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Discipline building in Germany: women and genetics at the Berlin Institute for Heredity Research

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Abstract. The origin and the development of scientific disciplines has been a topic of reflection for several decades. The few extensive case studies support the thesis that scientific disciplines are not monolithic structures but can be characterized by distinct social, organizational and scientific–technical practices. Nonetheless, most disciplinary histories of genetics confine themselves largely to an uncontested account of the content of the discipline or occasionally institutional factors. Little attention is paid to the large number of researchers who, by their joint efforts, ultimately shaped the discipline. We contribute to this aspect of disciplinary historiography by discussing the role of women researchers at the Institute for Heredity Research, founded in 1914 in Berlin under the directorship of Erwin Baur, and the sister of the John Innes Institute at Cambridge. This paper investigates how and why Baur built a highly successful research programme that relied on the efforts of his female staff, whose careers, notably Elisabeth Schiemann's, are also assessed *in toto*. These women undertook the necessary 'technoscience' and in some cases innovative work and helped increase the prestige of the institute and its director. Together they played a pivotal role in the establishment of genetics in Germany. Without them the discipline would have developed much more slowly and along a divergent path.

Introduction: historiography of disciplines and women in science

In 1914 the first Institut für Vererbungswissenschaft (Institute for Heredity Research) in Germany was founded in Berlin as part of the Landwirtschaftliche Hochschule (Agricultural College). Erwin Baur (1875–1933) was appointed director. The majority of his scientific staff was made up of women: Gerda von Ubisch, Elisabeth Schiemann, Luise von Graevenitz, Emmy Stein and Paula Hertwig.[1] The only male staff member who worked at the institute for a considerable period was Hans Nachtsheim. In 1930, 'thanks to the efforts of Baur and his colleagues the Dahlem Institute had reached a

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1 We have come across a few other names, but they were, contrary to the persons mentioned, connected to the institute only for brief periods.

position of world renown'.[2] In this period genetics developed from a new and vulnerable field of research into an established and accepted discipline in Germany. Women scientists were largely responsible for this development.

From the second half of the nineteenth century, mainly through the establishment of the research universities, scientific knowledge increased enormously, and that growth was by and large canalized by the emergence of so-called 'scientific disciplines'. Recent scholars have tried to explain that a disciplinary division of knowledge was by no means 'natural', but developed in the course of time and was influenced by interdependent scientific, social, economic and national factors.[3] In their accounts, disciplines are not monolithic structures but are best understood as heterogeneous families of disciplinary programmes, which can be characterized by distinct social, organizational, and scientific–technical practices in various places and at different times.[4] Notwithstanding these insights, most disciplinary histories confine themselves largely to the content of the discipline or occasionally institutional factors. The study of social and human factors – if undertaken at all – is mostly focused on a few pioneers or participants who are considered to have opened up new horizons within the discipline. Little attention is usually paid to the large number of researchers who, by their joint efforts, ultimately shaped the discipline.

We want to contribute to social and human factors of disciplinary historiography by discussing the role of women researchers at the first German institute for genetics. The emergence of heredity as a scientific discipline can be located relatively precisely in the

2 Werner Plarre, 'A contribution to the history of science of heredity in Berlin', *Englera* (1987) 7, pp. 147–217, 172.

3 Michel Foucault, *The Archeology of Knowledge*, tr. A.M. Sheridan Smith, New York: Pantheon, 1972; Pierre Bourdieu, 'The specificity of the scientific field and the social conditions of the progress of reason', *Social Science Information* (1975) 14, pp. 19–47; David R. Shumway and Ellen Messer-Davidow, 'Disciplinarity: an introduction', *Poetics Today* (1991) 12, pp. 201–224; Ellen Messer-Davidow, David R. Shumway and David J. Sylvan, *Knowledges: Historical and Critical. Studies in Disciplinarity*, Charlottesville and London: University Press of Virginia, 1993; Richard M. Burian and Wim J. van der Steen, 'Introduction', *Biology and Philosophy* (1993) 8, pp. 255–257; Timothy Lenoir, *Instituting Science: The Cultural Production of Scientific Disciplines*, Stanford, CA: Stanford University Press, 1997; Jan Golinski, *Making Natural Knowledge: Constructivism and the History of Science*, Cambridge: Cambridge University Press, 1998; Steve Fuller, *Social Epistemology*, Bloomington: Indiana University Press, 2002; Judy Thompson Klein, 'Disciplinary origins and differences', Australian Academy of Sciences, 2004; Simon Schaffer, 'How disciplines look', in Andrew Barry and Georgina Born (eds.), *Interdisciplinarity: Reconfigurations of the Social and Natural Sciences*, Abingdon: Routledge, 2013, pp. 57–81. Recent literature is more about interdisciplinarity and transdisciplinarity than about disciplinarity, and more about the humanities than about the sciences.

4 Robert E. Kohler, *From Medical Chemistry to Biochemistry: The Making of a Biomedical Discipline*, Cambridge: Cambridge University Press, 1982; Rudolf Stichweh, *Zur Entstehung des modernen Systems wissenschaftlicher Disziplinen: Physik in Deutschland 1740–189*, Frankfurt am Main: Suhrkamp, 1984; Timothy Lenoir, 'The discipline of nature and the nature of disciplines', in Messer-Davidow, Shumway and Sylvan, op. cit. (3), pp. 70–102, 85; Peter Galison, 'Introduction: the context of disunity', in Peter Galison and David J. Stump (eds.), *The Disunity of Science: Boundaries, Contexts, and Power*, Stanford, CA: Stanford University Press (1996), pp. 1–36; Andrew Barry,'Pharmaceutical matters: the invention of informed materials', *Theory, Culture and Society* (2005) 22, pp. 51–69; Andrew Barry and Georgina Born (eds.), *Interdisciplinarity: Reconfigurations of the Social and Natural Sciences*, Abingdon: Routledge, 2013, pp. 57–81.

time between 1900 and 1930. During this period, parallel processes took place in various countries. Journals were set up, national societies were founded, institutes dedicated to the study of heredity were established and international conferences were held. The first histories of genetics were written in the 1960s by geneticists who did not pay significant attention to these institutional factors. Instead, they mainly restricted themselves to scientific concepts, to the importance of the experimental character of the new study of heredity, to Mendel, to the rediscoverers, and to a few other pioneers.[5] These histories reflect the naive idea that science is a mechanical fact-gathering exercise instead of a human activity, and in so doing serve the purpose of legitimizing both the discipline itself and the professional identity of its practitioners.[6] Disciplinary histories canonize what belongs to the discipline, which predecessors must be considered the founders, and what must be considered its heart and what the edges. Various factors play a role in such a legitimization: the existence of an undisputed founding father, and in the case of genetics undisputed 'rediscoverers', and undisputed demarcations with other knowledge fields. These histories in general pay attention to only a few very prominent researchers, and disregard the social context and the networks on which these 'captains of the discipline' depended.

A second generation of (professional) historians highlighted many of these early histories' deficiencies, in focusing on a founding father (Mendel) and his 'independent rediscoverers'. Robert Olby was prominent amongst these, giving shrewd accounts of early priority debates and interpretations of Mendel as reputational and discipline-building moves.[7]

What of women in these two generations of genetic historiography? During the period when the first histories were written, the dominant attitude was that women played at best a supportive role in the scientific enterprise, and accordingly they mention women contributors rarely and cursorily in their heroic narratives.[8] The historiography, in this sense, reflected the prejudices of many of its subjects, as is illustrated by the case of Theodor Roemer. Sounded out as a potential successor to Baur at the Berlin Institute, Roemer expressed distaste for its substantially female workforce, calling it a *Damenstift*; that is, a convent for noblewomen.[9]

5 Hans Stubbe, *History of Genetics: From Prehistoric Times to the Rediscovery of Mendel's Laws*, Cambridge, MA: MIT Press, 1971; Leslie C. Dunn, *A Short History of Genetics: The Development of Some of the Main Lines of Thought, 1864–1939*, New York: McGraw-Hill Book Company, 1965; Alfred H. Sturtevant, *A History of Genetics*, New York: Harper and Row, 1965; Elof A. Carlson, *The Gene: A Critical History*, Philadelphia and London: W.B. Saunders Company, 1966.

6 Peter J. Bowler, *The Mendelian Revolution: The Emergence of Hereditarian Concepts in Modern Science and Society*, London: Athlone Press, 1989, 12; Shumway and Messer-Davidow, op. cit. (3), pp. 205–208.

7 Robert C. Olby, *Mendel, Mendelism and Genetics* (1997), on *MendelWeb*, at www.mendelweb.org/MWolby.html, 7. Olby, 'William Bateson's introduction of Mendelism to England: a reassessment', *BJHS* (1987) 20, pp. 399–420; William B. Provine, *The Origins of Theoretical Population Genetics*, Chigaco: The University of Chicago Press, 1971. A recent study by Alice White on the so-called Tavistock group discusses the extent to which this group self-consciously tried to create a discipline. Alice Victoria White, 'From the science of selection to psychologising civvy street: the Tavistock group, 1939–1948', PhD thesis, University of Kent, 2016.

8 Dunn, op. cit. (5), pp. 65–66; Sturtevant, op. cit. (5), pp. 136–138, 139–143.

9 Geheimes Staatsarchiv Preußischer Kulturbesitz, Berlin (hereafter GSPK), I. HA. Rep. 87. B. Nr. 20095. Min. LDF, Akten betreffend Landw. Hochschule in Berlin, Professoren (Dozente) Band 7, 1929–1930, letter

Although the second generation of historical studies approached the history of genetics more broadly, the women factor still did not receive attention.[10] However, this situation has started to change. In 2007 a thematic issue of the *Journal of the History of Biology* was dedicated to the role of women in the formation of the discipline of genetics and a few other articles have also paid attention to this topic.[11] Efforts have been initiated to draw more general conclusions about the role of women in the field of genetics.[12] The contributions of women in several countries, such as Britain, Norway and the Netherlands, have been evaluated, but Germany has not yet received the attention it deserves.

The history of scientific disciplinarity is in large part the history of universities, since, in the course of the nineteenth century, university departments and academic institutions became the main sites of scientific research. Universities' inaccessibility to women therefore adds another aspect to the history of women's participation in science.[13] Universities in most countries had opened their doors to women by the end of the nineteenth century, but when, and to what extent, varied from country to country. In Germany, university

from Roemer, 05-12-1929: 'Die Tatsache, dass ich … gewissermassen ein Damenstift uebernehme – schreckt ja zu haechst ab. Ueber die Loesing dieser Frage muss diskutiert werden.'

10 To mention only a small selection: Robert C. Olby, *Origins of Mendelism*, 2nd edn, Chicago: The University of Chicago Press, 1985; Olby, *Mendel, Mendelism and Genetics*, op. cit. (7); Jan Sapp, *Beyond the Gene: Cytoplasmic Inheritance and the Struggle for Authority in Genetics*, Oxford: Oxford University Press, 1987; Bowler, op. cit. (6); Jonathan Harwood, *Styles of Scientific Thought: The German Genetics Community 1900–1933*, Chicago: The University of Chicago Press, 1993; Hans-Jörg Rheinberger and Staffan Müller-Wille, *Vererbung: Geschichte und Kultur eines Biologisches Konzepts*, Frankfurt am Main: Fischer Taschenbuch Verlag, 2009. See this monograph for further references.

11 Paul Farber (ed.), 'Special section on women in genetics', *Journal of the History of Biology* (2007) 40, pp. 389–528; Ida H. Stamhuis, 'A female contribution to early genetics: Tine Tammes and Mendel's laws for continuous characters', *Journal of the History of Biology* (1995) 28, pp. 591–605; Marsha L. Richmond, 'Women in the early history of genetics: William Bateson and the Newnham College Mendelians, 1900–1910', *Isis* (2001) 92, pp. 55–90; Richmond, 'The "domestication" of heredity: the familial organization of geneticists at Cambridge University, 1895–1910,' *Journal of the History of Biology* (2006) 39, pp. 565–605; Ida H. Stamhuis and Arve Monsen, 'Kristine Bonnevie, Tine Tammes and Elisabeth Schiemann in early genetics: emerging chances for a university career for women', *Journal of the History of Biology* (2007) 40, pp. 427–466.

12 Ida H. Stamhuis and Marsha L. Richmond, 'Opportunities for women in early genetics: an international perspective', in Reiner Nürnberg, Ekkehard Höxtermann und Martine Voigt, *Elisabeth Schiemann (1881–1972): Vom* Auf*Bruch der Genetik und der Frauen in den* Um*Brüchen des 20. Jahrhunderts*, Rangsdorf: Basilisken Presse, 2014, pp. 54–85; Marsha L. Richmond, 'Women as Mendelians and geneticists', *Science and Education* (2015) 24, pp. 125–150.

13 Margaret Rossiter, *Women Scientists in America: Struggles and Strategies to 1940*, Baltimore and London: John Hopkins University Press, 1982; Pnina G. Abir-Am and Dorinda Outram (eds.), *Uneasy Careers and Intimate Lives: Women in Science, 1789–1979*, New Brunswick and London: Rutgers University Press, 1987; Londa Schiebinger, *The Mind Has No Sex? Women in the Origins of Modern Science*, Cambridge, MA: Harvard University Press, 1989; Sally Gregory Kohlstedt (ed.), *History of Women in the Sciences: Readings from* Isis, Chicago: The University of Chicago Press, 1999; Margaret Rossiter, 'A twisted tale: women in the physical sciences in the nineteenth and twentieth centuries', in Mary Jo Nye, ed., *The Cambridge History of Science*, vol. 5: *The Modern Physical and Mathematical Sciences*, Cambridge: Cambridge University Press, 2003, pp. 54–71; Soňa Štrbáňová, Ida H. Stamhuis and Kateřina Mojsejová (eds.), *Women Scholars and Institutions: Proceedings of the Conference held June 8–11, 2003, in Prague*, 2 vols., Prague: Studies in the History of Sciences and Humanities, Vol. 13, 2004.

access for women was introduced relatively late and was dependent on the state where a university was located: in Berlin (which was in Prussia) women only gained the right to study at university after 1908.[14]

Gaining an academic degree was one thing; finding academic employment often turned out to be much more complicated.[15] By the early twentieth century, however, universities were expanding at unprecedented rates and this meant that there was a growing demand for qualified staff. As a result, several modes of academic employment became available for women. One of these was the emergence of 'female' disciplines. Home economics emerged in the USA in the late nineteenth century and women had the opportunity to reach its highest ranks.[16] A second possibility for women was to accept 'unattractive' jobs in traditional disciplines. A well-known example was the calculators in astronomy. This work, which involved precise calculations and was rather invisible, required great patience. Women often stayed in such jobs for many years. Another example was the workers in US fruit fly research that took place between 1934 and 1970. In this period the workers involved were mostly non-faculty staff members and the percentage of female non-faculty members was much higher than the percentage of females in the faculty.[17] How widespread this practice was is difficult to assess because non-faculty staff members are usually not mentioned in the available sources.[18]

A third way of finding an academic position was to look for work in a new field. Newly established areas have no established positions. Their prestige is often lower than that of established disciplines, and as a result they are frequently avoided by more ambitious scientists. In turn, these new disciplines benefit from the eagerness of new participants to make contributions. Thus, when the first academic women sought opportunities in science, many entered disciplines in this early stage of development. Radioactivity, biochemistry, ecology and the study of heredity were all such areas in the early twentieth century.[19] For genetics an additional factor must be mentioned.

14 Annette Vogt, *Vom Hintereingang zum Hauptportal? Lise Meitner und ihre Kolleginnen an der Berliner Universität und in der Kaiser-Wilhelm-Gesellschaft*, Stuttgart: Franz Steiner Verlag, 2007; Vogt, 'Wissenschaftlerinnen in Kaiser-Wilhelm-Instituten A–Z', *Veröffentlichungen aus dem Archiv der Max-Planck-Gesellschaft*, Band 12, 2. erweiterte Auflage, Berlin, 2008.

15 Rossiter, *Women Scientists in America*, op. cit. (13), Chapter 3; Rossiter, 'A twisted tale', op. cit. (13).

16 Rossiter, *Women Scientists in America*, op. cit. (13), Chapter 7.

17 Rossiter, *Women Scientists in America*, op. cit. (13), pp. 53–57; Michael R. Dietrich and Brandi H. Tambasco, 'Beyond the boss and the boys: women and the division of labor in *Drosophila* genetics in the United States, 1934–1970', *Journal of the History of Biology* (2007) 40, pp. 509–528.

18 The assumption that women working as 'invisible technicians' carried out essential work in several areas of scientific research cannot easily be tested because of their 'invisibility': Steven Shapin, 'The invisible technician', *American Scientist* (1989) 77–76, pp. 554–563.

19 On radioactivity see Maria Rentetzi, 'Women pioneers in radioactivity research: introduction', in Štrbáňová, Stamhuis and Mojsejová, op. cit. (13), pp. 579–589. On genetics see Ute Deichmann, 'Frauen in der Genetik: Forschung und Karriere bis 1950', in Renate Tobies (ed.), *'Aller Männerkultur zum Trotz': Frauen in Mathematik und Naturwissenschaften*, 2nd edn, Frankfurt am Main: Campus, 2008, pp. 245–282; Helga Satzinger, 'Women's places in the new laboratories of genetic research in early 20th century: gender, work, and the dynamics of science', in Štrbáňová, Stamhuis and Mojsejová, op. cit. (13), pp. 265–294; Richmond, 'Women in the early history of genetics', op. cit. (11); Richmond, 'The "domestication" of heredity', op. cit. (11); Stamhuis, op. cit. (11); Stamhuis and Richmond, op. cit. (12). For the Berlin case see Vogt, *Von Hintereingang zum Hauptportal?*, op. cit. (14).

Originally, most geneticists were educated as botanists, and botany was a field of knowledge that had attracted and admitted women since the first half of the nineteenth century, because it was considered more suitable for women than most other fields.[20]

As a relatively large number of women researchers was incorporated in the discipline of genetics, the question can be asked, what was their role? The answer seems to be that it was restricted to what the sociologist Timothy Lenoir has characterized as 'techno-science'; that is, research that meets criteria of scientificity and hence publishability. But this, as Lenoir goes on, is only the first step in a truly successful career. To become an acknowledged authority in a field, institution-oriented activities are also necessary: such as institution building, founding a journal or a scientific society, or organizing a conference.[21] In the early twentieth century, women could not engage in institution-oriented activities, even in new disciplines.

With these considerations in place, a number of questions arise that will be discussed in the remainder of the paper. The sympathies of institutional directors seem intuitively relevant to the matter under discussion. Several examples can be given. As one of us has previously discovered, the head of the zoological laboratory of the University of Oslo, Georg Ossian Sars, was a supporter of women, actively arguing for the appointment of the later geneticist Kristine Bonnevie, against a background where, in his words, 'it is still far more difficult for women than for men to reach an independent position in society'.[22]

In this article it will become clear, however, that an institute or laboratory director with a clearly positive attitude towards women was not necessarily a prerequisite for women to be employed at such a place. Although we do not know of explicit statements by Baur on the women question, his behaviour suggests that he was not supportive of the women's movement, even though his Institute for Heredity Research was dominated by women staff members. The important point for us is that human and institutional factors in building genetic disciplinarity are best traced through action and event. The outstanding questions are as follows: first, what role did women play in the development of genetics as a discipline; second, was it limited, as Lenoir suggests, and if so what consequences did this have – for the discipline (*sui generis* and as a historical entity), for their own careers, and for the careers of their male superiors?

20 Ann B. Shteir, 'Botany in the breakfast room: women and early nineteenth-century British plant study', in Abir-Am and Outram, op. cit. (13), pp. 31–43.

21 Lenoir, op. cit. (4), p. 79.

22 Stamhuis and Monsen, op. cit. (11), pp. 444–445. See also Brigitte Bischof, 'The "Marie Curie syndrome": the role of mentors and romanticism or why were there so many women in radioactivity research in Vienna?', in Štrbáňová, Stamhuis and Mojsejová, op. cit. (13), pp. 639–658; Maria Rentetzi, 'Gender and radioactivity research in interwar Vienna: the case of the Institute for Radium Research', in Štrbáňová, Stamhuis and Mojsejová, op. cit. (13), pp. 611–638; Astrid Schürmann, 'Promoting international women's research on radioactivity: Marie Curie and the laboratory', in Štrbáňová, Stamhuis and Mojsejová, op. cit. (13), pp. 579–589; Marelene F. Rayner-Canham and Geoffrey W. Rayner-Canham (eds.), *A Devotion to Their Science: Pioneer Women of Radioactivity*, Quebec: McGill-Queen's University Press, 1997.

The study of heredity in Germany

In the early twentieth century, it was not obvious which part of the previous and multivalent study of heredity would become part of the new discipline of genetics, and which would not.[23] Heredity originally was taken to include not only the transmission of characters from parents to offspring, but also the development of a fertilized egg into an embryo and into an adult organism, and included physiological investigations conducted to support theories of this type. By the second half of the nineteenth century in Britain, various theories of the transmission of heredity had been formulated, all of them quite speculative. It resulted in two conflicting statistical approaches, and ultimately one of these, Mendelian genetics, became the dominant form of genetics at the expense of the biometrical approach.

In the case of Germany, Jonathan Harwood has distinguished two traditions between 1900 and 1933: a 'comprehensive' and a 'pragmatic' one, each with its own style of scientific thought.[24] The most important concentration of researchers in the comprehensive tradition could be found in Göttingen, notably the zoologist Alfred Kühn and the botanist Fritz von Wettstein. These viewed Mendelian and Morganian genetics (the pragmatic tradition), of which the Baur school was the most dominant representative, as much too narrow, because it did not take the development of an organism into account. In addition, the comprehensives thought that concentrating on the cell nucleus and ignoring the influence of the cytoplasm could not result in a comprehensive theory of inheritance. They found that in Mendelian genetics there was too much emphasis on overly specialist experiments; the problems that were studied seemed simply irrelevant. Nor did they like the quantitative and statistical approach connected with Mendelian and Morganian genetics.[25] Mendelians originally did not pay attention to the physiological basis of heredity; they collected mainly quantitative data from hybridization experiments.[26]

Another factor in the development of German genetics was the potential applicability of the new science. The new discipline was expected to help increase agricultural yields, make nicer and better gardens, and improve the quality of the human race.[27] In Germany the first genetics institute was founded at the Agricultural College in Berlin in 1914. The promise of the applicability of the results of genetics in agriculture was explicitly stated as an important reason for this establishment. In Britain the similarly intended John Innes institute was in 1910 privately funded by horticultural and agricultural

23 Sapp, op. cit. (10); Bowler, op. cit. (6) 1989; Harwood, op. cit. (10); Kohler, op. cit. (4).

24 Harwood, op. cit. (10).

25 Gerta von Ubisch, 'Aus dem Leben einer Hochschuldozentin', *Mädchenbildung und Frauenschaffen* (1956), pp. 413–422, 498–507, and (1957), pp. 35–45, 421.

26 Olby, op. cit. (7); Nils Roll-Hansen, 'Sources of Wilhelm Johannsen's genotype theory', *Journal of the History of Biology* (2009) 42, pp. 457–493.

27 On agriculture see Barbara Kimmelman, 'A progressive era discipline: genetics at American agricultural colleges and experiment stations, 1900–1920', PhD dissertation, University of Pennsylvania, 1987; Rheinberger and Müller-Wille, op. cit. (10); 173–174; on horticulture see Robert C. Olby, 'Horticulture: the font for the baptism of genetics', *Nature Reviews: Genetics* (2000) 1, pp. 65–70; and Olby, 'Mendelism: from hybrids and trade to a science', *Comptes rendus de l'Académie des sciences* (2000) 323, pp. 1043–1051; on eugenics see Daniel J. Kevles, *In the Name of Eugenics: Genetics and the Use of Human Heredity*, Cambridge, MA: Harvard University Press, 1985; Rheinberger and Müller-Wille, op. cit. (10), esp. Chapter 5 and 175–181.

entrepreneurs. In the Netherlands and in the US, a promising applicability to agriculture was also important, and in Norway and the US eugenics played a role in the foundation and development of genetics.[28]

The fact that the first German institute for genetics was established at an agricultural college must also be understood against the background of a hierarchy in institutions of higher education in Germany at the start of the twentieth century. The universities were the most prestigious, followed by the technical colleges, and at the bottom were the agricultural colleges and other specialized institutions. The first genetics institute belonged to an organization of higher education with relatively low prestige. Moreover, between 1900 and 1930 there were no institutes or chairs exclusively devoted to genetics in German universities. The Kaiser-Wilhelm-Institut für Biologie (Kaiser Wilhelm Institute for Biology, KWIB, f. 1914), was until about 1933 the only other major institution, besides Baur's, to carry out hereditarian research. Two of the five divisions of the KWIB focused on genetics: Carl Correns (1864–1933) was head of plant genetics and Richard Goldschmidt (1878–1958) headed research into animal genetics. Some of the members of the protozoology division of Max Hartmann (1876–1962) were also working on questions of heredity. Although some of the staff in this institute were working within the Mendelian and Morganian framework, the dominant approach was the comprehensive one.[29] Like Baur's Agricultural Institute, the KWIB employed female staff.[30] The genetics institute of the agricultural institute, however, was the only German genetics research centre that consistently had an almost exclusively female staff.

In the following parts of this paper, a narrative of the Institute for Heredity Research, from its inception until the departures of Baur in 1929 and Schiemann in 1931, is constructed and discussed (Figure 1). Special attention is paid to the scientific staff members and their work, with the aim of uncovering the role of gender in this history. Additionally, the Institute for Heredity Research will briefly be compared with its 'sister': the John Innes Horticultural Institute.

The Berlin Institute for Heredity Research: early years

It took Erwin Baur longer than he had hoped to establish his own genetics institute.[31] He was educated in medicine and gained his doctoral degree in 1900, but he had been

28 Stamhuis, op. cit. (11); Robert C. Olby, 'Scientists and bureaucrats in the establishment of the John Innes Horticultural Institution under William Bateson', *Annals of Science* (1989) 46, pp. 497–510; Alan G. Cock and Donald R. Forsdyke, *Treasure Your Exceptions: The Science and Life of William Bateson*, New York: Springer, 2008; Garland E. Allen, 'The Eugenics Record Office, Cold Spring Harbor, 1910–1940', *Osiris* (1986), 2nd series 2, pp. 225–264.

29 Both the work done at the Kaiser-Wilhelm-Institut für Anthropologie, menschliche Erblehre und Eugenik (Kaiser Wilhelm Institute for Anthropology, Human Genetics and Eugenics) and the Kaiser-Wilhelm-Institut für Hirnforschung (Kaiser Wilhelm Institute for Brain Research) is not relevant in the considered period of our study.

30 Vogt, *Vom Hintereingang zum Hauptportal?*, op. cit. (14); Vogt, 'Wissenschaftlerinnen in Kaiser-Wilhelm-Instituten A–Z', op. cit. (14).

31 Mathilde Schmitt, 'Aufbrüche und Umbrüche in der experimentellen Genetik: Erwin Baurs Personalpolitik unter den Genderperspektive', in Nürnberg, Höxtermann and Voigt, op. cit. (12), pp. 390–

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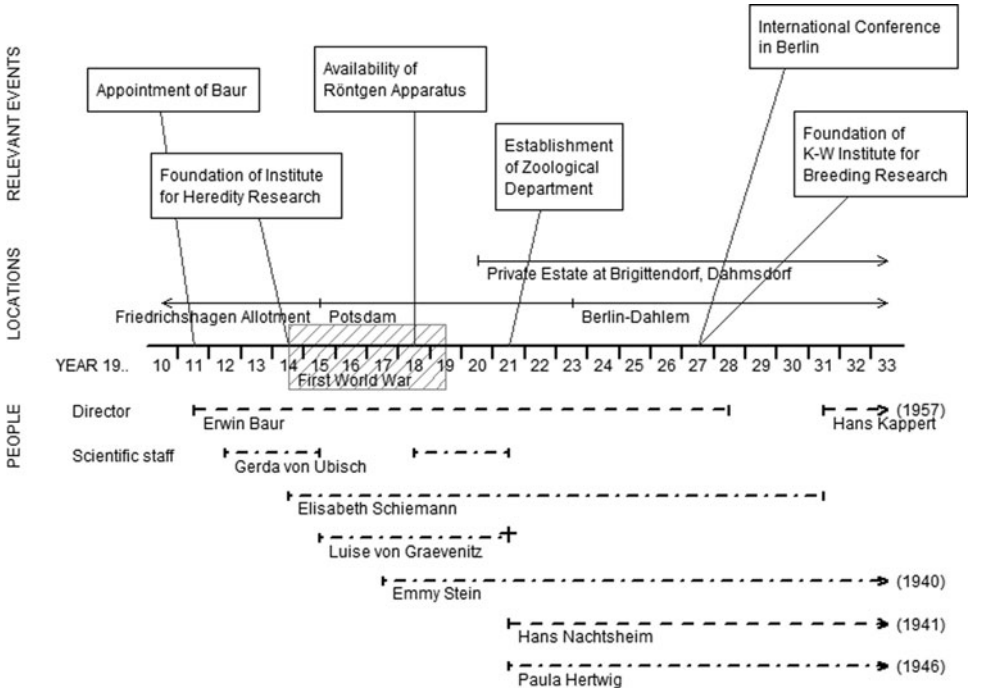

Figure 1. Timeline of the Institute for Heredity Research of the Agricultural College in Berlin (1911) 1914–1933, prepared by Janneke M. Ravenek.

interested in botany from early on (Figure 2).[32] In 1904 he came to Berlin to work as a first assistant at the Botanical Institute of the Friedrich Wilhelm University (now the Humboldt University of Berlin). He looked for a research area of his own and he realized that he might be able to occupy the as yet (in Germany) rather empty niche of Mendelian genetics. He began to do hybridization experiments with snapdragons (*Antirrhinum majus*), an established model plant in evolutionary and hereditary studies in Britain. He established a private allotment in Friedrichshagen in the east of Berlin. In addition to snapdragons, he grew and hybridized grains, potatoes and cabbage. In 1905 he began to offer a course on Mendelian genetics at the University of Berlin. In 1908 he was the main founder of the *Zeitschrift für inductive Abstammungs- und Vererbungslehre* (Journal of Inductive Science of Descent and Heredity), which can be considered the first journal of genetics (William Bateson (1861–1926) would follow in 1910 with the

409, focuses on Baur and his two institutes, but is brief about the genetics institute. Deichmann, op. cit. (19), gives interesting biographies of the individual women.

32 Elisabeth Schiemann, 'Erwin Baur', *Berichte der Deutschen Botanischen Gesellschaft* (1935) 52, pp. 51–114; Rudolf Hagemann, *Erwin Baur (1875–1933): Pionier der Genetik und Züchtungsforschung. Seine wissenschaftlichen Leistungen und ihre Ausstrahlung auf Genetik, Biologie und Züchtungsforschung von heute*, Eichenau: Verlag Roman Kovar, 2000; W. Rudorf, *Dreißig Jahre Züchtungsforschung: Zum Gedenken an Erwin Baur, 16.4.1875–2.12.1933*, Stuttgart: Gustav Fischer Verlag, 1959.

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Figure 2. Demonstrations of the botanical work by Erwin Baur at the Berlin International Congress of Genetics, 1927. Hans Nachtsheim, *Verhandlungen des V. Internationalen Kongresses für Vererbungswissenschaft Berlin 1927 Band*, Leipzig: Verlag von Gebrüder Borntraeger, 1928, p. 79.

Journal of Genetics). In 1911 Baur's textbook *Einführung in die Experimentelle Vererbungslehre* (Introduction to the Experimental Science of Heredity) was published and would be reprinted several times.[33]

In the same year, 1911, Erwin Baur was appointed professor of botany at the Agricultural College in Berlin and his remit was to focus on heredity (see Figure 1). He was promised his own institute with an official assistant. Before this was realized, in 1912 he appointed his first scientific worker, Gerta von Ubisch (1882–1965). She came from an intellectual Jewish background; her father was an art historian. After attending a girls' school, from which she graduated with a *Gymnasium* diploma, she became one of the first female physics students in Germany. She first attended the university in Heidelberg but then moved to Freiburg, because, according to her, 'at the

33 Erwin Baur, *Einführung in die experimentelle Vererbungslehre*, Berlin: Verlag von den Gebrüder Borntraeger, 1911. In this period there appeared also textbooks by the botanist Goldschmidt and by the zoologists Valentin Häcker and Ludwig Plate.

https://doi.org/10.1017/S0007087417000048 Published online by Cambridge University Press

universities in Baden the conditions for women studying were the most favourable' – and then to Berlin.[34]

Gerta von Ubisch eventually decided that she was not really talented in physics, so after her PhD in 1911 she moved into botany and was advised to go to Baur.[35] 'Baur was full of enthusiasm for his field'.[36] He welcomed her, promised to teach her botany and said that once the official position of assistant in genetics became available, she would be appointed. For the time being he gave her an agricultural allowance for the study of grains.[37]

Contrary to his promises, Baur did not make her his official assistant. He said, whether by way of reason or excuse, that 'the Agricultural College found it important that the assistants were from an agricultural background with a diploma in breeding theory'.[38] He therefore advised her to accept the position of assistant that was being offered by Correns at the new Kaiser Wilhelm Institute for Biology, which at the time was located in Münster. In the spring of 1914 she moved to this institute, where Hans Kappert, whom we shall meet again, was very helpful to her.[39] Because, according to von Ubisch, Correns was afraid that Baur would lay claim to her work, she did not get the opportunity to do her own research.[40] In 1915 she decided to leave the institute and she accepted various jobs in the private plant-breeding sector.[41]

In the meantime, in 1914, the Prussian parliament had made funding available for the establishment of the Institute for Heredity Research at the Agricultural College.[42] Baur appointed Elisabeth Schiemann (1881–1972) his scientific assistant.[43] She also came from an educated background, the daughter of Theodor Schiemann, a professor of east European history at the University of Berlin.[44] She became a teacher, which at the

34 Von Ubisch, op. cit. (25), pp. 414–416, 416: 'An den badischen Hochschulen lagen die Verhältnisse für das Frauenstudium am günstigsten, da Baden sehr liberal war.' Vogt, op. cit. (14) 'Wissenschaftlerinnen in Kaiser-Wilhelm-Instituten', pp. 191–192.

35 Von Ubisch, op. cit. (25), pp. 420–421.

36 Von Ubisch, op. cit. (25), p. 421: 'Baur was von ehrlicher Begeisterung für sein Fach erfüllt.'

37 Von Ubisch, op. cit. (25), p. 421.

38 Von Ubisch, op. cit. (25), p. 498: 'die landwirtschaftliche Hochschule darauf Wert legte, daβ die assistenten Landwirte wären, die ihr Diplom in Züchtungslehre machten'.

39 Von Ubisch, op. cit. (25), p. 500.

40 Von Ubisch, op. cit. (25), pp. 499–500.

41 Von Ubisch, op. cit. (25), pp. 500–503.

42 Schiemann, op. cit. (32), pp. 72–73, 77.

43 Schiemann, op. cit. (32), pp. 79–81.

44 Hermann Kuckuck, 'Elisabeth Schiemann 1881 bis 1972', *Berichte der Deutsche Botanische Gesellschaft* (1980) 93, pp. 517–537; Anton Lang, 'Elisabeth Schiemann: life and career of a woman scientist in Berlin', *Englera* (1987) 7, pp. 17–28; Elisabeth Schiemann, 'Erinnerungen an meine Berliner Universitätsjahre', in *Studium Berolinense: Gedenkschrift der Westdeutschen Rektorenkonferenz und der Freien Universität Berlin zur 150. Wiederkehr des Grundungsjahres der Friedrich-Wilhelms-Universität zu Berlin*, Berlin, 1960, pp. 845–856; Deichmann, op. cit. (19), esp. pp. 232–236; Elvira Scheich, 'Science, politics, and morality: the relationship of Lise Meitner and Elisabeth Schiemann', *Osiris* (1997), pp. 143–168; Scheich, 'Elisabeth Schiemann (1881–1972): Patriotin im Zwiespalt', in Suzanne Heim (ed.), *Autarkie und Ostexpansion: Pflanzenzucht und Agrarforschung im Nationalsozialismus*, Göttingen: Wallstein Verlag, 2002, pp. 250–229; Vogt, 'Wissenschaftlerinnen', op. cit. (14), pp. 164–67. A volume dedicated to Schiemann was published recently: Nürnberg, Höxtermann and Voigt, op. cit. (12).

time was one of the few professional opportunities open to women. In 1906, she became an auditor in the natural sciences at the University of Berlin. She could not become a regular student, as women were not yet admitted. Baur directed her attention to genetics. According to her later pupil Herman Kuckuck, Baur drew Schiemann 'entirely under his spell'.[45] In 1912, supervised by Baur, she earned her doctorate at the University of Berlin with a thirty-five-page paper on mutations.[46] And in 1914 she received the job that was originally promised to von Ubisch.

When the war broke out, Baur had to join the army as a physician and all efforts related to acquiring premises for the institute came to a standstill. In 1915, however, the Agricultural College acquired a plot of land in Potsdam. Baur's institute was given part of it and under the supervision of Schiemann provisional facilities were built where plants as well as animals could be investigated. These premises developed into a fruitful place for research.[47] For the time being the research was largely but not exclusively restricted to plants. Baur also worked with rats, dogs and rabbits.[48]

Two new members were added to the staff during the war, again women (Figure 1). Luise von Graevenitz (1877–1921) joined the institute in 1915 and Emmy Stein (1879–1954) in 1917.[49] Luise von Graevenitz was educated at the Horticultural School for women in Marienfelde near Berlin, where she met Emmy Stein in 1904. They became lifelong friends. Emmy Stein came from a family of wealthy industrialists in Düsseldorf, which made her independent throughout her life.[50] After Marienfelde, von Graevenitz and Stein went to the Botanical Institute of the University of Berlin, where they met Baur and prepared themselves for a journey to the tropics. In 1906–1907 they stayed in the Dutch East Indies and visited Japan. On their return they studied botany in Zurich. Both gained their PhDs in botany in 1913 in Jena.[51] During the war von Graevenitz worked at Baur's newly established institute and did 'a very promising study on potatoes'.[52] She was not employed by the Agricultural College, but was modestly paid from other sources.[53] In 1917 Emmy Stein became a volunteer assistant, which meant a low salary or no salary at all. In 1918 Gerta von Ubisch returned to Baur's institute. Baur had obtained funding to work on plants used for making fabrics, because during the war and its aftermath it was difficult for Germany

45 Kuckuck, op. cit. (44), p. 520: 'ganz in seinen Bann'.

46 Elisabeth Schiemann, 'Über Mutationen bei Aspergillus niger van Tiegh', thesis, Friedrich-Wilhelm-Universität Berlin, 1912.

47 Schiemann, op. cit. (35), p. 80.

48 Schiemann, op. cit. (35), p. 79; *Jahresbericht der Königlichen Landwirtschaftlichen Hochschule in Berlin* (1914–1915) 23, p. 39. Dogs were given up later, because they were too expensive. *Jahresbericht* (1915–1919) 24–27, p. 50.

49 Von Ubisch, op. cit. (27), pp. 503–504.

50 Elisabeth Schiemann, 'Emmy Stein, 21.VI.1879–21.IX.1954', *Der Züchter* (1955) 25, pp. 65–67; Vogt, 'Wissenschaftlerinnen', op. cit. (14), pp. 178–179; Deichmann, op. cit. (19), p. 265.

51 Schmitt, op. cit. (31), pp. 390–409, 395.

52 Von Ubisch, op. cit. (27), pp. 503–504: 'eine sehr aussichtsreiche Arbeit über Kartoffeln'.

53 Baur gave her Schiemann's salary when Schiemann was absent during a couple of months in 1918. GSPK: I. HA Rep 87. Min. Landw. Dom. Forsten. Abt. I. A. LWH. Nr. 20103, Assistenten Band 1, 189, letter from Baur, 02-09-1918.

to import foreign plants like cotton and sisal. Von Ubisch also continued work on barley. She characterized her work with Baur as a 'sweet and valuable cooperation'.[54]

During this period a couple of research lines can already be detected. Baur intended to do the research on snapdragons as a botanical parallel to the work on fruit flies by Thomas Hunt Morgan (1866–1945) and his co-workers in the USA.[55] He became interested in the relevance of mutations for evolution and for eugenics.[56] At Baur's initiative, Emmy Stein was one of the first to study the effect of radiation on plants, which was possible because of the availability of an X-ray machine acquired with the help of the Notgemeinschaft der Deutschen Wissenschaft (Emergency Council for German Science).[57] She studied irradiated snapdragons and discussed various aspects of irradiated tissue that became affected by cancer.[58] In addition, Mendelian genetics was considered promising to produce useful applications in agriculture. When Baur's institute was established the board of the Agricultural College insisted that he work with agriculturally interesting organisms. Baur cherished his good relationship with agricultural organizations in Germany. They made funds available for this kind of research, which Baur farmed out to his assistants. Von Ubisch's and Schiemann's research focused on Mendelian factors by hybridizing barley varieties. Schiemann tried to find a winter-hardy variety.[59]

That von Ubisch and Schiemann did similar research resulted in a difficult relationship between the two. According to a recent biographer of von Ubisch, 'An agreement with Miss Schiemann … to wait with publication until Ubisch had completed her work on barley was not fulfilled by Miss Schiemann, and therefore both scientists published their work at the same time'.[60] According to the biographer, von Ubisch concluded in 1921 that she had to abandon 'the cooperation with Baur which was so dear and

54 Von Ubisch, op. cit. (27), p. 505: 'Liebe und wertvolle Zusammenarbeit'.

55 His monograph on snapdragons appeared in 1924. Erwin Baur, *Untersuchungen über das Wesen, die Entstehung und die Vererbung von Rassenunterschieden bei Antirrhinum majus, Bibliotheca Genetika* (1924) 4.

56 Erwin Baur, 'Die Bedeutung der Mutation für das Evolutionsproblem', *Zeitschrift für induktive Abstammungs- und Vererbungslehre* (1925) 37, pp. 107–115; Alexander von Schwerin, 'Seeing, breeding and the organisation of variation: Erwin Baur and the culture of mutations in the 1920s', in Staffan Müller-Wille and Hans-Jörg Rheinberger (eds.), *A Cultural History of Heredity IV: Heredity in the Century of the Gene*, Berlin: Max Planck Institute for the History of Science, Preprint 343, 2008, pp. 259–278.

57 Schiemann, op. cit. (32), p. 83; Schiemann, op. cit. (50).

58 Amongst others, see Emmy Stein, 'Zur Entstehung und Vererbung der durch Radiumbestrahlung erzeugten Phytocarcinome', *Zeitschrift für induktive Abstammungs- und Vererbungslehre* (1932) 62, pp. 1–14; Deichmann, op. cit. (19), pp. 264–265.

59 Gerta von Ubisch, 'Analyse eines Falles von Bastardatavismus und Faktoren-koppelung bei Gerste', *Zeitschrift für induktive Abstammungs- und Vererbungslehre* (1915) 14, pp. 226–237, and other publications. Elisabeth Schiemann, 'Zur Genetik des Sommer- und Wintertypus bei Gerste', *Zeitschrift für induktive Abstammungs- und Vererbungslehre* (1925) 37, pp. 139–209, and other publications.

60 Gudrun Fischer, 'Gerta von Ubisch (1882–1965): erste Professorin an der Universität Heidelberg', in Fischer (ed.), *Darwins Schwestern: Porträts von Naturforscherinnen und Biologinnen*, Berlin: Orlanda Berlin, 2009, pp. 105–123, 109–110: 'Eine Vereinbarung mit Frl. Schiemann, die ebenfalls über Gerste forscht, mit einer Veröffentlichung zu warten, bis Ubisch ihre Arbeit zu Gerste abgeschlossen hat, wird von Frl. Schiemann nicht eingehalten, und so veröffentlichen die beiden Wissenschaftlerinnen zur gleichen Zeit ihre Arbeiten'.

valuable' to her.[61] She went to Heidelberg where she taught genetics.[62] As she was Jewish, she was dismissed in the Nazi period. She travelled to Brazil in 1935, had various jobs in genetics and biology, and returned to Heidelberg in 1952 where she had to accept jobs outside biology. She died in 1965.[63]

In 1919, after the war, Baur started to lecture again and Schiemann prepared the practical parts.[64] Four scientific assistants were working at Baur's institute, all of them female (see Figure 1). This composition was unusual and their positions were weak. In a list of the salaried official assistants of the Agricultural College from 1920, Schiemann was the only female on the list.[65] Her salary was RM 3,900, almost the lowest on the scale of salaries.[66] In a list of the paid assistants of the Agricultural College, there were eight assistants who were partly or entirely remunerated from external funds and three of these were women. It is interesting to note that of these assistants, the only two who had a doctorate were female: von Graevenitz and von Ubisch.[67] They were apparently overqualified. During this period Baur wrote a letter to the rector, in which he stated that his fund assistants did the same work as the regular assistants and therefore should earn the same salary.[68] He asked for financial compensation for von Graevenitz and von Ubisch.[69] In 1924 he stated that it was unacceptable that his assistants were only employed for short periods of time, because their research would become 'entirely invalid'.[70] It is thus clear that Baur employed these women in temporary and lower positions and at a lower salary than they should have had in view of the work they did. Stein was not mentioned, because she did not have an appointment, but her case was of course even more compelling. Unfortunately, in 1921 von Graevenitz died of diabetes; this affected the whole institute but in particular Emmy Stein, with whom she lived as well as worked.[71]

Right from the start, Baur decided that the work at the institute should not be restricted to plant genetics. In 1921 two zoologists, Hans Nachtsheim and Paula Hertwig, became attached to the institute. Hans Nachtsheim (1890–1979), the first male assistant, came from a relatively intellectual background, as his father was a

61 Von Ubisch, op. cit. (27), p. 505: 'die mir so liebe und wertvolle Zusammenarbeit mit Baur'.

62 Von Ubisch, op. cit. (27), pp. 505, 35–38.

63 Deichmann, op. cit. (19), pp. 255–258.

64 Kuckuck, op. cit. (44), p. 521.

65 GSPK, I. HA Rep 87. Min. Landw. Dom. Forsten. Abt. I A. LWH. Nr. 20103, Assistenten Band 1, pp. 262–263.

66 The salaries ranged from RM 3,420 to 8,380. GSPK, I. HA Rep 87. Min. Landw. Dom. Forsten. Abt. I A. LWH. Nr. 20103, Assistenten Band 1, pp. 262–263.

67 GSPK, letter from the rector, 10 November 1920, I. HA Rep 87. Min. Landw. Dom. Forsten. Abt. I A. LWH. Nr. 20104, Assistenten Band 2, pp. 1–5.

68 GSPK, I. HA Rep 87. Min. Landw. Dom. Forsten. Abt. I A. LWH. Nr. 20104, Assistenten Band 2, pp. 1–5, letter from Baur, 12-11-1920.

69 He mentioned DM 13,680 once for von Graevenitz: GSPK, I. HA Rep 87. Min. Landw. Dom. Forsten. Abt. I A. LWH. Nr. 20103, Assistenten Band 2, pp. 1–5.

70 GSPK, I. HA. Rep. 87. Min LDF. B. Nr. 20282, Abt. I. Akten Landw. Hochschule B, Inst. Vererb. Forschung, Juni 1923 to Jan. 1927, letter from Baur, 09-10-1924, p. 113: 'volkommen wertlos.'

71 Schiemann,op. cit. (50), p. 67: 'Der frühe Tod von Luise von Graevenitz, mit der sie all die Jahre zusammen im gemeinsamen Heim gelebt hat, hat Emmy Stein tief getroffen.'

geheimer Justizrat (privy counsellor).[72] Nachtsheim studied at the universities of Bonn and Munich. His thesis of 1913 and his *Habilitationsschrift* discussed the cytological characteristics of the sex differences of bees.[73] Between 1915 and 1919 he was in the military, but this did not prevent him from accepting the position of assistant at the Zoological Institute of the University in Munich in 1916. Here he worked under the directorship of the professor of zoology, Richard Hertwig (1850–1937). In 1919 he became a *Privatdozent* (unsalaried lecturer) in zoology, and while in this position he came into contact with Erwin Baur and learnt about Morgan's school of fruit fly research.[74] He then published a long overview article on the heredity of fruit flies and the German translation of Morgan's famous book.[75]

A document written by Nachtsheim in 1929 makes clear that it was difficult for Baur to recruit suitable personnel. The way Baur tried to employ this zoologist reveals to what lengths he was willing to go.[76] In January 1920 Baur sent Nachtsheim a letter saying that he was looking for a head of the zoological department at his institute.[77] He asked whether Nachtsheim was interested, but also pointed out the risks involved in accepting such a position: Nachtsheim should take into account that a position at the Agricultural College would diminish the chance of an academic post at a university. Moreover, he would have to specialize in a field in which professorships were not yet available. In a subsequent letter Baur stated that he could offer him a permanent job with a salary of between about RM 8,000 and 9,000. In March, Baur informed Nachtsheim that the position needed final approval by the Prussian parliament. In a letter in June Baur wrote that everything would be settled before the winter semester. The salary of a head of department would start at RM 18,000.[78] This was apparently so attractive that Nachtsheim resigned from his position in Munich. In early September, however, he received a telegram from Baur telling him to stay on in Munich until spring. But Nachtsheim's former position had already been offered to someone else. Fortunately he could be reappointed, albeit in a lower position. In November 1920 Baur once again asked

72 GSPK , I. HA Rep. 87B, Nr. 20101, Akten Privatdoz. Von 1913–1928, 90–91, short autobiography.

73 Hans Nachtsheim, 'Cytologische Studien über die Geschlechtsbestimmung bei der Honigbiene (Apis mellifaca L.)', *Arch. Zellforsch.* (1913) 11, pp. 169–241, and other publications.

74 Ute Deichmann, *Biologists under Hitler*, Boston: Harvard College Boston, 1996, p. 231; Alexander von Schwerin, *Experimentalisierung des Menschen: Der Genetiker Hans Nachtsheim und die vergleichende Erbpathologie 1920–1945*, Göttingen: Wallstein, 2004; Hans Nachtsheim, 'Ein halbes Jahrhundert Genetik: akademische Festrede zur Immatrikulationsfeier am 2. Juni 1951', *Veröffenlichung der Freien Universität Berlin*, Berlin and Munich: Verlag Duncker & Humblot, 1951, p. 10; Klaus Günther, Walter Hirsch and Dorothea Opitz, *Bibliographie und Biographie Hans Nachtsheim zum 80. Geburtstag, 13 Juni 1970*, Munich, 1980.

75 Hans Nachtsheim, 'Die Analyse der Erbfaktoren bei *Drosophila* und deren zytologische Grundlage, *Zeitschrift für induktive Abstammungs- und Vererbungslehre* (1919) 20, pp. 118–156; Thomas Hunt Morgan, *Die Stoffliche Grundlage der Vererbung* (translation by Hans Nachtsheim of *The Physical Basis of Heredity*), Berlin: Borntraeger, 1921.

76 Hans Nachtsheim, 'Auszug aus meinen Verhandlungen mit Professor Baur', GSPK, Ministerium Landwirtschaft (Ministry of Agriculture) and AMPG: Nachlass Max Hartmann (Dir. K-W-I Biologie Berlin) 1929. In November 1930 he would bring an English text of thirty-four pages into circulation: Hans Nachtsheim, *My Complaint against Professor Dr. Erwin Baur* (GSPK: Ministerium Landwirtschaft 1930).

77 Nachtsheim, 'Auszug aus meinen Verhandlungen', op. cit. (76), p. 1.

78 Nachtsheim, 'Auszug aus meinen Verhandlungen', op. cit. (76), p. 2.

Nachtsheim to join his institute and become head of department. Nachtsheim hesitated, but Hertwig advised him to go ahead, because it would improve his position.[79]

Finally, in 1921, Nachtsheim began work in Baur's institute. He acted as the head of the zoological department but his appointment was that of an assistant. The position of department head appeared not to exist. Nachtsheim 'felt paralysed'.[80] Nachtsheim later commented that the discrepancy between his position and Baur's promises had soured the years that he spent at the institute.[81] That Nachtsheim felt badly treated did not prevent him from making important contributions to the institute's activities. In 1928 he published an account of his experiments on the topography of the X-chromosome of the fruit fly.[82] He also initiated new research lines that strengthened the focus of the institute on the study of agriculturally interesting organisms. He discussed the origin of rabbit races from a genetic point of view and discussed, within the Mendelian framework, the heredity of quality and colour of fur. In 1935 his monograph on the heredity of rabbits appeared.[83]

Paula Hertwig (1889–1983) joined the institute in 1921.[84] She was a daughter of Richard Hertwig's brother Oscar, a professor of anatomy at the university in Berlin. In 1908, the university had just opened its doors to women and she studied zoology, botany, chemistry and comparative anatomy and attended Baur's courses.[85] She worked on cytogenetics at her father's laboratory and collaborated with her brother Günther (1888–1970) on studies of the effect of radiation and chemicals on the division of cells in frog and fish embryos. In 1916 she received her PhD *magna cum laude* and in 1919 she was the first woman to gain the right to teach in the philosophical faculty of the University of Berlin.[86] In 1921 Hertwig was employed at Baur's institute as a supernumerary assistant, which meant no job security and a lower salary than a regular assistant. She began to study the heredity of chickens. In preparation she studied the work of Bateson and his co-workers, amongst others Edith Saunders, and of many others. She completed this research by writing an overview article for a wider audience in 1930.[87] Her research on irradiated mice and rats would become her best-known work.[88] She

79 Nachtsheim, 'Auszug aus meinen Verhandlungen', op. cit. (76), p. 3.

80 Nachtsheim, 'Auszug aus meinen Verhandlungen', op. cit. (76), p. 4: 'Ich war wie erlähmt.'

81 Nachtsheim, 'Auszug aus meinen Verhandlungen', op. cit. (76), p. 4, underlining in original: 'Von der mir seit mehr als acht Jahren gegebenen Versprechungen ist nichts erfüllt.'

82 Hans Nachtsheim, 'Beitrag zur Topographie des X-Chromosoms von *Drosophila melanogaster*', *Zeitschrift für induktive Abstammungs- und Vererbungslehre* (1928) 48, pp. 245–258.

83 Hans Nachtsheim, *Von Wildtier zum Haustier*, Berlin: Alfred Metzner, 1935.

84 Sybille Gerstengarbe, *Paula Hertwig – Genetikerin im 20. Jahrhundert: Eine Spurensuche*, Stuttgart: Wissenschaftliche Verlagsgesellschaft Stuttgart, 2012.

85 Plarre, op. cit. (2), p. 171.

86 Paula Hertwig, 'Durch Radiumbestrahlung verursachte Entwicklung von halbkernigen Triton- und Fischembryonen', *Archiv für mikroskopische Anatomie* (1916) 87, pp. a63–a122.

87 Paula Hertwig, 'Was wissen wir über die Vererbung der Nutzeigenschaften bei den Haushühnern?' *Der Züchter* (1930) 2, pp. 44–50.

88 These animals were demonstrated to the attendants at the International Conference of Genetics in Berlin. Paula Hertwig, 'Sterilitätserscheinungen bei röntgenbestrahlten Mäusen', *Zeitschrift für induktive Abstammungs- und Vererbungslehre* (1935) 70, pp. 517–523, and later articles.

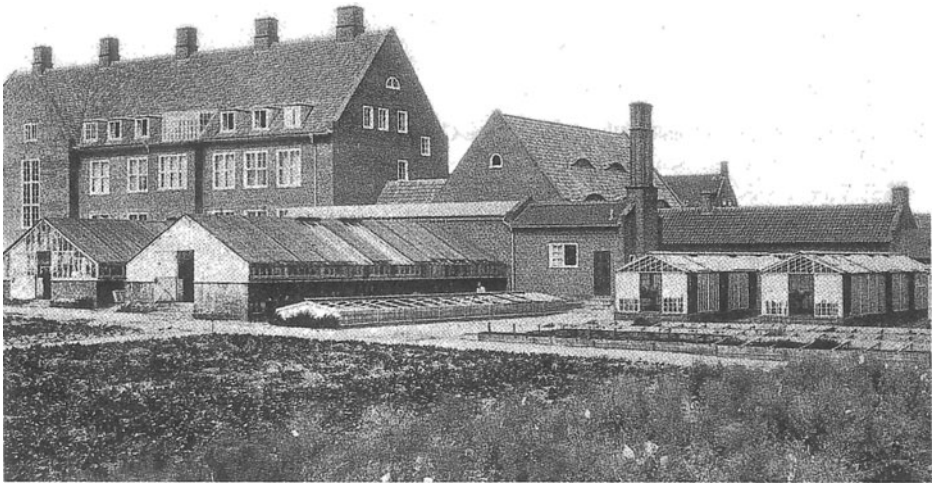

Figure 3. Institute for Heredity Research of the Agricultural College, Berlin-Dahlem. Nachtsheim, op. cit., p. 4.

discussed the influence of radiation on humans and human heredity and concluded that radiation therapy could harm offspring.[89]

Although the Potsdam provisional institute became, according to Schiemann, a fruitful place for research in plant as well as animal genetics, Baur put tireless efforts into acquiring new premises.[90] He found that he did not receive sufficient funding. To get his requests accepted he made a variety of threats to the officials of the Agricultural College.[91] According to Schiemann in her biography of Baur, 'And so the years after 1918 became a bitter, often desperate, struggle. The delay of the construction of the Dahlem institute strained Baur's nerves.'[92]

In 1922 Baur convinced the Emergency Council for German Science that he needed new facilities and an impressive institute was inaugurated in Berlin-Dahlem in the summer of 1923 (Figure 3).[93] Schiemann was involved in the institute's move. The new institute was equipped in the most advanced way and the non-academic staff consisted of one female stenotypist, a female laboratory assistant, three garden workers, five male and four female workers, and one boilerman.[94] By 1925 no fewer than seventy

89 Paula Hertwig, 'Die künstliche Erzeugung von Mutationen und ihre theoretischen und praktischen Auswirkungen', *Zeitschrift für induktive Abstammungs- und Vererbungslehre* (1932) 61, pp. 1–35.

90 Schiemann, op. cit. (35), p. 80.

91 Harwood, op. cit. (10), p. 149.

92 Schiemann, op. cit. (35), p. 81: 'Und so sind die Jahre von 1918 ab eine erbitterter, oft verzweifelter Kampf gewesen. Die Verschlepping des Dahlemer Institutsbaus hatte Baur's Nerven angespannt.'

93 GPSK, I.HA Rep 76 Kultusministerium, Sekt. 1 Nr. 14 Bd. 4, Die Landwirtschaftlichen Institute, July 1920–April 1925, letter, 29 May 1922.

94 These details are to be found in a letter of 1929 to a potential successor of Baur. There is no information on the kind of work they did. GSPK, I HA. Rep 87, Ministerium für Landwirtschaft, Domänen und Forsten, B

students attended the plant breeding practicals and sixteen PhD students were working in the institute, several of them with snapdragons.[95]

Schiemann developed into 'something like an older sister to the young co-workers … tireless in helping them to start their own research projects and assisting them with various other problems as well'.[96] In 1924 she qualified officially as a *Privatdozent*. She enjoyed international contacts and visited colleagues in foreign countries.[97] To be able to perform interesting hybridization experiments, foreign varieties of cultivated plants were needed and Baur travelled widely to collect seeds. These collections stimulated Schiemann's interest in the origins of cultivated plants and she began to concentrate on morphological, cytological and geographical aspects of grasses and grains.[98] This research encompassed not only genetics, but also aspects of archaeology, history, ethnography and geography, and fitted the comprehensive style of the study of heredity in Germany. She was inspired by the theory on the centres of origin of cultivated plants of the Russian geneticist Nicolai Vavilov (1887–1943). In 1932 Schiemann's monograph on the emergence of cultivated plants appeared, and this became the standard text on the subject for the time.[99] This meant that thanks to her independent mind she managed to step out of the shadow of Baur's research programme and was able to develop a field of research not directly connected to the 'technoscience' done at the institute. We shall see what consequences this choice would have for her.

Baur strove for recognition, both in the scientific and the agricultural world, and adapted the institute's research accordingly. Together with Stein, Baur carried out a research programme with the snapdragon as model organism, a botanical parallel to what Morgan was doing with the fruit fly. The problems he studied had no obvious practical application. Stein also worked from a 'pure' scientific perspective in her study of irradiated snapdragons. She was not funded by anyone, neither the Agricultural College nor an agricultural agency, and therefore her research was free from the expectations of patrons, at least. All the other staff members of the institute studied the genetics of organisms that were relevant for agriculture or for human heredity. Von Ubisch and Schiemann studied barley and Schiemann also studied other grains and grasses, and tomatoes. Nachtsheim studied the fertility of pigs and the quality and colour of rabbit fur. Hertwig first studied the fertility and the productivity of chickens, and her later shift to irradiated mice was a result of the growing interest in human heredity. Both official assistants, Schiemann and Nachtsheim were apparently able to do

Nr. 20095, Akten betreffend: Landw. Hochschule in Berlin, Professoren (Dozenten) Band 7, nr. 4 1919–1930, letter to Roemer, 02-05-1929.

95 Schiemann, op. cit. (35), p. 82; Harwood, op. cit. (10), p. 200.

96 Lang, op. cit. (44), p. 20.

97 GSPK, I. HA. Rep. 87B, Nr. 20281, Min. L.D. F. Inst. Vererb. Forsch. 1920–1923, pp. 287–290, 427, letter from Nachtsheim (on behalf of Baur) to Rektor, 19-08-1922, and other documents.

98 Elisabeth Schiemann, 'Zytologische und pflanzengeografische Beiträge zur Gattung Aegilops III. Chromosomenzahlen und Morphologie', *Berichte der Deutsch. Bot. Gesellschaft* (1929) 47, pp. 164–181, and other publications; Ekkehard Höxtermann, 'Die genetischen Arbeiten Schiemanns', in Nürnberg, Höxtermann and Voigt, op. cit. (12), pp. 204–235, 214–215.

99 Elisabeth Schiemann, *Entstehung der Kulturpflanzen: Handbuch der Vererbungswissenschaft*, vol. 3, Berlin: Verlag von Gebrüder Borntraeger, 1932.

some 'pure' scientific work in which they could follow up on results published in the international literature. Nachtsheim's work on fruit flies and Schiemann's work on the history of cultivated plants fall into this category. However, this work was of secondary importance during our period of study. The conclusion is that the choice of research topics and organisms of the staff members can be understood against the background of the respective positions they held. And because their positions were not gender-neutral, one can say that there was a connection between the gender of the researcher and his or her research topic, albeit indirect. In turn, these research topics built the discipline of genetics, entraining various degrees of prestige and types of institutionalization in their wake.

The Berlin Institute for Heredity Research: The fifth congress and after

The two years culminating in the Fifth International Congress for Genetics (1927) were a period of activity and success. In 1925 Nachtsheim became an extraordinary professor and was asked to teach the genetics of domestic animals. In 1926 he visited Morgan's lab at Columbia University.[100] During this period Hertwig received Nachtsheim's salary and functioned as head of the zoological department. In 1927 she became an extraordinary professor at the Medical Faculty of the Berlin University. From that time on she held seminars and lectured on genetics for students of medicine.[101]

In this period Baur acquired the country estate of Brigittenhof in Dahmsdorf, forty kilometres south-east of Berlin, and was generally present at the institute no more than once a week.[102] Baur's frequent absence seems not to have prevented the institute from functioning successfully. Schiemann wrote, 'For us as members of the institute, the Dahlem period was after all pleasant and rich'. Notwithstanding the tensions between Baur and Nachtsheim, and those between Baur and Schiemann, which we will hear about, the atmosphere at the institute was apparently good on the whole.[103] Von Graevenitz and Stein, for example, were close friends. Schiemann had good relationships with Stein and Hertwig; she went on walking holidays with Hertwig in the mountains on several occasions. The photograph of the Christmas celebration in 1926 also gives the impression of a pleasant and cooperative atmosphere (Figure 4).[104]

The Fifth International Congress for Genetics, held in Berlin in September 1927, was a milestone.[105] It attracted a large number of participants, and an excursion to the

100 GSPK, I. HA. Rep. 87, Min LDF. B. Nr. 20282, Abt. I, Akten Landw. Hochschule B, Inst Vererb F, Juni 1923–Jan. 1927, p. 235, letter from Baur to Rektor, 27-05-1925, and other letters.

101 Sybille Gerstengarbe, 'The geneticist Paula Hertwig (1889–1983): a female scientist under various regimes', in Štrbáňová, Stamhuis and Mojsejová, op. cit. (13), pp. 295–317, 300–301.

102 Hans Stubbe, 'Gedächtnisrede auf Erwin Baur gehalten am 25. Todestag (2. Dezember 1958) in Müncheberg/Mark', *Der Züchter* (1959) 29, pp. 1–6, 4; Schiemann, op. cit. (35), p. 80.

103 Schiemann, op. cit. (35), pp. 82–84: 'Für uns Institutsmitglieder ist durch all das die Dahlemer Zeit schön und reich gewesen.'

104 Stamhuis and Richmond, op. cit. (12), pp. 74–76; Schmitt, op. cit. (31), p. 390, for the picture of the 1926 Christmas celebration.

105 Stamhuis and Richmond, op. cit. (12), pp. 71–75.

Figure 4. Christmas at the Institute, 1926. Reiner Nürnberg, Ekkehard Höxtermann und Martine Voigt, *Elisabeth Schiemann (1881–1972), Vom* Auf*Bruch der Genetik und der Frauen in den* Um*Brüchen des 20. Jahrhunderts*, Rangsdorf: Basilisken Presse, 2014, p. 390.

institute's premises and the aquarium was organized.[106] All the staff members of the institute contributed to making the conference a success. Both official assistants were deeply involved, but Nachtsheim's tasks were more visible than Schiemann's. As the secretary of the Organizing Committee, Nachtsheim was a central figure in the preparations, and edited the volume of abstracts and afterwards the voluminous proceedings.[107] He was a member of the so-called *Ehrenpräsidium* (honorary presidency) (Figure 5). During the excursion, Baur, Schiemann and Stein demonstrated the botanical work and Nachtsheim and Hertwig the zoological work. The whole international community of geneticists could see to what height Baur's institute had ascended. The conference greatly increased Baur's prestige; according to Hans Stubbe, one of his later pupils, 'he reaped great successes at that time. His name was known all over the world and his voice was heard everywhere'.[108]

106 Hans Nachtsheim, 'V. Internationalen Kongresses für Vererbungswissenschaft Berlin 1927: Programm und Referate der angemeldeten Vorträge', *Zeitschrift für induktive Abstammungs- und Vererbungslehre* (1928) 41, Supplementband, pp. 78–80.

107 Hans Nachtsheim, *Verhandlungen des V. Internationalen Kongresses für Vererbungswissenschaft Berlin 1927 Band*, Leipzig: Verlag von Gebrüder Borntraeger, 1928; Nachtsheim, op. cit. (106).

108 Stubbe, op. cit. (102), p. 4: 'es waren groβe Erforlge, die ihm in jener Zeit beschieden waren. Sein Name war der Ganzen Welt bekannt und seine Stimme wurde überall gehört'.

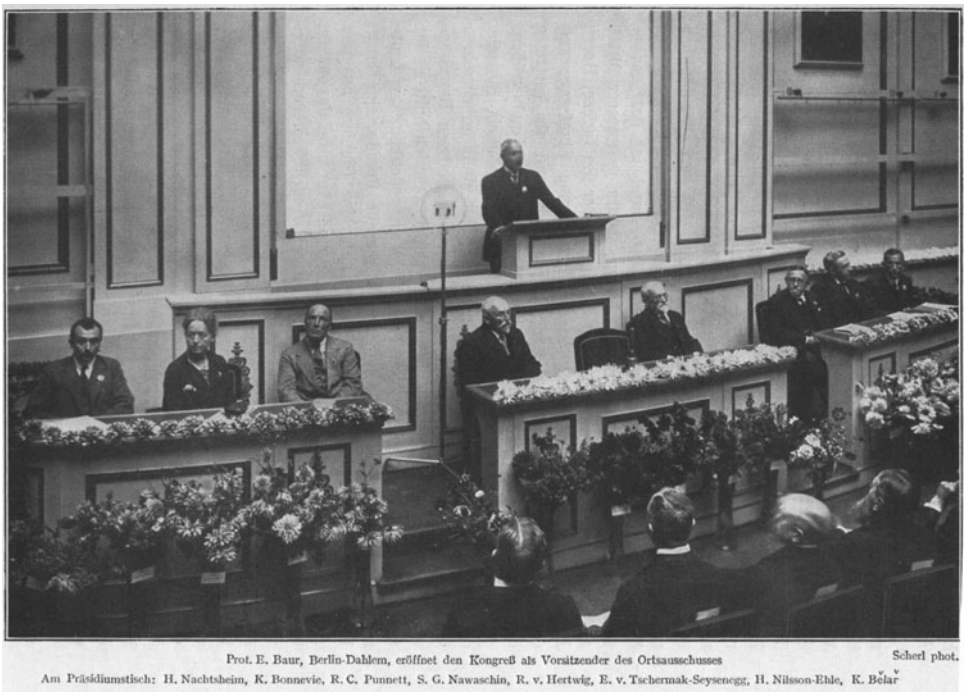

Figure 5. The conferral of the honorary presidency at the opening ceremony of the Berlin International Congress of Genetics, 1927. Erwin Baur is giving the opening speech. At the left is Hans Nachtsheim, next to him Kristine Bonnevie. Nachtsheim, op. cit., p. 51.

Baur was in the meantime passionately trying, together with leading seed companies, to establish a Kaiser Wilhelm institute for plant breeding research. In 1927 the decision to found such an institute was taken. Schiemann could not understand why Baur wanted this, because she found the institute in Dahlem entirely satisfactory.[109] Still, she was happy to become the head of a department dedicated to the history and emergence of cultivated plants. But ultimately Baur appointed not her but Klaus von Rosenstiel, who had close ties to the ascendant Nazi regime and later became his son-in-law.[110] This was a disaster for Schiemann, professionally as well as personally.[111] Discussions of this event do not explain satisfactorily why Baur and Schiemann cooperated smoothly for such a long time, and why this came to an end when Baur moved to the new institute.[112] Schiemann's own analysis, that Baur used people in his undertakings as long as they were useful to him and dropped them when they no longer fitted in with his

109 Stubbe, op. cit. (102), p. 5; Kuckuck, op. cit. (44), p. 21.

110 Kuckuck, op. cit. (44), p. 522.

111 This disappointment was reflected in the correspondence with her colleague and friend, the Dutch professor of genetics Tine Tammes. Stamhuis and Richmond, op. cit. (12).

112 In the discussion of this event Schiemann's biographers stress the different personalities of Baur and Schiemann: Kuckuck, op. cit. (44), pp. 522–523; Lang, op. cit. (44), pp. 21–23; The historian Scheich,

plans, seems a plausible explanation. Maybe Baur did not like the fact that she had developed into an independent researcher with her own well-respected line of research and was therefore happy to get rid of her.

Schiemann eventually discussed this in an extensive obituary. She added that at the beginning of his career Baur was on several occasions accused of interfering in other people's fields, which accusation she thought was not entirely unjustified. Von Ubisch referred to this when she related that in 1914 Correns feared that Baur would lay claim to her work. In addition, Schiemann stated that Baur 'quickly gained people's sympathies, not only in public life, but also in more intimate personal contact'. Additionally, 'Whoever was allowed to participate in this work, would sooner or later feel that he became part of it'. Baur was so deeply immersed in his own pursuits that he was not aware of the effect they might have on others; he included people in his own lifework, but when he no longer needed them he dropped them as if they were 'chess pieces'.[113] Schiemann concluded that Baur's behaviour inevitably resulted in conflicts. So

> the years of the most beautiful and successful joint work at the newly created Institute for Genetics Research at the Agricultural College, which Baur could organize as he saw fit, were occupied by serious attendant struggles beneath the surface, which were often full of bitterness and which on both sides consumed energy that could have been deployed much better.[114]

Nachtsheim's account revealed similar aspects of Baur's personality. And in 1959 his later pupil and assistant, Hans Stubbe, in his commemorative speech, would describe Baur's personality in comparable, albeit more guarded, terms.[115] He related that Baur, because of his fighting spirit, was nicknamed the man with the *Löwenmaul* (lion's mouth), after his favorite model plant, as *Löwenmaul* is the German word for snapdragon.[116]

After it was decided that Baur would become the director of the new Kaiser Wilhelm Institute of Plant Breeding there was a period of transition, during which, in 1928, both Schiemann and Nachtsheim were appointed senior assistants.[117] Schiemann became, in addition, an extraordinary professor at the Agricultural University in 1931, which was a customary promotion. Baur first tried to combine both directorships, but the

'Science, politics, and morality', op. cit. (44), p. 260, stresses that one has to take into account that Baur belonged to the upcoming technocratic class while Schiemann was a member of the traditional elite.

113 Schiemann, op. cit. (35), pp. 63, 77, 78, 100, 101: 'gewann er sich schnell die Sympathien, nicht nur in gesellschaftlichem Verkehr, sondern auch im näheren persönlichen Umgang'; 'Wer an dieser Arbeit mit hat teilnehmen dürfen, früher oder später in sie hineingestellt, hat sich selbst als ein Stück derselben gefühlt'; 'Schachsteine'.

114 Schiemann, op. cit. (35), pp. 77, 78: 'dass Jahre schönsten und erfolgreichsten *gemeinsamen* Arbeitens an dem neu geschaffenen Institut für Vererbungsforschung an der Landwirtschaftlichen Hochschule, dem Baur ganz seinen Stempel aufzudrücken vermochte, erfüllt sind und ausklingen mit nebenhergehenden und unter der Oberfläche verlaufenden schweren Kämpfen, die oft voll Bitterkeit waren und auf *beiden Seiten* Kräfte verbraucht haben, die besserer Verwendung wert waren'.

115 Stubbe, op. cit. (102), pp. 3, 6; Plarre, op. cit. (2), p. 156.

116 Stubbe, op. cit. (102), p. 6.

117 Schiemann's salary was then RM 7,125 per year. GSPK, I HA. Rep 87 Ministerium für Landwirtschaft, Domänen und Forsten, B Nr. 20105, Abt. 1, Akten betreffend: Landw. Hochschule in Berlin, Assistenten 1924–1931, letter from Rektor LWH to Minister. 29-05-1928, pp. 160, 224.

Agricultural College found this unacceptable. Therefore someone else had to be found. Neither Schiemann nor Nachtsheim were serious candidates. The fact that Nachtsheim was a zoologist probably prevented him from becoming the new director. The reason why Schiemann was not considered as a potential successor was seemingly her gender, as she clearly possessed the desired profile: she had run the institute in Baur's absence more than once and had developed into a recognized geneticist.[118]

The Ministry of Agriculture started negotiations with Roemer, who was the head of the institute of plant breeding at the University of Halle, but, as already mentioned, did not like the dominance of women on the staff. He declined the position.[119] The appointment of a young and inexperienced colleague was considered problematic because there were three professors at the institute: Nachtsheim and Hertwig were already professors and an application for Schiemann was in the pipeline.[120] Nonetheless, the position was offered to the relatively unknown Hans Kappert, whom we came across earlier as a staff member of the Kaiser Wilhelm Institute for Biology in 1914.

Beyond the Berlin Institute for Heredity Research: its members' careers

In 1931 Schiemann resigned without having another position to go to. The appointment of a colleague whose scientific standing was far below her own was probably the last straw.[121] She then spent a few months at the John Innes Horticultural Institute near London, which had been led by William Bateson until his death in 1926. It was certainly no accident that she went here. This, and her previous base, can be characterized as 'sister' institutions, because they had several similar characteristics. Indeed, Baur had taken the John Innes Institute as an example and inspiration for his institute, for 'external organization as well as for the internal structure'.[122]

The John Innes Horticultural Institute was founded in 1910 as an institute for the advancement of horticulture and was financed by private means.[123] Expectations of

118 In 1928, in a letter from Baur to the Rektor in which Baur announced that he intended to travel to Russia, he said, 'In den täglichen Arbeiten in meinem Institut sowie im Colloquium wird mich Frl. Privatdozentin Dr. Schiemann ohne weiteres vertreten können.' GSPK, I HA. Rep 87 Ministerium für Landwirtschaft, Domänen und Fürsten, B Nr. 20283, Band 3, Akten betreffend: Landw. Hochschule in Berlin. Institut für Vererbungsforschung, 1927 bis 1934, p. 183, letter of Baur to the Rektor 12-12-1928.

119 GSPK, I. HA. Rep. 87. B. Nr. 20095, Min. LDF, Akten betreffend Landw. Hochschule in Berlin. Professoren (Dozente) Band 7. 1929–1930; Harwood, op. cit. (10), pp. 161–162. GSPK, I. HA. Rep. 87, B. Nr. 20095, Min. LDF, Akten betreffend Landw. Hochschule in Berlin, Professoren (Dozente) Band 7. 1929–1930, letter 05-12-1929. Roemer's skewed attitude towards female staff members is affirmed by Gerta von Ubisch, op. cit. (27), p. 500.

120 GSPK, I HA. Rep 87 Ministerium für Landwirtschaft, Domänen und Fürsten. B Nr. 20283, Band 3, Akten betreffend: Landw. Hochschule in Berlin, Institut für Vererbungsforschung, 1927 bis 1934, p. 321, internal notes, 21-10-30 and 29-10-1930.

121 Marsha L. Richmond, 'Timeline: opportunities for women in early genetics', *Nature Reviews/Genetics* (2007) 8, pp. 897–902; Richmond, 'Women in early genetics: comparisons of private research institutes. Cold Spring Harbor (US) and John Innes Institute (UK)', power point presentation, 2009.

122 Schiemann, op. cit. (35), p. 72: 'für die äußere Gestaltung, wie für den inneren Aufbau'.

123 Richmond, op. cit. (12), pp. 138–142.

the usefulness of genetics for the advancement of horticulture and agriculture were high at the time. Recall that the Berlin institute (formed a couple of years later) was part of an agricultural college. One similarity was that although the Berlin institute was not a private institute like the John Innes, a great deal of the research that was carried out there was also financed by private means. It is therefore not surprising that the research programmes at both institutes were mixed in the sense that they were partly driven by genetics questions per se and partly by the possible applicability of genetics in horticulture and agriculture. In both institutions a research programme of genetics in the Mendelian framework was carried out.[124] The organisms that were studied in London overlapped with those studied in Berlin. These were mainly horticultural plants. *Antirrhinum* was studied in both locations, for example. Researchers at both locations also investigated animals such as mice and bees. Both institutions viewed their studies as fundamental for giving the new field of genetics a solid foundation and making it a respected science, beyond the practical desiderata of agriculture.

Beyond the joint interests outlined above, the most obvious similarity between the German and British genetics institutions was the high number of low-paid staff members, mainly women. In both cases the women originated from the middle or upper middle class and had intellectual backgrounds. They often had private means and were single. Both directors were keen to employ these women so that a great deal of research could be carried out by well-educated investigators, but at low cost. Another similarity was that in both cases a good working atmosphere contributed to the success of the institute. In the German case Schiemann referred to the pleasant work environment on several occasions and this was also reported of the English institute. That the directors, who were both dominant and prominent, occasionally organized meetings of students and staff at their properties, will have contributed to the good atmosphere. In both cases guests were cordially received by the staff. There was a gendered element to the work of hospitality; Caroline Pellew at the John Innes Institute and Elisabeth Schiemann at the Berlin institute were noted for their attentiveness to guests.[125]

There were, of course, also differences, with respect to both content and context. Bateson was never convinced that genes could be localized on the chromosomes, so the Morgan school's interest in determining the exact location of genes on a chromosome never flourished at the Innes, whereas this topic was taken up at the Baur institute. More generally, it is said that part of the research programme at the Innes in the 1920s lost touch with mainstream genetics, which did not happen at the Berlin institute.[126] Another difference was that Baur had to operate in the organizational context of a larger educational institute, whereas Bateson's institute was independent.

A relevant difference for the purposes of this article was the attitude of the director towards the emancipation of women. From what we know about William Bateson, we may conclude that his attitude was positive. He not only belonged to a family that for generations had had liberal attitudes towards the position and abilities of women,

124 Richmond, op. cit. (12), p. 139.
125 On Caroline Pellew see Richmond, op. cit. (12), p. 142.
126 Huxley in 1902, quoted in Richmond, op. cit. (12), p. 141; Olby, op. cit. (28), pp. 497–510, 507–508.

but he also actively promoted their emancipation. Although the passionate discussion on the admission of women to titles of degrees at Cambridge University had, in 1897, resulted in defeat, Bateson was an outspoken supporter and had served as secretary of the committee that had pleaded for it. His sister Anna was a botany graduate of the Cambridge Newnham College for women. She published papers together with him, as well as on her own. He also cooperated with several other women, of whom Edith Saunders was the most important. Although his wife Beatrice Durham never co-authored with him, she was deeply involved in his genetics experiments. Bateson socialized with his male and female co-workers on a relatively equal basis. He extensively referred to the attainments of his female staff in his work on heredity and when giving official speeches among his colleagues.[127] On the contrary, at least as far as we know, his German colleague Baur never expressed himself about the gender question or on the scientific abilities of women, neither pro nor contra, although he knew by experience that women were able to attain the required scientific standards. What we noticed about his attitude is that he viewed the female assistants as instruments, just as he did his male staff members. He never openly praised their abilities. He included them in his enterprise as long as they could contribute to his aims and dismissed them once they were no longer useful. Despite his willingness to make female appointments, there is no indication that Baur was a supporter of the women's emancipation movement.

We must take into account that when Schiemann went to the John Innes in 1931, Bateson was no longer there; in 1926 the founding director had died suddenly. But the women staff members remained. Only in 1939 would several of them be forced to retire.[128] The John Innes Institute would have been like a safe haven for Schiemann in 1931. She would have known most of the female researchers; six of them had attended the Berlin conference in 1926.[129] In this institution, with its tradition of openness to women, she was amidst researchers with the same scientific interests. She could feel at ease professionally as well as socially. This gave Schiemann time to recover from her disappointment of being bypassed by Baur, and to reflect on how to move on with her professional life.

After Schiemann's return to Berlin, she became an unpaid guest researcher at the Botanical Museum.[130] She was lucky that her specialization, the history of cultivated plants, could be carried out with a minimum of means. In 1939, probably because she distanced herself from the Nazi regime, the University of Berlin turned down the request to turn her position from an extraordinary into a special full professorship, and in 1940 she also lost the right to teach.[131] From 1940 to 1943 she was a paid

127 Richmond, 'The "domestication" of heredity', op. cit. (11), esp. pp. 579–580; Cock and Forsdyke, op. cit. (28), pp. 602–610; W. Wilks (ed.), *Report of the Third International Conference (1906) on Genetics, London*, London: printed for the Horticultural Society, 1907, pp. 70, 74.

128 Richmond, op. cit. (12), p. 140.

129 Stamhuis and Richmond, op. cit. (12), pp. 20–26.

130 Hermann Kuckuck, 'Elisabeth Schiemann zum 80. Geburtstag am 15. August 1961', *Der Züchter* (1961) 31, pp. 117–118, 117.

131 This can be found in all the biographies. See especially Sven Kinas, 'Elisabeth Schiemann und die "Säuberung" der Berliner Universität 1933 bis 1945', in Nürnberg, Höxtermann and Voigt, op. cit. (12), pp. 342–369.

academic guest at the Kaiser Wilhelm Institute for Biology. Shortly thereafter, in 1943, an Institut für Pflanzengenetik und Kulturpflanzenforschung (Institute for Plant Genetics and Crop Plant Research) was established in Vienna, mercifully free of the wartime constraints dominating research in Berlin. Schiemann was very happy when she heard that she was appointed head of its division for the history of cultivated plants, which she was able to set up in Berlin. The choice of an independent research line had paid off. After the war this department developed into an independent small institute in the then eastern part of Berlin. Here, Schiemann trained a number of young, often female, co-workers. In 1945 she was appointed ordinary professor at the Humboldt University (formerly the Friedrich Wilhelm University).[132] She remained active until her death in 1972.

After Schiemann's departure from the institute in Berlin, Emmy Stein received her position and salary.[133] In 1940 Stein also left the institute, because bombs had destroyed her equipment. She became a staff member of the Kaiser Wilhelm Institute for Biology in Berlin-Dahlem. In 1948 her last important publication appeared and she died in 1954. Nachtsheim remained at the institute until 1941.[134] Then he became the head of the department of experimental hereditary pathology of the Kaiser Wilhelm Institute for Anthropology, Human Genetics and Eugenics. He subsequently became involved in eugenics experiments on animals.[135] In 1946 he became a full professor of genetics at the Humboldt University. In 1949 he moved to the newly established Free University in Berlin, where he became the head of the newly founded institute of genetics, a position he kept until 1960.[136] He died in 1979.

It is interesting to compare the careers and salaries of the two official assistants Schiemann and Nachtsheim. It is well known, and we detected this already in the cases of the careers of von Graevenitz and von Ubisch, that women have to wait longer to be promoted than their male counterparts, that they earn less in similar positions, and that they need to be more qualified to obtain and keep them. Do these differences also apply to Schiemann and Nachtsheim? Both became senior assistants in 1928, whereas Schiemann had already been an assistant for fourteen years and Nachtsheim had been one for only seven years. Moreover, Nachtsheim became an extraordinary professor in 1925, four years after his appointment, and Schiemann in 1931, seventeen years after her appointment. There were also differences in their salaries; Nachtsheim earned at least twice as much as Schiemann. After the war both became full professors at the Humboldt University. However, Schiemann became director of a small, relatively cheap, 'niche' institute and Nachtsheim ended as a professor of genetics and head of an institute for genetics at the then prestigious Free University in Berlin.[137] There were differences between both careers on all fronts, and always favouring Nachtsheim.

132 When she officially retired in 1956, the institute closed down. Lang, op. cit. (44), p. 26.

133 GSPK, I HA. Rep 87, Ministerium für Landwirtschaft, Domänen und Fürsten, B Nr. 20283, Band 3, Akten betreffend: Landw. Hochschule in Berlin, Institut für Vererbungsforschung, 1927 bis 1934, pp. 340, 350, 351.

134 Deichmann, op. cit. (74); von Schwerin, op. cit. (74).

135 Deichmann, op. cit. (74), pp. 232–234.

136 Deichmann, op. cit. (74), pp. 240, 245.

137 Lang, op. cit. (44), pp. 27–28.

Hertwig remained in the institute until 1946. In 1939 it seemed as if her right to teach might be withdrawn because in 1933 she had been a candidate for the Staatspartei, a democratic political party. She mobilized key figures at the university and the ministry and it was concluded that not allowing her to lecture would adversely affect university education. She was then allowed to continue teaching, but without a salary.[138] She became a regular assistant in 1939 and in 1941 she was appointed a senior assistant. After the war, in 1946, she became a full professor in Halle in the former East Germany, thanks to her brother.[139] She took her mutant mice strains with her. She was not only very successful as the only female professor at the Medical Faculty, but also, from 1948 to 1950, its dean.[140] She retired in 1960 and died in 1983.

An aspect of this history not yet discussed is that despite the low salaries (if any) on offer, Baur not only managed to attract qualified staff, but also kept them at the institute for a long time: Schiemann seventeen years, Nachtsheim twenty years, Stein twenty-one years and Hertwig even twenty-five years. How can this be understood? As discussed, the field of genetics was new and, as Baur pointed out to Nachtsheim, irrespective of gender there were not many positions in this field. Another reason not to leave was that it was in general interesting and pleasant to work at the institute. The staff members could establish and develop their own research programmes, and use non-scientific staff for the fieldwork that had to be done. They could attend national and international conferences and develop national and international contacts. Moreover, Baur had an enthusiastic, engaging and charismatic personality and the spirit at his institute was in general cooperative and relations were friendly. Staff could support each other professionally and socialize with each other. Friendships were established, at least among the female staff. What also played a role for the women was that Germany was at that time rather hostile towards women workers. There were very few opportunities for women to find suitable positions elsewhere; the women did not leave because they had nowhere else to go.[141] Of the women it was only Hertwig who left for a better position, and she needed her brother to make that possible. Besides, by then it was already 1946, by which time genetics had developed into a respected discipline. However, it was apparently not easy for Nachtsheim either, the only male, to find a job elsewhere; he had to wait for the Nazi period, with its interest in human genetics and eugenics, to find another position. When he did leave, it turned out to be the first of several successive improvements of his position.

Conclusions

In this paper we discussed the Institute for Heredity Research of the Berlin Agricultural College, which, with its Mendelian and Morganian research programme, played a pivotal role in the establishment of genetics in Germany. For a long time it was the

138 Gerstengarbe, op. cit. (101), p. 302.
139 A biological institute was established for her. Gerstengarbe, op. cit. (101), p. 304.
140 Gerstengarbe, op. cit. (101), p. 307.
141 Vogt, *Vom Hintereingang zum Hauptportal?*, op. cit. (14).

only institute in Germany exclusively dedicated to genetics. During the first decades of the twentieth century its director, Baur, worked actively on various fronts to make the new genetics into an established discipline. To achieve this he needed many well-qualified people. The Berlin Agricultural College was very reluctant to provide him with money. To be able to attract sufficient personnel in the face of these difficulties, he had to apply several strategies. He managed to attract money from external organizations, often from the agricultural world, but also from the Prussian government. One reason why he was successful was that he – and others in other countries – argued that the new discipline of genetics was expected to help to increase the yields of agriculture, to make nicer and better gardens, and to improve the quality of the human race. In many countries, private companies as well as governments were willing to spend money on the development of such a promising discipline.[142]

Baur welcomed every available person he could employ. He was most successful in his ability to attract well-qualified people for minimal remuneration. These were almost exclusively women, the beneficiaries of the recent admission of women to universities in Germany, but for whom there were few other attractive opportunities in scientific employment. The only male assistant was also paid a relatively low salary, but the female assistants had even lower wages, and were sometimes not paid at all. With respect to personnel, Baur was extremely thrifty in the use of the available means.[143]

After Baur's and later Schiemann's departures, the Institute for Heredity Research continued to exist, even when the Agricultural College merged with the University of Berlin, later the Humboldt University, in 1934. However, over the years, the institute lost its position as a prominent research institute for genetics. Baur's new Kaiser Wilhelm Institute for Plant Breeding, as well as the Kaiser Wilhelm Institutes for Biology, Brain Research, Anthropology, Human Genetics and Eugenics, became important sites in the landscape of research in genetics. Genetics had by then become an important and promising discipline. The possible applications of genetics in eugenics made genetics an important field for which many resources became available – all the more so during the Nazi period.

We have argued that the lack of attention paid to women in early genetic historiography can be explained by the fact that the situation of women between 1900 and 1930 made it almost impossible for them to do more than the so-called 'technoscience'. Women were inclined to do 'normal' technoscience and not to be innovative. In addition, it is well known that in general minorities have the tendency to remain as inconspicuous as possible and that this behaviour can also be observed in the case of women pioneers in the sciences.[144] The result was that their work did contribute to the strengthening of the discipline, but was not very eye-catching. This recalls what the historian Margaret

142 On agriculture see Kimmelman, op. cit. (27); Rheinberger and Müller-Wille, op. cit. (10); pp. 173–174; on horticulture see Olby, 'Horticulture', op. cit. (27); on eugenics see Kevles, op. cit. (27); Rheinberger and Müller-Wille, op. cit. (10), esp. Chapter 5 and pp. 175–181.

143 Schmitt, op. cit. (31).

144 R. Moss Kanter, 'Some effects of proportions on group life: skewed sex ratios and responses to token women', *American Journal of Sociology* (1977) 82, pp. 965–990.

Rossiter has called the 'Matilda effect in science': the fact that, in general, women's contributions are more often undervalued than those of men.[145]

That Elisabeth Schiemann started her own research programme – the history of cultivated plants – can therefore be considered quite extraordinary and courageous. It was only possible because, in the course of time, she had attained enough scientific prestige in the genetics community. Nevertheless she was bypassed by Baur for the position of department head in his new institute in 1928. Only much later, in 1943, having obtained more scientific rewards, did Schiemann become head of an institute entirely dedicated to her subject.

This review of the emergence of the discipline of genetics in Germany makes it clear that genetics developed so quickly and successfully in the period from 1910 to 1930 in large part because of the women: qualified, available and willing to work while receiving almost no remuneration for their efforts. If they had not been available under these conditions, it would not have been possible to execute the necessary technoscience in such a brief period of time. Without them the discipline would have developed much more slowly and, because of different circumstances, along a divergent path. The parallels with the sister institution, John Innes, provide further evidence for this claim. The important role of women in the development of a new discipline is mirrored in, for instance, the field of radioactivity. Further research with a wider focus may show that in the period from around 1890 to 1930 women contributed much more to scientific research than has been recognized previously. It may be that this was not restricted to new disciplines, but was also the case in more traditional areas. Such research may also support the thesis, which we could only touch upon in this paper, that the various networks of cooperation and support built by women scientists at the start of the twentieth century were cornerstones of these attainments.[146]

145 Margaret Rossiter, 'The Matilda effect in science', *Social Studies of Science* (1993) 23, pp. 325–341.

146 Rossiter, *Women Scientists in America*, op. cit. (13), Chapter 11; Christine von Oertzen, *Science, Gender, and Internationalism: Women's Academic Networks, 1917–1955*, London and New York: Palgrave Macmillan, 2012.