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Satzinger, Helga

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FREIE UNIVERSITÄT BERLIN

HUMBOLDT-UNIVERSITÄT ZU BERLIN

WOMEN'S PLACES IN THE NEW LABORATORIES OF GENETIC RESEARCH IN EARLY 20TH CENTURY: GENDER, WORK, AND THE DYNAMICS OF SCIENCE

Helga Satzinger

Abstract

In genetic research of the first decades of the 20th century women's work became a substantial resource. Women worked at different positions in scientific institutions; as independent scientists, wives of leading scientist, and as technical or other assistants. Male and female scientists had different opportunities to draw on the workforce of others; mostly women were doing the routine experimental work in the laboratories. This difference was crucial for the scientists' choice of experimental systems. To interpret the contributions of men and women scientists to the development of genetics these gendered differences in their working conditions have to be taken into account. Some examples of such interpretation are given in the article.

The case of the US group around Thomas Hunt Morgan illustrates that the existing historiographic literature is blind to the importance of gender for the functioning and success of the group. For the case of the Berlin group of Nikolaj Timoféeff-Ressovsky a combination of several types of primary sources is used to explain the group's power to deal with the "big questions" of genetics, mutation research, and evolutionary theory in the 1920s and 1930is based on the work force of women assistants. The comparison of the group's work with that of the contemporary geneticist Paula Hertwig in the same field of mutation

research indicates an important difference at the level of experimental systems, which was due to the different ranks of male and female scientists. A comparison of Paula Hertwig with other women geneticists of her time in Berlin illustrates the common situation of these women-pioneers, as they did not gain access to secure institutional positions and experimental systems at a large scale when genetics became a respective field in science. It is suggested that further comparison of women's and men's experimental systems and scientific careers may lead to interesting new insight into the dynamics of genetic science.

Introduction Gender as Organiser of Hierarchies in Scientific Work

Women made considerable contributions to the development of biology in the 20th century. During the last two decades this has come to our knowledge thanks to the meticulous work of women historians of science, including a marginal number of men.[1] The numerous reports

1) See for example: Pnina Abir-Am, Women in modern scientific research: A historical overview, in: Sandra Harding – E. McGregor (Eds.), The gender dimension of science and technology (world science report) Paris, 1996, p. 348-356; Pnina Abir-Am – Dorinda Outram (Eds.), Uneasy Careers and Intimate Lives. Women in Science 1789-1979, New Brunswick – London: Rutgers Univ. Press 1987; Stephen G. Brush, Nettie M. Stevens and the Discovery of Sex Determination by Chromosomes, ISIS 69, 1978, p. 163-172; Nathaniel C. Comfort, The Tangled Field. Barbara McClintock's Search for the Patterns of Genetic Control. Cambridge MA: Harvard Univ. Press 2001; Ute Deichmann, Frauen in der Genetik. Forschung und Karriere bis 1950, in: Renate Tobies (Ed..), „Aller Männerkultur zum Trotz". Frauen in Mathematik und Naturwissenschaften, Frankfurt/M.: Campus 1997, p. 221-251. Donna Haraway, Primate Visions. Gender, Race and Nature in the World of Modern Science, New York: Routledge 1989; Ilse Jahn, Die Ehefrau in der Biographie des Gelehrten, in: Christoph Meinel – Monika Renneberg (Eds.), Geschlechtererhältnisse in Naturwissenschaft, Medizin und Technik; Bassum, Stuttgart: GNT 1996, p. 110-116; Louise S. Grinstein – Carol A. Biermann – Rose K. Rose (Eds.), Women in Biological Science. A Biobibliographic Sourcebook. Westport – London: Greenwood Press 1997, p. 339-344; Evelyn Fox Keller, A Feeling for the Organism. The Life and Work of Barbara McClintock, New York: Freeman 1983; Evelyn Fox Keller, Drosophila embryos as transitional objects: The Work of Donald Poulson and Christiane Nüsslein-Volhard, History, Sociology and Philosophy of Science 26, 1996, p. 313-346; Brenda Maddox, Rosalind Franklin. The Dark Lady of DNA, New York: Harper & Collins Publishers, 2002. Marsha Richmond, Women in the Early History of Genetics. William Bateson and the Newnham College Mendelians, 1900-1910, ISIS 92, 2001, p. 55-90; Margaret Rossiter, Women scientists in America. Before affirmative action, 1940-1972, Baltimore: Johns Hopkins University Press, 1995; Helga Satzinger, Die Geschichte der genetisch orientierten Hirnforschung von Cécile und Oskar Vogt (1875-1962, 1870-1959) in der Zeit von 1895 bis ca. 1927, Stuttgart: Deutscher Apotheker Verlag 1998; Helga Satzinger, Weiblichkeit und Wissenschaft. Das Beispiel der Hirnforscherin Cécile Vogt (1875-1962), in: Johanna Bleker (Ed.), Der Eintritt der Frauen in die Gelehrtenrepublik. Zur Geschlechterfrage im akademischen Selbstverständnis und in der wissenschaftlichen Praxis am Anfang des 20. Jahrhunderts, Husum: Matthiesen 1998, p. 75-93; Helga Satzinger – Annette Vogt, Elena Aleksandrovna und Nikolaj Vladimirovic Timoféeff-Ressovsky (1898-1973, 1900-1981), in:

of women working in genetics show a specific pattern, which allows further analysis of women's contributions to the development of this discipline. In this paper I would like to shift the focus from single women scientists' biographies to the complex structure of the women's work in biological research, including their experimental systems, their informal work, and the work of technical assistants as well. Doing so, gender as a social organiser of hierarchies comes into the picture of women's work and places in scientific institutions.

Women worked at various levels in the hierarchical division of scientific labour. Their places were defined by at least two social structures, which were highly intertwined. Women got their formal status in the relevant research institution according to their training and job position, they worked as scientists or technical or other assistants. In addition, women could get and they did get their status according to their personal or family relationships to male colleagues and superiors. To give some examples: Women in genetics worked as unmarried scientists and on their own formal job position, with more or less their own salary, like Nettie M. Stevens (1861-1912), Tine Tammes (1871-1947), Elisabeth Schiemann (1881-1972), Gerta von Ubisch (1882-1965), or Barbara McClintock (1902-1992). Women scientists worked as wives of leading scientists, sometimes with their own positions, but most of the time as informal co-workers, like Marcella Boveri (1864-1950), Lilian Morgan (1870-1952), and Elena Timoféeff-Ressovsky (1898-1973). They worked as daughters or nieces of important men, and they

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Ilse Jahn – Michael Schmitt (Eds.), Darwin & Co. Eine Geschichte der Biologie in Portraits, Bd. 2, München: C.H.Beck 2001, p. 442-470, p. 553-560; Anne Sayre, Rosalind Franklin and DNA, New York: Norton&Co 1975; Elvira Scheich, Science. Politics, and Morality. The Relationship of Lise Meitner and Elisabeth Schiemann, OSIRIS 12, 1997 p. 143-168; Elvira Scheich, Elisabeth Schiemann (1881-1972) Patriotin im Zwiespalt, in: Susanne Heim (Ed.), Autarkie und Ostexpansion. Pflanzenzucht und Agrarforschung im Nationalsozialismus. Göttingen: Wallstein 2002, p. 250-279; Peter Schneck, »... ich bin ja nur eine Frau, aber Ehrgefühl habe ich auch«. Zum Schicksal der Berliner Zellforscherin Rhoda Erdmann (1870-1935) unter dem Nationalsozialismus, in: Karl-Friedrich Wessel et al. (Ed.), Ein Leben für die Biologie(geschichte). Festschrift zum 75. Geburtstag von Ilse Jahn, Bielefeld: Kleine 1997, pp. 170-189. Ida Stamhuis, A Female Contribution to Early Genetics: Tine Tammes and Mendel's Laws for Continuous Characters, Journal for the History of Biology, 28, 1995, p. 495-531; Londa Schiebinger, Has Feminism Changed Science? Cambridge MA: Harvard Univ. Press 1999; Annette Vogt, Wissenschaftlerinnen in Kaiser-Wilhelm-Instituten, Berlin: AGMPG 1999/2000; Marianne van den Wijngaard, Feminsism and the Biological Construction of Female and Male Behaviour, Journal of the History of Biology 27, 1994, p. 61-90; Margaret R. Wright, Marcella O'Grady Boveri (1863-1950). Her Three Careers in Biology, ISIS 88, 1987, p. 627-652.

worked as sisters like Paula Hertwig (1889-1983) and Marguerite Vogt (born 1913). And last but not least they worked as partners of male scientists that they sometimes did not marry.[2] Women worked as technical assistants as well. This they did with full academic training, they did it with training on the job, and they did it as scholars of specific schools for female assistants in science.[3] Women worked as unskilled labourers like cleaners, glass washers, cooks in animal food preparation and the scientists' canteen. Women worked as highly specialised artists providing illustrations of scientific results, Edith Wallace from the Group of Thomas Hunt Morgan is to be mentioned here. Women worked as typists and translators, they worked as readers of other scientists' papers, they worked as multilingual secretaries contributing to their husband's or superior's knowledge or making it possible that men could act as editors of scientific journals.[4]

In the early decades of genetics, a specific type of scientific group developed. Headed by a male scientist, several scientists and their assistants co-operated in a specific project like the groups around William Bateson (1861-1926), Thomas Hunt Morgan (1866-1945), Erwin Baur (1875-1933), Alfred Kühn (1885-1968), and Nikolaj Timoféeff-Ressovsky (1900-1981), just to mention a few.[5] In addition there were the groups in the USSR around Sergej Chetverikov (1880-1959), Nikolaj Vavilov (1887-1943), and Nikolaj Kol'cov (1872-1940), however little is known about them in the West. In Germany these

2) Calvin Bridges (1889-1938) in the group of Thomas H. Morgan was well known for his various affairs with female assistants. Robert E. Kohler, Lords of the Fly. Drosophila Genetics and the Experimental Life, Chicago: Univ. Chicago Press 1994, p. 113-115.

3) The Berlin based "Lette-Verein" started training women for the new jobs as technical assistants in science and medicine around 1900, Doris Obschernitzki – Karin Weber-Andreas, Im Blick die Fotografin ... aber was noch? Frauenberufe im Lette-Verein 1866-1982. Berlin 1991. In the 1920s latest the women technical assistants formed a professional organisation, the BOTAWI, the "Bund der Organisationen Technischer Assistentinnen". Since 1921 this Bund published its own journal "Die technische Assistentin". Due to these activities the importance and the widespread use of the work of these women in science can be taken for granted.

4) For the group of Morgan see Robert E. Kohler, Lords of the Fly. Drosophila Genetics and the Experimental Life. Chicago-London: University of Chicago Press 1994. The geneticist Richard Goldschmidt at the Kaiser-Wilhelm-Institute for Biology in Berlin-Dahlem from 1914 to 1936 remembered his female assistants and multilingual secretaries in Berlin, Richard Goldschmidt, In and Out of the Ivory Tower. The Autobiography of Richard B. Goldschmidt, Seattle: Univ. of Washington Press 1960 p. 203-204, 305.

5) For the German case see Jonathan Harwood, Styles of Scientific Thought. The German Genetics Community 1900-1933, Chicago: Univ. of Chicago Press 1993.

groups could be characterised by a division of scientific labour along the lines drawn both by gender and education. Gender differentiated between female and male scientists and between scientists and female technical assistants as well. Taking gender as "a process of creating distinguishable social statuses for the assignments of rights and responsibilities"[6], we get an analytical category to understand why women and men did in science what they did. This notion of "gender" helps to understand the work of women in science without inventing some mystical entity of a "feminine" approach to science,[7] and to look very carefully into the social circumstances of their activities, its limitations and potentials.

My main point is the following: The gender-driven situation within scientific institutions created different possibilities and options for male and female scientists at a specific point of the development in their field. Due to their gender-affected position in the scientific institutions women and men had different capacities to draw on the work of others - normally women - in subordinate positions to do successful research. Due to that, men and women scientists had different options in the choice of experimental systems. As the choice and development of experimental systems is crucial for the development of a certain field or discipline,[8] women and men scientists find themselves at different positions within this field. Gender is at work in the establishment of the fragile balance of innovation and stabilisation of scientific knowledge. It is part of the power to define important and less important questions. I do not claim that gender reveals the only forces that determine the development of a specific field of science. But I do claim that looking for women's places in this field of human activity and work will reveal highly contested regions of authority, it will reveal contests on knowledge claims and types of scientific approaches made

6) Judith Lorber, cit. by Anne Fausto Sterling, Sexing the Body. Gender Politics and the Construction of Sexuality, New York: Basic Books 2000, p. 250.

7) The complexity of this problem is shown in the case of Christiane Nüsslein-Vollhard by Evelyn Fox Keller, Developmental Biology as a Feminist Cause? OSIRIS 12, 1997, p. 16-28.

8) Hans-Jörg Rheinberger, Toward a History of Epistemic Things. Synthesizing Proteins in the Test Tube, Stanford: Stanford Univ. Press 1997; see also the special issue on experimental animals in: Journal of the History of Biology 26, 1993, especially Richard M. Burian, How the Choice of Experimental Organism Matters: Epistemological Reflections on an Aspect of Biological Practice, Journal of the History of Biology 26, 1993, p. 351-367.

possible and their rating in a certain time and place. History of science investigates the social, mental, and technical processes that have been necessary at a certain point of time to develop "scientific facts" in the sense of Ludwik Fleck. Therefore the organisation of scientific work, the choice of experimental systems and its dynamics, the capacity to raise funds for research, the promises of useful applications, the inclusion and exclusion of specific persons in the process of knowledge production are to be investigated. All these elements are elements where gender is at work as a powerful organiser of hierarchies.[9)]

Genetics in the 20th century is an especially interesting case as there has been no initial exclusion of women followed by a slow and hesitant inclusion due to the pressure of women scientists. In Germany, Ute Deichmann serves as an example that women have taken part at the beginning, starting the field. The percentage of women biologists in genetic research was much higher than that of male biologists.[10)] When the scientific field became prestigious, women scientists got marginalised or were made invisible providing supportive work for male scientists. Marsha Richmond showed a similar pattern for Great Britain. Here the group around William Bateson included a high percentage of women scientists in the early years when the work was of low prestige.[11)] The same can be said of the group of Thomas Hunt Morgan, as I will show later. In all the European cases women had not yet won the regular access to university education. Genetics in its beginnings was not a field of high value, so the women, themselves coming from outside the academic hierarchy could join successfully. The process of marginalisation and exclusion came later, and was followed by the pressure for the re-inclusion of women decades later. Still, Margaret Rossiter is right in her summary, that "women tend to be where the

9) I follow the approach described by Sally Gregory Kohlstedt and Helen Longino, The Women, Gender, and Science Question. What Do Research on Women in Science and Research on Gender and Science Have to Do with Each Other? OSIRIS 12, 1997, p. 3-15.

10) U. Deichmann, Frauen in der Genetik, p. 227. According to Deichmann 57,1 % women of all women biologists were active in genetics compared to 11.7 % men biologists only.

11) M. Richmond, Women in the Early History.

12) Margaret Rossiter, Which Science? Which Women? OSIRIS 12, 1997, p. 169-185. Generalising for women in all professions in 20th century Germany, see: Angelika Wetterer, Arbeitsteilung und Geschlechterkonstruktion. „gender at work" in theoretischer und historischer Perspektive. Konstanz: UVK, 2002.

money is not", and that the feminisation of a certain discipline shows its low status in the community.[12)]

The Aims of the Paper

In this paper I would like to give some examples how gender in scientific work, especially at the level of experimental systems, can be investigated. I will focus on the process of the investigation and my reasoning behind it. Doing so I cannot give you a fine polished final result, some results, however, can be given. I hope to convince you that the approach proposed might be a useful one for further research of women in genetics and the development of genetics as well. I have chosen the examples from the first three decades of the 20th century. Let me start with a rereading of a historiographic account of the Morgan group and continue with the Department for Genetics at the Kaiser-Wilhelm-Institute for Brain Research in Berlin, led by the couple Elena and Nikolaj Timoféeff-Ressovsky. Using scientific texts and graphs as an important source I will show how gender has been part of the groups' potential to do successful research. In the next step I will compare two experimental systems in the field of mutation research of the 1930s in Berlin. One system was run by the group of the Timoféeff-Ressovsky couple, the other one by the women scientists Paula Hertwig and her doctoral student. An obvious difference of the two experimental systems lies in their power to deal with the "big" questions of genetics of their time. As this difference at the level of experimental systems cannot be referred to gender alone, further analysis of the career of Paula Hertwig as one pioneering women geneticist in Berlin is necessary. To see "gender at work" the experimental system has to be contextualised into the biography of the scientist and her possibilities as a woman scientist to do research at times of discrimination against women. In the next step of analysis, Hertwig's situation is compared to the situation of contemporary women geneticists in Berlin. This biographical contextualisation helps to understand in what ways gender and the specific situation of women scientists have been relevant in the development of early genetics.

Gender and the Research Group: Thomas Hunt Morgan "and His Boys" at Columbia University (1904-1928)

Thomas Hunt Morgan began his career as an Assistant Professor at the women's college Bryn Mawr. Here he met his future wife Lilian (1870-1952), who gained her Masters degree in biology. In 1904, at Columbia University in New York, he established a powerful group of geneticists. From 1911 onwards, the group of Thomas Hunt Morgan produced the most influential and leading theory of the gene of the 20th century using the experimental animal Drosophila melanogaster. The group started the mapping of genes on chromosomes, and it provided the specifically bred stocks for the research of other groups with respective information services. Morgan got the Nobel Prize in 1939. Women's work was substantial for the group's success, nevertheless, it is underscored in current historiography.[13] Robert Kohler wrote about the "particular and famous community of experimental biologists, the Drosophila geneticists, and their no less famous co-worker, the fruit fly".[14] He did so by putting emphasis on the relationship between the male scientists and their artefact, the stocks and breeding technique, which enabled them for years to produce continuously scientific facts of genetics. Kohler did not give equivalent attention to the women, including Morgan's wife, who did work in the group. His book, however, gives enough information to see that gender was a crucial part of the enterprise.

In the beginning the work with Drosophila was not a prestigious one as it was not the best way to come to important scientific results in a short time. The animal and the people using it were both "relatively low in academic status". A lot of women and some men aiming at a career of a college teacher worked with Drosophila.[15] In 1910 things changed when continuous inbreeding produced a fly with white eyes. This new character turned out to follow Mendelian rules and to be linked with the inheritance of female sex. Based on this result further mutants were found in the experimental work, which was extended immediately to

13) R. Kohler, Lords; Garland E. Allen, Thomas Hunt Morgan: The Man and His Science, Princeton 1978; R. Kohler, Lords; Lily E. Kay, The Molecular Vision of Life. Caltech, the Rockefeller Foundation, and the Rise of the New Biology, Oxford – New York: Oxford Univ. Press 1993.

14) R. Kohler, Lords, p. 1.

15) R. Kohler, Lords, p. 36.

the autocatalytic fact producing "breeder reactor", as Kohler put it.[16] In 1912, chromosome mapping began, three young students in their early twenties started their career with their dissertations in this field: Alfred H. Sturtevant (1891-1970), Hermann J. Muller (1890-1967), and Calvin Bridges (1889-1938). In co-operation with Morgan they wrote the book "The Mechanism of Mendelian Heredity" in 1915[17], which served as the founding work of the group now seen as the "boss and the boys". Their technique of mapping the genes became the standard procedure. Their interpretation of the genes as discrete entities on the chromosomes became the dominating one at least in the US.[18] Their work did not need sophisticated knowledge of biology, but it turned out to be a very productive one in terms of genes localised: "genetic mapping and chromosomal mechanics were designed by young persons to be a young persons game".[19] "Up to 1915, women were a majority of Morgan's recruits to Drosophila work, though only one went on to an academic career. From 1914 to 1930, in contrast, only four women got their Ph.D. using drosophila, along with eighteen men..."[20] After gaining their Ph.D. the women left the group, only one of them, Mary Stark, established her own research at a New York hospital with a specific tumour mutant.[21] Helen Redfield seems to be the only one, despite Morgan's wife Lilian who stayed in the group as a scientist. She married the group member Jack Schultz. Kohler does not give an explanation for this selection process against women not married to a group member. According to Kohler, the three "Morgan boys" developed an elite style, "unusually ambitious and aggressive, and more devoted to a fast-paced, highly productive style of experimental work than was the norm".[22] The group of young men managed to create a position for themselves where they could decide which work was the most important one – their own – and which one was the supportive. They trained

16) R. Kohler, Lords, p. 43-52.

17) A.H. Sturtevant – H.J. Muller – C.B. Bridges, The mechanism of Mendelian heredity. New York: Henry Holt and Co. 1915.

18) The most important alternative concept of the gene was developed the same time by Richard Goldschmidt (1878-1958) from the German Kaiser-Wilhelm-Institute for Biology in Berlin.

19) R. Kohler, Lords, p. 64.

20) R. Kohler, Lords, p. 95.

21) R. Kohler, Lords, p. 95.

22) R. Kohler, Lords, p. 97.

the newcomers and decided what they should do. They dominated the laboratory talking all the time while counting their flies.[23] The men, "Morgan and his boys", also were of a specific brand, one being a notorious womaniser, one was well known for his rude habits and the other for keeping the best results for himself and his publications.[24] Without suggesting that this was repelling for women scientists at the time, as there are no sources, the undoubted effect has to be stated, that women did not become part of the group as scientists with their own profile to be referred to by current historiography. According to that, only men joined the game and enjoyed a co-operative spirit, which, however, turned out to be a very fragile one.[25] "The group's formative psychosocial relationships were male: master and disciple, father and son, Boss and 'boys'".[26] The public image of the group, including the photographs, was male. But still, "women did work in the fly lab", they worked in the group as unpaid technicians and stock-keepers, they were the wives of graduate students.[27] Sturtevant married a technical assistant. Kohler mentions it, but he gives little information and does not challenge the view, that it was a male group that did the scientific Drosophila work. "Edith Wallace, a trained biologist served as the group's artist in addition to being Morgan's personal technician."[28] Kohler does not acknowledge the fact that the excellent drawings of Edith Wallace were of specific importance for the group. They were the means to show how the Drosophila mutants the geneticists referred to in their abstract chromosome maps looked like. These drawings were crucial to communicate the findings of the group to other scientists.[29] Kohler does not tell the story of Morgan's wife and biologist Lilian. After having raised their four children, Lilian Morgan joined her husband's laboratory around 1920 where she established her own cytology based research agenda with Drosophila.[30] In 1921, she found a special and important aberration in the chromosomes, the so-called attached-

23) R. Kohler, Lords, p. 98

24) R. Kohler, Lords, p. 98-106, 111-117.

25) R. Kohler, Lords, pp. 110-132.

26) R. Kohler, Lords, p. 96.

27) R. Kohler, Lords, p. 96.

28) R. Kohler, Lords, p. 96.

29) See e.g. Thomas H. Morgan – Calvin B. Bridges, Sex-linked Inheritance in Drosophila, Washington: Carnegie Institution of Washington 1916, plate I and II.

X-chromosomes, where two chromosomes stick together. On this finding she based the establishment of a specific strain of Drosophila which became an important tool for further analysis for generations of geneticists to come. Her second important finding was a ring-chromosome, which also became an important starting point for further genetic work. Quite surprisingly, she never attended scientific meetings, and she never presented her work there, although the 1920s and 30s were a time, when women scientists were already established in the field. After the death of her husband she got an academic appointment for one year. During his lifetime she didn't have her own formal job position, she worked as the invisible woman behind him, using the scientific facilities attached to his formal position as a leading scientist. According to Kohler, she was the one who provided "one of the group's most productive experimental systems in the 1920s."[31] Nevertheless he continued to recreate the image of the group as a male one, which has pervaded current historiography.[32] An adequate understanding of the dynamics of the Morgan group should take its gendered division of work serious. I think it will be worth rewriting its history using the scientific papers and archival material to analyse the interdependence and relevance of the different work and experiments done by the men and women in the group.

Gender and the Research Group: the Department for Genetics at the Kaiser-Wilhelm-Institute for Brain Research in Berlin (1926-1945)

The Kaiser-Wilhelm-Institute for Brain Research was founded by the neurologists Cécile and Oskar Vogt (1875-1962, 1870-1959) who had started their common work as a married couple at the turn of the century in Berlin.[33] In 1925, they installed a Department for Genetics

30) Katherine Keenan, Lilian Vaughan Sampson Morgan (1870-1952), in: Louise S. Grinstein – Carol A. Biermann – Rose K. Rose (Eds.), Women in Biological Science. A Biobibliographic Sourcebook. Westport – London: Greenwood Press 1997, p. 339-344. All following information on L.M. is drawn from this source.

31) R. Kohler, Lords, p. 96.

32) Allen, Morgan; Kay, Molecular Vision.

33) Helga Satzinger, Die Geschichte, p. 1-99. At that time women in Berlin had no regular access to university. Cécile Vogt was French; she had studied medicine in Paris and got her doctor title there in 1900. At the Vogt's institute a considerable number of female technical assistants and women scientists worked at the end of the 1920s and early 1930s. This unusual frequency of women scientists is to be interpreted as the result of the political convictions of the Vogts concerning women in science.

under the leadership of Elena and Nikolaj Timoféeff-Ressovsky from Moscow, who stayed until 1945. The department became very influential in German genetics in the 1930s for its mutation research and its contributions to the development of the new evolutionary theory.[34] Its favourite experimental systems were based on drosophila. The Timoféeff-Ressovskys had close links to the Morgan group via Herman J. Muller, who had been in the USSR before to introduce drosophila genetics there. Around 1933, Muller stayed at the Berlin Institute. Further scientists were working at the genetics department. The unmarried biologist Esthera Tenenbaum (1904-1963) had a doctor title from Berlin University and dealt with questions Oskar Vogt assigned to her for solution. She didn't gain independent status with her work. Coming from Jewish background she was forced to emigrate after 1933. She went to Palestine and abandoned genetics. Another Russian worked at the department for genetics in subordinate position, Sergej Zarapkin (1892-1960). He and his wife and children had joined the institute in the late 1920s.

The photograph in Fig. 3 was taken at the institute's inauguration at the new site in Berlin-Buch in 1931. It shows the staff and some friends with a considerable number of women present.[35] At that time the institute had more than six departments. Each of them was led by a scientist with the status of a non-regular professor (Außerordentlicher Professor), and it had several subordinate co-workers and assistants. The persons to be seen on the picture faced nearly all the possible fates a female or male scientist could face in German science between the 1920s and 40s. The women on the picture worked at all possible levels in science. They were scientists, married and co-working, they were unmarried scientists, they were daughters and sisters, they worked as technical assistants, as unskilled labourers, as nurses in the clinic or they worked as wives at home, supporting their husband's career.

To find the hidden work of women in science, specific sources have to be used. Especially in the case of the technical assistants scriptural

34) H. Satzinger – A. Vogt, Elena; Helga Satzinger, Die blauäugige Drosophila. Ordnung, Zufall und Politik als Faktoren der Evolutionstheorie bei Cécile und Oskar Vogt und Elena und Nikolaj Timoféeff-Ressovsky am Kaiser-Wilhelm-Institut für Hirnforschung Berlin 1925-1945, in: Rainer Brömer – Uwe Hoßfeld – Nicolaas Rupke (Eds.), Evolutionstheorie von Darwin bis heute. Berlin: Verlag für Wissenschaft und Bildung 1999, p. 161-196.

35) Archiv zur Geschichte der Max-Planck-Gesellschaft, Berlin (hereafter cited as AGMPG).

material in archives tends to be silent. Pictures like Fig. 3a represent them, but to identify the group of young women working as technical assistants in the genetics department the help of one of them was needed. Natascha Kromm gave forgotten names and duties.[36] To understand the relevance of the work of these women, the scientific publications of the researchers proved to be very useful. My example of a specific source is a scientific graph and a new way of reading it. This graph – a map – belongs to the history of genetics, evolutionary theory, and brain research. (Fig. 1.)

It was published 1975 in the German version of the Russian textbook "Kurzer Grundriss der Evolutionstheorie" – "Introduction into evolutionary theory" by Nikolaj Timoféeff-Ressovsky, Nicolaj Voroncov, and A. Jablokov.[37] The authors claim that the map shows reproductive isolation between certain geographic subgroups of one species. This type of isolation is seen in modern evolutionary theory as the crucial factor among others driving the development of the species. The map shows a terrain of approximately 8 acres, the ground plans of some buildings and the distribution of three species of one important experimental animal of geneticists of the 1920s, the flies drosophila obscura (NR. 1), drosophila funebris (NR. 2), and drosophila melanogaster (NR. 3). For each species there is a different type of dot, the three species are distributed irregularly around buildings.

The map had been published nearly forty years earlier in the 1939 paper of Nikolaj Timoféeff-Ressovsky on genetics and evolution.[38] In this paper he presented for the first time his new interpretation of biological evolution using results from genetics, mutation research, and the geographical distribution of species and their subgroups called races. The paper belongs to the so-called Modern Synthesis

36) Interview with the author, Berlin, June 1992. Natascha Kromm, Berlin, was technical assistant of the Department of Genetics at the Kaiser-Wilhelm-Institute for Brain research in the 1930s until 1945.

37) Nikolaj Timoféeff-Ressovsky - N.N. Voroncov - A.V. Jablokov, Kurzer Grundriss der Evolutionstheorie, Jena: VEB Fischer 1975, p. 130. In 1945 and after the war Nikolaj Timoféeff-Ressovsky was deported to the Soviet-Union and sentenced to 10 years Gulag as he had stayed in Germany during National Socialism. In 1947 he was hired for the secret atomic research program in the USSR, his wife could join him, and she did. In the 1960s and 70s they could publish again.

38) Nikolaj Timoféeff-Ressovsky, Genetik und Evolution, Zeitschrift für induktive Abstammungs- und Vererbungslehre 76, 1939, p. 154-219, 198.

in Evolutionary Theory, which overcame Neo-Lamarckism.[39] A little footnote on the map from 1939 stated: "schematised according to H. A. Timoféeff-Ressovsky, unpublished". In the 1975 publication the footnote is missing. This footnote shows that it was his wife, Helene (i.e. Elena) Timoféeff-Ressovsky who had provided the necessary data and their first interpretation. The map not only represents drosophila populations on a specific terrain, but also an element of modern evolutionary theory. It also represents the work of the wife of the author, who herself supposedly was supported by women assistants. To read the map as a map of work one has to reformulate the technical data of the experiments into the description of the work necessary in the experiment. Each square of the map signifies one bottle to catch drosophila. There are more than 110 squares on the map. Twice a day the bottles had to be examined, that makes 220 bottles a day with probably 100 flies per bottle. The flies had to be identified and counted. Two or three days in one month of one season these examinations took place. So some thousands of animals had to be looked at.

The map in Fig. 1 represents the results of a scientific investigation. At the same time this figure represents the work of women in science, if one knows how to read it. In addition, the figure represents the site of research, the Kaiser-Wilhelm-Institute for brain research in Berlin-Buch in 1931. One simply has to turn the map around and compare it with the photograph of the institute (Fig. 2).

Elena and Nikolaj Timoféeff-Ressovsky are one of these couples in science, where the woman's work lies in the shadow of her husband's.[40] However, they co-operated scientifically all their life. They were trained the same way in Biology in the scientific school of Nikolaj and Maria Kol'cov and in the group of Chetverikov, both held no doctor title when they came to Berlin. In the beginning, both worked along the lines given by the research of Cécile and Oskar Vogt. When Herman J. Muller presented his experiments on X-rays induced mutations in Drosophila in 1927, the Timoféeff-Ressovskys immediately started mutation research on a large scale. After the forced retirement of the Vogts in 1936, Nikolaj Timoféeff-Ressovsky became director

39) H. Satzinger, blauäugige Drosophila.

40) In the USSR they continued their common scientific work, she even became more important for him as he had lost his eyesight. See: H. Satzinger - A.Vogt, Elena....; H. Satzinger, blauäugige Drosophila.

of the now independent department for genetics. Now he was called Professor, at the same time Elena Timoféeff-Ressovsky lost her status as an employed scientist, but she could continue her scientific work unpaid as the wife of the leader of the department - without any formal status and degree. Of course she was not recognised as an equal of husband - she was seen as just helping. Nikolaj Timoféeff-Ressovsky acted as the leader of the group of several male scientists and a group of women technical assistants, his wife Elena Timoféeff-Ressovsky working with indirect pay via her husband, or on some grants of the Notgemeinschaft. In addition to the group financed by the institute's resources, Nikolaj Timoféeff-Ressovsky established a very productive informal network of young male scientists coming from atomic physics. This network included Karl Zimmer (born 1911), Pascal Jordan (1902-1980), Robert Rompe (1905-1993) and Max Delbrück (1906-1981), who emigrated soon after. This group of men dealt with new theoretical and practical problems of mutation research. They used X-rays, neutrons and other atomic radiation to irradiate drosophila, and they used their knowledge of physics to develop hypotheses of the interaction between radiation and molecules. The biological side of the experimental work of the genetics department relied on specifically bred stocks and wild populations of drosophila. This part of the work included breeding, crossing, feeding, caring, counting, looking for mutant morphological features etc. and was done by the group of women assistants and Elena Timoféeff-Ressovsky, who most probably supervised this part of work.[41]

The gender-driven organisation of work enabled the group to deal experimentally with the questions of genetics and evolutionary biology of the time, which were regarded as the most important one by the predominantly male community. The group of Timoféeff-Ressovsky had the resources to perform extended series of experiments to answer the question if there was a directed, environment dependant mutation as the Neo-Lamarckians claimed, or if mutation was something unpredictable and non-adaptive. This question was still pending and important for any theory of evolution in the early 1930s. Deeply related to that problem was the balance between environmentally and

41) Her son Andrej Timoféeff (born 1927) saw her scientific work and integrative mediator function of her social activities as crucial for the success of his father and the group. Interview 2000 in Berlin, together with Annette Vogt.

genetically induced variations in certain characters. This had to be known to be able to talk about the gene – which should be independent from environment in its effects. The group was able to pursue a series of radiation and breeding experiments to come to the suggestion that the gene or "unit of mutation" – as they called it – was a molecule of a definite size within the cell. The group of the Timoféeff-Ressovskys were the first in the history of genetics to produce these data in experiments. In cooperation with the physicists Max Delbrück and Karl G. Zimmer, Nikolaj Timoféeff-Ressovsky published their famous "three men's work" in 1935.[42] The basis for these scientific results were the large scale breeding experiments and the carefully designed and cared for stocks, which involved hundreds of thousands of animals, and a reliable female workforce.[43]

Gender and the Power of Experimental Systems: Comparing Nikolai Timoféeff-Ressovsky's and Paula Hertwig's Mutation Research of the 1930s

The power of the Timoféeff-Ressovsky group can be shown by a comparison of its experimental system to that of two contemporary women scientists in Berlin: Paula Hertwig and Hildegard Brenneke. Both experimental systems used X-rays to induce a biological effect. Both show an increase in harmful effects with the increase of the dosage of radiation. But the range of questions to be answered was very different.

Fig. 4 gives data of a series of experiments with Drosophila melanogaster. They were published by Nikolaj Timoféeff Ressovsky, Karl Zimmer and Max Delbrück in their famous three men's work in 1935.[44] To start with and to show the normal rate of mutations they had to look

42) Nikolaj Timoféeff-Ressovsky – Karl G. Zimmer – Max Delbrück, über die Natur der Genmutation und der Genstruktur, Nachrichten von der Gesellschaft der Wissenschaften zu Göttingen, Mathematisch-physikalische Klasse, Fachgruppe VI, Neue Folge 1935, 1, 190-245.

43) Natasha Kromm, interview with the author, Berlin, 1992. For example see: Helena Timoféeff-Ressovsky, Temperaturmodifikabilität des Zeichnungsmusters bei verschiedenen Populationen von Epilachna chrysomelina F, Biologisches Zentralblatt, 1941, 61, 68-84. This paper mentions the measurements of approximately 24,000 dots of epilachna wings. The data were used for the differentiation between environmentally and genetically induced variations.

44) N. Timoféeff-Ressovsky et al., Natur, p. 203.

at animals of more than three thousand cultures, i.e. bottles with flies, where approximately 50 – 100 animals were bred. In this amount of animals they found four specimens only with mutant characters which could be interpreted as alterations of one gene. To show the increase in mutation with dosage, approximately five thousand cultures had been irradiated which provided ca. 350 mutants.

Fig. 5 shows data of experiments conducted by Paula Hertwig and Hildegard Brenneke, published in 1937.[45] The two women irradiated the sperm of vertebrates and looked for alterations in the offspring. They used mice and cytological features of fertilised eggs. Due to the design of their experimental system they could not show mutants as alterations of genes – which would have needed proof in further breeding experiments with carefully designed stocks. Hertwig's and Brenneke's experiments provided effects of radiation in a much smaller amount of cases, the women observed and investigated some hundred fertilised eggs of mice, and they looked for the percentage of eggs developing normally. This also was a tremendous lot of work, keeping the animals, radiating them and letting them mate, and then operating the females to get the fertilised eggs and early embryonic stages for investigation. Hertwig and her doctoral student Hildegard Brenneke showed the harmful effects of radiation at the cellular level, which they interpreted as the cause for reduced fertility found in mice after irradiation of the males. With this type of experiment it was not possible to trace the effects of radiation down to the level of genes, including an estimation of their size.

In what sense can the obvious differences of the two experimental systems in the field of mutation research be seen as related to gender? I definitely do not see gender present in the sense that the women scientists chose a female approach looking into the fate of eggs and embryos. There is no reason for this interpretation. One might say that the obvious difference lies in the experimental animal and its peculiarities: invertebrates against vertebrates. The two women scientists simply did not have access to large-scale breeding experiments with mice, which would have needed much greater experimental facilities

45) Paula Hertwig - Hildegard Brenneke, Die Ursachen der herabgesetzten Wurfgröße bei Mäusen nach Röntgenbestrahlung des Spermas, in: Zeitschrift für Induktive Abstammungs- und Vererbungslehre 1937, 72, 483-487.

and work force than drosophila genetics. 50,000 mice in the third generation of breeding experiments would have been necessary to prove, for vertebrates, an increase in mutations of 1 % due to X-rays, Paula Hertwig estimated in 1932/33.[46] The facilities for breeding mice in these dimensions might have been inaccessible for contemporary male scientists as well. So gender not necessarily was part of the difference between the two experimental systems based on drosophila on the one hand and mice on the other. But still, there are reasons to see gender at work.

Gender and the Power of a Woman Scientist: Paula Hertwig as a Pioneering Geneticist in Berlin, 1911-1939

At the time of the publication of the experiment, Paula Hertwig was not a young, unknown scientist in the field but a very experienced and renowned one; at the age of 48 she was eleven years older than Nikolaj Timoféeff-Ressovsky.[47] Paula Hertwig was the daughter of the very influential biologist Oskar Hertwig (1849-1922), who had been professor at the medical faculty at the Berlin University, and the niece of Richard Hertwig (1850-1937), renowned professor of zoology in Munich. Oskar and Richard Hertwig were famous for their basic work on fertilisation. Her brother Günther also was a biologist and geneticist. Paula Hertwig was one of the first women scientists in Berlin who got the habilitation in 1919, and she became Professor at the Medical Faculty in Berlin in the 1930s.[48] Before World War I she had begun the investigation of the effects of radiation, especially radium, on fertilised eggs and early embryos in the laboratory of her father.[49] This work aimed at the understanding of the harmful effects at the level of cellular structures, especially chromosomes, which had

46) P. Hertwig, Die gentischen Grundlagen, p. 198.

47) See Gerstengarbe for further biographical information.

48) Please help – see Gerstengarbe.

49) Paula Hertwig, Durch Radiumbestrahlung hervorgerufene Veränderungen in den Kernteilungsfiguren der Eier von Ascaris megalocephala. Archiv für mikroskopische Anatomie 77, 1911, p. 301-312; Paula Hertwig, Das Verhalten des mit Radium bestrahlten Spermachromatins im Froschei. Ein cytologischer Beweis für die parhtenogenetische Entwicklung der Radiumlarven, Archiv für mikroskopische Anatomie 81 II, 1913, p. 173-182; Paula Hertwig, Keimesschädigungen durch Radium und Röntgenstrahlen, in: Erwin Baur – Max Hartmann (Eds.), Handbuch der Vererbungswissenschaft, Vol. III, Berlin: Gebrüder Borntträger 1927.

been established as hereditary material in the first decade of the 20th century. It is not known if Paula Hertwig ever considered using this new experimental system of drosophila genetics when it became famous and efficient in the 1920s. At that time she was working at the Berlin Institute for "Vererbungswissenschaft" headed by Erwin Baur. She undertook genetic experiments with chicken, and she supervised the dissertation of Tine Rittershaus, localising certain genes on the sex determining chromosome in these animals. This work was funded by poultry breeding companies and the Prussian ministry for agriculture.[50] So the choice of the experimental animal might have been a question of how to get funding. However, by mapping genes onto chromosomes and using sex linkage, Hertwig applied to vertebrates the approach of the pioneering Morgan group, who had started this work with drosophila in 1911. The time span between the work of hers to that of Morgan can be explained as due to the First World War and the economic difficulties following it in the Germany of the 1920s. In 1930, Hertwig changed to mice as experimental animals and continued her early work experimenting with the effects of X-rays. This type of research, including the work of Hertwig and Brenneke presented above, was of considerable importance due to the political circumstances of the time – as racial hygiene was on top of the agenda. In this context it was of high value to show that the genetic effects found in drosophila were also present in higher vertebrates, and supposedly in humans. Paula Hertwig became one of the respected geneticists in the early 1930s who tried to convince obstetricians to restrain the use of X-rays, especially to stop temporary sterilisation of women by radiation because of the expected increase of negative mutations in the generations to come.[51]

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50) Paula Hertwig - Tine Rittershaus, Ueber Fehlfedern bei gesperberten Hühnern, Archiv für Geflügelkunde 1929, 3, 65-76.

51) Paula Hertwig, Die genetischen Grundlagen der Röntgenmutationen, Monatsschrift für Geburtshülfe [sic!] und Gynäkologie XCIII, 1933, p. 197-198. Whether her work was relevant for the development of forced sterilisation using X-rays is not yet known. Ute Deichmann, Biologen unter Hitler, Vertreibung, Karrieren, Forschung. Frankfurt a. M.: Campus 1992, p. 109 suggests such a connection; in her later publication (1997), Frauen in der Genetik, no connection of this type is mentioned.

52) Nikolaj Timoféeff-Ressovsky, Experimentelle Mutationsforschung in der Vererbungslehre. Beeinflussung der Erbanlagen durch Strahlung und andere Faktoren, Dresden: Steinkopf, 1937, p. 9-13.

In the year 1937 Nikolaj Timoféeff-Ressovsky appreciated Paula Hertwig's work as a pioneering research in the biological effects of radiation.[52] However, he criticised that her experiments were restricted to cytological and embryological investigations and lacked the combination with large scale breeding experiments. He claimed that her experiments in changing the substance of heredity in the cells were not linked with the new work on the mechanisms and laws of heredity. Timoféeff-Ressovsky's remarks can be understood that he did not consider Paula Hertwig as being a real expert in genetics - as he himself was. He criticised the obvious lack of breeding experiments, as if Hertwig was omitting deliberately an important experimental approach. Nevertheless, Nikolaj Timoféeff-Ressovsky conceded that Hertwig at least knew how to design the necessary experiment with mice to show an increase of mutations with dosage in higher vertebrates.[53] But in his paper he did not consider the problem whether she was able, due to her institutional position, to create the necessary facilities for the intended research. Nikolaj Timoféeff-Ressovsky's evaluation of Paula Hertwig's work clearly shows a devaluation in terms of the Matilda [sic!] effect in science.[54] His own work was real mutation research and genetics; hers was not.

Late in the 1930s Paula Hertwig succeeded to establish appropriate breeding facilities for mice. Now she could produce radiation-induced mutations in vertebrates, which was important to make substantial hypothesis for the effect of X-rays in humans.[55] Still, Paula Hertwig did not belong to the top 15 researchers in zoology during National Socialism who got most of the funding from the Deutsche Forschungsgemeinschaft. This is surprising as mutation research was on top of the agenda of the time, but it is probably not surprising, as she was a woman scientist.[56]

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53) Nikolaj Timoféeff-Ressovsky, Experimentelle Mutationsforschung, p. 13.

54) Margaret Rossiter, The Matilda Effect in Science, Social Studies of Sciences 1993, 23, p. 325-341.

55) Paula Hertwig, Zwei subletale rezessive Mutationen in der Nachkommenschaft von röntgenbestrahlten Mäusen, Der Erbarzt, Beilage zum Deutschen Ärzteblatt 1939, p. 41-47.

56) U. Deichmann, Biologen, p. 81. Hertwig's work conducted during that time still has to be investigated in necessary detail.

57) see Gerstengarbe....

However, Paula Hertwig's scientific biography, which is not known in necessary detail yet, can be seen as successful.[57] She managed to start mutation research in higher vertebrates and she became well known for that. But she did it late and on a small scale. The important time in her career, where the substantial time span and late choice of the crucial experimental system took place lies in the second half of the 1920s and first half of the 1930s. At that time Paula Hertwig was working at the Berlin Agricultural College (Institut für Vererbungsforschung) led by Erwin Baur until 1927/28. The crucial time for mutation research was a time of institutional insecurity for Hertwig, as Baur left the Institute to become director of the newly founded Kaiser-Wilhelm-Institute for (plant) Breeding Research (Züchtungsforschung) in Müncheberg. So it is this time of her career which needs further historiographic investigation to clarify in detail her institutional situation, which has determined the choice of experimental systems and its potential.

Gender in German Genetics: Contextualising Paula Hertwig's Career as Part of a Group of Pioneering Women Geneticists

Paula Hertwig was part of a group of women geneticists who started their scientific career and genetics in Germany before World War I with Erwin Baur acting as their mentor: Elisabeth Schiemann (1881-1972), Emmy Stein (1879-1954), Luise von Graevenitz (1877-1921), and Gerta von Ubisch (1882-1965). The biographies of these women show that these women who pioneered in genetic research did not get an equal access to experimental resources like the younger male colleagues, when the scientific field became prestigious in the late 1920s. Compared to the other women, but not to other men, Paula Hertwig most probably had the best conditions for her work.

In the early years Baur did not have a high status within the scientific community, he had come from medicine, psychiatry and bacteriology before he turned to botany.[58] In the German genetics community he belonged to the representatives of the "pragmatic style" and not to the powerful "mandarins" who followed the "comprehensive style".[59]

58) Gerta von Ubisch, Aus dem Leben einer Hochschuldozentin, Mädchenbildung und Frauenschaffen. Part I, 1956, 6, p. 413-422, here 420.

59) See J. Harwood, Styles.

Until 1921, the group around Baur was an all-women scientists group. Jonathan Harwood gave an explanation, which is still held valid: the women worked unpaid at Baur's institute in the beginnings, and during the war the men were missing.[60] Except for the younger Paula Hertwig all the women were in their thirties, when they started their scientific career. They had to overcome the restrictions against academic training for women first. So they took indirect ways until they came to genetics. Gerta von Ubisch began as the first one before the war. She worked with barley. She had studied physics, got her doctor title there, then she changed to botany and to Baur.[61] In 1914 she went to Carl Correns, one of the re-discoverers of Mendel's laws. He wanted her as his first assistant at the newly founded Kaiser-Wilhelm-Institute for Biology in Berlin, but the war made the start of the work in Berlin impossible, so they stayed at the University of Münster. Ubisch had very little possibilities to pursue her own research, while working for Correns. Since 1915 she worked for private plant breeders after being refused two times a comparable position, as she was a woman. Her payment was poor, and so were her living conditions, but she got the job as the men were at war. In 1918, she went back to the institute of Baur. Elisabeth Schiemann, the daughter of the Berlin Professor for European history Theodor Schiemann, did her dissertation under the supervision of Baur. She got the Dr. phil. in 1912 and a position as an assistant at the institute in 1914, which she kept until 1931.[62] She got her habilitation in 1924 in Berlin. Luise von Graevenitz and Emmy Stein joined the institute before the war.[63] Graevenitz became an assistant before 1914 and Stein after the war. The two women were educated at the "Gartenbauschule für gebildete Frauen (educated women's school for gardening) in Berlin. In 1906, they travelled for one-year via Egypt, Ceylon, Java, Japan and Russia and stayed for several months at the botanical garden Buitenzorg and the jungle station Tjibodas in Java. For this scientifically motivated journey they had won the support of

60) J. Harwood, Styles, p. 200-202.

61) Gerta von Ubisch, Aus dem Leben; Part I, 1956, 6, p. 413-422, Part II, 498-507; Part III, 1957, 7, p. 35-45.

62) Elisabeth Schiemann, Autobiographie, Nova Acta Leopoldina N.F. vol. 21, 1959, 291-292; U. Deichmann, Frauen in der Genetik, p. 232-236.

63) All biographical data: AGMPG III, 30, 1, Emmy Stein, 2 pages typoscript, autobiography Aug. 1951.

the Berlin botanists Georg Volkens (1855-1917), Simon Schwendener, (1829-1919), and Erwin Baur, at that time Schwendener's assistant. After the journey they studied in Zürich, Tübingen and Jena and got their doctor title there. Graevenitz' career ended very early due to her untimely death in 1921. Emmy Stein stayed at the institute until 1939. Gerta von Ubisch left the Baur Institute in 1921; she became an assistant at the institute for botany at the university of Heidelberg and got the habilitation in 1923 as the first woman in Baden. She was the only one at the Heidelberg University teaching genetics, but she did not get a professorship. In 1933, she lost her position as an assistant due to the anti-Semitic laws of the National Socialists. The support of her colleagues brought her back to the university where she had to face the boycott of her lectures in genetics by national socialist students. With the help of the Dutch Professor Johanna Westerdijk she could stay and work for two winter terms in Utrecht, in summer she went to Zürich. In 1935, she emigrated to the Butantant Institute in Brasil, where she was offered a job. Due to various complications and miss-fortunes she could not continue her scientific career. In 1952, she came back to Heidelberg, poor, and 70 years old.[64]

After World War One, Emmy Stein, Elisabeth Schiemann, and Paula Hertwig worked at the Baur institute. In the 1920s genetics became one of the key sciences for agriculture and medicine, eugenics and racial hygiene and Baur became quite famous as co-founder and co-editor of the "Zeitschrift für induktive Abstammungs – und Vererbungslehre" and as the author and co-author of important textbooks of genetics and racial hygiene.[65] He was one of the highly influential advocates of the application of genetics in agriculture and medicine. In 1927, he got the directorship of the most prestigious and newly established Kaiser-Wilhelm-Institute for Breeding Research. At the new institute neither Ubisch, nor Stein, nor Schiemann got a position as a researcher and leader of a department, which would have been appropriate according

64) G. v. Ubisch, Aus dem Leben; Part III, p. 39-45; U. Deichmann, Frauen in der Genetik, p. 229-232; Meike Sophia Baader, „Wissenschaft als Beruf" in den Naturwissenschaften, in: Hubert Treiber – Karol Sauerland (Eds.), Heidelberg im Schnittpunkt intellektueller Kreise: zur Topographie der „geistigen Gesellschaft" eines Weltdorfes 1850-1950, Opladen: Westdeutscher Verlag 1995, p. 445-460.

65) Erwin Baur - Eugen Fischer - Fritz Lenz, Grundriß der menschlichen Erblichkeitslehre, München: Lehmanns 1921-1931.

to their qualification. The discrimination of women was more than obvious in the case of Elisabeth Schiemann, as she had been involved in the planning of the new institute and was supposed to become leader of one department. Baur, however, preferred his future son-in-law for this position in spite of her. At the Institute für Vererbungsforschung in Berlin Baur chose a young male scientist as his successor. The scientifically much more experienced and advanced Elisabeth Schiemann saw herself forced to leave the institute. She got the opportunity to work at the Berlin botanical garden, but lost her former experimental facilities and regular salary.[66] Emmy Stein stayed longer, but like Schiemann she felt forced to leave her position at the institute under the new director "aus unerfreulichen Gründen", because of "unpleasant reasons" in 1939. She could continue her work – unpaid – at the Kaiser-Wilhelm-Institute for Biology in Berlin.[67] Her research material got destroyed during the World War II bombing of Berlin.[68]

At the Kaiser-Wilhelm-Institute für Breeding Research Baur chose young male students to do research for their dissertations in the paradigm he provided. The institute had excellent support system for the work with the plants in the gardens and on the fields, as approximately 200 non-scientific assistants worked there in the year 1934.[69] The women scientists however, who had contributed to pave the way for the new scientific field, who had contributed to re-established international co-operation after World War One by co-organising the International Congress for Genetics in Berlin in 1927, did not get access to the new working facilities after 1927. They could not base their scientific work and experiments on large support systems in terms of labour, space, and specimen involved.

For mutation research large support systems were crucial, as I have shown for the zoologist Paula Hertwig and Nikolaj Timoféff-Ressovsky. Emmy Stein was active in mutation research from the beginning of her work. She irradiated snapdragons to investigate the harmful effects of radiation at a cytological and embryological level in plants. For her, the access to large-scale experimental systems also might have been

66) U. Deichmann, Frauen in der Genetik, p. 232-236; E. Scheich, Elisabeth Schiemann, p. 253-261.

67) AGMPG, III, 30, 1, Emmy Stein, autobiography 1951, autobiography Dec. 1948.

68) Ubisch, Aus dem Leben, Part II, p. 504.

69) J. Harwood, Styles p. 202-226.

crucial. Similar to the case of Paula Hertwig a comparison of Stein's work with that of a younger male colleague is possible. It was Hans Stubbe (1902-1989), friend of Nikolaj Timoféeff-Ressovsky, who became famous for his mutation research in plants. Supervised by Baur he started his dissertation doing mutation research on snapdragons in the 1020s. In 1929, Baur gave him the position as his assistant at the new Kaiser-Wilhelm-Institute for breeding research and soon after the leadership of his own department. So Stubbe enjoyed at least for some time the favourable conditions at the new Kaiser-Wilhelm-Institute with Baur.[70] Emmy Stein did not. She had succeeded to produce various mutations, which created inheritable cancer in the plant. She investigated these cases using breeding techniques and the ontogenetic analysis of the effects of mutations within the cell nuclei.[71] It would be worthwhile comparing the work of Stein and Stubbe to see how the institutional position of an experienced female and a young male scientist determined the choice, scale and modification of an experimental system in mutation research. It also will be an interesting task to evaluate the potential of Emmy Stein's experimental system with inheritable cancer induced by radiation, if she would have had the research facilities of her time and enough work force to make the best use of it.

I hope that I have convinced the reader that it would be a rewarding task to find out how gender, the choice of experimental systems, the appreciation of specific methodological approaches and scientific authority were intertwined. Using gender as an indicator for the establishment of hierarchies in everyday life and scientific work, women's work in the production of genetic knowledge should be reconsidered and the resulting genetic knowledge as well.

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70) Susanne Heim, „Die reine Luft der wissenschaftlichen Forschung". Zum Selbstverständnis der Wissenschaftler der Kaiser-Wilhelm-Gesellschaft, Ergebnisse 7, Vorabdrucke aus dem Forschungsprogramm „Geschichte der Kaiser-Wilhelm-Gesellschaft im Nationalsozialismus", Carola Sachse (Ed.), Berlin 2002.

71) AGMPG, III, I, 30, 1, Emmy Stein, 8 pages typoscript of a talk at the 6th International Meeting of Botanists in Amsterdam, 1935: Über neuere Fragen der experimentellen Gen Mutation [sic!].

Conclusions

In the early, not yet prestigious phases of the development of genetics as a scientific field, women were part of the scientific activity. Most of them did not come the direct way from school and university, as women were not given equal access to education compared to men. In the more informal situation of a newly developing scientific field women could start their work in research. In the second phase certain experimental systems based on supportive work of several people yielded results of a high value in a male dominated scientific community. Now the situation changed. Women scientists did not lead scientific groups, which used these experimental systems. Some did work there scientifically being the wife of the leader of such groups. Drawing on the resources of that group including the work force of female technical assistants these women contributed by a work of considerable value to the group's and their husband's successes. Others worked informally as wives of a member of a prestigious group, augmenting the work power at his disposal and promoting his status in the community. The work of these women scientists was made more or less invisible due to their informal status, due to their own activities as co-operative wives, due to the underrating by colleagues, and due to the neglect of the historians. In the second phase, independent, not married women scientists found themselves outside the large prestigious groups, they found themselves on precarious job positions, with experimental systems they could manage on their own and without the potential to extend them to large scale and diversified experiments. The questions they could answer gained less recognition than the ones asked by the large male groups. In very few cases the recognition came decades later, as the example of Barbara McClintock has shown.[72] The question has been left to answer, what this stratification of work and power created by gender did to the knowledge produced by genetic research.

Dr. Helga Satzinger

72) E. Fox Keller, A Feeling; N. Comfort, Entangled Field.

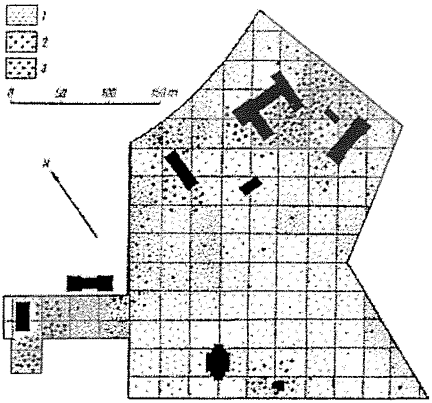

Figure 1.
Distribution of three species of Drosophila on an area of 8 acres (= i.e. Kaiser Wilhelm-Institute for Brain-Research in Berlin-Buch) 1= Drosophila obscura, 2=Drosophila funebris, 3=Drosophila melanogaster. From:
Nikolaj Timoféeff-Ressovsky - N. N. Voroncov - A. V. Jablokov, Kurzer Grundriss der Evolutionstheorie, Jena: VEB Fischer 1975, p. 130.
Nikolaj Timoféeff-Ressovsky, Genetik und Evolution, Zeitschrift für induktive Abstammungs- und Vererbungslehre 76, 1939, p.154-219, 198.

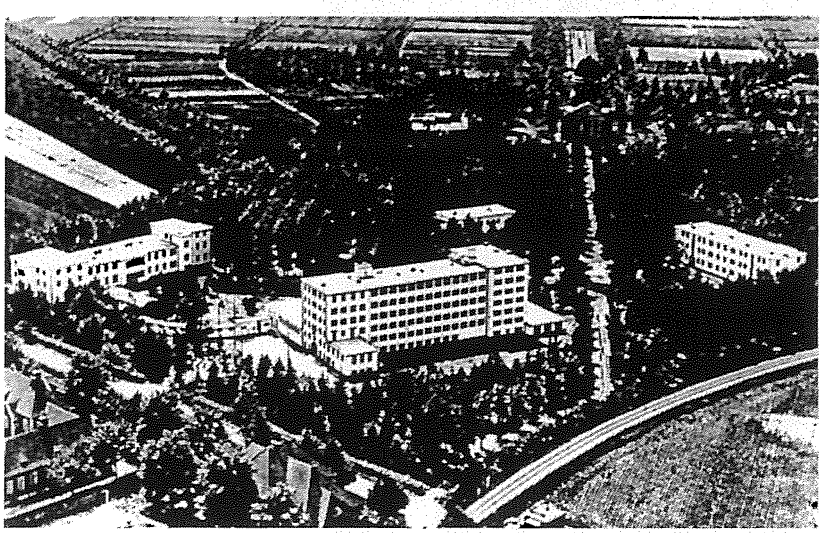

Figure 2.
Clinic Institute with laboratories and flats for staff
Kaiser-Wilhelm-Institute for Brain Research, Berlin-Buch ca. 1931.

Figure 3.
Staff and some invited friends of the Kaiser-Wilhelm-Institute for Brain Research Berlin-Buch, inauguration 1931.

Figure 3a.
Technical assistants of the department for genetics, Kaiser-Wilhelm-Institute for Brain Research, Berlin-Buch, (1931/33), working with Epilachna chrysomelina.

Tab. 3. Proportionalität zwischen den Raten geschlechtsgebundener Mutationen und den Röntgenbestrahlungsdosierungen bei *Drosophila melanogaster*. (50 KV, 1 mm Al.)

Röntgenstrahlen-dosis in r	Zahl der Kulturen	Zahl der Mutationen	Prozent der Mutationen
Kontrolle	3058	4	0,13 ± 0,07
750 r	988	21	2,12 ± 0,46
1200 r	718	27	3,76 ± 0,71
1500 r	803	34	4,23 ± 0,71
2400 r	518	39	7,53 ± 1,16
3000 r	619	53	8,56 ± 1,12
3600 r	430	46	10,69 ± 1,49
4800 r	392	54	13,77 ± 1,74
6000 r	416	65	15,62 ± 1,78

Figure 4.
Icrease of Mutations with increase of radiation dosage (X-rays):
Nikolaj Timoféeff-Ressovsky – Karl G. Zimmer – Max Delbrück, über die Natur der Genmutation und der Genstruktur, Nachrichten von der Gesellschaft der Wissenschaften zu Göttingen, Mathematisch-physikalische Klasse, Fachgruppe VI, Neue Folge 1935, 1, p. 190-245; p. 203.

Dosis in r	Zahl der ausgezählten zweigeteilten Eier	Normal %
800	186	76,8 ± 3,14
1400	106	58,4 ± 4,90
1800	82	42,9 ± 5,50
2200	44	22,9 ± 6,3
3000	110	13,63 ± 3,8
4000	81	9,88 ± 3,7

Figure 5.
Increase of harmful effect with increased dosage of X-rays (decrease in number of normally developing fertilised egg with increased dosage of X-rays):
Paula Hertwig - Hildegard Brenneke, Die Ursachen der herabgesetzten Wurfgröße bei Mäusen nach Röntgenbestrahlung des Spermas, in: Zeitschrift für Induktive Abstammungs- und Vererbungslehre 1937, 72, p. 483-487.