the Berlin psychological institute Hermann Ebbinghaus was not considered because he was seen as too limited in his experimental orientation and because he had a rocky relationship with the influential Dilthey.[119] It soon became clear that there were only two serious contenders: Stumpf and the Göttingen philosopher and experimentalist Georg Elias Müller. Though his Göttingen colleague Ulrich von Wilamowitz-Moellendorf described Müller as one of those who were called 'inconvenient subordinate' (*unbequem Untergebene*) in the military — who the historian Treitschke called 'academic porcupine' (*akademisch Stachelschwein*) — Müller was widely recognised as one of the foremost experimental philosophers of his time.[120] Wilamowitz also added that since he had been lifted out of poverty, overcome prolonged illness, finished his book and finally married, Müller had become increasingly sociable.

On most accounts Stumpf and Müller seemed to be equally qualified. The character of both men was harshly evaluated by at least some of their peers. Otto Hartwig stated that Stumpf was 'a very arrogant gentleman and therefore rather irritable and morose'.[121] Friedrich Schollmeyer, Professor of Law in Halle, was not very enthusiastic either: 'Personally, I have the impression of a very tense (*nervös*) human being and that has been confirmed to me by people who have had him as examiner'.[122] Both Stumpf's and Müller's teaching skills received modest praise at best. Hartwig mentioned that Stumpf's lectures attracted large numbers of students, but that his teaching was far from outstanding. The librarian and legal scholar Hans Paalzow observed that his lectures were 'carefully prepared' and 'rich in ideas' but also noticed that most attendants of his first lecture on logic did not return for the second one.[123] The Göttingen mathematician Felix Klein remarked that Müller's lectures were well-attended, original and clear but, at the same time, emphasised that he was apparently unable to convince students to write their dissertation with him.[124] All the above

[118] Theodor Lipps to Friedrich Althoff, 31 August 1893, GStA PK, VI. HA, Nl Althoff, No. 118.
[119] Sprung, Lothar, 'The Berlin Psychological Tradition: Between Experiment and Quasi-Experimental Design, 1850–1990,' in: Woodward, William R. and Robert S. Cohen (eds.), *World Views and Scientific Discipline Formation: Science Studies in the German Democratic Republic*, Springer Science+Business Media, Dordrecht, 1991, 107–116. 108–109.
[120] Ulrich von Wilamowitz-Moellendorf to Friedrich Althoff, 2 September 1893, GStA PK, VI. HA, Nl Althoff, No. 118.
[121] Otto Hartwig to Friedrich Althoff, 27 July 1893, GStA PK, VI. HA, Nl Althoff, No. 118.
[122] Friedrich Schollmeyer to Friedrich Althoff, 28 July 1893, GStA PK, VI. HA, Nl Althoff, No. 118.
[123] Hans Paalzow to Friedrich Althoff, 31 July 1893, GStA PK, VI. HA, Nl Althoff, No. 118.
[124] Felix Klein to Friedrich Althoff, 30 July 1893, GStA PK, VI. HA, Nl Althoff, No. 118.

correspondents, however, lavished praise on both men's accomplishments as independent thinkers and skilled researchers.

Because Stumpf and Müller scored equally well on the significance of research output, personal character and teaching skills, other criteria became decisive. One letter by Wilhelm Schrader suggests that a consideration of their respective schools of thought would not be helpful; both belonged to 'that side of Lotze's school that follows the so-called exact psychological investigations […] without forsaking ideality'.[125] Other advisers, however, described Müller not so much as a philosopher but rather as a physiologist with a limited interest in philosophy. Klein explained that Müller's 'true goal is the creation of a *psychophysics* that corresponds with all our knowledge of today'.[126] Lipps likewise argued that 'Müller's actual literary activities […] throughout many years concerned *physiology*. […] It is indeed to be feared […] that physiology will eventually completely engross him. In any case, for Müller, *philosophy* is not at the *centre* of his interest, at this moment'.[127] Wilamowitz then added: 'he places himself in the natural sciences, and I believe that he trains the students who affiliate themselves especially with him somewhat one-sidedly'.[128]

Althoff's confidants repeatedly underlined that Stumpf was less disposed to limit himself to physiology. Lipps explained that Stumpf's ambition was to reach 'a comprehensive intellectually and ethically satisfying perception of the world and of life'.[129] Max Dessoir saw Stumpf as a philosopher with a 'brilliant general intuition' who uses 'essentially logical tools to 'approach the questions of psychic life (*Seelenleben*)'.[130] Dilthey, finally, emphasised Stumpf's attempts to 'harmonise the spirit of the natural sciences with the highest interest of humankind' and added that 'in this deeper relationship with religion and ethics he is unique among the scientific (*naturwissenschaftliche*) philosophers'.[131] Even if Schrader's letter suggested that Stumpf and Müller represented the same school of thought, this seems to have been only superficially true. A closer look at their teaching, research and publications showed that Müller's interest in experimental psychology was part of an overarching interest in psychophysics and physiology while Stumpf's use of methods from the natural sciences was intimately connected to aesthetic, religious and ethical concerns. Since most of the influential Prussian philosophers — not least of all Dilthey —

[125] Wilhelm Schrader to Friedrich Althoff, 28 July 1893, GStA PK, VI. HA, Nl Althoff, No. 118.
[126] Felix Klein to Friedrich Althoff, 30 July 1893, GStA PK, VI. HA, Nl Althoff, No. 118. Klein's emphasis.
[127] Theodor Lipps to Friedrich Althoff, 31 August 1893, GStA PK, VI. HA, Nl Althoff, No. 118. Lipps's emphases.
[128] Ulrich von Wilamowitz-Moellendorf to Friedrich Althoff, 2 September 1893, GStA PK, VI. HA, Nl Althoff, No. 118.
[129] Theodor Lipps to Friedrich Althoff, 31 August 1893, GStA PK, VI. HA, Nl Althoff, No. 118. 134–141.
[130] Max Dessoir to Friedrich Althoff, no date, GStA PK, VI. HA, Nl Althoff, No. 118.
[131] Wilhelm Dilthey to Friedrich Althoff, no date, GStA PK, VI. HA, Nl Althoff, No. 118. 154–155.

sympathised more with Stumpf's school of thought than with Müller's, the former was eventually appointed.

The individual, the institution and the state

The preceding case studies illustrate certain characteristics of the Althoff system as well as some interesting features of the recommendation he received. As far as the Althoff system is concerned, the above cases show that Althoff could base his decisions on an extensive and intimate knowledge about Germany's most successful and promising scholars. The reports of the Seminar for Oriental Languages show that he even kept files about such things as alleged plagiarism, simmering feuds and accusations of student-day aberrations.

Althoff kept himself informed about scholars in the whole German speaking world. He did not encounter difficulties in gathering information about Avenarius in Zürich and Heubner in Leipzig. When he received unanimous advice, Althoff was often willing to follow it, even if the Behring case shows that, in exceptional cases, he would push his own candidate. Such use of force was rare, however, because his confidants would argue in favour of his views at faculty meetings, all over Prussia. For example, Fritsch represented him at the Breslau medical faculty; Fraenkel did this at the Marburg medical faculty; Dilthey was his most trusted representative at the Berlin faculty of philosophy. Having representatives in faculty councils was especially important when there was disagreement among the council members. The many letters of recommendation sent to Althoff paint a colourful picture of the many shapes such discussions could take. The pieces of correspondence discussed in this chapter show three main sources of disagreement.

In the first place there was no general agreement on what qualities made an individual electable to a professorial chair. In Heubner's case all advisors agreed that he was a preeminent paediatrician, both as a researcher and as the manager of his clinic. At the same time, there were doubts about his teaching skills; even if what he said was appropriate, some considered the way in which he said it to be disqualifying. His Saxon accent and his supposedly typical Saxon demeanour would make it hard for his students to take him seriously and hamper his ability to transfer his knowledge. Behring's invention of the diphtheria blood serum was also widely praised and the idea to create a special blood serum professorship for him was well-received. At the same time his educational prowess and the breadth of his knowledge were widely questioned. Therefore, the idea to give him his desired hygienic professorship, which would require him to teach on a wide range of subjects, met with strong opposition. The discussions about the appointments of Heubner and Behring

show an unwillingness among academics to rank each according to one set of criteria. Nobody called Heubner's competitor Kohts an all-round better candidate. Kohts' supporters rather first praised Heubner's accomplishments, then noted that his way of presenting himself made him — alas — unfit for a Berlin professorship and only then suggested that Kohts might be a viable option. Behring was not represented as inferior to any of his competitors for a Chair of Hygiene in Marburg, either. The main argument was rather that his merits were not decisive for the decision about his appointment. All in all, critical evaluation could be focused on the many different qualities of a scholar.

This consideration brings us to the second source of disagreement. The correspondence characterised as consisting of 'letters of recommendation' in this chapter is different from what we designate as such in the 21st century. Modern-day letters of recommendation are usually written at the request of individuals and mostly emphasise the accomplishments and character traits that make them suitable candidates for a job. The letters of recommendation discussed in this chapter, however, were not requested by the applicants but by the man who made the hiring decisions. Therefore, they were rarely a reflection of the merit of one candidate, but rather an evaluation of both a number of candidates and the specific needs of the institution that hoped to appoint one. Fritsch's letters to Althoff focus on the state of affairs at the Breslau medical faculty rather than on the professional accomplishments of any single candidate. The decision to hire Küstner was largely based on the fact that his wealth would allow him to accept a professorship at a university where he would be required to direct his attention to a troubled clinic rather than to a more profitable private practice. Those Althoff confidants who argued that teaching was a more decisive criterion for a professorial appointment than research and publications were also concerned with the institution rather than with the individual. Both Heubner and Behring were praised for their accomplishments by men who advised against their appointment; the reason to oppose hiring them was informed by institutional considerations about the preferable balance between teaching and research. We can therefore conclude that letters of recommendation provided an opportunity to show loyalty to both individuals scholars and institutions of learning.

A final source of disagreement was the existence of different schools of thought in many fields. In hygiene you could distinguish between bacteriological and environmental schools of thought. In orientalism there was a difference between proponents of ancient philology and advocates of research into contemporary culture and living languages. In philosophy the distinctions between different schools were especially well-defined. As Vaihinger's overview suggests, the divide between what he called apriorists and empiricists was very deep. The examples of Stumpf, Wundt and Müller illustrate how a philosophical outlook fully informed by the natural sciences could harm

a candidate. Stumpf was a good candidate in the light of the supposed neglect of aesthetics in Wundt's experimental methodology and the overemphasis on physiology in Müller's work. People representing schools of thought that were considered to be religiously radical could be denied a professorship not only on grounds of personal beliefs of the faculty members but also because their influence might pose a threat to the moral state of the student population. This is illustrated by the example of Avenarius' lack of success in finding an appointment in Prussia. All in all, belonging to the same school of thought was a very common reason for providing loyal support to a colleague by sending a glowing recommendation to the ministerial authorities.

These observations show that in most cases letters of recommendation were not primarily about individual candidates, but considered both the specific needs of the hiring institutions and a range of different qualities of the scholars under consideration. Most scholars were not presented as overall good, but rather as suitable or unsuitable for a specific vacancy at a specific institution. Potential points of interest for these positions included teaching skills (e.g. Behring), financial situation (e.g. Küstner), social profile (e.g. Heubner), school of thought (e.g. Stumpf) and religious affiliation (e.g. Avenarius). Different criteria could be added to different disciplines; a medical professor was often required to also be a good clinician, and a philosopher would improve his chances to be appointed if his potential future colleagues would have some affinity with his school of thought. The one quality discussed surprisingly rarely was that of the candidates' research and scholarly publications. Apparently, such considerations were only of secondary importance, although Althoff's support of Behring shows that he definitely took innovative research into account.

5. A Moral Economy of Scholarship

Balancing Critical Independence and Loyal Collegiality

Moral economies

The preceding chapters look at scholars in various capacities. The first chapter explores how scholars used private correspondence to support each other before their work was made public. The second chapter illustrates how the interests of publishers, audiences and reviewers shaped the choices made at editorial offices. The third chapter looks more closely at evaluation practices in book reviews. The last chapter examines the assessments made in letters of recommendation solicited by the Prussian Ministry of Education to help them decide about professorial appointments. At first sight, the various arguments made in these chapters suggest little more than the observation that scholars assessed each other in different ways, depending on the role they had to play. In this chapter, however, I argue that there is a red thread running through this variety of assessments. All the presented cases exemplify different aspects of one moral economy of scholarly evaluation. This moral economy asks scholars to balance the ideals of loyalty and independence in their different capacities.

The word loyalty refers to a discourse about related concepts like mutual obligation, collegiality, collaboration and a shared understanding of what it means to be a good scholar. The word independence pertains to ideas about individual accomplishment, ownership, autonomy and critical distance.[1] Through their long history both ideals have acquired indelible connotations of class and gender. In the introduction I have already mentioned Steven Shapin's observation that reliable knowledge was the product of such gentlemanly virtues as 'sociability, pliancy and politeness'.[2] Such bourgeois ideals often drew on earlier aristocratic notions of virtue. Deirdre McCloskey has drawn attention to traces of older aristocratic values in the moral language of the 19th-century middle classes.[3] Robert Nye argues that the relationships between 19th-century scholars were mediated by 'intense bonds of personal loyalty and a discourse of chivalric etiquette'.[4] In the

[1] It is often impossible to distinguish between the German words *Unabhängigkeit* and *Selbständigkeit* in English translations. Both can be translated as 'independence' or 'autonomy', though it can be argued that *Unabhängigkeit* emphasises an agent's independence *from others* while *Selbständigkeit* emphasises the individual autonomy without an explicit reference to others. I will translate both as 'independence' and provide the original German when I think this might be relevant.
[2] See, Introduction, 8.
[3] McCloskey, Deirdre N., *The Bourgeois Virtues: Ethics for an Age of Commerce*, University of Chicago Press, Chicago (IL), 2006. 66, 218, 243.
[4] Nye, Robert A., 'Medicine and Science as Masculine "Fields of Honor",' *Osiris*, 12, 1997, 60–79. 61.

middle-class discourse about scholarship, an emphasis on loyalty of even chivalry went hand in hand with an insistence on personal independence. Nye observes a continuing interest in independence in both early modern cultures of honour and the 20th-century laboratories described by Latour and Woolgar.[5] Shapin presents gentlemanly independence as a necessary condition for reliable scholarly witnesses.[6] Manfred Hettling confirms this close relationship between *Bürgertum* and independence by stating the *Bürger* 'has to make his way through life *independently*'.[7]

Loyalty and independence are easily characterised as middle-class ideals. Bonnie Smith, however, argues that the ideals and practices of scholarship that are usually characterised as bourgeois traits also 'proposed a masculine identity'.[8] Discourses about independence tended to be highly gendered. Learned men often claimed to be unable to recognise independence of mind in women. The 19th-century legal scholar Ludwig von Bar typically argued that certain sub-fields of law were 'not only about keen interpretations and logical inference, but also about comprehensive consideration of various possible consequences […]'.[9] He then concluded that strong emotions of women were likely to prevent them from mastering this skill. Similar arguments were made by contemporary historians; women's physical and cognitive qualities would hinder them in acquiring the critical faculties needed to carry out successful historical research.[10] This conception fitted a broader intellectual trend in 19th-century Germany in which independence was increasingly acknowledged in men but not recognised in women.[11] Ideas about loyalty tended to be gendered as well. Together with courage and a sense of duty and it was part of a catalogue of masculine, martial virtues. In the light of this discourse, Nicolaus Sombart even argues that in 19th-century Germany '..."friendship" is of course a male preserve, just like the "state" is a male preserve (only men can be friends)'.[12]

In the introduction I have advocated an approach to moral economies that combines Lorraine Daston's description of a moral economy as a 'balanced system of emotional forces, with

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[5] Ibid., 78.
[6] Shapin, Steven, *A Social History of Truth: Civility and Science in Seventeenth-Century England*, Chicago University Press, Chicago, 1994. 39.
[7] Hettling, Manfred, 'Die persönliche Selbständigkeit: Der archmedische Punkt bürgerlicher Lebensführung,' in: Hettling, Manfred and Stefan-Ludwig Hoffmann (eds.), *Der bürgerliche Wertehimmel: Innenansichten des 19. Jahrhunderts*, Vandenhoeck & Ruprecht, Göttingen, 2000, 57–78. 57. Hettling's emphasis on the word 'independently'.
[8] Smith, Bonnie G., 'Gender and the Practices of Scientific History,' 1153.
[9] Bar, Ludwig von, in: Kirchhoff, Arthur (ed.), *Die Akademische Frau: Gutachten hervorragender Universitätsprofessoren, Frauenlehrer und Schriftsteller über die Befähigung der Frau zum wissenschaftlichen Studium und Berufe*, Hugo Steinitz, Berlin, 1897. 19.
[10] Schnicke, Falko, *Die männliche Disziplin: Zur Vergeschlechtlichung der deutschen Geschichtswissenschaft 1780–1900*, Wallstein, Göttingen, 2015. 246.
[11] Hettling, 'Die persönliche Selbständigkeit,' 70–71; Kühne, Thomas, 'Männergeschichte als Geschlechtergeschichte,' in: *Männergeschichte – Geschlechtergeschichte: Männlichkeit im Wandel der Moderne*, Thomas Kühne (ed.), Campus, Frankfurt, 1996, 7–30. 11.
[12] Sombart, Nicolaus, 'Männerbund und Politische Kultur in Deutschland,' in: Kühne, Thomas, 'Männergeschichte als Geschlechtergeschichte', in: *Männergeschichte – Geschlechtergeschichte: Männlichkeit im Wandel der Moderne*, Thomas Kühne (ed.), Campus, Frankfurt, 1996, 136–155. 137.

equilibrium points and constraints' with an emphasis on thick description along the lines of Robert Kohler's work on the history of fruit fly genetics, in which he painstakingly describes the values and expectations regulating the work of the early 'fly people'.[13] His thick description contains sections on the individual biographies of the most prominent researchers, the everyday working arrangements at *Drosophila* laboratories, personal relationships between the main protagonists and between various research centres and the values and emerging traditions shared by all involved parties. In this way, Kohler reveals what Clifford Geertz described as 'a stratified hierarchy of meaningful structures'.[14] He gives a detailed description of an 'elite, bound by a shared sense of participating in a remarkable history' that shares 'a moral ethos of cooperation and communality'.[15] In the early years of *Drosophila* research this ethos fostered habits of loyalty.[16] However, as the community of fruit fly researchers grew, assertions of independence from an ever-increasing number of fly people caused the erosion of mutual trust.[17] The peculiarities of this early moral economy only survived for a couple of decades.

An often-quoted study on the moral economy of scholarship with a similar descriptive approach is W. Patrick McCray's study on the sharing of large telescopes by late 20th-century US astronomers.[18] The particular moral economy described by McCray is very different from that of the fly people. The ethos of communality of Kohler's researchers was largely shaped by the abundance of research material and research questions. The values and expectations of McCray's astronomers, on the other hand, were a product of scarcity. There were strict limits to the availability of large telescopes to the members of an ever-increasing community of astronomers. This scarcity shaped a moral economy characterised by fierce competition and reluctant compromise. The fact that Kohler and McCray have used the idea of a moral economy of scholarship to outline very different sets of values and attitudes shows the broad potential analytic scope of the concept.

This chapter argues that the cases presented in this study can be interpreted as Kohlerian thick descriptions. The following section first focuses on the private correspondence described in the opening chapter. What is most striking here is how easily loyalty and independence could go hand in hand, within the private sphere. The following sections, however, will illustrate how the need for and the difficulty of balancing these values increased once evaluation was taken out of the

[13] See, Introduction, 8.
[14] Geertz, *The Interpretation of Cultures*, 7.
[15] Kohler, *Lords of the Fly*, 92–93.
[16] Ibid., 123–124.
[17] Ibid., 167.
[18] McCray, W. Patrick, 'Large Telescopes and the Moral Economy of Recent Astronomy,' *Social Studies of Science*, 30(5), 2000, 685–711.

private sphere. The course of action required by loyalty became less clearly defined when a scholar had to deal with an increasing number of demands on his loyalty; the claims of students, teachers, collaborators, colleagues, academic institutions and the state did not always dovetail. It was also impossible to hold on to a fully independent and critical attitude towards all of these people and institutions at all times.

Before I turn to these analyses there is one more issue in need of further clarification. A superficial look at this chapter's structure might suggest that my analysis is based on a continuum model. The first subsection deals with close friends in academia; the second section looks at collaboration in often hierarchical relationships; the third section looks at the broader peer group. Finally, after looking at the relationship between individual scholars and some of the institutions that shaped scholarship, I take a look at the relationship between these scholars and society at large, as represented by the state. The impression may be that I am trying to fit all these people and institutions on one continuum that ranges from intimate closeness to abstract distance. This impression is only justified to a limited extent; in a way, friends are indeed closer to individuals than to their broader peer group and collaborators are generally closer acquaintances than journal editors or state representatives. At the same time, however, it is important to realise that such a continuum model of closeness and distance has some very obvious shortcomings.

The first of these shortcomings is that not all individual scholars necessarily share the same assessment of closeness and distance. Some might, for example, feel closest to peers with shared interests at faraway places, while others might identify more strongly with the institution at which they are employed. The second obvious shortcoming of such a model is that it is not at all self-evident that all people and institutions discussed in this chapter can in any meaningful way be assumed to fit on one single continuum of closeness and distance. A scholar's dealings with laboratory co-workers on the one hand and state representatives on the other, for instance, can be seen as an example of two fundamentally different types of relationships, rather than as an example of two more or less intimate versions of a similar type of relationship. When reading this chapter, it is therefore important to keep in mind that, although its structure resembles a continuum model, individual scholars are likely to disagree both on the placement of specific relationships on a continuum and on the possibility and nature of coexisting continuums.

Friendship and collegiality

There is a reason the first case study of the first chapter deals with Nöldeke and De Goeje. Very few successful scholars were as close as they were for such a long time. In 1863 De Goeje passionately wrote: 'May this inner harmony continue to exist for our whole lives and may we always be able to stay proud to be each other's heartfelt friends'.[19] More than forty years later Nöldeke would warmly remember the 'immediate connection between us, even if we did not yet know how close and lasting our friendship would be'.[20] If loyalty can be expected between any two people in this study it is between these men. And they indeed dutifully supported each other's work until the death of De Goeje. In the light of my interest in the balance between loyalty and independence, however, it is more interesting to look at the issues about which they disagreed and the ways in which they subsequently criticised each other.

The most noticeable examples of extensive mutual criticism presented in the first chapter are the exhaustive lists of corrections of and suggestions for not yet published texts. Looking at Nöldeke's proofreading of long al-Ṭabarī excerpts it becomes clear that he felt free to comment on every element of the work of De Goeje and his collaborators, whether it was grammar, metre, or the appropriate use of auxiliary sources. His thorough criticism was gratefully accepted by De Goeje and it doubtlessly contributed to the critical acclaim that his edition would eventually receive.

Their acceptance of mutual criticism was not limited to the grateful acceptance of such comments. In 1864 they had a conflict about the merit of Dozy's *Israëlieten te Mekka*. In the eyes of De Goeje Dozy was a shining example of good scholarship and he wrote admiringly about his former teacher's new book, in which he made daring assumptions about the history of Mecca as a religious centre.[21] Nöldeke, however, was critical of both Dozy and De Goeje: 'It pains me that a bright man like Dozy delivers such unmethodical investigations; yet it pains me much more that you accept and praise without reservation, all this which I can only regard as a sham'.[22] For over a year, Dozy's book and their disagreement about its merits figured in their letters. Their judgements continued to be strongly opposed, but this did not damage of the amicable character of their correspondence.

[19] Michael Jan de Goeje to Theodor Nöldeke, 15 December 1863, UBL: BPL 2389.
[20] Theodor Nöldeke to Michael Jan de Goeje, 14 October 1907, UBL: BPL 2389. See Chapter 1, 34.
[21] On Dozy as example for De Goeje, see Chapter 1, 34; For examples of De Goeje praising Dozy's book, see: Michael Jan de Goeje to Theodor Nöldeke, 3 February 1864, UBL: BPL 2389 and Michael Jan de Goeje, 'Een stap vooruit,' *De Gids*, 28(2), 1864, 297–312; On Dozy's Israëlieten te Mecca and the discussion it invoked, see: Paul, Herman, 'Virtue language in nineteenth-century orientalism: a case study in historical epistemology, *Modern Intellectual History*, 14(3), 2017, 689–715.
[22] Theodor Nöldeke to Michael Jan de Goeje, 12 June 1864, UBL: BPL 2389.

One reason for the firmness of their friendship may have been that it had already survived worse disagreements. In 1863, De Goeje had expressed his disappointment in Nöldeke's inclination to focus on the study of numerous smaller Semitic languages rather than on the preparation of text editions of the many classic Arabic texts not yet available in Europe: 'You have worked with care and diligence and delivered what could be delivered, but has it been worth the hassle? Would you not have endowed the public with much more important results if you would have dedicated the same amount of time and diligence to Arabic things?'[23] Nöldeke casually dismissed his friend's reproach: 'You, dear boy, confuse your inclination with the importance of things. You probably don't have an inclination to geology or the history of Roman law; do you therefore think that these subjects are less important than yours? You surely don't! I ask for the same respect for my favourite studies'.[24] De Goeje replied that he did not mean to sound this judgemental ('You now think of me too much as being narrow-minded, my dear friend'), and that he evidently respected Nöldeke's choices.[25] They would never discuss the issue again.

One thing the above examples show is that in their private correspondence Nöldeke and De Goeje hardly felt any tension between loyalty and independence. In fact, their close friendship allowed them to be highly critical of each other's work, teacher and career choices. Hidden from the public eye they almost effortlessly found a balance between both values at the heart of the moral economy of scholarly evaluation. A look at the private correspondence between Wundt and Fechner further illustrates how a personal relationship and mutual criticism could go hand in hand. Although Wundt had already engaged with Fechner's work during his assistantship with Helmholtz in the early 1860s, they would not become personally acquainted until he moved to Leipzig in 1875.[26] In the years before Fechner's death in 1887 the men grew so close that Fechner's widow later asked Wundt to sort out his papers.[27] Their mutual respect and the closeness of their relationship also allowed them to critically distance themselves from many elements of each other's work in private correspondence without damaging their cordial relationship.

As was the case with Nöldeke and De Goeje both men felt free to criticise each other. Their discussion about the authenticity of the spiritist phenomena associated with Henry Slade, as

[23] Michael Jan de Goeje to Theodor Nöldeke, 15 December 1863: UBL: BPL 2389.
[24] Theodor Nöldeke to Michael Jan de Goeje, 16 December 1863, UBL: BPL 2389.
[25] Michael Jan de Goeje to Theodor Nöldeke, 20 December 1863, UBL: BPL 2389.
[26] On Wundt's early engagement with Fechner's work, see: Wilhelm Wundt, *Erlebtes und Erkanntes*, 202. On Wundt meeting Fechner after moving to Leipzig, see: Ibid., 301.
[27] Wundt, Wilhelm, 'Gustav Theodor Fechner,' in: Wilhelm Wundt, *Reden und Aufsätze*, Alfred Kröner, Leipzig, 1914, 254–343. 315. This was a reprint of a speech given in 1901 at the invitation of the *Sächsische Gesellschaft der Wissenschaften*. Wundt would be involved with the Fechner estate for a long time, as shown by his invitation to Lipps to work on an unfinished Fechner manuscript mentioned in Chapter 2, 73.

discussed in the first chapter, is a good example.[28] In his first letter to Wundt Fechner explicitly stated that he discussed this issue in private rather than in public. This privacy created the right setting for a candid debate.[29] Wundt had earlier published a scathing criticism of the trustworthiness of the observations made by those Leipzig scholars who believed in the authenticity of the phenomena produced by Slade.[30] It would have been easy for Fechner to take this personally; he had been present at some of the Slade sessions and his bad eyesight made him the most untrustworthy observer of all.[31] Fechner realised this and in his long letter to Wundt he did not claim to be a trustworthy observer himself, but rather emphasised the trustworthiness of the other attendees. At the same time, he did not hold back against Wundt and accused him of spreading 'suspicion of the investigations so far on the basis of vague assumptions'.[32] But Wundt did not back down and wrote a cordial but uncompromising response. In his reply Fechner thanked him for the 'friendly amiable attitude of his letter' and concluded that spiritism was one of those things about which they would not agree.[33] A similar willingness to critically but cordially engage with each other's work was later shown in their discussions about the work the work coming from Wundt's laboratory discussed in the first chapter.[34]

The above examples suggest that among close friends it was relatively easy to combine loyal support and honest criticism without endangering this personal relationship. The continuous and warm relationships between Nöldeke and Goeje and Wundt and Fechner, however, benefited from favourable conditions. First, there was always a clear distinction between their research projects; usually their collaboration only went as far as commenting on each other's work, rather than working together on some project.[35] They were therefore able to avoid the disagreements to which close collaboration could give rise. Second, they were not in direct competition for university appointments or funding. Nöldeke never considered to move to the Netherlands permanently and when De Goeje could not find a well-paid university position he never thought about pursuing an unlikely appointment in Germany. He rather considered quitting academic life altogether.[36] Wundt

[28] See Introduction, 52.
[29] Gustav Theodor Fechner to Wilhelm Wundt, 18 June 1879, UAL, Nl. Wilhelm Wundt. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[30] Wundt, *Der Spiritismus: eine sogenannte wissenschaftliche Frage*, 11–19.
[31] On Fechner's presence at the Slade session, see: Schneid, Mathias, *Der neuere Spiritismus philosophisch geprüft*, August Hornik, Eichstätt, 1880. 28; On Fechner's bad eyesight, see: Wundt, 'Gustav Theodor Fechner,' 313.
[32] Gustav Theodor Fechner to Wilhelm Wundt, 18 June 1879, UAL, Nl. Wilhelm Wundt. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[33] Gustav Theodor Fechner to Wilhelm Wundt, 25 June 1879, UAL, Nl. Wilhelm Wundt. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[34] See Chapter 1, 53–54.
[35] The exception is of course Nöldeke's membership of the al-Ṭabarī consortium. Still his contribution to this project was only a small part of his scholarly output in the 1870s.
[36] Michael Jan de Goeje to Theodor Nöldeke, 13 August 1864, UBL: BPL 2389.

and Fechner were not competing which each other either because they were at different points in their career when they met. Wundt had just been appointed to the secure position of *Ordinarius*, while Fechner was an emeritus professor from 1875 onwards. When scholars found themselves closely collaborating at one place while they were also at the same stage of their academic career the risk of conflict increased. The relationship between Emil Behring and Erich Wernicke provides a clear example. Though they were close friends and would remain so until Behring's death in 1917, their relationship could be very tense.

Wernicke's contributions had been indispensable to the eventual success of Behring's diphtheria serum.[37] Their correspondence shows that during the years in which the serum was developed the two men were very close. In November 1891, Behring even proposed that they should move in together: 'I promise you that I will not be on your back too much […]'.[38] Three months later, he argued in favour of ever-closer professional and private collaboration, in light of his assessment that they were 'already semi-married, after all'.[39] Wernicke explicitly expressed his appreciation of their friendship, too; in 1897, he adapted a line about loyalty that he had lifted from a Schiller poem for an entry in Behring's guest book: 'Friendship is not an empty illusion!'[40] Their close relationship also withstood candid mutual criticism. Behring repeatedly emphasised that Wernicke was making himself too dependent on him; if he really wanted to pursue a career as a researcher, he would have to make sure to publish independently (*selbständig*) about research that he initiated, planned and executed himself.[41]

Their relationship was tainted, however, by reproaches and disappointments. In 1897, only two months before his entry in the guest book, Wernicke accused Behring of being too dominant, to which Behring almost contemptuously reacted with the admonition to please write him 'many, but less reproachful, letters'.[42] A few years later this awkward exchange was succeeded by a painful discussion about the sharing of the financial profits of their serum. Wernicke had hoped to receive a larger share than Behring allowed him. He did not, however, dare to challenge Behring's claim to own the full 'scientific and financial rights of discovery'.[43] Only when Wernicke was appointed as the director of the Hygienic Institute of Posen in 1899 their friendship recovered. Behring contently

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[37] See Chapter 1, 46.
[38] Emil Behring to Erich Wernicke, 22 November 1891, BNd: EvB/B 1/177.
[39] Emil Behring to Erich Wernicke, 15 February 1892, BNd, EvB/B 1/186.
[40] Wernicke wrote: '*Die Freundschaft ist kein leerer Wahn!*' which is most likely a variation on '*Und die Treue, sie ist doch kein leerer Wahn*' in Schiller's poem *Die Bürgschaft,* a ballad in praise of friendship and loyalty. Wernicke's entry in the guest book is dated 22 April 1897 and can be found at: BNd: EvB/L 266.
[41] For example, see Emil Behring to Erich Wernicke, 11 March 1892, BNd: EvB/B 1/190 and 16 October 1894, BNd, EvB/B 1/220.
[42] Emil Behring to Erich Wernicke, 25 February 1897, BNd: EvB/B 1/244.
[43] Emil Behring to Erich Wernicke, 2 January 1899, BNd: EvB/B 1/248.

noted that 'a temporary separation often clears up the relationship in a favourable way'.[44] Eight years later, when Behring had retreated to the famous Neuwittelsbach sanatorium in Munich, he addressed Wernicke as 'the most faithful among the faithful' in a letter full of self-pity and nostalgia.[45] Behring would eventually recover and his relationship with Wernicke would be amicable until the end of his life.[46]

Students and subordinates

Notwithstanding the extensive correspondence and the expressions of affection between Behring and Wernicke there is at least one clear difference between their friendship that of the other scholars described above. Nöldeke, De Goeje, Wundt, and Fechner did neither depend on nor compete with each other. The relationship between Behring and Wernicke, however, was more complicated. Behring had been the creative driving force in the development of the diphtheria serum and because of Behring's limited teaching efforts in Marburg he was also responsible for Wernicke's first university appointment.[47] Behring's awareness of their uneven relationship is shown both by his nonchalant sidelining of Wernicke complaints and by his repeated insistence that Wernicke should publish something independent of him. Wernicke's obedient acceptance of the unequal benefits from the commercial exploitation of the serum shows that he recognised the inequality as well. To some extent Wernicke's loyalty to Behring therefore more closely resembles the loyalty of a subordinate to his superior than the loyalty of a friend and equal.

Nineteenth-century academia was very hierarchical. One modern-day scholar bluntly stated that '[...] students, assistants, *Privatdozenten* and *Extraordinarien* were all subject to the power and authority of the Ordinarius, each of whom ruled his cabinet in a 'strict patriarchal manner."[48] Another 20th-century scholar likewise emphasised the power differences in German academia, pointing out that '[...] he who controlled the institutes, the means of production in the field of research, thus also controlled therewith the research workers, their opportunities and their prospects'.[49] Another

[44] Emil Behring to Erich Wernicke, 30 December 1899, BNd: EvB/B 1/259.
[45] Emil von Behring to Erich Wernicke, 12 December 1908, BNd: EvB/B 1/273.
[46] After Behring's death Wernicke would stay in touch with his widow. After visiting Else Behring in 1924, Wernicke kindly that his had 'renewed the old feelings of friendship and veneration' for her husband in the family guest book: entry for August 26, BNd: EvB/L 266.
[47] On Behring as the driving force behind the diphtheria serum, see Chapter 1, 46; on Behring's teaching and Wernicke's appointment in Marburg, see Chapter 4, 145–146.
[48] Cahan, David, 'The institutional revolution in German physics, 1865–1914,' in: *Historical Studies in the Phyisical Sciences*, 15(2), 1985, 1–65. 5.
[49] Busch, Alexander, 'The Vicissitudes of the *Privatdozent*: Breakdown and Adaptation in the Recruitment of the German University Teacher,' *Minerva*, 1(3), 1963, 319–341. 328.

author recognises three co-existing and partly overlapping forms of hierarchy among 19th-century German scholars. Academic hierarchy is concerned with the different ranks between student and full professor, institutional hierarchy refers to the various positions in research institutes ranking from research student to director, and disciplinary hierarchy deals with differences in prestige among peers.[50] Such hierarchies shaped the extent to and the way in which assertions of independence and critical assessment of each other's work could be made. And even though the demands of hierarchy were strong in the relationships between full professors and other academic staff, they were often even more consequential in the relationships between these men and their current and former students.

This hierarchy is recognisable in the relationship between Robert Koch and his associates during the cholera outbreaks in Hamburg and Altona, in the early 1890s. When cholera broke out in Hamburg in August 1892 the Berlin authorities ordered Koch to examine the situation on the spot. Koch only stayed for one day and at his return to Berlin his former assistant Georg Gaffky, at that moment Professor of Hygiene in Giessen, was sent to Hamburg to supervise the fight against cholera.[51] When another outbreak occurred in neighbouring Altona the following winter, another former assistant, Richard Pfeiffer, was charged with dealing with this new threat.[52] Gaffky and Pfeiffer did not simply conceive of themselves as serving the city with their expertise, they also considered themselves to be representatives of the Kochian school of hygiene. Gaffky expressed his loyalty to Koch explicitly: 'I certainly know that you trust in me, that I will always aim to work in your spirit and that no greater appreciation in the world can befall me, then when you will later be able to say that I succeeded to some extent. Because you are the soul of it all and we are your executive bodies'.[53]

During their stay in Hamburg and Altona Gaffky and Pfeiffer kept Koch informed about all their findings and actions, which enabled Koch to support and direct their work from Berlin. Drawing on the observations of his trusted collaborators Koch was able to gain the upper hand in an ongoing debate about the aetiology of cholera. In a detailed article he discredited the arguments of his most important critic, the Munich hygienist Max von Pettenkofer.[54] Koch's descriptions of the

[50] Johnson, 'Hierarchy and Creativity in Chemistry,' 215.
[51] For more on Georg Gaffky, see: Benedum, Jost, 'Georg Gaffky (1850–1918) und die Gründung des Gießener Lehrstuhls für Hygiene im Jahre 1888,' *Gießener Universitätsblätter*, 221, 1989, 91–94.
[52] For more on Richard Pfeiffer, see: Rietschel, Ernst Th. and Jean-Marc Cavaillon, 'Richard Pfeiffer and Alexandre Besredka: creators of the concept of endotoxin and anti-endotoxin,' *Microbes and Infection*, 5, 2003, 1407–1414. At the time of the Altona cholera outbreak he was director of the scientific department of Koch's institute.
[53] Georg Gaffky to Robert Koch, 2 October 1892, Robert Koch Institut (hereafter RKI), as/b1/003.
[54] For a modern-day assessment of Pettenkofer's ideas, see: Locher, Wolfgang Gerhard, 'Max von Pettenkofer (1818–1901) as a Pioneer of Modern Hygiene and Preventive Medicine,' *Environmental Health and Preventive Medicine*,

cholera epidemics in Hamburg and Altona in this article were often almost literally lifted from the reports of Gaffky and Pfeiffer.[55] In this way, the loyal collaboration between Koch and his associates contributed to the long term success of the Kochian conception of the aetiology of cholera and more generally of hygiene as a sub-discipline of medicine.

Often, however, the relationship between an established scholar and his former students was more complicated. This becomes clear in Wundt's correspondence with those former associates who continued to publish in the *Studien*.[56] His letters to Merkel, especially, illustrate the occasional awkwardness of their relationship. They sometimes contained such thorough methodological criticism that Merkel was almost driven to despair. In his only response kept in Wundt's papers Merkel sounds disheartened by his harsh tone: 'Your criticism of my work, for which I would like to thank you sincerely, is, if it can be maintained on all points and especially the last one, so devastating that it could well take away all my courage to continue working in this way for even one more minute'.[57] He continued, however, to defend his work against Wundt. After acknowledging the importance and legitimacy of his comments he explained why he had made certain methodological choices, while emphasising his loyal adherence to Wundt's school of thought: 'After all, I have emphasised in the paper that I already sent you two years ago, that […] of the attempts to explain Weber's law, only the one originating from you comes into consideration'.[58] Merkel's letter did not contain any criticism of any assertion or comment by Wundt. The letters of the other former associates of Wundt discussed in the second chapter, Kiesow, Lange and Lipps, did not contain such criticism either.

Notwithstanding the above observations it would be unfair to characterise Wundt as someone who pressured all his former students and assistants into working within the strictly defined bounds of his own research programme. Indeed, almost all of his students who had an academic career developed their own research interests and methods. Modern-day historians seem to be univocal in their recognition of Wundt's failure to establish his own school. Edward Haupt argues that G.E. Müller's laboratory in Göttingen had already taken over Wundt's lead in developing promising

12, 2007. 238–245; Morabia, Alfredo, 'Epidemiologic Interactions, Complexity, and the Lonesome Death of Max von Pettenkofer,' *American Journal of Epidemiology*, 166 (11), 2007. 1233–1238.

[55] For example, see Koch, Robert, 'Die Cholera in Deutschland während des Winters 1892 bis 1893,' *Zeitschrift für Hygiene und Infectionskrankheiten*, 15, 1893, 89–165. 117; Richard Pfeiffer to Robert Koch, 5 February 1893, RKI, as/b1/007.

[56] See Chapter 2, 69–73.

[57] Julius Merkel to Wilhelm Wundt, 8 October 1887, UAL, Nl. Wilhelm Wundt. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)

[58] Ibid. His assurance of adherence to Wundt's thought was a reaction to a remark in Wundt's letter in which he suggested that Merkel's explanation differed from his views: Wilhelm Wundt to Julius Merkel, 5 October 1887, UAL, Nl. Wilhelm Wundt. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)

methods for the newly established field of experimental psychology in the 1890s.[59] Edwin Boring – who dedicates a full chapter to Wundt in his *History of Experimental Psychology* – observes that his main influence may have been in the fact that 'almost all the new schools have been founded as a protest against some one or other characteristic of Wundt's psychology'.[60] Wolfgang Mack, finally, reaches the conclusion that Wundt's philosophical legacy did not fare any better; hardly anyone has bothered to engage with it.[61]

Some of Wundt's most successful former associates include Emil Kraepelin, who would become one of Germany's foremost psychiatrists, Oswald Külpe, the founder of the Würzburg school of psychology, Ernst Meumann, whose main interest would become pedagogy, and Hugo Münsterberg, who accepted a position at Harvard in the 1890s and is remembered chiefly for his contributions to applied psychology.[62] In his correspondence with these people Wundt repeatedly underlined that he did not think that loyalty required them to work within the bounds of his own research programme. Külpe remembered a conversation about a book he was writing in which his former superior had said: 'The more independent it is, the more it will delight me'.[63] When Wundt ended his co-editorship of a journal managed by Meumann he underlined their friendship and concluded that '[...] each has to follow his own star in this respect'.[64] His comments to Münsterberg were even more explicit: 'I don't ask for gratitude anyway. If, however, someone wants to show his gratitude by his own choice, this can only happen in this way, by working solidly, diligently and carefully, without caring about authorities or his career'.[65] Modest and accommodating as this may sound, we should take this encouragement of independence with a grain of salt. When the work of an independent former student diverged too much from what he considered valuable scholarship,

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[59] Haupt, Edward, J., 'Laboratories for Experimental Psychology: Göttingen's Ascendancy over Leipzig in the 1890s,' in: Rieber, Robert W. and David K. Robinson (eds.), *Wilhelm Wundt in History: The Making of a Scientific Psychology*, Springer Science + Business Media, New York, 2001, 205–250.
[60] Boring, Edwin G., *A History of Experimental Psychology*, 343.
[61] Mack, Wolfgang, 'Wundt's programmatische Erbe,' in: Jüttemann, Gerd (ed.), *Wilhelm Wundts anderes Erbe: Ein Missverständniss löst sich auf*, Vandenhoeck & Ruprecht, Göttingen, 2006, 232–243. 241.
[62] On Kraepelin, see: Steinberg, *Der Briefwechsel zwischen Wilhelm Wundt und Emil Kraepelin*; On Külpe, see: Gundlach, Horst, 'Oswald Külpe und die Würzburger Schule,' in: Janke, Wilhelm and Wolfgang Schneider (eds.), *Hundert Jahre Institut für Psychologie und Würzburger Schule der Denkpsychologie*, Hogrefe Verlag für Psychologie, Göttingen, 1999, 107–124 and Lindenfeld, David, 'Oswald Külpe and the Würzburg School,' *Journal of the History of the Behavioral Sciences*, 14, 1978, 132–141; On Meumann, see: Müller, Paul, *Ernst Meumann als Begründer der experimentellen Pädagogik*, E. Kalberer, Bazenheid, 1942. 7–15 and Probst, Paul, *Bibliographie Ernst Meumann: Mit einer Einleitung zur Biographie*, Traugott Bautz, Herzberg, 1991; On Münsterberg, see: Keller, Phyllis, *States of belonging: German-American intellectuals and the First World War*, Harvard University Press, Cambridge (MA), 1979. 21–30 and Landy, Frank J., 'Hugo Münsterberg; Victim or Visionary?,' *Journal of Applied Psychology*, 77(6), 1992, 787–802.
[63] Oswald Külpe to Wilhelm Wundt, 22 September 1895, UAL, Nl. Wilhelm Wundt. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[64] Wilhelm Wundt to Ernst Meumann, 20 December 1904, UAL, Nl. Wilhelm Wundt. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[65] Wilhelm Wundt to Hugo Münsterberg, 12 November 1890, UAL, Nl. Wilhelm Wundt. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)

he often found means to criticise him. We have seen that his relationship with Münsterberg, for instance, was awkward at times.[66] When he did not agree with the gist of his work, Wundt argued that it lacked in solidity, diligence and care. He even argued that this could be interpreted as ungratefulness from Münsterberg.[67]

In summary, I found that a professor like Wundt could demand loyalty from his former students in at least two ways. He asked those who were unable to make their career independently to produce work that fit his own research programme. His more successful former students and assistants were asked to live up to broader standards of good scholarship. In return former associates could count on his support in multiple ways. Wundt's willingness to publish the work of Kiesow, Lange, Lipps and Merkel, while they had unfulfilled academic ambitions is one example of such support. Another is provided by Wundt giving the editing job on Fechner's unfinished manuscript to Lipps.[68] The most common way, however, to advance the career of former pupils was by helping them in obtaining an academic appointment. This is why Avenarius, Meumann and Lipps were all hired at Wundt's former Chair of Inductive Philosophy in Zürich.[69] And Wundt was not the only one working behind the scenes to ensure professorial appointments for his most trusted students. De Goeje put a large amount of effort into lobbying the professoriate and the Mayor of Leiden into convincing them to reserve a professorial chair for his best student, Christiaan Snouck Hurgronje.[70] Behring's efforts to bring Wernicke to Marburg also fit this pattern.

On the rare occasion that a former teacher refused to support a promising former student, it was occasionally considered acceptable for the latter to explicitly distance himself from his senior. This happened rarely, however. Even Behring, whose relationship with Koch was uneasy at best, tried to avoid the 'semblance of disloyalty'. When he criticised another staff member of Koch's Institute for Infectious Diseases, he therefore sent his polemical manuscript to Koch before publication.[71]

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[66] Münsterberg was, for example, the most prominent former student of Wundt who was not invited to contribute to the two volume *Festschrift* for Wundt's seventieth birthday, published as volume 19 and 20 of the *Philosophische Studien* in 1902. See Introduction, 1–2.
[67] Wilhelm Wundt to Hugo Münsterberg, 12 November 1890, UAL, Nl. Wilhelm Wundt. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[68] See, Chapter 2, 73.
[69] Münsterberg was almost hired there as well, but when his appointment was delayed, he spoiled his chances with an angry and impatient letter to the philosophical faculty of Zürich: Theodor Vetter to Wilhelm Wundt, 20 March 1897, UAB: NA Wundt/III/1501–1600/1519/65–68.
[70] Michael Jan de Goeje to Theodor Nöldeke, 4 November 1888, UBL: BPL 2389. The mayor of Leiden, Louis Marie de Laat de Kanter, was also one of the custodians of Leiden University. Snouck would not accept the professorship in Malay arranged by De Goeje. He entered state service in the Dutch East Indies and only returned to the Netherlands in 1906 to succeed his former teacher.
[71] Emil Behring to Erich Wernicke, 11 April 1892, BNd: EvB/B 1/204.

Among the people discussed in this study Nöldeke was the only one who openly severed his ties to his teacher, Heinrich Ewald.

As a teacher, Ewald had not been very accommodating but when Nöldeke started teaching alongside him, in 1860, their relationship deteriorated even further.[72] Looking back he recounted that 'Ewald was without even a trace of humour. Whoever had a little bit of independence (*Selbständigkeit*), could not stay on good terms with him in the long run'.[73] Nöldeke also started to feel increasingly uneasy about the way in which Ewald presented himself. He often cast himself in the role of 'guardian of faith' and accused his peers of moral and religious shortcomings. One of his favourite antagonists was Heinrich Leberecht Fleischer, 'who, I hope, does not want to betray Christianity as well as the Evangelical faith'.[74] Fleischer mockingly noted that '[...] in the end, we fully owe it only to [Ewald's] magnanimity and forbearance that we quasi-scholars still exist; if he would wish to destroy us, it would take him, like the JHWH of the Old Testament, just one breath and we would be gone!'[75] This self-presentation was a long shot away from the rationalistic and secular approach to scholarship that Nöldeke advocated.[76] When he was appointed as *Ordinarius* in Kiel in 1868 Nöldeke broke with Ewald with a letter in which he stated that he would 'never ever stop honouring [him] as a groundbreaking researcher and my foremost teacher' but that he had to reserve for himself 'the full freedom to express [his] honest scholarly conviction unhindered by personal considerations'.[77] Ewald was furious, but most of Nöldeke's other colleagues, including De Goeje and Fleischer, fully approved of his decision.

The larger peer group and the reputation of the discipline

For most scholars, the relationship with their everyday colleagues was closer than with peers in faraway places. This physical distance between colleagues continued to exist, even though the late 19th century has often been described as a time during which an increasing number of scholarly

[72] On Ewald as a teacher, see Chapter 1, 32.
[73] Theodor Nöldeke to Ignaz Goldziher, 22 February 1904, MTAK, GIL/32/01/160. (accessed at http://opac.mtak.hu/F?RN=511094755)
[74] Ewald, Heinrich, 'Ueber die heutige sicherheit des verständnisses und der anwendung der Bibel,' *Jahrbücher der biblischen Wissenschaft*, 12, 1865, 26–64. 37. For more on Fleischer's far reaching influence on German orientalism, see Chapter 2, 73–77.
[75] Heinrich Leberecht Fleischer to Michael Jan de Goeje, 11 March 1865, UBL: BPL 2389.
[76] This approach is, for example, reflected in his book reviews, see Chapter 3, 100.
[77] Theodor Nöldeke to Heinrich Ewald, 4 October 1869, in: Littmann, Enno, 'Theodor Nöldeke,' in: *Nachrichten von der Gesellschaft der Wissenschaften zu Göttingen: Geschäftliche Mitteilungen aus dem Berichtsjahr 1930/31*, Weidmannsche Buchhandlung, Berlin, 1931, 48–57. 56.

congresses facilitated an ever-growing sociability among scholars.[78] But even in an era that offered increasing opportunities for meeting one's peers, most scholars were not particularly close to each other. Visits to international congresses, which have received considerable attention in recent scholarship, did not feature very prominently in the careers of many scholars.[79] Access to such congresses was often limited; participation required an invitation or being nominated by one's government. Therefore, these events usually were hosted for only the most well-established and widely respected scholars. National and local congresses were more accessible. National scholarly societies, such as the German Oriental Society, often organised annual meetings that combined a plenary assembly meeting, the reading of scholarly papers and opportunities for socialising. The number of people who occasionally visited such meetings was significantly higher than the number of visitors of the more prestigious international congresses.

This does not mean, however, that national and local congresses were faithfully visited and enjoyed by all. Wundt, for example, was not a keen visitor of congresses. He once wrote to Külpe that he was 'not a friend of congresses anyway, least of all of the international ones'.[80] Behring missed out on congresses for a whole different reason. He was disliked by many of his colleagues and although he wanted to visit their congresses, his peers simply did not invite him to some of their meetings.[81]

In addition, scholars could be disappointed by what they got out of their congress attendance; they did not always facilitate learned discussions and the making of new scholarly acquaintances. Snouck Hurgronje, for example, strongly criticised the International Orientalist Congress in Stockholm. This particular congress had been organised by a man he thoroughly disliked — count Carlo Landberg — who managed to be the centre of attention at numerous festive ceremonies.[82] He therefore complained that 'everything, fun and seriousness, was aimed to put one man […] in a false magic light'.[83] Years later, Snouck also complained that a congress in Athens had 'more or less

[78] An interesting overview of the increasing occurrence of scholarly congresses is provided in: Feuerhahn, Wolf and Pascale Rabault-Feuerhahn, 'Présentation: la science à l'échelle internationale,' *La Fabrique internationale de la science. Les congrès scientifiques internationaux de 1865 à 1945. Revue germanique internationale*, 12, 2010, 5–15. 5.
[79] Relevant present-day studies of international congresses include: Everett-Lane, 'International Scientific Congresses' and Feuerhahn and Rabault-Feuerhahn, *La Fabrique internationale de la science.*
[80] Wilhelm Wundt to Oswald Külpe, 1 January 1909, UAL, Nl. Wilhelm Wundt. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm)
[81] Ferdinand Hueppe to Emil von Behring, 13 February 1906; Emil von Behring to Friedrich Althoff, 15 February 1906, GStA PK, VI. HA, Nl Althoff, No. 668.
[82] For more on the Stockholm congress, see: Rabault-Feuerhahn, Pascale, '"La science la robe au vent" Le Congrès Internationaux des Orientalistes et la disciplinarisation des études Orientales,' in: Chiss, Jean-Louis and Dan Savatovski, *La Disciplinarisation de la linguistique. Histoire et épistémologie, Histoire épistémologie langage*, Dossier en ligne, 5, 2012, 1–16.
[83] Christiaan Snouck Hurgronje to Theodor Nöldeke, 4 May 1890, in: Koningsveld, *Orientalism and Islam*,19. Snouck Hurgronje was not the only scholar who was disappointed after the Stockholm congress, see: Rabault-Feuerhahn, Pascale, '"La science la robe au vent"'.

been swallowed by the celebration of the anniversary of the university'.[84] So, although the increasing frequency of all types of congresses facilitated a certain measure of 'banding together' as well as the development of a shared 'identity as professionals' the ties between most individual scholars remained weak compared to the relationships they developed with everyday collaborators and pupils.[85]

The relative weakness of these personal relationships decisively shaped the way in which these casual scholarly acquaintances reviewed each other. Harshly critical reviews were widely accepted. In the third chapter we have encountered numerous examples, ranging from very personal accusations of dilettantism and carelessness to reproaches of dogmatism and mysticism. Because such criticisms were often politely phrased, not even the rare but rude occurrence of anti-Semitic dog whistling automatically resulted in indignant objections. Nöldeke's sneer that it 'requires much less subjection of reason, to believe in the 'authenticity' of the Pentateuch […] than to believe in the infallibility of the pope' was perfectly acceptable, as well. The same is true of Wundt's wry observation that '[...] the reading of this work could be recommended as a good exercise for future natural scientists and especially physiologists. They could learn some very striking examples from this of what they should *not* do, when they aim to engage in research'.[86]

Most of the reviews written by Nöldeke and Wundt were not this caustic, as, of course, many of the works they reviewed simply did not merit ridicule or harsh criticism. Another reason may have been the fact that editors and publishers were often unwilling to print a large number of negative reviews. Even if this preference might not always have encouraged individual authors to write positive reviews, it did motivate editors to make sure that the reviews they published were written by authors who were known to be relatively lenient.[87] A third factor contributing to the large number of positive reviews, could have been that these concerned favours to friends rather than the genuinely positive judgement by a critical peer. A final reason could have been that, even when reviewers did not feel a particularly strong loyalty towards any individual author, they often did feel such loyalty towards the scholarly community to which both reviewer and reviewee belonged.

Nöldeke was very explicit about this last reason in a letter to Goldziher in which he praised him for the apparent mildness of a review: 'Your mildness is able to hide to a certain extent from the laymen that the work has no particular value, while you point that out sufficiently for the person, who is somewhat in the know. I remember that Benfey […] sometimes said that reviews of

[84] Christiaan Snouck Hurgronje to Theodor Nöldeke, 7 May 1912, in: Koningsveld, *Orientalism and Islam*, 95.
[85] Everett-Lane, 'International Scientific Congresses,' 222–223.
[86] Quoted in Chapter 3, 101.
[87] See, Chapter 2, 75.

scholarly works should be such that they do not put the non-scholarly world at risk to develop a low opinion of the scholarly work, at which it is directed'.[88] Benfey and Nöldeke argued that the tendency to assert one's independence from other scholars and critically scrutinise their work should be kept in check by loyalty to a scholarly community, a willingness to promote its prestige, and an effort to safeguard its reputation in the eyes of a lay audience. A similar appreciation of a Goldziher's lenience is present in Snouck Hurgronje's obituary of him, in which he remembers that he 'never forgot the respectful piety, which he deemed mandatory for him to show to his elders [and] the necessary indulgence of the weaknesses of his contemporaries'.[89] He did, however, add that 'the only reproach that I sometimes made him, concerned his almost deceptive mildness in the reviewing of products of doubtful quality' — an accusation that Nöldeke agreed to.[90] The distinction between loyal lenience and deceptive mildness was not always easy to make.

In the third chapter I have already emphasised one reason for a reviewer to choose for either lenience or criticism. Both Nöldeke and Wundt proved to be significantly more critical of academic outsiders than of their peers with university affiliations.[91] Even if they felt some obligation to live by Benfey's maxim, they did not recognise any requirement to apply his guideline to outsiders. At the same time their reviews of their university peers where not always mild enough to guarantee that the outside world would be prevented from developing a low opinion of academic work. Wundt's sneer about an author providing an example of how *not* to do science quoted above, was, for example, made in a review of a book by an *Ordinarius* from Jena. Nöldeke's repeatedly stated disapproval of religiously inspired dogmatism and Wundt's insistent criticism of what he considered to be old-fashioned and unscientific mysticism, suggest that they did not mind discrediting certain subgroups of the academic community in public — especially those who advocated a school of thought to which they were strongly opposed.

Public criticism was not limited to book reviews. The founding of the *Philosophische Studien* by Wundt can be seen as a more ambitious attempt to publicly criticise the proponents of other schools of thought, based on the accusation that they were wasting time on insignificant questions on issues like 'a typo in Kant'.[92] Loyalty towards one's peers as advocated by Benfey had to be

[88] Theodor Nöldeke to Ignaz Goldziher, 2 January 1910, MTAK, GIL/32/01/235. (accessed at http://opac.mtak.hu/F?RN=511094755) Theodor Benfey taught Sanskrit in Göttingen during the time Nöldeke was associated with its university as a student and a *Privatdozent*.
[89] Snouck Hurgronje, Christiaan, 'Ignaz Goldziher. 22 juni 1850 – 13 november 1921,' *De Gids*, 85, 1921, 489–499. 494.
[90] Theodor Nöldeke to Christiaan Snouck Hurgronje, 7 December 1921, UBL: Or: 8952 A: 767.
[91] See Chapter 3, 110–16.
[92] On the aspirations of the *Philosophische Studien*, see Chapter 2, 73; On the sharp disagreements among the representatives of different schools of thought in late 19th-century German, see Chapter 4, 150–155.

balanced with the acceptance of in-depth and sometimes harsh criticism of these same colleagues. Philosophical and religious assumptions underlying scholarship were acceptable reasons for such criticism, even if a reputation as a loudmouthed polemicist or vocal ideologist could hurt one's career. Even though few people would have argued that outspoken early-career scholars, such as Avenarius and Behring, were obliged to refrain from voicing their strong opinions, both men had trouble finding a suitable position at a German university. Avenarius' assumed religious radicalism and Behring's polemic disposition were explicitly mentioned as reasons against hiring them.[93] Nöldeke and Wundt, on the other hand, were hardly affected by their stands against religious dogmatism and philosophical mysticism. This can be explained both by the fact that their criticisms were in line with the influential intellectual current of *Kulturprotestantismus* and by the circumstance that they published most of their sharpest criticism after they had already secured the comfort of a full professorship.[94]

Institutions I: scholarly journals

Scholars not only had to balance their relationship with other scholars, they were also supposed to maintain a good relationship with other parties, such as journals, faculties, industry and state authorities. This section takes a closer look at the relationship between individual researchers and scholarly journals. The subsequent sections look into their relationships with universities, faculties, industry and the state.

First, it is important to note that the relationships between academics and scholarly journals can vary. As the editor and founder of his own journal, for instance, Wundt's relationship with the *Studien* was very different from that of most scholars with the journals in which they published. Wundt could use the pages of the *Studien* to engage in self-chosen polemics, although this had not been the initial reason to found the journal. During the first decades of his career Wundt never even considered founding one. This only became attractive when he established his own laboratory and the hard work of his collaborators caused an ever-increasing production of papers. The fact that Richard Avenarius was unwilling to publish most of these papers in the *Vierteljahrsschrift für wissenschaftlichen Philosophie* encouraged Wundt to argue for the founding of his own journal with his long-time publisher Rudolf Engelmann. Even if Engelmann had some initial doubts about publishing doctoral dissertations, he could not refuse Wundt's request because he was one of his

[93] On Avenarius, see Chapter 4, 148–150; on Behring, see Chapter 4, 142–146.
[94] On Nöldeke's and Wundt's relationship with *Kulturprotestantismus*, see Chapter 3, 125.

best-selling authors. From 1881 onwards the *Studien* would therefore enable Wundt to offer his loyal associates an easy road to getting their research published. The most loyal and talented of his collaborators could count on his long-term loyal support. Even after they had left Leipzig to pursue academic careers elsewhere, trusted former collaborators, such as Kiesow, Lange, Lipps,and Merkel, would not have to worry about publishing opportunities.[95]

If the *Studien* are an example of a scholarly journal that can be seen as both one man's assertion of independence and a gesture of loyalty, the *Zeitschrift der Deutschen Morgenländische Gesellschaft* can be perceived of as a model of a public sphere of scholarship within which questions of loyalty and independence had to be continuously balanced. The society publishing the *Zeitschrift*, the *Deutsche Morgenländische Gesellschaft*, presented itself as the representative body of all German orientalists and the *Zeitschrift* was therefore supposed to both reflect the variety of opinions among them and to project an image of unity within this very diverse community. Its editors often found themselves making case-by-case decisions about the publication of possibly divisive contributions.

After the publication of Friedrich Heinrich Dieterici's *Thier und Mensch*, for instance, Fleischer looked for a reviewer who would write a mild review of the controversial book. After the publication of an article by Eduard König in the *Zeitschrift*, the editorial board felt obliged to also publish Gustav Jahn's polemic reaction, even if its members shared a low opinion of its merits. They did, however, edit Jahn's most offensive statements out of his paper. After the publication of August Fischer's criticism of Karl Barth in the *Zeitschrift* Barth's indignant reply was rejected. His strong wordings were deemed unfit for inclusion. The fact that Fischer was himself a member of the editorial board may have played a role here as well. Finally, Fleischer's careful handling of the discussion about Konstantin Schlottmann's role in the purchase of the forged Moabitica provides another example of a case in which loyalty to the peer group was at least as important as independent criticism in the eyes of the leadership of the *Gesellschaft* and its *Zeitschrift*.[96]

Fleischer's and Fischer's case-by-case management of the *Zeitschrift* was guided by three rules of thumb. The first was a requirement of basic politeness and abstention of *ad hominems*.[97] When Jahn accused König of dull-witted orthodoxy and a lack of moral courage, these offensive words were deleted from his paper. In a similar fashion Fleischer sounded relieved after the debate on the Moabitica, when he observed that the discussion had been guided by a 'spirit of moderation,'

[95] See Chapter 2, 69–73.
[96] For a more extensive treatment of the above examples, see Chapter 2, 75–81.
[97] Raf de Bont also observes the requirement to refrain from *ad hominems*, but finds that not everybody lived up to this standard: De Bont, Raf, "'Writing in letters of blood": Manners in scientific dispute in nineteenth-century Britian and the German lands,' *History of Science*, 51, 2013, 309–335.

'mutual respect,' and 'avoidance of all personal remarks and offensive insinuations'.[98] A second rule of thumb suggested that members of the *Gesellschaft* should have the right to share their critical responses to anything published in the *Zeitschrift* on its pages, as well. This is why Jahn's diatribes were eventually published in an only slightly edited form. This is also why the decision to reject Barth's reply to Fischer caused so much outrage. A final rule of thumb stipulated that no debate should be allowed to linger on in the *Zeitschrift*. This was the easiest way to make sure that the journal would not turn into an arena for prolonged polemics. Therefore, König was allowed one last reply to Jahn, but Jahn was not given another chance to retort. This is also why a lenient review of Dieterici's book could be expected to prevent a more hostile debate from occurring; once one review of his book had been published, the editors would have a fair reason not to print a second, potentially more inflammatory, review. With these three rules of thumb, the leadership of the *Gesellschaft* and the editors of the *Zeitschrift* were able to create a public sphere of scholarship in which loyalty to the peer group was encouraged but not strictly enforced, while independent stances were tolerated but kept within well-defined bounds.

These same rules of thumb did not guide Wundt's editing of the *Studien* as is illustrated by his repeated attacks on Stumpf.[99] The *Literarische Centralblatt*, however, shared many of its editorial policies with the *Zeitschrift*. The most rabid of Gutschmid's anti-Semitic dog whistles did not fit the norms of polite debate and, therefore, did not make it to print; reviewees were allowed to reply to harsh and potentially unjust reviews; finally, no discussion was allowed to linger on after the original reviewer had been given the opportunity to reply to a reviewee's comments.

However, the moral economy of scholarly evaluation was not the only economy shaping the policies of the *Centralblatt*. Unlike the *Studien* and the *Zeitschrift,* the continued existence of the *Centralblatt* depended on its profitability in a market of both scholars and an educated lay audience. Therefore, the number of its reviews as well as their length and tone could not be exclusively based on shared ideas about a fair moral economy of scholarship. It was, for example, important to review more books than the *Jenaer Literaturzeitung* to compete in this niche market. It was also essential not to publish too many overly long reviews in order to safe space for advertisements. In addition, the journal's publisher, Eduard Avenarius, assumed that its readership would not appreciate too many negative reviews; he argued that the journal might not be taken seriously if it reviewed too many books that its own reviewers considered to be unworthy of such close scrutiny.[100] Therefore, there was at least some pressure on the *Centralblatt*'s reviewers to write relatively positive reviews. In

[98] UBT: Md 782 A 68, Heinrich Leberecht Fleischer to Theodor Nöldeke, 21 July 1875.
[99] See Chapter 4, 151–152.
[100] Eduard Avenarius to Friedrich Zarncke, 28 May 1864, UBLE: NL 249/1/A/565.

regard to the *Centralblatt,* it is therefore difficult to disentangle the demands rooted in the moral economy of scholarly evaluation and those grounded in the economy of the marketplace.

Institutions II: universities, faculties and research institutes

Another institution that played a major role in every academic's life was, of course, the university and especially the faculty or research institute at which he was employed. Worries about the functioning of one's faculty or institute were the perfect excuse to be critical of one's peers. This is especially obvious in the confidential reports about the Institute for Oriental Languages.[101] Even if Carl Arendt wrote his scathing report at the explicit request of Althoff, he still felt the urge to justify his account by referring to the greater good: 'Everywhere I have attempted to hold back my own *judgement*, I have, however, attempted to put together with a certain completeness those facts that I know of, which seem suitable to contribute to the clarification of the not very pleasant overall atmosphere at the Institute.[102] Martin Hartmann's similarly harsh opinions about some of the Institute's staff members were likewise justified by the 'the state of affairs there, which cannot but lead to severe damage and complications'.[103] References to the good of a faculty or institute also frequently occurred, however, in environments not characterised by distrust and infighting.

The discussion about Otto Heubner's appointment in Berlin is an example.[104] None of Althoff's advisors expressed any doubts about his merits as a paediatrician. Even Eduard Külz, who strongly objected to Heubner's candidacy, admitted that some of his works were important and that his lectures were 'not bad'.[105] His warnings not to hire Heubner were largely grounded in his conception of the university of Berlin as the elite teaching institution of the unified German *Reich*. This sentiment was widely shared in late 19th-century Germany; the contemporary historian Max Lenz, for example, emphasised that, after the establishment of the *Reich,* it was its university that turned Berlin into 'a sanctuary and centre of German nature and art and all intellectual freedom'.[106] In Külz's eyes, Heubner's Saxon accent and unpretentious demeanour did not suit the dignity and best interest of an institution like this. Though Külz's words are, of course, a judgement of Heubner

[101] See Chapter 4, 131–134.
[102] Professor Arendt über die Zustände am Seminar für Orientalische Sprachen. 4.VII 1889, GStA PK, VI. HA, Nl Althoff, No. 156. Arendt's emphasis.
[103] Martin Hartmann to 'Sehr geehrter Herr Kreisschulinspektor,' 26 September 1889, GStA PK, VI. HA, Nl Althoff, No. 156.
[104] See Chapter 4, 138–142.
[105] Eduard Külz to Friedrich Althoff, 13 November 1893, GStA PK, VI. HA, Nl Althoff, No. 112.
[106] Lenz, Max, *Geschichte der königlichen Friedrich-Wilhelms-Universität zu Berlin*, zweiter Band, zweite Hälfte: auf dem Wege zur deutschen Einheit im neuen Reich, Verlag der Buchhandlung des Waisenhauses, Halle an der Saale, 1918. 357.

as an individual scholar he weighed his appraisal of him against his assessment of the perceived interests of the university. His loyalty to a very specific metropolitan ideal of this particular institution trumped his appreciation of the scholarly, educational and managerial merit of his Leipzig colleague.

Heinrich Fritsch's advice to Althoff went even further than Külz's recommendations by being almost exclusively concerned with the state of affairs at his university.[107] This is most clearly shown by his recommendation for the succession of Friedrich Müller. In the light of the low number of students, the limited number of patients at the university clinics and the current division of work among the remaining staff members, he successfully argued against the appointment of a successor. His later recommendation of Otto Küstner for the Chair of Gynaecology was likewise grounded in the challenges facing the medical faculty at Breslau. Only a man of independent means, Fritsch argued, could be expected to refrain from private practice in order to fully focus on his academic responsibilities.[108]

The search for a balance between regard for the interest of individual scholars and regard for the needs of prospective employers was not limited to medical faculties. The 1894 discussions about the Berlin Chair of Philosophy also illustrate such considerations.[109] Most of Althoff's correspondents were unwilling to argue that either Wilhelm Wundt, Carl Stumpf, or Georg Elias Müller was superior as a teacher or researcher. They all fitted the faculty's profile of a scholar with both psychological and experimental experience. Wundt, however, was easily discarded. Not only was it unlikely that he would leave Leipzig, he was also disliked by many Berlin faculty members. The choice between Müller and Stumpf was more difficult. In the end most advisors emphasised that Stumpf was more likely to fit in than Müller. Müller's emphasis on the experimental side of psychology was so strong that it could be expected that his future endeavours would be almost indistinguishable from those of the physiologists at the medical faculty. Stumpf, however, was not only interested in experimental philosophy but also aspired to contribute to aesthetics and ethics. These additional interests did not make him a more accomplished scholar than Müller, but they did provide a convincing argument that he was a better fit for the Berlin Faculty of Philosophy.

The above analysis of appointment procedures shows how hard it was for 19th-century German scholars to unambiguously point out one of their peers as more accomplished than the others.

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[107] See Chapter 4, 134–138.
[108] At least one 20th-century scholar argues that these scholars of independent means, who he calls 'rentier intellectuals' (*Rentenintellektuellen*) who were willing and able to refrain from certain monetary rewards were instrumental in managing an ever-expanding university system: Busch, 'The Vicissitudes of the *Privatdozent*,' 331.
[109] See Chapter 4, 152–154.

Even Heubner's most vocal detractors did not claim that he was somehow falling short as a researcher or clinician. In a similar fashion Fritsch refrained from judging Küstner's merits as a researcher, teacher and clinician. Instead, he emphasised his wealth, health and assumed willingness to accept the vacant position. Likewise, Müller and Stumpf were both depicted by most correspondents as highly qualified candidates for a Berlin professorship. The most significant point of comparison in this case was to what extent they would be a good fit with the Berlin philosophy department. The fact that so much praise was given to all candidates suggests a basic loyalty among peers. It was considered impolite to harshly criticise those who had already proven to be viable candidates for a professorial chair. A display of loyalty towards a hiring institution, however, was a socially acceptable way to assert one's independence from the demands of collegial politeness. Even if it was impolite to question the merit of a qualified colleague, it was perfectly acceptable to point out that he was not the best candidate for a specific appointment.

Institutions III: state and society

Scholars also cultivated relationships outside the confines of the academic community. The interests of commercial stakeholders and governments shaped the working environment of a substantial number of scholars. Some of the case studies have already pointed at this influence. The commercial considerations of the Avenarius, Brill and Engelmann publishing houses affected the opportunities of Zarncke, De Goeje and Wundt.[110] The pursuance of both loyalty and independence played an important role in all of the above examples. Engelmann tried to secure the loyalty of their successful author Wundt, while De Goeje needed the loyal long-term support of his publisher Brill to finish his al-Ṭabarī edition. Some of the other case studies have also emphasised the importance of a good relationship with government authorities. The discussion about professorial appointments is decisively shaped by the relationship between individual scholars, faculties and the Ministry of Education.[111] Meanwhile, faculty boards tried to assert their independence from the government and hoped that their preferences would guide the ministry's eventual hiring decisions.

Sometimes, the demands on a scholar's loyalty even transcended his relationship with the state and its representatives, and turned into an explicitly voiced attachment to entities, such as 'society' and 'the nation.' At the outbreak of the First World War, for example, a large number of German

[110] See Chapter 2, 66–67 and 83.
[111] See Chapter 4, 154–157.

intellectuals came to the defence of the German war objectives.[112] Soon after the start of the war, on 14 September 1914, Wundt gave a lecture at one of Leipzig's biggest venues, the *Alberthalle* of the *Krystallpalast*, where thousands of people gathered to hear the old philosopher lambasting Germany's opponents and arguing for the expansion of Germany's sphere of influence.[113] This lecture was published to great acclaim under the title *Über den wahrhaften Krieg* (On the true war).[114] Wundt's peers in oriental studies joined the conversation, as well. Nöldeke's younger colleague Carl Heinrich Becker, for instance, published a number of brochures eulogising the close relationship between Germany and its new ally, the Ottoman Empire.[115]

The broad societal commitment of bacteriologist like Koch and Behring had already been established long before the outbreak of the war. Finding a cure for deadly diseases is after all even more important to people suffering from these diseases than to scholars with a primarily scholarly interest. The significance of their work was wholeheartedly acknowledged in contemporary news media, in which they were often depicted as noble warriors, fighting for a world without deadly disease. Robert Koch was, for example, depicted as a brave horseman wielding his microscope to fight the threatening snake of tuberculosis (Figure 1) and Behring was drawn as answering to the desperate prayers of a mother by wrestling a child from the greedy hands of Death himself (Figure 2).

The examples of the World War and the fights against deadly diseases are rather straightforward. As such they do not provide a clear picture of the way in which state and industry contributed to a complex entanglement of aspirations to loyalty and independence among scholars. A further elaboration on one of the case studies from the preceding chapter — the development of Behring's academic career — can shed light on these relationships. In the previous chapter, the messy negotiations towards his eventual appointment in Marburg were used to illustrate how Althoff could overrule faculty preferences. The following sections will set out how Behring carefully negotiated a precarious balance between loyalty to and dependence on both the chemical industry and the Prussian state, to further his own career as a researcher.

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[112] See, for example, Mommsen, Wolfgang J., 'Einleitung: Die deutschen kulturellen Eliten im Ersten Weltkrieg,' in: Mommsen, Wolfgang J., *Kultur und Krieg: Die Rolle der Intellektuellen, Künstler und Schriftsteller im Ersten Weltkrieg*, Oldenbourg, München, 1996, 1–15 and the discussion in: Keller, *States of belonging*.

[113] Bruendel, Steffen, *Volksgemeinschaft oder Volksstaat: Die "Ideen von 1914" und die Neuordnung Deutschlands im Ersten Weltkrieg*, Akademie, Berlin, 2003. 36–37.

[114] Wundt, Wilhelm, *Über den wahrhaften Krieg*, Alfred Kröner, Leipzig, 1914.

[115] Becker, Carl Heinrich, *Deutschland und der Islam*, Deutsche Verlags-Anstalt, Stuttgart and Berlin, 1914; Becker, Carl Heinrich, *Deutsch-türkische Interessengemeinschaf*, Friedrich Cohen, Bonn, 1914. See also: Engberts, Christiaan, 'Orientalists at War: Personae and Partiality at the Outbreak of the First World War,' in: Engberts, Christiaan and Herman Paul (eds.), *Scholarly Personae in the History of Orientalism, 1870–1930*, Brill, Leiden, 2019. 176–177.

Robert Koch fighting tuberculosis. Source: Ligon, B. Lee, 'Robert Koch: Nobel Laureate and Controversial Figure in Tuberculin Research,' *Seminars in Pediatric Infectious Diseases*, 13(4), 2002, 289–299. 296.

Emil Behring fighting death. Source: BNd: EvB/S 27.

Institutions IV: between state and commerce

Behring's increasing closeness to state and industry went hand in hand with a growing isolation from his peers. This development took place in the aftermath of the development of his diphtheria serum. After it was established that the serum was suitable for mass-production, it was produced and distributed by the *Hoechster Farbwerke* from 1894 onwards. Suddenly Behring was a wealthy man. In 1894, the serum generated a profit of 444,200 Mark, which increased to a staggering 764,800 Mark the next year.[116] Behring's first five-year contract with the *Farbwerke* stipulated that he would receive half of these profits, which meant that he received more than half a million Mark, during the first two years of the commercial production of his serum.[117] The enormity of this figure

[116] These number are listed at a note that reads '*Diphtherie-Heilserum*': BNd, EvB/B 196/115. I take this to be a list of annual profits of the serum because a report by *Farbwerke* director Laubenheimer states that the profit for 1894 was 444,200 in 1894, which is the same number as occurs in this list: BNd, EvB/B 196/7.
[117] Vertrag zwischen Emil von Behring und den Farbwerken Vormals Meister, Lucius & Brüning betreffend 'Gewinnung von Diphterie-Heilserum', § IV, EvB/B 196/2/4.

is best understood in comparison to the lump sum that academic best-seller author Wundt received around the same time for his *Grundriss der Psychologie*, a comparatively modest 2,000.[118]

Behring's sudden wealth changed the relationship with his peers for the worse. Above, this chapter already points at how financial disagreements damaged his relationship with Wernicke. Something similar happened to his relationship with Paul Ehrlich. Behring and *Farbwerke* director August Laubenheimer convinced Ehrlich to refrain from the financial benefits from the commercial production of the serum and promised him the directorship of an independent organisation for serum testing, instead.[119] Ehrlich could not resist their pressure but soon regretted this. Behring and Laubenheimer could not live up to their promise on short notice and when Ehrlich was finally put in charge of the newly established *Institut für Serumforschung und Serumprüfung* (Institute for Serum Research and Serum Testing) he was disappointed by Behring's attitude towards it. Behring seemed to perceive of the Institute as just another organisation contributing to his own research programme rather than as an independent body.

The relationship between Behring and Ehrlich never recovered. Althoff tried to repair it but met with Ehrlich's indelible distrust over and over again. A collaboration with Behring, he argued, could only be expected from 'a slavish character, but not from a researcher of independence (*Selbständigkeit*) with the greatest thirst for freedom (such as I am, after all)'.[120] Even Ehrlich's wife wrote to Althoff to stress how unpleasant a renewed collaboration would be: 'I know exactly that my husband can only work successfully when he follows a straight, self-chosen path and when he enjoys the full freedom of his creative work. […] If despite everything he would decide to work with Behring, I want to beg you that his free, independent (*unabhaengige*) status will be guaranteed and that he can terminate this association at any time'.[121]

Meanwhile Behring felt that his other peers kept him at a distance, too. He was well aware of the resistance against his appointment in Marburg.[122] As mentioned earlier in this chapter he started to notice that his colleagues had stopped inviting him to meetings as well.[123] During the most sorrowful days of his life, when he had withdrawn himself at the Neuwittelsbach sanatorium, he found time for self-reflection and came to a sad conclusion: '[...] among the many reproaches I

[118] Emanuel Reinicke to Wilhelm Wundt, 14 October 1895, UAL, Nl. Wilhelm Wundt. (accessed at http://home.uni-leipzig.de/wundtbriefe/home.htm) Reinicke had become co-owner and manager of Wundt's publisher Engelmann after the death of Rudolf Engelmann.
[119] Hüntelmann, *Paul Ehrlich*, 104.
[120] Paul Ehrlich to Friedrich Althoff, 12 September 1903, GStA PK, VI. HA, Nl Althoff, No. 709.
[121] Hedwig Ehrlich to Friedrich Althoff, 13 September 1903, GStA PK, VI. HA, Nl Althoff, No. 709.
[122] See Chapter 4, 142–145.
[123] This chapter, 172.

make myself, one of the most severe is that through the years I have lost one friend after the other by my own fault'.[124] But, while he was bad at maintaining good relationships with other scholars, Behring carefully cultivated those with influential representatives of the state and the pharmaceutical industry, especially with Friedrich Althoff and August Laubenheimer.

Behring highly valued his independence. One reason to carefully cultivate his relationship with state and industry was that he could guarantee this independence by pitting institutional agents against each other. In Behring's eyes, there was a risk that his highly profitable relationship with the *Farbwerke* would come at the expense of his scholarly credibility. Therefore, his university position in Marburg was very important to him. During one particularly vicious conflict with the direction of the *Farbwerke* he indignantly underlined that he did not consider himself to be a 'slave of the industry'.[125] To make sure that he would retain the possibility to be this outspoken towards the management of the *Farbwerke,* he had already started to lobby Althoff for a professorial appointment: 'How would I be able to safeguard my authoritative and independent position towards the *Höchster Farbwerken* when me and my institute are dependent on their goodwill. How would I be able […] to follow up on the threat that I will immediately associate myself with another industrial enterprise?'[126] Shortly after making this argument Behring was appointed in Marburg.

His relationship with the *Farbwerke* was, however, not only a threat to Behring's independence. During the second half of the 19th century, collaboration with the industry had gained intellectual respectability, not the least because commercial manufacturers were increasingly managed by university graduates.[127] Laubenheimer, for example, had been an *Extraordinarius* for chemistry in Giessen.[128] Behring, therefore, confidently drew on his association with *Hoechst* to assert his independence from the state and the university system. He carefully cultivated his relationship with the *Farbwerke*, complaining to Laubenheimer that, if Althoff would arrange a professorship for him, his 'hands would be even more tied than before' and telling him that he was considering to 'throw off all still existing chains' that kept him from financially benefiting from his work.[129]

His close ties with the industry allowed him to maintain some independence from the ministry. In 1894 Althoff was considering the establishment of a state institute for serum production, but he was not sure if he could find funding. Without promising anything he asked Behring if he could pre-empt the eventual establishment of such an institute by immunising about twelve horses at the

[124] Emil von Behring to Erich Wernicke, 12 December 1908, BNd: EvB/B 1/273.
[125] Emil von Behring to Die Direktion der Farbwerke Höchst/Main, 27 July 1903, BNd: EvB/B 196/78.
[126] Emil Behring to Friedrich Althoff, 8 February 1895, GStA PK, VI. HA, Nl Althoff, No. 325.
[127] Borscheid, Peter, *Naturwissenschaft, Staat und Industrie in Baden (1848–1914)*, Ernst Klett, Stuttgart, 1976. 115, 127.
[128] Linton, *Emil von Behring*, 147.
[129] Emil Behring to August Laubenheimer, 30 March 1894, BNd: EvB/B 196/9.

expense of the state.[130] Behring, however, refused this and argued that 'as long as a more profitable practical activity has not been secured for me through the establishment of a central institute warranted by the state, I cannot decide — in the interest of the cause — to give up the cooperation with the works so successfully carried out at the *Höchster Farbwerke*'.[131] When his professorial appointment was delayed by the opposition in Marburg he again pointed at the opportunities offered by the *Farbwerke*. If he would not get the desired professorial chair, Behring argued, 'nothing is left for me but to secure another place of employment for me and my loyal collaborators'.[132] The most promising offer for such secure employment had been made by the *Farbwerke*.

Another way for Behring to maintain a measure of independence from the Prussian state was to refer to the willingness of other states to hire him. In his correspondence with Althoff he repeatedly mentioned the opportunities in Petersburg and Budapest. He also painstakingly detailed all the honours bestowed on him during a trip to France between December 1894 and February 1895, where he had visited the *Institut Pasteur* and enjoyed the good weather and company in Cannes.[133] After his appointment in Marburg he would continue to use similar threats to ensure government funding. As late as 1906, Otto Naumann, an official at Althoff's department, would wearily mention that '[Behring] finally played the familiar trump card again, that if the educational administration does not protect his interests, he will go to Bavaria where the government will accommodate him with open arms'. Naumann continued his complaint, stating that 'it is typical that he played the same trump card with colleague Elster; here, he demanded that the *Extraordinariat* for internal medicine in Marburg should be occupied by a child's clinician, which he needed for his work, and if that would not happen, then off to Bavaria he would go'.[134]

Finally, Behring did not only pit the Prussian ministry and the *Hoechster Farbwerke* against each other, he also used his relationship with both institutions to assert his independence from Koch's Institute for Infectious Diseases. His frustrations at the Institute were threefold. In the first place some people of his own generation seemed to advance faster than him in the institutional hierarchy. In 1891 Behring complained to Wernicke that he was too old to still be held accountable to others who were of his own age or even younger.[135] Secondly he grew frustrated with being subordinated to Koch, who could either command him to divert his attention from his diphtheria research or,

[130] Friedrich Althoff to Emil Behring, 15 November 1894, GStA PK, VI. HA, Nl Althoff, No. 326.
[131] Emil Behring to Friedrich Althoff, 17 November 1894, GStA PK, VI. HA, Nl Althoff, No. 325.
[132] Emil Behring to Friedrich Althoff, 24 December 1894, GStA PK, VI. HA, Nl Althoff, No. 325.
[133] This is the case is all of Behring's letters between 24 December 1894 and 8 February 1895 in: GStA PK, VI. HA, Nl Althoff, No. 325.
[134] Otto Naumann to Friedrich Althoff, 18 February 1906, GStA PK, VI. HA, Nl Althoff, No. 668.
[135] Emil Behring to Erich Wernicke, 22 November 1891, BNd: EvB/B 1/177.

even worse, could order other members of the institute to work on similar things. He felt pressured into accepting Ehrlich's help in determining the dosage of his serum and was annoyed about the fact that Koch had appointed someone else to look into the possibility of creating a blood serum against cholera.[136]

The third and final issue was the fact that, as a military doctor employed at a state institute, he could not patent his serum for commercial purposes. To reap the monetary benefits from his discovery he would have to quit both the military service and his assistantship at Koch's institute. He complained to the management of the *Farbwerke* that if he would stay at the institute he would 'indeed have the fame of having accomplished a scientific achievement in the eyes of later generations' but he 'would have to leave the fruits of his labour to the colleagues with business experience'.[137] He further emphasised his willingness to collaborate with the *Farbwerke* by promising to dedicate his new book to Laubenheimer.[138] The combination of the *Farbwerke*'s willingness to invest in his research and Althoff's stubborn attempts to find him a professorial chair enabled Behring to break with Koch and the military to actively pursue his own financial interests.

The moral economy of scholarship and its limits

The scholars discussed in this study had to take care of their relationships with a large number of interested parties. Friends, colleagues, students, *Doktorvaters*, universities, faculties, institutes, editorial boards, professional societies, state authorities and commercial enterprises competed for their loyalty while they simultaneously strived to be independent professionals and intellectuals. With so many actors involved it was a challenge for each individual scholar to find an appropriate balance between loyal collegiality and an independent critical stance. But this myriad of different ties did not only provide a challenge, it also offered opportunities. Close ties to one person, group, or institution allowed a scholar to assert his independence from others. The attempt to find a balance between loyalty and independence can therefore be characterised as an effort to balance the expectations and demands of all these different interested parties. This study has provided a

[136] On Ehrlich, see: Emil Behring to Friedrich Althoff, 8 February 1895, GStA PK, VI. HA, Nl Althoff, No. 325; on the development of a cholera blood serum, see: Emil Behring to Friedrich Althoff, 24 December 1894, GStA PK, VI. HA, Nl Althoff, No. 325.
[137] Emil Behring to August Laubenheimer, 30 March 1894, BNd: EvB/B 196/9.
[138] August Laubenheimer to Emil Behring, 5 April 1894, BNd: EvB/b 196/10. This seems to have been an empty promise, however, no book published by Behring in the years 1894–1896 is dedicated to Laubenheimer. Still, the relationship between Behring and Laubenheimer was quite close: the guest book mentioned in note 34 shows that Laubenheimer paid friendly visits to the Behring family, see the entries for 19 June 1896 and 12 May 1900.

large number of examples of people pursuing their personal ambitions by choosing to strategically address different people and institutions.

Nöldeke, for example, only announced his independence from his *Doktorvater* Ewald, after two conditions were met. In the first place, he had finally been appointed a full professorship in Kiel and, secondly, he knew that he could count on the loyal support of the influential circle of colleagues that had taken shape around Fleischer. De Goeje's career provides another example of cleverly addressing various actors. If he would have relied exclusively on the support that his own university and close colleagues could offer, he never would have been able to finish his ambitious al-Ṭabarī edition. However, because he was also able to secure the support of a loyal publisher and government officials, he could finish his project after three decades of collaborative work. Later in his career, he would again bring together his enterprising publisher, an international consortium of scholars, a number of scholarly societies and the support of different governments. This time, it would be to work on the *Encyclopaedia of Islam*, the first edition of which was only published after his death under the supervision of his former students Christiaan Snouck Hurgronje and Theodor Houtsma.[139]

Wundt, in his turn, cleverly used the high sales numbers of his books to convince his publisher to also support a less commercially attractive project. The *Studien* would never yield Engelmann any significant profits but Wundt was able to collect and disseminate the work of his pupils and assistants. This was not the first time that Wundt experienced the freedom that could be gained by writing bestselling books. He could afford to quit his first salaried academic position, his time-consuming assistantship in Helmholtz's Heidelberg laboratory because he could support himself with the revenues of his book sales.[140] Within Wundt's circle Münsterberg is another example of someone who actively tried to advance his career through the cultivation of his ties with people outside his academic peer group. Among other things he collaborated with Althoff on the establishment of the *Amerika Institut* and the *Internationale Wochenschrift*.[141] These efforts can be interpreted as an attempt to sidestep the rather unpromising conventional route of waiting for a

[139] The long process that led to the publication of the *Encyclopaedia of Islam* is extensively covered in: Zande, Daniel van der, 'Martinus Th. Houtsma: 1851–1943: Een bijdrage aan de geschiedenis van de Oriëntalistiek in Nederland en Europa,' (diss., Universiteit Utrecht, 1999). 225–260.
[140] Bringmann, Bringmann and Balance, 'Wilhelm Maximilian Wundt,' 25–27.
[141] For Münsterberg's involvement with the *Amerika Institut*, see: Freitag, Christian H., 'Die Entwicklung der Amerikastudien in Berlin bis 1945,' (diss., Freie Universität Berlin, 1977). 39–49; for Münsterberg's involvement with the *Wochenschrift*, see: Minutes of the conference on the *Internationale Monatsschrift* by Wilhelm Lexis, 3 January 1907. GStA PK, VI. HA, Nl Althoff, No. 295. The initial plan to publish a *Monatschrift* (monthly journal) was eventually replaced by the plan to publish a weekly journal instead.

professorial nomination by a German faculty by cultivating close ties to government authorities to further his academic career instead.

The bacteriologists and hygienists around Koch had even better opportunities to address a myriad of interested parties. Within governments circles the interest in new cures was not limited to Althoff's ministry. The Ministry of War also followed the developments in this field with great interest. This is illustrated by Behring's position as a military doctor stationed at Koch's institute during the development phase of the diphtheria serum. As discussed earlier in this chapter, industrial enterprises were highly interested as well. After all, they could count on a huge consumer demand if they could introduce new and affordable cures for widespread diseases to the market. In addition, work on such lofty goals as the eradication of a disease often attracted the interest of wealthy private donors. The work of Paul Ehrlich, for example, was extended beyond his tasks at the Institute for Serum Research and Serum Testing and its Frankfurt successor, the *Institut für experimentelle Therapie* (Institute for Experimental Therapy). He carried out most of his research into the possibility of a chemotherapeutic cure for cancer in the so-called Georg-Speyer-Haus, paid for from the bequest from the banker Georg Speyer, after mediation by industrialist and amateur historian of science Ludwig Darmstaedter.[142]

This far-reaching interest of state, industry and private donors in the development of new cures did not, however, make the relationship between researchers and other involved parties unproblematic. Even if money could buy some independence, wealthy scholars were vulnerable to the charge that their work was inspired by a love for money rather than by their dedication to scholarship. An overt love for money was considered to be in conflict with both internationally shared ideals of scholarship and the self-image of the German bourgeoisie.[143] August Wilhelm Hoffmann — a main player in the emergence of the close relationship between academic chemists and the German industry — was quite typical when he neglected his industrial relationships and instead emphasised his 'pure fascination with the knowledge of truth and beauty, free from the pursuit of material benefit'.[144] Behring's earlier statement about not wanting to be a 'slave of the industry' was a reaction to this concern, as well. The legitimacy of his worries is illustrated by the

[142] Hüntelmann, *Paul Ehrlich*, 280–284.
[143] In the scholarly community such ideals guided the oftentimes somewhat awkward discussion about the patenting of inventions and innovations, see: Lucier, Paul, 'Court and controversy: patenting science in the nineteenth century,' *British Journal for the History of Science*, 29, 1996, 139–154. 141; For an in-depth analysis of the ambiguous attitude towards the collecting of wealth in 19th-century German bourgeois circles, see: Hodenberg, Christina von, 'Der Fluch des Geldsacks. Der Aufstieg des Industriellen als Herausforderung bürgerliche Werte,' in: Hettling, Manfred and Stefan Ludwig Hoffmann (eds.), *Der bürgerliche Wertehimmel: Innenansichten des 19. Jahrhunderts*, Vandenhoeck & Ruprecht, Göttingen, 2000, 79–104.
[144] Quoted in: Borscheid, *Naturwissenschaft, Staat und Industrie*, 125.

fact that Ehrlich tried to discredit Behring in the eyes of Althoff more than once by highlighting his commercial endeavours.[145] In addition, Behring had to defend himself against accusations of greed in the professional journals as well.[146] And to make matters even worse, even the popular press occasionally accused him of an inappropriate love of money.[147]

In her analysis of the moral economy of scholarship, Lorraine Daston contrasted her take on science with a traditional view in which the suggestion that 'science depends in essential ways upon highly specific constellations of emotions and values has the air of proposing a paradox'.[148] She then introduced the idea of the existence of a moral economy of science as an alternative to this old-fashioned position and states that 'certain forms of empiricism, quantification and objectivity itself […] require moral economies'. In this study I have not focused on the development of such guiding intellectual concepts as empiricism, quantification and objectivity. I have instead focused on how the everyday practices of scholarship forced individual academics to find a balance between loyal collegiality and critical independence. Just like Daston's focus on concepts this study's emphasis on the experience of individuals draws attention to the importance of 'highly specific constellations of emotions and values'. Some of the values discussed in this study are easily categorised as the sort of epistemic virtues that fit a traditional take on science. Reviews of scholarly work, for example, often contained references to such highly valued qualities as industriousness, caution, ingenuity and critical ability.[149] Most of the emotions, values and commitments described in this study, however, explicitly refer to the relationships between individual scholars as well as to ties between academia and an outside world with its own moral — sometimes different — economies.

From the point of view of individual 19th-century scholars, the balancing act by which they asserted their place within the moral economy of scholarship actually consisted of a number of different balancing acts. In the first place, they had to balance collegial loyalty and critical independence in their relationships with various individuals in academia, such as colleagues, students, supervisors and collaborators. The many hierarchies in German academia only further complicated these

[145] In one letter Ehrlich argues that Behring cannot be seen as 'an impartial expert' because 'as producer he has pecuniary interests in the highest degree'. This last comment has been crossed out but is still easily readable: Paul Ehrlich to Friedrich Althoff, [no date, presumably January 1900], NBd: EvB/B 169/1; See also: Paul Ehrlich to Friedrich Althoff, 17 February 1906, GStA PK, VI. HA, Nl Althoff, No. 668.
[146] Behring, Emil, 'In eigener Sache,' *Deutsche Medizinische Wochenschrift*, 37, 15 September 1898. 595.
[147] One folder in the Althoff archives, for example, contains an overview of the discussion about Behring's attempt to get a US patent for his blood serum and a comical fictional report of a visit to Behring's laboratory from the short-lived Berlin satirical magazine *Das Narrenschiff*, in which Behring convinces the unwitting reporter to pay for a newly developed serum against the 'hardening of the heart': GStA PK, VI. HA, Nl Althoff, No. 358.
[148] Daston, Lorraine, 'The Moral Economy of Science,' 3.
[149] See Chapter 3, 117–119.

balancing efforts. Secondly, scholars had to balance their relationship with the institutions at the heart of scholarship, such as universities, research institutes, scholarly journals and professional organisations. Thirdly, they had to deal with expectations that were not primarily grounded in conceptions of scholarship, including such widely differing commitments as those to friendship, industry, the state and the nation.

A large number of people and institutions did not clearly fall within only one of these categories. The editor of a scholarly journal was sometimes seen as a colleague, while at other times he could be viewed as the representative of a journal. A research institute could be seen as the primary focus for research or as a result of purposeful state policies. An official at the Ministry of Education could be seen as an influential individual with whom one should cultivate a good relationship, but he was more commonly perceived as a major representative of the state. This difficulty in clearly defining the character of certain relationships, draws attention to what might have been the most challenging task of finding a balance between all the commitments listed above.

This was a constant struggle. A scholar reviewing a friend's work had to decide whether he would choose to promote the work of a friend or to criticise the work of a colleague. At the same time there was some ambiguity about how harsh a reviewer could be about anyone's work at all. Recommending colleagues for professorial chairs also involved a balancing act; should one recommend a scholar one personally liked, a man whose work one admired, a scholar representing one's own school of thought, or someone who would be most likely to respond to the needs of the institution advertising the vacancy? A strong interest of the state and the industry in one's research posed further challenges. To what extent should one allow these institutions to set one's research agenda? To what extent should one be willing to share the financial benefits of one's involvement with these institutions with other researchers?

The keeping of all these balances was a complex necessity because in all relationships both loyalty and independence were at stake. A close friendship could be the starting point of a detailed and thorough criticism of each other's work. Even when students and collaborators were proud to be their *Doktorvater*'s academic offspring, they often distanced themselves from some of his teachings. Most reviews that were published contained both criticism and praise. A strong dependence on industry money could be balanced by a state appointment at a university, even if these same industrial relationships served to safeguard a scholar's independence from the demands made by the same state. Although all the protagonists of this study worked in very different disciplines, the continuous struggle to balance loyal collegiality and independent criticism was a recurring feature of all their careers. The many guises in which this question of balance pops up, again and again,

attests to the centrality of loyalty and independence to the moral economy of scholarship in late 19th and early 20th-century Germany.

The thesis that the moral economy of scholarship can fruitfully be described as a balancing act between loyal collegiality and critical independence can further be illustrated by taking another look at the earlier thick descriptions of scholarly communities by Kohler and McCray. The moral ethos of communality that Kohler found among his fly people can be interpreted as a reflection of the value put on loyalty among individual researchers as well as among various institutes. In this particular case, the balance between loyalty and independence is a very peculiar one. It was exactly the loyal sharing of the abundant data and *Drosophila* cultures that allowed for the emergence of independent initiatives. In this state of abundance every researcher was able to develop his or her own line of research exactly because he or she had access to a wealth of data and cultures to build on. As noted, McCray's paper describes a very different moral economy characterised by a continuous competition between various research institutes with widely differing resources. Different as it may be, this moral economy fits the model sketched in this chapter as well. Especially the struggles of the less well-to-do institutes can be interpreted as a continuous effort to assure their independence from their better-endowed competitors. Although this study largely focuses on the efforts of individual scholars to find a balance in their relationship with other individuals and institutions, McCray's example suggests that an emphasis on balancing loyalty and independence might also be a fruitful starting point for an analysis at the institutional level.

Epilogue:
The Moral Economy of 21st-Century Scholarship

An analysis of scholarly dos and don'ts through the prism of moral economies lends itself very well to synchronic comparison. Indeed, one of the main advantages of the concept of moral economies is that it allows for transdisciplinary comparisons of the kind undertaken in this thesis. Although German Orientalists, experimental psychologists, and bacteriologists engage in rather different research practices, each with their own instruments, techniques and methods, it is on the level of moral economies that their work could be compared and even proved to be related in demanding a fine balance between independence and loyalty.

This epilogue supplements the synchronic analysis presented in this report with a brief diachronic account of what has changed — or remained the same — since the days of Koch, Nöldeke and Wundt. At first sight, the continued significance of loyal collegiality and critical independence might seem self-evident; most researchers still try to conduct original research in a collegial atmosphere. However, it cannot be taken for granted that the equilibrium favoured by 19th-century researchers corresponds with the balance pursued by modern-day academics. Over the 20th century, scholarship has gone through many changes and this is expected to have left its mark on its moral economies. This is even more likely because the notion of 'moral economy' explicitly allows for continuous reassessment of commitments, as well as for readjustments in the hierarchies between different virtues.[1]

Still, despite all changes, the moral demands placed on modern academics are not fundamentally different from those made on Koch, Nöldeke, and Wundt. Important discontinuities between 'then' and 'now' notwithstanding, late 19th-century moral economies still make their impact felt today. On the following pages, I therefore reflect on the way in which my analysis is not just a contribution to the history of scholarship around 1900, but also reveals patterns of moral reasoning, the enduring influence of which can be detected even in contemporary academia.

A century of change?

Just how suddenly moral economies could change already became visible during the First World War. In an upsurge of what is called 'scientific nationalism', German scholars with long and

[1] See, Introduction, 9.

successful careers reassessed commitments that had been taken for granted for a long time.[2] During this war, loyalty to the nation outweighed other virtues of scholarship. Carl Heinrich Becker explicitly acknowledged this in a letter to his Dutch friend and colleague Christiaan Snouck Hurgronje: 'For us Germans, even our scholarly ideals fade into the background as insignificant in this great time, now that our fatherland is struggling for its existence'.[3]

This was only the first of many changes that would significantly alter the scholarly landscape of the 20th century. The Second World War and the Cold War also left their respective marks on scholarship as practiced in and outside of universities, as did decolonisation and globalisation. Of special interest, however, are the transformations of the practices and ideals of scholarship that touch directly on the themes discussed in this thesis, most notably the funding of scholarly research, its organisational structure, the relationship between scholarship and industry, the degree of international collaboration, and the culture of scholarly publishing.

The funding of research projects has changed considerably from the early 20th century onwards. As described in this report, 19th-century German academia was largely funded by government. However, from the early 20th century onward, private funding by foundations such as the John D. Rockefeller Foundation and the Andrew Carnegie Foundation became increasingly important.[4] These foundations developed highly competitive selection processes to ensure that only the most promising research proposals would be rewarded. After the Second World War, new government agencies for the promotion of scientific research were established in many countries.[5] These organisations were often modelled after the aforementioned private foundations and adopted similar competitive selection processes.

The increase in these new funding arrangements was closely related to a major change in the organisational structure of research, namely that of the emergence of what is commonly referred to as Big Science. From the 1960s onwards, this term was used for highlighting certain characteristics of post-World War II science that many considered to be distinctively modern. Derek de Solla Price emphasised that science had grown in many different ways and he, therefore,

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[2] On scientific nationalism, see: Crawford, Elisabeth*, Nationalism and internationalism in science, 1880–1939: Four studies of the Nobel Population*, Cambridge University Press, Cambridge, 1992. Chapter 2, 28–46.
[3] Carl Heinrich Becker to Christiaan Snouck Hurgronje, 17 September 1914, UBL: Or. 8952 A: 149. For more details, see: Engberts, 'Orientalists at War'.
[4] For example, see Huistra, Pieter and Kaat Wils, 'Fit to Travel: The Exchange Programme of the Belgian American Educational Foundation: An Institutional Perspective on Scientific Persona Formation (1920–1940),' *Low Countries Historical Review*, 131(4), 2016, 112–134. 116 and Kohler, Robert, *Partners in Science: Foundations and Natural Scientists 1900–1945*, University of Chicago Press, Chicago (IL), 1991. 3.
[5] For example, the *Nederlandse Organisatie voor Wetenschappelijk Onderzoek* (NWO, Netherlands Organization for Scientific Research) and the National Science Foundation (NSF) in 1950, and the *Deutsche Forschungsgemeinschaft* (DFG, German Research Foundation) in 1951.

started his analysis with the observation that all indicators 'show with impressive consistency and regularity that if any sufficiently large segment of science is measured in any reasonable way, the normal mode of growth is exponential'.[6] Alvin Weinberg, who had coined the term a few years earlier, described Big Science as 'much more complicated' than earlier 'Little Science' and as requiring 'extremely elaborate equipment and staffs of large teams of professionals'.[7] More recent authors argue that the roots of Big Science — both in the humanities and the STEM fields — are much older. They can, for example, be traced back to the team of scholars working on Theodor Mommsen's *Corpus Inscriptiorum Latinarum* and the physical and chemical laboratories of the 19th century.[8] Most contemporary scholars, however, associate Big Science with the 'big organizations, big machines, and big politics' that became increasingly common at the time of the Cold War.[9] The emphasis on 'big politics' provides a further insight into the working of post-World War II Big Science; for its funding, it is often dependent on institutions founded on the initiative of national or international political actors. The research programmes and priorities established under the aegis of these actors shape the careers of a large number of researchers. In Europe, such research programmes have been advanced by the founding of national organisations for scientific research, large-scale international collaborative efforts, and, more recently, the European Research Council.[10]

The increasing importance of foundation funding and the growth in Big Science have been accompanied by an ever-more intricate entanglement of research and industry. This interconnectedness has taken different shapes. For example, 20th-century researchers and their academic employers have demonstrated a growing interest in the commercial viability of their findings. A recent study even starts with the assertion that it 'is clear that universities need to become more entrepreneurial, changing their strategies, their structures and their practices, changing their culture and helping students and faculty members to develop their entrepreneurial mindsets and entrepreneurial actions'.[11] At the same time, a growing number of university educated

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[6] De Solla Price, Derek J., *Little Science, Big Science*, Columbia University Press, New York (NY), 1963. 4–5.
[7] Weinberg, Alvin M., *Reflections on Big Science*, M.I.T. Press, Cambridge (MA), 1967. 39.
[8] Daston, Lorraine, *Before the Two Cultures: Big Science and Big Humanities in the Nineteenth Century*, Israel Academy of Sciences and Humanities, Jerusalem, 2015. 9–10; Nye, Mary Jo, *Before Big Science: The Pursuit of Modern Chemistry and Physics 1800–1940*, Harvard University Press, Cambridge (MA), 1996. xvi.
[9] Hallonsten, Olof, *Big Science Transformed: Science Politics and Organization in Europe and the United States*, Palgrave Macmillan, New York (NY), 2016. 17.
[10] Examples of national organizations have can be found in note 5. On European scientific collaboration outside of the European Union framework, see: Hallonsten, Olof, 'Myths and realities of the ESS project: A systematic scrutiny of readily accepted 'truths',' in: Kaiserfeld, Thomas and Tom O'Dell, *Legitimizing ESS: Big Science as a collaboration across boundaries*, Nordic Academic Press, Lund, 2013, 43–66. 55–56. On the European Research Council, see: Hoenig, Barbara, *Europe's New Scientific Elite: Social Mechanisms of Science in the European Research Area*, Routledge, London, 2017. 111–112.
[11] Fayolle, Alain and Dana T. Redford, 'Introduction: towards more entrepreneurial universities — myth or reality?' in: Alain Fayolle and Dana T. Redford (eds.), *Handbook on the Entrepreneurial University*, Edward Elgar, Cheltenham, 2014.

researchers found employment in the industrial sector rather than in academia. Steven Shapin points out that, as early as in the 1950s, more than half of all US scientists and research engineers were working in industry.[12] Such connections between academia and industry were not entirely new. The last chapter presents the example of Emil Behring's close collaboration with the *Höchster Farbwerke*.[13] This rapprochement of university and industry in 19th-century Germany is also pointed out by others.[14] In modern scholarship, however, the convergence of academia and entrepreneurship has become so inescapable that one commentator feels justified in plainly stating that the 'sale of science is a relatively new phenomenon, and it follows the modern ways of business, rather than the ancient ways of science'.[15]

Another development related to the growth in Big Science and entrepreneurship is that of the increasingly international character of scholarship. International contacts were of course not completely absent, in the late 19th century. From the 1860s onwards, an increasing number of international scientific congresses were organised all over Europe.[16] The consortium that was formed for De Goeje's al-Ṭabarī edition provides another example of international collaboration, in this period.[17] In the 21st century, however, science became more international than ever. This is perhaps best illustrated by the fact that a remarkably large share of scholarly communication is carried out only in English. A recent author even refers to contemporary scientists as 'the most resolutely monoglot international community the world has ever seen'.[18] This is in stark contrast with the linguistic practices of the early 20th century. The proceedings of the international Orientalist congress in Hamburg, in 1902, were quite typical in accepting papers in the three major languages of science: English, French, and German.[19] In addition, the attending Italians submitted their contributions in their own language, which had been a prominent language of science, until the early 19th century.[20] The contribution of the Albanian poet, philologist, and former Jesuit, Ndre Mjeda, was published in the scholarly *lingua franca* of an even earlier era: Latin.[21]

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[12] Shapin, Steven, *The Scientific Life: A Moral History of a Late Modern Vocation*, University of Chicago Press, Chicago (IL), 2008. 110.
[13] See, Chapter 5, 182–186.
[14] For example: Borscheid, Peter, *Naturwissenschaft, Staat und Industrie*.
[15] Greenberg, Daniel S., *Science for Sale: The Perils, Rewards, and Delusions of Campus Capitalism*, University of Chicago Press, Chicago (IL), 2007. 2.
[16] Crawford, *Nationalism and internationalism in science*, 38–39.
[17] See, Chapter 1, 37.
[18] Gordin, Michael D., *Scientific Babel: The Language of Science from the Fall of Latin to the Rise of English*, Profile Books, London, 2015. 2.
[19] *Verhandlungen des XIII. Internationalen Orientalisten-Kongresses. Hamburg September 1902*, Brill, Leiden. 1902.
[20] Gordin, *Scientific Babel*, 10.
[21] Miedia, Andreas, 'De pronunciatione palatalium in diversis albanicae linguae dialectis,' in: *Verhandlungen des XIII. Internationalen Orientalisten-Kongresses. Hamburg September 1902*, 14–15. Mjeda's visit to the congress is mentioned in: Gawrych, George W., *The Crescent and the Eagle: Ottoman Rule, Islam and the Albanians, 1874–1913*, I.B. Tauris, London, 2006. 89.

Foundation funding, Big Science, scholarly entrepreneurship, and internationalisation have left their respective marks on the academic community. Although men such as Koch, Nöldeke, and Wundt were personally acquainted with most other influential scholars in their fields of research, the modern-day scholarly community is too big to facilitate such close personal relationships between the most prominent researchers in each discipline. This has created a larger but more impersonal academic community, in which a new culture of publishing has taken shape. As described, the evaluation of manuscripts in 19th-century Germany was often facilitated by an informal exchange of perspectives — either between friends and colleagues or between authors and editors.[22] Today, the most visible form of scholarly evaluation is the double-blind editorial peer review. This form of evaluation was not very common in scholarly publishing until after the Second World War. By the 1960s, this evaluative practice was recognised as a defining feature of modern scholarship.[23] Even though it has been subject to increasing scrutiny, many still consider it to be 'the lynchpin about which the whole business of Science is pivoted'.[24]

Moral Economies in the 21st century

How did these changes in the organisation of scholarly publishing, funding, collaboration, and work environment impact moral economies of the kind existing among scholars in late 19th- and early 20th-century Germany? It might be tempting to conclude that this century-old moral economy has largely been superseded. One could argue that the intimacy of the private correspondence has been replaced by the anonymity of double-blind peer review; that the evaluation of Big Science does not require the same commitment to the creation and maintenance of a community of peers such as promoted by Nöldeke's and Wundt's book reviews, that the pursuit of funding from private foundations at an international level cannot meaningfully be compared to the professorial appointment procedures designed by national governments; or that an old-fashioned commitment to selflessly contributing to the growth of a shared body of knowledge is insurmountably far removed from the increasing willingness to monetise research findings.

[22] The support among friends and colleagues is discussed in Chapter 1, the relation between author and editor is addressed in Chapter 2.
[23] On the relatively recent development of peer review see: Csiszar, Alex, *The Scientific Journal: Authorship and the Politics of Knowledge in the Nineteenth Century*, University of Chicago Press, Chigago (IL), 2018, Chapter 3. One recent author even argues that editorial peer review was not yet a ubiquitous practice until the 1970s: Baldwin, Melinda, 'Scientific Autonomy, Public Accountability, and the Rise of "Peer Review" in the Cold War United States,' *Isis*, 109(3), 2018, 538–558. 543.
[24] Ziman, *Public Knowledge*, 111.

The interest in the virtues and vices of individual scholars, however, did not subside in the late 20th century. This is illustrated by the fact that reports of the moral failure among scholars and its damaging impact on the perceived trustworthiness of scholarship continue to resonate with a wide audience. In the early 1980s, for example, journalists William Broad and Nicholas Wade collected an impressive number of fraud cases at prestigious US research institutes.[25] Their book was a major impulse to the lengthy debate on research ethics.[26] A more recent study by Horace Freeland Judson can be seen as a follow-up to their work. In his 2007 book, Judson wonders whether the pressing questions of research ethics can be framed in terms of individual guilt or innocence.[27] The book subsequently provides an elaborate discussion of fraudulent incidents in molecular and cellular biology — the author's fields of expertise. The continued preoccupation with the supposed moral failure of individual researchers suggests that the idea of a moral economy of scholarship may still hold relevance today.

Widespread worries about the perceived commercialisation of contemporary scholarship support this suggestion. Daniel Greenberg, for example, argues that 'contemporary science is embedded in, and financed by, a society that worships money and profits and celebrates personal wealth,' and at times may find itself at odds with the ancient 'sacred obligation' of the scientist, that of truth-seeking.[28] Steven Shapin pays in-depth attention also to the scientific entrepreneur, defining him as 'one who is both a qualified scientist and, like all commercial entrepreneurs, a risk taker. […] They have one foot in the making of knowledge and the other in the making of artifacts, services, and, ultimately, money'.[29] According to Shapin, these modern-day dilemmas are similar to moral considerations of the past, to the extent that they are best understood as matters of virtue. Therefore, his study's central contention is that 'personal virtue, familiarity, and charisma feature in such characteristically later modern configurations as the industrial research laboratory and the entrepreneurial network'.[30] This emphasis on the continued significance of charisma and familiarity entails a rejection of Max Weber's contention that modernity is characterised by its reliance on institutionalised administrative rules. Shapin, instead, argues that the personal qualities (charisma) and relationships (familiarity) continue to shape research in its different 21st-century guises. The persistent significance of such qualities guarantees the continued modern-day relevance of assessments of scholarly virtue.

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[25] Broad, William and Nicholas Wade, *Betrayers of the Truth: Fraud and Deceit in the Halls of Science*, Touchstone, New York (NY), 1982.
[26] Davis, Michael, *Ethics and the University*, Routledge, London, 1999. 49–52.
[27] Freeland Judson, Horace, *The Great Betrayal: Fraud in Science*, Harcourt, Orlando (FL), 2004. 8.
[28] Greenberg, *Science for Sale*, 5.
[29] Shapin, *The Scientific Life*, 210.
[30] Ibid., 5.

Building on this central contention of Shapin, I believe that my thesis offers insights that are relevant to understanding modern-day scholarship, in at least three ways. In the first place, it draws attention to the continued significance of personal interaction in scholarship. Its importance is often obscured by the genre conventions of the modern-day research paper, which one author describes as 'a story reduced to the elements deemed essential to its outcome, pared [….] of all details of procedure and background that readers sharing the author's professional expertise will be able to supply from their own experience"[31] Even customary sections, such as the introduction and methods section, he argues, can be interpreted as logical components of an analytical structure rather than as a report of actual research proceedings.[32] Even though recent developments, such as the publication of negative results and the sharing of data sets, add transparency to scholarly research, the importance of personal interaction tends to remain unclear.[33] However, footnotes or dedicated sections in which contributions by others are emphasised are very common. These acknowledgements suggest that informal support still shapes the production and evaluation of scholarly knowledge today. The significance of personal interaction is even more obvious in the collaborative efforts of Big Science. Especially virtues such as what one research director calls 'a willingness to submerge personal desires in joint accomplishment' are indispensable in this type of environment.[34]

Secondly, the virtues discussed in this study are still highly valued in the age of Big Science and entrepreneurial scholarship. In an era in which teamwork has become increasingly important, it is quite self-evident that loyal collegiality continues to be highly appreciated. One Nobel Prize winning scientist recently gratefully emphasised that 'collegiality is the great privilege of science, to be a part of that huge international community. It's probably the most cohesive and enlightened international community that exists'.[35] The ideal of critical independence is also as alive as ever. The rationale behind contemporary double-blind peer review processes, for instance, is that the resulting anonymity guarantees that the reviewers will be able to keep a critical distance. In the education of a new generation of scholars, the importance of independence is explicitly stressed,

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[31] Holmes, Frederic L., 'Argument and Narrative in Scientific Writing,' in: Dear, Peter (ed.), *The Literary Structure of Scientific Argument: Historical Studies*, University of Pennsylvania Press, Philadelphia (PA), 1991, 164–181. 180–181.
[32] Ibid., 179.
[33] On publishing negative results, see: Pfeffer, Christian and Bjorn R. Olsen, 'Editorial: Journal of Negative Results in Biomedicine,' *Journal of Negative Results in BioMedicine*, 1(2), 2002, 1–2. On data sharing, see: Schickhardt, Christoph, Nelson Hosley and Eva C. Winkler, 'Researchers' Duty to Share Pre-publication Data: From the Prima Facie Duty to Practice,' in: Mittelstadt, Brent Daniel and Luciano Floridi (eds.), *The Ethics of Biomedical Big Data*, n.p., Springer, 2016, 309–338.
[34] Quoted in: Shapin, *The Scientific Life*, 185.
[35] J. Michael Bishop quoted in: Hargittai, István and Magdolna Hargittai, 'J. Michael Bishop,' in: István Hargittai and Magdolna Hargittai, *Candid Science VI: More Conversations with Famous Scientists*, Imperial College Press, London, 2006, 182–199. 199.

as well. The doctorate regulations at my own institution, Leiden University, underline that the 'dissertation describes research conducted *independently* by the PhD candidate or research to which the candidate has made an essential contribution'.[36]

Finally, this study's focus on the virtues that shaped the moral economy of 19th-century German scholarship invites us to take a closer look at new virtues that have become more important in the age of entrepreneurial scholarship and Big Science. Shapin points out that the developments in this new age have created new professional roles in which scholars are expected to display a novel constellation of virtues. Directors of industrial research facilities, for example, are expected 'to be unusually persuasive,' to be able to build up', over time, a relationship of trust with corporate headquarters,' and to 'actively manage the moral regimes of their laboratories'.[37] They are also supposed to be able 'to accept the reality of compromising about research agenda's,' to know how to 'interact with dissimilar people,' and to accept that a company might demand unquestioning, absolute loyalty within a hierarchical environment.[38] Like Behring, some scholars experience this as a form of corporate slavery, but others thrive in the industrial sector.[39] Scientific entrepreneurs, Shapin adds, need to possess the virtue of 'flexibility' or 'adaptability', as well.[40] Entrepreneurship also requires a measure of bravery to cut ties with the academy and become a 'risk taker' instead.[41]

Regardless of the novel expectations and new opportunities that characterise an academic career in the 21st century, modern-day scholars find themselves in a position that is not fundamentally different from the predicament of 19th-century researchers. For scholars in Wilhelmine Germany, it was evident that the virtues of loyal collegiality and critical independence were central to good scholarship. Though these requirements did not necessarily always clash, they were forced to weigh them up against each other, over and over again. Even if they were often able to find a balance between these virtues, this balance was open to continuous re-evaluation. The virtues of 19th-century scholarship still matter greatly to academic researchers in the 21st century. For many of them, however, it may have become more challenging than ever to find an acceptable balance. One reason for this is the fact that new developments in the structuring of a research career have come with new virtues, such as persuasiveness, flexibility, and the courage to handle the risks that come with scientific entrepreneurship.

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[36] Leiden University Doctorate (PhD) Regulations 2018 (https://www.organisatiegids.universiteitleiden.nl/binaries/content/assets/ul2staff/reglementen/onderzoek/promotiereglement/promotiereglement-2018-eng.pdf, last accessed at 2 May 2019). My italics.
[37] Shapin, *The Scientific Life*, 162.
[38] Ibid., 234–235.
[39] Ibid., 237. For Behring's aversion of becoming a 'slave of the industry,' see Chapter 5, 184.
[40] Ibid., 258.
[41] Ibid., 261, 210.

Balancing both old and new virtues can obviously be very demanding — sometimes maybe unreasonably so. Some contemporary scholars, therefore, renunciate the claims of the modern neoliberal university and, instead, become advocates of what they call 'slow scholarship'. They encourage an ethics of care for oneself and others, 'deep reflexive thought, engaged research, joy in writing and working with concepts and ideas driven by our passions'.[42] Even if this appeal is not explicitly phrased in virtue language, the ideals and dispositions promoted by its authors are not fundamentally different from the emphasis on virtue among Wilhelmine scholars.[43] Although the language of virtue has been common both among 19th-century scholars and modern-day historians of science, contemporary discourse about skills, abilities and sensitivities deals with similar issues. As their predecessors of the 19th century, modern-day scholars have to find some sort of balance, whether it is between virtues, skills, or sensitivities. Similar to their predecessors, they also often find that a hard-won balance remains open to reinterpretation and recalibration. The continuous effort to re-evaluate and recalibrate the balance between old and new scholarly virtues lies at the heart of the 21st-century moral economies of scholarship.

[42] Mountz, Alison et al., 'For Slow Scholarship: A Feminist Politics of Resistance through Collective Action in the Neoliberal University,' *ACME*, 14, 2015, 1235–1259. 1253.
[43] Paul, Herman, *De deugden van een wetenschapper: Karakter en toewijding in de geesteswetenschappen, 1850–1940*, Amsterdam University Press, Amsterdam, 2018. 192.

Bibliography

Unpublished Sources

Behring-Nachlass digital

Correspondence of Emil Behring

EvB/ B 1: Correspondence with Erich Wernicke

EvB/ B 169: Correspondence with Friedrich Althoff

EvB/ B 196: Correspondence with Farbwerke Vormals Meister, Lucius und Brüning

EvB/ S 27: Miscellaneous

Accessed through: https://www.uni-marburg.de/fb20/evbb/behring-digital

Last accessed: 18 June 2019

Geheimes Staatsarchiv Preußischer Kulturbesitz

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VI. HA, Nl Althoff, F. T., No. 156, Orientalisches Seminar, Lehrer, Lehraufträge

VI. HA, Nl Althoff, F. T., No. 159, Orientalisches Seminar: Diversa

VI. HA, Nl Althoff, F. T., No. 313, Das Kochsche Tuberkulin

VI. HA, Nl Althoff, F. T., No. 325, v. Behring. Heilseruminstitut. Briefe v. Behrings

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VI. HA, Nl Althoff, F. T., No. 709, Ehrenberg-Ehrlich

VI. HA, Nl Althoff, F. T., No. 801, Koban-Kögel

Halle-Wittenberg University Archive

Archive of the Deutsche Morgenländische Gesellschaft

Rep. 90:67: Geschäftsführung 1905

Rep. 90:70: Geschäftsführung 1908

Leiden University Library

Correspondence of Michael Jan de Goeje

LUL: BPL: 2389: Personal correspondence of Michael Jan de Goeje

LUL: Or. 5585e: Correspondence about the De Goeje's al-Ṭabarī edition

LUL: Or. 5585f: Correspondence about the De Goeje's al-Ṭabarī edition

Correspondence between Theodor Nöldeke and Christiaan Snouck Hurgronje

LUL: Or. 8952 A: 754-772: Correspondence between Nöldeke and Snouck Hurgronje

Leipzig University Archive

Correspondence of Wilhelm Wundt

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Partially accessible through: https://home.uni-leipzig.de/wundtbriefe/viewer.htm

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GIL/32/01: Correspondence with Theodor Nöldeke

accessed through: https://konyvtar.mta.hu/index_en.php?name=v_3_2_1_goldziher

Last accessed: 18 June 2019

Robert Koch Institut

Correspondence of Robert Koch

as/b1/003: Correspondence with Georg Gaffky

as/b1/007: Correspondence with Richard Pfeiffer

Staatsbibliothek zu Berlin

Sammlung Darmstaedter in der Staatsbibliothek zu Berlin

3b 1882: Correspondence of Robert Koch

3a 1890: Correspondence of Emil Behring

3a 1875: Correspondence of Paul Ehrlich

Nachlass 156: Correspondence of Erich Wernicke

Tübingen University Library

Correspondence of Theodor Nöldeke

Md 782 A 68: Correspondence with Heinrich Leberecht Fleischer

Printed Sources

Arendt, Hans-Jürgen, 'Gustav Theodor Fechner (1801–1887) und die Leipziger bürgerliche Gesellschaft im 19. Jahrhundert,' *N.T.M. Zeitschrift für Geschichte der Wissenschaften, Technik und Medizin*, 9, 2001, 2–14.

Ash, Mitchell G., 'Academic Politics in the History of Science: Experimental Psychology in Germany, 1879–1941,' *Central European History*, 13(3), 1980, 255–286.

Avenarius, Ludwig, *Avenarianische Chronik: Blätter aus drei Jahrhunderten einer deutschen Bürgerfamilie*, O.R. Reisland, Leipzig, 1912.

Backhaus, Jürgen G., 'The University as an Economic Institution: The Political Economy of the Althoff System,' *Journal of Economic Studies*, 20(4,5), 1993, 8–29.

Badie, Bertrand, Dirk Berg-Schlosser and Leonardo Morlino, *International Encyclopedia of Political Science*, Volume 1, SAGE, Thousand Oaks (CA), 2011.

Baker, Robert, Before Bioethics: *A History of American Medical Ethics from the Colonial Period to the Bioethics Revolution*, Oxford University Press, Oxford, 2013.

Baldwin, Melinda, 'Scientific Autonomy, Public Accountability, and the Rise of "Peer Review" in the Cold War United States,' *Isis*, 109(3), 2018, 538–558.

Barth, Jakob, 'Ursemit. *e*, zum Demonstrativ *d̲*, *tī* und Verwandtes,' *Zeitschrift der Deutschen Morgenländischen Gesellschaft*, 59, 1905, 633–643.

_____, *Sprachwissenschaftliche Untersuchungen zum Semitischen*, J.C. Hinrichs'sche Buchhandlung, Leipzig, 1907.

Bäumler, Ernst, *Paul Ehrlich: Forscher für das Leben*, 3., durchgesehene Auflage, Edition Wötzel, Frankfurt am Main, 1997.

Becker, Carl Heinrich, *Deutschland und der Islam*, Deutsche Verlags-Anstalt, Stuttgart and Berlin, 1914.

_____, *Deutsch-türkische Interessengemeinschaf*, Friedrich Cohen, Bonn, 1914.

Beeston, A.F.L. and Lawrence I. Conrad, 'On Some Umayyad Poetry in the History of al-Ṭabarī,' *Journal of the Royal Asiatic Society*, Series 3, 3(2), 1993, 191–206.

Behring, Emil, *Die Blutserumtherapie I: Die praktischen Ziele der Blutserumtherapie und die Immunisierungsmethoden zum Zweck der Gewinnung von Heilserum*, Georg Thieme, Leipzig, 1892.

_____, 'Das neue Diphtheriemittel,' *Die Zukunft*, neunter Band, 1894, 97–109 and 249–264.

_____, 'In eigener Sache,' *Deutsche Medizinische Wochenschrift*, 37, 15 September 1898.

Behring, Emil and Erich Wernicke, 'Ueber Immunisirung und Heilung von Versuchsthieren bei der Diphtherie,' *Zeitschrift für Hygiene*, 12, 1892, 45–57.

Ben-David, Joseph and Randall Collins, 'Social factors in the origins of a new science: the case of psychology,' *American Sociological Review*, 31(4), 1966, 451–465.

Benedum, Jost, 'Georg Gaffky (1850–1918) und die Gründung des Gießener Lehrstuhls für Hygiene im Jahre 1888,' *Gießener Universitätsblätter*, 221, 1989, 91–94.

Benjamin, Marina, 'Introduction,' in: Marina Benjamin (ed.), *Science and Sensibility: Gender and Scientific Enquiry*, 1780–1945, Basil Blackwell, Oxford, 1991, 1–23.

Berger, Silvia, *Bakterien in Krieg und Frieden: Eine Geschichte der medizinische Bakteriologie in Deutschland 1890–1933*, Wallstein, Göttingen, 2009.

Blumenthal, Arthur L., 'Shaping a Tradition: Experimentalism Begins,' in: Claude E. Buxton (ed.), *Points of View in the Modern History of Psychology*, Academic Press, Orlando, 1985, 51–83.

Bod, Rens, *A New History of the Humanities: The Search for Principles and Patterns from Antiquity to the Present*, Oxford University Press, Oxford, 2013.

_____, 'A Comparative Framework for Studying the Histories of the Humanities and Science,' *Isis*, 106(2), 2015, 367–377.

_____, 'Has There Even Been a Divide? A *Longue Durée* Perspective, *History of Humanities*, 3(1), 2018, 15–25.

Bonnell, Victoria E. and Lynn Hunt, 'Introduction,' in: Victoria E. Bonnell and Lynn Hunt (eds.), *Beyond the Cultural Turn: New Directions in the Study of Society and Culture*, University of California Press, Berkeley (CA), 1999, 1–32.

Booth, Christopher C., 'The Origin and Growth of Medical Journals,' *Annals of Internal Medicine*, 113(5), 1990, 398–402.

Boring, Edwin G., 'The Psychology of Controversy,' *The Psychological Review*, 36(2), 1929, 97–121.

_____, *A History of Experimental Psychology*, second edition, Prentice-Hall, Englewood Cliffs (NJ), 1950.

Borscheid, Peter, *Naturwissenschaft, Staat und Industrie in Baden (1848–1914)*, Ernst Klett, Stuttgart, 1976.

Bringmann, Wolfgang G., Norma J. Bringmann and William D.G. Balance, 'Wilhelm Maximilian Wundt 1832–1874: The Formative Years,' in: Wolfgang G. Bringmann and Ryan D. Tweney (eds.), *Wundt Studies: A Centennial Collection*, C.J. Hogrefe, Inc., Toronto, 1980, 13–32.

Bringmann, Wolfgang G. and Ryan D. Tweney (eds.), *Wundt Studies: A Centennial Collection*, C.J. Hogrefe, Inc., Toronto, 1980.

Bringmann, Wolfgang G. and Gustav A. Ungerer, 'The Foundation of the Institute for Experimental Psychology at Leipzig University,' *Psychological Research*, 42 (Wundt Centennial Issue), 1980, 5–18.

Broad, William and Nicholas Wade, *Betrayers of the Truth: Fraud and Deceit in the Halls of Science*, Touchstone, New York (NY), 1982.

Brocke, Bernhard vom, 'Friedrich Althoff: A Great Figure in Higher Education Policy in Germany,' *Minerva*, 29(3), 1991, 269–293.

Broman, Thomas H., 'J.C. Reil and the "Journalization" of Physiology,' in: Peter Dear (ed.), *The Literary Structure of Scientific Argument: Historical Studies*, University of Pennsylvania Press, Philadelphia (PA), 1991, 13–42.

Brown, Penelope and Stephen C. Levinson, *Politeness, Some universals in language usage*, Cambridge University Press, Cambridge, 1987.

Bruendel, Steffen*, Volksgemeinschaft oder Volksstaat: Die "Ideen von 1914" und die Neuordnung Deutschlands im Ersten Weltkrieg*, Akademie, Berlin, 2003.

Burke, Peter, 'The Republic of Letters as a communication system: An essay in periodization,' *Media History*, 18(3–4), 2012, 395–407.

Busch, Alexander, 'The Vicissitudes of the *Privatdozent*: Breakdown and Adaptation in the Recruitment of the German University Teacher,' *Minerva*, 1(3), 1963, 319–341.

Cahan, David, 'The institutional revolution in German physics, 1865–1914,' in: *Historical Studies in the Physical Sciences*, 15(2), 1985, 1–65.

Calder III, William M. and Bernhard Huss, 'Introduction,' in: William M. Calder III and Bernhard Huss (eds.), *"Sed serviendum officio …" The Correspondence between Ulrich von Wilamowitz-Moellendorf and Eduard Norden (1892–1931)*, Weidmann, Hildesheim, 1997, xi–xvii.

Charle, Christophe, 'Patterns,' in: Walter Rüegg (ed.), *A History of the University in Europe*, Volume III, Universities in the Nineteenth and Early Twentieth Centuries (1800–1945), Cambridge University Press, Cambridge, 2004, 33–80.

Cook, Alan, 'Academic Publications before 1940,' in: Einar H. Fredriksson (ed.), *A Century of Science Publishing: A Collection of Essays*, IOS Press, Amsterdam, 2001, 15–24.

Crawford, Elisabeth*, Nationalism and internationalism in science, 1880–1939: Four studies of the Nobel Population*, Cambridge University Press, Cambridge, 1992.

Csiszar, Alex, *The Scientific Journal: Authorship and the Politics of Knowledge in the Nineteenth Century*, University of Chicago Press, Chigago (IL), 2018.

Danziger, Kurt, *Constructing the subject: Historical origins of psychological research*, Cambridge University Press, Cambridge, 1990.

Daston, Lorraine, 'The Moral Economy of Science,' *Osiris*, 10, 1995, 2–24.

_____, *Before the Two Cultures: Big Science and Big Humanities in the Nineteenth Century*, Israel Academy of Sciences and Humanities, Jerusalem, 2015.

Daston, Lorraine and Peter Galison, *Objectivity*, Zone Books, New York (NY), 2007.

Daston, Lorraine and Glenn W. Most, 'History of Science and History of Philologies,' *Isis*, 106(2), 2015, 378–390.

Davis, Michael, *Ethics and the University*, Routledge, London, 1999.

De Bont, Raf, '"Writing in letters of blood": Manners in scientific dispute in nineteenth-century Britian and the German lands,' *History of Science*, 51, 2013, 309–335.

De Solla Price, Derek J., *Little Science, Big Science*, Columbia University Press, New York (NY), 1963.

Diamond, Solomon, 'Wundt before Leipzig,' in: Robert W. Rieber and David K. Robinson (eds.), *Wilhelm Wundt in History: The Making of a Scientific Psychology*, Springer Science + Business Media, New York (NY), 2001, 1–68.

Diestel, Ludwig, 'Die moabitischen Alterthümer,' *Jahrbücher für deutsche Theologie*, 21, 1876, 451–473.

Dieterici, Friedrich, *Thier und Mensch vor dem König der Genien*, Leipz, Hinrichs, 1879.

Domanski, Cezary W., 'A biographical not on Max Friedrich (1856–1887), Wundt's first PhD student in experimental psychology,' *Journal of the History of the Behavioral Sciences*, 40(3), 2004, 311–317.

Dongen, Jeroen van and Herman Paul, 'Introduction: Epistemic Virtues in the Sciences and the Humanities,' in: Dongen, Jeroen van and Herman Paul (eds.), *Epistemic Virtues in the Sciences and the Humanities*, Springer, Cham, 2017, 1–10.

Dülmen, Richard van, *Die Entdeckung des Individuums*, 1500–1800, Fischer, Frankfurt am Main, 1997.

Elkeles, Barbara, 'Der »Tuberkulinrausch« von 1890,' *Deutsche Medizinische Wochenschrift*, 115 (45), 1990, 1729–1732.

Engberts, Christiaan, 'Gossiping about the Buddha of Göttingen: Heinrich Ewald as an unscholarly persona,' *History of Humanities*, 1(2), 2016, 371–385.

_____, 'The Scholar as Judge: A Contested Persona In Nineteenth-Century Orientalism,' *BMGN – Low Countries Historical Review*, 131 (4), 2016, 93–111.

_____, 'Orientalists at War: Personae and Partiality at the Outbreak of the First World War,' in: Engberts, Christiaan and Herman Paul (eds.), *Scholarly Personae in the History of Orientalism, 1870–1930*, Brill, Leiden, 2019. 172–192.

Engberts, Christiaan and Herman Paul (eds.), *Scholarly Personae in the History of Orientalism, 1870–1930*, Brill, Leiden, 2019.

Eskildsen, Kasper Risbjerg, 'Inventing the archive: Testimony and virtue in modern historiography,' *History of the Human Sciences*, 26(4), 2013, 8–26.

Everett-Lane, Debra Adrienne, 'International Scientific Congresses, 1878–1913 : Community and Conflict in the Pursuit of Knowledge' (diss., Columbia University, 2004).

Ewald, Heinrich, 'Ueber die heutige sicherheit des verständnisses und der anwendung der Bibel,' *Jahrbücher der biblischen Wissenschaft*, 12, 1865, 26–64.

Fahrmeir, Andreas, 'Ort des Konsenses oder Historische Streitschrift? Zur Geschichte des 19. und 20. Jahrhunderts in der Historischen Zeitschrift,' *Historische Zeitschrift*, 289, 2009, 199–222.

Fayolle, Alain and Dana T. Redford, 'Introduction: towards more entrepreneurial universities — myth or reality?' in: Alain Fayolle and Dana T. Redford (eds.), *Handbook on the Entrepreneurial University*, Edward Elgar, Cheltenham, 2014.

Fechner, Gustav Theodor, *Elemente der Psychophysik*, Breitkopf und Härtel, Leipzig, 1860.

Feuerhahn, Wolf and Pascale Rabault-Feuerhahn (eds.), *La Fabrique internationale de la science. Les congrès scientifiques internationaux de 1865 à 1945. Revue germanique internationale*, 12, 2010.

_____, 'Présentation: la science à l'échelle internationale,' *La Fabrique internationale de la science. Les congrès scientifiques internationaux de 1865 à 1945. Revue germanique internationale*, 12, 2010, 5–15.

Fischer, August, 'Redakteurglossen', *Zeitschrift der Deutschen Morgenländischen Gesellschaft*, 59, 1905, 442–456.

_____, 'II.,' *Zeitschrift der Deutschen Morgenländischen Gesellschaft*, 59, 1905, 644–671.

_____, 'Allerlei von J. Barth „verbesserte" arabische Dichterstellen,' *Zeitschrift der Deutschen Morgenländischen Gesellschaft*, 61, 1907, 926–938.

Foy, Karl, 'Zur Persönlichkeit Carl Arendt's: Einige Erinnerungen,' *Ostasiatische Studien. Mittheilungen des Seminars für Orientalische Sprachen zu Berlin*, 1, 1902, 177–182.

Freeland Judson, Horace, *The Great Betrayal: Fraud in Science*, Harcourt, Orlando (FL), 2004.

Freitag, Christian H., 'Die Entwicklung der Amerikastudien in Berlin bis 1945,' (diss., Freie Universität Berlin, 1977).

Galison, Peter, *How Experiments End*, University of Chicago Press, Chicago (IL), 1987.

Galison, Peter and Andrew Warwick, 'Introduction: Cultures of Theory,' *Studies in History and Philosophy of Modern Physics*, 29(3), 287–294.

Gawrych, George W., *The Crescent and the Eagle: Ottoman Rule, Islam and the Albanians, 1874–1913*, I.B. Tauris, London, 2006.

Geertz, Clifford, *The Interpretation of Cultures: Selected Essays*, Basic Books, New York (NY), 1973.

Gerdmar, Anders, *Roots of Theological Anti-Semitism: German Biblical Interpretation and the Jews, From Herder and Semler to Kittel and Bultmann*, Brill, Leiden, 2009.

Giddens, Anthony, 'Durkheim as a Review Critic,' *The Sociological Review*, New Series, 18(2), 1970, 171–196.

Goeje, Michael Jan de, *Specimen literarium inaugurale exhibens descriptionem al-Magribi suntam e libro regionum al-Jaqubii versione et annotatione illustratam*, Brill, Leiden, 1860.

_____, 'Literarische Notiz,' *Zeitschrift der Deutschen Morgenländischen Gesellschaft*, 16, 1862, 759–762.

_____, 'Von Herrn Dr. de Goeje,' *Zeitschrift der Deutschen Morgenländischen Gesellschaft*, 17, 1863, 393–394.

_____, 'Een stap vooruit,' *De Gids*, 28(2), 1864, 297–312.

Goerke, Heinz, *Berliner Ärzte: Virchow, Graefe, Koch, Leyden, Bergmann, Bier, Heubner, Moll, Stoeckel*, zweite Auflage, Berlin Verlag, Berlin, 1984.

Gooding, David, Trevor Pinch and Simon Schaffer (eds.), *The Uses of Experiment: Studies in the Natural Sciences*, Cambridge University Press, Cambridge, 1989.

Gordin, Michael D., *Scientific Babel: The Language of Science from the Fall of Latin to the Rise of English*, Profile Books, London, 2015.

Goschler, Constantin, 'Deutsche Naturwissenschaft und naturwissenschaftliche Deutsche: Rudolf Virchow und die »deutsche Wissenschaft«,' in: Ralph Jessen and Jakob Vogel (eds.), *Wissenschaft und Nation in der Europäischen Geschichte*, Campus, Frankfurt/New York, 2002, 97–114.

Götz, Norbert, ''Moral economy': its conceptual history and analytical prospects,' *Journal of Global Ethics*, 11(2), 2015, 147–162.

Gradmann, Christoph, 'Robert Koch und das Tuberkulin — Anatomie eines Fehlschlags,' *Deutsche Medizinische Wochenschrift*, 124(42), 1999, 1253–1256.

_____, 'Robert Koch and the Pressures of Scientific Research: Tuberculosis and Tuberculin,' *Medical History*, 45, 2001, 1–32.

_____, 'A harmony of illusions: clinical and experimental testing of Robert Koch's tuberculin 1890–1900,' *Studies in History and Philosophy of Biological and Biomedical Sciences*, 35, 2004, 465–481.

_____, *Krankheit im Labor: Robert Koch und die medizinische Bakteriologie*, Wallstein, Göttingen, 2005.

Graham, M. Patrick, 'The Discovery and Reconstruction of the Meshaʿ Inscription,' in: Andrew Dearman (ed.), *Studies in the Mesha inscription and Moab*, Scholars Press, Atlanta (GA), 1989, 41–92.

Greenberg, Daniel S., *Science for Sale: The Perils, Rewards, and Delusions of Campus Capitalism*, University of Chicago Press, Chicago (IL), 2007.

Greiling, Johann Christoph, 'Einige allgemeine Grundsätze zu einer Theorie der Recensionen,' *Archiv für die Physiologie*, Dritter Band, 1799, 349–385.

Gundlach, Horst, 'Oswald Külpe und die Würzburger Schule,' in: Wilhelm Janke and Wolfgang Schneider (eds.), *Hundert Jahre Institut für Psychologie und Würzburger Schule der Denkpsychologie*, Hogrefe Verlag für Psychologie, Göttingen, 1999, 107–124.

Habel, Thomas, *Gelehrte Journale und Zeitungen der Aufklärung: Zur Entstehung, Entwicklung und Erschließung deutschsprachiger Rezensionszeitschriften des 18. Jahrhunderts*, Edition Lumiere, Bremen, 2007.

Hall, G. Stanley, *Founders of modern psychology*, Appleton and Company, Ney York, 1912.

Hallonsten, Olof, 'Myths and realities of the ESS project: A systematic scrutiny of readily accepted 'truths',' in: Kaiserfeld, Thomas and Tom O'Dell, *Legitimizing ESS: Big Science as a collaboration across boundaries*, Nordic Academic Press, Lund, 2013, 43–66.

_____, *Big Science Transformed: Science Politics and Organization in Europe and the United States*, Palgrave Macmillan, New York (NY), 2016.

Hammerstein, Notker, 'Universitäten in Kaiserreich und Weimarer Republik und der Antisemitismus,' in: Wilfried Barner and Christoph König (eds.), *Jüdische Intellektuelle und die Philologien in Deutschland 1871–1933*, Wallstein, Göttingen, 2001, 25–34.

Hapke, Thomas, *Die Zeitschrift für Physikalische Chemie: Hundert Jahre Wechselwirkung zwischen Fachwissenschaft, Kommunikationsmedium und Gesellschaft*, Traugott Bautz, Herzberg, 1990.

Hardy, Anne I. and Mikael Hård, 'Common Cause: Public Health and Bacteriology in Germany, 1870–1895,' *East Central Europe*, 40(3), 2013, 319–340.

Hargittai, István and Magdolna Hargittai, 'J. Michael Bishop,' in: István Hargittai and Magdolna Hargittai, *Candid Science VI: More Conversations with Famous Scientists*, Imperial College Press, London, 2006, 182–199.

Harrison, Peter, 'Curiosity, Forbidden Knowledge, and the Reformation of Natural Philosophy in Early Modern England,' *Isis*, 92(2), 2001, 265–290.

Haupt, Edward, J., 'Laboratories for Experimental Psychology: Göttingen's Ascendancy over Leipzig in the 1890s,' in: Robert W. Rieber and David K. Robinson (eds.), *Wilhelm Wundt in*

History: The Making of a Scientific Psychology, Springer Science + Business Media, New York, 2001, 205–250.

Heide, Martin, 'The Moabitica and Their Aftermath: How to Handle a Forgery Affair with an International Impact,' in: Meir Lubetski and Edith Lubetski (eds.), *New inscriptions and seals relating to the biblical world*, Society of Biblical Literature, Atlanta (GA), 2012, 193–242.

Heidelberger, Michael, Nature from within: *Gustav Theodor Fechner and his psychophysical worldview*, translated by Cynthia Klohr, University of Pittsburgh Press, Pittsburgh (PA), 2004.

Herbst, Klaus-Dieter and Stefan Kratochwil (eds.), *Kommunikation in der Frühen Neuzeit*, Peter Lang, Frankfurt am Main, 2009.

Hermann, Armin, 'Die Funktion von Briefen in der Entwicklung der Physik,' *Berichte zur Wissenschaftsgeschichte*, 3(1/2), 1980, 55–64.

Hesse, Volker, 'Leben und Werk Otto Heubners: Eine Würdigung des ersten Lehrstuhlinhabers der Kinderheilkunde in Deutschland anlässlich seines 90. Todestages,' *Monatschrift Kinderheilkunde*, 12, 2016, 1116–1123.

Hettling, Manfred, 'Die persönliche Selbständigkeit: Der archmedische Punkt bürgerlicher Lebensführung,' in: Manfred Hettling and Stefan-Ludwig Hoffmann (eds.), *Der bürgerliche Wertehimmel: Innenansichten des 19. Jahrhunderts*, Vandenhoeck & Ruprecht, Göttingen, 2000, 57–78.

Heyd, Michael, *"Be Sober and Reasonable": The Critique of Enthusiasm in the Seventeenth and Early Eighteenth Centuries*, Brill, Leiden, 1995.

Hiller, Mary Ruth, 'The Identification of Authors: The Great Victorian Enigma,' in: J. Don Vann and Rosemary T. van Arsdel (eds.), *Victorian Periodicals: A Guide to Research*, The Modern Language Association of America, New York (NY), 1978, 123–148.

Hillix, William A. and James W. Broyles, 'The Family Tree of American Psychologists,' in: Bringmann, Wolfgang G. and Ryan D. Tweney (eds.), *Wundt Studies: A Centennial Collection*, Hogrefe, Toronto, 1980, 422–434.

Hodenberg, Christina von, 'Der Fluch des Geldsacks. Der Aufstieg des Industriellen als Herausforderung bürgerliche Werte,' in: Manfred Hettling and Stefan Ludwig Hoffmann (eds.), *Der bürgerliche Wertehimmel: Innenansichten des 19. Jahrhunderts*, Vandenhoeck & Ruprecht, Göttingen, 2000, 79–104.

Hoenig, Barbara, *Europe's New Scientific Elite: Social Mechanisms of Science in the European Research Area*, Routledge, London, 2017.

Höffding, Harald, 'Zur Theorie des Wiedererkennens,' *Philosophische Studien*, VIII, 1893, 86–96.

Hoffmann, Dieter, Birgit Kolboske and Jürgen Renn (eds.), *"Dem Anwenden muss das Erkennen vorausgehen" Auf dem Weg zu einer Geschichte der Kaiser-Wilhelm-/Max-Planck-Gesellschaft*, Edition Open Access, Berlin, 2015.

Holmes, Frederic L., 'Argument and Narrative in Scientific Writing,' in: Peter Dear (ed.), *The Literary Structure of Scientific Argument: Historical Studies*, University of Pennsylvania Press, Philadelphia (PA), 1991, 164–181.

Holton, Gerald, *Einstein, history, and Other Passions: The Rebellion against Science at the End of the Twentieth Century*, Addison-Wesley, Reading (MA), 1996.

Houghton, Bernard, *Scientific periodicals: their historical development, characteristics and control*, Clive Binley, London, 1975.

Hübinger, Gangolf, *Kulturprotestantismus und Politik: Zum Verhältniss von Liberalismus und Protestantismus im wilhelmischen Deutschland*, Mohr, Tübingen, 1994.

Hui, Alexandra E., 'The bias of "music-infected consciousness": the aesthetics of listening in the laboratory and on the city streets of fin-de-siècle Berlin and Vienna,' *Journal of the History of the Behavioral Sciences*, 48(3), 2012, 236–250.

Huistra, Pieter and Kaat Wils, 'Fit to Travel: The Exchange Programme of the Belgian American Educational Foundation: An Institutional Perspective on Scientific Persona Formation (1920–1940),' *Low Countries Historical Review*, 131(4), 2016, 112–134.

Hüntelmann, Axel C., 'Diphtheria serum and serotherapy. Development, Production and regulation in *fin de siècle* Germany,' *Dynamis*, 27, 2007, 107–131.

_____, 'The Dynamics of *Wertbestimming*,' *Science in Context*, 21(2), 2008, 229–252.

_____, *Paul Ehrlich: Leben, Forschung, Ökonomien, Netzwerke*, Wallstein Verlag, Göttingen, 2011.

Hyland, Ken, *Disciplinary Discourses: Social Interactions in Academic Writing*, University of Michigan Press, Ann Arbor (MI), 2004.

Irwin, Robert, *For Lust of Knowing: The Orientalists and their Enemies*, Penguin, London, 2007.

Jahn, Gustav, 'Die Mesha-Inschrift und ihr neuester Vertheidiger. In Verbindung mit einer Textkritik mehrerer Bibelstellen,' *Zeitschrift der Deutschen Morgenländischen Gesellschaft*, 59, 1905, 723–742.

Janichs, Georgius, *Animadversiones Criticae in versionem Syriacam Peschitthonianam Librorum Koheleth et Ruth*, Teubner, Leipzig, 1869.

_____, *Animadversiones Criticae in versionem Syriacam Peschitthonianam Librorum Koheleth et Ruth*, Schletter'sche Buchhandlung, Breslau, 1871.

Jardine, Nicholas and Emma Spary, 'The natures of cultural history,' in: Nicholas Jardine, James A. Secord and Emma C. Spary (eds.), *Cultures of Natural History*, Cambridge University Press, Cambridge, 1996, 3–13.

Jessen, Ralph and Jakob Vogel, 'Die Naturwissenschaften und die Nation: Perspektiven einer Wechselbeziehung in der europäische Geschichte,' in: Ralph Jessen and Jakob Vogel (eds.), *Wissenschaft und Nation in der europäische Geschichte*, Campus, Frankfurt, 2002, 7–40.

Johnson, Jeffrey A., 'Hierarchy and Creativity in Chemistry, 1871–1914,' in: Kathryn M. Olesko, *Science in Germany, The Intersection of Institutional and Intellectual Issues*, Osiris, 2nd Series, 5, 1989. 214–240.

Jonsen, Albert R., *The Birth of Bioethics*, Oxford University Press, Oxford, 1998.

Jordanova, Ludmilla, 'Gender and the historiography of science,' *British Journal for the History of Science*, 26, 1993, 469–483.

Joskowicz, Ari, *The Modernity of Others: Jewish Anti-Catholicism in Germany and France*, Stanford University Press, Stanford (CA), 2014.

Kautzsch, Emil and Albert Socin, *Die Aechtheit der moabitischen Altertümer geprüft*, Strasbourg, Trübner, 1876.

Keller, Phyllis, *States of belonging: German-American intellectuals and the First World War*, Harvard University Press, Cambridge (MA), 1979.

Kemp, Martin, 'Medicine in View: Art and Visual Representation,' in: Irvine Loudon, *Western Medicine: An Illustrated History*', Oxford University Press, Oxford, 1997, 1–22.

Kiesow, Friedrich, 'Un appareil simple pour determiner la sensibilité des points de temperature,' *Archives Italiennes de Biologie*, 30, 1898, 375–376.

_____, 'Ein einfacher Apparat zur Bestimmung der Empfindlichkeit von Temperaturpunkten,' *Philosophische Studien*, 14, 1898, 589–590.

_____, 'F. Kiesow,' in: Murchison, Carl (ed.), *A history of psychology in autobiography*, volume I, Russell & Russell, New York (NY), 1961, 163–190.

Kirchhoff, Arthur (ed.), *Die Akademische Frau: Gutachten hervorragender Universitätsprofessoren, Frauenlehrer und Schriftsteller über die Befähigung der Frau zum wissenschaftlichen Studium und Berufe*, Hugo Steinitz, Berlin, 1897.

Kirchner, Joachim, *Das deutsche Zeitschriftwesen: seine Geschichte und seine Probleme*, Teil 1, 2. neu bearbeitete und erweiterte Auflage, Harrasowitz, Wiesbaden, 1958.

Kivistö, Sari, *The Vices of Learning: Morality and Knowledge at Early Modern Universities*, Brill, Leiden, 2014.

Kleist, P. and C. Zerobin Kleist, 'Eine kurze Geschichte der klinischen Studie,' *Schweizerische Ärztezeitung*, 86(44), 2005, 2475–2482.

Klooster, H.S. van, 'The Story of Liebig's Annalen der Chemie,' *Journal of Chemical Education*, 34(1), 1957, 27–30.

Knorr Cetina, Karin, *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science*, Pergamon, Oxford, 1981.

_____, *Epistemic Cultures: How the Sciences Make Knowledge*, Harvard University Press, Cambridge (MA), 1999.

Koch, Adolf, *Moabitisch oder Selimisch? Die Frage der moabitischen Altertümer*, Stuttgart, Schweizerbart, 1876.

Koch, Robert, 'Weitere Mittheilungen über ein Heilmittel gegen Tuberkulose,' *Deutsche Medizinische Wochenschrift*, 16, 1890, 1029–1032.

_____, 'Die Cholera in Deutschland während des Winters 1892 bis 1893,' *Zeitschrift für Hygiene und Infectionskrankheiten*, 15, 1893, 89–165.

Kohler, Robert, *Partners in Science: Foundations and Natural Scientists 1900–1945*, University of Chicago Press, Chicago (IL), 1991.

_____, *Lords of the Fly: Drosophila Genetics and the Experimental Life*, University of Chicago Press, Chicago (IL), 1994.

_____, 'Lab History: Reflections,' *Isis*, 99(4), 2008, 761–768.

König, Eduard, 'Ist die Mesa-Inschrift ein Falsifikat?,' *Zeitschrift der Deutschen Morgenländischen Gesellschaft*, 59, 1905, 233–251.

_____, 'Mesa-Inschrift, Sprachgeschichte und Textkritik,' *Zeitschrift der Deutschen Morgenländischen Gesellschaft*, 743–756.

Koningsveld, Pieter S. van (ed.), *Orientalism and Islam: the letters of C. Snouck Hurgronje to Th. Nöldeke from the Tübingen University Library*, Documentatiebureau Islam-Christendom, Leiden, 1985.

Korselt, Ernst, *Jahresbericht des Königlichen Realgymnasiums mit Höherer Handelsschule in Zittau für das Schuljahr Ostern 1914 bis Ostern 1915*, Zittau, 1915.

Kosegarten, Joannes Godefredus Ludovicus (ed.), *Annales regum atque legatorum dei ex codice manuscript berolinens*i, Ernst Mauritius, Greifswald, 1831–1853.

Krämer, Fabian, 'Shifting Demarcations: An Introduction,' *History of Humanities*, 3(1), 2018, 5–14.

Krauße, Erika, 'Vorbemerkung: Der Brief als wissenschaftshistorische Quelle,' in: Erika Krauße (ed.), *Der Brief als wissenschaftshistorische Quelle*, Verlag für Wissenschaft und Bildung, Berlin, 2005, 1–28.

Kronick, David A., *A History of Scientific and Technical Periodicals: The Origins and Development of the Scientific and Technological Press, 1665–1790*, The Scarecrow Press, New York (NY), 1962.

Kühne, Thomas, 'Männergeschichte als Geschlechtergeschichte,' in: *Männergeschichte – Geschlechtergeschichte: Männlichkeit im Wandel der Moderne*, Thomas Kühne (ed.), Campus, Frankfurt, 1996, 7–30.

Lackie, John, Oxford Dictionary of Biomedicine, 2010, DOI: 10.1093/acref/9780199549351.001.00.

Lamberti, Georg, *Wilhelm Maximilian Wundt (1832–1920): Leben, Werk und Persönlichkeit in Bildern und Texten*, Deutscher Psychologen Verlag, Bonn, 1995.

Landy, Frank J., 'Hugo Münsterberg; Victim or Visionary?,' *Journal of Applied Psychology*, 77(6), 1992, 787–802.

Lange, Ludwig, 'Die geschichtliche Entwicklung des Bewegungsbegriffes und ihr voraussichtliches Endergebnis. I.,' *Philosophische Studien*, 3(3), 1886, 337–419.

_____, 'Die geschichtliche Entwicklung des Bewegungsbegriffes und ihr voraussichtliches Endergebnis. II. (Schluss),' *Philosophische Studien*, 3(4), 1886, 643–691.

Latour, Bruno and Steven Woolgar, *Laboratory Life: The Construction of Scientific Facts*, Sage, Beverly Hills (CA), 1979.

Laue, M. v., 'Dr. Ludwig Lange. 1863–1936. (Ein zu Unrecht Vergessener.),' *Die Naturwissenschaften*, 35(7), 1948, 193–196.

Lenz, Max, *Geschichte der königlichen Friedrich-Wilhelms-Universität zu Berlin*, zweiter Band, zweite Hälfte: auf dem Wege zur deutschen Einheit im neuen Reich, Verlag der Buchhandlung des Waisenhauses, Halle an der Saale, 1918.

Leppin, Hartmut, 'Eduard Nordens Berufung nach Greifswald: Handlungsspielräume im „System Althoff",' *Philologus*, 142(1), 1998, 162–172.

Lick, Thomas, *Friedrich Zarncke und das „Literarische Centralblatt für Deutschland": Eine buchgeschichtliche Untersuchung*, Harrasowitz, Wiesbaden, 1993.

Ligon, B. Lee, 'Robert Koch: Nobel Laureate and Controversial Figure in Tuberculin Research,' *Seminars in Pediatric Infectious Diseases*, 13(4), 2002, 289–299.

Lilienfeld, Abraham M., 'Ceteris paribus: the evolution of the clinical trial,' *Bulletin of the History of Medicin*, 56(1), 1982, 1–18.

Lindenfeld, David, 'Oswald Külpe and the Würzburg School,' *Journal of the History of the Behavioral Sciences*, 14, 1978, 132–141.

Linstrum, Erik, 'Strauss's Life of Jesus: Publication and the Politics of the German Public Sphere,' *Journal of the History of Ideas*, 71(4), 2010, 593–616.

Linton, Derek s., *Emil von Behring: Infectious Disease, Immunology, Serum Therapy*, American Philosophical Society, Philadelphia, 2005.

Littmann, Enno, 'Theodor Nöldeke,' in: *Nachrichten von der Gesellschaft der Wissenschaften zu Göttingen: Geschäftliche Mitteilungen aus dem Berichtsjahr 1930/31*, Weidmannsche Buchhandlung, Berlin, 1931, 48–57.

Locher, Wolfgang Gerhard, 'Max von Pettenkofer (1818–1901) as a Pioneer of Modern Hygiene and Preventive Medicine,' *Environmental Health and Preventive Medicine*, 12, 2007. 238–245

Lock, Stephen, *A Difficult Balance: Editorial Peer Review in Medicine*, London, The Nuffield Provincial Hospitals Trust, 1985.

Lorenz, Carl, 'Untersuchungen über die Auffassung von Tondistanzen,' *Philosophische Studien*, 6, 1890, 26–103.

Lucier, Paul, 'Court and controversy: patenting science in the nineteenth century,' *British Journal for the History of Science*, 29, 1996, 139–154.

Mack, Chris, '350 Years of Scientific Journals,' *Journal of Micro/Nanolithography, MEMS, and MOEMS*, 14(1), 2015, 1–3.

Mack, Wolfgang, 'Wundt's programmatische Erbe,' in: Gerd Jüttemann (ed.), *Wilhelm Wundts anderes Erbe: Ein Missverständniss löst sich auf*, Vandenhoeck & Ruprecht, Göttingen, 2006, 232–243.

Maier, Bernhard (ed.), *Gründerzeit der Orientalistik: Theodor Nöldekes Leben und Werk im Spiegel seiner Briefe*, Ergon, Würzburg, 2013.

Mangold, Sabine, *Eine "weltbürgerliche" Wissenschaft — Die deutsche Orientalistik im 19. Jahrhundert*, Franz Steiner, Stuttgart, 2004.

Marchand, Suzanne L., *German Orientalism in the Age of Empire: Religion, Race, and Scholarship*, Cambridge University Press, Cambridge, 2009.

McCloskey, Deirdre N., *The Bourgeois Virtues: Ethics for an Age of Commerce*, University of Chicago Press, Chicago (IL), 2006.

McCray, W. Patrick, 'Large Telescopes and the Moral Economy of Recent Astronomy,' *Social Studies of Science*, 30(5), 2000, 685–711.

Mehl, Ernst, 'Deutsche Bibliotheksgeschichte,' in: Wolfgang Stammler (ed.), *Deutsche Philologie im Aufriss*, Band I, Erich Schmitt, Berlin/Bielefeld, 1952, 315–378.

Merkel, Julius, 'Die zeitlichen Verhältnisse der Willenstätigkeit,' *Philosophische Studien*, 2(1), 1883. 73–127.

Métraux, Alexandre, 'Wilhelm Wundt und die Institutionalisierung der Psychologie: Ein Beitrag zu einem kontroversen Kapitel der Psychologiegeschichte,' *Psychologische Rundschau*, Band XXXI, 1980, 84–98.

Miedia, Andreas, 'De pronunciatione palatalium in diversis albanicae linguae dialectis,' in: *Verhandlungen des XIII. Internationalen Orientalisten-Kongresses. Hamburg September 1902*, 14–15.

Mildenberger, Florian, 'Auf verlorenem Posten — der einsame Kampf des Heinrich Dreuw gegen Syphilis und Salvarsan,' *Würzburger medizinhistorische Mitteilungen*, 30, 2011, 163–203.

Mommsen, Wolfgang J., 'Einleitung: Die deutschen kulturellen Eliten im Ersten Weltkrieg,' in: Wolfgang J. Mommsen, *Kultur und Krieg: Die Rolle der Intellektuellen, Künstler und Schriftsteller im Ersten Weltkrieg*, Oldenbourg, München, 1996, 1–15.

Morabia, Alfredo, 'Epidemiologic Interactions, Complexity, and the Lonesome Death of Max von Pettenkofer,' *American Journal of Epidemiology*, 166 (11), 2007. 1233–1238.

Morrell, J.B., 'The chemist breeders: the research schools of Liebig and Thomas Thomson,' *Ambix*, 19(1), 1972, 1–46.

Mountz, Alison, Anne Bonds, Becky Mansfield, Jenna Loyd, Jennifer Hyndman, Margaret Walton-Roberts, Ranu Basu, Risa Whitson, Roberta Hawkins, Trina Hamilton and Winifred Curran, 'For Slow Scholarship: A Feminist Politics of Resistance through Collective Action in the Neoliberal University,' *ACME*, 14, 2015, 1235–1259.

Müller, Georg Elias, *Zur Grundlegung der Psychophysik: Kritische Beiträge*, Griebel, Berlin, 1878.

Müller, Paul, *Ernst Meumann als Begründer der experimentellen Pädagogik*, E. Kalberer, Bazenheid, 1942.

Müller, Philipp, 'Geschichte machen. Überlegungen zu lokal-spezifischen Praktiken in der Geschichtswissenschaft und ihrer epistemischen Bedeutung im 19. Jahrhundert. Ein Literaturbericht,' *Historische Anthropologie*, 12(3), 2004, 415–433.

Münch, Ragnhild, *Robert Koch und sein Nachlaß in Berlin*, De Gruyter, Berlin, 2003.

Murphy, Kathryn and Anita Traninger (eds.), *The Emergence of Impartiality*, Brill, Leiden, 2014.

Muth, Franz-Christoph, *Die Annalen von at□ -T□ abarī im Spiegel der europäischen Bearbeitungen*, Peter Lang, Frankfurt, 1983.

Myers, Greg, 'The pragmatics of politeness in scientific articles,' *Applied Linguistics*, 10(1), 1989, 1–35.

Nicholson, Reynold A., *A Literary History of the Arabs*, Second Impression, London, T. Fischer Unwin, 1914.

Nicoli, Miriam, 'Faced with the flood: scholarly working practices and editorial transformations at the highpoint of scientific publication,' in: Andre Holenstein, Huberts Steinke, Martin Stuber and

Philippe Rogger (eds.) *Scholars in action: the practice of knowledge and the figure of the savant in the 18th century*, Brill, Leiden, 2013, 609–629.

Nöldeke, Theodorus, *De origine et compositione Surarum qoranicarum ipsiusque Qorani*, Verlag der Dieterichschen Buchhandlung, Göttingen, 1856.

_____, *Geschichte des Qorāns*, Verlag der Dieterichschen Buchhandlung, Göttingen, 1860.

_____, '"Bädeker" im Morgenlande,' *Im neuen Reich. Wochenschrift für das Leben des deutschen Volkes in Staat, Wissenschaft und Kunst*, 5. Jahrgang, 2. Buch, 1875, 15–20.

_____, 'Die moabitischen Fälschungen,' *Deutsche Rundschau*, 6, 1876, 447–451.

_____, *Geschichte der Perser und Araber zur Zeit der Sasaniden: aus der Arabischen Chronik des Tabari übersetzt und mit ausführlichen Erläuterungen und Ergänzungen versehn*, Leiden, Brill, 1879.

_____, 'Zum „Buch der Gesetze der Länder,' *Zeitschrift der Deutschen Morgenländischen Gesellschaft*, 64, 1910, 555–560.

Nye, Mary Jo, *Before Big Science: The Pursuit of Modern Chemistry and Physics 1800–1940*, Harvard University Press, Cambridge (MA), 1996.

Nye, Robert A., 'Medicine and Science as Masculine "Fields of Honor",' *Osiris*, 12, 1997, 60–79.

Nyhart, Lynn K., 'Writing Zoologically: The Zeitschrift für wissenschaftliche Zoologie and the Zoological Community in Late Nineteenth-Century Germany,' in: Peter Dear (ed.), *The Literary Structure of Scientific Argument: Historical Studies*, University of Pennsylvania Press, Philadelphia (PA), 1991, 43–71.

Oedingen, Christina and Joseph W. Staerk, 'First Cure for Diphtheria by Antitoxin as Early as 1891,' *Annals of Science*, 54, 1997, 607–610.

Ogden, Robert M., 'Oswald Külpe and the Würzburg School,' *The American Journal of Psychology*, 64(1), 1951, 4–19.

Osman, Ghada, 'Oral vs. Written transmission: The case of Tabarī and Ibn Sa'd,' *Arabica*, 48(1), 2001, 66–80.

Paret, Rudi, *Arabistik und Islamkunde an deutschen Universitäten: Deutsche Orientalisten seit Theodor Nöldeke*, Franz Steiner, Wiesbaden, 1966.

Paul, Herman, 'Performing history: how historical scholarship is shaped by epistemic virtues,' *History and Theory*, 50(1), 2011, 1–19.

_____, 'The Scholarly Self: Ideals of Intellectual Virtue in Nineteenth-Century Leiden, in: Bod, Rens, Jaap Maat and Thijs Weststeijn (eds.), *The Making of the Humanities, vol. II: From Early Modern to Modern Disciplines*, Amsterdam University Press, Amsterdam, 2012, 397–411.

_____, 'What is a Scholarly Persona? Ten Theses on Virtues, Skills, and Desires,' *History and Theory*, 53, 2014, 348–371.

_____, 'Virtue language in nineteenth-century orientalism: a case study in historical epistemology, *Modern Intellectual History*, 14(3), 2017, 689–715.

_____, 'Weber, Wöhler, and Waitz: Virtue Language in Late Nineteenth-Century Physics, Chemistry, and History,' in: Dongen, Jeroen van and Herman Paul (eds.), *Epistemic Virtues in the Sciences and the Humanities*, Springer, Cham, 2017, 91–107.

_____, *De deugden van een wetenschapper: Karakter en toewijding in de geesteswetenschappen, 1850–1940*, Amsterdam University Press, Amsterdam, 2018.

_____, 'Introduction: Scholarly Personae in the History of Orientalism, 1870–1930,' in: Christiaan Engberts and Herman Paul (ed.), *Scholarly Personae in the History of Orientalism, 1870–1930*, Brill, Leiden, 2019, 1–16.

_____, 'Germanic Loyalty in Nineteenth-Century Historical Studies: A Multi-Layered Virtue,' *História da Historiografia*, forthcoming.

Paulsen, Friedrich, *Die deutschen Universitäten und das Universitätsstudium*, A. Asher & Co., Berlin, 1902.

Pawliczek, Aleksandra, 'Kontinuität des informellen Konsens': Die Berufungspolitik der Universität Berlin und ihre Dozenten im Kaiserreich und in der Weimarer Republik,' in: Rüdiger von Bruch, Uta Gerhardt and Aleksandra Pawliczek (eds.), *Kontinuitäten und Diskontinuitäten in der Wissenschaftsgeschichte des 20. Jahrhunderts*, Franz Steiner, Stuttgart, 2006, 69–92.

Peirce, William.S. and Peter. Krüger, 'Entrepreneurship in a Bureaucracy: The Case of Friedrich Althoff,' *Journal of Economic Studies*, 20(4,5), 1993, 52–70.

Pennock, Robert T. and Michael O'Rourke, 'Developing a Scientific Virtue-Based Approach to Science Ethics Training, *Science and Engineering Ethics*, 23(1), 2017, 243–262.

Pfeffer, Christian and Bjorn R. Olsen, 'Editorial: Journal of Negative Results in Biomedicine,' *Journal of Negative Results in BioMedicine*, 1(2), 2002, 1–2.

Pickering, Andrew (ed.), *Science as practice and culture*, Chicago University Press, Chicago (IL), 1992.

Pischel, Richard, Franz Praetorius, Ludolf Krehl and Ernst Windisch, *Die Deutsche Morgenländische Gesellschaft 1845–1895. Ein Ueberblick*, Leipzig, F.A. Brockhaus, 1895.

Preissler, Holger, 'Die Anfänge der Deutschen Morgenländischen Gesellschaft,' *Zeitschrift der Deutschen Morgenländischen Gesellschaft*, 145(2), 1995, 241–327.

Probst, Paul, *Bibliographie Ernst Meumann: Mit einer Einleitung zur Biographie*, Traugott Bautz, Herzberg, 1991.

Profos Frick, Claudia, *Gelehrte Kritik: Albrecht von Hallers literarisch-wissenschaftliche Rezensionen in den Göttingischen Gelehrten Anzeigen*, Schwabe, Basel, 2009.

Rabault-Feuerhahn, Pascale, '"La science la robe au vent" Le Congrès Internationaux des Orientalistes et la disciplinarisation des études Orientales,' in: Jean-Louis Chiss and Dan Savatovski, *La Disciplinarisation de la linguistique. Histoire et épistémologie, Histoire épistémologie langage*, Dossier en ligne, 5, 2012, 1–16.

Rebenich, Stefan, *Theodor Mommsen: Eine Biographie*, C.H. Beck, München, 2002.

Renger, Johannes, 'Altorientalistik und jüdische Gelehrte in Deutschland – Deutsche und österreichische Altorientalisten in Exil,' in: Wilfried Barner and Christoph König (eds.), *Jüdische Intellektuelle und die Philologien in Deutschland 1871–1933*, Wallstein, Göttingen, 2001, 247–262.

Rieber, Robert W. and David K. Robinson (eds.), *Wilhelm Wundt in History: The Making of a Scientific Psychology*, Springer Science + Business Media, New York (NY), 2001.

Rietschel, Ernst Th. and Jean-Marc Cavaillon, 'Richard Pfeiffer and Alexandre Besredka: creators of the concept of endotoxin and anti-endotoxin,' *Microbes and Infection*, 5, 2003, 1407–1414.

Ringer, Fritz K., *The Decline of the German Mandarins: The German Academic Community, 1890–1933*, Harvard University Press, Cambridge (MA), 1969.

Roberts, Robert C. and W. Jay Wood, *Intellectual Virtues: An Essay in Regulative Epistemology*, Clarendon Press, Oxford, 2007.

Robinson, Daniel N., *An Intellectual History of Psychology*, revised edition, Macmillan, New York (NY), 1981.

Robinson, David Kent, 'Wilhelm Wundt and the establishment of experimental psychology, 1875–1914: The context of a new field of scientific research,' (PhD diss., University of California, Berkeley, 1987).

Rödiger, Emil, August Pott, Heinrich Leberecht Fleischer and Hermann Brockhaus, 'Vorwort,' *Zeitschrift der Deutschen Morgenländischen Gesellschaft*, 1, 1847, III–VI.

Rössner, Stephan, 'Max Rubner (1854–1932),' *Obesity Reviews*, 14, 2013, 432–433.

Rouse, Joseph, *Engaging Science: How to Understand Its Practices Philosophically*, Cornell University Press, Ithaca (NY), 1996.

Rowe, David E., *A Richer Picture of Mathematics: The Göttingen Tradition and Beyond*, Springer, Cham, 2018.

Russo Kraus, Chiara, 'Back to the origins of the repudiation of Wundt: Oswald Külpe and Richard Avenarius,' *Journal of the History of the Behavioral Sciences*, 53(1), 2017, 28–47.

Salager-Meyer, Françoise, María Ángeles Alcaraz Ariza and Maryelis Pabón Berbesí, 'Collegiality, critique and the construction of scientific argumentation in medical book reviews: a diachronic approach,' *Journal of Pragmatics*, 39, 2007, 1758–1174.

Sarton, George, *Introduction to the History of Science*, Volume 1, Robert E. Krieger, Williams and Wilkins, Baltimore (MD), 1931.

Schagen, Udo, 'Germany, Austria, and Switzerland,' in: *The Oxford Companion to Medicine*, Oxford University Press, Oxford, 2001.

Scheiber, Alexander (ed.), *Ignaz Goldziher: Tagebuch*, Leiden, Brill, 1978.

Schickhardt, Christoph, Nelson Hosley and Eva C. Winkler, 'Researchers' Duty to Share Pre-publication Data: From the Prima Facie Duty to Practice,' in: Brent Daniel Mittelstadt and Luciano Floridi (eds.), *The Ethics of Biomedical Big Data*, n.p., Springer, 2016, 309–338.

Schilfert, Sabine, 'Friedrich Althoff – ein preußischer Geheimrat von Format? Bemerkungen zu einem wissenschafts-historischen Kolloquium anläßlich seines 150. Geburtstag,' *Zentralblatt für das Bibliothekswesen*, 103, 1989, 546–552.

Schlesier, Renate, 'Homeric Laughter by the Rivers of Babylon: Heinrich Heine and Karl Marx,' in: Mark H. Gelber (ed.), *The Jewish Reception of Heinrich Heine*, Niemeyer, Tübingen, 1992, 21–44.

Schlottmann, Konstantin, 'Neue Moabitische Funde und Räthsel: Erster Bericht,' *Zeitschrift der Deutschen Morgenländischen Gesellschaft*, 26, 1872, 393–416.

_____, 'Ueber die Aechtheit der Moabitischen Alterthümer,' *Zeitschrift der Deutschen Morgenländischen Gesellschaft*, 28, 1874, 171–184.

Schneid, Mathias, *Der neuere Spiritismus philosophisch geprüft*, August Hornik, Eichstätt, 1880.

Schneider, Ulrich Johannes, (ed.), *Kulturen des Wissens im 18. Jahrhundert*, Walter de Gruyter, Berlin, 2008.

Schneider, Ute, 'Die Funktion wissenschaftlicher Rezensionszeitschriften im Kommunikationsprozess der Gelehrten,' in: Ulrich Johannes Schneider Schneider (ed.), *Kultur der Kommunikation: Die europäische Gelehrtenrepublik im Zeitalter von Leibniz und Lessing*, Harrassowitz, Wiesbaden, 2005, 279–291.

Schnicke, Falko, *Die männliche Disziplin: Zur Vergeschlechtlichung der deutschen Geschichtswissenschaft 1780–1900*, Wallstein, Göttingen, 2015.

Schulte, Erika, *Der Anteil Erich Wernickes an der Entwicklung des Diphtherieantitoxins*, Mensch & Buch, Berlin, 2001.

Schütze, Johannes, *Jahresbericht des Königl. Realgymnasium mit höherer Handelsschule zu Zittau für das Schuljahr 1888/89*, Zittau, 1889.

Scott, James C., *The Moral Economy of the Peasant: Rebellion and Subsistence in Southeast Asia*, Yale University Press, New Haven (CT), 1976.

Secord, James A., *Victorian Sensation: The Extraordinary Publication, Reception, and Secret Authorship of Vestiges of the Natural History of Creation*, University of Chicago Press, Chicago (IL), 2000.

Sewell Jr., William H., 'Geertz, Cultural Systems, and History: From Synchrony to Transformation,' in: Sherry B. Ortner (ed.), *The Fate of "Culture": Geertz and Beyond*, University of California Press, Berkeley, 1999, 35–55.

_____, 'The Concept(s) of Culture,' in: Victoria E. Bonnell and Lynn Hunt (eds.), *Beyond the Cultural Turn: New Directions in the Study of Society and Culture*, University of California Press, Berkeley (CA), 1999, 35–61.

Shapin, Steven, 'The House of Experiment in Seventeenth-Century England,' *Isis*, 79(3), 1988, 373–404.

_____, '"A Scholar and a Gentleman": The Problematic Identity of the Scientific Practitioner in Early Modern England, *History of Science*, 29, 1991, 279–327.

_____, *A Social History of Truth: Civility and Science in Seventeenth-Century England*, Chicago University Press, Chicago, 1994.

_____, *The Scientific Life: A Moral History of a Late Modern Vocation*, University of Chicago Press, Chicago (IL), 2008.

Shapin, Steven and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life*, Princeton University Press, Princeton (NJ), 1985.

Shatz, David, *Peer Review: A Critical Inquiry*, Rowman & Littlefield, Lanham (MD), 2004.

Shoshan, Boaz, *Poetics of Islamic Historiography: Deconstructing Ṭabarī's History*, Brill, Leiden, 2004.

Small, Robin, *Nietzsche and Rée: A star friendship*, Oxford University Press, Oxford, 2005.

Smith, Bonnie G., 'Gender and the Practices of Scientific History: The Seminar and Archival Research in the Nineteenth Century,' *American Historical Review*, 100(4), 1995, 1150–1176.

Snouck Hurgronje, Christiaan, 'Michaël Jan de Goeje', in: *Jaarboek der Koninklijke Akademie van Wetenschappen*, Amsterdam, 1909, 107–146.

_____, 'Ignaz Goldziher. 22 juni 1850 – 13 november 1921,' *De Gids*, 85, 1921, 489–499.

Socin, Albert, 'Ueber Inschriftfälschungen,' *Zeitschrift der Deutschen Morgenländischen Gesellschaft*, 27, 1873, 133–135.

Sombart, Nicolaus, 'Männerbund und Politische Kultur in Deutschland,' in: Kühne, Thomas, 'Männergeschichte als Geschlechtergeschichte', in: *Männergeschichte – Geschlechtergeschichte: Männlichkeit im Wandel der Moderne*, Thomas Kühne (ed.), Campus, Frankfurt, 1996, 136–155.

Sprung, Lothar, 'The Berlin Psychological Tradition: Between Experiment and Quasi-Experimental Design, 1850–1990,' in: William R. Woodward and Robert S. Cohen (eds.), *World Views and Scientific Discipline Formation: Science Studies in the German Democratic Republic*, Springer Science+Business Media, Dordrecht, 1991, 107–116.

Staubermann, Klaus B., 'Tying the knot; skill, judgement and authority in the 1870s Leipzig spiritistic experiments,' *The British Journal for the History of Science*, 34(1), 2001, 67–79.

Steckel, Sita, 'Einleitung: Akademische Wissenkulturen zwischen Mittelalter und Moderne,' in: Martin Kintzinger and Sita Steckel (eds.), *Akademische Wissenskulturen: Praktiken des Lehrens und Forschens vom Mittelalter bis zur Moderne*, Schwabe, Basel, 2015, 1–5.

Steinberg, Holger (ed.), *Der Briefwechsel zwischen Wilhelm Wundt und Emil Kraepelin: Zeugnis einer jahrzehntelangen Freundschaft*, Hans Huber, Bern, 2002.

Stigler, Stephen M. and William H. Kruskal, 'Normative Terminology,' in: Stephen M. Stigler, *Statistics on the Table: The History of Statistical Concepts and Methods*, Harvard University Press, Cambridge (MA), 1999, 403–430.

Storost, Jürgen, 'Zu einigen germanistischen Zeitschriften in der zweiten Hälfte des 19. Jahrhunderts,' *Zeitschrift für Germanistik*, Neue Folge II, 1992, 341–354.

Stössel, Waldemar, 'Anton Klette: 1870–1878,' in: [Arbeitsgemeinschaft wissenschaftlicher Bibliothekare der Universitätsbibliothek Jena], *Geschichte der Universitätsbibliothek Jena*, Hermann Böhlhaus Nachfolger, Weimar, 1958, 485–515.

Stumpf, Carl, 'C. Lorenz: Untersuchungen über die Auffassung von Tondistanzen,' *Zeitschrift für Psychologie und Physiologie der Sinnesorgane*, 1, 1890, 26–103.

_____, 'Mein Schlußwort gegen Wundt,' *Zeitschrift für Psychologie und Physiologie der Sinnesorgane*, 1891, 438–443.

Thompson, Edward P., 'The moral economy of the English crowd in the eighteenth century,' *Past & Present*, 50(1), 1971, 76–136.

Timmer, Antje, 'Kontrollierte klinische Studien vor Archie Cochrane,' *Zeitschrift für Evidenz, Fortbildung und Qualität im Gesundheitswesen*, 102, 2008, 473–481.

Tollebeek, Jo, *Fredericq & Zonen: Een antropologie van de moderne geschiedwetenschap*, Bert Bakker, Amsterdam, 2008.

_____, 'L'historien quotidien: pour une anthropologie de la science historique moderne,' *Schweizerische Zeitschrift für Geschichte*, 61(2), 2011, 143–167.

Tomasevic, Klaudia, 'Die medizinische Versorgung von Kindern Mitte des 19. Jahrhunderts am Beispiel von Würzburg,' (diss. Bayerischen Julius-Maximilians-Universität zu Würzburg, 2002).

Trautmann-Waller, Céline, 'Selbstorganisation jüdischer Gelehrsamkeit und die Universität seit der ›Wissenschaft des Judentums‹,' in: Wilfried Barner and Christoph König (eds.), *Jüdische Intellektuelle und die Philologien in Deutschland 1871–1933*, Wallstein, Göttingen, 2001, 77–86.

Vogl, Alfred, 'Six hundred years of medicine in Vienna: A History of the Vienna School of Medicine,' *Bulletin of the New York Academy of Medicine*, 1967, 43(4), 292–299.

Vrolijk, Arnoud and Richard van Leeuwen, *Arabic Studies in the Netherlands: A Short History in Portraits, 1580–1950*, Brill, Leiden, 2014.

Wagner, Gerhard and Gilbert Weiss, 'Editors' Introduction,' in: Gerhard Wagner and Gilbert Weiss (eds.*), A Friendship That Lasted a Lifetime: The Correspondence Between Alfred Schütz and Eric Voegelin*, translated by William Petropulos, University of Missouri Press, Columbia and London, 2011, 1–8.

Wang, Jessica, 'Broken Symmetry': Physics, Aesthetics, and Moral Virtue in Nuclear Age America,' in: Jeroen van Dongen and Herman Paul (eds.), *Epistemic Virtues in the Sciences and the Humanities*, Springer, Cham, 2017, 27–47.

Ward, Gerald W.R. (ed.), *The Grove Encyclopedia of Materials and Techniques in Art*, Oxford, Oxford University Press, 2008.

Weinberg, Alvin M., *Reflections on Big Science*, M.I.T. Press, Cambridge (MA), 1967.

Whitley, Richard, *The Intellectual and Social Organization of the Sciences*, Clarendon Press, Oxford, 1984.

Wundt, Eleonore, *Wilhelm Wundts Werk: Ein Verzeichnis seiner sämtlichen Schriften*, C.H. Beck, München, 1927.

Wundt, Wilhelm, *Grundzüge der Physiologischen Psychologie*, Engelmann, Leipzig, 1874.

_____, *Der Spiritismus: eine sogenannte wissenschaftliche Frage: offener Brief an Herrn Prof. Dr. Hermann Ulrici in Halle*, Wilhelm Engelmann, Leipzig, 1879.

_____, 'Schlusswort zum ersten Bande,' *Philosophische Studien*, I, 1883, 615–617.

_____, 'Ueber Vergleichungen von Tondistanzen,' *Philosophische Studien*, 6, 1891, 605–640.

_____, 'Das Institut für experimentelle Psychologie,' in: Rektor und Senat der Universität Leipzig (eds.), *Festschrift zur Feier des 500 jährigen Bestehens der Universität Leipzig*, 4. Band, I. Teil, 1909, 118–133.

_____, 'Gustav Theodor Fechner,' in: Wilhelm Wundt, *Reden und Aufsätze*, Alfred Kröner, Leipzig, 1914, 254–343.

_____, *Über den wahrhaften Krieg*, Alfred Kröner, Leipzig, 1914.

_____, *Erlebtes und Erkanntes*, Alfred Kröner, Stuttgart, 1920.

Yale, Elizabeth, *Sociable Knowledge: Natural History and the Nation in Early Modern Britain*, University of Pennsylvania Press, Philadelphia (PA), 2016.

Young, Julian, *Friedrich Nietzsche: A Philosophical Biography*, Cambridge University Press, Cambridge, 2010.

Zagzebski, Linda Trinkaus, *Virtues of the mind: An inquiry into the nature of virtue and the ethical foundations of knowledge*, Cambridge University Press, Cambridge, 1996.

Zande, Daniel van der, 'Martinus Th. Houtsma: 1851–1943: Een bijdrage aan de geschiedenis van de Oriëntalistiek in Nederland en Europa,' (diss., Universiteit Utrecht, 1999).

Zeiss, H. and R. Bieling, *Behring: Gestalt und Werk*, Bruno Schultz, Berlin-Grunewald, 1940.

Ziman, John M., *Public Knowledge: An Essay Concerning the Social Dimensions of Science*, Cambridge University Press, Cambridge, 1968.

Zott, Regine, 'Hochgeehrter Herr Geheimer Rath! … Friedrich Theodor Althoffs Beziehungen zur Chemie,' *Chemie in unserer Zeit*, 42(5), 2008, 322–328.

Summary in Dutch

De geschiedenis van de wetenschap heeft veel te winnen bij het onderzoeken van wetenschappelijke deugden. Hoewel ook historisch onderzoek met een andere insteek, zoals studies die het levenswerk van grote wetenschappers of de ontwikkeling van wetenschappelijke instituties in kaart brengen, onontbeerlijk zijn, verdienen de normen en waarden die onder wetenschappers leven ook aandachtige studie. Deze geven immers vorm aan de dagelijkse praktijk van de wetenschapsbeoefening. Vanaf de late jaren 80 van de 20ste eeuw is onder wetenschapshistorici de belangstelling voor deugden dan ook toegenomen. Zij hebben de aandacht gevestigd op de rol die verschillende deugden gepeeld hebben in de ontwikkeling van de wetenschap. Zij hebben in dit kader onder meer gewezen op de waarde die wetenschappers in de loop der eeuwen hebben toegekend aan deugden als waarheidsliefde, onpartijdigheid, loyaliteit, verbeeldingskracht en doorzettingsvermogen. Het leeuwendeel van dergelijke hedendaagse studies is gewijd aan de analyse van *individuele* deugden. Hoewel het verre van vanzelfsprekend is dat verschillende deugden zonder conflict samengaan, wordt er echter nauwelijks aandacht besteed aan de wijze waarop deze zich tot elkaar *verhouden*. Dit proefschrift wil het onderzoek hiernaar een stap verder brengen op basis van casestudies uit de Duitse wetenschap in de late 19e en vroege 20e eeuw.

In dit proefschrift betoog ik dat verschillende wetenschappelijke deugden in deze tijd in een complexe verhouding tot elkaar stonden. Soms gingen deugden moeiteloos samen, terwijl ze op andere momenten met elkaar in conflict kwamen. De vraag die in deze studie centraal staat is: Hoe verhielden wetenschappelijke deugden zich tot elkaar in de alledaagse wetenschappelijke praktijk? Deze vraag roept vervolgens verdere vragen op naar de mate waarin deugden ten grondslag lagen aan de ongeschreven regels en stilzwijgende conventies van wetenschappelijk werk, de mate waarin deugden in conflict kwamen met de verwachtingen die door deze regels en conventies werden gewekt en naar de manier waarop wetenschappers omgingen met het risico op dergelijke conflicten. Het begrip 'morele economie van de wetenschap', zoals dat vanaf 1990 steeds vaker door wetenschapshistorici gebruikt wordt, is bij uitstek geschikt om dergelijke vragen te beantwoorden, met name omdat het de aandacht richt op de veranderlijkheid van het delicate evenwicht tussen verschillende deugden.

Hoewel de potentiële veranderlijkheid van het evenwicht tussen verschillende deugden uitnodigt tot diachrone studie, heeft dit proefschrift een synchrone insteek. Dit maakt het mogelijk om de casestudies in voldoende detail te behandelen om de vaak onuitgesproken verwachtingen van deugdzaamheid aan het licht te brengen. Toch komt de variëteit aan manieren om een balans te

vinden tussen de claims die verschillende deugden wetenschappers oplegden vanuit dit synchrone perspectief op verschillende manieren naar voren. Een eerste rede hiervoor is het feit dat ik aandacht besteed aan casestudies uit zeer verschillende disciplines, namelijk de oriëntalistiek, de experimentele psychologie en de bacteriologie. Daarnaast richt ieder hoofdstuk zich op een ander aspect van de wetenschapsbeoefening, dat steeds uitnodigt tot reflectie op en onenigheid over de kwaliteiten van wetenschappers en hun werk. Het eerste hoofdstuk richt zich op briefwisselingen van geleerden, het tweede hoofdstuk gaat in op de werkzaamheden van redacteuren van wetenschappelijke tijdschriften, het derde hoofdstuk bevat een inhoudelijke analyse van boekrecensies en het vierde hoofdstuk onderzoekt aan de Pruisische overheid gerichte aanbevelingsbrieven voor leerstoelen. In het licht van de bespiegelingen en meningsverschillen die in deze bronnen naar voren komen, worden de contouren van een morele economie van wetenschap zichtbaar.

Het eerste hoofdstuk van deze studie richt zich op de wijze waarop geleerden privé-correspondentie zowel gebruikten om elkaar de helpende hand te bieden als om elkaar van stevige kritiek te voorzien. De briefwisseling tussen de bevriende semitisten Theodor Nöldeke en Jan de Goeje toont aan dat ondersteuning en kritiek voor hen grotendeels samenvielen. Beiden waren zich ervan bewust dat stevige kritiek kon bijdragen aan betere tekstedities. Diepgravende kritiek op elkaars werk was mogelijk omdat filologische arbeid een solitaire aangelegenheid was en beide mannen elkaars volledige werk- en denkproces konden herhalen. Wetenschap die het resultaat is van samenwerken in een laboratoriumomgeving kon niet op dezelfde manier worden geëvalueerd. Dit werd ook niet altijd nodig geacht. Het bloedserum dat Emil Behring tegen difterie ontwikkelde, moest alleen op zijn werkzaamheid getest worden. Hiervoor had Behring geen verdere bacteriologische expertise nodig. Hij zocht echter wel de hulp van een kinderarts die ervaring had met de ziekte en toegang had tot kinderen waarop het serum getest kon worden. De medewerkers van het psychologische laboratorium van Wilhelm Wundt, ten slotte, stonden slechts in beperkte mate open voor kritiek van buiten. De nauwe onderlinge samenwerking moedigde aan tot het vormen van een gesloten front. Daarnaast onderhield Wundt weinig persoonlijke banden met wetenschappers die zinvol commentaar konden leveren op onderzoek dat onder zijn leiding tot stand kwam. Bovendien misten de onderzoeksresultaten uit zijn laboratorium de praktische toepasbaarheid die tot klinische testen noopte.

In dit eerste hoofdstuk blijkt dat er grote verschillen waren in hoe geleerden de verhouding tussen behulpzaamheid en kritiek ervoeren. Deze kunnen grotendeels herleid worden tot verschillen tussen disciplines. Het relatief individuele karakter van de werkzaamheden van semitische filologen maakte het makkelijker om een relatie van wederzijds vertrouwen op te bouwen waarbinnen steun

en kritiek op elkaars tekst konden samenvallen. In de bacteriologie was het eindresultaat van onderzoek vaak echter geen tekst maar een geneesmiddel. De belangrijkste evaluatie waaraan dit onderworpen diende te worden, had niets met replicatie van doen. Omdat de werkzaamheid van het middel centraal stond, was het voor bacteriologen van belang iemand met aanvullende klinische expertise te zoeken om het nieuw ontwikkelde product te testen. Omdat onderzoeksresultaten uit Wundts psychologische laboratorium een dergelijke praktische toepasbaarheid misten en de laboratoriumleider zelf slechts in beperkte mate contact onderhield met collega's met gedeelde interesses, ontstond in zijn laboratorium een cultuur die slechts in beperkte mate openstond voor hulp en kritiek van buiten.

Het tweede hoofdstuk onderzoekt de werkzaamheden van redacteuren van wetenschappelijke tijdschriften. Wilhelm Wundt richtte zijn eigen tijdschrift, de *Philosophische Studien*, op om de onderzoeksresultaten uit zijn laboratorium publiek te maken. Hoewel het tijdschrift financieel weinig succesvol was, bleek Wundt in staat om de redactionele onafhankelijkheid ten opzichte van zijn uitgever te bewaren. Hij compenseerde de matige financiële resultaten van zijn kwakkelende tijdschrift met de opbrengsten van zijn goedverkopende boeken bij dezelfde uitgever. Hij gebruikte deze redactionele vrijheid vervolgens met name om zijn eigen studenten en medewerkers een podium te bieden. De redacteuren van het *Zeitschrift der Deutschen Morgenländischen Gesellschaft* hadden ook weinig last van bemoeizucht van hun uitgever. Er waren warme persoonlijke banden tussen de tijdschriftredactie en de uitgever en omdat alle leden van *de Deutsche Morgenländische Gesellschaft* (*DMG*) automatisch abonnee waren, was het op financieel vlak een risicoloze onderneming. De status van het *Zeitschrift* als ledenblad vormde wel een uitdaging voor de redactie. Aan de ene kant voelde zij zich verplicht alle leden van de *DMG* aan het woord te laten. Aan de andere kant zag zij het als haar taak ervoor te zorgen dat geen enkel lid nodeloos door een ander lid gekwetst werd. Er werden dus slechts in beperkte mate mogelijkheden tot debat geboden. De redacteur van het *Literarisches Centralblatt*, tot slot, kon zich minder vrij van zijn uitgever bewegen. De uitgever streefde naar een winstgevend blad en hoofdredacteur Friedrich Zarncke kon de winstgevendheid niet compenseren of garanderen met goedverkopende boeken of een stabiel abonneebestand. Omdat het *Centralblatt* wekelijks uitkwam, was het daarnaast van belang om goede relaties met een groot aantal auteurs te onderhouden. Omdat Zarncke in sterke mate afhankelijk was deze auteurs, was het moeilijk voor hem om hun bijdragen aan te passen of te weigeren.

In het eerste hoofdstuk konden de karakteristieke eigenschappen van verschillende disciplines gebruikt worden om verschillen in steun en kritiek te duiden. In het tweede hoofdstuk zijn de verschillende omstandigheden waarin redacteuren zich vinden beter te verklaren op basis van het type tijdschrift dat zij redigeerden. Wundt's *Studien* boden weinig ruimte voor discussie omdat de

hoofdredacteur zich onafhankelijk van zijn uitgever kon richten op het verspreiden van de onderzoeksresultaten van zijn eigen medewerkers. Slepende debatten waren hierbij niet te verwachten. Voor de redactie van het *Zeitschrift* van de *DMG* was het met name belangrijk te onderkennen dat zij een brede groep oriëntalisten vertegenwoordigde. Tegen deze achtergrond moest iedereen vrij zijn om inzichten te delen en collega's te bekritiseren, maar moesten slepende debatten voorkomen worden. Zarncke moest als redacteur van het *Centralblatt* voortdurend balanceren tussen de verwachtingen van zijn uitgever, zijn auteurs en zijn publiek. Het toestaan van teveel kritische onafhankelijkheid was hierin net zo riskant als het aanmoedigen tot kritiekloze boekrecensies.

Het derde hoofdstuk bevat een inhoudelijke analyse van de boekrecensies die Theodor Nöldeke en Wilhelm Wundt in de jaren 1870 voor het *Centralblatt* schreven. Beide geleerden bespraken boeken over uiteenlopende onderwerpen. Nöldeke recenseerde zowel filologische als theologische werken, terwijl Wundt zowel over geneeskunde als over filosofie schreef. Een opvallende bevinding is dat Nöldeke veel kritischer was over theologische boeken dan over filologische. Bij Wundt is eenzelfde patroon waar te nemen: veel van zijn filosofische recensies waren zeer negatief, terwijl zijn commentaar op medische werken vaak neutraal of lovend was. Daarnaast valt op dat beide recensenten positiever stonden tegenover het werk van auteurs met een academische affiliatie dan tegenover het werk van leken. Bij Nöldeke uitte zich dit vooral in een welwillende houding ten opzichte van proefschriften. Bij Wundt valt met name op dat hij het werk van hoogleraren opvallend mild beoordeelde. Daarnaast waren beide recensenten zich bewust van het dubbele publiek waartoe zij zich verhielden. Ook als zij zich kritisch uitten over het werk van een individuele auteur, benadrukten zij het gemeenschappelijk belang dat recensenten, gerecenseerden en een breder publiek van vakgenoten met elkaar verbond.

Hoewel recensies zeer kritisch konden zijn, speelden zij dus ook het rol in het creëren en in stand houden van een gemeenschap van geleerden. Recensies geven ook een goed beeld van de kwaliteiten die de leden van deze gemeenschap verondersteld werden te hebben. Auteurs werden herhaaldelijk geprezen voor wetenschappelijke deugden als vlijt, scherpzinnigheid, kritisch denkvermogen en nauwkeurigheid. Niet iedere categorie van auteurs kwam echter in dezelfde mate in aanmerking voor dergelijke lofbetuigingen. Vooral schrijvers in vakgebieden die uitnodigden tot speculatie of dogmatiek – theologie en filosofie – werden zowel door Nöldeke als door Wundt veelvuldig en stevig bekritiseerd. Een relatief groot deel van deze theologische en filosofische werken was geschreven door auteurs zonder academische affiliatie. Filologische en geneeskundige werken werden daarentegen meestal geschreven door auteurs die aan een universiteit verbonden waren en konden meestal rekenen op een welwillende receptie.

Het vierde hoofdstuk gaat in op de evaluaties van collega's die geleerden schreven voor Friedrich Althoff, de invloedrijkste ambtenaar op het Pruisische Ministerie van Onderwijs in de late 19e en vroege 20e eeuw. Omdat dergelijke evaluaties een reële impact konden hebben op de carrières van collega's konden de deugden van kritische distantie en collegiale loyaliteit hier flink botsen. Tegen verschillende achtergronden kwam Althoff tot verschillende besluiten. Bij benoemingen aan de medische faculteit in Breslau volgde hij vaak het advies van zijn vertrouweling Heinrich Fritsch. Deze stelde het belang van de universiteit over het algemeen boven de belangen van individuele geleerden. Het belang van de universiteit speelde ook een rol in de discussie over de opvolging van de Berlijnse kinderarts Eduard Henoch. Velen keerden zich tegen Althoffs aanstelling van Otto Heubner, omdat diens Saksische accent en eenvoudige manieren niet zouden passen bij de hoofdstedelijke universiteit. Althoffs benoeming van Emil Behring als hoogleraar voor hygiëne in Marburg riep nog meer tegenstand op. Behring was weinig geliefd onder zijn collega's en stond bekend als een slechte docent. Omdat hij als onderzoeker echter zeer succesvol was, negeerde Althoff de wensen van de Marburgse faculteit en benoemde Behring tot hoogleraar. Verdere overwegingen die in de aan Althoff verzonden evaluaties aan bod konden komen waren de morele en religieuze kwaliteiten van kandidaat-hoogleraren en de intellectuele tradities waarbinnen deze zich plaatsten. Vooral bij discussies omtrent de aanstelling van filosofen waren dergelijke overwegingen soms zeer belangrijk.

In dit vierde hoofdstuk wordt duidelijk dat een onafhankelijke kritische houding en een loyale instelling niet slechts deugden waren die vorm gaven aan de relaties tussen individuele geleerden. Veel wetenschappers voelden ook loyaliteit ten opzichte van faculteiten, universiteiten, intellectuele tradities en idealen van moraliteit en religiositeit. Sommige van de kritische evaluaties die Althoff ontving, richtten zich in de eerste plaats op de kwaliteiten van individuen, zoals Heubners Saksische eenvoud en Behrings tekortschietende onderwijskwaliteiten. Vaak wogen andere loyaliteiten echter minstens net zo zwaar. Fritsch' loyaliteit aan de medische faculteit in Breslau en de breed gedeelde wens om het prestigieuze karakter van de Berlijnse universiteit te handhaven zijn hier voorbeelden van. Idealen van moraliteit, religiositeit en intellectuele traditie konden echter ook leidend zijn in de beoordeling van individuen. Zo droeg de inschatting dat Richard Avenarius een vertegenwoordiger was van een extremistische denkrichting ertoe bij dat hij nooit een leerstoel in Duitsland bekleedde.

In het slothoofdstuk wordt de morele economie van wetenschap die in de voorgaande hoofdstukken aan het licht kwam nauwkeuriger beschreven. Het betoogt dat er een spanning waarneembaar is tussen loyale collegialiteit en kritische onafhankelijkheid in alle contexten van wetenschapsbeoefening die in dit proefschrift aan bod komen. Deze spanning is waarneembaar in

de privécorrespondentie van het eerste hoofdstuk, de redactiepraktijken van het tweede hoofdstuk, de boekrecensies van het derde hoofdstuk en in de aan Althoff verzonden aanbevelingen van het laatste hoofdstuk. De zoektocht naar een evenwicht tussen loyaliteit en onafhankelijkheid deed zich voor op verschillende niveaus. In de eerste plaats gaf het zoeken naar deze balans vorm aan de verhouding tussen individuele geleerden. Hierbij is het belangrijk rekening te houden met de verschillende relaties waarin zij tot elkaar konden staan, zoals collega, student, voormalig student, leidinggevende of partner binnen een gezamenlijk uitgevoerd project. Ten tweede zocht iedere wetenschapper naar een juiste balans tussen loyaliteit en onafhankelijkheid ten opzichte van wetenschappelijke instituties, zoals laboratoria, faculteiten, universiteiten, vaktijdschriften en vakorganisaties. Tot slot moesten geleerden hun verhouding bepalen ten opzichte van niet-wetenschappelijke actoren. Hierbij kan zowel gedacht worden aan grote onpersoonlijke actoren als de staat en het bedrijfsleven als aan persoonlijke vrienden.

Het concept van de morele economie van de wetenschap werpt licht op rol van wetenschappelijke deugden voor de alledaagse praktijk van de wetenschapsbeoefening in de 19e eeuw. Het concept vestigt de aandacht op twee karakteristieke eigenschappen van de relatie die wetenschappers tot deze deugden hadden. In de eerste plaats benadrukt de notie van een morele economie het voortdurende zoeken naar een balans. Juist omdat collegiale loyaliteit en kritische onafhankelijk met elkaar in conflict konden komen, was het te allen tijde noodzakelijk een evenwicht te vinden tussen de tegenstrijdige verwachtingen die beide breed gewaarde deugden opriepen. In de tweede plaats richt de notie van een morele economie de aandacht op de veranderlijkheid van deze balans. Omdat er geen eenvoudige en altijd aanvaardbare vuistregels bestonden om conflicten tussen loyaliteit en onafhankelijkheid te beslechten, moest de omgang met deze conflicten steeds opnieuw vastgesteld worden. De balans tussen beide deugden was hierdoor voortdurend in beweging. Het veranderlijke evenwicht tussen loyaliteit en onafhankelijkheid toont aan dat we meer inzicht verkrijgen in de dagelijkse wetenschappelijke praktijk als we wetenschappelijke deugden niet in isolatie van elkaar analyseren en ons in plaats daarvan richten op de wijze waarop ze zich tot elkaar verhouden.

In verschillende opzichten bevinden hedendaagse wetenschappers zich in een heel andere positie dan hun 19e-eeuwse voorgangers. Ze werken in het tijdperk van 'Big Science' waarin zij vaak afhankelijk zijn van financiering door onderzoeksfondsen, in contact staan met een wereldwijd netwerk van collega's, een relatie dienen te onderhouden met vertegenwoordigers uit het bedrijfsleven en hun werk moeten laten beoordelen door de anonieme lezers die daarvoor als onderdeel van double-blind peer review processen worden uitgenodigd. Desondanks hechten veel wetenschappers nog altijd veel waarde aan collegiale loyaliteit en kritische onafhankelijkheid.

Universiteiten claimen bijvoorbeeld nog steeds onderzoekers met een onafhankelijke geest op te leiden en de samenwerking waartoe de hedendaagse 'Big Science' uitnodigt is zeer gebaat bij loyale solidariteit.

Misschien is de morele economie van de wetenschap is echter wel complexer geworden. Zo worden deugden als flexibiliteit en de bereidheid om risico's te nemen steeds belangrijker naarmate de band tussen de wetenschap en het bedrijfsleven sterker wordt. Tegelijkertijd ontwikkeld zich echter ook een tegenbeweging. Voorvechters van 'slow scholarship' vragen steeds vaker de aandacht voor deugden als zelfzorgzaamheid en betrokkenheid. De 21ste-eeuwse onderzoeker staat daarom voor de uitdaging om een goede balans te vinden tussen zowel oudere als recentere wetenschappelijke deugden.

Curriculum Vitae

Christiaan Engberts was born in 1982 in Amsterdam. He studied philosophy and history in Leiden. In 2010 he graduated *cum laude* for his Master in philosophy and he completed (again *cum laude*) the Research Master 'History: Societies and Institutions' in 2012. In 2013 he started his PhD research at Leiden University as part of the NWO-funded project 'The scholarly self: character, habit, and virtue in the humanities, 1860–1930. Within this project he worked on the moral economy of scholarship among late-19th and early-20th-century German scholars. His research was supervised by the project's leader, Professor Herman Paul and Professor Kaat Wils of the KU Leuven. He spent three months as a visiting scholar at the Graduate School Global and Area Studies of the Universität Leipzig in 2015, the Friedrich Meinecke Institut of the Freie Universität Berlin in 2016 and the Onderzoeksgroep Cultuurgeschiedenis vanaf 1750 of the KU Leuven in 2017. He has published articles in *History of Humanities* and *Low Countries Historical Review*, as well as co-edited (with Herman Paul) a volume on personae in the history of Orientalism that was published by Brill in 2019.