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

Engberts, C.A.

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Author: Engberts, C.A.

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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 Krauß, Erika, 'Vorbemerkung: Der Brief als wissenschaftshistorische Quelle,' in: Erika Krauß (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-Moellendorff 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.

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 Jazīdi il et Hishāmi*.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 Jazīdi II et Hishāmi,' *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 Ṭabarī and Ibn Sa'd,' *Arabica*, 48(1), 2001, 66–80. 66.

³² Muth, Franz-Christoph, *Die Annalen von al-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 manuscripto 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 interest 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.

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 collect 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 versehen*, 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-Ṭabarî'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 *tamwīn*.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 *tamwīn*, see: UBL: Or. 5585f, undated list of comments on series II, pages 978–1275.

29 84,20 bleibt ich bei m. p. Ansicht. 30 allein macht die Vollständigkeit der Reimwörter nicht aus, obgleich sie immer begrenzter ist als das 51, 21, aber 30 ist für 30 nicht unmöglich.

3003, 13. An d. Stelle in Schabus liest ich Dankenollen! 74
fuss es als "zitternder Fluss".

^{30 58} 30 58, 18. Du hast Recht, dass mit 30 58 3 beginnt. Aber durch den Zusatz von 30 58 wird keine besondere Construction hergestellt. Es muß etwas anderes fehlen; ^{hoff} = - la ghat, der Dergt.

30 62, 16. Dabei bleibe ich, dass die Enthaltsamkeit ein Bezug auf das Elypt von den Arabern als ganz aufgesetzt war.

30 63, 3. 30 63 ist metrisch unmöglich, die Verlängerung des Endworts im Reim ist obligatorisch, nicht undenkbar. Also nur im 1. Vers auch nach dem ersten 30 63.

30 71, 16. Ja, wenn äwly, da ständ! aber 71 ist keine Form, die im Fern. ohne 3 bleiben könnte. Dergt ist ja die Ueberlieferung darüber nicht gerade wly.

~~30 72, 17. Ich bleibe bei dem 30 72, 17, wie es steht.~~

30 82, 18. Ja, ^{30 82} 30 82 mit d. 30 82 und dem pl. habe ich auch Beispiele, aber nur aus d. Koran und aus der Oessie und auch die ich das selbe. In Arab einfacher Prosa ganz

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 'Alī 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-Mas'ūdī.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

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 Medicine*, 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 December 1890, both in: GStA PK, VI. HA, NI 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 Hensch 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 Vormal’s Meister, Lucius & Brüning, 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 Vormal's Meister, Lucius & Brüning, 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

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 Vormal's Meister, Lucius & Brüning, 20 April 1894, BNd: EvB/B196/13. For more on Ehrlich's contribution to the diphtheria serum, see: Bäuml, 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 Diphtherieserum, [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 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 Versuchsthiere bei der Diphtherie,' *Zeitschrift für Hygiene*, 12, 1892, 45–57.

¹⁰⁴ Bäumlér, Ernst, *Paul Ehrlich*, 93 and Hüntelmann, Axel C., 'The Dynamics of *Wertbestimmung*,' *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 Heidelberg. 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 Fütth, 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

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 interpretative 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

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, NI. Wilhelm Wundt, Gustav Theodor Fechner to Wilhelm Wundt, 27 October 1880 and 28 October 1880. (accessed at <http://home.uni-leipzig.de/wundtbrieft/home.htm>)

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

¹²² See, for example, UAL, NI. Wilhelm Wundt, Gustav Theodor Fechner to Wilhelm Wundt, 24 March 1885 and 12 July 1885. (accessed at <http://home.uni-leipzig.de/wundtbrieft/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, NI. Wilhelm Wundt, Gustav Theodor Fechner to Wilhelm Wundt, 7 April 1885. (accessed at <http://home.uni-leipzig.de/wundtbrieft/home.htm>)

¹²⁴ UAL, NI. Wilhelm Wundt, Gustav Theodor Fechner to Wilhelm Wundt, 13 April 1886. (accessed at <http://home.uni-leipzig.de/wundtbrieft/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 Psychologie* — 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

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, NI. 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

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/wundtbrieft/home.htm>)

¹³² UAL, Nl. Wilhelm Wundt, Gustav Theodor Fechner to Wilhelm Wundt, 14 April 1885. (accessed at <http://home.uni-leipzig.de/wundtbrieft/home.htm>)

¹³³ For example, see UAL, Nl. Wilhelm Wundt, Wilhelm Wundt to Gustav Theodor Fechner, 19 October 1882 and 27 October 1882. (accessed at <http://home.uni-leipzig.de/wundtbrieft/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/wundtbrieft/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 reconciling 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, Holtzmann 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, NI. Wilhelm Wundt, Heinrich Holtzmann to Wilhelm Wundt, 28 December 1866. (accessed at <http://home.uni-leipzig.de/wundtbrieft/home.htm>)

¹⁴¹ UAL, NI. Wilhelm Wundt, Adolf Hausrath to Wilhelm Wundt, 6 November 1904. (accessed at <http://home.uni-leipzig.de/wundtbrieft/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 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, NI Althoff, No. 118, Otto Liebmann to Friedrich Althoff, 23 January 1884. See also: Chapter 4, 148-150.

¹⁴³ GStA PK, VI. HA, NI Althoff, No. 118, Carl Stumpf to Friedrich Althoff, 5 October 1893.

¹⁴⁴ GStA PK, VI. HA, NI 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/wundtbriefer/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.