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*Annals of the history and philosophy of biology, Volume 12 (2007)*

*Contents*

Kärin *Nickelsen*
From Leaves to Molecules:
Botany and the Development of Photosynthesis Research……………………….1

Rudolf *Hagemann*
The reception of the Schimper-Mereschkowsky endosymbiont hypothesis
on the origin of plastids – between 1883 and 1960
–many negative, but a few relevant positive reactions…………….……………..41

Vítězslav *Orel*
Science studies and nature of Mendel´s paradigm……………………………….61

Manfred D. *Laubichler,* Jane *Maienschein,* Grant *Yamashita*
The Embryo Project and the Emergence of a Digital Infrastructure
for History and Philosophy of Science …………………………………….……79

Luminita *Göbbel*, Rüdiger *Schultka*, Lennart *Olsson*
Collecting and dissecting nature: Meckel's Zootomical Museum at the
University of Halle, Germany…………………………………………..……. 97

Brigitte *Wirth*
Maria Sibylla Merian, Baltasar Scheid und Richard Bradley.
Die Künstlerin und Naturforscherin, ein Kaufmann und ein Botaniker.....……..115

Dieter *Mollenhauer*, Burkhard *Büdel*, Ekkehard *Höxtermann*, Volker *Wissemann*
Gregor Kraus (1841–1915) – versatile botanist from Lower Franconia……..…..155

Reimer *Kornmann*
Zwei lebensgeschichtliche Dokumente von Peter Kornmann (1907 – 1993)........173

Instructions for authors

# From Leaves to Molecules: Botany and the Development of Photosynthesis Research

*Kärin Nickelsen*

**Abstract.** From the middle of the nineteenth century to the middle of the twentieth century, the discipline of botany went through enormous change: from being a branch of natural history, botany developed into an experimental laboratory science, while intensive interdisciplinary collaboration developed between the neighbouring disciplines of biophysics and biochemistry. This paper traces these changes through the presentation of a few selected landmarks in the history of photosynthesis research.

## 1 Introduction

From leaves to molecules – this is the path that botany took between 1860 and 1960. These one hundred years saw fundamental changes in most of the biological sciences, among them botany: although in the nineteenth century botany was still a part of natural history, with a strong emphasis on morphological studies, by the middle of the twentieth century it had been transformed into the experimentally dominated laboratory science that we know today (although, of course, morphological studies are still an important aspect of botany).[1] In the following

[1] Allen (1978) is still the classic reference for a description of this transformation process, although his account has since been challenged, first, for placing too much emphasis on the alleged "revolt from morphology" aspect of the transformation and for neglecting the fact that descriptive parts of biology did not cease to exist in the twentieth century, and, second, that the shift should be seen as evolutionary rather than revolutionary, with more continuities than discontinuities. See Maienschein,

paper, I trace these changes, using photosynthesis research, which lends itself readily as a case in point, by way of example. During the period under consideration, photosynthesis research developed at an astonishing pace: from the modest, qualitative observation that starch grains in chloroplasts were, most probably, the first stable product of photosynthesis, studies in this field moved, over time, to a detailed model of the process on a molecular scale – elegantly described in the still well-known Z-scheme of the photosynthetic light reactions, which was first proposed in 1960 by Robin Hill and Fay Bendall (Hill & Bendall 1960), and in the carbon dioxide reduction cycle, elucidated by, among others, Melvin Calvin, Andrew Benson and James Bassham by 1956 (summarised in, e.g., Calvin & Bassham 1957).

These changes, not only in photosynthetic research but also in plant physiology in general, were nicely caught by the German plant physiologist Erwin Bünning (1906–90), who discovered the physiological clock in the 1950s.[2] In his 1977 autobiographical account, Bünning described the difference between the "old" and the "new" plant physiology as follows:

> The earlier plant physiologists were botanists. They demonstrated the validity of the biogenetic law: ontogeny repeats phylogeny. They repeated in their life the whole history of botany. As children they collected plants, later on they ordered them in a herbarium according to the rules of taxonomy, they continued with morphology and anatomy, and finally became interested in physiology. Only a few of these botanists were able to fill the gaps in their knowledge of physics and chemistry. This type of plant physiologist was still predominant in my country in the years after the second war. […]
>
> The new plant physiologist is actually what he should be in the present situation of biology: not primarily botanist or zoologist, but rather a chemist or physicist who succeeds in recognizing the physical and chemical complexity of those special natural structures which we call organisms. Most of the earlier chemists and physicists did not realize this. (Bünning 1977: 21; cited also in Höxtermann 2000: 501)

This change from plant physiologist to physico-chemical physiologist is exactly what occurred in the development of photosynthesis research; I would like to demonstrate this transformation, using as example a few selected landmarks in the history of photosynthesis research, which also illustrates the more general course of developments.[3]

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Rainger & Benson (1981) for a summary of their critique, which was published together with more elaborate essays by each of the three authors, complemented by a response by Allen and a lucid comment by Frederick B. Churchill (Churchill 1981) in the *Journal of the History of Biology* (1981), vol. 14, pp. 83–191.

[2] On Bünning, see, among others, the tribute by Mohr (1987); Bünning (1987) and Bünning (1977) are autobiographical accounts of selected aspects of Bünning's life.

[3] I apologise in advance for the inevitable incompleteness of the picture that I give here. The history of photosynthesis research during the nineteenth and twentieth centuries has been told many times,

## 2 Julius Sachs and the Foundation of Plant Physiology

The German plant physiologist Julius Sachs (1832–97) is the obvious starting point of this story, since it was Sachs who initiated experimental plant physiology as a promising field of research and as an academic discipline.[4] He mastered the whole range of what became the standard topics of this field, from studying the movements of plants to osmotic processes and photosynthesis, although the latter was then still seen as a side issue – a minor part of a plant's metabolism.

Sachs's scientific achievements were enormous. He was also one of the first proponents of a mechanistic view on biological phenomena; not coincidentally, Jacques Loeb (1859–1924), who became its leading spokesperson in the early twentieth century, was one of Sachs's students.[5] However, Sachs achieved his greatest fame through his textbooks, which were translated into English and quickly became the standard literature in courses of general botany and plant physiology (see Sachs 1865, 1868 for the first editions). The American botanist William G. Farlow was one of many international scientists to be impressed by the quality of these textbooks. Farlow recalled coming across Sachs's books for the first time in an address of 1913:

> In the laboratory [of De Bary], I noticed that the students seemed to refer frequently to a book of which I had never seen a copy or even heard. The book was Sachs's "Lehrbuch", second edition 1870. I bought the book and was perfectly amazed. I had never dreamed that botany covered so large a field. […] It may be that the facts there given were generally known in Germany, but they were not known in other countries. (Cited Hottes 1932: 16)

Sachs's influence, thus, cannot be overestimated; and it was greatly because of his influence that chairs of plant physiology became standard in institutes of botany elsewhere.

The one scientific contribution by Sachs that I would like to single out concerns his observation of starch grains. It was already known in 1860 that the chlorophyll "corpuscles" (which were the chloroplasts) of leaves usually contain starch grains; and it was even believed that these grains might actually be chlorophyll precursors. Sachs set out to explore this phenomenon in more detail,

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and my account benefits strongly from earlier versions. For more comprehensive approaches, see, in particular, Govindjee et al. (2005), which comprises a large collection of historical perspectives on the history of photosynthesis, including detailed "timelines" of discoveries in oxygenic and anoxygenic photosynthesis. The latter were previously published as Govindjee & Krogmann (2004) and, respectively, Gest & Blankenship (2004). See also Huzisige and Ke (1993), which gives the full references of most of the influential scientific contributions to photosynthesis research published until 1993; see, furthermore, Myers (1974), for a lucid and comprehensive perspective on the development of the major concepts in twentieth-century photosynthesis research.

[4] On Sachs's life and work, see, among others, Pringsheim (1932) and Mägdefrau (1992), pp. 259–264, both with numerous additional references. Valuable information can also be found in the German series of books and booklets entitled "Materialien zur Bibliographie und Biographie von Julius von Sachs", edited by Hartmut Gimmler. See, e.g., Gimmler et al. (2003).

[5] See, e.g., Osterhout (1928). For a more recent biography of Loeb, see Pauly (1987).

and, in 1862, he came to the conclusion, based on a detailed study of etiolated plants and their changes when moved into the light, that: first, "the chlorophyll grains [= the chloroplasts] are the only and sole place where starch is formed from inorganic matter" (Sachs 1862: 372);[6] and, second, "the embedded starch grains in the chlorophyll are not only a secondary phenomenon but are formed under the influence of a certain light intensity by means of the assimilating action of the latter" (Sachs 1862: 365).[7] Sachs maintained that the starch would then be used for the further growth of the plant, in particular, for the growth of new plant organs (since he found that only plants in which starch had accumulated grew). However, Sachs immediately added that he did not imagine that this process of starch formation would be necessarily simple and straightforward:

> By this I did not mean to say that starch in the chlorophyll is immediately yielded, in its perfect state, from carbonic acid and water, by the elimination of oxygen; rather, it is much more likely that here, within the chlorophyll grains themselves, a longer series of chemical transformations takes place. (Sachs 1862: 371)[8]

These findings were completed by studies that were published in 1864, in which Sachs additionally demonstrated that not only the formation but also the storage of starch were functions of the plant's exposure to a minimum intensity of light: if a plant, or parts of it, were not exposed to sunlight, the starch in the chlorophyll grains would disappear (Sachs 1864: 289). However, he was also able to show that, if, in a second step, the same plants were fully illuminated again, the starch recovered. This led Sachs to the assumption that there might be a daily rhythm in plants: although starch is formed and accumulated in the plant during the day, this starch is dissolved during the night and is transported, in the form of a solution of sucrose or other substances, to other parts of the plant and used for its growth and development (Sachs 1864: 294).

## Digression: Generally accepted knowledge around 1860

In order to evaluate Sachs's contribution and all the developments that followed, it is worth taking a brief look at what had been established on photosynthesis up to this point. Because of methodical limitations, not that much had been achieved; in the nineteenth century metabolic processes were still a black box, and

[6] German original: "… so wird man zu der Folgerung genöthigt, dass die Chlorophyllkörner der einzige und ausschließliche Ort sind, wo Stärke aus unorganischem Material erzeugt wird".

[7] German original: "… dass Amylumeinschlüsse des Chlorophylls nicht nur eine secundäre Erscheinung in diesem sind, sondern dass sie unter dem Einfluss einer bestimmten Lichtintensität durch die assimilirende Thätigkeit des Letzteren erzeugt werden."

[8] German original: "Ich will mit diesen Worten nicht etwa gesagt haben, dass die Stärke in dem Chlorophyll so entstehe, dass aus Kohlensäure und Wasser unter Elimination von Sauerstoff sogleich fertige Stärke sich bilde; es bleibt vielmehr die Möglichkeit offen, dass hier, innerhalb der Chlorophyllkörner selbst eine längere Reihe von chemischen Umsetzungen eintritt."

photosynthesis was no exception. The only aspects most scientists agreed on was a basic idea of what went in – carbon dioxide and water – and what came out: oxygen and carbohydrates of some type. Although Sachs had observed that starch was a prominent and detectable product, most scientists thought that it could not be the first true product, since starch was far too big and too complicated a molecule.

In addition to the educts and products, it was also clear that light was indispensable; and since this light had to be absorbed, the leaf's pigments, in particular the green chlorophyll, were also deemed necessary. Finally, it transpired that photosynthesis seemed to stop as soon as the cell was damaged. Hence, there had to be something in the "living cell" that was also essential for photosynthesis. These factors should somehow interact and produce oxygen and carbohydrates (see Fig. 1). It was clear that the whole process had to involve a complicated series of smaller intermediary steps, but what these stages were was the subject of intense discussion.

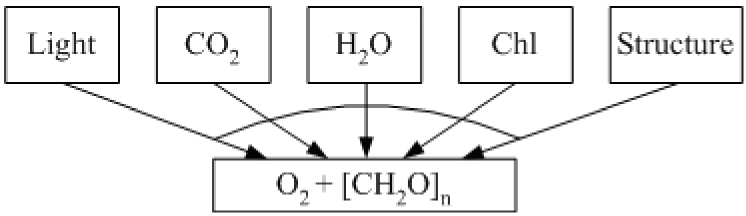


*Fig. 1 The one-step model of photosynthesis, representing what was generally accepted knowledge around 1860.*

However, there was one other thing that most people in the second half of the nineteenth century agreed on: namely, the fact that the amount of carbon dioxide consumed and the amount of oxygen produced were stoichiometrically equivalent. This seemed to point strongly to the assumption that carbon dioxide was the source of oxygen. Thus photosynthesis was conceived of as the decomposition of carbon dioxide under the influence of light. Alternative assumptions existed such as the thought that the oxygen originated from water. This had first been proposed by the French chemist Marcellin P. E. Berthelot (1827–1907) in 1864; it was taken up by the German physical chemist Georg Bredig (1868–1944) and then strongly promoted by the French biologist René Wurmser (1890–1993) (see e.g. Berthelot 1864; Bredig 1914; Wurmser 1921, 1930); yet, the former point of view predominated by far.

Finding out the mechanism of this decomposition process became a major research programme of the second half of the nineteenth century. Yet it was neither the botanists who primarily concerned themselves with this task, nor the plant physiologists. It was the organic chemists that began to dominate the field.

## 3 In Pursuit of a Pathway: The Formaldehyde Hypothesis

Perhaps it is not that surprising that the main contribution made by botanists to nineteenth-century photosynthesis research was to identify the site of the process (the chloroplasts), and that their incentive to elucidate the mechanism was brought to a halt when Sachs found that starch was a product. Nineteenth-century botanists simply did not have the methods and the skills to address the mechanism as such: they were trained in morphology and microscopy, and they had only just started to develop an interest in other, more quantitative techniques. They were certainly not skilled in carrying out chemical analyses; nor did they yet think about plants in terms of biochemical pathways.

This left the task of finding a possible mechanism of photosynthesis to the chemists, who were eager to take up the challenge. The second half of the nineteenth century saw the rise of organic chemistry, which experienced one staggering success after another. It was only a matter of time before these chemists discovered biological phenomena as another, rewarding field of research. However, as one might expect, it was not the plants as such that interested them, but the fact that plants were able, at room temperature, to synthesise sugars – something that chemists would have loved to do themselves. Their reasoning was that, as soon as the photosynthesis process in plants was known, it would become possible to synthesise sugar artificially, which was a strong incentive to enter this field of research.[9]

In actual fact, the roots of "biochemistry" – in the sense of trying to elucidate the chemical path of biological reactions – could already be seen in the work of Justus Liebig (1803–73), who was also the first scientist to suggest a chemical model of photosynthesis, albeit a rudimentary one (Liebig 1843). However, the most influential of the many chemists who spent their time modelling photosynthesis was the German organic chemist Adolf von Baeyer (1835–1917).[10] One of the most eminent figures of his time, he became famous mainly for artificially synthesising and elucidating the structure of the dyestuff indigo, which he achieved in the late 1870s. It was primarily for this work that he was made one of the first Nobel laureates in 1905.

[9] The possibility of reproducing photosynthesis under artificial conditions was even seen as the final and necessary "proof" of the fact that the one or other reconstructed mechanism was accurate (in the way that in organic chemistry a successful artificial synthesis was the required proof of a postulated structure). Even as late as 1940, the English chemist Edward C. C. Baly wrote: "Complete confidence in this explanation of the mechanism of photosynthesis can only be established by convincing proof that each of the two reactions can be achieved in the laboratory." (Baly 1940: 32).

[10] See, e.g., Willstätter's obituary of Baeyer (Willstätter 1915).

In 1870, before he had become that well known, Baeyer formulated and published his so-called formaldehyde hypothesis of the photosynthetic process (Baeyer 1870); and it was this model that dominated attempts to understand the photosynthesis mechanism until well into the twentieth century.[11] In essence, the model comprised the assumption that the first reduction product of photosynthetic assimilation, which resulted from the photolysis of carbon dioxide in the presence of water, light and chlorophyll, was formaldehyde, and that oxygen was released at the same time.

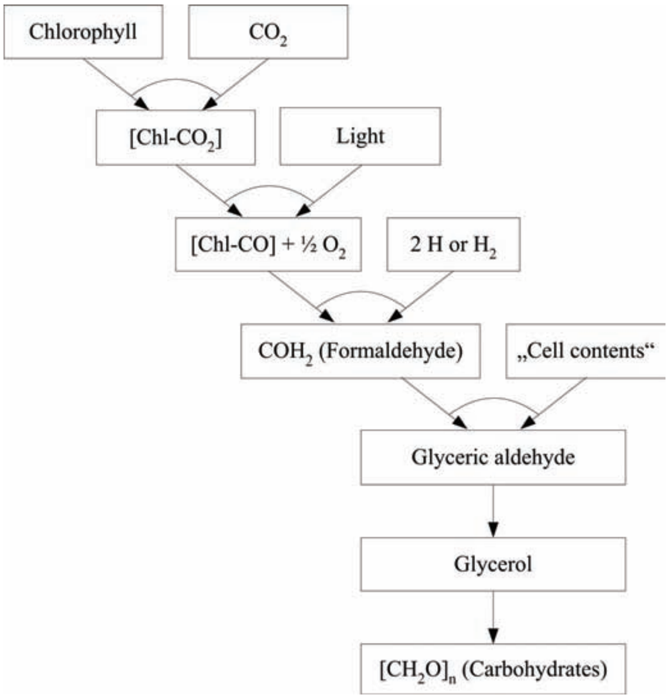


*Fig. 2 The formaldehyde hypothesis, reconstructed from Baeyer (1870).*

According to this hypothesis, the carbon reduction in photosynthesis consisted of several partial processes. First, carbon dioxide binds to the chlorophyll; in this state and under the influence of light carbon dioxide is reduced to carbon monoxide, upon which oxygen escapes. Then, the carbon monoxide is reduced further to formaldehyde, by the integration of either molecular hydrogen or two

[11] On the content and reception of the formaldehyde hypothesis, see, e.g., Florkin (1977), p. 147ff., and Stiles (1925), p. 94ff.

atoms of hydrogen from other sources (which were not specified). Thus, Baeyer assumed that the actual reduction of carbon dioxide down to the final oxidation state (which was present, for example, in formaldehyde) occurred without the presence of any intermediates. In subsequent reactions, the formaldehyde was then thought to condense to carbohydrates – a process that was presumably promoted by the cell content, the influence of which was unknown. For example, Baeyer hypothesised that the first sugar product might still be associated with the components of cells, and that it would only later be released as sucrose, starch or cellulose.

Baeyer's theory was eagerly taken up by his contemporaries and by succeeding generations of scientists, many of whom regarded this model as the first experimentally supported proposal to explain carbohydrate synthesis inside and outside the living plant (one of the points that Liebig's earlier model had not even touched upon). The persuasiveness of Baeyer's hypothesis was partly due to the fact that, in the 1890s, the eminent German chemist Emil Fischer (1852–1919), who had done his PhD with Baeyer, had succeeded in demonstrating that formaldehyde was, indeed, a possible starting point of the synthesis of the two hexoses, which were thought to be among the major end products of photosynthesis (*d*-glucose and *d*-fructose). In addition, one of the pathways that Fischer purported included glyceric aldehyde as an intermediate, the possible existence of which Baeyer had already hypothesised; Fischer also demonstrated that glycolic aldehyde, which can also be derived from formaldehyde, could also be a possible intermediate (Fischer 1909a, 1909b). At the time, these findings were believed to corroborate strongly the formaldehyde hypothesis.

If one goes back to the original paper, however, the evidence looks surprisingly flimsy. The general assumption was based on observations made by the Russian chemist Alexander Mikhailovich Butlerow (1828–86) in 1861. On heating trioxymethylene, a condensation product of formaldehyde, in an alkaline medium, Butlerow had found that a viscous fluid, which had some of the properties of sugar, emerged. Baeyer took this as the starting point of his theory of carbohydrate synthesis in living plants: he suggested that this same process occurred in plants (Baeyer 1870).

The second point in favour of Baeyer's hypothesis was that it looked so straightforward and simple. It seemed to be the obvious way to produce carbohydrates from carbon dioxide – on paper at least. By adding two hydrogen atoms to carbon monoxide, one arrives immediately at formaldehyde; these formaldehyde units would then only need to be slightly rearranged in order to yield, after subsequent condensation reactions, carbohydrates. The failure to detect formaldehyde – a strong cell poison – in plants clearly made the formaldehyde hypothesis problematic, although most scientists did not consider this absence to be fatal. The only conclusion that they drew from these negative results was that the methods were inappropriate.

The principle argument brought forward by Baeyer was typical of chemists of the time who were addressing the photosynthetic mechanism (or any other biological problem): results that had been obtained in artificial model systems were immediately transferred to the processes in living plants. The chemists seemed to have been neither particularly bothered by the fundamental differences between their test tubes and physiological cell conditions (in terms of, for example, temperature, pressure or pH), nor by the potentially poisonous effect of an assumed intermediate.

## 4 The Physiological Turn: F. F. Blackman and two types of reactions

It was only after several decades of eager model building on the part of chemists that botanists – and plant physiologists, in particular – started to become troubled by this assumption that a plant cell should function in the same way as experiments in a test tube. By 1900, the predominance of the chemists and their peculiar way of dealing with biological phenomena were being increasingly challenged. One of the main proponents of a new approach to these problems was the German plant physiologist Wilhelm Pfeffer (1845–1920), Sachs's most famous student. In his influential textbook on plant physiology, Pfeffer made it clear from the onset that plant physiology could only advance from its present state if it made use of the techniques and the knowledge of the fields of physics *and* chemistry (Pfeffer 1897: 1-7). At the same time, Pfeffer underlined the risks inherent in using chemical model systems for the simulation of organic processes. Thus, although he emphasised the urgent need to exploit chemical and physical knowledge and methods, Pfeffer also reminded his readership of the limitations of this practice:

When utilising the experiences of the other branches [of the sciences], one must not forget that one can only decide with recourse to the specific problem and the organism itself whether a physiological process actually proceeds according to what seems possible from the point of view of what is known today in the fields of physics and chemistry. To construct the processes in the organism in general, solely based on those experiences and under the pressure of these sciences resembles the reasoning of the peasant who was convinced, when he first saw a locomotive, that there was a horse inside. (Pfeffer 1897: 6)[12]

[12] German original: "Bei der Nutzbarmachung der Erfahrungen auf anderen Gebieten [der Naturwissenschaften] darf aber nicht vergessen werden, dass nur durch die Fragestellung und den Organismus selbst entschieden werden kann, ob ein physiologischer Vorgang sich gerade so abspielt, wie es nach den derzeitigen physikalisch-chemischen Kenntnissen möglich erscheint. Denn auf Grund solcher Erfahrungen und unter dem Drucke dieser schlechthin das Geschehen im Organismus construiren, das gleich der Logik jenes Landmanns, der beim Erblicken einer Lokomotive sicher zu wissen glaubte, dass ein Pferd darin stecke."

Perhaps it was not coincidental that, only a short time later in 1905, it was a plant physiologist who made another landmark contribution to photosynthesis research: the Englishman Frederick Frost Blackman (1866–1947).[13] Blackman and his collaborators – notably Gabrielle L.C. Matthaei – were the first to study systematically and quantitatively the influence of external factors on the course of photosynthesis. Thus, they shifted the emphasis from the search for actual intermediates – which was hopeless, given the methods of the time – to the study of the rate of photosynthesis under different circumstances (Blackman 1905, Matthaei 1905, Blackman & Matthaei 1905).

What they found was a complex interplay of three main factors: temperature, carbon dioxide concentration and light intensity. At strong light intensities, increasing concentrations of carbon dioxide promoted photosynthesis, but only up to a certain point, while under these conditions, a rise in temperature was able to accelerate photosynthesis even further. At low light intensities, however, the maximum rate of the process was reached at a much lower concentration of carbon dioxide, and a rise in temperature had no accelerating effect. Based on these findings, Blackman formulated the so-called law of the limiting factor, which states that the rate of a process, which is influenced by several factors, is limited by the factor in least supply (Blackman 1905: 289). This dealt a strong blow to the predominant dogma that all processes should have clearly defined, fixed "optimum conditions". Blackman's findings rather pointed to the fact that the optimal intensity of any factor strongly depended on the prevailing intensities of the other factors. In Figure 3, Blackman's original graph, which represents schematically his law of the limiting factor, is reproduced. Applied to photosynthesis, Blackman interpreted the curve as the rate of the process at different carbon dioxide concentrations; the different curves show the values at different light intensities (Blackman 1905: 291).

These results seemed to indicate that there were conditions in photosynthesis under which a rise in temperature accelerated the rate of the process (if carbon dioxide is in relatively short supply compared with light intensity). This implied that there was one partial reaction, in which carbon dioxide was involved, that was of a thermochemical nature. By contrast, under conditions where there was a relative deficit of light, the temperature had no influence at all – which was to be expected if, under these conditions, a photochemical process was limiting the rate of photosynthesis. Thus, this was the first intimation that photosynthesis might consist of a light-dependent process and at least one additional light-independent process. However, Blackman's results were not interpreted as such for a long time to come – and when they were, it would, again, not be by the plant physiologists but by the chemists.

[13] On Blackman's life and work, see, e.g., Steward (1947) and Briggs (1948).

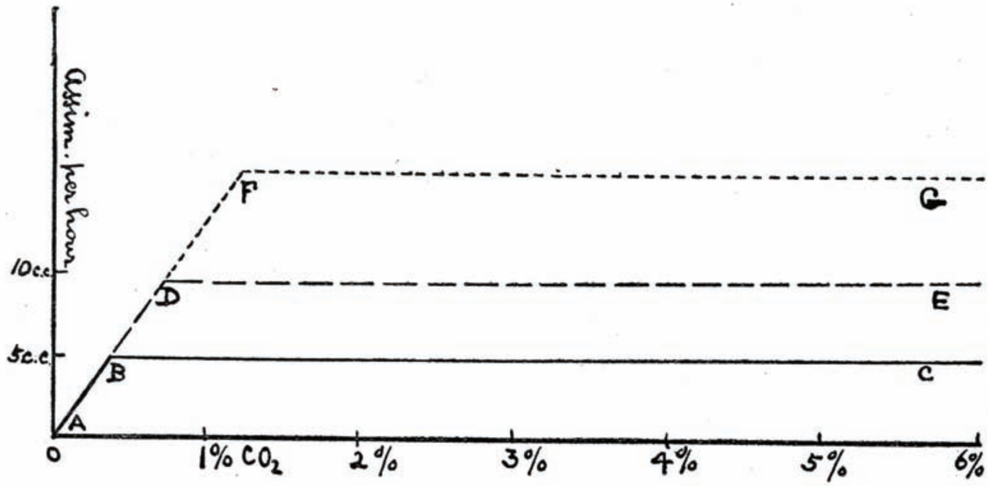


*Fig. 3 F. F. Blackman's curve, illustrating schematically the effect of a limiting factor on the rate of a process (taken from Blackman 1905: 291). The curve ABC exemplifies the course of the process in which the limiting factor rapidly comes into play, whereas the curve ABFG illustrates the course in which the factor only becomes limiting after a relatively high rate has been reached.*

## 5 Richard Willstätter and Chlorophyll

The next landmark in photosynthesis research was the seminal work on chlorophyll and other plant pigments carried out by the German organic chemist Richard Willstätter (1872–1942) – a student of Baeyer's – and several collaborators, primarily his long-standing Swiss assistant Arthur Stoll (1887–1971).[14] Willstätter and Stoll were the first to succeed in isolating chlorophyll from plant leaves and determining its formula: the prerequisite for any further detailed study of the structure and chemical reactions of chlorophyll. (It was another German chemist, Hans Fischer, who would fully determine the structure in the 1940s.) Their studies culminated in a voluminous monograph on chlorophyll that was published in 1913 (Willstätter & Stoll 1913). With this background and experience, Willstätter and Stoll felt themselves qualified to study the mechanism of photosynthesis as well; they started to do so around 1915, and in 1918 published another large

[14] For Willstätter's autobiography (published posthumously by Stoll), see Willstätter (1949), translated into English as Willstätter (1965); for one of the many obituaries, see, e.g., Kuhn (1949). A full biographical account of Willstätter's life and work has yet to be written. On Stoll, see, e.g., Ruzicka (1971).

monograph, in which they presented their own model of the process (Willstätter & Stoll 1918).

Willstätter and Stoll's general idea was similar to Baeyer's approach: they too believed that, in a first step of the photosynthetic process, a complex of chlorophyll and carbon dioxide was formed; and they also assumed that the formation of carbohydrates went through a formaldehyde stage. However, they contributed something new to photosynthesis research by interpreting Blackman's results as indicating that two different types of reactions occurred in photosynthesis, although they did not go into much detail. And although their conception was eventually proven to be wrong, this model (with minor changes) remained the predominant idea of the photosynthesis mechanism until well into the 1930s (see, for example, Stoll 1936 for a summary of the most elaborate version of the model).

No less important was the fact that, although Willstätter and Stoll were both chemists, they explicitly distanced themselves from the prevailing practice of using, rather light-handedly, artificial model systems in order to find out how the different processes of the plant worked. They always worked with actual plant materials and, like Blackman and his collaborators, tried to follow the rate of the process by measuring gaseous exchanges in a gas chamber. In the final essay of their 1918 book, Willstätter and Stoll briefly reviewed the chemical work done in the field, for example, chemical studies that demonstrated how carbon dioxide could be reduced through the effect of ultraviolet rays, electrical discharges and radioactivity. Commenting on these investigations, the authors wrote:

These studies deserve considerable interest from a chemical point of view, while less from the point of view of plant physiology. They say nothing at all about the process in the chlorophyll grain. There is no need to prove that it is possible to decompose the most stable compound of carbon through the effects of the chemical energy of reducing agents or through other means of the supply of energy. The reduction has indeed successfully been achieved, in the most different kinds of ways, which are by no means connected to the conditions in the plant and from which, thus, no conclusions may be drawn as far as the natural process is concerned. One task of plant physiology is to explore the apparatus of the chloroplasts for the decomposition of carbonic acid and the synthesis of carbohydrates, and to determine the different stages of the process in the assimilation organs. This task is not going to be solved if the decomposition of carbonic acid is brought about under some conditions, that is, instead of merely through heating by other means of energy supply. (Willstätter & Stoll 1918: 371f.)[15]

[15] German original: "Diese Arbeiten verdienen großes Interesse in chemischer Hinsicht, geringes in pflanzenphysiologischer. Sie sagen gar nichts über den Vorgang im Chlorophyllkorn aus. Es bedarf keines Beweises für die Möglichkeit, die stabilste Verbindung des Kohlenstoffs durch die chemische Energie von Reduktionsmitteln oder durch andere Mittel der Energiezufuhr zu zerlegen. In der Tat ist die Reduktion auf die verschiedenartigsten Weisen gelungen, die keinen Zusammenhang mit den

Yet even though the model suggested by Willstätter and Stoll was very influential, the real breakthrough in photosynthesis research would come a couple of years later, in 1919, when the chemist and cell physiologist Otto Warburg entered the stage.

## 6 Otto Warburg: New Techniques and Approaches

Otto Heinrich Warburg (1883–1970) started his academic career as a student of chemistry, among other places in the Berlin laboratories of Emil Fischer, whom he always greatly admired.[16] Like many other organic chemists of the time, Warburg felt attracted to the chemistry of living matter, but unlike many others, he made sure that he gained qualifications in this area: having completed his doctorate in chemistry in 1906, Warburg went on to study medicine in Heidelberg; at the same time, he turned his interests to the fundamental, biochemical problems of life.

The first topic that Warburg addressed was the problem of cell respiration, which is examined in his first independent paper of 1908. His contributions to this field were seminal, and would eventually win him the Nobel Prize in Medicine or Physiology (in 1931). What is less well known about Warburg is that, besides his studies in respiration and cell cancer, he also worked continuously on photosynthesis – his first paper on the subject was published in 1919, the final one in 1969 (Warburg 1919, Warburg et al. 1969).

Warburg's work in photosynthesis proved to be a major turning point in the development of the mechanism (see Warburg 1919, 1920; see Nickelsen 2007 for an analysis of these papers' arguments). Although his conceptual contributions all turned out to be flawed, he had a lasting and revolutionising impact on photosynthesis research as he introduced new, sophisticated techniques that at last provided the means to investigate the gaseous exchange processes in plant cells accurately and quantitatively. This was achieved mainly by Warburg bringing the technique of manometry, which had hitherto only been used in animal physiology and physics, to the plant sciences; and in order to make the best use of this technique, Warburg additionally introduced a new model organism: the unicellular green alga *Chlorella* (see Zallen 1993).

The advantages of replacing the study of whole leaves or even plants with this tiny organism were enormous. In leaves, diffusion processes and many other

Verhältnissen in der Pflanze haben und aus denen keine Schlussfolgerungen auf den natürlichen Vorgang gezogen werden könne. Eine Aufgabe der Pflanzenphysiologie besteht darin, die Vorrichtungen der Chloroplasten für die Kohlensäurezerlegung und die Kohlehydratsynthese genauer zu erforschen und die einzelnen Phasen des in den Assimilationsorganen verlaufenden Vorganges zu bestimmen. Diese Aufgabe wird der Lösung nicht näher gerückt, wenn unter irgendwelchen Bedingungen der Zerfall der Kohlensäure bewirkt wird, nämlich statt einfach durch Erhitzen mittels anderer Arten der Energiezuführung."

[16] For biographical information on Warburg, see, e.g., Krebs (1972, 1979), Henning (1987), Werner (1988, 1991), Höxtermann & Sucker (1989) and Höxtermann (2001).

factors inevitably distort the measurements and present insurmountable inaccuracies; in unicellular organisms, by contrast, conditions are as homogeneous as is possible, and confounding parameters are considerably reduced. These advantages were so obvious to the scientific community that Warburg's techniques spread rapidly; particularly in the 1930s and 1940s, Warburg apparatuses were almost ubiquitous in the world's physiological laboratories.

Equipped with these new techniques, Warburg revisited the influence of external factors on photosynthesis; and, like Blackman and Willstätter before him, he confirmed the peculiar interplay of temperature, carbon dioxide concentrations and light intensity under varied conditions. Warburg also interpreted these phenomena as indicating that photosynthesis consisted of two different types of processes. While there was no doubt that at least one of these was a photochemical one – that is, light dependent – there was much in favour of the assumption that there was, in addition, at least one other process that was a light-independent, purely chemical reaction. In a later article, he would term this light-independent process the "Blackman reaction" of photosynthesis (Warburg 1921: 355).

Furthermore, Warburg's innovative contributions also included introducing completely new approaches to the problem of how photosynthesis worked. Inspired by his own studies in cell respiration, Warburg was the first to use specific inhibitors to find out more details about the reactions; and he was the first to try to determine quantitatively the energetic details of the process, in terms of the quantum requirement. Together with his long-standing collaborator Erwin Negelein (1897–1979), Warburg deduced from his measurements that, in order to release one molecule of oxygen, photosynthesis required a minimum of four to five light quanta (Warburg & Negelein 1922, 1923). It is well-known today that these figures were in error by a factor of two to three; and it is also known how stubbornly Warburg refused to acknowledge this fact, which sparked off an extremely fierce controversy between Warburg and the larger part of the US American photosynthesis community. This dispute lasted for about twenty years and grossly impeded further advances in the field, because too many people devoted too much of their time trying to determine quantum yields under a variety of conditions (see, for example, Govindjee 1999 for a survey).

Nevertheless, the Warburg–Negelein papers were a major breakthrough: after all, the quantum requirement and, in principle, the energetic parameters of photosynthesis provided a completely new and important way of finding out more about the mechanism. If the overall energy requirements were known, then it would be easier to exclude those theories that did not fit. In fact, the incredibly high efficiency of the process, which Warburg and Negelein seemed to have detected, led people to look for a new and unique photophysical process, although most still chose to cling to Willstätter's vision that this process had to act upon a chlorophyll–carbon dioxide complex and produce carbohydrates via formaldehyde. No other promising alternatives presented themselves at the time.

Measuring quantum yields was not something that Warburg had invented himself. He had picked up this technique from the field of physics; in 1920, quantum yields were already a standard parameter in analysing usual photochemical reactions. Physicists had started to study the effects of light quanta soon after Albert Einstein had formulated the light quantum hypothesis in 1905. After this date, the analysis of photochemical processes in terms of quantum phenomena gained considerable attention. Otto Warburg's father, Emil, concerned himself with these phenomena, and it was most probably through him that Warburg developed his innovative approach to photosynthesis. (See Nickelsen, forthcoming, which explores the various roots of Warburg's peculiar photosynthesis model.)

Thus, in a way, Warburg exemplified the new and more integrated approach to photosynthesis and other physiological processes: Warburg had a thorough grounding in chemistry, a sound understanding of biological phenomena (if only on a cellular level), and he also tried to emulate the latest physical techniques in his physiological studies. In other words, he came close to reaching the interdisciplinary character of plant physiology that Pfeffer and others had called for, although he surely had no understanding of the complexities of higher plants, and absolutely no wish to learn anything about them!

## Digression: Universities vs. Kaiser Wilhelm Institutes

One might thus expect that photosynthesis would now have taken centre stage. Yet this did not happen. One of the problems may have been that both Willstätter and Warburg had developed their seminal contributions to photosynthesis studies at two of the first Kaiser Wilhelm Institutes in Berlin-Dahlem: Willstätter at Haber's Institute for Physical Chemistry and Electrochemistry, Warburg at the Institute for Biology. These institutions were marvellously equipped, and had access to high-quality instrumentation, yet they were also somewhat isolated from mainstream science. And thus, their findings and, in particular, their techniques only slowly found their ways into university curricula. The plant physiologist André Pirson (1910–2004) wrote in an autobiographical account that even in the 1930s the application of manometric techniques was mostly unheard of in general institutes of botany, and even a decade after Warburg's first articles had been published, the use of unicellular algae as objects of study remained unfamiliar to most botanists (Pirson 1994).

Consequently, since students of botany were not usually exposed to these cutting-edge findings and methods but had to put up with more traditional approaches, these new fields did not attract many young and promising researchers. Plant physiology continued to be centred predominantly on questions of, for example, cell permeability, the movement of plants and, perhaps, respiration; photosynthesis remained a side issue.

There was, however, one notable exception to this rule: the laboratory of Kurt Noack (1888–1963), who had taken over the chair of plant physiology at the Berlin University in 1931.[17] Noack was not only the leading photosynthesis expert in Germany, he was also one of the leading plant physiologists (and plant biochemists) of the time. However, he can hardly be considered a representative example of botany – or even of plant physiology. Pirson recalled that when, as a high school graduate, he approached Noack to ask him how to become a biologist, Noack told him to study chemistry. After having recovered from the shock, Pirson did precisely that; and on successfully completing this first curriculum, Pirson was immediately taken on by Noack to work with him (Pirson 1994: 210). And if you look at what Pirson achieved in the sciences (not only in the field of photosynthesis), then Noack's advice was pretty good.

## 7 The Photosynthetic Unit

There was, however, a group of scientists that would contribute to spreading the manometric techniques all over the scientific world: namely, those students or scholars who had spent some time in Warburg's research laboratories. One of them was the US-American biologist Robert Emerson (1903–59), who completed his PhD with Warburg in 1927, after having received his Bachelor's degree at Harvard.[18] Interestingly, Emerson's education was not in traditional biology but mainly in general physiology; he had studied primarily with Winthrop J. V. Osterhout, the co-founder, together with Jacques Loeb, of the *Journal of General Physiology*. Thus, Emerson mastered far more biochemistry, physics and mathematics than the usual botany or zoology student of the time; he was also used to using comparative approaches and to working with model organisms. Emerson truly mastered manometry with *Chlorella* cells – perhaps he was *the* master next to Warburg – and before his untimely death (in an airplane crash, at the age of 56) he contributed much to photosynthesis research. Among his major achievements was the experimental evidence for what would later be called the Photosynthetic Unit.

Emerson's evidence was given in the second of two closely related papers, published in 1932 and co-authored by William Arnold (1904–2001), a physicist who was about to receive his Bachelor's degree (Emerson & Arnold 1932a, 1932b; see also Myers 1994).[19] Emerson and Arnold had developed a new and refined technique, based on Warburg's pioneering work, to investigate photosynthesis in *Chlorella* by means of flashing light experiments. Their disconcerting finding was

[17] See, on Noack, e.g., Müller-Stoll (1963), Pirson (1965) and Höxtermann (1998, 1999).

[18] On Emerson's life and work, see, e.g., Rabinowitch (1961) and Govindjee (2001, 2004).

[19] In 1996, two full issues of the journal *Photosynthesis Research* (vol. 48, 1996) were dedicated to William Arnold, comprising a selection of articles on his life and work; see Govindjee et al. (1996). See also Arnold (1991) for an autobiographical account.

that even under optimal conditions, only one oxygen molecule per about 2,500 chlorophyll molecules was released. Trying to make sense of these results, the authors raised the possibility "that for every 2,480 molecules of chlorophyll there is present in the cell one unit capable of reducing one molecule of carbon dioxide each time it is suitably activated by light" (Emerson & Arnold 1932b: 199).

Although many scientists were reluctant to accept this idea, and either challenged the validity of the data or the soundness of their interpretation (see, e.g., Myers 1974: 421), the idea was eventually taken up and enlarged on in 1936 by the German photochemist Hans Gaffron (1902–79),[20] in collaboration with the German physicist Kurt Wohl. Gaffron had already been working on bacterial photosynthesis for some time, and the 1936 paper drew on some of his earlier suppositions (see Gaffron & Wohl 1936; for English reviews of the concept, see, e.g., Gaffron 1939, Wohl 1940). It is interesting to note, however, that Gaffron and Wohl acknowledged that their conceptual interpretation had grown out of the discussions at the regular colloquia held by the physicist-turned-geneticist Max Delbrück (1906–81) in Berlin.[21]

In their paper, Gaffron and Wohl first gave a sketch of the then prevailing theory of photosynthesis:

Carbonic acid is bound to the magnesium in the chlorophyll molecule, while at the same time water is adsorbed at an appropriate place. Within this complex of molecules, consisting of active chlorophyll, carbonic acid and water, the carbonic acid is reduced with the help of light energy, which is absorbed by the same molecule. The reduction occurs in four steps, which take place in the successive exchange of hydrogen against hydroxyl groups. Special chemical properties of the chlorophyll, which are enhanced as its binds to chloroplast protein ("Chloroplastin"), enable the exchange reactions to run smoothly. (Gaffron & Wohl 1936: 81)[22]

This was the view that most photosynthesis researchers held; but the authors immediately went on to add: "In the following, we shall demonstrate that this theory is not tenable":

Even if the specific mechanism were indisputable, it could not explain certain fundamental findings of photosynthesis research. These findings can only be

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[20] On Gaffron's life and work, see, e.g., Homann (2003).

[21] See Gaffron & Wohl 1936: 81: "Aber erst aus Diskussionen in einem von Herrn M. Delbrück veranlassten Kolloquium ergab sich, dass man genötigt ist, diese Vorstellung [dass es der lebenden Zelle möglich ist, die Energiebeträge zeitlich und räumlich getrennter Absorptionsvorgänge zu sammeln und für einen Reduktionsprozess zu verwerten] einer Theorie der Assimilation zugrunde zu legen. Die folgenden Erörterungen haben ihren Ausgangspunkt in jenen Diskussionen."

[22] German original: "Die Kohlensäure wird am Magnesium des Chlorophyllmoleküls gebunden, ebenso wird an einer passenden Stelle Wasser angelagert. Innerhalb dieses aus aktivem Chlorophyll, Kohlensäure und Wasser bestehenden Molekülkomplexes wird die Kohlensäure mit Hilfe der im gleichen Molekül absorbierten Lichtenergie reduziert. Die Reduktion erfolgt in 4 Stufen, die im sukzessiven Austausch von Wasserstoff gegen Hydroxyl bestehen. Besondere chemische Eigenschaften des Chlorophylls, die durch seine Bindung im Chloroplasteneiweiss ("Chloroplastin") verstärkt werden, ermöglichen den glatten Ablauf der Austauschreaktionen.

understood if one abandons the assignment of one molecule of carbonic acid to one molecule of chlorophyll and, in accordance with the observations of the way the chlorophyll in vitro behaves, one does not allocate chlorophyll the role of photoenzyme but rather the role of sensitising agent. (Gaffron & Wohl 1936: 81)[23]

Gaffron and Wohl suggested that, in order to explain Emerson and Arnold's findings, one had to assume a process previously unheard of in photochemistry: a process in which thousands of chlorophyll molecules collaborated to bring about the photochemical reaction that eventually led to the release of molecular oxygen. The strongest argument proposed by Gaffron and Wohl was that, if this collaborative work were not the case, then, under conditions of low light intensity, photosynthesis would take a very long time to start. In fact, if the traditionally assumed reaction – that a complex of only one chlorophyll molecule reacted with one particular molecule of carbon dioxide – took place under the conditions used by, for example, Warburg and Negelein, one would have to wait about ten minutes before the first oxygen molecule escaped (Gaffron & Wohl 1936: 86). This clearly was not the case; hence, the traditional assumptions had to be inaccurate.

The idea of a "unit" consisting of many chlorophyll molecules and the distinction between "antenna molecules" and "reaction centres" are a matter of course today. However, in the 1930s not many scientists were ready to accept even the general sketch of Gaffron and Wohl's idea. The German physicist James Franck (1882–1964) was particularly opposed to this concept. Having emigrated from Nazi Germany and, finally, settled in Chicago, Franck had turned to photosynthesis studies, yet from the angle of physics (see Franck 1935 for his first theoretical contribution to the field and Franck & Rosenberg 1964 for his last).[24] Franck was convinced that photosynthesis worked in the same way as usual photochemical reactions, which he had studied earlier; and thus, he developed several alternative scenarios to the Gaffron–Wohl concept (see, e.g., Franck & Herzfeld 1937, Franck & Herzfeld 1941; see also, however, the collaboratively written review of the whole field, Franck & Gaffron 1941). Although most of his suggestions were flawed, and some of them were extremely short-lived, Franck did succeed in confronting photosynthesis researchers with state-of-the-art theoretical physics, which was all too frequently overlooked by the more biologically oriented scientists. Franck vigorously argued that photosynthesis and all other life processes eventually had to conform to the laws of physics; this was an extremely important reminder, especially as attempts were being made to elucidate the photochemical processes of photosynthesis.

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[23] German original: "Selbst wenn der spezielle Mechanismus einwandfrei wäre, könnte sie doch gewisse grundlegende Ergebnisse der Assimilationsforschung nicht erklären. Diese Ergebnisse sind nur verständlich, wenn man die Zuordnung von einem Kohlensäuremolekül zu einem bestimmten Chlorophyllmolekül aufgibt und, übereinstimmend mit den Beobachtungen des Chlorophylls in vitro, diesem bei der Assimilation nicht die Rolle eines Photofermentes, sondern die eines Sensibilisators zuweist."

[24] On Franck and his work in photosynthesis, see, e.g., Lemmerich (2007), Beyerchen (1996) and Rosenberg (2004).

## 8 General Microbiology and the Equation of Photosynthesis

At around the same time, people were also being reminded that photosynthesis had to conform to the general laws of chemistry. The seminal contribution in this direction came from the Dutch microbiologist Cornelis B. van Niel (1897–1985), who suggested that photosynthesis conformed to a general reaction pattern in all organisms.[25] The year 1935 can, perhaps, be considered the date of his major breakthrough, which was widely received (although van Niel had already developed the general concept by 1931; see van Niel 1930, 1931, 1935 for the first crucial papers, and van Niel 1941 for a comprehensive review).

Van Niel's argument can be broken down into three steps (Myers 1974: 421). First, van Niel insisted that bacteria also perform a kind of photosynthesis – albeit an anoxygenic one. This was not easy to swallow, since up to this point photosynthesis had been *defined* as a process that produced molecular oxygen. Van Niel, by contrast, clearly stated that in certain sulphur bacteria, carbon dioxide was also reduced to carbohydrates by a light-driven process, and that this should be considered "photosynthesis", even if no oxygen was emitted. Second, van Niel purported that bacterial processes should best be understood as a series of hydrogen transfers – this referred to a general principle of oxidation-reduction reactions that had first been put forward by the German chemist Heinrich Otto Wieland (1877–1957), but only later became a core interest in biochemistry thanks to van Niel's teacher in Utrecht, Albert Jan Kluyver (1888–1956).[26] The pertinent general reaction can be formulated as $H_2A + B \rightarrow A + BH_2$. Third, having brought his audience thus far, van Niel only needed to point out that photosynthesis in plants was closely analogous to this reaction, since both conformed to the general formula:

$$CO_2 + 2\ H_2A\ (+\ \text{light}) \rightarrow [CH_2O] + H_2O + 2\ A.$$

The (crucial) difference, of course, is the nature of the respective "A", the specific hydrogen donator for the final carbon dioxide reduction: in some bacteria, it is sulphur, while in plants, algae and cyanobacteria, it is oxygen.

The implications of this general conception of photosynthesis were enormous. Not only was this another forceful argument in favour of the unity of nature at the metabolic level, it also strongly pointed to the concept of a photolysis of water, and countered the view that carbon dioxide was the source of oxygen – just as Wurmser had suggested in 1921 (Wurmser 1921, 1930; see also Debru 1994 on Wurmser's work in photosynthesis).

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[25] On van Niel's life and work, see, e.g., Barker & Hungate (1990), Spath (1999) and Hungate (1986). For an autobiographical account, see van Niel (1967).

[26] On the controversy, in particular between Warburg and Wieland, see, e.g. Werner (1997) and the introduction to Werner (1996). On Kluyver's life and work, see, e.g., Woods (1957) and Kamp et al. (1959).

## 9 Isolated Chloroplasts and Water Splitting

The next crucial step in destroying the concept of photosynthesis as a decomposition of carbon dioxide came from the Cambridge (UK) biochemist Robert, known as Robin, Hill (1899–1991) – discoverer of the well-known Hill reaction.[27]

By very ingenious – albeit at the first glance rather primitive – means, Hill succeeded in preparing a suspension of isolated chloroplasts: he ground up leaves of, for example, *Stellaria media* or *Lamium album*, in a buffered sucrose solution (pH 7.9) and then filtered it through glass wool. (Note that this was before the ultracentrifuge had become a standard instrument in biological laboratories.) Hill wanted to find out under which conditions these chloroplasts were able to produce oxygen. So he implemented a very sensitive indicator, namely, haemoglobin, which is very easily converted into oxyhaemoglobin if small amounts of oxygen are present. Hill mixed his chloroplast suspension with a solution of haemoglobin, under the exclusion of air, and observed spectroscopically the conversion of haemoglobin into oxyhaemoglobin at high light intensities.

Yet Hill found that oxygen was only produced if an aqueous leaf extract was added to the suspension (see Hill 1937, 1939); although this came as no surprise, Hill took it to indicate that certain enzymes were involved in the process. However, in further developing these experiments, Hill observed, together with his collaborator Richard Scarisbrick, that a yeast extract could also promote the release of oxygen, and that the efficiency of the latter depended on its content of organic iron compounds. Finally, it transpired that oxygen evolution could even be triggered by simply adding Fe-III-ions (ferric salts), for example, in the form of ferric potassium oxalate (Hill & Scarisbrick 1940a, 1940b).

This reaction became later known as the Hill reaction – a term that was dubbed in 1941 by C. Stacy French (1907–95) and Mortimer Louis Anson (1901–68). After they had successfully repeated Hill's experiments – and even improved upon them – French and Anson prepared a paper to be presented at a conference; however, since neither of them was able to atttend the conference, their friend and colleague Jack Myers (1913–2006) read out the paper to the audience (French 1979: 10). As might be expected, it was not exactly a sweeping success. As Myers later recalled: "It [the paper] was greeted by a rather stony silence" (Myers 1974: 422). Some years would pass before the importance and accuracy of these findings would be realised.

Hill's major achievement was, first, that he succeeded in separating the photosynthetic production of molecular oxygen from the reduction of carbon dioxide to carbohydrates. Thus, he decisively demonstrated that these two parts of

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[27] A special issue of the journal *Photosynthesis Research* was dedicated in 1992 to the memory of Hill (*Photosynthesis Research*, vol. 34, 1992; see Rich 1992). See Bendall (1994) for a biographical account and Walker (2002) for a tribute to Hill's work on chloroplasts. Hill (1965) provides an autobiographical perspective.

photosynthesis occur in separate processes; as he and Scarisbrick put it in their extended paper of 1940: “It is concluded from the present observations that the light reaction in vegetable photosynthesis is the production of the oxygen molecule and is not the reduction of carbon dioxide” (Hill & Scarisbrick 1940a).

Furthermore, Hill’s experiments again strongly reinforced the belief that water splitting was occurring. If ferric ions were reduced and molecular oxygen released, it appeared entirely probable that the oxygen originated from the oxidation of water. Fully appreciating these implications, some photosynthesis researchers then dropped the traditional framework. As Gaffron wrote in an essay of 1969:

As late as 1936 Wohl and I were thinking about a hypothetical way to reduce a carbon dioxide compound directly à la Willstätter–Warburg. Only when Hill’s chloroplast reaction and later my photoreduction experiments made any other than van Niel’s view untenable was I ready to give in. (Gaffron 1969: 11)

## 10 The Carbon Dioxide Reduction Cycle

Finally, the key to elucidating the so-called “dark reactions” of photosynthesis came with the emergence of radioactive tracer molecules (of course, these reactions are only “dark” insofar as they are not photochemical; in actual darkness they quickly stop functioning). Two young chemists at the University of California at Berkeley – Samuel Ruben (1913–43) and Martin Kamen (1913–2002) – were the first to carry out experiments with radioactive carbon atoms, in order to trace the path of carbon in photosynthesis.[28] The site of their discoveries was the university’s famous Radiation Laboratory, headed by Ernest O. Lawrence (1901–58), which housed the even more famous first series of operating cyclotrons. Radioactive isotopes were a regular by-product of the cyclotron runs, and scientists working in very different fields of inquiry – medicine and biology being only two of them – were eager to make use of them.[29]

The first experiments on the carbon metabolism in photosynthesis were carried out in 1937 using the carbon isotope $^{11}C$. However, the short half-life of this isotope (which was just under twenty-one minutes) made life very difficult for the two chemists and their co-workers, the plant physiologist W. Zev Hassid (1899–1974) and the chemist Don DeVault (1915–90), since this hardly gave them enough time to carry out all the necessary operations. In his autobiography, Martin Kamen described the way typical experiments were carried out after he had retrieved suitable amounts of $^{11}CO_2$:

At the Rat House [their lab], Sam and Zev would be waiting for me like sprinters at the starting gate. Beakers would be filled with boiling water or other

[28] On the life and work of Ruben, see, e.g., Gest (2004) and Johnston (2003), Chapter 3; Kamen has left a number of autobiographical accounts: see Kamen (1963, 1985, 1986, 1989).

[29] On Lawrence, the Radiation Laboratory and the early history of the cyclotron, see, e.g., Heilbron & Seidel (1989) and Herken (2002).

solvents and pipettes ready to suck up measured volumes of radioactive solutions onto absorbent blotters, which would be held by tongs over hot plates and dried. All the necessary reagents and apparatus would be in place. The [Geiger] counter would be ticking away establishing the background activity. Each experiment had to be planned ahead in every detail so that no time was lost in confusion or delay in deciding what procedure to follow. Anyone looking in on the Rat House when an experiment was in progress would have had the impression of three madmen hopping about in an insane asylum, what with the frenzied activity punctuated by loud classical music from the radio monitor, and Sam's yells to get on with it and hand him samples while he sat at the counter table, feverishly taking background and sample counts. We had no idea of what had happened until hours later when, with all samples assayed, we sat in exhausted consultation, calculating and evaluating the results. (Kamen 1985: 86)

These demanding experiments went on for three years. The stress finally forced Hassid to stop working with the team; he suffered from high blood pressure and, after having collapsed several times in the laboratory, his physician advised him to stop this type of work. Thus, although Kamen and Ruben were able to produce their first data on the fate of carbon in photosynthesis with the isotope $^{11}C$ (see Ruben et al. 1939), there was a huge incentive to find a carbon isotope with a longer half-life; and in February 1940 Kamen and Ruben found $^{14}C$ (Ruben & Kamen 1940, 1941; see Kamen 1963 for his account of the discovery).

However, not only the carbon isotopes gave groundbreaking results: experimenting with radioactive oxygen isotopes in $H_2{}^{18}O$ finally provided evidence that the oxygen from photosynthesis really did originate from the water (although the data, of course, did not provide any information on the mechanism): the isotope composition of the oxygen produced during photosynthesis was similar to the oxygen isotope composition of oxygen in water but unlike the isotope composition of oxygen in carbon dioxide and atmospheric oxygen. This was found independently by a group working around Ruben and Kamen and another scientific team at roughly the same time, which gives an indication of how many people jumped at the opportunities provided by this new way of elucidating metabolic processes (see Ruben et al. 1941; Vinogradov & Teiss 1941, 1947).

However, 1941 was also the year in which the United States entered the Second World War. Other topics consequently appeared on the chemists' research agenda, which put on hold further studies with the promising new carbon isotope in photosynthesis. And once the war was over, it was not Ruben and Kamen who were able to continue their work with $^{14}C$: Ruben had died in 1943, in a tragic laboratory accident while experimenting with phosgene gas, while Kamen became a victim of the McCarthy "witch hunts" of 1950s America. Because of his alleged involvement with communists, he was fired from the Berkeley laboratory, and it took him about a decade to establish his innocence in court (see Kamen 1985).

Thus, instead of Kamen, it was the chemist Melvin Calvin (1911–97) who, in 1946, was made head of the Bio-Organic Chemistry Group at Berkeley, the task of which was to investigate the use of radioactive isotopes in chemical and biochemical studies. A subdivision of this group, headed by Andrew A. Benson (*1917) was to specialise in photosynthesis.[30] After ten years of intense work, their results were finally fully published in 1956, after a series of twenty-three publications, all of them entitled "The Path of Carbon in Photosynthesis" (see the bibliographic details in, e.g., Seaborg & Benson 1998: 18–20; among those Calvin & Benson 1948 and Calvin et al. 1950 are of particular relevance). However, in order to improve on Kamen and Ruben's less successful attempts, a suitable technique for detecting and identifying labelled intermediates had to be combined with the $^{14}C$ approach: the method of partition paper chromatography, developed in 1944 (Consden et al. 1944), became the principal analytical tool of the Calvin–Benson team.

It transpired that the photosynthetic carbon was reduced by means of a curiously cyclic path, which today is still associated with Calvin's name. Calvin was awarded the Nobel Prize in Chemistry in 1961 for this work; and there was little doubt in the scientific community that this discovery deserved the highest honour. The great impression that Calvin's presentations had on contemporary scientists is nicely described by Calvin's biographers Glenn T. Seaborg and Andrew A. Benson:

One evening at a meeting of the American Association for the Advancement of Science, his [Calvin's] novel theory of photosynthesis was so impressive that, when he finished, the great C. B. van Niel jumped up from his seat in [the] front row and, with tears in his eyes, congratulated Melvin for making the ultimate discovery of the mechanism of photosynthesis. It was a great theory, indeed. (Seaborg & Benson 1998: 7)

It should be emphasised, however, that Calvin did not work alone. Indeed, the photosynthetic carbon cycle should, in actual fact, be called the Calvin–Benson–Bassham cycle (as some scientists in the field do refer to it).[31] Although Calvin was the brilliant group leader, some of the major discoveries – including the discovery of the key enzyme that is today known as Rubisco – were made by his collaborators Andrew A. Benson and the latter's student, James A. Bassham. As R. Clinton Fuller (*1925) rightly remarked in his autobiographical account of the story, it is unfortunate that Benson's contributions, in particular, were not mentioned in Calvin's autobiography: there is no mention of his name, no citing of Benson's papers, no photograph of him, while many other scientists of less relevance to Calvin's field of expertise are prominently featured. As Fuller put it: "This appears to be an undeserved slight to a great scientist both personally and professionally who had contributed in a major way to all of Calvin's research and technology in the field of photosynthesis" (Fuller 1999: 10; cf. Calvin 1992).

[30] On Calvin's life and work, see, e.g., Loach (1997) and Seaborg & Benson (1998). See also Calvin (1989, 1992) for autobiographical accounts.

[31] For personal perspectives on this story by the two other major actors in the Calvin team, see Benson (2002a, 2002b) and Bassham (2003).

## 11 Red Drop Effect, Enhancement Effect and Two Photosystems

Thus, by 1956 the "dark reactions" of photosynthesis were comparatively well understood, thanks to the concerted action of a number of chemists, biologists and physicists. Understanding the "light reactions" to the same extent became the programme of the 1950s – a very productive period, which is sometimes referred to as the Golden Age of photosynthesis research. Of all the seminal discoveries made in this decade, I only wish to highlight one finding, which decisively contributed to the concept of two light reactions: the Enhancement Effect discovered by Robert Emerson – one of the fathers of the concept of the photosynthetic unit mentioned earlier.

In 1943, Emerson, together with Charleton M. Lewis (1905–96), a physicist, had found that, in the far red region of the spectrum, the maximum quantum yield of oxygen evolution in photosynthesis of the green alga *Chlorella* curiously declined, even though chlorophyll still absorbs well at these wavelengths (Emerson & Lewis 1943). The effect – known today as the Red Drop Effect – had been confirmed by other researchers in other organisms, but nobody knew how to explain this curious anomaly in *Chlorella*.

It was only much later, in 1957, that Emerson finally found, together with Ruth Chalmers and Carl Cederstrand (again, a physicist), that this red drop phenomenon did not occur if the algae were illuminated with a second beam of light of a slightly shorter wavelength at the same time, that is, if in addition to the far red light at, say, 700nm, a second light beam was given at, say, 650nm (Emerson 1957; Emerson et al. 1957). Furthermore, under these conditions the yield of oxygen was greater than the sum of the yields when only single beams were used. This observation would later come to be called the (Emerson) Enhancement Effect.

This was a most interesting phenomenon – if it really *was* a phenomenon and not an artifact, as some contemporary scientists suspected. Many potentially interfering factors had to be excluded; the effect might have been caused, for example, by respiration effects as a confounding factor. It took another five years of experimental work before the reality of the Enhancement Effect would be established (see e.g. R. Govindjee et al. 1960 for a demonstration of the effect's independence from respiration).

Notwithstanding these potential difficulties, Emerson and his co-authors proposed a possible explanation in 1957:

Since the supplementary light must be of shorter wave lengths than those which by themselves give diminished yield, the significance of the supplementary light may be that it adds excitation of other pigments besides chlorophyll *a*; and they surmised that, at least in green algae, photosynthesis required additional excitation of chlorophyll *b*. (Emerson et al. 1957: 142)

Yet at the same time, the authors acknowledged that this conjecture had its problems:

It is in conflict with the widely accepted view that transfer of excitation energy to chlorophyll a from other pigments takes place with practically 100 per cent efficiency. (Emerson et al. 1957: 142)

The transfer of excitation energy to chlorophyll *a* from other pigments had been demonstrated by the Dutch biophysicist Louis N. M. Duysens, who brought to photosynthesis research the important and highly influential technique of difference absorption spectroscopy (Duysens 1952). In a lecture, Eugene I. Rabinowitch (1901–73), another photosynthesis giant, is reported to have compared the importance of this technique for the study of photosynthesis with the ability to look under the hood of a car, in order to find out its mechanism (cf. Duysens 1989: 67f.). Thus, Emerson's own explanation of how the Enhancement Effect came about had obvious weaknesses and, in actual fact, did not last very long: one of Emerson's doctoral students, Govindjee, demonstrated, together with Rabinowitch, that the photosystem which Emerson thought was sensitised by chlorophyll *b* in fact contained chlorophyll *a* (see Govindjee & Rabinowitch 1960).

However, the general hypothesis – that two different pigment systems might contribute to photosynthesis – was exciting enough, and sparked off a lively discussion of how these two systems might function. This discussion was intensified through the work of the Dutch biophysicist Bessel Kok (1918–79):[32] in 1956, Kok had discovered, at a wavelength of 700nm, the so-called P700 – a specific type of chlorophyll *a*, which today is known as one of the "reaction centres" of photosynthesis (Kok 1956); and in 1959, Kok published his observations on the antagonistic effect of red and orange light on P700 in a cyanobacterium: although this pigment was oxidised by red light, this oxidation was reversed if it was subsequently illuminated with orange light (Kok 1959), which seemed to suggest that not only were there two pigment systems in photosynthesis, the effects of light on these systems also differed.

## 12 The Z-Scheme of Photosynthesis

Thus, the idea that photosynthesis comprised two light reactions was clearly in the air in the second half of the 1950s (Govindjee & Krogmann 2004: 32). The possible details were discussed in many places, publicly and privately – notably at the IXth International Botanical Congress in Montreal in 1959 (see Govindjee 2006 for details) and at the interdisciplinary symposium on Light and Life, held in March 1960 at The Johns Hopkins University in Baltimore (see the proceedings, McElroy & Glass 1961, in which not only presentations but also subsequent discussions are documented). Moreover, an early version of this concept – that of

[32] On Kok's life and work, see, e.g., Myers (1987).

cytochromes being oxidised by one system and reduced by another – had been proposed in 1956, in the final volume of Rabinowitch's seminal monograph on photosynthesis:

[Evidence] suggests photochemical transfer of electrons from reduced cytochrome to the organic acceptor (perhaps via DPN or TPN). The transfer of hydrogen (or electrons) from $H_2O$ to the oxidized cytochrome would then require another photochemical reaction. […] The quantum requirement of the hydrogen transfer reaction as a whole would be (at least) 8, since two quanta will be needed to transfer each of the four required H atoms (or electrons), first from water to the cytochrome, and then from the cytochrome to the final acceptor. (Rabinowitch 1956: 1862; previously quoted in, e.g., Govindjee 2006: 154).

However, as Rabinowitch's ingenious suggestion was buried on page 1862 of this monumental work, it went largely unnoticed; it would be dug up only years later by Govindjee and Duysens, two of Rabinowitch's collaborators, and made known (see, e.g., Govindjee 2006, Govindjee 1995 and many earlier talks; see also Duysens 1989). One could even read Rabinowitch's interpretation of Franck & Herzfeld (1941) as a prototype of a two light-reaction system (Rabinowitch 1945: p. 162, scheme 7.V; see Govindjee 2006: 154. Govindjee 1995:139 has a reproduction of this scheme); yet, nobody took up the theme and elaborated it.

Thus, it was the elegant paper by Robin Hill and Fay Bendall in 1960 that was received as the first elaborate vision of how the two light reactions functioned in series. A subsequent version of the diagram (prepared by Govindjee and Rajni Govindjee) explaining the two reactions is reproduced in Figure 4. It is immediately clear why it was called the "Z-scheme": the form of the letter Z is obvious. In later versions, though, which are more common today, the orientation of the axes was reversed (see, e.g., Walker 2002 for a description of the scheme's discovery).

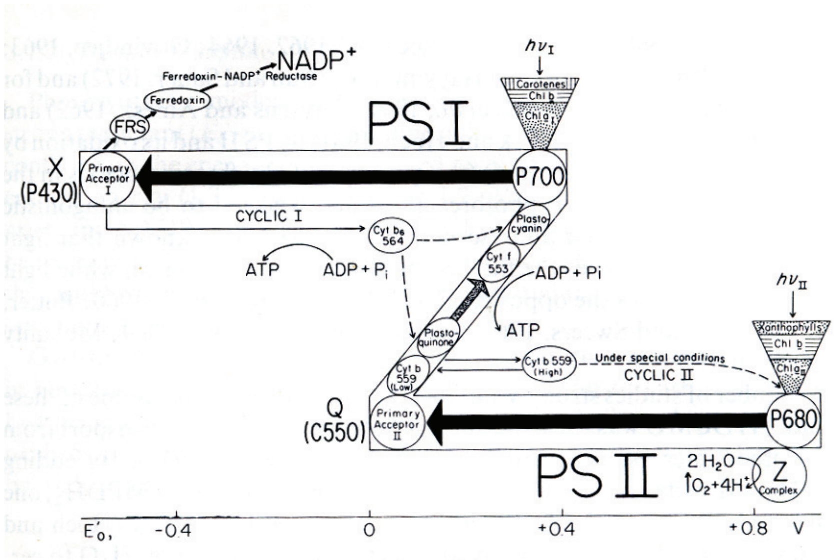


*Fig. 4 The Z-scheme, in a version by Govindjee & R. Govindjee 1975, p. 27.*

This scheme presented a synthesis of most of the available evidence on how the light reactions in photosynthesis work; and although it was later shown to be inaccurate in certain details, most of its elements were confirmed and elaborated in later studies. The scheme proposed how the two reactions were connected to each other, as part of a series, and also identified them with the two pigment systems. But the scheme was far more than a mere enumeration of a number of substances in line: it combined evidence from physical, chemical and biological research, taking into consideration the photochemical, thermodynamic and kinetic aspects of photosynthesis. It was almost immediately and universally accepted thanks to its great explanatory power: if the two pigment systems had different absorption spectra, then the Emerson Enhancement Effect could easily be explained, as well as the observations of Kok, Duysens and others. Indeed, at the 1960 Light and Life symposium, Kok had presented, together with George Hoch, an independent scheme that also included two photoreactions, albeit with only one pigment system, which did not stand the test of time (Kok & Hoch 1961).

The Hill–Bendall scheme also included the concept of photophosphorylation: definite suggestions were made as to the sites of ATP formation, which were later tested and refined. Photophosphorylation was discovered in 1954 by two groups working independently of each other: Daniel I. Arnon (1910–94) and collaborators found evidence of light-driven ATP formation in isolated chloroplasts (see Arnon et al. 1954a, 1954b); in the same year Albert W. Frenkel discovered the same phenomenon in membrane fragments of photosynthetic bacteria (see Frenkel 1954).[33]

The paper by Hill and Bendall was a theoretical one, a "working hypothesis", as they called it in the title. Decisive empirical evidence for the existence of two light reactions related to two pigment systems was actually provided by the experiments presented in Duysens et al. (1961), which dealt with the antagonistic effect of light, absorbed in pigment systems I and II, on the redox state of cytochrome *f*; although these experiments were completed before Hill and Bendall's publication of the Z-scheme, the two suggestions were compatible and were taken to corroborate each other (see Duysens 1989 for an account of this story). In addition to that, the flashing light experiments carried out by the biophysicist Horst T. Witt (1922-2007) and coworkers also provided quantitative support for a mechanism with two photochemical reaction centres in series; Witt and coworkers even succeeded in specifying the roles of some of the intermediates, which up to then had remained totally unclear (see Witt et al. 1961).[34]

At that time, at the latest, the Z-scheme became widely accepted as the most promising explanation of phenomena that had previously been not only inexplicable but had also seemed totally unconnected. The Z-scheme put an end to the endless groping in the dark: it marked a new beginning in the field of photosynthesis research. Thus, this first culmination of interdisciplinary thinking seems to be an appropriate point at which to end this account of selected landmarks in the history of photosynthesis research.

## 13 Conclusions

From leaves to molecules: this was chosen as the title of this paper in order to try and capture the transformation that took place in the objects of study in photosynthesis research from the middle of the nineteenth to the middle of the twentieth centuries. However, there are other dimensions to this change – one of them, obviously, was the development from rather crude and qualitative accounts of biological phenomena to the use of detailed, quantitative models. If one looks at the elements that initiated some of the most important changes, it also transpires that these quantitative models only became possible thanks to the enormous developments made in instrumentation: whereas in Sachs's time, scientists had to put up with a light microscope and a home-made gas chamber, Calvin, Benson and their co-workers were able to use radioactive tracer molecules and sophisticated chromatography techniques.

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[33] On Arnon, see, e.g., two special issues of the journal *Photosynthesis Research* dedicated to him (vol. 46, 1–2, 1995; see Melis & Buchanan (1995). On Frenkel, see, e.g., his autobiographical account in Frenkel (1993).

[34] See on Witt's life and work, e.g., his own autobiographical perspective Witt (1991), and the obituary in Nature by Junge & Rutherford (2007).

What also dramatically altered was the attitude to disciplinary boundaries. From a state of mutual disregard – sometimes even contempt – between the fields of physics, chemistry and biology, the climate changed to one of close collaboration. Photosynthesis laboratories in the middle of the twentieth century were already interdisciplinary sites of research; even the individuals as such were usually trained in the techniques of more than one discipline, thus fulfilling to a certain degree Pfeffer's vision of 1897.

At the same time, however, plant physiologists narrowed their field of expertise. Sachs still mastered the full range of plant physiology and made use of very different techniques; but Emerson, for example, perfected his skills in only one method, manometry, which he almost exclusively employed in his research studies on one single topic, namely, the photosynthesis of unicellular freshwater algae. However, Emerson always saw to it that he had competent collaborators, who were well versed in the field of photophysics, for example. Working in a team can, to some extent, make up for the highly specialised and thus limited expertise of the individual.

Although the enormous success of this new generation of plant physiologists cannot be disputed, it might still be appropriate to close this paper with a quotation from the recollections of the plant physiologist Pirson. Looking back, in 1994, on the changes that occurred in his field of research and expertise, Pirson strongly favoured an organismic reorientation of plant physiology in order to avoid the pitfalls of undertaking chemical research without adequate knowledge of the living system:

I believe that young biologists should not enter the field by first studying in vitro systems, as is often practiced in photosynthesis research. In a first step, they should "live together" with their organisms for some time (from unicellulars upwards), in order to become aware of their life manifestations, including the "reluctance" to imposed laboratory conditions. Biochemistry and molecular biology, with their analytical procedures, should follow in the next, although indispensable step. (Pirson 1994: 219)

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# The reception of the Schimper-Mereschkowsky endosymbiont hypothesis on the origin of plastids – between 1883 and 1960 – many negative, but a few relevant positive reactions[1]

*Rudolf Hagemann*

A survey is given about the reception of the Schimper-Mereschkowsky endosymbiont hypothesis on the origin of plastids in the time period between 1883 and 1960 in which the decision about the rightness of the hypothesis or its falsity was open. (Many opinions have been originally published in German; therefore the original German version is added in brackets to the English text.)
*The foundation of the endosymbiont hypothesis*: The idea was first expressed in a short footnote of two sentences by A.F.W. Schimper (Bonn) in 1883. It was worked out in detail by C. Mereschkowsky (Kasan, Russia) and published in German in 1905.
*Critical reactions*: The majority of established German, Russian and American professors were very sceptical and expressed their rejection and dislike. The American cytologist E. B. Wilson (New York) spoke of entertaining fantasy and flights of imagination and wrote that such speculations are too fantastic for present mention in polite biological society.
Also the German biologists P. Schürhoff (Berlin) and P. Buchner (Breslau, Leipzig) expressed their dislike; the botanist E. Küster (Gießen) spoke of a bizarre idea and the negative value of this hypothesis. Astonishing is the severe criticism from A. Famintzin (St. Petersburg), who himself was engaged in studies on symbiosis, but called the hypothesis of his young Russian colleague Mereschkowsky totally unfounded.

[1] This paper is the enlarged version of a talk read during the "10th International Colloquium on Endocytobiology and Symbiosis" of the International Society of Endocytobiology in Gmunden, Austria (September 10-13, 2007).

*Positive reactions*: Nevertheless several biologists expressed their interest in and their support of the endosymbiont hypothesis, thus showing 'a good nose' for new scientific developments. The Austrian botanist G. Haberlandt (Graz) published his sympathy with this hypothesis after a study of symbiontic algae in a flatworm and assumed that they may represent transitional states on the way to real chloroplasts. The young Viennese botanist L. Geitler studied two symbiontic associations of colourless chlorophyceae with blue-green algae and assumed that these associations may further develop into chloroplasts. The microbiologists A. Pascher (Prague) and J. Schiller (Vienna) as well as the plant physiologist V. Vouk (Zagreb) supported the endosymbiont hypothesis and emphasized its heuristic value.

A potential supporter of the endosymbiont hypothesis was R. B. Goldschmidt (Berlin, Berkeley) who declared the decision about proof or disproof at present (1955) open, but considered the possibility that it may turn out to be true.

The swing of opinion in favour of the endosymbiont hypothesis: The change in favour of the hypothesis began in the fifties with studies about the presence of DNA in organelles; but the results were at first controversial. A convincing proof of specific DNAs in chloroplasts and mitochondria was provided by H. Ris and W. Plaut who immediately referred to Mereschkowsky's hypothesis. Ris initiated the swing of opinion. Very soon many investigators and theoreticians followed. In an Appendix the question is discussed why the founders of the theory of plastid inheritance, Erwin Baur and Otto Renner, did not give their opinion on the endosymbiont hypothesis. This article goes back to the roots of the endosymbiont theory and will deal with the reception of the Schimper-Mereschkowsky endosymbiont hypothesis on the origin of plastids during the time in which the decision about the correctness of this hypothesis or its falseness was open. Many relevant opinions have been originally expressed in German language; therefore I add the original German version in brackets to the English text.

## 1 The foundation of the endosymbiont hypothesis

The idea of the endosymbiotic origin of plastids (chloroplasts) was first published by Andreas Franz Wilhelm Schimper (1856-1901) in his paper of 1883 (Fig. 1) with the following two sentences:

"Should it turn out definitely, that the plastids are not formed de novo in the egg cells, then their relation to the organism, which contains them, would reasonably remind one of a symbiosis. Possibly the green plants have their origin in the unification of a colourless organism with another organism which is evenly coloured green by chlorophyll."[2] (Schimper 1883, Fn, pp. 111-112).

[2] „Sollte es sich definitiv bestätigen, dass die Plastiden in den Eizellen nicht neu gebildet werden, so würde ihre Beziehung zu dem sie enthaltenden Organismus einigermaßen an eine Symbiose erinnern. Möglicherweise verdanken die grünen Pflanzen wirklich einer Vereinigung eines farblosen Organismus mit einem von Chlorophyll gleichmäßig tingierten ihren Ursprung."

*Fig. 1 Andreas Franz Wilhelm Schimper (1856-1901) (from the private archive of the author).*

The way how this idea was published – just as a footnote of two sentences – gives the impression that Schimper just wanted to present the idea to his fellow botanists as a "trial balloon" ('Versuchsballon') and then wait and see their reactions. This impression is supported by the fact, that in his very comprehensive paper on plastids, 248 pages long, and published only two years later (1885) "Investigations on the chlorophyll bodies and their homologous entities" („Untersuchungen über die Chlorophyllkörner und die ihnen homologen Gebilde"). Schimper did not even give it a mention.

In contrast, Constantin S. Mereschkowsky (1855-1921) published in 1905 his hypothesis on the endosymbiotic origin of chloroplasts from free-living Cyanophyceae in great detail (Fig.2). His paper was published in German language: C. Mereschkowsky: „Über die Natur und Ursprung der Chromatophoren im Pflanzenreiche" ("On the nature and origin of chromatophores in the plant kingdom"; Biologisches Centralblatt 25: 593-604, 689-691, 1905).

*Fig. 2 Constantin S. Mereschkowsky (1855-1921) (from the private archive of the author).*

His main arguments were:

(1) The chloroplasts have continuity: they multiply only by division.

(2) The chloroplasts are (in their structure) independent of the nucleus.

(3) There is an analogy between chloroplasts and zoochlorellae.

(4) Cyanophyceae are free-living organisms which have great similarity to chloroplasts; they can be considered free-living chloroplasts.

Thus, in my opinion, Mereschkowsky has to be considered the real founder of the endosymbiont hypothesis. The reception of this endosymbiont hypothesis in the scientific community (botanists and zoologists) varied greatly.

## 2 Critical reactions

The majority of the established (German, Russian and American) professors, who mostly were conservative in their way of thinking and also in their scientific views, had great difficulties to accept the ideas and speculations of Mereschkowsky. **Edmund Beecher Wilson** (1856-1939) put it – in his well-known textbook "The Cell in Development and Heredity" (1925) – in the following words:

"Mereschkowsky (1905), in an entertaining fantasy, has developed the hypothesis that the dualism of the cell…resulted from a symbiotic association… In further flights of the imagination Mereschkowsky suggests the origin ...of the green plants by a symbiotic union of between colorless nucleated cells and minute Cyanophyceae, the latter giving rise to the chloroplasts." (Wilson 1925, p. 738)

At the end of this textbook chapter Wilson also criticizes the idea of Wallin (1923, 1925) that chondriosomes (= mitochondria) may be regarded as symbiotic bacteria, and states (with regard to Mereschkowsky and Wallin): "To many, no doubt, such speculations may appear too fantastic for present mention in polite biological society" (Wilson 1925, p. 739).

Frankly speaking, these reactions cannot entirely be dismissed. On the one hand Mereschkowsky put clearly forward his scientific arguments. They have been cited above. On the other hand Mereschkowsky used similes and pictures, which were normally not found in scientific botanical literature:

"Let us imagine a palm tree, growing peacefully near a spring, and a lion, hiding in the brush nearby, all of its muscles taut, with bloodthirsty eyes, prepared to jump upon an antelope and to strangle it. The symbiotic theory, and it alone, lays bare the deepest mysteries of this scene, unravels and illuminates the fundamental principle that could bring forth two such utterly different entities as a palm tree and a lion. The palm behaves so peacefully, so passively, because it is a symbiosis, because it contains a plethora of little workers, green slaves (chromatophores) that work for it and nourish it. The lion must nourish itself.

Let us imagine each cell of the lion filled with chromatophores, and I have no doubt that it would immediately lie down peacefully next to the palm, feeling full, or needing at most some water with mineral salts."[3] (1905, English translation by Martin and Kowallik 1999, pp. 292-293).

[3] „Denken wir uns eine Palme ruhig am Ufer einer Quelle wachsend und einen Löwen, der neben ihr im Gebüsch verborgen liegt, alle seine Muskeln angestrengt, mit Blutgier in den Augen, fertig auf eine Antilope zu springen und sie zu erwürgen. Nur die Symbiosetheorie gestattet es, bis ins tiefste Geheimnis dieses Bildes einzudringen und die fundamentale Ursache, die zwei so ungeheuer verschiedene Erscheinungen, wie eine Palme und einen Löwen hervorbringen konnten, zu erraten und zu verstehen. Die Palme benimmt sich so ruhig, so passiv, weil sie eine Symbiose ist, weil sie eine Unzahl von kleinen Arbeitern, grünen Sklaven (Chromatophoren) enthält, die für sie arbeiten und sie ernähren. Der Löwe hat sich selbst zu ernähren. Denken wir uns jede Zelle des Löwen von Chromatophoren gefüllt, und ich zweifle nicht, dass er sich sofort neben die Palme ruhig hinlegen würde, sich satt fühlend oder höchstens noch etwas Wasser mit mineralischen Salzen bedürfend." (1905, p. 604).

Understandably, such statements appeared 'too fantastic for present mention in polite biological society' of established professors. In general, the critical comments on the Mereschkowsky-Schimper hypothesis are expressed in a rather aggressive tone. Critical views were expressed by **Paul Schürhoff** (1878-1939) in his book "Die Plastiden" (The Plastids), 1924.

Distinctly critical were also the statements by **Paul Buchner** (1886-1978), a highly esteemed Professor of Zoology at the Universities of Breslau and Leipzig and specialist of symbiosis. In his book "Tier und Pflanze in Symbiose" (Animals and Plants in Symbiosis) Buchner (1930) criticized Schimper and Mereschkowsky (chloroplasts originated from cyanophyceae) as well as Portier and Wallin (mitochondria originated from bacteria) and speaks of an "airy building of ideas" (p. 811). He also criticized followers of the endosymbiont hypothesis. For example, he wrote about Professor Friedrich Meves as follows:

"Even F. Meves, a scientist who always keeps a level head, takes up the standpoint of Altmann, which he had previously rejected, and states that it seems possible that mitochondria are intensely adapted symbiotic bacteria."[4] (Buchner 1930, p. 810).

About all the endosymbiontic ideas (regarding chloroplasts and mitochondria) Buchner stated:

"But until now we cannot anywhere see serious evidence for such far-reaching concepts. We are much more modest in the valuation of the principles of symbiosis."[5] (p. 817)

The gruffiest statements against the endosymbiont hypothesis came from the Professors of Botany Ernst Küster (Gießen) and Andrej Famintzin (St. Petersburg). In his book "Die Pflanzenzelle", (in all 3 editions of 'The Plant Cell': 1935, 1951, 1956) **Ernst Küster** (1874-1953) has written:

"The idea to explain the typical plant cell as a symbiosis of colourless cells and coloured invaders comes from Schimper; it was in detail presented by Mereschkowsky (1905) who believed in the invasion of cyanophyceae (into colourless cells) without being able to make this probable… Another theory of symbiosis declares mitochondria to descend from bacteria; Portier (1918), Wallin (1927) and others have held this bizarre idea. Buchner (1921, 1930) has said all what is necessary about the negative value of this theory."[6] (Küster 1951, S. 782).

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[4] „Selbst ein so kühler Forscher, wie es Meves war, greift den anfänglich von ihm abgelehnten Standpunkt Altmanns wieder auf und gesteht, dass er es für sehr wohl möglich halte, dass die Mitochondrien weitangepaßte symbiontische Bakterien seien (1918)"

[5] „Aber wir können bis jetzt nirgends ernsthafte Beweise für eine so weittragende Vorstellung sehen und sind in der Bewertung des Symbiosenprinzips wesentlich bescheidener."

[6] („Der Gedanke, die typische Pflanzenzelle als eine Symbiose von farblosen Zellen und farbigen Einwanderern zu erklären, stammt von Schimper; ausführlich behandelt hat ihn Mereschkowsky (1905), der an eine Einwanderung von Zyanophyzeen glaubte, ohne sie irgendwie wahrscheinlich machen zu können…Eine andere Symbiosetheorie erklärt die Mitochondrien für Bakterien.; Portier (1918), Wallin (1927) u. a. haben diesen abwegigen (! Hg)) Gedanken verfochten, Buchner (1921; 1930) hat über den Unwert (! Hg) der Theorie das Nötige gesagt."

Most astonishing is the severe criticism, which came from **Andrej S. Famintzin** (1835-1918), a well established Professor of Plant Physiology at the University of St. Petersburg. He was intensely engaged in studies on symbiosis, but he sharply criticized his younger Russian colleague (from the University at Kasan in the provinces):

"The identification of cyanophyceae with chromatophores is totally unfounded, just as the claim of the author, that the chromatophores are cyanophyceae which had invaded the cytoplasm."[7] (Famintzin 1907, p. 361).

Many more established professors agreed with this opinion and accepted it; but they did not themselves publish about this hypothesis.

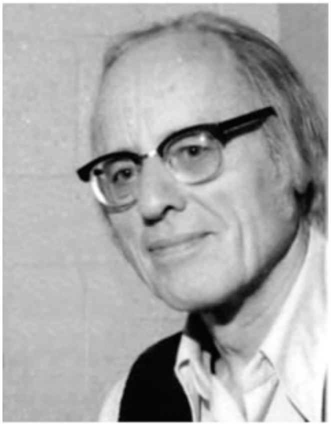

*Fig. 3 Hans Ris (1914-2004) (from the private archive of the author).*

First signs of a swing of opinion:

Today we all know that the essence of the endosymbiont hypothesis of Mereschkowsky and Schimper proved to be true. The change in favour of this hypothesis began in the middle of the fifties with studies about the presence of DNA in plastids; but the results were partly positive and partly negative.

A convincing proof of specific DNAs in chloroplasts and mitochondria was provided by the electron microscopic studies of Hans Ris (Fig. 3) and Walter Plaut (Ris 1961, Ris and Plaut 1962). **Hans Ris** (1914-2004) already referred to the hypothesis of Mereschkowsky in both papers (1961, 1962).

Very soon many other investigators followed: Researchers (Sager and Ishida 1963, Chun et al. 1963, Gibor and Izawa 1963) as well as theoreticians (Sagan – Margulis – 1967, Margulis 1970 ff) (review: Kirk and Tilney-Bassett 1967).

## 3 **Positive reactions**

Today it seems appropriate to mention those scientists who supported the hypothesis of Schimper and Mereschkowsky in the early days (between 1890 and 1960) – in other words: those who had a good nose for new scientific developments.

[7] „Die Identifizierung der Cyanophyceae mit Chromatophoren ist rein aus der Luft gegriffen (! Hg), wie auch die weitere Behauptung des Autors, dass die Chromatophoren ins Plasma eingedrungene Cyanophyceen sind."

Looking at the critics and at the supporters a curious geographical aspect makes itself felt: The critics of the endosymbiont hypothesis for the origin of chloroplasts lived in Germany, in pre-revolutionary Russia and in the United States. The supporters were, to a great part, citizens of the old Austro-Hungarian monarchy and Austria respectively.

*Fig. 4 Gottlieb Haberlandt (1854-1945) (from the private archive of the author).*

Is it possible that in Austria-Hungary – somewhat isolated from the critical and influential science centres in Germany – new scientific ideas developed more freely?

One of the first supporters of the endosymbiont hypothesis of the origin of chloroplasts was Professor **Gottlieb Haberlandt** (1854-1945), at that time professor of botany at the University of Graz in Austria (Fig. 4). Later on, in 1910, Haberlandt became full professor of botany at the Friedrichs-University in Berlin and became famous as the founder of the discipline "physiological plant anatomy", a field which brought him great national and international esteem during the following decades.

In Graz, Gottlieb Haberlandt studied in cooperation with Ludwig von Graf, Professor of Zoology at the University of Graz, the flat worm Convoluta roscoffensis (a plathelminth; Turbellaria). This flat worm is living in close symbiosis with green algae. The results of these investigations were published in the book: Ludwig von Graf: "Die Organisation der Turbellaria acoela" with an addendum by Gottlieb Haberlandt: "Über den Bau und die Bedeutung der Chlorophyllzellen von Convoluta Roscoffensis". ("On the structure and the significance of the chlorophyll cells of Convoluta Roscoffensis").

In this publication of 1891 Haberlandt described these algae-like symbionts and referrred to the hypothesis voiced by Schimper (1883). He expressed his sympathy with Schimper's idea and stated his opinion on that these symbiontic algal cells (living in this flat worm) may represent a transitional state on the way from free living algae to adapted symbionts and further, to real chloroplasts:

"I wish to further elaborate on my standpoint regarding this question by alluding to the hypothesis on the phylogenetic origin of the chlorophyll grains, put forward by Schimper…Let us assume that the chlorophyll grains of higher green plants indeed originate from unicellular algae. We would then be confronted with the highly strange case of a uniform organism owing its existence to the unification of two very different organisms, and that one organism was transformed into an organ of the other. This would be the highest level of symbiosis, to which, of course, many transitional steps must lead. From the former alga, only the chloroplasts would eventually remain, thus reaching the stage, nowadays, represented by the green plants."[8] (1891, pp. 82-83)

Interestingly, the English zoologist and anatomist **E. Ray Lankester** (1847-1929), Oxford, published in "Nature" (Vol. 44, 1891) a long review about the book of L. von Graf and G. Haberlandt, cited above. He considered Haberlandt's article as the "chief matter of interest" and devoted most of this review to the content of this contribution:

*Fig. 5 Lothar Geitler (1899-1990) (from the private archive of the author).*

"Haberlandt is inclined to place his theory as to the green cells of Convoluta alongside the suggestion of Schimper as to the origin of the chlorophyll corpuscles (= chloroplasts, Hg) of higher plants – namely, that these are due to the union in the remote past of a green-coloured with a colourless organism. In this case and in that of Convoluta the highest phase of symbiotic association is attained, for the green organism can no longer be separated and cultivated apart…We can well suppose it possible that the green cells of Convoluta might proceed further in their modification, so as to lose the colourless protoplasm and the cell-nucleus; they would then become simple chlorophyll corpuscles (= chloroplasts, Hg) like those of higher green plants." (1891, p. 465)

In this way the English scientific public opinion was informed about the ideas of Schimper and Haberlandt already in 1891.

Some decades later the young Austrian botanist **Lothar Geitler** (1899-1990) expressed similar ideas. He later became a highly esteemed professor of botany at the University of Vienna and a world-known specialist for Cyanophyceae and cytology (Fig. 5). The young Geitler (24 years old) was obviously open for new ideas. In **1923** he reported in one of his first papers on: "The structure of the cells of Glaucocystis Nostochinearum and Gloeochaete Wittrockiana and the symbiontic theory of chromatophores of Mereschkowsky". („Der Zellbau von

[8] „Ich möchte meinen Standpunkt in dieser Frage noch durch den Hinweis auf eine von Schimper hinsichtlich der Abstammung der Chlorophyllkörner geäußerte Vermutung näher beleuchten…Nehmen wir nun an, …dass die Chlorophyllkörner der höher entwickelten grünen Pflanzen tatsächlich von einzelligen Algen abstammen…Es läge dann einfach der allerdings höchst merkwürdige Fall vor, dass ein einheitlicher Organismus der Vereinigung zweier ganz verschiedenartiger Organismen seine Entstehung verdankt hat, wobei der eine von ihnen zu einem Organ des anderen geworden ist. Es wäre dies die höchste Stufe der Symbiose, zu welcher natürlich zahlreiche Übergangsstufen führen müssten…Von den einstigen Algen werden dann schließlich nur mehr die Chloroplasten übrig bleiben, und damit wäre dann jener Zustand erreicht, wie ihn gegenwärtig die grünen Pflanzen repräsentieren.“

Glaucocystis Nostochinearum und Gloeochaete Wittrockiana und die Chromatophoren-Symbiosetheorie von Mereschkowsky." Arch. Protistenkunde 47, 1923).

Geitler's investigations of the two species Glaucocystis Nostochinearun and Gloeochaete Wittrockiana led to the result that these two organisms are really *symbiontic associations*; they represent a symbiosis between a colourless species of Chlorophyceae and blue–green algae (Cyanophyceae) which fulfil the functions of chloroplasts. Already in the title of his paper Geitler pointed to the hypothesis of Mereschkowsky, that during evolution, free living cyanophyceae engaged in a symbiosis with a colourless cell and gradually developed into chloroplasts.

After a detailed microscopic analysis of both organisms he concluded:

"Although Mereschkowsky cannot prove his assumption that all chromatophores of plants are symbionts, it is well possible that in some cases such a development has really taken place. According to my studies with Glaucocystis and Gloeochaete, **I consider this certain for these organisms**."[9] (Geitler, 1923, S. 21).

*Fig. 6 Adolf Pascher (1881-1945) (from the private archive of the author).*

A third supporter of Mereschkowsky's ideas was **Adolf Pascher** (1881-1945), professor of botany at the German University of Prague, and specialist for algae (Fig. 6). His series of books on the 'Freshwater Flora' made him highly respected.

In his paper of 1929, "Studies on symbioses. I. Some endosymbioses of blue-green algae in protists" („ Studien über Symbiosen. I. Über einige Endosymbiosen von Blaualgen in Einzellern", 1929), Pascher reports new examples of 'endocyanoses'. In detail he describes the organism Peliaina which is living in symbiosis with a blue-green alga.

Summarizing his opinion, he stated:

"The principal correctness of the theory of (Famintzin and) Mereschkowsky is proven – as also Geitler has emphasized. But the universal validity of this hypothesis is not established…The theory describes only one of several possibilities. The new findings about endocyanoses do not only deepen the theory; they also broaden it, because the analysis of the monad Peliaina demonstrated a type of symbiosis which so far is not yet fully balanced. Such endosymbiontic blue-green

[9] „Ist nun Mereschkowsky auch nicht imstande, die Annnahme, daß alle Chromatophoren der Pflanzen als Symbionten aufzufassen seien, zu beweisen, so ist noch immer die Möglichkeit vorhanden, dass in einigen Fällen dieser Entwicklungsgang wirklich eingeschlagen worden ist. Nach meinen Untersuchungen an Glaucocystis und Gloeochaete halte ich dies für sicher."

algae are able to unite to entities, which totally resemble the cellular chromatophores of other organisms."[10] (1929, p. 458).

Regarding the mentioning of Famintzin together with Mereschkowsky, Pascher was wrong in thinking that Famintzin supported the hypothesis on the phylogenetic relationship of cyanophyceae with chloroplasts; as mentioned above, Famintzin was in fact against this idea of Mereschkowsky.

*Fig. 7 Josef Schiller (1877-1960) (from the private archive of the author).*

The ideas and conclusions of the endosymbiont hypothesis for the origin of chloroplasts were supported by the hydrobiologist **Josef Schiller** (1877-1960)**,** Professor at the University of Vienna (Fig. 7). In his paper of 1954 he referred especially to Pascher's results and concluded: "The cyanoses are beautiful examples for a possibility, how the evolution of chromatophores may have happened."[11] (1954, p. 125).

Höxtermann (1998) has pointed to the fact that the plant physiologist **Valentin Vouk** (1886-1962) from the University of Zagreb, as early as **1914**, also expressed his support of the hypothesis of Mereschkowsky and emphasized "the great heuristic value" of this concept.

A further supporter of the endosymbiont hypothesis was the well-known geneticist Professor **Richard B. Goldschmidt** (1878-1958). Goldschmidt has been a scientific member of the "Kaiser-Wilhelm-Institut für Biologie" in Berlin (1914-1935) and its deputy director from 1919-1935 (Fig. 8). He was forced to emigrate from Germany to the United States in 1935, because he was Jewish. In 1936 he became Professor of Zoology at the University of Berkeley. After his emigration he often expressed non-conformist ideas, which were different from the mainstream of genetic ideas.

In his book "Theoretical Genetics" he wrote in 1955: "It is well known that a number of animal species…have returned to purely or partial autotrophic metabolism by incorporating in their cells symbiontic unicellular algae. This suggests the possibility…that the plastids may be considered as symbionts of the plant cell, that is, dependent organisms like parasites and viruses. Many biologists

[10] „Die prinzipielle Richtigkeit der (Famintzin-)Mereschkowsky'schen Theorie hat, wie auch Geitler betont, als erwiesen zu gelten…Die Allgemeingültigkeit dieser Hypothese ist nicht erwiesen…Durch die Theorie wird eben nur eine der bestehenden Möglichkeiten erfaßt. (S. 455)…Die …Theorie…wird durch die neuen Befunde von Endocyanosen nicht nur vertieft: dadurch, dass mit dem Monadencyanom Peliaina eine Symbiose bekannt wurde, die noch nicht völlig ausgeglichen erscheint.., sondern auch erweitert: solche endosymbiontisch lebende Blaualgen können sich in ihrer Gesamtheit tatsächlich zu Gebilden zusammen-schließen, die völlig den zelleigenen Chromatophoren anderer Organismen gleichen"

[11] „Die Cyanosen sind also schöne Beispiele für eine Möglichkeit, wie Chromatophorenbildung vor sich gegangen sein kann."

*Fig. 8 Richard B. Goldschmidt (1878-1958) (from the private archive of the author)*

are favourable to such a view which, however, can neither be proved nor disproved at present." But he takes into consideration "the possibility that it may be true" (1955, p.229*)*.

Finally the statements of **Bruno Schussnig** (1892-1976)**,** Professor of Botany at the Friedrich-Schiller**-**University in Jena**,** should be mentioned. In his book "Handbuch der Protophytenkunde", Volume 2 (1960) he just mentions Mereschkowsky's idea, without really supporting it:

"The entirety of such cyanelles forms a system which physiologically equals the plastidom of another species. Also the transmission of the photosynthetic partner to the daughter cells of the host raises the impression that, by taking a detour via symbiosis, a stable autotrophic system has arisen. The unification of two organisms of such different taxonomic and physiological values also was the basis of (Famintzin and) Mereschkowsky's hypothesis on the origin of chromatophores, which assumes that all recent plastids originate from former coloured symbionts."[12] (1960, S. 214)

Very interesting are the statements in the book „Symbionticism and the Origin of Species" (1927) by **Ivan E. Wallin** (1883-1969), Professor of Anatomy in the University of Colorado, Denver. Wallin is well-known as the founder – together with P. Portier (1918) – of the concept that the mitochondria originated from symbiotic bacteria. In his book Wallin described in detail this hypothesis.

Remarkable are his statements about the hypothesis of Mereschkowsky:

"Merejkovsky (he writes the name in this way, Hg) has recently advanced the hypothesis that the chloroplast is a microsymbiont, genetically related to the blue-green algae, and that all the higher green plants are symbiontic complexes. He has emphasized the morphologic and physiologic similarity of chloroplasts to the Cyanophyceae…In lowly forms like algae, it is possible for the plastids to divide and multiply as such; the transmission of microsymbionts in algae from one generation to another is a comparatively simple matter." (1927, p.111)

But then Wallin was misled by his trust in the idea of Guilliermond (1921) that plastids descend in ontogenetic development from mitochondria:

[12] „Die Gesamtheit solcher Cyanellen macht ein System aus, welches physiologisch einem artfremden Plastidom gleichzusetzen ist. Auch die Übertragung des photosynthetischen Partners auf die Tochterzellen des Wirtes erweckt den Eindruck, dass auf dem Umweg über die Symbiose ein stabiles ‚autotrophes' System entstanden ist. Die Vereinigung zweier Organismen von so verschiedener systematischer und physiologischer Dignität war auch der Ausgangspunkt für die Famintzin-Mereschkowskysche Hypothese von der Chromatophorenherkunft, nach welcher alle rezenten Plastiden von früheren gefärbten Symbionten herstammen sollten."

"The origin of the chloroplasts in algae and higher plants has interested botanists for many years. Schimper (1885) first suggested the continuity of chloroplasts in development. He believed that chloroplasts arise from pre-existing chloroplasts by a process of division. Guilliermond (1921) and others have shown that chloroplasts originate from mitochondria in ontogeny by a process of transformation. The various stages of transformation have been so clearly illustrated by Guilliermond that very little doubt exists in regards to the accuracy of his observations." (Wallin, 1927, p. 108)

"We cannot agree with his (Merschkowsky's, Hg) rejection of the association of mitochondria in ontogenetic chloroplast formation. The careful researches of Guilliermond and others, appear to have definitely established this relationship. Merejkovsky perhaps has been misled" (1927, p.111).

It is almost sad to state that – on the contrary – Wallin has been misled by the wrong ideas of Guilliermond which obviously were of great influence on cytologists in the twenties and thirties of the twentieth century.

Summarizing this section, one can state that some meritorious and well-known scientists realized the future evolutionary importance of Mereschkowsky's concept. Thus it can be stated that from Schimper's and Mereschkowsky's publications a continuous line of supporters can be seen – also in the decades of doubts and uncertainties – which led to the time period (starting in 1961) in which more and more evidence was elaborated in support of the hypothesis of the endosymbiont origin of chloroplasts.

(In parallel, in principle the same development took place regarding the idea of the endosymbiont origin of mitochondria.)

## 4 Prospects of further information and future development

Many more details on the development of the research in the field of symbiosis and on the endosymbiont hypotheses for chloroplasts and mitochondria are published in the books:

Geus, Höxtermann and Müller: „Bakterienlicht und Wurzelpilz" (1998) and Geus and Höxtermann: „Evolution durch Kooperation und Integration" (2007); the latter publication contains among many articles also the reprints of several of the first relevant publications by Mereschkowsky and Famintzin.

The discussions and intellectual struggles in the course of the gradual assertion of the endosymbiont hypothesis for the origin of chloroplasts and mitochondria, which took place in the sixties and seventies of the 20th century, are beyond the scope of this article.

The development of these discussions has been outlined in many books and articles; only a small selection of them – which partially may be chosen by chance – is cited in a separate list of references following the 'normal' references.

## Appendix: What was the position of Erwin Baur and Otto Renner to the endosymbiont hypothesis on the phylogenetic origin of chloroplasts ?

Finally, an aspect will shortly be discussed which is of interest to all geneticists working in the field of plastid genetics. The theory of plastid genetics (or chloroplast genetics) was founded by Erwin Baur (1909) and supported and further developed by Otto Renner (1922, 1934, 1936; comp. Hagemann (2002).

What was the position of Baur and Renner regarding the hypothesis of the endosymbiont origin of plastids (chloroplasts)?

The answer is: Neither Baur nor Renner published any comment on this hypothesis. At first sight, this seems astonishing, but only at the first moment. I am sure that at least Otto Renner who was very much interested in the history of biology, especially botany and genetics, was certainly informed about this hypothesis and the discussions about it. Nevertheless he did not publish any comment about it.

My explanation for this behaviour is as follows:

Renner certainly knew that many professor of biology were rather conservative in their general opinion about unproved speculations. As Wilson (1925) stated: To many, no doubt, such speculations may appear too fantastic to be mentioned at present in reputable biological societies!

In the twenties and thirties of the 20th century, Baur and Renner had to fight an intense struggle for the acceptance of plastid inheritance against two groups of opponents:

(1) One group did only accept the *chromosome theory* in its strict sense, i.e. the theory that the nucleus is the *sole* carrier of hereditary factors.

(2) The other camp of scientists took the view that the cytoplasm (or protoplasm) also contains hereditary factors: The so-called 'plasmon theory' (by Fritz von Wettstein 1927, 1928) was accepted by many biologists. These scientists considered the plastids as part of the cytoplasm and wished to lump together all extranuclear genetic phenomena under the term 'plasmon'.

In contrast, Renner (1929) advocated the theory of plastid inheritance and kept plastids and cytoplasm clearly separate: The hereditary factors of the plastids form the plastome, which is different from the cytoplasmon and has a distinct mode of inheritance.

I hold the view that Renner – and Baur as well – did not wish to contaminate their efforts for the acceptance of the theory of plastid inheritance with the speculative and partially "flowery arguments" of Mereschkowsky for the endosymbiont hypothesis of the evolution of plastids. The unnecessary mixing of these two aspects might have jeopardized the approval of their theory of plastid inheritance.

According to my impression: In the twenties and thirties of the last century this was an acceptable strategy in the fight for the general acceptance of the theory of plastid inheritance. And this may explain their reservation.

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# Science studies and nature of Mendel's paradigm[1]

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*What we need is an inductive history in which all things, which have ever been exactly observed and proved beyond the possibility of mistakes to be true, are faithfully collected and set before our eyes; so that by an adequate examination of each one and the comparison of one with another, the Universal Laws themselves of Nature may be brought within our knowledge.*

J. A. Comenius to members of Royal Society of London in 1668[2]

**Abstract.** In 2000 Steve Fuller attempted to derive a philosophical history for our times, based on the book by Thomas Kuhn (1922-1996), Structure of Scientific Revolutions published in 1962. Within this context he stated that scientific discovery is not recognized as such unless it can be justified as issuing from the history of relevant science. As the standard case in point Fuller presented Gregor Mendel (1822-1884), whose pea-breeding experiments were neglected for forty years. In 1900 he was 'rediscovered as the father of modern genetics'. Kuhn found it most striking that natural scientists usually could agree on how to evaluate a piece of research, typically by reference to an earlier exemplar

[1] Dedicated to MUDr Vladimír Zapletal (1900 - 1983), pioneer historian of medicine in Brno, who drew my attention to the extraordinary worth of the book, Via Lucis, by Jan Amos Comenius (1592-1670, dedicated to the newly found Royal Society of London).

[2] Comenius, J.A. (1668) Via Lucis, Vestigata et vestiganda, Amsterodami. English translation: The Way of Light, published in 1938 by The University Press Liverpool and Hodder and Stoughton, London. pp. 152-3.

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that the research was said to resemble. From this insight grew his protean term, paradigm, which he deemed necessary for the conduct of 'normal science'. Paradigm acquisition, the model upon which scientific training is based, requires a commitment so deep that scientists cannot readily change their research orientation afterward in the face of mounting anomalies that resist paradigmatic treatment. Kuhn's Structure helped to level hierarchies of scientific disciplines and overturn inappropriate methodological standards, thereby contributing to the climate of pluralism that continues to flourish in most systems of higher education. Historians and sociologists of science have turned structure into the new research program known as the sociology of scientific knowledge and the interdisciplinary field of science and technology studies, science studies, for short. Within this context the origin of the research question of heredity and its explanation by Mendel can now be attempted to be elucidated.

In 2000 Steve Fuller attempted to derive a *philosophical history for our times,* based on the book by Thomas Kuhn (1922-1996), *Structure of Scientific Revolutions* published in 1962.[3] Within this context he stated that scientific discovery is not recognized as such unless it can be justified as issuing from the history of relevant science. As the standard case in point Fuller presented Gregor Mendel (1822-1884), whose pea-breeding experiments were neglected for forty years. In 1900 he was 'rediscovered as the father of modern genetics'.[4] Kuhn found it most striking that natural scientists usually could agree on how to evaluate a piece of research, typically by reference to an earlier exemplar that the research was said to resemble. From this insight grew his protean term, *paradigm*, which he deemed necessary for the conduct of 'normal science'. Paradigm acquisition, the model upon which scientific training is based, requires a commitment so deep that scientists cannot readily change their research orientation afterward in the face of mounting anomalies that resist paradigmatic treatment. Kuhn compared the shift between paradigms to a religious conversion or to a change in the worldview, the overall effect of which produces a revolution in the science. The conclusion was that the revolutionaries in natural sciences often turn out to be younger or marginal scientists who have 'little invested in the old paradigm'.[5] Mendel began his hybridizing experiments when he was 32 years of age.

Kuhn's *Structure* also helped to level hierarchies of scientific disciplines and overturn inappropriate methodological standards, thereby contributing to the climate of pluralism that continues to flourish in most systems of higher education. Historians and sociologists of science have turned structure into the new research program known as the *sociology of scientific knowledge* and the interdisciplinary field of

[3] Kuhn, T. (1962) The structure of scientific revolutions. The University of Chicago Press, Chicago.

[4] Fuller, S. (2000) Thomas Kuhn a philosophical history for our times. The University of Chicago Press, Chicago and London, p. 89.

[5] Ibid. p. 2.

*science and technology studies*, *science studies,* for short.[6] Within this context the origin of the research question of heredity and its explanation by Mendel can now be attempted to be elucidated.

## Contentious discovery

Mendel's paper on *Plant hybridization*, published in Brno in 1866, was 'rediscovered' sixteen years after authors' death. Carl Erich Correns (1864-1933), the mostly acknowledged rediscoverer, described the explanation of segregation and the independent combination of parental traits in the progeny of hybrids as two principles, soon generalized into basic laws of heredity ascribed to Mendel.[7] Recently Hans-Jörg Rheinberger brought to light that Correns made notes from Mendel's *Pisum* paper already in 1896. He was right to assume that Mendel had in hand the reprint of paper sent by Mendel to Carl Wilhelm Nägeli (1817-1991), his father of law. Correns investigated the influence of the pollen from different plant varieties on the coloration of mother seeds in crossing experiments with *Zea mays* and *Pisum sativum.* The experiences led him in 1899 to read Mendel's paper anew, 'with another eyes' and to prepare for publishing the results of his peas experiments. In April 1900 Correns was surprised to read the paper by Hugo de Vries 'Sur la loi de disjonction des hybrides' without quoting Mendel's paper, Correns wrote in two days his paper and sent for publishing, acknowledging Mendel's priority already in the title.[8]

In 1900 William Bateson (1861 - 1926) conceived Mendel's insight on the possibility to study *the law of inheritance in hybridizing experiments* and announced new science of heredity as *genetics.*[9] After 1900 naturalists soon accepted the rediscovery of Mendel's experiments in plant hybridization. But the topic of hybridization and heredity, colored by the explanation of the origin of new species through natural selection by Charles Darwin (1809-1882), remained controversial. In the 1930s the development of genetics on the new methodological level resulted in the synthesis of evolution and heredity. In 1936 Ronald Aylmer Fisher (1890-1962), one of the protagonists of the synthesis, wrote about the tale of Mendel's discovery of the laws inheritance and the sensational rediscovery of his work. Thus, he instigated geneticists to study Mendel paper anew to know*: What did Mendel discover? How did he discover it?* and *What did he think he had discovered?*[10]

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[6] Ibid. p. 3.

[7] Correns, C. (1900) Gregor Mendels Regel über das Verhalten der Nachkommenschaft der Bastarde. Berichte der Deutschen Botanischen Gesellschaft 8, 158-68.

[8] Rheinberger, H. J. (2006) Epistemologie des Konkreten. Studien zur Geschichte der modernen Biologie. Suhrkamp Verlag Frankfurt am Main, pp. 75-113.

[9] Bateson, W. (1902) Mendel´s Principles of Herededy: A Defence. Cambridge. University Press.

[10] Fisher, R.A. (1936) Has Mendel´s work been rediscovered? Annals of Sciences I, pp. 115-37.

Fisher's criticism found resonance in 1950 when geneticists were commemorating the fiftieth anniversary of the rediscovery of Mendel's paper as the Golden Jubilee of Genetics.[11] Cyril Dean Darlington (1903-1981) emphasized that it had also taken 50 years to rediscover the determinants of heredity, which Mendel called elements, elevating his discovery to the level of 'rather the primary law of biology'[12]. Jay Laurence Lush (1896-1982), the acknowledged 'father of modern animal breeding', noted that scientific discovery is usually not accepted by the contemporaries and it is good, because the first interpretation used to be erroneous, at least by part. But its acceptance too late could be harmful.[13] Geneticists began to pay attention also to the history of genetics in order to defend the discipline, which was at that time stigmatized in countries under communist control as a reactionary science and subordinated to political ideology followed by with the persecution of geneticists. The origin and essence of Mendel's discovery and the rehabilitation of the discipline and of his founder became of real concern.

In 1962 American human geneticist Curt Stern (1902-1981) visited Brno to speak with Jaroslav Kříženecký (1896-1964), at that time the dismissed professor of animal breeding and genetics, to discuss Mendel's research which was to be examined in 1965 within the context of the origin and development of genetics on the occasion of the one-hundredth anniversary of the publication of Mendel's *Pisum* paper.[14] Both individuals were convinced that Mendel's research was not sufficiently known to geneticists.[15] Before his sudden death toward the end of 1964 Kříženecký elaborated a plan for the historical investigation of Mendel and his role in the origin and development of genetics with the priority of the explanation of Fisher's questions.[16]

After 1965 the historical investigation began to offer new information about Mendel's research. Soon a series of papers brought to light the unique communications between animal and plant breeders and the newly named professors of agriculture and natural history in Moravia, a province of the Hapsburg monarchy, within the newly organized *The Royal and Imperial Moravian and Silesian Society for the Furtherance of Agriculture*, *Natural Sciences and Knowledge of the Country,* briefly called the Agricultural Society. The main organizer was Christian Carl André (1763-1831), who after studying at the Jena University began his teaching activity at the newly established Philantropinum Institute at Dessau and

[11] Dunn, L.C. (ed) (1951) Genetics in the 20th century - essays in the progress of genetics during its first 50 years. MacMillan, New York.

[12] Darlington, C.D. (1951) Mendel and determinants. In: cit. 11, pp. 315-32.

[13] Lush, J. L. (1951) Genetics and animal breeding. In: cit. 11, pp. 493-525.

[14] Sosna, M. (1966). G. Mendel memorial symposium 1865-1965. Academia Prague.

[15] Kříženecký, J. (1965a) Fundamenta genetica. The revised Edition of Mendel´s classic paper with a collection of 27 original papers published during the rediscovery era. Academia, Prague. Stern, C., Sherwood, E. (1966). The origin of genetics a Mendel source book. W. H. Freeman, San Francisco.

[16] Orel, V. (1992) Jaroslav Kříženecký (1896-1964), tragic victim of Lysenkoism in Czechoslovakia. The Quarterly Review of Biology 67, 487- 95.

later at Schnepfenthal in Saxony.[17] Soon he was known as authors of natural scientific and economic publications. His broad interest in natural sciences led him to be one of the founders of the first Mineralogical Society in Jena in 1798. At that time he accepted the invitation to Brno to be the teacher at the first evangelic school. Immediately he became one of the foremost figure in scientific development, emphasizing the importance of developing basic and applied research in natural sciences. In Brno, the center of the textile industry in the monarchy, André was to devote increasing attention to scientific sheep breeding for the improvement of wool production. On the proposal of André first professors of agriculture and natural sciences have been named at the university in Olomouc and at the Philosophical Institute in Brno.[18]

The exchange of information on scientific breeding at the annual meetings of sheep breeders, organized inside of the Agricultural Society, led to the formulation of empirical *genetic laws* in 1818, four years before the birth of Mendel. Three years later the discussion on plant breeding provoked the investigation of the *law of hybridization*. The long lasting discussions between professors of natural history and sheep breeders on the enigma of the transfer of parental traits to progeny culminated in Brno in 1836-1837 with the formulation of the physiological research question *what is inherited and how*?[19] Professor Johann Karl Nestler (1783-1841) from Olomouc University summarizing the long lasting discussion in the serialized paper *Heredity in sheep breeding* also indicated that nature produces natural species with undoubted constancy through forces beyond the hand of man, and man can modify the deviations in organic bodies 'with increasing or disappearing inheritance'.[20] Twenty-two later Charles Darwin explained the natural selection in the origin of new species. According to the leading sheep breeder, Emanuel. Bartenstein (1769-1838), floated above the heads of the discussants 'the genius of the truth and highly education'. The prolific communication of breeders with naturalists was appreciated by a senior member of the Agricultural Society, Joseph Waniek: 'Differences of opinion, freely expressed in the meeting, stimulate experiments that open the way for new reflection and experimentation which, according to natural law, cannot be stopped.[21] But, after the revolutionary year of 1848 in the Hapsburg monarchy, naturalists in Brno established the independent *Natural Science Society* with the aim of cultivating *pure science* and the previous

[17] Franke. H., Orel, V. (1981) Christian Carl André (1763-1831) as a mineralogist and organizer of scientifici sheep breeding in Moravia. In: Gregor Mendel and the foundation of genetics (eds. V.Orel and A. Matalová), Moravian Museum, Brno, pp. 47-56.

[18] Wood, R. J., Orel, V. (2005) Scientific breeding in central Europe during the early nineteenth century: background to Mendel´s later work. Journal of the History of Biology 38, 239-72.

[19] Bartenstein, E., E.Teindl, F. Hirsch , C. Lauer. (1837) Protokol über die Verhandlungen bei der Schafzüchter-Versammlung in Brünn 1837. Mittheilungen, pp. 177-9.

[20] Nestler, J.K. (1837) Ueber Vererbung in der Schafzucht. Mittheilungen, pp. 265-9; 273-9; 281-6; 289-300; 318-320.

[21] Waniek, J. (1845) Repräsetantenbericht über die achte Versammlung der deutschen Land- und Forstwirthe zu München. Mittheilungen, pp. 249-52; 261-4.

communications between breeders and naturalists was interrupted and the problems of heredity in Brno were left undiscussed.[22]

In 1843 Mendel was accepted into the Augustinian monastery in Brno and during his studies of theology he also attended lectures on agriculture and became acquainted with the role of hybridization in creating new plant varieties. During his studies at the University of Vienna in 1851-1853 he devoted his main attention to physics, mathematics, chemistry and plant physiology and acquired the theoretical background and the skill to perform experiments and to undertake independent research. To Brno he returned with the plan of plant hybridization experiments already in mind. In his initial theoretical framework he connected the experience of plant breeders and the knowledge of plant hybridizing experiments by botanists to the latest physiological investigation of fertilization on the cell level. The model of discrete contrasting trait-pairs was the starting point for the generation of hypotheses tested by experimentation, which led Mendel towards an explanation of the transmission of the material determinants of heredity through the germ cells.[23] In 1999 Sander Gliboff in his paper *Gregor Mendel and the laws of evolution* indicated that the main motive for Mendel's research was encouraged by his professor F. Unger, who wished to investigate the goal of creation *a physics of plant organism.*[24] He applied the German term *Bildung* for the growth and development of individual plant and the term *Entwicklung* for the changes in the flora through in geological time. Mendel in his lecture in 1865 was explaining 'a generally applicable law of the formation *(Bilding*) and development (*Entwicklung)* of hybrids*'*. According to Gliboff his phrases *Bildung* und *Entwicklung* would be redundant, unless he was distinguishing between individual ontogeny and evolution in the lineage. In 1865 Mendel presented his theoretical explanation to listeners from the new natural scientific community who did not grasp that he was addressing the research question that had arisen from the discussions between breeders and naturalists in Brno thirty years earlier.

The neglect of his discovery and of its contentious rediscovery in 1900 could be explained within Kuhn's structure of scientific revolution. His revolutionary paradigms arose from the history of the physical sciences in Europe originating mainly from the years 1620 to 1920 and from chemistry after the mid-nineteenth century, connected with the names of revolutionary geniuses such as Isaac Newton (1642-1727) and Antoine-Laurent Lavoisier (1643-1694). According to Kuhn most branches of physics and chemistry 'were paradigm based, while most branches of biology were not'.[25] The exclusion of biology from paradigmatic status was later

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22 Orel, V. (1970) Die Auseinandersetzung um die Organisation der Brünner Naturforscher in der Zeit, da G. Mendel seine Pisum-Versuche durchführte. Folia Mendeliana, 5, pp. 55-72.

23 Orel, V. and Hartl, D. L. (1994) Controversies in the interpretation of Mendel´s discovery. History and Philosophy of the Life Sciences, 16, pp. 423-64.

24 Gliboff, S. (1999) Gregor Mendel and the laws of evolution. History of Sciences. XXXVII, pp. 217-35.

25 Fuller, cit. 4, p. 76.

explained as his failure to deal with that discipline on grounds no more distinctive than sheer ignorance. Kuhn was influenced by Karl Popper (1902-1994), who assumed that evolutionary theory is unscientific because it contains no *falsifiable predictions*.[26] In Fuller's view, if Darwin 'is treated as comparable to Newton in providing the exemplar for pursuit of normal science, than a strict Kuhnian would place the origin of paradigm-driven biology no earlier than the 1930s, the dawn of so-called neo-Darwinian synthesis'.[27] In fact, the synthesis was proved by geneticists, who arose from the rediscovery of Mendel's hybridizing experiments.

Scientific discovery does not occur in a sociological vacuum, but rather against the background of a normal paradigm within which it could be justified. Fuller illustrated the selection metaphor from evolutionary biology. Individuals function as generators of novelty for the scientific community, as modeled on a population-based concept of species. Just as an individual organism that successfully reproduces itself increases the overall fitness of the species, if the novelty is accepted as a genuine discovery, it strengthens the community and, in that sense, the discovery is justified. When the community fails to incorporate the novelty, then only the individual who proposed the novelty suffers.[28] Mendel could be presented as an individual who was affected. In December 1866 he sent the reprint of the Pisum paper to Carl Wilhelm Nägeli (1817-1891), professor of botany at the University in Munich, the acknowledged expert in plant hybridization, who did not accept his explanation. In his letter in April 1867 Mendel resolutely wrote: *I knew that the results I obtained were not easily compatible with our contemporary scientific knowledge, and that under the circumstances publication of one such isolated experiment was doubly dangerous; dangerous for the experimenter and for the cause he represented.*[29] After 1900 Abbot Franz Bařina, who was the last novice accepted into the monastery by Abbot Mendel recorded Mendel's feelings about his research in optimistic words rather then suffering: *Though I have had to live through many bitter moments in my life, I must admit with gratitude that the beautiful and the good prevailed. My scientific work brought me much satisfaction, and I am sure it will soon be recognized by the whole world.*[30]

## Science studies and pansophy

Within the context of Western science Fuller called to mind the rehabilitation of 'philosophical titans of yore', René Descartes (1596-1650) and Gottfried Wilhelm Leibniz (1646-1716), who came before Isaac Newton (1642-1727) and who are

[26] Ibid, p. 14.
[27] Ibid. p. 76.
[28] Ibid. p. 90.
[29] Stern and Sherwood, cit. 15, p. 60.
[30] Křížlenecký, J. (1965b) Gregor Johann Mendel 1822-1884. Texte und Quellen zu seinem Wirken und Leben. Leopoldina Akademie, Halle, p. 6.

now classified more as philosophers than scientists.[31] The Czech Jan Amos Comenius (1592-1670), known in the literature as a reformer of theology, pedagogy and education in the pre-enlightenment era, could be placed among these titans. From 1618 to 1620 he was administrator of the school of Moravian Brethren in the town of Fulnek, near to Mendel's birthplace. At that time he became acquainted with the works of Francis Bacon (1581-1626), the renovator of natural sciences in England, who recommended natural history in programme of general education. Following the Battle of the White Mountain in Prague in 1620, the repressive reestablishment of Catholicism forced Comenius into exile. Living in Poland in the town of Leszno as a teacher at the gymnasium of the Moravian Brethren he wrote pedagogical books, which were soon translated into different languages, and he acquired an international reputation.

In the tense religious climate of the late Renaissance many philosophical authorities turned to search for the truly pious philosophy, called also Mosaic or Christian philosophy, to overcome the extremes between rationalism and biblicism. Comenius in his attempt to reform physics according to the divine light of the Scripture, published in Leipzig in 1633 the book *Physicae ad lumen divinum reformatae synopsis.* In 1643 and 1645 the Latin version was republished in Amsterdam and in 1643 in Paris. The English translation appeared in 1651 in London under the title *Naturall Philosophie Reformed by Divine Liht: or A Synopsis of Physicks.*[32] In 2000 American historian Ann Blair indicated that the book was widely read by philosophers of the late Renaissance and by the mid-century. Comenius was well known as a unique explicit visionary who saw in pious philosophy the key to a reformation of knowledge and to a realization of his pansophy-wisdom for everyone and everywhere.[33] He praised F. Bacon for presenting a true key of Nature, but later he regretted that Bacon did not open the secrets of nature and only showed how they were to be opened. Not satisfied with the slow progress produced under Bacon's induction to abstain from axioms till full-induction could be made, Comenius wished to make better experiments 'whether more light might be let into our minds by means to observe the secrets of nature the more easily'.[34] The discovery of blood circulation by William Harvey (1578-1657) he later presented as an example of full-induction.[35]

Comenius' enthusiasm for the growth of scientific knowledge was warmly received by Samuel Hartlib (1600-1662), the renovator of the sciences in England.

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[31] Fuller, cit. 4, p. 25.

[32] Comenius, J.A. (1633) Physicae ad lumen divinum reformatae synopsis, Philodidacticorum et theodidactorum. Leipzig: Gotofredus Grossius (quoted by A. Blair, cit. 33, p. 24).

[33] Blair, A. (2000) Mosaic physics and the search for a pious natural philosophy in the late renaissance. ISIS 91, pp. 32-58.

[34] Ibid, p. 41.

[35] Červenka, J. et al. (eds) Jan Amos Komenský, De rerum Humanorum Emendatione. Obecná porada o nápravě věcí lidských. In English: General Consultation on the Improvement of All Things Human, Svoboda, Praha, vol. I, cit. p. 453.

He was born in Prussia and after studying at the Königsberg University he moved to England in 1628 where he briefly studied at Cambridge. Later he took a lively interest in the reform of education using his network of relations to promote the educational ideas of Comenius. In 1637 Hartlib published in Oxford Comenius's 'certain meditations' under the title *Preludia Conatuum Pansophicorum* which encouraged Comenius to publish the book titled *Pansophiae prodromus* in London in 1639.[36] In the introduction he stressed that the time had arrived when the enthusiasm to open schools in all nations had appeared, followed by the publishing of studies of such perfection that all human things could flow mostly easy into the mind. He expressed the emergence of method for learning the general and full wisdom as pansophia. In 1841 Hartlib arranged for Comenius' invitation to England to participate in the modernization of the educational system and to negotiate for the establishment of the society of scientists with the aim of creating pansophy. Revolution broke out in England, and in 1842 Comenius returned to The Netherlands, leaving behind a certain portion of his studies under the title *Via Lucis vestigata et vestiganda*, written in the environment of the diffusion of scientific knowledge and published later.[37]

In 1642 Comenius met Descartes in Endegest near Leiden and for four hours they discussed mutually interesting philosophical problems. In 1941 the Czech comeniologist Jan Patočka (1907-1977) described how Descartes was explaining the secret of his philosophy, and Comenius defended his conviction that all human knowledge, gained only through human senses and reflection, is insufficient. Descartes did not intend to put forward the scope of his philosophy and expected that it would become part of what Comenius would treat as the integral whole.[38] For Descartes the connection of theological pansophy with pan-harmony was not conceivable. Patočka quoted Comenius's explanation of man's ability to investigate (ars investigendi), to discover (ars inveniandi) and to demonstrate (ars demonstrandi).[39] His recommendation of experiment in scientific education led Patočka to acknowledging Comenius as theorist of natural sciences teaching. His conclusion was that Descartes' negative judgment of Comenius should be subjected to critical revision.[40] In 2001 Dutch historians Jeron van de Ven and Erik-Jan Bos brought to light new information about Descartes' view on

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[36] Comenius (cit.1, p. 4) in the Dedication of his book Via Lucis gave: I wrote certain meditations, dispatched them to England, where they were printed first at Oxford and later in London under the title of *Essays introductory to Pansophia (Praeludia Conatuum Pansophicorum).*

[37] Comenius, cit. 2, p. 5.

[38] Patočka, J. (1941) O nový pohled na Komenského, (In English: On the new view on Comenious), In: Schifferová, V. (ed) (1997) Jan Patočka Komeniologické studie I. (Collection of papers I. by Jan Patočka, Vol. 9, Archives of J. Patočka, Praha, cit. pp. 11-21.

[39] Patočka, J. (1957) Filosofické základy Komenského pedagogiky. (In English: Philosophical foundations of Comenius´s pedagogy), cit. 38, p, 187.

[40] Patočka. J. (1957).Některé z dnešních úkolů bádání o Komenském. (In English: Some tasks for the contemporary investigation about Comenius). Cit. 38, p. 310.

Comenius' pansophy.[41] In two newly-found letters, written to the mathematician Cornelius Van Hogelande (1590-1662) in Leiden in 1639-1640 Descartes withdrew his entirely negative judgment of Comenius' universal wisdom, described in the book *Pansophiae prodromus.* He depicted him as an intelligent, learned and devout genuine seeker of truth who too closely combined human science and theology promising a new science. But, he was no nearer, gathering from his work 'so learned as to be able to lay out the whole of human knowledge in one book'. Descartes admitted that someone could lay down a new foundation for the sciences that will be firmer and more stable than those available at present, but he did not know anyone who had enough intelligence to make such a new start. Descartes did not mention Comenius's book on physics and could not have read Comenius' book *Via lucis*, published eighteen year after his death and his masterpiece published much later. He might have changed his view and acknowledged Comenius' 'start of a new science', at least.

In 1642 Comenius accepted an invitation to work as reformer of education in Sweden for two years and for four years in Hungary. During this time he was also captivated with his pansophia studies. The manuscript of his masterpiece, finished in Leszno in 1657, was destroyed in a great fire in the town. Later living in The Netherlands with exertion Comenius renovated the manuscript. But, the whole volume was lost and discovered as late as the 1930s in the archives in Halle in Germany. It was published in Prague in 1992 in three volumes in Czech (in 1637 pages) under the title in Latin *De rerum humanorum Emendatione* (General Consultation of the Improvement of All Things Human).[42]

After the foundation of the Royal Society Comenius finished the manuscript of his book *Via Lucis*, published in Latin 1668 in Amsterdam.[43] It was dedicated to *The Torch Bearers of this Enlightened Age, members of the Royal Society of London now bringing real philosophy to a happy birth, greeting and good fortune'.* For achieving the Universal light he recommended the foundation of Universal Books which should be true and perfectly arranged summaries of all things with the threefold virtue: fullness, order and truth. The first book, called *Pansophia,* would include all things that are simply necessary for all men who desire to be wise. It would contain the very marrow of eternal truth, that is, the whole fundamental condition of all things as they are in their ideas, showing how all things proceed backwards and forwards from a single principle along the lines of a single order to one fixed form of truth. The second book would be *Panhistoria,* unfolding all the variety of particular things, that is, all the particular actions, accidents, and issues of things, which have hitherto been discovered, from their origin up to the present time. It would give the knowledge of the particular course of things, whether in the domain of Nature

[41] van de Ven, J. and Bos, E.J. (2004) Se nihil daturum. Unpublished judgment of Comenius´ pansophiae prodromus (1639). British Journal for the History of Philosophy 12, 369-86.

[42] Červenka, cit. 35, vol. I. 563 pages, vol. II. 500 pages, vol. III. 594 pages.

[43] Comenius, cit. 2, p. 3.

or in that of Art, or Ethics, or Politics, or, finally, of Religion, which will greatly strengthen, illuminate and increase universal wisdom. The third book, *Pandogmatia,* will review the various theories or opinions, which have been held about things wherever and however they have been produced and whether they are true or mistaken. This book would not be necessary as much as useful to men who covet full and complete learning, and are able to find somewhat ample leisure for it.[44]

Later in his masterpiece Comenius wished to explain the way how to achieve the highest grade of the universal light. The harmonious endeavour for connecting theory with practice led him to reject the traditional differentiation between *scientia* and the practical ability, *ars*, both considered as creative processes.[45] 'The World of human creative ability' he treated as the pansophic light arising from human diligence, which transforms nature. The latest discoveries in nature Comenius considered as the imitation of what is being in nature and the remarkable achievements in *chimia* he saw as very near to miracles.[46] The term *gnostica* (ars) he presented in three grades of knowledge: empiric - what we know from observation, epistemic - what we understand how it is, and heuristic - which allows us to investigate new problems and establish new principles.[47] According to Comenius everything in nature can be classified by counting, measuring and weighing and for learning things, perceived with the senses. Within this context he mentioned the help of mathematics, arithmetic, geometry and statistics and the repetition of experiments to eliminate the effect of chance.[48]

## Mendel and Comenius

It is not well known that Comenius' ideas survived in the last place of his activity in Moravia, the town of Fulnek not far from Mendel's birthplace. In 1792 the Countess Walpurga Truchsess-Zeil (1762-1828) founded a private Philanthropinum Institute at her seat at Kunín, not far from Fulnek. Its aim was to teach an essential part of economic, cultural, and social development of country people. Before the establishment the Countess visited the reputed educational institute Philanthropinum at Schnepfenthal in Saxony, which largely owed its origin to the spreading of Comenius' ideas in education. The inspiration for its foundation came from *Philantropism,* a method of education in which pupils were encouraged to acquire knowledge from direct observation of nature. It was developed by the protagonists of English dissenting academies, who were introducing the teaching of new subjects, ignored at that time at Cambridge and Oxford Universities. The children were encouraged to acquire knowledge from

44 Ibid. pp. 148-61.

45 Červenka, cit. 35, vol. I, p. 33.

46 Ibid. vol. II, p. 32.

47 Ibid, vol. II. p. 69.

48 Ibid. vol. I. p. 155 and vol. II. p. 104.

direct and open-minded observation of nature, and freely to exercise their bodies as well as their minds. Among the influential protagonists was the chemist Joseph Priestley (1733-1804), who had earlier led the teaching at Warrington academy 25 km from Manchester. With his colleagues he practised a non-authoritarian form of education, based, whenever possible, as in the natural sciences, on experimentation. The aim was to teach an essential part of the economic, cultural and social development of country people.[49]

In 1774 the first German Philantropinum was set up at Dessau in Saxony by Prince Leopold III Friedrich of Anhalt - Dessau. Visiting British Isles he appreciated the benefit, both economic and social, of the education provided at some of the English famous academies and mostly by Priestley. The teaching programme elaborated its director J. B. Basedow (1724-1790) who published in 1768 an acclaimed demanding educational reform based on the writings of men such as J. A. Comenius, John Locke (1632-1704), and Jean Jacques Rousseau (1712-1778).[50] In 1771 in his book *Methodenbuch* Basedow described the methods of teaching of school children supported with published copper engravings, emphasized as a visual playful teaching, for which the right way was indicated by Comenius.[51] He inspired a series of teachers at Dessau Philantropinum as Christian Gotthilf Salzmann (1744- 1811) who founded the Philantropinum at Schnepfenthal and Ch. C. André, later teacher at Schnepfenthal, who in 1798 moved to Brno.

Jan Kollár (1798-1852), later professor of the Czech language at the Vienna University, when studying theology at the Jena University, visited the Schnepfenthal Instititute. In the book of visitors I found his note: *here breathes the spirit of Jan Amos*. The Kunín Institute was administrated by Johann Schreiber (1769-1850). In 1801 André publishing his programme of education described Schreiber's teaching as 'training for diligence and permanent occupation for acquiring useful knowledge and noble feeling' and recommended him to others in the provinces as an exemplary pedagogue.[52] In reaction to the French Revolution the Kunín institute was accused of 'spreading alien notions' due to its teaching of the natural sciences, and in 1802 Schreiber, who was held responsible for this 'scandal', was forced to leave the school. He became the parish priest in the village

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[49] Umbach, M. (2000) Federalism and Enlightenment in Germany 1740-1806. London, Habledon Press, quoted by Wood and Orel, cit. 18, p. 264.

[50] Basedow, Johann Bernhard, Ecyclopedia Britannica, (2003) p. 932.

[51] Frizsch,T. (1913) Joh. Bernhard Basedows Methodenbuch für Väter, Mütter der Familien und Völker. Mit Einleitung, Anmerkungen und Register von Dr. T. Frizsch, K.F. Koehler, Leipzig, VIII. Frizsch T. (1909) J.B. Basedows Elementarwerk mit den Kupfertafeln Chodowieckis u.a. Kritische Bearbeitung in drei Bänden. Detailed explanation in vol. I. (543 pages), vol. II. (576 pages), and vol. III. with 96 graphical tables.

[52] Schreiber, (1801) Nachricht von der Industrialschule zu Kunewald. Patriotisches Tagesblatt für Erblande, Brünn, pp. 42-45.

to which Mendel's birthplace belonged.[53] While teaching religion at the village school, he included the basic useful knowledge of natural history. His broad passion for natural history may well have later inspired Mendel's interest in the natural sciences.

Mendel's knowledge of Comenius can be inferred indirectly from the name of a newly bred fuchsia variety, *Orbis pictus*, which was the title of one of the famous pedagogical books by Comenius. The fuchsia variety was bred by Jan Tvrdý (1806-1883), the acknowledged breeder of ornamental plants in Brno, with whom abbot Mendel had friendly cooperation.[54] Basedow's two volumes of the book 'Praktische Philosophie für alle Stände' are preserved in the library of Augustinian monastery in Brno.[55] The fragment of poetry written by Mendel while studying at the gymnasium expressed his conviction that scientific knowledge would rid mankind of the superstitions and reflects the ideals of the Enlightenment and Philantropism which father Schreiber has spread.[56]

Remarkable ca be the comparison of Mendel's research method with Comenius' recommendation to members of London Royal Society is remarkable: *Let your researches into Natural objects be so well established, let them bear upon their face so complete an assurance of trustworthiness, that if a man desires not merely to contemplate your work as long as he likes with his unaided eyes, but even to try its accuracy by the most exacting tests of his own device, he shall be certain to find that the facts are precisely what you have shown them to be.*[57] In the first volume of his masterpiece Comenius also outlined the origin of scientific idea as a component of pansophic light from human diligence writing: *The creator forms his new ideas, first of all for himself, and for a certain period of time rejoices in it. When he wishes to show his work to another, he sketches it on paper or he shapes it into other matter. Later he comes to the realization of his work in the final form, allowing all to see it in order to gain the real profit of it'.*[58]

Mendel established his research programme on the new scientific level he had learned during his studies natural sciences at the Vienna University, and was proving his theoretical expectation in agreement with Comenius' principles by the most exacting tests of his own device. When his experiments were repeated in 1900 by his rediscoverers, they found precisely the same results as Mendel had shown. Comenius wished to 'bring more light into our mind' and expected the elaboration of 'the really mathematical method' rested on exact definition and elimination of errors. Mendel introduced the mathematical method with full-

[53] Orel, V., Vávra, M. (1979) Pedagogue Johann Andreas Schreiber (1769-1850). Folia Mendeliana 14, pp. 243-50.

[54] Vávra, M. (1984) Mendel´s cooperation with fuchsia breeder J. N. Tvrdý. Folia Mendeliana 19, pp. 251-6.

[55] Basedow, J.B. (1777) Praktische Philosophie fuer alle Stände. I-II, Dessau.

[56] Marvanová, L. (1966) Mendels dichterischer Versuch aus seinen Studienjahren. Folia Mendeliana 1, pp. 15-18.

[57] Comenius, cit. 2, pp. 22-3.

[58] Červenka, cit. 35, vol. I, p. 374.

induction and explained the hidden reality of determinants of the transmission of parental traits to offspring, described in 1979 by Horace Freeland Judson in the context of the achievements of *The Makers of the Revolution in Biology* 'as algebraic units'.[59]

The examination of Mendel's discovery within Comenius' universal wisdom for everybody and everywhere instigates to remind Comenius' passion for the growth of scientific knowledge in his sigh: *If I could only know more, or desire less! But, even now, as I learn more, I recognize that there is much more to be learned. I am always able to see farther, and I cannot suppress the desire for additional and more perfect scientific knowledge.*[60]

## Conclusion

The attempted elucidation of the origin of the research question of heredity before Mendel came to Brno and of the essence of his discovery and its justification within T.Kuhns's interdisciplinary sociology of scientific knowledge and science and technology studies and also within the broader historical network of J.A. Comenius' teaching of pansophia, panhistoria and pandogmatia. Could not be it also considered as the confirmation of the prophetic dictum by Comenius' younger contemporary G. W. Leibniz? *The time will come, Comenius, when crowds of the generous will pay honor for what you have done and for your hopeful dream.*[61]

## Addendum

The interdisciplinary approach in the explanation of the growth of scientific knowledge is now penetrating into the teaching of history and philosophy of sciences. At the University of Geneva Professor Bernardino Fantini (the faculty of medicine) and professor Francois Walter (faculty of letters) introduced a new course of study for the 2005-2006 school year *Histoire sociale et culturelle des saviors et des pratiques de santé* for achieving the *Diplome d'Etudes Approfondies (DEA)*.[62]

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[59] Judson, H. F. (1979) The eighth day of creation. The makers of the Revolution in Biology. A Touchstone Books by Simon and Shuster, New York, cit. pp. 205-6.

[60] Červenka, cit. 35, vol. I, p. 23.

[61] Ibid. p. 11.

[62] Fantini, B. and Walter, F. (2005-6) Diplome d´ Etudes Approfondies (DEA) Histoire sociale et culturelle des savoirs et des practiques de santé. Université Geneve, 24 pages.

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Abbreviations:

*Folia Mendeliana* - Brno, published yearly by the Moravian Museum, Brno since 1966.

*Mittheilungen* - Mittheilungen der k.k. Mährisch-Schlesischen Gesellscfat zur Beförderung des Ackerbaues, der Natur- und Landerkinde in Brünn, published weekly from 1851.

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# The Embryo Project and the Emergence of a Digital Infrastructure for History and Philosophy of Science

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**Abstract.** The international collaborative Embryo Project[1], housed at Arizona State University is developing an online open access encyclopedia covering 2500 years of embryo research in multiple varied contexts. It is also one of the core projects that make up a new initiative to bring the tools of the bioinformatics revolution in the life sciences to the areas of history and philosophy of science (HPS)[2]. This paper discusses the structure and accomplishments of the Embryo Project in the wider context of digital HPS with a special emphasis on ongoing work in developing a cyberinfrastructre for HPS.

Over the last two decades the life sciences have experienced unprecedented growth and have developed a new explanatory paradigm of systems biology (Kitano 2001; Alon 2006). These conceptual developments have been based on a shift away from simple causal explanations based on clearly identifiable factors, such as individual genes, and towards an emphasis on multiple factors and their interactions. Technological advances, especially in the area of bioinformatics, have played a central and crucial part in these conceptual changes (see for instance the National Center for Biotechnology Information for an overview of the tools and principles

[1] http://embryo.asu.edu
[2] http://www.digitalhps.org

of currently deployed in bioinformatics)[3]. As a consequence, the life sciences are now to a large degree information-based with the relevant information stored in both centralized and distributed databases. Sophisticated search algorithms and queries based on conceptual models and ontologies (in the computer science designation of the word), standardized annotation practices and new generations of relational databases connecting different kinds of data are the foundation of these new forms of life science research (Ouzounis and Valencia 2003).

Scholars in the Science Studies community (history, philosophy, and sociology of science) have always emphasized complex explanations of historical events, which are mostly presented as historical and richly contextual narratives and are thus always the end result of years of individual scholarship. What the science studies community has not yet embraced are the enormous benefits of the informatics revolution that has transformed the life sciences with respect to the organization of multiple forms of complex data, shared access to these data, searches in distributed relational databases that are organized around standardized practices of database management, and the possibilities of digital workbenches for collaborative and distributed research. All these developments have also contributed to robust cyberinfrastructure, which has changed the ways biologists go about their research (Ouzounis 2002, Lesk 2008).

In other words, the science studies community is missing out on new ways to conduct and organize research and to store, distribute and analyze data. One of the main consequences of the bioinformatics revolution has been the possibility of large-scale and comparative analysis of data and the integration of detailed experimental research with readily available points of comparison. This strategy has facilitated a bottom-up approach that allows biologists to find patterns of increasing generality. Insofar as one goal of the science studies community is to better understand both individual sciences as well as science at large in its various contexts (technological, theoretical, historical, social or political), it too will have to move beyond the particular and focus on general patters wherever these do exist, a goal greatly facilitated by the tools of the informatics revolution.

Unfortunately, current practices of both training and research have left researchers in the history and philosophy of science, and the humanities as a whole, ill-prepared and ill-equipped to take advantage of the new developments that have transformed research in the life sciences over the last two decades (Cantara 2004). Within the science studies community the history and philosophy of biology provides an excellent entryway into examining how to incorporate informatics technologies into its research because it bridges studies in the life sciences (which have developed the informatics technologies) and the humanities.

To give just one concrete example of the scope of the challenges, John Tyler Bonner, Professor Emeritus at Princeton University and doyen of the slime mold research community, mentioned at a recent workshop on morphogenesis at

[3] http://www.ncbi.nlm.nih.gov/

Arizona State University (ASU) that for most of his career he could read and keep track of every paper published on his preferred research subject, slime molds, but that today he is no longer able to come even close to doing that even though slime mold research is still a small field by anybody's standards (Bonner 2008). And slime mold research is still a small field by anybody's standards. The explosion of information produced by an increasingly globalized research community is simply staggering.

While all academics have experience in what may be appropriately called information triage, it is almost impossible to keep up without the help of newly developed information technologies that affect all areas of knowledge production, storage and dissemination (Renar and Palmer 2009). But before individual areas of research can take full advantage of these new possibilities we not only need to develop additional technologies and solve problems related to standards, we also need to change the culture in respective fields, such as the humanities and science studies, in order for them to embrace these new options (Martin 2007).

In this context the ASU-based collaborative Embryo Project (EP) has already provided unique insights into the possibilities of a digital approach to history and philosophy of science (HPS). The Project is also revealing numerous and varied technical and sociological challenges that still need to be addressed before serious digital HPS can come to fruition. Here we first introduce the Embryo Project and then give a brief overview of the possibilities and challenges of a digital framework for history and philosophy of science.

## The Embryo Project

The Embryo Project resides in the Center for Biology and Society at Arizona State University (ASU). Funded by several grants from the National Science Foundation, the Project has so far brought together an international network of scholars to develop the scholarly content and editorial functions that are producing the online Embryo Project Encyclopedia at http://embryo.asu.edu. At the surface the encyclopedia as such can be seen as a more traditional digital project within the HPS community in that it includes a peer-reviewed journal along with collections of archived historical materials such as videos, photos, and manuscripts. However, the Embryo Project team, working with others has also been developing a Digital HPS Collaborative centered at ASU and the Marine Biological Laboratory (MBL) in Woods Hole, Massachusetts.[4] This consortium of projects is developing new ways of doing scholarly research in HPS based on newly emerging digital tools that help support these endeavors (for further discussion see Maienschein and Laubichler in press).

In 2007 the Embryo Project received its first round of funding from the National Science Foundation, but by that time we had already done a great deal of

[4] see also http://www.digitalhps.org

preparation. In partnership with the ASU and MBL libraries, we decided to adopt the Fedora Commons repository software[5] and began working with programmers at ASU, the MBL, and the Max Planck Institute for the History of Science (MPI) in Berlin to establish basic ground rules for the data-basing and informatics parts of the project. We soon learned that such efforts are best undertaken within a larger collaborative setting as individual projects, if they want to succeed, grow and persist, need to follow common standards and practices. We soon found that for HPS no community comparable to Bioinformatics existed and that there were a variety of different projects, all more or less trying to reinvent the wheel. This is one reason we are dedicated to work in a collaborative way, since this is the only way to assure interoperability, divide necessary work on cybertechnology, and be able to share limited resources efficiently. We discuss some of our recent technological advances below. The important point here is that we are still building on the Fedora standard and related technologies that many libraries use since we want to be integrated with library archiving, collecting, and federated search functions.

On the editorial side, we worked with scholarly editing experts to establish an ISSN number as a peer-reviewed journal for our new publications. It is extremely important to do this at the very beginning, and not doing so is a mistake many projects have made. We want to be open-access and online, but also a trusted information source that is peer-reviewed and respected at the highest scholarly levels.

We then had to decide what the scholarly content of the Embryo Project would be. That is the main focus of this section. Our dream, also quite insanely naïve but still inspiring to us, was to include everything anybody has said about research related to embryos forever – from Aristotle till tomorrow, as we put it. While the focus would remain on the science of embryo research, we also wanted to contextualize this rich history and include the many layers of cultural, legal, bioethical, policy, and other knowledge relevant to understanding embryos in context as well.

The first step was to decide that although we welcome contributions of all sorts, we would initially provide a framework by looking at several historical episodes that each brought significant changes in theory and practice of studying embryonic development. These included the period of the Hypothetical Embryo, dominated by Aristotle and Aristotelianism. In this period the embryo remained largely a matter of theory in part because of limited technologies and also because of limited reason to ask questions and make observations. Microscopes and renewed enthusiasm for observing and describing that came with the 17$^{th}$ century brought the Observed Embryo, though observations remained on non-human embryos and fetuses. The Biological Embryo period began at the end of the 19$^{th}$ century, with the rise of experimentation and observation inside the developing

[5] http://www.fedora-commons.org

organism. Experimental tools for studying patterns, processes, differentiation, morphogenesis, and other cellular observations brought tremendous advances in the detailed understanding of what embryos are and how they change over time. The Inherited Embryo, starting in the 1950s and '60s, emphasized developmental genetics. Then in 1978, the human embryo became visible from the beginning

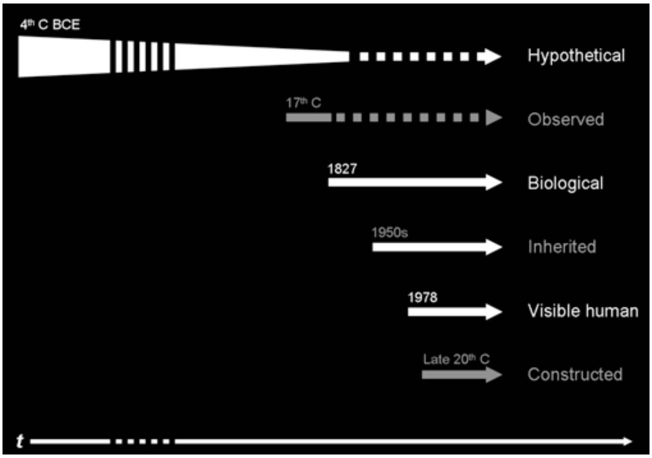


*Fig. 1 Episodes in the History of Embryo Research. Courtesy of Jason Scott Robert.*

stages with in vitro fertilization. This Visible Human Embryo and its presentations and representations brought considerable public attention. Finally, cloning, stem cell research, and genetic engineering have changed the concept of what an embryo is since researchers can produce the Constructed Embryo. Another period is emerging as well, that of the Computed Embryo, but this episode remains in early stages. These stages are captured below and discussed further in (Maienschein and Robert, in press).

The Project is not constrained by these episodes, but they provide a general framework for thinking about how all the individual pieces fit within an historical framework. Intellectually, what is important about the network of researchers and the approach we have adopted together is that the knowledge produced within the Embryo Project goes far beyond the expertise of any one contributor. While some of us are historians of science, others lawyers or bioethicists or philosophers or historians of technology or other types of specialists, the core Encyclopedia brings all the approaches together. A user of the project therefore does not have to be an expert in legal scholarship to have access to information about laws, information that is linked through the relational database to other articles about the science. The Fedora repository and its store of relationships links ideas that we as scholars, necessarily limited by our own specific training, cannot access without such help.

So far, the Encyclopedia has been working with three main types of entries, but we will be adding more and are already experimenting with some types, as we discuss below. The first of the existing types of entries is photographs and videos.

These existed at the MBL library, and we have digitized them and developed standards for metadata through the libraries and have developed relevant ontologies through our IT team members. To date, we have included more than 1000 photographs of biologists and a few videos, and we will be adding many more related to other areas of science. Since the Encyclopedia is Open Access, working with Creative Commons licensing agreements[6], we have added considerably to research materials available for scholars and the public with these images. But so far, we have worked with fairly simple collections to get all the details worked out smoothly. We will be adding more kinds of collections soon.

The second type of entry is the short derivative descriptive article, written largely by undergraduate students in a writers workshop seminar setting. These are all heavily and rigorously peer-reviewed before being submitted to the formal editing and publishing process. The products are therefore authored short articles.

Finally, the third type of product is the new scholarly papers by our contributors. The very first of these is the Ph.D. dissertation of Dr. Mary Sunderland (Sunderland 2008), on the history of regeneration research. This was added as a "proof of principle" in order to work out the details of how to digitize such a scholarly article in intelligent and standards-based ways. We have a line-up of other articles to add, but the informatics team is still working out the best way to mark up relationships in long articles (see below for a discussion on relationship mark-up).

We are working with librarians and informatics experts to add a fourth kind of "found object." These are the publications already out there, and there are many different search approaches for finding them. What we seek is a federated search that will lead the user easily to the kinds of publications the user needs. That takes a lot of sorting of the potentially millions of "hits" for a search, and librarians and informatics experts are working with many different experimental approaches to make searches more effective. We are close to settling on a standard, but are still working on this. When we have effective searches in place, we can add well-chosen short descriptive articles that will find dozens to hundreds of others, including linking to references where those are available online. As a result, what we have is an emerging powerful scholarly Encyclopedia. Our intended users include researchers and also multiple groups of users - students of all ages and the interested public at large. Each needs a different kind of result from a search, and we are also working on refining the tools for diverse user groups.

In the following sections, we introduce work in progress. This is very exciting in its capacities to go beyond what we can do with any existing scholarly tools. What we have described so far is the way an Encyclopedia like ours can link and bring discoveries as a result of finding relationships. Now we turn to new ways of thinking, with completely new kinds of knowledge as a result. The new results in turn lead to new questions and new ways of working.

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[6] http://creativecommons.org/

## A Cyberinfrastructure of HPS

As mentioned above, the enormous success of the life sciences over the last decades has been facilitated in no small part by the bioinformatics revolution, which has enabled new ways of managing, annotating, and sharing data. It also increasingly allows for the integration of different kinds of data, such as genomic and pathological data, genomic and environmental data, pathological and population data, genomic and historical data, etc. Fundamentally the computational challenges for HPS are very similar to those in the life sciences, but, as we have learned in the context of the Embryo Project, existing computational solutions need to be adapted and some novel challenges for history and philosophy of science need to be addressed as well. Specifically these challenges include

- ontology development
- natural language processing and semantic web tools
- digital workbench applications and web interfaces
- database design and management

Of these, ontology development and database design are the most crucial and foundational. Unlike in the life sciences, there are not yet established and accepted ontology standards for HPS or HPS-related fields, partly due to the late adoption of digital technologies into the humanities and partly due to the difficulties of identifying and parsing the subtle interpretations that are indicative of the abstract language used in many humanities writings (Niepert et al. 2007).

We have begun to develop a working ontology for the Embryo Project and several tools that allow us to utilize our ontology to extract meaningful information from digital texts, which is a good start but requires further testing and refinement. Our tools are based on the Open Biomedical Ontology standards[7] and are based on scripts that mine texts and extract meaningful information and relationships within the context of large data sets. However, the data relevant for historical analyses are generally varied and include different data types (ranging from different styles of literature to images, audio and more recently video). As a result, these scripts need to be further developed and tailored to multiple specific databases. The challenges are substantial and (bio) informatics is a broad and complex field. Nonetheless, the Embryo Project has already proven that applying informatics approaches can also transform HPS projects.

Informatics in biology and the biomedical fields works because all participants adhere to a strict set of standards about data representation and publication, which includes agreed-upon ways to make primary data available for others to use. In this way, large-scale genomics studies that span multiple species can be performed. Standardizing the ways information is captured and stored is integral to the effective management and sharing of knowledge. The world-wide web is itself a standard that allows people to exchange information via the creation of websites

[7] see http://www.obofoundry.org/

that contain all kinds of content. Microsoft's document format has become a standard in many academic and non-academic circles for the creation and sharing of textual information.

If one wants to be involved with the larger community that exchanges information in this way, one must make sure that information is captured and represented in the standard format. Meaningful collaborations that facilitate sharing of knowledge effectively build upon standardized sets of tools and methodologies. The term "cyberinfrastructure" is used to describe the sets of technologies and methodologies that enable data to be acquired, managed, and analyzed effectively so that disparate projects can share information. For example, in the biomedical field there are libraries of ontologies that describe entities and their relations to one another within specific research domains (Smith et al. 2007). The mission of the OBO Foundry specifically states their hope that "ontologies will be fully interoperable, by virtue of a common design philosophy and implementation, thereby enabling scientists and their instruments to communicate with minimum ambiguity. In this way the data generated in the course of biomedical research will form a single, consistent, cumulatively expanding, and algorithmically tractable whole."[8]

As outlined above, the Embryo Project is one such project in the area of science studies that has adopted the principles of biomedical informatics and has taken an open access approach. Based on the advice of the developers and librarians at the ASU library, the MBL and the MPI, the Embryo Project has adopted protocols that ensure that we are following common standards and practices and adopting technologies and platforms that make the Project open and freely and easily accessible. All articles are marked up under the National Library of Medicine's journal article DTD, which ensures long-term sustainability and exchangeability with a broad range of collaborators. Moreover, citations in the Embryo Project are tagged with MODS metadata[9], a schema developed by the Library of Congress for bibliographic entities. Likewise, images are collected and stored in the JPEG2000 format, which is fast becoming a standard for archiving digital images.

The Embryo Project has adopted a set of practices that rely on W3C standards for creating content and storing relationship information among objects, and has taken great care to annotate textual articles with these relationships in mind. Presently, all articles are marked up in the XHTML format and hand-coded with relationship information that is relevant to the various aspects of embryo research. This relationship information is currently referenced from an informal ontology of categories - People, Places, Organization, Contexts, Awards, Concepts, Law, Ethics, Religion, Technology, Experiments, Organisms, and Literature – and are stored as a RELS-EXT data-stream in the Fedora repository. Articles, images, and

[8] http://www.obofoundry.org

[9] http://www.loc.gov/standards/mods/

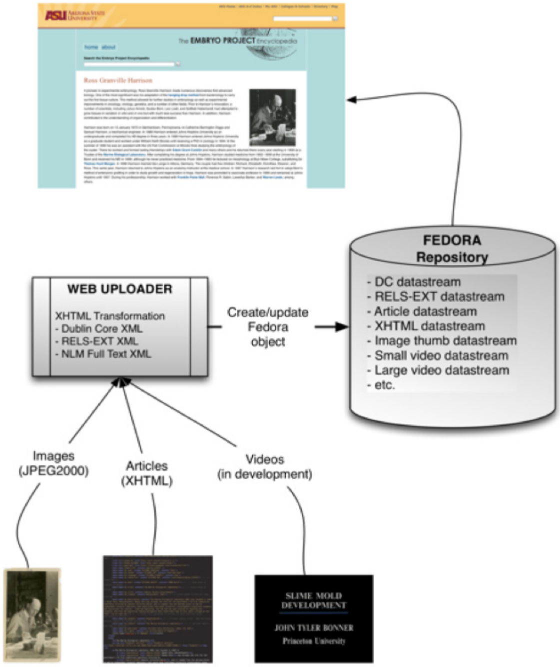


*Fig. 2 Embryo Project Workflow for creating, ingesting, and managing objects. Objects are uploaded to the repository via a web interface (currently only images, but will soon be enabled for images and videos). XHTML transformations take the text and extract and create XML representations that are then stored as datastreams in Fedora, which are disseminated via the website.*

videos are put into Fedora via a web uploader that the library has developed, which gives control of the ingestion process to the content creators. The information contained within the XHTML document is parsed and stored as separate data-streams in the Fedora repository. The relevant data-streams are then displayed in the encyclopedia via a web browser (see Figure 2).

Until now, technological contributors to the Embryo Project have devoted most of their effort to developing workflows and identifying best practices to address core questions about how to create unique content, edit and publish this content, and manage this content effectively. Phase One of the Embryo Project, then, has been about creating and managing its own content. This, however, is only the beginning.

The next stage of the Embryo Project involves development of an expansive and robust repository of embryo-related information that accesses other repositories and incorporates the vast sources of already-published materials on embryo research. In order to do this, the Embryo Project requires an informatics approach to mining, extracting, and analyzing content from diverse repositories. To accomplish this effectively the following challenges that the Embryo Project faces in its phase two, the semantic web phase, need to be met (these are the same challenges that all other digital projects in these areas of scholarship are also facing):

*Ontology development*

A formal ontology is critical to any digital project, to organize and structure the relevant terms and their relationships to one another. This ontology will be a necessary component of using semantic web technologies to do text mining and natural language processing of text documents in both the Embryo Project repository as well as other repositories. We have begun the difficult process of ontology development utilizing common tools in the informatics community (OWL, Protégé), but ontology development is an ongoing part of all digital projects (Noy and McGuinness 2001; Horridge et al 2007). One approach that we have been taken is to utilize multiple Obo-Foundry based ontologies in order to facilitate data mining and annotation (see below).

*Text mining and natural language processing (NLP)*

In order to extract relevant information from text, we must be able to develop tools that recognize not only exact terms matched against some ontology, but also perform natural language processing by recognizing words based on structures such as parts of speech (Riloff 1999). This will enable us to access large amounts of textual information and analyze its content computationally (see Hamed and Sarkar forthcoming).

*Working with other repositories and databases*

In order to increase both the size and range of objects in our repository, we need the capability of accessing other databases to cull relevant information and populate our repository with either the content or references to the content. In the larger HPS community, there are no federated databases in the way that PubMed aggregates text sources in the biomedical field. Because there are so many separate databases, many of which are specific to a journal or publisher, many different strategies for accessing the content are required.

*RDF creation, storage, and use*

As part of the Embryo Project workflow of writing original articles, we are careful to include relationship information within the source of that article. These relationships are stored as a RELS-EXT datastream that contains the RDF metadata of all the objects and their relationships to one another (Brickley and Guha 2004). We have not yet determined how to utilize this rich source of information, but its use is critical if we are to leverage the power of the semantic web. If the sharing of data, rather than documents, is to be accomplished, we need to develop standards among HPS digital projects so that RDF representations are captured and made available to other projects (Allemang and Hendler 2006). Tools that facilitate the exchange of RDF information as well as its analysis within the scope of the project are also important.

Each of the above problems is not unique to the Embryo Project, but for all fields of research. HPS research currently does not have the ability to leverage the informatics tools being used in the life sciences to solve these problems. Right now, we are working with our partners in Digital HPS to take initial steps in that direction to address each of the issues and understand the kinds of technologies and expertise necessary to move forward. In general, two main problems need to be addressed: (1) how to analyze large amounts of textual information computationally and (2) how to share this data with others.

Currently, all our articles are marked up and annotated by hand. While this has the advantage of being accurate, it does not scale when hundreds and thousands of articles need to be annotated. NLP tools, like OBO-Annotator and Ontotator (Hamed and Sarkar, forthcoming), are able to analyze text and, run against one or more ontologies, extract relevant terms in these ontologies.

We also have developed an extension of the OBO-Annotator tool, which we have called Vogon, that takes a text, displays any instances of terms in an ontology, and provides a user interface that allows researchers to identify relationships based on a standardized relationship ontology (see Figure 3 and 4). This information is then stored in a relational database that can be searched. This kind of NLP tool not only extracts terms in the ontology, but performs NLP tasks such as sentence tokenization and part-of-speech tagging. The data can be stored in a relational database and transformed into RDF triples that are then deposited into a triple store, which is a large repository of statements with the subject-predicate-object schema. Alternatively, these triples can be extracted directly from the text mining process. This triple store is the repository of all data of all the relevant content in the articles that were analyzed. It can be queried utilizing languages such as Prolog and SPARQL, and a large, federated triple store can also be created by incorporating triples that have been extracted in other projects.

The Encyclopedia of Life[10], for example, is creating this kind of federated triple store that will contain little bits of information about all species on earth. Researchers on other projects are encouraged to provide their triples to the Encyclopedia of Life, thus increasing both the amount and variety of information on species culled from projects with different interests. In this way, data about a species' habitat and range can "live" alongside data about a species' embryology. This promises not only a single pool of all information on species, which is the main goal of the Encyclopedia of Life, but holds the added promise of creating new knowledge because queries to the triple store will reveal relationships that were not detectable by any single project or database. The potential of sharing data among projects is now realized, with the further goal of inferring new knowledge

[10] http:///www.els.org

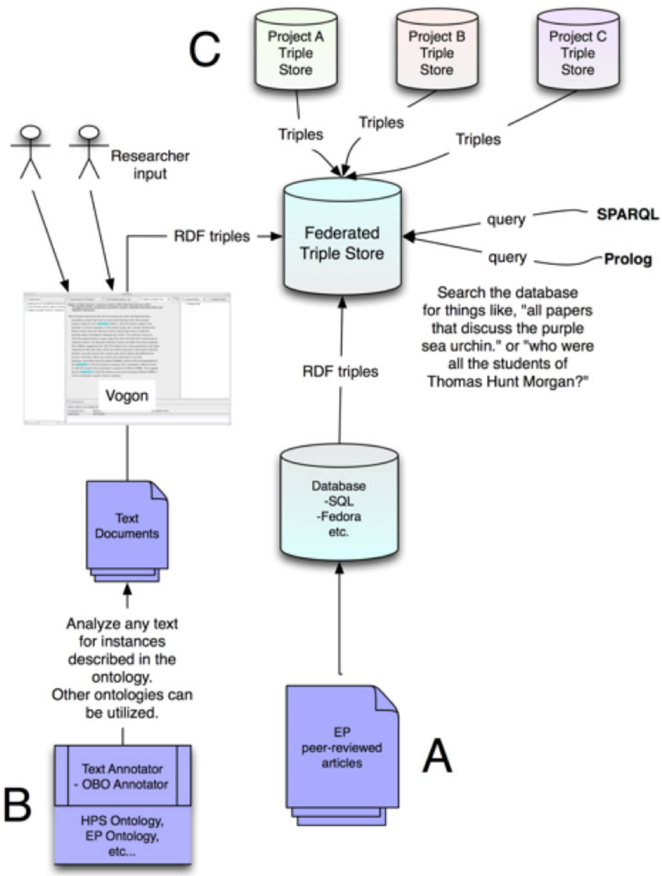


*Fig. 3 Ontology Workflow. In our scenario, a federated triple store would incorporate triples produced in a number of ways. (A) Embryo Project peer-reviewed articles are marked up utilizing a specific ontology. Relationship information is rigorously checked and hand-curated so that each article that is incorporated into the Encyclopedia also provides a rich set of RDF information that is stored in the triple store. (B) Various ontologies are utilized to analyze text documents using an application like OBO-Annotator (or Ontotator). The results of the analysis are passed on to Vogon, which highlights terms that OBO-Annotator found and allows end-users to edit relationship information that is then stored as RDF triples in the triple store. (C) Other digital projects produce their own triples and share this information with each other. This federated triple store can then be queried utilizing query languages such as SPARQL and Prolog. The richer the store of information contained in the triples, the more interesting and novel the results may be, ideally leading to discoveries that would not otherwise be possible.*

from this pool of collective data (Allemang and Hendler 2006). Figure 3 outlines a simplified view of how this informatics approach works.

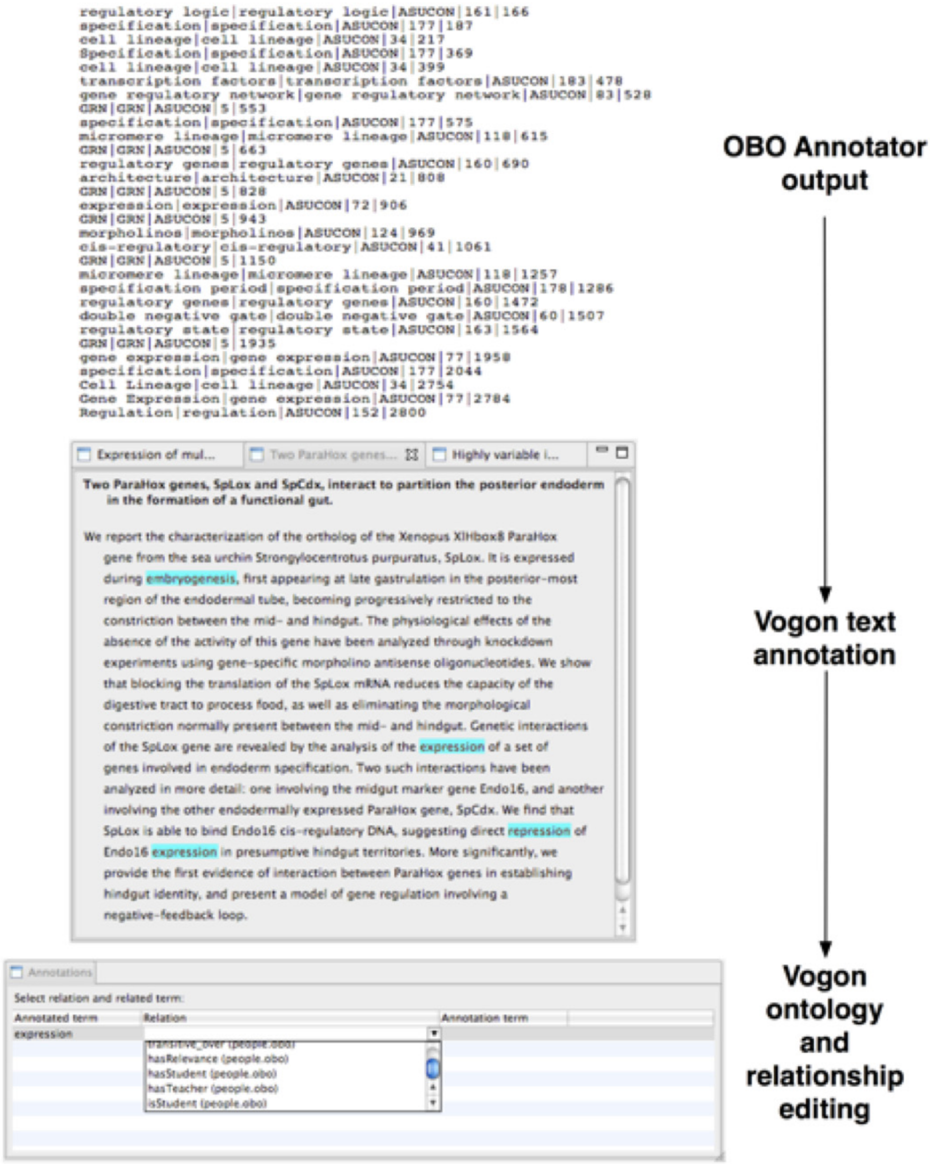


*Fig. 4 OBO-Vogon Tool. OBO-Annotator analyzes text documents and outputs found terms in the ontology and their locations within the text. Vogon takes this output and highlights these terms. Based on another relationship ontology, the user is then able to specify the relationship of the found term to other terms in the document. These "triples" relating two terms to one another are then passed on as RDF statements to the triple store.*

In addition to the database and annotation tools, the Embryo Project has also adapted and further developed visualization and analysis tools that enable and support the interpretation of our data for a wide range of users and scholarly interests. These tools include a test-case front-end web application that highlights concepts by matching their locations within a text. Thus, rather than searching for a specific string pattern and highlighting that string, the application highlights the location of the concept. These locations are important for future development of the semantic web aspects of the application, since one can then start inferring meaning within the text by connecting the relative proximities of concepts (and in

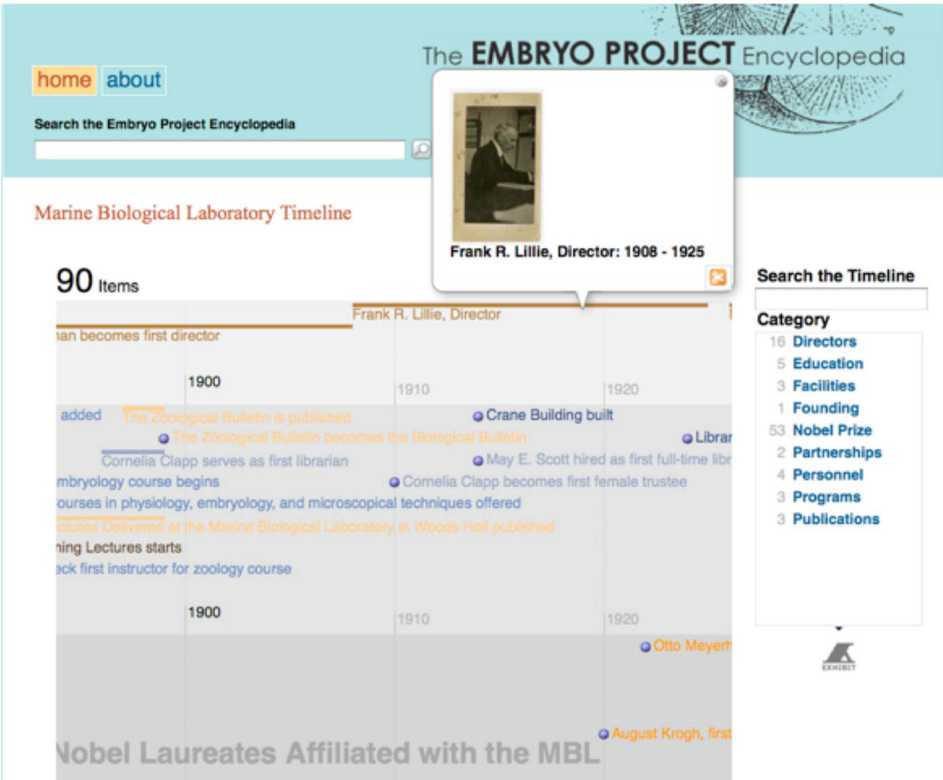


*Fig. 5 Timeline Tool. Timelines are one technology that allows rich textual historical information to be visualized in a clean interface yet still allow for user interaction with the technology. Rather than providing a static set of information, each datum within the timeline contains links to other objects (articles, videos, images) within the repository or links to other relevant websites. In the timeline for the Marine Biological Laboratory, for example, we are able to aggregate all the directors of the MBL and juxtapose this information against various important events in the history of the MBL as well as note the various Nobel Laureates that were affiliated with the MBL. The technology used for this timeline is adapted from MIT's SIMILE framework, an open source project that rests on standards that any web browser can read and understand. We have developed workflows that make it easy for a student, researcher, or anyone interested in the history of a particular subject to build these timelines.*

the future, organisms, relationships, experiments, people, and institutions). The concepts within a text are therefore unique because they occupy unique locations within the text, so "embryogenesis" in the beginning of an abstract is distinguished from "embryogenesis" later on.

This also allows us to take advantage of several available tools that highlight networks between concepts and allow us to dynamically refocus the display. For example, starting with one concept, such as “cis-regulatory logic” in a text, one can, by simply clicking on it in the text displayed in the application, get all additional papers that mention this concept, all authors associated with that concept, etc. Or, one can refocus on one author and immediately get all co-authors or all concepts that this author is connected with, etc. With such dynamic applications it is thus possible to test multiple scholarly hypotheses at an unprecedented scale and to reveal patters that are otherwise difficult to detect. Similarly, timeline applications also allow the user to display a lot of information simultaneously based on a temporal ordering principle. As Figure 5 shows, based on one of the focal projects within the EP—the history of the Marine Biological Laboratory - such tools also enable us to display events in a variety of different categories, thus allowing to highlight events of interest, such as a Nobel Prize, within a variety of contexts, such as the availability of new technologies or facilities.

## Conclusion — Challenges and Possibilities of Digital HPS

Therefore the development of a cyberinfrastructure for the science studies community will have to follow the same distributed approach it will ultimately enable. But, even though the only way to make substantial and sustainable progress is through an organized community effort, something the Embryo Project team has been organizing with the support of NSF, this approach also requires that we change the culture in HPS to include both a scholarly and an informatics perspective and to focus more on interdisciplinary teams of informatics experts and scholars working on these challenges. One additional benefit of an informatics approach to HPS is that it will also allow for a much closer integration of historical and philosophical scholarship into current science. HPS used to be closely aligned with scientific endeavors and disciplines, but has unfortunately lost some of these ties as it developed its own disciplinary identity. Rebuilding these bridges is essential for fields that are, by their very nature, interdisciplinary. And in the context of 21$^{st}$ century science this means that HPS needs to be integrated utilizing the new informatics tools as those have become the way scientists conduct business.

The biggest challenge to success is that the HPS community needs to take advantage of existing informatics tools and approaches. We do not need to start over and reinvent what has already been widely tested and proven elsewhere. Yet we do need to develop tools to capture the unique aspects of HPS research, including especially the importance of time and changes over time. Unfortunately, existing funding agencies are focused on supporting new computing technologies rather than on developing effective applications in defined communities. We recognize the need to carry out much of this work ourselves, collectively and collaboratively, sharing our tools and approaches as well as our scholarly discoveries. HPS scholarship must embrace traditional research and also development of new ways of working, which in turn leads to new discoveries. We are encouraged by the support for such types of development in various places, such as the Max Planck Institute for the History of Science, The Marine Biological Laboratory, Indiana University, Caltech, ASU and many museum and library based projects. What is still lagging behind is an improved coordination between these different projects.

We therefore call for cooperative and open development of digital HPS tools and collaborative working groups through Digital HPS.

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# Collecting and dissecting nature: Meckel's Zootomical Museum at the University of Halle, Germany

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**Abstract.** The anatomy collections at the University of Halle, Germany rank among the largest of their kind in Europe. They started as a private collection in the hands of the famous Meckel family of physicians. Johann Friedrich Meckel the Younger (1781-1833), one of the most famous scientists in the first third of the 19th century, expanded the collection to 12.000 items, including normal (human) anatomy, pathological anatomy, and comparative anatomy. For Meckel, the primary function of his collections was to provide material for professional scientific research. In the comparative anatomy collection - formerly Zootomical Museum -, there were also around 5.000 whole specimens preserved in alcohol, including many rare species. Meckel's Museum with its thousands of glass jars and barrels was made to resemble museums of comparative anatomy like Cuvier's in Paris, where circulatory organs of various animals formed one series, respiratory organs another, and so on. The present comparative anatomy collection only partly reflects the original collections in the Zootomical Museum. Several types of changes have occurred: 1. Around 5.000 animals were never catalogued and have probably disappeared from the Zootomical Museum during the 19th century; 2. Around 400 valuable specimens that Meckel had prepared (dismembered skulls mounted on cardboard, organs in different stages of development etc.) were lost in the 20th century; 3. Many mounted skeletons of large mammals, such as rhinoceros and elephant, were removed to gain space, and are now available in dismounted form; 4. Around 400 new specimens have been added, and have sometimes been mixed with original Meckel specimens.

The specimens in the comparative anatomy collection are of great scientific as well as historical value. Many of them would be impossible to replace, if they were lost. Some species are extinct or rare nowadays, and many are protected. It is important to stress that the collection was created with scientific purposes in mind, not in the "Naturalienkabinett" tradition. Therefore, the collection houses several type specimens, and also reflects the progress in morphology and developmental biology in Germany in the 19th century.

## Introduction

Johann Friedrich Meckel the Younger (1781-1833) played an important role for the development of comparative anatomy, developmental biology and fetal pathology in Germany. He became known as the "German Cuvier" already during his lifetime, because of his outstanding knowledge (Göbbel and Schultka 2002a, 2002b, 2003). He translated the *Leçons* published by George Cuvier (1769-1832) and wrote that he wanted to fill a gap in Cuvier´s work, the development of organs and organisms. He wrote that "It is therefore ... my goal to at least try to describe the metamorphoses in the entire animal system, of both single organs and the whole organism, from its first inception until its death." [1]

Meckel built upon Cuvier´s work, and developed it substantially by incorporating not only embryology but also malformations into his comparative anatomical work. He was always looking for ways to enlarge his collections, and spent lots of both energy and money for this purpose. The comparative anatomy collection kept growing into a kind of temple of dissected animal bodies, which was named the "Zootomical Museum".[2] This paper focuses on the subject of animal specimens, which formed the contencts of the Meckel's Zootomical Museum and were also central to medical research and teaching during the nineteenth century. The aim is to readdress the question of medical research and education in Halle and shed light on the anatomical collections, which were among the most important services offered by these institutions.

## Biography

Rudolf Beneke (1934) has written an extensive biography of Johann Friedrich Meckel the Younger. Here we will limit ourselves to a short summary. Johann Friedrich Meckel the Younger was born on October 17, 1781 in Halle an der Saale, Germany, into a family of famous physicians (Beneke 1934, Viebig and Schultka

[1] Translated from Meckel (1810), p. V. German original: "Es war daher anfänglich meine Absicht, wenigstens einen Versuch zu machen, in der ganzen Thierreihe die Metamorphosen, welche sowohl die einzelnen Organe als der ganze Organismus von seinem ersten Entstehen an bis zu seinem Tode erleidet, darzustellen."

[2] see Schultka (1999) for an overview.

1998). His grandfather Johann Friedrich Meckel the Older (1724-1774), a student of Albrecht von Haller (1708-1777), was Professor for Anatomy, Botany and Obstetrics in Berlin, and a member of the Royal Academy. Meckel the Younger´s father, Philipp Friedrich Theodor Meckel (1755-1803), studied in Göttingen and Straßburg. From 1779 until his death he worked as a professor of Anatomy, Physiology, Surgery and Obstetrics in Halle. Philipp Meckel was above all active in medical practice. He made thousands of anatomical preparations, thereby enriching the collection inherited from his father, which became important teaching material.

Johann Friedrich Meckel the Younger often took part in his father´s medical practice, and was present at post-mortem examinations already as a child. He thereby learnt medicine, and in particular anatomy, already at an early age (Meckel 1806 a). In addition, the famous Meckel Collection was housed at home. In 1795 Meckel the Younger started to study at the Domgymnasium in Magdeburg. From the autumn of 1798 he studied medicine at Halle University, where he listened to the lectures in anatomy and physiology given by his father and by Johann Christian Reil (1759-1813). The last two semesters (1801-1802) of his studies Meckel spent in Göttingen, where he heard both clinical and anatomy lectures by Heinrich August Wrisberg (1739-1808). Meckel was also taught comparative anatomy by Johann Friedrich Blumenbach (1752-1840). On April 8, 1802 he obtained a doctoral degree with a thesis on heart malformations: "*De cordis conditionibus abnormibus"*. Thereafter he visited Karl Friedrich Kielmeyer (1765-1844) in Tübingen, and also went to Würzburg and Vienna (Beneke 1934; Jahn 2002). The death of his father on March 17, 1803 put an end to his travels for a while, but in 1804 he resumed his educational travels through Europe. Between 1804 and 1806 Meckel did research in Paris under the supervision of Cuvier in the Jardin des Plantes. Here he improved his knowledge of the natural sciences and came into contact with other contemporary scientists such as Étienne Geoffroy Saint Hilaire (1772-1844), Jean-Baptiste de Lamarck (1744-1829), Alexander von Humboldt (1769-1859) and George Louis Duvernoy (1777-1855). During his stay in Paris, Meckel planned the translation of Cuvier´s five-volume "Leçons d'Anatomie comparée" (1800-1805), which he finished a few years later (1809-1810). Meckel did not just translate Cuvier´s "Leçons" into German, he also added results from his own research as well as new references.

Meckel the Younger became a full professor (Ordinarius) of Anatomy, Surgery and Obstetrics on March 9, 1808 at Halle University, where he would remain for the rest of his life. From 1810 and onwards, his work was focused on anatomy, but in addition he also lectured from 1810 to 1815 on *Zoologiam sive Historiam animalium* and *Historiam naturalem* in the faculty of philosophy in Halle (Göbbel and Schultka 2002a, 2002b).

Meckel´s anatomical collections as well as his handbooks on pathological, human and comparative anatomy were of great importance. The "Handbook of pathological anatomy"[3] (1812-1818) became the standard work on teratology in the 19th century. In his extensive "Handbook of human anatomy"[4] (1815-1820), human embryology occupied the center stage. Through his "System of comparative anatomy"[5] (1821-1833), Meckel presented facts from both medicine and the natural sciences of importance for the teaching of comparative anatomy. Both through the translation of Cuvier´s "Leçons" and through his "System", Meckel´s morphogenetic method influenced morphological research until the end of the 19th century.[6]

Meckel was famous for his work ethic, and remained a hard-working scientist all his life. In 1833, when he was already quite ill, Meckel asked Hinrich Martin Lichtenstein (1780-1857), professor of zoology and Director of the Museum of Zoology in Berlin, to help him with the evaluation of the large bird collection. Meckel was investigating the brain and sensory organs of different animals for the seventh part of his "System of comparative anatomy" when he died on 31 October 1833, only 52 years old (Göbbel and Schultka 2007).

## Short history of the Meckel's anatomical collections

Today the anatomical collections in the department of anatomy and cell biology in Halle contain more than 7.000 specimens. J. F. Meckel the older, while in Berlin, started the collections, which are among the largest in Europe. He made hundreds of anatomical preparations, using e.g. the mercury injection method. During his time, valuable injection and corrosion preparations were made, as well as nerve preparations. He used examples of different malformations, which he had prepared as demonstration specimens, when teaching pathological anatomy.[7] Some specimens are still in the collections, including a complete *Situs inversus*.

Philipp, the son of J. F. Meckel the older, became a professor in Halle in 1779 and brought the collections there.[8] Philipp Meckel added specimens to the collections, so that in the early 19th century it contained 3.476 specimens. This was a mix of dry preparations of e.g. nerves and blood vessels, wet preparations of organs, developmental series, injected blood vessels, corrosions, and bones (including complete human and animal skeletons). He used the collections for medical dissertations and teaching.

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3 German original: "Handbuch der pathologischen Anatomie".

4 German original: "Handbuch der menschlichen Anatomie".

5 German original: "System der Vergleichenden Anatomie".

6 Carl Gegenbaur (1826-1903) used Meckel´s translation of *Leçons d'Anatomie comparée* as well as the *System* as one of his main sources in all three editions (1859, 1870, 1898) of his textbook on the comparative anatomy of vertebrates.

7 See Schultka and Göbbel (2007).

8 See Schultka and Göbbel (2007).

J. F. Meckel the Younger, Philipp´s son, enlarged the human and pathological collections and founded the comparative anatomy collection formerly known as Zootomical Museum. In 1829 Meckel estimated that they contained 16.000 specimens.[9] He used the collections to undertake most of the research that he later described in scientific papers and textbooks.[10] He had a long-standing conflict with the university and ministry, which wanted the collections to be available to the public, while Meckel wanted them to be used exclusively for research.[11] The collections must have anomalies and specimens of immature individuals, including embryos, since affinities could be sought between the early form of one species and the adult form of another.[12] After his death (in 1833) his widow Friederika Meckel sold (in 1836) the private collection to Halle University[13], but it was not until the 1840s that the specimens became part of the collections in the University´s department of anatomy. Because the localities (e.g. Rezidenzgebäude) were in such bad shape, the collections were reduced to about half their original size by e.g. fungal infections and insect pests. Eduard d'Alton (1803-1854), Meckel the Younger´s successor, tried to improve the situation, but lacked the funds to do much.[14] It was only under his successor Alfred Wilhelm Volkmann (1801-1877), that things improved, and the collections were brought into some order, received labels, and were properly catalogued. Only in 1880, when todays anatomy building was opened, did the Meckel collections receive proper exhibition space - 8 large rooms with an area of around 1500 square meters.[15] This happened under the new director Hermann Welcker (1822-1897), who also added his skull collection and specimens important for education to the collections.[16] Valuable Meckel specimens were also renovated under his leadership.[17]

## Catalogues and systematics of the Meckel's Zootomical Museum

The labels we find today on the specimens, and the catalogues, were mostly produced after the Meckel era. Gustav Wilhelm Münter (1804-1870), who worked in the department during Meckel the Younger´s lifetime, produced a catalogue of the comparative anatomy collection and the Zootomical Museum between 1829

[9] See Schwarz (1999), p. 83.

[10] e. g. Meckel's „Abhandlungen aus der menschlichen und vergleichenden Anatomie und Physiologie" (1806), „Beyträge zur vergleichenden Anatomie" (1808, 1809a, 1811, 1812a).

[11] See Göbbel and Schultka (2007).

[12] See Meckel (1806, 1809b, 1810, 1821, 1827)

[13] See Taschenberg (1894), Anlage 13, p. 60.

[14] See Sturm (1997); Zwiener (2004).

[15] See Sturm (1997).

[16] See Göbbel and Schultka (2007); Heller (2007); Schultka and Göbbel (2007).

[17] See Heller (2007).

and 1831.[18] Meckel negotiated with the ministry about the price of the collections, should they buy them, and needed a catalogue to support his case, so maybe Münter acted on his orders.

This catalogue is designed more as a book than a normal catalogue, with 12 chapters and an index (Fig. 1).

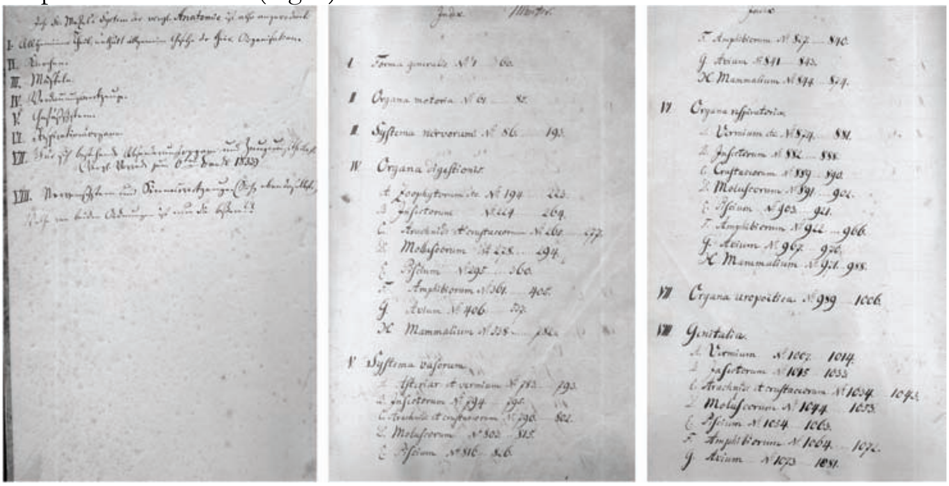

*Fig. 1 Index of the catalogue of the comparative anatomy collection from 1831. Original in the Institut für Anatomie und Zellbiologie, Martin-Luther Universität, Halle-Wittenberg.*

The first part, the "Forma generalis", contains 60 "Positions", mostly containing species names, all of invertebrates, some referring to more than one specimen. Position 41, for example, contains the species name "Doridium maculatum", which refers to 5 specimens. Some positions refer to organ systems, such as position 6, which refers to the species "Vertillum cynomorium" but also to "Digestio. Generatio. Tentacula". The second part of the catalogue - "Organa motoria" - contains 94 positions. These are vertebrates and invertebrates in which the musculature has been exposed. In his textbook "System of comparative anatomy", Meckel describes the locomotory system in three volumes, and the catalogue continues to be very textbook-like.[19] The third part contains 108 positions on nervous system specimens, and the fourth part 589 positions on the digestive system. In the fifth part there are 92 positions of circulatory system specimens, in the sixth 114 positions of respiratory system specimens, in the seventh part 18 position of specimens representing the excretory system, and in the eighth part 128 positions on genitalia. Sensory organ specimens run into 274 positions, and there is a separate chapter on "Historia evolutionis" (embryonic development) in the catalogue with 67 positions.

18 See Kapitza (2004 ), p. 66.

19 See Meckel (1824, 1825, 1828).

**Abstract.** *Fig. 2 One of the specimens in the Meckel Collections, which were "artificially prepared and glued onto cardboard". Text in the accession catalogue: "5471. Sternum & extremities from Chelone mydas. N72". Original specimen in the Meckel collections in Halle.*

The twelfth chapter is on the skeletal system and arranged systematically; 120 species are listed as "Sceleta piscium", 75 species as "Sceleta amphibiorum", 267 species as "Sceleta avium", and 229 species as "Sceleta mammalium". Some skeletons were mounted, some dismembered and mounted on cardboard (Fig. 2). The total number of specimens was larger than 691, because many species were represented by several specimens. This part of the catalogue has an appendix with a list of additional specimens. These were catalogued later, in 1857[20], and the 200 specimens received proper labels in Latin.

In addition to the prepared specimens, many rare species of vertebrates (and also some large invertebrates), were just fixed in alcohol and placed in hundreds large and medium-sized glass jars and in barrels. According to the catalogue these specimens included e.g. pangolins, anteaters, sloths, armadillos, pacas, marsupials, echidnas, exotic birds, ophidiens as well as a very beautiful large horseshoe crab specimen (Fig. 3).

The scientific value of the collections was estimated by the professor of anatomy and physiology, Eduard d'Alton, in a major review.[21] d'Alton wrote that 8.500 of the 12.000 specimens were completed and on show. Of these 8.500 specimens, around 3.000 belonged to normal (human) anatomy, another 3.000 to pathological and 2.500 to comparative anatomy. In addition, around 5.000 animals and their inner organs were kept in alcohol and not prepared.

[20] See "Accessions=Catalog, Tom I; Verzeichnis sämmtlicher anatomischer Praeparate, welche sich im Besitz der Königl. Preuss. Universität zu Halle a/S befinden, nach den laufenden Nummern angelegt. Angelegt von professor Dr. A. W. Volkmann und professor M. S. Schultze. Halle 1857".

[21] See MA Hauptabteilung I. Rep.89 2.2.1, Nr.20560, Folio 12-22; Sturm (1997).

**Abstract.** *Fig. 3* Trachypleus *[*Limulus*]* gigas. *Horseshoe crab of the Family Xiphosura, sometimes called living fossils. From the Pacific Ocean. Indopazifik vor. Original specimen from the Meckel collections.*

When the university had bought the collections in 1836, the ministry ordered d'Alton to make a catalogue covering all specimens in the collections, also those not yet dissected or prepared. This took several years, and a catalogue in Latin, written by Münter for d'Alton, was finished in 1841.[22] However, the part on the comparative anatomy collection is an exact copy of the older work from 1829-1931. Volkmann was not satisfied with the catalogue and made a new "complete" version from 1857 to 1864, in collaboration with his Prosector Max J. S. Schultze (1825-1879).[23] Old labels, some of which are still to be found on some specimens, refer to this version of the catalogue. According to the catalogue, the collection had been reduced from around 12.000 specimens to 7.228 during Eduard d'Alton´s professorship. 3.223 of these specimens are listed in the comparative anatomy collection. The undissected animal bodies (around 5000) were never catalogued and have probably disappeared from the collections during the 19th century.

## Cuvier's "Cabinet d'anatomie comparée", an example of "completeness" and "perfection" for Meckel's planned museum[24]

When Meckel the Younger returned from Paris, he developed a grand plan. He wanted to create a collection that would overshadow Cuvier´s "Cabinet d'anatomie comparée". In Germany, there was nothing like it at the time.

In Paris the museum was founded in 1793, and well known scientists like G. Duvernoy, E. Geoffroy Saint-Hilaire, B. de Lacépède and J. B. Lamarck in zoology, and Cuvier in comparative anatomy, were hired.[25] Cuvier´s collection was catalogued in 1822, and contained 11.486 specimens. Of these, 6.231 were dry preparations and 5.255 were wet specimens.[26] Many osteological specimens were human skeletons and skulls, and 1.500 vertebrate skeletons as well as 1.041 skulls

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22 See „Katalog der ehemaligen Meckelschen Sammlungen. Dritte Abtheilung. Vergleichende Anatomie. wurde von Münter geschrieben und 1840 von d'Alton, auf Richtigkeit geprüft".

23 See "Accessions = Catalog, Tom I; Verzeichnis sämmtlicher anatomischer Praeparate, welche sich im Besitz der Königl. Preuss. Universität zu Halle a/S befinden, nach den laufenden Nummern angelegt. Angelegt von professor Dr. A. W. Volkmann und professor M. S. Schultze. Halle 1857".

24 See Meckel (1809, 1810).

25 See Appel (1987).

26 In 1833 the collection had grown to 13.313 specimens. See Valenciennes (1833).

were also included in the collection. Furthermore, 300 dismembered and mounted skulls, several series of limb bones, and 870 different tooth specimens were in the collection. In addition, the collections contained viscera as dry and injection preparations, branchial arches from different fish species, sterna from different bird species, hyoid bones, feathers, nails, and scales. The wet specimens included 172 muscles, 216 brains, 327 eyes, 220 hearts, 915 mixed organs from different species, 80 fetuses and fetal membranes, 881 dissected molluscs, and 1.097 dissected other invertebrates. Meckel saw this collection as an example of "completeness" and "perfection"[27] and organized his own collections according to Cuvier´s classification scheme. Meckel also made the same type of preparations as he had seen in Paris.

In Paris Cuvier had not only a professorship, but also relatively large sums of money and a large book collection. He also had a number of scientists and students, as well as technicians and taxidermists, at his disposal. In addition he was surrounded by colleagues at the museum, with whom he could collaborate. Cuvier mentions, in the introduction to his *Règne animal*, that he could use Lamarck´s as well as Geoffroy St: Hilaire´s works and expertise, and that von Lacépède´s work was important for his ideas on fish systematics, just to mention some aspects of the importance of the scientific milieu at the museum for Cuvier´s work.[28]

Although Meckel's genius was on a par with Cuvier´s, Halle could definitely not compete with Paris. Meckel faced buildings in disrepair, untrustworthy members of staff, and eternal battles with colleagues.[29] In spite of all these problems, Meckel were to remain in Halle for the rest of his life, and to create magnificent collections. The minister von Altenstein wrote that the collections were "richer and better organized […] than even those in the Jardin de Plants in Paris".[30] Although this might have been an exaggeration, it seems fair to say that Meckel the Younger was the most prominent German scientist in the idealistic evolutionary tradition.

## The origins of specimens in the Zootomical Museum

There are several reasons why Meckel the Younger managed to create collections almost as large as Cuvier´s. One of them was that he inherited animal skeletons from his father, but also his many travels abroad and his activity as a buyer (from hunters, collectors and explorers), as well as support from colleagues, were all important factors.

27 See Meckel (1809, 1821).
28 See Cuvier (1829) Tome I.
29 See e.g. Schwarz (1999), Zwiener (2004).
30 See MA Rep.76 Vf, LitM, Nr.7, Folio 100; Schwarz (1999), p. 88.

Whenever the possibility arose, Meckel travelled. After his stay in Paris he went to Italy in 1806, to Sardinia in 1807, and again to Italy (e.g Naples) in 1810 and 1812, in the company of his brother Albrecht Meckel. In the summer of 1818 he took his wife Friederike to Holland, England and France. In 1819 they visited Vienna, and in 1821 Paris and Cette. To finance his travels Meckel used his private funds, and it was expensive. For his research he needed a large number of both invertebrates and vertebrates, as a main goal was to investigate the development of the organs and organ systems in different types of animals. Without this material, he could not have produced a "System of comparative anatomy". His economic troubles eased a bit only when he could use his scientific reputation, and in particular job offers from abroad (in 1824 and 1828) as leverage. In 1828 Meckel saw it as his duty to express his gratitude to his employer for its support of his efforts.[31] In 1824 his travels to Italy for scientific purposes, and relaxation, received funding, and in 1828 he travelled to Salzburg, in 1829 and 1830 again to Naples, in 1831 to northern Italy, to Trieste and to Switzerland.

In the 1831 catalogue, the year and place of collection was noted, as well as the year of preparation. Therefore, it is possible to follow his collection success at least in part. Meckel also wrote to colleagues asking them to sell or exchange material that he needed for his collections. He often turned to Hinrich Martin Lichtenstein (1780-1857), professor of zoology in Berlin. In 1814 Lichtenstein had taken over the leadership of the Zoological Museum in Berlin, that had been founded by Karl Illiger (1775-1813). In the early 1820s, the Berlin government supported several collection expeditions to different parts of the world, e.g. the Americas and Africa.[32] One of the expeditions in particular, organized by the Berlin Academy of Sciences and lead by Wilhelm Friedrich Hemprich (1796-1825) and Christian Gottfried Ehrenberg (1795-1876), brought a lot of valuable material back to Berlin from northern Africa.[33] The first material reached Berlin in October 1820, and already on 24 October, Meckel wrote to Lichtenstein to remind him that he expected to get fixed material and skeletons from birds, mammals, amphibians and reptiles, "fishes, molluscs and insects, everything that is at all

*Fig. 4 Letter from Meckel to Lichtenstein from October 24, 1820 (Museum für Naturkunde, Humboldt-Universität Berlin, Arbeitsstelle, ZM, S I, Meckel F. 1). Letter 5, p. 9.*

[31] Meckel (1828), p. V.

[32] See letter from Meckel to Lichtenstein (Museum für Naturkunde der HU Berlin, Arbeitsstelle, ZM, p I, Meckel F. 1) (Letter 18, 19, p. 36-39).

[33] The expedition lasted for 6 years, and collected around 46.000 plants, 34.000 animals and 300 minerals. See Landsberg (2001).

possible to get".[34] Other letters were to follow, containing lists of species that Meckel needed (Fig. 4). Meckel often complained that his collections only got the leftovers when the Berlin museum and other German collections had been served. He managed to get lots of material to Halle anyway, and many species mentioned in his letters are still in the collections, including both African and American species (Fig. 5).

Meckel also got material from other colleagues. From Daniel Frederik Eschricht (1798-1863), a danish physician, physiologist and zoologist, he obtained (in May 1828) a specimen illustrating the development of Lister´s river snail *Viviparus* [*Paludina*] *contectus vivipara*] [Millet, 1813] and another illustrating the development of the midwife toad, *Alytes* [Bombinator] *obstetricans* [Laurenti, 1768]. Professor Christian Ludwig Nitzsch (1782-1837) gave Meckel a turtle dove skeleton, *Streptopelia* [*Columba*] *turtur* [*turtica*] [Linné, 1758] and the stomach of a spotted hyena *Crocuta* [*Hyaena*] *crocuta* [*striatae*] [Erxleben, 1777] as a dry preparation. A brown capuccin monkey, *Cebus* [*Simia*] *apella* [Linné, 1758] was a gift from one Dr. Ullrich.

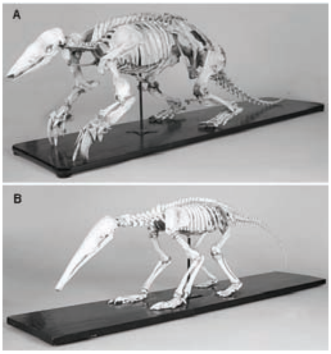


*Fig. 5 A. Skeleton of* Priodontes maximus. *Original specimen from the Meckel collections. The giant armadillo (Tatú) is a mammal in the family Dasypodidae, and is found in South America. It is the largest living representative of its family and listed as "endangered" by the IUCN (International Union for Conservation of Nature). B. Skeleton of giant anteater,* Myrmecophaga tridactyla*, a member in the family Myrmecophagidae.*

Several mammalian skeletons, for example of the bactrian camel *Camelus bactrianus* [*dromedarius*] [Linné, 1758], the American black bear *Ursus americanus* [Pallas, 1870], the brown bear *Ursus arctos* [Linné, 1758], the Asian water buffalo *Bubalus* [*Bos*] *bubalis* (Linné, 1758), and the gray wolf *Canis lupus* [Linné, 1758], came from Stuttgart.

From Paris Mackel obtained the following skeletal preparations: The skeleton of a spider monkey, *Ateles* spec., the skeleton of a Brent Goose, *Branta* [*Anas*] *bernicla* [Linné, 1758], the skeleton of a White-tailed Eagle, *Haliaeetus* [*Falco*] *albicilla* [Linné, 1758], and the skull and limb bones of a Southern two-toed Sloth *Choloepus didactylus* [Linné, 1758].

[34] See letter from Meckel to Lichtenstein (Museum für Naturkunde der HU Berlin, Arbeitsstelle, ZM, p I, Meckel F. 1) (Letter 5, p. 9).

Two specimens are from Holland: The skeleton of a Skua, *Catharacta* [*Procellaria*] [*glacialis*] spec. and the skeleton of a red deer (elk), *Cervus elaphus* [Linné, 1758].

Among other skeletons from large mammals, we would like to mention the fin whale *Balaenoptera physalus* [Linné, 1758] that stranded in 1825 on the Danish coast, and a kangaroo that arrived from Vienna in 1829. Three further skeletons in the collections; an Asian Elephant, *Elephas maximus* [indicus] [Linné, 1758], a Hippopotamus, *Hippopotamus amphibius* [Linné, 1758], and a Rhinoceros, *Rhinoceros unicornis* [indicus] [Linné, 1758], came from London.

The English surgeons Green and Home gave Meckel two specimens of the platypus, *Ornithorhynchus anatinus* [*paradoxus*] [Shaw, 1799]. In 1826 Meckel published his results on the anatomy of the platypus and dedicated his monograph to the Royal Society in London. In this work he desribed mammary glands, a spectacular result that proved that this mysterious animal is a mammal.[35] Therefore the wet specimens of mammary and poison glands, liver, lungs, skin, musculature and the skeleton still present in the collections today are some of it most valuable pieces.

## The Zootomical Museum as a collection for teaching and research

Meckel used his Museum for his publications and medical dissertations (Klunker 2003; Göbbel and Schultka 2007). He dissected thousands animal specimens at the time, when he writing the "System of comparative anatomy". The original intention was that this work was to contain all organ systems and all groups of animals. Meckel writes in the foreword to the third volume in 1828 that the "coming volumes" will appear "quickly and without delay", because he already has the specimens needed, and has performed the necessary investigations, and even started to edit.[36] Meckel had problems getting all his planned publications finished. The volumes on the nervous system and the sensory organs, as well as on the urogenital system, were never published. In 1833, right after the printing of the seventh volume, Meckel died. In the catalogue, there are specimens from many species, covering also the unpublished organ systems. Probably, a large number of specimens are from the time period when Meckel was translating Cuvier´s "Leçons d'anatomie comparée" into German. Is it clear from the many additions and notes that Meckel used both the collections in Paris and his own material to be able to understand, and thereby translate, the work better.

35 See Meckel (1826).

36 See Meckel (1825).

## The Meckel's Zootomical Museum today

The "Global Taxonomic Initiative" and "Biodiversity" are concepts that have inspired new research and documentation projects to both enlarge and improve the care of collections in museums in many countries, and Germany is no exception. At the Institut für Anatomie und Zellbiologie in Halle, it was also necessary to renovate many of the specimens in the Meckel collections.[37] Between 1996 and 2001 the specimens in the comparative anatomy collections were restored and rearranged (Fig. 6). Between 1999 and 2003 all specimens were catalogued anew, and the taxonomy revised and adapted to present scientific standards. A computerised catalogue was created, and new labels with species name, trivial name, and a reference to the old catalogues were made. It was important to keep all the information present on old labels, skeletal parts etc., and bring this into the new system. The wet collection caused special problems, because many glass jars contained several individuals or specimens. It was not always clear whether specimens in the same jar also had a common history and belonged together. Often the specimens themselves were the only information carriers, because the labels had been lost. We were then not only were faced with making a new identification of the specimen, but also with trying to understand its history and how it had entered the collection. For this we used the original works of J.F. Meckel t. Y. and other anatomists who had curated the collections in the 19th century, the dissertations written by Meckel´s students, and the secondary literature on morphological questions in the 19th century. The archives in the department and at Halle University proved to be important sources, as well as the 38 letters from Meckel to H. M. Lichtenstein that have been preserved.

The inventory of the animal specimens gave the following results. Of the once 3.223 animal specimens, who belonged to the comparative anatomy collection between 1854 and 1864, 3.036 still exist today. Of those invertebrate and vertebrate specimens, 1.974 are dry preparations in the form of complete skeletons, skulls, limb bones, teeth, tracheas, integuments etc., and 1.062 are glass jars containing either the entire animals or organs, organ systems, embryos and body parts such as single limbs etc.[38] So the losses are relatively small and a more precise analysis shows that almost all 392 original preparations (e.g., dismembered skulls and mounted on cardboard, dry viscera, branchial arches, sterna, hyoid bones, scales) which were labelled in Latin by Meckel the Younger and later exhibited in the Residenzgebäude in room N, have been lost, and little else.

1.958 specimens have labels with scientific names, often also trivial names and accession numbers. Many loose labels were found in the collections, so that it can

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[37] See Sturm (1997), p. 93.

[38] See "Accessions = Catalog, Tom I; Verzeichnis sämmtlicher anatomischer Praeparate, welche sich im Besitz der Königl. Preuss. Universität zu Halle a/S befinden, nach den laufenden Nummern angelegt. Angelegt von professor Dr. A. W. Volkmann und professor M. S. Schultze. Halle 1857". The specimens in positions 5297 to 5722 are only sparsely present in the present collections.

be assumed that also specimens without labels really belong to the Meckel collections. The labels for the vertebrates were written by Münter, who made an enormous number of preparations.[39] On these one often finds his name or the names of other taxidermists or scientists, and the year in which the specimens were prepared or restored. There is seldom any information on the source of specimens. The glass jars with invertebrates carry labels with series and number, which refer to a catalogue, whose nomenclature shows that it was made around 1900. It remains, however, unclear who the author (or authors) was. Also the invertebrates belonged to the Meckel collections, and this collection might contain valuable type material.

For most of the specimens from J. F. Meckel the Younger´s active period, we have been able to determine when they were added to the collections. Although 5/6 of the collections are from Meckel the Younger´s Zootomical Museum, it is remarkable that no specimen can safely be ascribed to the active periods of J. F.

*Fig. 6 The comparative anatomy collection - formerly Meckel the Younger´s Zootomical Museum - in the anatomical collections of the Institut für Anatomie und Zellbiologie, Martin-Luther Universität, Halle-Wittenberg.*

Meckel the older or Philipp Friedrich Theodor Meckel. Of the 18 specimens that are labelled as "old" in the accession catalogue, only 3 have been identified. What exactly "old" means in this context remains unclear. We were also able to identify specimens from the post-Meckel era: 65 are from E. d'Alton´s time as director, 89 from A. W. Volkmann´s directorate, 89 can be safely ascribed to H. Welcker´s time as director, and 88 specimens are from the 20th. century. In 56 glass jars, containing invertebrates, fishes, reptiles, birds and mammals, many specimens are present that have so far not been determined down to the species level. Some glass jars contain specimens from the fuana of Suriname. They were bought in 1851 by Dr. Deutschbein. Moreover, hundreds of glass jars containing organs or organ systems have no labels whatsoever. In these cases only a rough determination at a high taxonomic level is possible, because important species characters are missing.

[39] See Kapitza (2004).

Among these specimens are probably also some, which earlier were kept in barrels or larger containers, and had not yet been prepared by Meckel.

**Acknowledgments.** We wish to express our sincerely thanks to Dr. H. Landsberg from the Museum für Naturkunde der Humboldt-Universität Berlin for placing the valuable letter collection (Meckel to Lichtenstein, Arbeitsstelle, ZM, p I, Meckel F, Letter 1-38) at our disposal.

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# Maria Sibylla Merian, Baltasar Scheid und Richard Bradley - Die Künstlerin und Naturforscherin, ein Kaufmann und ein Botaniker.

*Brigitte Wirth*

**Abstract.** This paper describes the contacts of a Dutch trader and of an English botanist to Maria Sibylla Merian, the famous artist and natural scientist. She published her observations of the metamorphosis of butterflies and moths in three books: Der Raupen wunderbare Verwandelung und sonderbare Blumennahrung in quarto (I, Nürnberg 1679; II, Frankfurt/M. 1683); a third part she prepared but died before publication (Amsterdam 1717, posthum); and the famous Metamorphosis Insectorum Surinamensium in folio (Amsterdam 1705). Merian lived since 1691 in Amsterdam. The Amsterdam trader Baltasar Scheid wrote in several letters to J.G.Volkamer about Merian's Metamorphosis, desribing in a few words its development from the early beginning to the end. With the details given by Scheid it is possible to specify the time of production of this book. Comparing the letters of B.Scheid with those of M.S.Merian who also wrote to Volkamer, it is obvious that the person named „Schrey" or „Schey" by her and who was up to now not identified (he had to send natural produce and new printed copperplates of her Metamorphosis to Volkamer), is the Amsterdam trader Baltasar Scheid. It was the London apothecary James Petiver (Merian had good connections to him) who helped the English botanist Richard Bradley to come in contact with M.S.Merian. Bradley probably ordered at least one painting for his Historia plantarum succulentarum (London 1716-1727) from Merian. One of the copperplates in this work looks very similar to one of Merian's aquarells. On his journey to Holland Bradley visited Merian 1714 (three years after Petiver) in Amsterdam. From there he wrote enthusiastically to Petiver about all her original paintings and mentioned the prices Merian asked for. Prices for original paintings of Maria Sibylla Merian were not yet known, only those for her printed books.

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# 1 Einleitung

Von der Künstlerin und Naturforscherin Maria Sibylla Merian (1647 – 1717) sind bislang achtzehn Briefe aufgefunden worden.[1] Geschrieben wurden sie in der Zeit von 1682 bis 1712 in Frankfurt und Amsterdam. Adressaten waren eine ehemalige Schülerin ihres Malzirkels in Nürnberg, Clara Regina Imhoff, der Nürnberger Arzt und Botaniker Johann Georg Volkamer (1662 – 1744) sowie der Londoner Apotheker und Naturforscher James Petiver (1664 – 1718). Ein spätes Schreiben ist an einen nicht näher bekannten Christian Schlegel in Rastatt gerichtet. Alle Briefe sind Geschäftsbriefe, in denen es hauptsächlich um die Vermittlung von Farben, Tierpräparaten und Merians druckgraphischem Werk geht. An C.R. Imhoff sind einige wenige persönlichere Zeilen geschrieben, Volkamer und Petiver bittet sie einmal um Rat bezüglich ihres „Surinambuches".

In den Briefen an J.G. Volkamer, geschrieben in Amsterdam in den Jahren von 1702 bis 1705, wird mehrfach der Name „Schrey" oder auch „Schey" genannt.[2] Dieser Herr Schey hatte es offensichtlich übernommen, zum Verkauf anstehende Naturalien von Merian an Volkamer zu übermitteln.[3] Ebenso leitete er einzelne, gerade fertiggestellte Druckblätter von Merians *Metamorphosis Insectorum Surinamensium* nach Nürnberg weiter, sowie 1705 das komplette Druckwerk in einer holländischen und einer lateinischen Ausgabe.[4] Volkamer hat umgekehrt über Schey einen Brief der Jungfer Auer, einer ehemaligen Nürnberger Schülerin Merians, an sie weiterleiten lassen.[5] – Pfister-Burkhalter weist auf die mehrfache Erwähnung von Schey oder Schrey in Merians Briefen hin, macht aber keine weiteren Angaben zu der Person.[6]

In der Handschriftenabteilung der Universitätsbibliothek Erlangen befinden sich 44 Briefe eines Amsterdamer Kaufmanns mit Namen Baltasar Scheid, einige davon auch mit Schayd unterzeichnet. Die Briefe stammen aus der Briefsammlung des Nürnberger Arztes Christoph Jacob Trew (1695 – 1769) und wurden geschrieben in den Jahren von 1699 bis 1708.[7] Sie sind in deutscher Sprache abgefaßt und von der Verfasserin dieser Arbeit transkribiert worden. Adressat ist interessanterweise Johann Georg Volkamer in Nürnberg, der auch Empfänger von Merians Briefen war. Unter diesen 44 Briefen befinden sich vierzehn, in denen Scheid Maria Sibylla Merian erwähnt[8], darunter zwölf aus der Zeit von 1703-1705,

[1] Katalog Frankfurt/M. (1997), S. 262-269.
[2] Ebd., Briefe 7, 8, 13, 14, S. 264-268.
[3] Ebd., Briefe 7 und 8, S. 264-265.
[4] Ebd., Briefe 13 und 14, S. 267-268.
[5] Ebd., Brief 14, S. 267/268.
[6] Pfister-Burkhalter (1980).
[7] Universitätsbibliothek Erlangen, Briefsammlung Trew - Baltasar Scheid - 1 - 44.
Einige der Briefe haben durch Mäusefraß gelitten. – Im weiteren Verlauf werden diese Briefe im Text mit (Bf. x) zitiert.
[8] Ebd., B.Scheid, Briefe 18, 19, 21, 23-27, 29-32, 42, 43.

in denen er, wenn auch nur kurz, über Merians in der Entstehung begriffene Werk *Metamorphosis Insectorum Surinamensium* berichtet.[9] Ebenso schreibt er über die Arbeiten an der *D'Amboinsche Rariteitkamer* von Georgius Everhardus Rumphius (1627/28 – 1702), zu der Merian Vorlagen angefertigt hat.[10] Dieses Werk wurde wie die *Metamorphosis* 1705 in den Handel gebracht.

Ein Vergleich der Schreiben Merians mit denjenigen Scheids aus den Jahren 1703 – 1705 zeigt, daß es sich bei dem von Merian genannten Herrn Schey oder auch Schrey um den Amsterdamer Kaufmann Baltasar Scheid handelt.

Die sieben bisher bekannten Briefe Maria Sibylla Merians an den Londoner Apotheker und Naturforscher James Petiver belegen, daß Merian lebhafte geschäftliche Kontakte nach England gehabt hat.[11]

Bei der Durchsicht der Arbeit von Blanche Henrey[12] wurde erstmals M.S.Merian im Zusammenhang mit dem englischen Botaniker Richard Bradley (1688 – 1732) erwähnt gefunden. Ein dort zitierter Satz aus einem Schreiben Bradleys an James Petiver erweckte den Eindruck, daß Merian für Bradley eine Auftragsarbeit ausführen sollte. Für B.Henrey war die eher flüchtig erscheinende Verbindung zwischen Bradley und Merian wahrscheinlich nicht von Interesse und wurde demzufolge nicht weiter verfolgt.

Durch Henrey auf die Korrespondenz zwischen Bradley und Petiver und auf die Möglichkeit einer Merianschen Arbeit für Bradley aufmerksam geworden, wurde die Arbeit von Will Tjaden herangezogen, der in den Jahren von 1973 bis 1976 den gesamten Briefwechsel zwischen Richard Bradley und James Petiver in 18 Folgen veröffentlicht hat.[13] Tjaden verneint die Möglichkeit einer Auftragsarbeit von Merian für Bradley. Er ist der Meinung, Bradley, welcher oft in finanziellen Schwierigkeiten steckte, habe zur Finanzierung einer solchen Arbeit kein Geld gehabt.[14] Bradleys Briefe an Petiver zeigen jedoch, daß er sich häufiger Geld borgte, um seine Vorhaben verwirklichen zu können. Es besteht also durchaus die Möglichkeit, daß Bradley Merians Arbeit mit geliehenem Geld bezahlte. - Gordon Rowley vermutet, Zeichnungen Merians könnten als Vorlage zu Bradleys *Historia plantarum succulentarum* (1716-1727) gedient haben, geht aber nicht näher darauf ein.[15]

Merian hat bekanntlich Auftragsarbeiten ausgeführt. Sie trugen neben dem Verkauf ihrer eigenen Werke und dem Handel mit Naturalien und Farben sowie dem Unterweisen im Malen wesentlich zu ihrem Lebensunterhalt bei. Bekannt sind

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[9] Ebd., B.Scheid, Briefe 18, 19, 21, 23-27, 29-32.
[10] Ebd., B.Scheid, Briefe 6, 18, 20, 21, 23-26, 29, 30.
[11] Kat. Frankfurt/M. (1997), Briefe 9-12, 15, 16, 18, S. 266 - 269.
[12] Henrey (1975), Bd.2.
[13] Tjaden (1973 – 1976).
[14] Derselbe, Part 5, S. 57, in: Bull. of the ASPS, Vol. 9 (1974).
[15] Rowley (1987).

jene Arbeiten für die Amsterdamer Patrizierin Agnes Block (1629 – 1704)[16], aber auch die Erstellung der Vorlagen zur *D'Amboinsche Rariteitkamer*. Die Annahme einer Auftragsarbeit nach England wäre also durchaus möglich, zumal Bradley nach seinem Besuch bei Merian im Juli 1714 Petiver darüber informiert, daß diese bereit sei, jeden Bildwunsch bei korrekter Bezahlung ausführen zu wollen.[17] Merian hat demnach bis mindestens Juli 1714 Auftragsarbeiten angenommen und ausgeführt, muß somit körperlich noch in relativ guter Verfassung gewesen sein, denn Bradley schreibt nichts von einer kranken Künstlerin, hätte dies aber sicherlich Petiver mitgeteilt. Auch ein letzter Brief aus Amsterdam vom Oktober 1714 enthält keinerlei derartige Mitteilung[18], der Schlaganfall, den Merian erlitt, trat erst danach ein.

Über eine Verbindung zwischen Maria Sibylla Merian und Richard Bradley steht in den Biographien zu Merian nichts geschrieben, in denen zu Bradley wird eine solche nur bei B.Henrey, W.Tjaden und G.D.Rowley erwähnt.

Es wird im Weiteren der Frage nachgegangen, ob Merian einen Auftrag von Bradley aus England angenommen und ausgeführt haben könnte. Das Zustandekommen des Auftrages hing von James Petiver ab, der als Vermittler hinzugezogen worden war. - Bradleys Interesse galt im Übrigen nur der Künstlerin, nicht der Naturforscherin Merian. Petiver hingegen war sowohl an der Künstlerin als auch an der Naturforscherin und auch der Naturalienhändlerin Merian interessiert.

## 2 Maria Sibylla Merian (1647-1717): Leben und Werk

Maria Sibylla Merian, Tochter des Kupferstechers und Verlegers Matthäus Merian d.Ä. (1593-1650) und seiner zweiten Frau Johanna Sibylla Heim, hat die Malerei, das Kupferstechen und die Herstellung von Farben in der Werkstatt ihres Stiefvaters Jacob Marell (1614-1681) erlernt. Nach der Heirat 1665 mit dem Nürnberger Architekturmaler und –graveur Johann Andreas Graff (1637-1701), Schüler Marells, geht sie mit diesem 1670 nach Nürnberg, der Heimatstadt Graffs. Der Ehe entstammen zwei Töchter: Johanna Helena (1668-?) und Dorothea Maria (1678-1743). In Nürnberg erscheinen ihre ersten Werke: *Florum fasciculus primus* (1675), *Florum fasciculus alter* (1677) und *Florum fasciculus tertius* (1680), die drei zusammengefaßt 1680 unter dem Titel *Neues Blumenbuc*h. Diese Florilegien bestanden aus je zwölf Kupfertafeln ohne begleitenden Text und waren angelegt als Vorlage zu Stickereien oder auch zum Nachzeichnen wie es im Vorwort zum *Neues Blumenbuch*[19] heißt. Zur gleichen Zeit hat sie an dem Werk gearbeitet, das sie

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[16] Segal (1997); van Gelder (1997); Schmidt-Loske (2004) und (2007).
[17] Tjaden, Part 4, S. 9/10, in: Bull. of the ASPS, Vol. 9 (1974).
[18] Tjaden, Part 12, S. 76 - 77, in: Bull. of the ASPS, Vol. 10 (1975).
[19] Faksimile (1999).

sofort nach Erscheinen auf dem Buchmarkt berühmt macht, *Der Raupen wunderbare Verwandelung und sonderbare Blumen=nahrung*, 1679 herausgegeben. Es beruht auf ihren eigenen Beobachtungen der Insektenmetamorphose, enthält 50 Kupfer mit den Entwicklungsstadien des Insektes vom Ei bis zur Imago und den zugehörigen spezifischen Futterpflanzen; zu jeder Abbildung gehört ein detaillierter beschreibender Text über Art, Dauer und Besonderheit der Entwicklung. – Ein zweiter Band erscheint in gleicher Aufmachung 1683 in Frankfurt[20], auch für die späteren Werke behält sie diesen Stil bei.

Nach der Trennung von ihrem Mann – die Ehe wird 1692 in Nürnberg auf Verlangen Graffs geschieden – geht sie 1685 in Begleitung ihrer verwitweten Mutter und beider Töchter, die von ihr künstlerisch ausgebildet werden, nach Friesland, wo sie für sechs Jahre in der Gemeinschaft der Labadisten, einer pietistischen Vereinigung lebt, von der sie sich aber nach dem Tod der Mutter 1691 löst. Sie siedelt im gleichen Jahr mit den Töchtern nach Amsterdam über. Von dort aus tritt sie 1699, angeregt durch die Schönheit der in Raritätenkabinetten gezeigten tropischen Insekten und von dem Wunsch getrieben, deren Entwicklung vor Ort zu beobachten und festzuhalten, mit der jüngsten Tochter Dorothea Maria die Reise nach Surinam an, um dort zwei Jahre lang das Insektenstudium zu betreiben. Diese von den beiden Frauen ohne männliche Begleitung unternommene Forschungsreise war für die damalige Zeit unglaublich und einzigartig. Wie sie selbst im Vorwort zu ihrem Hauptwerk sagt, konnte sie nur unter großen Schwierigkeiten arbeiten, die klimatischen Verhältnisse setzten ihr so stark zu, daß sie den Aufenthalt vorzeitig, da schwer krank, abbrechen muß.

Nach ihrer Rückkehr im Jahr 1701 erstellt sie auf Pergament die Vorlagen zu ihrem Hauptwerk *Metamorphosis Insectorum Surinamensium*, die Kupferplatten läßt sie stechen, drei Platten weisen keine Stechersignatur auf und stammen vermutlich von ihr selbst. Das Werk erscheint 1705 in einer holländischen und einer lateinischen Ausgabe[21]. Finanziert hat sie dieses Werk über Subskription und unter anderem durch die Annahme eines Auftrages, Vorlagen zu der *D`Ambionsche Rariteitkamer* von G.E. Rumphius zu erstellen. – 1713/14 gibt sie die beiden Teile des *Raupenbuches* in holländischer Sprache heraus und arbeitet an einem dritten Teil, der posthum nach ihrem Tod im Todesjahr 1717 von der Tochter Dorothea Maria in Druck gegeben wird. Anfang 1717 stirbt sie in Amsterdam, einige Jahre zuvor hatte sie einen Schlaganfall erlitten.

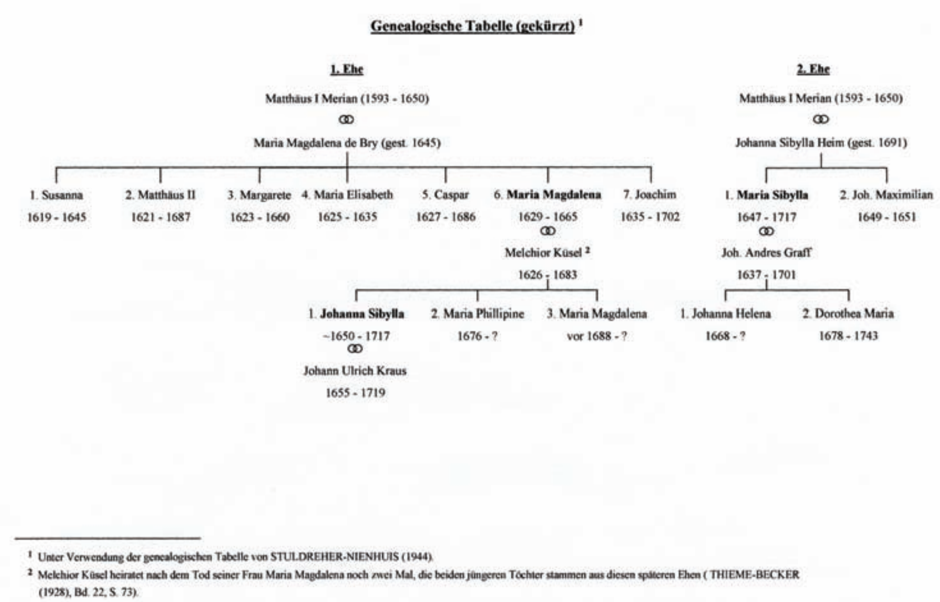
Allgemein wird davon ausgegangen, daß Merian zu ihren Halbgeschwistern – den Kindern aus der ersten Ehe von Math.Merian d.Ä. mit Maria Magdalena de Bry; ihr einziger leiblicher Bruder starb als Kleinkind – keine Beziehungen pflegte. Alleinige Ausnahme bildet Caspar Merian, dem sie nach ihrer Trennung von Graff 1685 nach Friesland zu den Labadisten folgt, wohin sich Caspar bereits einige

20 Merian (1679) und (1683).

21 Merian (1705).

Jahre zuvor begeben hatte. Dieses Nach-Ziehen setzt eine starke innere Verbundenheit voraus.

Man sollte aber auch Merians Brief vom 3.Juni 1685 aus Frankfurt an Clara R. Imhoff in Nürnberg beachten, in dem sie kurz vor der Trennung von Graff schreibt:"... und weillen Sich dan jetzt diese gelegenheit begeben hat, dasz dieser herr Krauss, unser gutter freundt, nacher Augspurg, und Also durch Nürnberg Reisen wirdt, ...".[22] – Bei dem genannten „Herrn Krauss" handelt es sich um den Augsburger Kupferstecher und Verleger Johann Ulrich Kraus (Krauß, Krauße, Kraußen) (1655-1719), der 1685 Johanna Sibylla Küsel (Küsell, Küssell, Kiesel) (ca.1650-1717), Enkeltochter von Math.Merian d.Ä. und somit Nichte von M.S.Merian, geheiratet hat.[23] J.S Küsel ist eine Tochter von Maria Magdalena Merian (1629-1665) und Melchior Küsel (1626-1683), einem ehemaligen Schüler von Math. Merian d.Ä.. Maria Magdalena stammte aus Merians erster Ehe (Tab.1).




Küsel war Augsburger und ging mit seiner Frankfurter Frau nach Merians Tod 1650 in seine Geburtsstadt zurück.[24] Da M.S.Merian Kraus namentlich erwähnt und als „guten Freund" bezeichnet, besteht unter Umständen die Möglichkeit, daß es zwischen der Familie Graff in Nürnberg und der Familie Küsel in Augsburg Verbindungen gegeben hat. J.S.Küsel war fast im gleichen Alter wie ihre Tante M.S.Merian. Ein Austausch zwischen beiden Frauen ist denkbar, beide waren sie

[22] Kat. Frankfurt/M. (1997), Brief 5, S. 264.
[23] Thieme-Becker (1927), Bd.21, S. 439 - 443.
[24] Ebd., (1928), Bd.22, S. 73 - 75.

künstlerisch tätig: Johanna Sibylla Küsel schuf zwar keine eigenständigen Arbeiten wie Maria Sibylla Merian, galt aber als eine außerordentlich geschickte Kupferstecherin, die mit ihrem Vater zusammenarbeitete, später auch mit ihrem Mann Johann Ulrich Kraus.[25] Auch zwischen den beiden Ehemännern ist ein beruflich bedingter Kontakt vorstellbar. Kraus wird die Verbindung zu Joh.Andreas Graff auch nach der Trennung der Eheleute Graff aufrechterhalten haben, das zeigen seine zahlreichen Arbeiten, die er nach Werken von Graff angefertigt hat.[26] – Schriftliche Unterlagen, die von einer Verbindung zwischen Nürnberg und Augsburg zeugen, konnten nicht aufgefunden werden.[27]

# 3 Baltasar Scheid, die Metamorphosis Insectorum Surinamensium von M.S.Merian und die D'Amboinsche Rariteitkamer des E.G.Rumphius.

## 3.1 Baltasar Scheid (ca. 1660-ca.1724): Versuch einer Biographie.

Über den Amsterdamer Kaufmann Baltasar Scheid ist wenig in Erfahrung zu bringen.[28] Der Frankfurter Gelehrte Zacharias Conrad von Uffenbach (1683-1734) suchte ihn am 26.März 1711, einige Wochen nach seinem Besuch bei Maria Sibylla Merian, in Amsterdam auf und berichtet darüber in seinem Reisetagebuch: „Den 26.Mart. Morgens waren wir bey Herrn Balthasar Scheid von Straßburg. Er hat ein groß Cabinet, so über fünfzig Schubladen hatte, in welchen eine große Menge von Conchylien ... Er sagte, daß er damit handle ...Er handelt auch mit Blumen ...“.[29] Von Uffenbach wird den Kaufmann Baltasar Scheid auf den gleichnamigen Theologen Balthasar Scheid (1614-1670) aus Straßburg angesprochen und nach einer möglichen Straßburger Abstammung gefragt haben. Der Urschrift seines Reistagebuches, die sich in der Handschriftenabteilung der Staats-und Universitätsbibliothek Göttingen befindet, kann man entnehmen, daß B.Scheid „ein Mann bey 50. Jahren und gar höfflich“ sei.[30] Als Geburtsjahr ist demzufolge etwa 1660 anzunehmen.

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[25] Stetten (1779), S. 386/387; Huber (1796), Bd.I, S. 303; Thieme-Becker (1928), Bd.22, S. 74.

[26] Stetten (1779), S. 393/394; Huber (1796), Bd.II, S. 18; Thieme-Becker (1927), Bd.21, S. 442.

[27] Staatsbibliothek zu Berlin, Preußischer Kulturbesitz, Handschriftenabteilung, Zentralkartei der Autographen; Stadt Augsburg, Stadtarchiv.

[28] Weder das Zentrum für Niederlande-Studien, Münster, noch das Algemeen Rijksarchief und auch nicht das Centraal Bureau voor Genealogie, beide Den Haag, haben Nachweise zu Baltasar Scheid. Auch in der Zentralkartei der Autographen, Berlin, befinden sich keine Nachweise.

[29] Uffenbach (1754), Teil 3, S. 683 - 684.

[30] Staats- und Universitätsbibliothek Göttingen, Handschriftenabteilung, Nachlaß Uffenbach, 8° Cod. Ms. Uffenbach 25, Bd.3, S.703. – Die Altersangabe zu Scheid wurde nicht in den Druck von 1753/54 übernommen.

In den Briefen Scheids finden sich einige wenige Angaben zu seiner Person. Über sein Geschäft teilt er Volkamer, den er bei dessen Amsterdamer Aufenthalt 1697 kennengelernt hatte (Bfe.6 u. 16), in einem Schreiben vom 19.Jan. 1700 nach Nürnberg mit: „Sonsten habe mit diesen berichten wollen, wie daß ich meine Companie Handlung den ersten Januar habe angefangen..." (Bf.2). Er handelt vorwiegend mit den damals so sehr begehrten Naturalien aus Übersee. Seine Arbeit ist gewissenhaft: vor jedem Versand werden alle Insekten auf Unversehrtheit und Ungeziefer untersucht (Bf.11). Nach dem Ausscheiden eines Mitarbeiters im Jahr 1703 führt er das Geschäft alleine weiter (Bf.16). 1707/08 muß Scheid über einen längeren Zeitraum krank gewesen sein, erst im Mai 1708 antwortet er auf einen Brief Volkamers vom Oktober 1707 und entschuldigt sich für sein langes Schweigen. Er schreibt, daß ein Freund „auch krank gelegen" habe, daher kann „Daß mir zugestoßene Unglück, wodurch ich durch Gottes Hülffe meistens schon gerettet" als eine schwere Krankheit gedeutet werden (Bf.40). Durch diese Krankheit hat Scheid offenbar große finanzielle Verluste erlitten, er ist Volkamer dankbar, „Daß mich E.E. bey ein und andern Freunden wollen recommendieren", und „durch Gottes Hülffe sollen baldigst alle meine Creditores vollkommen bezahlt sein ... Ich soll nicht sorgen Dominus providebit" (Bf.42). Scheid schreibt, obwohl ausgesprochen mitteilsam, nichts von einer Frau oder Familie, wahrscheinlich war er nicht verheiratet. Dagegen berichtet er lebhaft über namhafte Naturforscher Amsterdams, die zum größten Teil nicht nur Kunden für ihn waren, sondern zu denen er auch persönlichen Kontakt hatte. Auch politische Ereignisse wie der Spanische Erbfolgekrieg werden kommentiert. Deutlich zeigen sich die Schwierigkeiten eines Naturalienhändlers, der während der Seeblockade vergeblich auf die seinen Kunden versprochene Ware wartet.

Seine Ware gibt Scheid entweder Reisenden mit, was für ihn am kostengünstigsten ist, oder er sendet sie mit dem Postwagen. Hierbei benutzt er teilweise die Strecke via Hamburg, von wo das Stückgut von einem Herrn Mancken nach Nürnberg weitergeleitet wird, oder aber die Verbindung über Frankfurt/M. und auch die neue Linie über Osnabrück- Meiningen- Coburg nach Nürnberg.

Scheids gute Kontakte zu Caspar Commelin (1667 - 1731), Leiter des Hortus Medicus in Amsterdam, ermöglichen es ihm, von Volkamer gewünschte Samen und Pflanzen aus dem Hortus Medicus nach Nürnberg zu senden. Gleichzeitig leitet er Pflanzen von Volkamer an Commelin weiter.

Der letzte erhaltene Brief Scheids (ohne Datum, er wird dem Jahr 1708 zugeordnet) zeigt noch einmal einen Einblick in Volkamers Sammelleidenschaft. Commelin, so schreibt Scheid, wird schwerlich noch mehr Pflanzen herausgeben „dann selbiger mir vermeldet, wie daß er alles was der H.Dr. verlangt Hat, und im Horto Medico war, er Ihme dieses mahl zugesandt.". Es folgt eine längere Auflistung von Raritäten aus Ost- und West-Indien, „der Herr Doctor melde mir seine Gedancken darüber, und berichte was Ihm etwa noch von fremden insectis mangelt, weilen ich wohl dörffte Gelegenheit praesentiren, umb noch ein und das ander in dero Kunstkammer zu suppliren." (Bf.44). Aber auch schon früher schreibt er: „Alles was ich zu dero Gartenlust werde wißen zu contribuiren, soll ich jeder Zeit darin zu dienen parat haben." (Bf.16). Als Agnes Block, einst Auftraggeberin von Merian und in zweiter Ehe verheiratet mit Sybrand de Flines, 1704 stirbt, wird sofort der Katalog über die vielen exotischen Pflanzen, die diese Sammlerin zusammengetragen hatte und die nun zum Verkauf anstehen, nach Nürnberg geschickt (Bf.27).

Der Kontakt zu Volkamer geht aber über das rein Geschäftliche hinaus, wenn dieser ihm zu Weihnachten Nürnberger Lebkuchen zukommen läßt, die „mit guten Freunden auff deß Hern D^r Gesundheit verzehrt werden" (Bf.6) und Scheid zum Weihnachtsfest nach Nürnberg hin ankündigt: „gantz fein hell blatjes Tabac soll der Herr Doctor bekommen, und recht gute Soccolada" (Bf.9).

Die Briefe Scheids enden fast alle mit: „Wormit gantz freundl. salutirt u. Göttl. Gnad. Schutz befohlen bin ich des Herren ... Baltasar Scheid.".

Einem Schreiben Albert Sebas vom Februar 1724 kann man entnehmen, daß Baltasar Scheid noch lebt, allerdings krank und schwach ist.[31] Über sein Todesjahr ist nichts bekannt, es kann das Jahr 1724 vermutet werden.

## 3.2 „Mad. Merian avancirt wacker mit ihrem Werck": Baltasar Scheid, die *Metamorphosis Insectorum Surinamensium* von M.S.Merian und die *D'Amboinsche Rariteitkamer* des E.G.Rumphius: Briefe nach Nürnberg.

Die Briefe Baltasar Scheids sind die eines Händlers, der seine vorwiegend aus den fernen niederländischen Kolonien stammende Ware anpreist, Raritäten wie „extra schöne Colubritgens" besonders hervorhebt, sich stets dienstbereit zeigt und immer bemüht ist, seine Kunden so rasch wie möglich mit den neuesten Nachrichten bezüglich Naturalien aber auch Tagesgeschehen und Bücherneuheiten zu versorgen. Dabei sind es besonders zwei Werke, über deren Entstehungsprozeß er immer wieder nach Nürnberg an seinen Kunden Volkamer berichtet, welcher offenkundig ein großes Interesse am Erwerb dieser Bücher hat. Es handelt sich um die *D'Amboinsche Rariteitkamer* von G.E.Rumphius und die *Metamorphosis Insectorum Surinamensium* von M.S.Merian. Das erste entstand unter der Mitarbeit von M.S.Merian, das zweite war Merians eigenes Werk über ihre Forschungsergebnisse in Surinam.

Im Februar 1701 setzt er Volkamer davon in Kenntnis, „ ... daß mit nechstem allhier wird ein schönes buch so wohl im Lateinischen als Nieder Teutschen wird herauß kommen, Tituliert, Ambonische Kunst u. raritäten Kammer, oder Cabinet darinnen aller hand rares u. zu vor in Teutschland unbekandtes ... " (Bf.6). Scheid nennt weder Verfasser noch Verleger, beschreibt aber in wenigen Worten, daß dieses Werk von Muscheln, Seepflanzen, Krebsen und Steinen handelt und alles in Kupfer gestochen werden wird. Zu diesem Zeitpunkt – Anfang 1701 – befindet

[31] Universitätsbibliothek Erlangen, Briefsammlung Trew - Albert Seba - Brief 34.

sich M.S.Merian noch in Surinam, von wo sie im September desselben Jahres nach Amsterdam zurückkehren wird.

Der Autor der *D'Amboinsche Rariteitkamer* ist Georgius Everhardus Rumphius (1627/28-1702), gebürtiger Hanauer, der für die Verenigde Oost-Indische Compagnie (VOC) auf Amboina tätig war und in den Jahren seines dortigen Aufenthaltes unermüdlich Naturalien sammelte, zeichnete und beschrieb. Seine Aufzeichnungen – nach seiner Erblindung durch Helfer ausgeführt – schickte er zur Veröffentlichung nach Holland. Dabei gingen oft Teile seiner Manuskripte und Zeichnungen auf dem langen Transportweg verloren, darunter auch Unterlagen zur *Rariteitkamer*. Zur Wiederherstellung dieser Zeichnungen gelang es M.S.Merian zu gewinnen, die nach ihrer Rückkehr aus Surinam ein eigenes Werk über ihre dort gemachten Beobachtungen zur Metamorphose tropischer Falter herauszugeben plante und zur Finanzierung dessen dringend Geld benötigte. Merians Name wird in dem Werk von Rumphius nicht erwähnt, der Verleger François Halma (1653-1722) übergeht ihre Mitarbeit im Vorwort. Auch sie selber äußert sich nicht zu dieser für sie aus finanzieller Sicht so wichtigen Auftragsarbeit, sondern meint im Oktober 1702 Volkamer gegenüber nur, der Druck ihres eigenen Werkes sei nicht anders als über Subskription zu realisieren, so „wie mit dem Ambonischen werck“.[32] Jahre später allerdings bietet sie ein koloriertes Exemplar der *Rariteikamer* zum Verkauf an und setzt hinzu: „ ... von dem Ambonischen werde ich in das künftige keines mehr machen ... “.[33] Merian gibt hier zu verstehen, daß sie, die nie mit fremden Büchern Handel trieb, auf Grund ihrer Mitarbeit offenbar Belegexemplare besitzt.[34] Der Verkauf der *Rariteitkamer* als nicht eigenes Werk und die Bemerkung, in Zukunft „keines mehr machen“, d.h. kolorieren zu wollen (was nicht ausschließt, daß sie nicht noch weitere unkolorierte Exemplare besitzt), weisen auf eine Mitarbeit Merians an dem Werk hin. Zudem berichtet der Frankfurter Gelehrte Zacharias Conrad von Uffenbach nach seinem Besuch bei M.S.Merian im Februar 1711 von den in ihrem Besitz befindlichen Originalen zu der *Rariteikamer*.[35] Ebenso wie Uffenbach schreibt Richard Bradley nach einem Besuch bei Merian, der im Jahr 1714 stattfand, von den nun zum Verkauf angebotenen Originalen zu Rumphius‘ Werk.[36] Diese Quelle war bislang nicht bekannt.

Erst zwei Jahre später, am 16.März 1703, berichtet Baltasar Scheid an Volkamer in Nürnberg: „die Ambonische raritäten Cammer ist schon weit avancirt“, die meisten Kupfer seien gestochen und der Text bereits im Druck; und: „Madame Merian will auch ihr Werck heraußgeben, deßentwegen sie solches durch ein gedruckten bogen, die einschreibung als wie bey der Amboinischen raritäten

[32] Kat. Frankfurt/M. (1997), Brief 7, S. 264/265.
[33] Ebd., Brief 17, S. 269.
[34] Rücker (1997).
[35] Uffenbach (1754), Teil 3, S. 552 - 554.
[36] siehe hierzu Kapitel 4.

Cammer, laut beylage bekandt macht“ (Bf.18). Die Beilage ist nicht erhalten geblieben. Dieser Versuch Merians, über Scheid durch Volkamer in Deutschland um Subskribenden zu werben, hatte keinen Erfolg, eine geplante deutsche Ausgabe ihrer *Metamorphosis* kam auf Grund einer zu geringen Zahl von Einschreibungen nicht zustande.[37] Merian hat am 4.Juni 1703 an James Petiver den gleichen „gedruckten bogen“ mit den Subskriptionsbedingungen geschickt.[38] Eine Ankündigung für den englischen Markt erschien als „advertisement“ in den *Philosophical Transactions of the Royal Society of London* in der Ausgabe von 1704, vol. 23 (für die Jahre 1702 und 1703), Nr. 285 (für die Monate Mai und Juni 1703). Dabei handelt es sich aber nicht um den in diesem Brief (i.e. Brief 9) erwähnten „gedruckten bogen“, sondern eher um die von einem Herrn Schulz gewünschte Bekanntmachung in einer englischen Zeitung.[39] In diesem „advertisement“ wird M.S.Merians Vorhaben in lobenden Worten beschrieben, das Werk verdiene auf Grund seiner Besonderheit unterstützt zu werden. Die Subskriptionsbedingungen lauten: 30 shilling (das entspricht dem Preis für die unkolorierte Ausgabe), zu zahlen in drei Raten an James Petiver, davon die erste bitte „speedily“. Einzelne, z.T. kolorierte Blätter können bei Petiver bewundert werden. Auch Merians beide *Raupenbücher* werden verkaufswirksam erwähnt.

Der Druck von Merians Werk hat im März 1703 noch nicht begonnen, Scheid hätte darüber geschrieben und auch einen Probedruck geschickt, wie ihn Merian bereits im Oktober 1702 an Volkamer in Aussicht gestellt hatte.[40] Die Vorbereitungen dazu laufen allerdings auf Hochtouren, denn bereits vier Wochen später, am 17.April 1703, verkündet Scheid: „ ...habe auch daß erste Kupfferblatt von Mad. Merians buch beygepackt, umb die Freunde den Anfang vom Werck sehen zu laßen“ (Bf.19). Von Merian selbst hört man erst im Juni desselben Jahres vom Druckbeginn, als sie einen Probedruck nach London an Petiver schickt.[41]

In rascher Folge treffen nun die Meldungen aus Amsterdam bei einem offensichtlich ungeduldig auf die Fertigstellung beider Werke wartenden und hartnäckig nachfragenden Volkamer in Nürnberg ein.

Die *Rariteitkamer* soll bald fertig sein, heißt es im Mai 1703 (Bf.20), jedoch gleich darauf, im Juni, wird Volkamer vertröstet: „daß Werck wäre allbereits fertig, weilen aber von Halma unlängst durch ein Kranckheit an der correctur ist verhindert worden, als soll solches erst in 4 Wochen Herauß kommen“. – Bei Merian hat er für vier Exemplare einschreiben lassen, desgleichen für die *Rariteitkamer* (Bf.21).

Nur einige Wochen später, am 20.Juli 1703, bestätigt Scheid nochmals die Einschreibung bei Merian für vier Exemplare und weist gleichzeitig darauf hin:

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37 Kat. Frankfurt/M. (1997), S. 224.

38 Ebd., Brief 9, S. 266.

39 Ebd., Brief 10, S. 266.

40 Ebd., Brief 8, S. 265/266.

41 Ebd., Brief 9, S. 266.

„...und wird man nicht lang mehr einzeichnen können, weilen so bald die erste 20 blätter gestochen, sie daß übrige Werck auff ihren Kosten selbst Heraus geben und verlegen will, die Engelländer Haben allein vor 100 Exemplaria gezeichnet und müßten alle illuminirt sein, ob der Herr Doctor auch illuminirte verlangt, welches meinem urtheilen nach schier daß nöthigste, so kann man jetzo jeder blatt à 12 st. illuminirt bekommen, Hernacher so wird man wohl 1 fl. vor jeder blatt bezahlen müßen" (Bf.23).

Diese Angaben von Scheid sind nicht unbedingt richtig und beruhen sehr wahrscheinlich auf falschen oder unkorrekt wiedergegebenen Informationen, sie stehen im Widerspruch zu dem , was Merian selbst sagt. So hatte sie im Juni 1703, vier Wochen vor Scheids Schreiben, Petiver in London mitgeteilt, „wenn 100 Stück in England begehrt würden, so wird es auch ins Englische übersetzt werden". Zu einer englischsprachigen Ausgabe der *Metamorphosis* ist es nie gekommen, wiewohl Merian mehrfach über einen längeren Zeitraum versucht hat, durch Petiver ihre Werke auf den englischen Büchermarkt zu bringen. Eine Prachtausgabe für die englische Königin sollte ebenso dazu beitragen wie der Versuch, einen Buchhändler durch besonders günstige Konditionen zur Übernahme von 200 Exemplaren der *Metamorphosis* zu bewegen. Es wurde nichts dergleichen verwirklicht. Auch ihr Hinweis, einen deutschen Text liefern zu können, zeigt keine Wirkung.[42] Desgleichen widerspricht Scheids Angabe, wonach Merian nach Fertigstellung von 20 Tafeln die restlichen 40 von vorgesehenen 60 Kupfern auf eigene Kosten ohne Fremdfinanzierung herstellen wolle, den Aussagen Merians. Wieder ist es Petiver, an den sie im Oktober 1703 schreibt: in Kürze solle ein Drittel, das sind 20 Platten, fertig sein. Sollten sich noch weitere Interessenten (sprich Subskribenten) finden, so werde sie weitermachen, andernfalls müsse sie mit den 20 Tafeln aufhören.[43] Kein Wort von Produktion auf eigene Kosten. Da Merian 60 Platten stechen und drucken lassen konnte, trafen demnach ausreichend Gelder bei ihr ein.

In seinem oben zitierten Schreiben vom 20.Juli 1703 vergißt Scheid nicht, auf eine abermalige Verzögerung der Herausgabe der *Rariteitkamer* hinzuweisen, allerdings solle das Werk dafür auch um zehn Kupfer erweitert werden , van Halma sei momentan zu sehr mit der Beendigung eines anderen Buches beschäftigt.

Schon bald darauf, am 14.September 1703, läßt Scheid Volkamer die Nachricht zukommen: „Unser S$^{r}$. Van Halma trainirt schier lang mit der Ambonischen raritäten Kammer, nun soll solche erst in 8 Wochen Herauß kommen, ich treibe starck daran, ..., Hingegen so geht Mad. Merian ihr Werk beßer fort, Sie läßt den Herren Doctor freundlich grüßen, und hat sie mir von Ihrem Werck schon 15 bogen Kupfferstück zu jeder Exemplar zugestellt welche gestern bey S$^{r}$. Johann

[42] Kat. Frankfurt/M. (1997), Briefe 9-12, 15, 16, S. 266 - 269.
[43] Ebd., Brief 11, S. 266/267.

Herdegen Junior seinen Wahren mit beygepackt und dem Herren Doctor ... zusenden wollen“ (Bf.24).

Die Arbeiten an der *Metamorphosis* gehen, anders als bei der *Rariteitkamer*, zügig voran, immerhin sind fünf Monate nach Beginn des Druckes bereits 15 der großformatigen Kupferplatten gestochen und zum großen Teil gedruckt, gut vier Wochen später sind laut Merian fast 20 Platten fertig, und es wird weitergearbeitet trotz ständiger finanzieller Unsicherheiten.[44] Da Stechen und Drucken zwei unterschiedliche Arbeitsvorgänge sind, spricht Merian als Ausführende meist nur von Kupferplatten, fertigen oder noch zu stechenden, während Scheid ausschließlich über die durch ihn zu befördernden Merianschen Drucke schreibt. Beide Angaben ergänzen sich, Merian vermeldet die Fertigstellung einer Anzahl von Platten, einige Zeit später erhält Scheid die Abzüge, um sie nach Nürnberg weiterzuleiten.

Erst im Mai 1704 hört man wieder von Scheid. Er ist zutiefst betrübt, sein langes Schweigen und nicht Beantworten mehrerer Briefe Volkamers hatten nur den einen Grund: er hoffte und wartete auf die Fertigstellung der *Rariteitkamer*. Die sei aber noch nicht fertig „weilen der Index darvon noch nicht außgearbeitet“. Die Arbeiten am Text und den Abbildungen sind aber demnach zu diesem Zeitpunkt abgeschlossen. – „ ...indeßen aber so geht der Mad. Merian ihr Werck beßer von statten, wie sie dann Hofft mit erstem wieder 20 bögen der Kupfferstück fertig zu Haben ...“ (Bf.25). Es folgt am Briefende eine Meldung über nach Nürnberg geschickte Waren und der Hinweis: „Hierüber und über daß was an Mad. Merian bezahlt folgt beyliegende Rechnung“. Diese Rechnung mit Angaben über Abschlagszahlungen an Merian ist leider nicht erhalten geblieben. Kurze, Verpackungs- oder Transportkosten betreffende Abrechnungen hat Scheid bei einigen Schreiben direkt an das Briefende angefügt, alle anderen Rechnungen separat beigelegt.

Folgt man den bisherigen Angaben Scheids, so sind im späten Frühjahr 1704 35 der angestrebten 60 Tafeln fertig. Dies stimmt überein mit der Mitteilung Merians von April desselben Jahres an Petiver: „ ...Il en est prèst plus que la moitie ...“[45] (es ist mehr als die Hälfte davon fertig; eigene Übersetzung). Dieser im Original in französischer Sprache abgefaßte Brief Merians wurde in teilweise abweichenden Übersetzungen von E.Rücker wiedergegeben. Da es noch eine weitere Unstimmigkeit in der Übersetzung gibt, soll kurz genauer auf die fraglichen Passagen eingegangen werden. Nur in der Übersetzung von 1984[46] heißt es korrekt „über die Hälfte“, in denjenigen von 1982[47] und 1997[48] jedoch „fast die Hälfte“,

[44] Kat. Frankfurt/M. (1997), Brief 11, S. 266/267.
[45] Rücker (1982 b).
[46] Rücker (1984).
[47] Rücker (1982 b).
[48] Kat. Frankfurt/M. (1997), Brief 12, S. 267.

die Wiedergabe von 1985[49] enthält den Originaltext in Auszügen ohne nachfolgende Übersetzung. – In einer wahrscheinlich nicht weiter beachteten, 1704 angefertigten englischen Abschrift dieses Briefes von Merian heißt es ebenfalls: „ ... above half of it is ready ...“, also auch hier „mehr als die Hälfte“.[50] Der Produktionsverlauf stellt sich anders dar, wenn man davon ausgeht, daß im April 1704 bereits mehr als die Hälfte der 60 Platten gestochen worden ist denn bei einer Annahme von nur „fast die Hälfte“ zu demselben Zeitpunkt. Der Brief Scheids bestätigt die höhere Anzahl bereits fertiger Platten.

Diese englische Abschrift von Merians Brief vom April 1704 unterscheidet sich noch an anderer Stelle wesentlich von den bisher angefertigten deutschen und neuen englischen Übersetzungen. In der Wiedergabe der französischen Originalfassung lautet die Äußerung um mögliche Provisionszahlungen an Levinus Vincent wie folgt: „ ... autrement il faut que je donne L0 procent pour leur Provision ...“. Die deutschen und englischen Übersetzungen Rückers geben dies mit „ 50% Provision“ wieder. Das ist falsch. „L0“ existiert als römische Zahl nicht und läßt sich somit nicht übersetzen; es muß sich hier um einen Lese- und Übertragungsfehler handeln. Die zitierte Abschrift von 1704 lautet an gleicher Stelle: „... for otherwise I must give ten p cent; for their provideing, ...“ (denn sonst muß ich zehn Prozent geben für ihr Bereitstellen; eigene Übersetzung). Diese Unstimmigkeit – 50% in den neueren Übersetzungen, 10% in der Abschrift von 1704 – mag durch die Schreibweise der Ziffer „1“ entstanden sein. Nimmt man die Kopie der Handschrift von Merians Brief vom 27.April 1705 (i.e. Brief 15) an James Petiver zur Hand, so findet man am Ende des Schreibens ein „10 procent“, das auch ohne weiteres als „L0 procent“ gelesen werden könnte.[51]

Im Gegensatz zur französischen Originalfassung ist die zitierte Abschrift unversehrt erhalten geblieben. Der durch Tintenabrieb nicht vollständig lesbare und dadurch bislang unvollständig wiedergegebene Satz aus Brief 12 Merians lautet hier: „ The notes contain divers sortes of very rare Animalls, I thank you heartily for communicating them; ...“ (Die Briefchen enthalten verschiedene Arten von sehr seltenen Tieren, ich danke Euch herzlich für die Übersendung; eigene Übersetzung).

Zurück zu Baltasar Scheid und seinen Briefen an Volkamer in Nürnberg. Am 1.Juli 1704 berichtet er aus Amsterdam: „ ... Mad. Merian avancirt wacker mit ihrem Werck, in Zeit von 14 Tagen komt schon daß 40 Kupferblatt Heraus, auch ist sie schon bedacht umb auch den Text drucken zu laßen, und hofft sie gegen den November, so Gott will, damit fertig zu sein ...“ (Bf.26). Hier wird deutlich gemacht, daß sich Merian etwa ab Mitte des Jahres 1704 neben der Erstellung der Tafeln und deren Druck intensiv mit dem die Abbildungen beschreibenden Text befaßt. Von den Tafeln war über die Hälfte fertig, die Textseiten sollten möglichst

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[49] Rücker (1985).

[50] British Library, Dept. of Manuscripts, Sloane Ms. 4067, fol. 51.

[51] Rücker (1982 b).

zeitgleich mit den Abbildungen abgeschlossen sein. Dazu arbeitet sie eng mit Caspar Commelin zusammen, der ihre Ausführungen wissenschaftlich ergänzt. Für diesen Teil der Arbeit an der *Metamorphosis* hat sich Merian Scheids Angaben zufolge eine Frist von vier Monaten gesetzt, die aber nicht eingehalten werden konnte, wie den weiteren Briefen zu entnehmen ist. – Verzögerungen auch bei der *Rariteitkamer*: „ Mit der Amboinischen raritäten Kammer aber geht es noch langsam zu, der Autor macht nun den Indicem darzu, ob es nun bald zum Ende kommen soll welches mich der Verleger Halma vertröstet, muß die Zeit lehren ...“.

Vorerst werden von Merians Werk nur Drucke der großformatigen Kupfertafeln an Volkamer weitergeleitet, vom Text ist keine Rede mehr, als Scheid am 8.Juli 1704, also nur eine Woche später schreibt: „ ... von Mad. Merian kommen vor 4 Exemplaria von jedem 15 bögen ihres operis und Hofft sie in 3 Wochen wieder 10 zu lieffern ...“ (Bf.27). – Das stimmt mit Merians Angaben von Ende Juli 1704 überein, wenn sie schreibt, daß Herr Schey dreißig Blätter empfangen habe und weitere zehn bald folgen werden.[52] Als möglichen Zeitpunkt der Fertigstellung ihres Werkes nennt sie im gleichen Brief, realistischer als Scheid, den Januar 1705.

„bey Mad. Merian Habe auch à 10 st. jeder bogen zu illuminieren ihres Surinamischen Werckes bestellt“ (Bf.29); erst im folgenden Jahr, am 6.Februar 1705, berichtet Scheid wieder über Merian.Volkamer hat also die Empfehlung, wonach Merians Werk unbedingt illuminiert genommen werden müsse, angenommen. Der genannte Preis beläuft sich bei 60 Einzelblättern auf 30 Gulden für das Illuminieren, Merian nennt Volkamer die gleich Summe. Da dieser offenbar erst zu diesem Zeitpunkt und nicht schon früher den Auftrag zur Kolorierung der Tafeln gegeben hat, ist davon auszugehen, daß jetzt, im Februar 1705, der Druck abgeschlossen ist, d.h. das Werk komplett vorliegt. Die Kolorierungsarbeiten an Volkamers Exemplar haben sich, so kann man es den folgenden Schreiben entnehmen, bis in den April hingezogen.

Bei Abfassung des nächsten erhaltenen Briefes von Merian und auch von Scheid lag Merians Hauptwerk *Metamorphosis Insectorum Surinamensium* in kolorierter Fassung vor. – Gut zwei Monate benötigt Merian für die Kolorierung der 60 Seiten. Die von Scheid im Februar erwähnte Bestellung von illuminierten Bögen ist im April fertiggestellt. Das geht aus Merians Schreiben vom 16.April 1705 – wenige Tage vor Scheids Brief – an Volkamer hervor. „ auf dessen begehren volgens bericht von herrn schey habe ein buch von meinen Surinamschen Insecten veränderung Iluminirt, das stuck vor 10 Steuver ...“[53], der Wortlaut deckt sich mit dem Scheids vom 6.Februar (Bf. 29). Sie hat die Tafeln für Volkamer selbst koloriert und es getan „ ... So gut als es dunlichst auf schwarzen truck“ möglich ist (schwarzer Druck, d.h. es handelt sich hier wahrscheinlich nicht um ein Umdruckexemplar mit bekanntlich sehr schwachen Konturen). Weiter teilt sie

[52] Kat. Frankfurt/M. (1997), Brief 13, S. 267.
[53] Kat. Frankfurt/M. (1997), Brief 14, S. 267/268.

Volkamer die Auslieferung eines lateinischen und eines holländischen Textes für zwei Exemplare der *Metamorphosis* an Herrn Schey mit. Sollte Volkamers Bruder, der Handelsherr Johann Christoph Volkamer (1644–1720) den für ihn bestimmten holländischen Text nicht verstehen, so mögen die Brüder tauschen, schreibt sie. Die ausgestellte Rechnung lautet: 15Gulden der Druck, 30 Gulden das Illuminieren.

Die Auslieferung des holländischen Textes der *Metamorphosis* nach Nürnberg wird auch von Scheid ausdrücklich erwähnt. Hierzu sei der Brief vom 15.Mai 1705 an Volkamer vorgezogen (es ist noch einer vom April erhalten). Scheid schreibt also im Mai: „ ... Madame Merian salutirt, diese meldet daß so E.E. vor daß Niederteutsche lieber Lateinischen textum wollen haben, so dörften E.E. solches mir wieder zurück senden, so will Sie solches austauschen ... " (Bf.31). Gerade dieser Satz von Scheid zeigt, abgesehen von den vielen Übereinstimmungen in Merians und seinen Briefen, daß es sich bei der von Merian mit „Schey" benannten Person um den Amsterdamer Kaufmann Baltasar Scheid handeln muß, denn wenn dieser schreibt, Volkamer könne ihm den holländischen Text wieder zurücksenden zwecks Umtausch, so muß er diesen auch ursprünglich nach Nürnberg hingesandt haben. Merian sagt in ihrem vorher genannten Brief vom 16.April aber nichts anderes, nämlich:" ... ich habe an herrn Schey geliefert eine ladeinische und eine hollendische schrift ... wan des herren Docters bruder, das hollendische nicht verstehet, so kann er das ladeinisch nehmen dieweilen der herr DO: das hollendische versteht ...".[54] Beide Schreiber weisen ausdrücklich auf den holländischen Text hin.

Nun zu Scheids Schreiben vom 21.April 1705, wenige Tage nach Merians Brief an Volkamer abgefaßt, der deshalb von besonderem Interesse ist, als er die direkte Mitarbeit einer Merian-Tochter an der Kolorierung der *Metamorphosis* dokumentiert. Zunächst erfährt man : „ ... von deß Rumphii seinen Büchern Hat der Herr D^r^. groß format, und kann E.E. daß kleinere format bey S^r^. Herdegen zu sehen bekommen. Freylich ist der Mad. Merian ihr buch auff schöner und beßer Papier als dieses, und Habe es dem von Halma mit Nachdruck verwiesen, daß Er so schlecht Papier Zu einem solchen kostbahren buch genommen." Die *Rariteitkamer* wurde also von Anfang an in zwei unterschiedlichen Formaten geliefert, wobei die kleinformatige Ausgabe mit Sicherheit um einiges günstiger zu erwerben war; Preise werden weder für das Folio- noch für das kleinere Format genannt. – Ein Papiervergleich von *Rariteitkamer* und *Metamorphosis* gibt Scheid recht und zeigt in der Tat, daß die Tafelseiten des ersten Werkes häufiger Hadernreste erkennen lassen, was beim zweiten nicht der Fall ist. Merian selbst gibt an, das beste Papier für ihr Werk genommen zu haben.[55] In dem vorne

54 Ebd., Brief 14, S. 267/268.

55 Merian (1705), Vorwort. Das Papier für die Tafeln der *Rariteitkamer* stammt von Colombier aus der Auvergne, bei der *Metamorphosis* wurde holländisches Papier verwendet, das Gegenzeichen PLV steht für Pieter van der Ley.

zitierten „advertisement“ heißt es diesbezüglich, die Beschreibungen und Abbildungen sollen in „large Folio on Imperial Paper“ gedruckt werden. „Imperial“ ist allerdings keine Bezeichnung für Papierqualität, sondern für ein Papierformat.[56] Das Exemplar des Germ.Nat.Museums Nürnberg hat ein außergewöhnlich großes Format (73x51 cm)[57], es ist Imperial. – Weiter heißt es in dem Schreiben: „ Ferner so Habe durch einschlag dero Hren Bruders daß iluminirte von gedachter Frl. Merianin, darüber beyliegende Rechnung nebst dero brieff folget ... “(Bf.30) (Rechnung und Brief sind nicht erhalten.). Mit „Frl. Merian“ wird Dorothea Maria gemeint sein – Maria Sibylla wird immer „Mad. Merian“ genannt - , die ältere Schwester Johanna Helena befand sich zu dieser Zeit sehr wahrscheinlich noch in Surinam.[58] Durch die Aussage Scheids wird einmal mehr die enge Zusammenarbeit von Mutter und Tochter deutlich. Dies ist wahrscheinlich der einzige direkte Hinweis auf die Mitarbeit einer Merian-Tochter an einem Werk von M.S.Merian. Alle bisherigen Unterlagen weisen keine Nennung der Töchter als Mitarbeiterinnen auf, sondern lassen dies nur vermuten.

Die Brüder Volkamer sind damit im Besitz von zwei kolorierten Exemplaren der *Metamorphosis*, von denen eines die Mutter und eines die Tochter angefertigt hat. Diese Exemplare sind eine Extra-Bestellung, alle vorangegangenen Lieferungen sind unkoloriert.

Drei weitere Briefe Scheids enthalten noch kurze Bemerkungen zu Merians *Metamorphosis* bzw. zu dem Verkauf einiger ihrer Naturalien.

Am 25.7 bris (September) 1705 äußert sich Scheid besorgt: „ ... indeßen so hoffe es sollen die restirende bögen von Mad. Merian durch Einschlag Herren Herdegen wohl empfangen sein ... “ (Bf.32). Ob es sich hier um eine neue Bestellung handelt oder aber eine Verzögerung des Transportes durch fehlende Fuhrleute vorliegt, wie an anderer Stelle als Entschuldigung für verspätete Lieferung angegeben, läßt sich nicht sagen, die zeitliche Differenz zur ersten Sendung ist vergleichsweise groß und spricht eher für eine Neubestellung.

Erst drei Jahre später spricht Scheid wieder von Merian. Es haben sich zwar noch etliche Briefe aus der Zeit von 1705 bis 1707 erhalten, jedoch findet sie darin keine Erwähnung. Ihr Vorhaben, die beiden Teile von *Der Raupen wunderbare Verwandelung* ins Holländische zu übersetzen sowie ihr Wunsch, hiervon eine englischsprachige Ausgabe auf den Londoner Markt zu bringen,[59] sind offenbar nicht spektakulär genug um nach Nürnberg gemeldet zu werden. Am 3.Juli 1708 bittet Scheid Volkamer: „ Wegen Mad. Merian ihren raritäten will auch E.E. resolution Zu vernehmen gewärtig bleiben.“ (Bf.42). Merian bietet offenbar größere Mengen von Naturalien zum Verkauf an, Volkamer scheint interessiert gewesen zu sein, war aber nicht rasch genug mit einer Antwort oder aber Scheid

56 Corsten et al. (1991), Bd.III, S. 573.
57 Faksimile (1991), S. 154.
58 Segal (1997).
59 Kat. Frankfurt/M. (1997), Brief 16, S. 268/269.

hat die Nachricht zu spät weitergegeben, denn bereits am 17.Juli meldet er: „ Madame Merian Hat ihre Doosen mit insecta vor 8 Tagen verkaufft, ich will trachten damit ich die bedeutete Papilionen bey andern Freunden möge bekommen; ... “ (Bf.43). Der Verkauf von Naturalien geht bei Merian offensichtlich gut, besser jedenfalls als derjenige der *Metamorphosis*, wie man ihrem Schreiben aus dem gleichen Jahr entnehmen kann.[60]

Mit diesem Brief von 1708 enden Scheids Angaben zu Merian.

## 3.3 Zusammenfassung

Die Briefe von Baltasar Scheid dokumentieren den Entstehungsprozeß von Merians Hauptwerk und stellen ihn genauer dar als bislang bekannt. So kann jetzt der Druckbeginn bereits für April 1703 angesetzt werden; Merian hatte zwar schon im Oktober 1702 einen Probedruck in Aussicht gestellt, aber erst im Juni 1703 konkret von einem ersten Abzug geschrieben. Im September desselben Jahres sind Scheid zufolge fünfzehn Platten nebst Drucken fertig; Merian schreibt im Oktober von demnächst zwanzig gestochenen Platten. Ende Mai 1704 liegen laut Scheid 35 Kupferplatten vor, nur fünf Wochen später wird das 40.-te Kupferblatt in Aussicht gestellt, d.h. im Juli 1704 sind wahrscheinlich insgesamt 50 Tafeln gestochen und auch gedruckt. Zu welchem Zeitpunkt die restlichen zehn Platten fertiggestellt worden sind, läßt sich nicht mehr feststellen, man kann eingrenzend aber sagen, zwischen Juli 1704 und Februar 1705 wurde die Erstellung der 60 Kupfertafeln abgeschlossen. Auch der begleitende Text war laut Scheid von Merian bereits im Juli 1704 so weit zusammengestellt, daß an dessen Druck gedacht werden konnte. Scheid ging von einem Abschluß des Werkes im November 1704 aus; Merian sah realistischer den Januar 1705 als mögliches Ende der Arbeiten an. Da Scheid, der immer recht prompt reagierte, im Auftrag Volkamers Anfang Februar 1705 bei Merian eine Bestellung zur Kolorierung aller Bögen der *Metamorphosis* abgegeben hat, kann der Februar 1705 als wahrscheinlicher Abschlußmonat der Stech- und Druckarbeiten an Merians Hauptwerk angesehen werden.

Die *D'Amboinsche Rariteitkamer*, von van Halma verlegt, läuft wie Merians *Metamorphosis* über Subskription, entsteht fast im gleichen Zeitraum wie diese, beide Bücher erscheinen im Abstand von nur wenigen Monaten in Amsterdam, für beide hatte Volkamer sich in die Liste der Subskribenten eintragen lassen, aber nur bei der *Metamorphosis* schreibt Scheid von Vorabverkauf und schubweiser Weiterleitung von Einzelblättern. Nicht so bei der *Rariteitkamer*, die erst als Ganzes auf den Markt gebracht wird.

Was mag Merian dazu bewogen haben, bei laufender Produktion Tafeln einzeln zu verkaufen? Handelte sie aus finanzieller Notwendigkeit oder waren es vielmehr taktische Gründe? Beides trifft gleichermaßen zu.

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[60] Ebd., Brief 16, S. 268/269.

Merian möchte mehr Werbung betreiben als es ihr durch das möglicherweise recht nüchtern gehaltene Informationsblatt zur Einschreibung möglich war. Vor allem in Deutschland hatte man ein nur geringes Interesse gezeigt. Durch sofortige Übersendung einzelner Tafeln – großformatig, prachtvoll, auf sehr gutem Papier gedruckt – an Volkamer in Nürnberg war die Möglichkeit gegeben, diese interessierten Kreisen und damit potentiellen Käufern zugänglich zu machen. Sie selber fordert ihn zur Präsentation auf, „auf das die liebhaber sich deswegen resolvieren können".[61] Durch jede neue Lieferung konnten eventuell neue Kunden gewonnen werden. Und diese benötigte sie dringend. – Ob und in welchem Umfang sich ihre Überlegungen als richtig erwiesen haben, läßt sich heute nicht mehr nachweisen, bekannt ist nur die Meldung von zwölf Einschreibungen in Deutschland.[62]

Volkamer muß sich zufrieden über die erhaltenen Blätter geäußert haben, von neuen Interessenten scheint aber keine Rede gewesen zu sein (zumindest läßt sich das weder Merians noch Scheids Briefen entnehmen). Und doch dürfte Merian sich bestärkt gesehen haben, trotz bisheriger schwacher Resonanz weiterzumachen.[63] Immerhin hat er vier unkolorierte Exemplare bezogen, um nach Abschluß der Druckarbeiten zwei kolorierte Fassungen in Auftrag zu geben.

Der ständige Geldmangel, dem Merian durch verstärkte Werbung mittels Vorabverkauf von Einzelblättern entgegenzuwirken sucht – wahrscheinlich hat sie auch immer wieder auf die Möglichkeit der Subskription und den damit verbundenen günstigeren Preis hingewiesen. Der sich inklusive Text verstand, welcher bei einem frühen Kauf von Einzelblättern entfiel, da noch nicht verfaßt.

Subskription war eine sichere Geldquelle, mit der sie arbeiten konnte; zwar hatte sie durch die Mitarbeit an der *Rariteitkamer* und den Verkauf von Naturalien vermutlich gute Einnahmen, aber es mußten die Stecher, die großformatigen Kupferplatten, das gute Papier und der Druck bezahlt werden, dazu kamen die Kosten für den eigenen Lebensunterhalt. An James Petiver macht Merian den Vorschlag, nach dem die erste Hälfte des Buches sofort zum halben Preis ausgeliefert werden könnte, die zweite nach Fertigstellung für den Rest des Geldes.[64] Ein Entgegenkommen – wohl, um früher an benötigtes Geld von Interessenten zu gelangen, die sich nicht eingeschrieben hatten, welche es aber zu gewinnen galt.

Dagegen: einen Verkauf der *Metamorphosis* über Levinus Vincent laufen zu lassen, war Merian 1704 mit 10% Provision zu teuer[65], hinzu kommt ein wohl auch nicht sehr gutes Verhältnis zwischen beiden.[66] 1705 dagegen sah die Lage auf

[61] Kat. Frankfurt/M. (1997), Brief 8, S. 265/266.
[62] Ebd., Brief 14, S. 267/268.
[63] Ebd., Brief 13, S. 267.
[64] Kat. Frankfurt/M. (1997), Brief 12, S. 267.
[65] Ebd., Brief 12, S. 267.
[66] van Gelder (1997).

Grund der ernüchternd geringen Einschreibungen anders aus, jetzt waren 10% Provision für sie durchaus denkbar, sie bietet es Petiver direkt an, vorausgesetzt, er nennt ihr eine Person in Amsterdam, die für von Petiver gewonnene Abnehmer in Vorlage tritt.[67]

Auf Grund der vielen Übereinstimmungen in den Schreiben Baltasar Scheids und den fast zeitgleich verfaßten Briefen Maria Sibylla Merians zeigt sich, daß der von Merian genannte Herr Schey oder auch Schrey eben dieser Schreiber Scheid ist, Kaufmann in Amsterdam, der für sie als Übermittler von Druckwerk und Naturalien tätig war.

# 4 Richard Bradley und Maria Sibylla Merian

## 4.1 Richard Bradley (1688 – 1732): ein Überblick über sein Leben und Werk

Richard Bradley wurde um 1688 in der Nähe von London geboren. Über seine frühe Jugend ist nichts bekannt.[68] Seinen eigenen Angaben zufolge zeigte er aber bereits als junger Mensch ein Interesse an Pflanzen und an der Gärtnerei.[69] – Vermutlich hat er im Temple Coffee House Botany Club (London) James Petiver und durch diesen den Arzt und Naturforscher Sir Hans Sloane (1660 – 1753) kennengelernt, beide leidenschaftliche Sammler von Naturalien. Durch Petivers Vermittlung wird er 1712 Fellow der Royal Society in London. Zwei Jahre später reist er nach Holland, um in den dortigen großen botanischen Gärten sowohl für Petiver als auch für die Duchess of Beaufort (1630 – 1715), deren botanischer Garten von Badminton berühmt war,[70] aber auch für sich selbst Pflanzen und Samen zu sammeln und Verbindung aufzunehmen zu namhaften Naturwissenschaftlern. Hier fertigt er etliche Zeichnungen mit Insekten- und Pflanzenabbildungen aus Ost- und West-Indien an, die von Petiver angefordert waren.[71] In Amsterdam kommt er auch mit Maria Sibylla Merian zusammen.

Sein Aufenthalt in Amsterdam dauert einige Monate, Ende 1714 kehrt er nach London zurück. – Von 1717 bis 1719 arbeitet er an der Gartenanlage des Duke of Chandos (1674 – 1744). Nach anfänglicher Zufriedenheit endet diese Zusammenarbeit mit einem Zerwürfnis; ihm wird Mißwirtschaft vorgeworfen.[72]

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[67] Kat. Frankfurt/M. (1997), Brief 15, S. 268.

[68] Henrey (1975), Bd.2; Egerton (1970).

[69] Bradley (1717), Pt.1, Preface.

[70] Sie begann sehr früh damit, exotische Pflanzen in Glashäusern zu kultivieren. Das *Badminton-Florilegium*, entstanden in den Jahren 1703 – 1705, wurde in ihrem Auftrag erstellt.

[71] Tjaden, Part 4, S. 5 u. S. 11/12, in: Bull.of the ASPS, Vol. 9 (1974); Part 11, S. 52/53; Part. 12, S. 76/77, beide in: Bull.of the ASPS, Vol. 10 (1975).

[72] Henrey (1975), Bd.2; Tjaden, Part 18, S. 122 – 124, in : Bull.of the ASPS, Vol. 11 (1976).

1722 verliert er durch nicht näher bekannte Umstände seine Pflanzensammlung. Ein Jahr später bittet er Sloane, der ihn von Zeit zu Zeit finanziell unterstützt, um Fürsprache bei der Bewerbung um die vakant gewordene Stelle zum Professor für Botanik in Oxford. Diese Stelle erhält Bradley nicht, wird aber im folgenden Jahr, 1724, auf Grund seines Versprechens, auf eigene Kosten einen botanischen Garten anzulegen, zum ersten Professor für Botanik in Cambridge gewählt. Er kommt seinen Verpflichtungen aber nicht nach; weder legt er den botanischen Garten an, noch hält er Vorlesungen. Die Professur war undotiert.[73]

Bradley bestreitet seinen Lebensunterhalt hauptsächlich durch seine zahlreichen botanischen Schriften, von denen die bedeutendsten kurz erwähnt werden sollen.[74] – 1715 erscheint sein *Short historical account of coffee*, von 1716 bis 1727 gibt er in einer Folge von fünf Dekaden die *Historia plantarum succulentarum* heraus (mehr hierzu siehe Kap. 4.2). Ein Jahr später folgen die beiden Teile von *New improvements of planting and gardening*, der dritte Teil erscheint 1718. Ebenfalls 1718 kommt *Gentleman and gardeners kalendar* heraus, gefolgt 1721 von *A General treatise of husbandry and gardening*. Letzteres erscheint bis 1723 in fünfzehn Folgen. *A philosophical treatise of husbandry and gardening* (1721) ist eine Übersetzung Bradleys von Georg Andreas Agricolas (1672 – 1738) *Neu und nie erhörter doch in der Natur und Vernunft wohlgegründeter Versuch der Universal-Vermehrung aller Bäume, Stauden und Blumen-Gewächse* (erschienen 1716 – 1717). Ebenfalls 1721 erscheint *A philosophical account of the works of nature*. Von Bradleys 1728 erschienenen *Dictionarium botanicum: or, a botanical dictionary for the use of the curious in husbandry and gardening* heißt es, daß dieses das erste in England gedruckte Werk seiner Art sei.[75]

Trotz der Fülle von Schriften, die Bradley im Laufe seines Lebens herausgegeben hat, stand er finanziell meist kurz vor dem Ruin. Ständig hat er mit den Verlegern seiner Werke um Gelder zu kämpfen. Um seine Bücher herausgeben zu können, widmet er sie wohlhabenden Förderern, die ihm halfen, die eigenen Unkosten zu senken.[76]

Richard Bradley stirbt verarmt am 4.November 1732.

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[73] Henrey (1975), Bd.2; Egerton (1970).

[74] eine ausführliche Auflistung seiner Arbeiten findet sich bei Amherst (1896), S. 352 und Henrey (1975), Bd. 3, S. 14 – 18.

[75] Pulteney (1790).

[76] Rowley (1997).

## 4.2 „Please to Intimate that what Draughts of Ficoides are Done by Mad:$^{m}$ Marian may not Exceed the Quarto Size“[77]: Briefe an James Petiver.

Richard Bradley schreibt diesen Satz in einem Brief aus Brook Green[78] an den Londoner Apotheker und Naturforscher James Petiver, datiert vom 10.August ohne Jahresangabe, nach Meinung von Blanche Henrey in den Jahren zwischen 1711 und 1714.[79]

Wie kam es zu dieser Bitte um Vermittlung? – Bradley und Petiver kannten sich, wie bereits erwähnt, seit längerem, ihre Interessen lagen auf gleichen Gebieten, Petiver wird Merians druckgraphisches Werk, das er zu diesem Zeitpunkt längst besaß, dem Interessierten gezeigt und auf seine Kontakte zu der Naturforscherin und Künstlerin hingewiesen haben. Er stand ja bekanntlich seit mindestens 1703 mit Merian in Verbindung, eine frühere Kontaktaufnahme ist unbekannt.[80] Jedoch erwähnt er bereits 1695 in seinem Werk *Musei Petiveriani Centuria Prima [...]* die beiden Ausgaben von Merians *Raupenbuch* von 1679 und 1683.[81] Einige Jahre später gelangt er in den Besitz dieser beiden Bände und später noch erwirbt er auch ein Exemplar der *Metamorphosis*,[82] aus welcher er in seinem Werk *Gazophylacii Naturae & Artis* (s.a., wohl nach 1709) auf 12 Tafeln Motive abgebildet hat unter genauer Angabe der Herkunft. (*Jacobi Petiveri Opera [...]* wurde nach seinem Tod 1764 herausgegeben und stellt eine Zusammenfassung seiner früheren Arbeiten dar; es enthält auch die o.g. Tafeln und umfaßt zwei Folio- und einen Oktavband.) Petiver scheint ein starkes Interesse an einer Bekanntschaft mit Merian gehabt zu haben; sie handelte unter anderem mit Naturalien, und er war immer auf der Suche nach fremdartigen Insekten und Pflanzen. Dabei war ihm der Erhaltungszustand eines Insektes relativ unwichtig: „ No insect will come amiss & so I can have them cheap I shall not stand upon the loss of a leg or a wing“ schreibt er an den Botaniker William Sherard (1659 – 1728) und bittet Frederik Ruysch (1638 – 1731), Professor für Anatomie, und auch den Apotheker und Naturforscher Albert Seba (1665 – 1736), beide Amsterdam, „ to lay by for me what broken Insects you dayly receive from Surinam ...“.[83]

Persönlich kennengelernt hat Petiver M.S.Merian im Sommer des Jahres 1711, als er auf Wunsch von Sloane nach Holland gereist war, um an der Versteigerung der Sammlung des bereits 1695 verstorbenen Botanikers Paul Hermann teilzunehmen. Dessen Sammlung war in der damaligen Fachwelt bekannt, zum

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77 „Bitte zu verstehen geben, daß bei den Entwürfen von Ficoides, die von Mad:$^{m}$ Marian angefertigt werden, das Quart-Format nicht überschritten werden möge“ (eigene Übersetzung; Marian ist die alte englische Schreibweise des Namen Merian).

78 Brook Green gehörte in jener Zeit zu dem Flecken Hammersmith, Pfarrgemeinde Fulham.

79 Henrey (1975), Bd. 2.

80 Kat. Frankfurt/M. (1997), Brief 9, S. 266.

81 Davis (1996).

82 Kat. Frankfurt/M. (1997), Brief 9, S. 266; Briefe 15 u. 16, S. 268.

83 Stearns (1952).

Zeitpunkt der Auktion aber nicht mehr vollständig[84] und auch nicht in bestem Zustand, folgt man der Beschreibung des Zacharias Conrad von Uffenbach, der die Witwe Hermann nach einem Besuch Anfang 1711 als liederliche Frau beschreibt, die schier alles verderben läßt, in den meisten Gläsern sei der Spiritus vini verflogen, denn „Sie pflegt den Branntwein lieber selbst zu trinken, als diese Gläser damit aufzufüllen ...".[85] Von der Schönheit einiger Pflanzenbücher Hermanns konnte sich Uffenbach dennoch persönlich überzeugen, nachdem er Ende Januar 1711 einen Herrn Aymon in Den Haag aufgesucht hatte – von ihm als „Commissarius mala fide" bezeichnet -, der von dem für den König von Preußen bestimmten Teil der Sammlung „zwey hundert der raresten und schönsten ausländischen Pflanzen" unrechtmäßig für sich behalten hatte[86], die natürlich nicht zur Versteigerung kamen.

James Petiver hatte dennoch reichlich gekauft und an Sloane gemeldet: „ I was present all y$^{e}$ while & have bought you y$^{e}$ greatest share of y$^{e}$ choisest of them".[87]

Während seines mehr als einen Monat dauernden Aufenthaltes in Holland sah er sich Raritätenkabinette und die Gärten von Leiden und Amsterdam an, traf mit namhaften Naturforschern zusammen und besuchte Maria Sibylla Merian, von der er Zeichnungen kaufte.[88]

Diese Zeichnungen hat Bradley möglicherweise bei Petiver nach dessen Rückkehr nach London gesehen. Petiver hat unter Umständen auch Merians Abbildung der „Aster ficoides" erwähnt, die ihm Merian vorgelegt haben könnte; er wußte um Bradleys Interesse an Succulenten. Bradley ist offenbar von allem, was er von Merians Arbeiten sieht und darüber erfährt, so beeindruckt gewesen, daß er darum bittet, bei der Künstlerin wegen der Anfertigung von Abbildungen von Succulenten für ihn zu vermitteln.

Über diese Pflanzen plant er ein Werk herauszugeben. Es wird das erste Buch überhaupt sein, das ausschließlich von Succulenten handelt. Bereits im Februar 1711 hatte er in „The Post-Man" eine Folge von 50 Kupfern mit beschreibendem Text angekündigt. Das Werk sollte den Titel tragen *A treatise of succulent plants*.[89] Im Jahr davor hatte er Vorschläge für eine derartige Veröffentlichung per Subskription gemacht, mußte aber feststellen, daß nicht genügend Interesse vorhanden war. Einige Jahre später unternimmt er auf Anregung von Freunden einen neuen Versuch, das Werk soll nun in fünf Dekaden herausgegeben werden.

[84] Uffenbach (1754), Teil 3, S. 410: „Sie hat noch einige Curiositäten, allein die besten hat die Universität bekommen ...".

[85] Uffenbach (1754), Teil 3, S. 417.

[86] Ebd., S. 487.

[87] Henrey (1975), Bd.2.

[88] van Gelder (1997).

[89] Henrey (1975), Bd.2.

*Fig. 1 Impatiens balsamina, Lampranthus spec., Heliconus cf. melpomene; Maria Sibylla Merian, 1695; Leningrader Aquarell, Inv.-Nr. 10-89-5.*

Im Vorwort zu seiner *Historia plantarum succulentarum*, Decade 1 (1716), schreibt er (To the reader) „ that the spirit of botany was not powerful enough to pay the expense of engraving the copper-plates".

Die erste Dekade erscheint 1716, die fünfte und letzte 1727. Jede enthält zehn Abbildungen im Quart-Format, wobei Fig.26 und Fig.27 auf einer Tafel zusammengefaßt sind, so daß das Werk insgesamt aus 50 Abbildungen auf 49 Tafeln besteht. Zu jeder Darstellung gehört eine kurze Beschreibung der Pflanze in lateinischer und englischer Sprache. Die Stecher sind Clark, Hulsberg, Pine und Sturt. Einige Blätter weisen keine Stechersignatur auf.

Für dieses Werk hat Bradley Unterstützung bei Petiver gesucht. So bittet er z.B. in einem Schreiben vom März 1713 um eine Diskussion über *A treatise of succulent plants*.[90]

Aber bereits zwischen 1711 und 1714 schreibt er in einem Brief vom 10.August (wahrscheinlich 1712[91]):

„Her Maij:tie haveing Comanded my Attendance at Windsor I must content my Self with the thoughts of Seeing Amsterdam Gardens Some other time. You may Easyly Guess how much I am dissapointed after the Character You Gave me of

[90] Ebd.

[91] Ebd.

them & what a Loss it will be to my designs if your Interest can not procure me Some Cuttings of their Ficoides that are most rare & the Draughts of those which may be most Acceptable I believe your Letter to Dr. Rouse could gett us the former & Mad:m Mariana[92] might order the other I will Spare no Costs to gain what I desire of this kind & begg you will Send by the first, that we may have an answer before the Season be too farr advanced."

In einem Postscript sagt Bradley:

„Please to Intimate that what Draughts of Ficoides are Done by Mad:m Marian may not Exceed the Quarto Size".[93]

Es ist durchaus möglich, daß M.S.Merian eine oder mehrere Vorlagen für Bradleys *Historia plantarum succulentarum* angefertigt hat, mit großer Wahrscheinlichkeit wurde die Vorlage zu Plate 14, Decade 2, von ihr ausgeführt. Die Art der Pflanzenpräsentation unterscheidet sich in auffälliger Weise von den anderen in diesem Werk dargestellten Pflanzen. Bradley benennt diese Abbildung: „Ficoides Capensis, Caryophilli folio, flore aureo specioso. Pink-leav'd Fig-Marygold". – „Ficoides" ist die alte Bezeichnung für „Mesembryanthemum".

Der Verfasserin dieser Arbeit sind zwei Aquarelle bekannt, auf denen Maria Sibylla Merian die o.g. Pflanze dargestellt hat:


*Fig. 2 Ficoides Capensis, Caryophilli folio ..., Richard Bradley: „Historia plantarum succulentarum", Decade 2, Plate 14, 1717.*

a) Gelbe Aster und Schmetterling (Aster Vicoides Africanus mit gelben Blüten oder ficoides Ayzoydes mit breiten Blättern, 1695), Amsterdam,

[92] gemeint sind Dr. Frederik Ruysch und Maria Sibylla Merian.
[93] Henrey (1975), Bd.2.

Rijksprentenkabinett;[94] die Aster ist ein Lampranthus und gehört zur Familie der Mesembryanthemaceae;[95]

b) Impatiens balsamina, Lampranthus spec., Heliconus cf. melpomene (nach 1705), Leningrad (heute: St.Petersburg), Archiv der Akademie der Wissenschaften.[96]

Es ist die Darstellung auf dem Leningrader Aquarell von M.S.Merian (Fig.1), die der von Plate 14, Decade 2 (Fig.2) aus Bradleys *Hist. plant. succ.* in auffälliger Weise ähnelt. Die Art, die fleischigen, gegenständigen Blätter wiederzugeben, scheint identisch, ebenso die Wiedergabe des Blattansatzes. Die endständigen, margeritenartigen Einzelblüten tragen eine mehrkreisige Krone, die in beiden Abbildungen ähnlich dargestellt ist. Die Abbildung bei Bradley ist eine ausgesprochen harmonische, ausgewogene Pflanzendarstellung, welche nach einer Vorlage angefertigt worden ist, die von Merian stammen könnte. Der Unterschied etwa zu der Darstellung auf Plate 43, Decade 5 (Fig.3) ist auffällig groß. In diesem Fall scheint es sich um zwei verschiedene Personen gehandelt zu haben, welche


*Fig. 3 Ficoides Afric. folio triangulari ..., Richard Bradley: „Historia plantarum succulentarum", Decade 5, Plate 43, 1727.*

[94] Kat. Frankfurt/M. (1997), Kat.-Nr. 112, S. 169.
[95] Segal (1997).
[96] Faksimile (2003), Abb.11, S. 45.

hierzu die Vorlagen schufen.

Es spricht einiges dafür, daß Merian einen Auftrag Bradleys angenommen und mindestens die eine Pflanzendarstellung für seine *Hist. plant. succ.* angefertigt hat. Die Briefe Merians belegen, daß sie bereits durch Petiver gehofft und auch versucht hat, in England bekannt zu werden und einen neuen Abnehmerkreis für ihre Werke zu finden.[97] Durch Bradleys Auftrag ergab sich die Möglichkeit, ihren Bekanntheitsgrad dort zu erweitern und ein Geschäft zu tätigen. Sie scheint die Gelegenheit genutzt zu haben.

Bradley muß mit der Arbeit von Merian zufrieden gewesen sein, denn als er 1714 endlich nach Holland kommt, besucht er diese. Er schreibt am 4.Juli 1714 N.S.[98] aus Amsterdam an Petiver:

„M^rs^. Marian has 3 times feasted me with the Sights of her Curious paintings".

Was versetzte ihn so in Begeisterung, von welchen Bildern spricht Bradley? Der Brief lautet weiter:

„& is Desirous of a Chapman for the Originalls of Rumphius's Crustatious Animals & Shells, w th her own Originalls of Insects in 4° & Likewise 32 Leaves of fine birds, a Large folio of Choice plants from the Amsterdam Garden & D^r^. Comelins two Vol: in foll. with the prints Colour'd after the Life".

Alle von Bradley genannten Arbeiten wurden von Merian zum Verkauf angeboten:

„the first of these She asks 100 Guineas for, her Insects about 500 florins, the birds 400 florins, the Large folio – 600 florins & D^r^. Comelins Works Colour'd 200 florins".[99]

Die Originale zur *D'Amboinsche Rariteitkamer* von G.E. Rumphius wurden schon 1711 von Z.C. von Uffenbach im Zusammenhang mit Merian erwähnt.[100] Der Brief von R. Bradley enthält einen neuen, bislang unbekannten Hinweis auf Maria Sibylla Merians Mitarbeit an diesem Werk. Als koloriertes Druckwerk kostet die *D'Amboinsche Rariteitkamer* 1711 60 fl., Merian nennt diesen Preis in einem Schreiben an einen Christian Schlegel[101], für die Originale auf Pergament zu diesem Werk werden 1714 von Bradley 100 Guineas angegeben. – Die Preise, die Bradley für die Originale zu Merians *Der Raupen wunderbare Verwandelung* nennt, sind beachtlich hoch. Vergleicht man die hier genannten mit früheren – soweit dies möglich ist -, so kommt man zu dem Schluß, daß die von Merian selber in dem o.g. Brief von 1711 gemachten Preisangaben für die Aquarelle der *Raupenbücher* sehr wahrscheinlich für Arbeiten auf Papier galten[102], es sich dabei also um

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97 Kat. Frankfurt/M. (1997), Briefe 9–12, 15, 16, S. 266 – 269.

98 Der Gregorianische oder New Style Kalender wurde in Holland ab 1583 eingeführt.

99 Tjaden, Part 4, S. 9, in: Bull.of the ASPS, Vol. 9 (1974).

100 Uffenbach (1754), Teil 3.

101 Kat. Frankfurt/M. (1997), Brief 17, S. 269.

102 Ebd., Brief 17, S. 269. Dort heißt es „ ... von den Hochdeutschen in quart, die auch corios Iluminirt seint, das Stück vor 10 Gulden, wan man sie aber will haben gemahlt, so kost ... das in quart 20 fl hollendisch ...".

Repliken handelte, während die von Bradley genannten Preise von 1714 für die Darstellungen auf Pergament gelten. Die Preisunterschiede ließen sich so erklären. – Es ist allerdings zu berücksichtigen, daß es sich 1711 um die Vorlagen für Band I und II von Merians *Raupenbuch* handelte. Im Jahr 1714 dagegen sind mit Sicherheit Vorlagen zu Band III vorhanden gewesen. Merian arbeitete seit Jahren an einem dritten Teil des *Raupenbuches*.[103] Vermutlich waren zu diesem Zeitpunkt auch bereits etliche Kupferplatten dazu gestochen.

Die von Bradley angegebenen zweiunddreißig Blätter mit Vogelabbildungen sind zum großen Teil auch heute noch erhalten. Der Ornithologe François Haverschmidt erwähnt zwölf Vogelbilder, die sich in einem Album mit Bildern von Maria Sibylla Merian im British Museum befinden.[104] Sam Segal sagt, daß die Universitätsbibliothek Amsterdam im Besitz einiger Vogelzeichnungen von Merian ist[105]; (s. auch Schmidt-Loske (2004), S. 249-253 ; (2007), S. 110-112). Jantje Stuldreher-Nienhuis listet diese aus dem Nachlaß des Sammlers Valerius Röver stammenden Vogelbilder einzeln auf.[106] Im Rijksprentenkabinett Amsterdam befindet sich der „Rote Ibis"; die Graphische Sammlung Albertina, Wien, ist im Besitz von „Vier tote Bergfinken".[107] Ein weiteres Aquarell mit Vogelstudien befindet sich in der Fideikommissbibliothek in Wien.[108]

Bei dem „Large folio of choice plants from the Amsterdam Garden" handelt es sich mit großer Wahrscheinlichkeit um das von Z.C.v.Uffenbach als „sehr großes und über Hand dickes Volumen, in welchem allerhand, sowohl ausländische als Europäische Pflanzen und Früchte, auch nach dem Leben gemahlt"[109] beschriebene Werk, das 1714 beim Besuch Bradleys gekauft werden konnte, bis zu Merians Tod aber offensichtlich keinen Käufer fand, sondern später in die Sammlung von Sir Han Sloane gelangte. Es befindet sich heute im British Museum.[110]

Wie kommt Merian aber dazu, „Comelins Works" zu verkaufen? Es ist nicht bekannt, daß sie mit fremden Büchern gehandelt hat, man weiß lediglich um den Verkauf ihrer eigenen Bücher und solcher, an denen sie mitgewirkt hat. Mitgewirkt in Form von Anfertigen der Vorlagen für ein Druckwerk und dessen Kolorierung. So könnte nach Meinung von Rücker Merian für ihre Mitarbeit an der *D'Amboinsche Rariteitkamer* einige Belegexemplare erhalten haben, die sie dann auch zum Verkauf anbot.[111] – Nicht nachgewiesen ist, daß Merian für Jan Commelin

103 Ebd., Briefe 8-10, 16, S. 265 – 269.
104 Haverschmidt (1968).
105 Segal (1997).
106 Stuldreher-Nienhuis (1944).
107 Kat. Frankfurt/M. (1997), Kat.-Nr. 61, S. 117; Kat.-Nr. 149, S. 243.
108 Rücker (1967).
109 Uffenbach (1754), Teil 3, S. 552 – 554.
110 Rücker (1982 a); Beer (1976).
111 Rücker (1997).

(1629 – 1692), „commissarius“ des Hortus Medicus zu Amsterdam, oder dessen Neffen Caspar Commelin (1667 – 1731), Mediziner und Botaniker – ab 1706 Professor des Hortus Medicus in Amsterdam -, Vorlagen angefertigt hat.[112] Bei dem Werk, welches Bradley mit „D$^{r}$. Comelins two Vol: in foll.“ beschreibt, handelt es sich um *Horti Medici Amstelodamensis*. Das Druckwerk erschien 1697 – 1701 zweibändig in Folio. Der erste Band wurde von Jan Commelin vorbereitet, erschien aber erst 1697 posthum; Caspar Commelin gab den zweiten Band 1701 heraus. Die Originale hierzu sind zusammengefaßt in dem *Atlas Moninckx*, der in der Zeit von 1687 – 1749 entstanden ist und insgesamt neun Bände umfaßt.

An diesem *Atlas Moninckx* hat Johanna Helena Herolt (1668 - ?), die älteste Tochter von M.S. Merian, mitgearbeitet. Tafel 17 in Atlas 4 und Tafel 28 in Atlas 5 stammen von ihr. Beide Vorlagen von Herolt wurden in Band 2 von *Horti Medici Amstelodamensis* übernommen.[113] Es ist möglich, daß J.H.Herolt auch an einer Kolorierung des Druckwerkes beteiligt gewesen ist. Durch ihre Mitarbeit an dem *Atlas Moninckx* und dem genannten Druckwerk könnte sie also in den Besitz von Belegexemplaren von *Hort. Med. Amst.* gelangt sein. Da sie sich wahrscheinlich seit 1711 mit ihrem Mann Hendrik Herolt in Surinam aufhielt und von dort nicht zurückgekehrt ist[114], ist anzunehmen, daß Merian stellvertretend für die abwesende Tochter die beiden o.g. Bände zum Verkauf anbot.

Maria Sibylla Merian hatte zu dem Zeitpunkt, als Richard Bradley sie besuchte, die beiden Teile von ihrem *Raupenbuch* ins Holländische übersetzt und 1713/14 herausgegeben.[115] Gleichzeitig arbeitete sie an dem dritten Teil zu *Der Raupen wunderbare Verwandelung*. Ihr Hauptwerk *Metamorphosis Insectorum Surinamensium* war neun Jahre zuvor, 1705, von ihr herausgegeben worden. Merian war 1714 eine alte Frau von 67 Jahren, die sich offensichtlich von dem größten Teil ihrer Aquarelle trennen wollte. Sie hat – so Bradley – genügend Interessenten für ihre Arbeiten, denn es heißt weiter in dem Brief vom 4.Juli 1714 N.S. an Petiver:

„She has Likewise some Single Sheets which she will Dispose of but Does not care to Send them to England as you Desire for She has already Costomers enough here to buy them without that trouble as she Says, but if you will Employ any one here to pay her the Money you shall have them Reasonable & she will paint what you Desire“.

„Trouble“ gab es 1712 zwischen Petiver und Merian, als es wohl zu Verzögerungen bei der Übersendung der Ware von Merian und der Bezahlung dafür durch Petiver gekommen war. In einem Schreiben Petivers an Merian vom 17.Dezember 1712 heißt es nämlich:

112 Caspar Commelin hat für Merians *Metamorphosis Insectorum Surinamensium* den lateinischen Text geschrieben.

113 Wijnands (1983).

114 Davis (1996).

115 Wettengl (1997).

„... I therefore take this Opportunity again to assure you, y^r the Money you *demand* shall be paid on delivery of y^th things you mention. I therefore desire you will send *them fast* with, ...“.[116]

Merian hatte Petiver im August 1712 etliche surinamische Naturalien angeboten[117], die dieser auch gekauft zu haben scheint, wie dem Schreiben zu entnehmen ist.

Bradley stellt in seinem Brief an James Petiver auch klar, daß Merian jeden Bilderwunsch für Kunden aus dem Ausland – wie Petiver – bei sofortiger Bezahlung durch einen Mittelsmann ausführen würde. Merian wird hier als eine vorsichtige Geschäftsfrau beschrieben.

Von einem eigenen Kauf Merian’scher Arbeiten sagt Bradley nichts. – Wie man aber aus einem späteren Schreiben an Petiver erfährt, ist er dennoch in den Besitz von Merians *Raupenbuch* gelangt. In dem Brief vom 13.März ohne Jahresangabe heißt es, daß zwei von Frederik Ruysch für die Duchess of Beaufort vorbereitete Kisten durch den Tod der Dame in seinen, Bradleys, Besitz gelangt seien.[118] Darin enthalten sei „ the quarto of Madm. Marian’s Insects in colours which is very fine.“[119] Hieraus hat Bradley einige Abbildungen in sein 1721 erschienenes Werk *A Philosophical Account of the Works of Nature* ohne Angabe der Quelle übernommen. Darin zeigt Plate XXVII (zwischen S. 158/159), Fig.I die Entwickung des Seidenspinners ohne Pflanzendarstellung. Es ist eine Kopie von Tafel 1 aus dem *Raupenbuch*, Teil I, von Merian. Plate XXVII, Fig.II enthält Raupe, Puppe und fliegenden Falter ebenfalls aus dem *Raupenbuch*, Teil I, Tafel 44. Beide Abbildungen bei Bradley sind identisch zu den Kupferstichen von Merian, allerdings seitenverkehrt wiedergegeben. Der Stecher ist I. Cole. – Diese Übernahme von Arbeiten Merians durch Richard Bradley ist bisher nicht beachtet worden, zeigt aber, daß Bradley sich recht genau mit Merians Werk befaßt hat. Auch die *Metamorphosis* war ihm hinreichend bekannt. In Kapitel X „Of Frogs, Toads, and such Creatures ...“ des o.g. Werkes schreibt Bradley: „The curious ‚Surinam Frog‘, which Madam Mariana of Amsterdam has published in her „History of Surinam Insects“, brings its young ones perfectly fram’d into the World, ...“ (S. 123). In Kapitel XII und XIII „Of the Papilionaceous or Butterfly kind ...“ des gleichen Werkes wird Maria Sibylla Merian in Bezug auf den Laternenträger erwähnt: „In Surinam ... there is a large Fly, which they commonly call the Lanthorn Fly ... We have a good Cut of it in Madam Mariana’s History of Surinam Insects ...“ (S. 153/154).

In seinen späteren Briefen - teils in Holland, später, nach seiner Rückkehr, in England verfaßt – findet Merian keine Erwähnung mehr.

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[116] British Library, London, Dept. of Manuscripts, Sloane 3338, fol. 117. – Die kursiv geschriebenen Wörter sind kaum lesbar.

[117] Kat. Frankfurt/M. (1997), Brief 18, S. 269.

[118] Die Duchess of Beaufort starb am 7.Januar 1715, folglich muß o.g. Brief im März 1715 geschrieben worden sein.

[119] Tjaden, Part 14, S. 138, in: Bull. of the ASPS, Vol. 10 (1976).

In den Antwortschreiben an Bradley weist Petiver diesen wiederholt darauf hin, nur solche Insekten für ihn zu zeichnen, die nicht bereits von Merian dargestellt worden sind. „...& whatever Patterns you can gett w$^{th}$ y$^{m}$ pray refer to her Tables. None of those you need paint again because we already have then (sic!) in her Books y are so well done in y$^{r}$ Originalls“.[120] Ähnliches schreibt er in einem späteren Brief an den immer noch in Amsterdam weilenden Bradley.[121]

Abschließend kann gesagt werden, daß James Petiver sowohl für Merian als auch für Bradley von Bedeutung war. Für Maria Sibylla Merian stellte er einen Kunden dar, der zum einen von ihr sowohl Naturalien als auch ihr druckgraphisches Werk bezog, zum anderen ein potentieller Vermittler ihrer Arbeiten für den englischen Markt war. Durch Petiver kam die Auftragsarbeit zu Bradleys *Hist. plant. succ.* zustande. – Für Richard Bradley stellte er die diesem wichtige Verbindung zu Merian her und war vor allem ein häufig zu Rate gezogener wissenschaftlicher Berater, aber auch Abnehmer der von ihm gesammelten Pflanzen und Samen und der von ihm erstellten Pflanzenabbildungen.[122] Durch seine Vermittlung wurde er Fellow of the Royal Society und lernte Sir Hans Sloane kennen, welcher ebenfalls Zeichnungen von ihm gekauft hat..[123]

Aber auch Petiver war interessiert an einer Aufrechterhaltung der Beziehungen zu Bradley und zu Merian, die er als „non-pareill of her sex“ bezeichnete[124], von beiden bezog er die für ihn so sehr wichtigen Naturalien und von Bradley auch Zeichnungen für seine eigenen Werke.

## 4.3 Zusammenfassung

Die Korrespondenz zwischen Richard Bradley und James Petiver zeigt, daß Maria Sibylla Merian auch für englische Kunden Auftragsarbeiten ausgeführt hat. Die Bitte Bradleys an Petiver um Vermittlung bei der Künstlerin, ein sehr ähnliches Aquarell von Merian mit der Darstellung einer Sukkulenten-Pflanze, begehrt von Bradley für seine *Historia plantarum succulentarum*, sein ganz offensichtlich positiv aufgenommener Besuch bei ihr, seine Begeisterung beim Anblick Merianscher Originale – es spricht alles für eine tatsächlich ausgeführte Arbeit Merians für R. Bradley. Die Bezahlung für diese Arbeit muß zur beiderseitigen Zufriedenheit ausgefallen sein, Bradleys Schilderung des Besuches hätte sonst anders gelautet. Merian hatte an dem englischen Markt ein starkes Interesse und wahrscheinlich jede für sie günstige Gelegenheit wahrgenommen, dort bekannter zu werden. Wenngleich es ihr nicht gelungen war, eine englische Ausgabe der *Metamorphosis*

120 Tjaden, Part 4, S. 12, in: Bull. of the ASPS, Vol. 9 (1974).
121 Derselbe, Part 11, S. 53, in: Bull. of the ASPS, Vol. 10 (1975).
122 Henrey (1975), Bd.2.
123 Egerton (1970).
124 Tjaden, Part 3, S. 226, in: Bull. of the ASPS, Vol. 8 (1974).

herauszugeben, war sie dennoch in interessierten Kreisen Englands mit ihren Arbeiten vertreten. Die Duchess of Beaufort z.B. gehörte zu dem Kreis von Interessierten. Zwar gelangte sie nicht mehr in den Besitz der von ihr angeforderten *Raupenbücher* Merians, wie Bradley schreibt, aber sie kannte die *Metamorphosis* und ließ einige Tafeln daraus für ihr *Badminton Florilegium* kopieren, wobei ein Umdruckexemplar oder aber Repliken als Vorlage gedient haben.[125]

Die von Bradley wiedergegebenen Preisvorstellungen Merians für die Originale auf Pergament zu ihren Druckwerken zeigen eine sich dem Wert ihrer Arbeiten bewußte Künstlerin. Ihre Forderungen für das druckgraphische Werk und Repliken sind bekannt, nicht aber die für Originale. Wie realistisch sie waren, läßt sich allenfalls vermuten. Ein großer Teil wurde erst drei Jahre nach Bradleys Besuch Anfang 1717 von Zar Peter dem Großen und dessen Leibarzt Areskin gekauft; auch Sir Hans Sloane erwarb Originale offensichtlich noch nicht so früh, sondern Jahre später nach Merians Tod. Die Unverkäuflichkeit über einen so langen Zeitraum könnte – trotz der ausgezeichneten Qualität – auf zu hohe Preisforderungen zurückzuführen sein. Dagegen hat es – anders als bei den nur im Ganzen zu erwerbenden Vorlagen zu den Druckwerken – mit dem Verkauf von Einzelblättern keine größeren Schwierigkeiten und genügend Abnehmer gegeben. So zumindest stellt es Bradley dar. Inwieweit dies tatsächlich zutraf, kann nicht mehr nachgewiesen werden.

## 5 Preise bei Merian

In einigen ihrer Briefe gibt Maria Sibylla Merian Preise für die komplette, 60 Tafeln umfassende *Metamorphosis* an, sowohl für die unkolorierte als auch die kolorierte Ausgabe, wohingegen Baltasar Scheid ausschließlich von Einzelblattpreisen spricht.

So teilt sie im April 1704 James Petiver mit, daß ihr Werk unkoloriert 6 Reichstaler oder 3 Dukaten kostet.[126] Der Reichstaler galt zu dem Zeitpunkt 2 ½ Gulden, das ergibt einen Betrag von 15 Gulden. Den gleichen Preis nennt sie auch ein Jahr später in ihrem Schreiben vom 16.April 1705 an Volkamer[127] und kurz darauf nochmals an Petiver[128], wobei sie beide Male betont, daß die 15 Gulden den Subskriptionspreis darstellen, der Preis nach Ablauf der Frist - deren zeitliche Begrenzung sich nach den bisher bekannten schriftlichen Zeugnissen etwa auf die Zeit von März 1703 bis April 1705 erstreckte - aber 18 Gulden betragen werde.

Zum Vergleich mit Scheids Einzelblattpreisen seien Merians Angaben, die nur für das Werk als Ganzes gemacht wurden, ebenfalls auf das einzelne Blatt zurückgerechnet.

125 Cottesloe & Hunt (1983); Rowley (1987).
126 Kat. Frankfurt/M. (1997), Brief 12, S. 267.
127 Ebd., Brief 14, S. 267/268.
128 Ebd., Brief 15, S. 268.

Bei 15 Gulden (ein Gulden hat 20 Stüver) ist das ein Preis von 5 Stüver pro unkolorierte Seite mit zugehörigem Text. Der Blattpreis würde sich nach Ablauf der Subskriptionsfrist um1 Stüver auf 6 Stüver erhöhen. – Für das Illuminieren der 60 Kupfer verlangt Merian 30 Floren (Gulden)[129], das bedeutet für die Kolorierung eines einzelnen Blattes den Preis von 10 Stüver. Ein fertig ausgeführtes Blatt kostet somit 15 Stüver. Auch sechs Jahre nach Ersterscheinung verlangt sie entgegen eigenen früheren Ankündigungen immer noch 45 und nicht 48 Floren (kol. Ausgabe).

Baltasar Scheid nennt in seinem Schreiben vom 20.Juli 1703 (Bf.23) einen Preis von 12 Stüver je illuminierte Tafel; 3 Stüver weniger als von Merian verlangt. Für das Gesamtwerk bedeutet das 36 Gulden, 9 Gulden weniger als von Merian an Volkamer in Rechnung gestellt. Diese 9 Gulden Differenz stellen die Kosten für Papier (einfaches) und Druck des beschreibenden Textes sowie Titelkupfer und –seite dar. (Oder hat Merian, geht man von den von ihr genannten 5 Stüver pro gedruckter Tafel aus, in der Anfangsphase für die Kolorierung einer Tafel nur 7 Stüver veranschlagt, eine Erhöhung hierfür aber nicht ausgeschlossen, was bei Scheid im o.g. Schreiben zu der Vermutung einer Preisanhebung auf 1 Gulden pro Seite führt?)

Merian könnte tatsächlich zu Beginn der Drucklegung noch keine feste Preisvorstellung für die Kolorierung gehabt haben, die entweder von ihr selbst, ihrer Tochter Dorothea Maria oder eventuell von Schülerinnen ausgeführt wurde. Denn hier bestand für sie im Vorfeld die Möglichkeit einer Preisänderung; Stecher, Druck, Papier dürften ein festgesetzter Preis gewesen sein, den sie nach Ablauf der Subskriptionsfrist um 3 Gulden auf 18 Gulden anzuheben vorhat, nicht erhöhen wollte sie parallel dazu offenbar die Kolorierungskosten. -Merian kündigt zwar im April 1705 zwei Mal den nun zu zahlenden höheren Preis an[130], verlangt aber wie bereits gesagt Jahre später immer noch nur den Subskriptionspreis. Es erscheint fraglich, ob überhaupt jemals von ihr der Preis von 18 bzw. 48 Gulden (unkol. bzw. kol. Exemplar) für die *Metamorphosis* in Rechnung gestellt worden ist, einen Nachweis dafür gibt es nicht; oder ob nicht auf Grund eines enttäuschend verlaufenden Verkaufs die Preise bei 15 bzw. 45 Gulden belassen wurden.

Es findet sich aber noch eine ganz andere, wesentlich höhere Preisangabe für eine „gemalte“ Ausgabe der *Metamorphosis* in Merians Brief vom 2.Oktober 1711.[131] Dort spricht sie bekanntlich von einem illuminierten Exemplar zu 45 Gulden, „wan man sie aber will haben gemahlt, so kost das Indianische 75 fl Holländisch ...“. Hier handelt es sich um ein Angebot, Repliken auf Papier anzufertigen zu einem selbstverständlich höheren Preis. Dazu ist zu bemerken: schon im April macht Merian die Unterscheidung zwischen „sorgfältig gemalt oder illuminiert“ bei einem möglichen Exemplar für die englische Königin, allerdings ohne

[129] Ebd., Brief 14, S. 267/268.
[130] Kat. Frankfurt/M. (1997), Briefe 14 u. 15, S. 267 - 268.
[131] Ebd., Brief 17, S. 269.

Preisangabe.[132] Auch von Uffenbach beschreibt zweierlei Ausführungen: auf Pergament gefertigte Abbildungen aus Surinam „nach dem Leben unvergleichlich gemahlt“ sowie „ihr eigen Werk von Surinamischen Insecten, so sie selbst ... nach dem Leben illuminiert“.[133] Von Uffenbach benutzt den Ausdruck „illuminiert“ nur für das Druckwerk, so wie es Merian selbst auch tut, ohne daß sie eine Unterscheidung macht, ob die Kolorierung von ihr selbst oder anderen ausgeführt wurde. Wohingegen der Begriff „gemalt“ von v.Uffenbach nur für Arbeiten auf Pergament gebraucht wird. Und auch Merian hat den Ausdruck „ gemalt“ nur für Originalzeichnungen bzw. Repliken auf Pergament oder Papier verwendet [134]. Die Abbildungen für die *Metamorphosis* hat sie z.B. mehrfach „gemalt“.[135] Die Annahme von F.Pieters, „gemalt“ beziehe sich ausschließlich auf die kolorierten Umdruckexemplare der *Metamorphosis*, ist nicht eindeutig nachweisbar.[136]

Für die Teile I und II von *Der Raupen wunderbare Verwandelung* lassen sich vier unterschiedliche Preisangaben nachweisen. Einzig von Uffenbach macht eine Angabe zu unkolorierten Exemplaren in Höhe von 5 Gulden für beide Bände, d.h. 2 ½ Gulden pro Band; in kolorierter Form kosten beide zusammen 20 Gulden.[137] Die Kolorierungskosten betragen somit 7 ½ Gulden pro Band. Auch der Brief Merians vom 5.Okt. 1703 nennt für beide Teile zusammen (koloriert) den Preis von 8 Reichstalern oder 4 Dukaten, das sind ebenfalls 20 Gulden (1 Reichstaler = 2 ½ Gulden).[138] Gleiches entnimmt man ihrem Schreiben vom 2.Okt. 1711, nämlich 10 Gulden pro Band „corios Iluminirt“. Aber: „wan man sie aber will haben gemahlt, so kost ... das in quart 20 fl hollendisch ...“.[139] Auch hier wie für die *Metamorphosis* ein deutlich höherer Preis für Repliken. Die weitaus höchste Preisangabe findet sich in dem Schreiben Richard Bradleys vom 4.Juli 1714 N.S. an James Petiver.[140] Merian verlangt Bradleys Angaben zufolge etwa 500 Florin (Gulden) für die von ihr angefertigten Originalzeichnungen auf Pergament. Angenommen, 1714 waren bereits alle 50 Vorlagen für den dritten Teil des *Raupenbuches* erstellt, d.h. 150 Einzeldarstellungen für die drei Bände lagen vor, so würden 50 Abbildungen auf Pergament zu dem Zeitpunkt etwa 167 Gulden kosten. Die gleichen 50 Abbildungen 1711, gemalt auf Papier als Bildträger, 20 Gulden; als koloriertes Druckwerk nur noch 10 Gulden.

Was läßt sich zu anderen Arbeiten Merians sagen? Die auf Pergament angefertigten Vorlagen zur *D'Amboinschen Rariteitkamer* sollten laut Bradley 100

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132 Ebd., Brief 12, S. 267.

133 Uffenbach (1754), Teil 3, S. 552 - 554.

134 Kat. Frankfurt/M. (1997), Brief 7, S. 264/265.

135 Sie befinden sich in der Royal Library, Windsor Castle; im British Museum, Dept. of Prints and Drawings und in St. Petersburg : Rücker (1982 a).

136 Pieters / Winthagen (1999).

137 Uffenbach (1754), Teil 3, S. 552 – 554.

138 Kat. Frankfurt/M. (1997), Brief 11, S. 266/267.

139 Ebd., Brief 17, S. 269.

140 Tjaden, Part 4, S. 9, in: Bull. of the ASPS, Vol. 9 (1974).

Guineas kosten, das entspricht etwa 1167 Gulden (1 Guinea galt etwa 4 2/3 Reichstaler, der wiederum für 2 ½ Gulden gehandelt wurde).[141] Eine hohe Summe für großartige Abbildungen auf 60 Blättern. Für das kolorierte Druckwerk verlangte Merian dagegen 60 Gulden. – Für die 32 Vogelbilder setzt Merian Bradley zufolge den Preis von 400 Gulden an, eine große Summe für Arbeiten auf Pergament und scheinbar nur als Ganzes zu erwerben.[142]

## 6 Schlußbetrachtung

Die in der Universitätsbibliothek Erlangen befindlichen Briefe des Amsterdamer Kaufmanns Baltasar Scheid belegen, daß der in Merians Briefen mehrfach genannte Herr Schey oder Schrey und der Briefeschreiber Scheid ein und dieselbe Person sind. – Durch Scheids Angaben kann der Entstehungsprozeß von Merians *Metamorphosis* nicht nur zeitlich genauer eingegrenzt werden, es stellt sich auch der Produktionsverlauf – vom Stechen und Drucken bis hin zum begleitenden Text – deutlicher dar.

Die sich unerwartet lang hinziehende Herausgabe der *D'Amboinsche Rariteitkamer* von Rumphius, zu der Merian die Vorlagen angefertigt hatte, wird von Scheid in lebhaften Worten beschrieben.

Durch die Briefe Richard Bradleys konnte auf eine weitere Verbindung von Maria Sibylla Merian nach England hingewiesen werden. An Hand dieser Briefe und einem Vergleich von Bradleys *Historia plantarum succulentarum* mit einer Zeichnung Merians wurde der Versuch unternommen, eine Abbildung aus Bradleys Werk als einen nach Merians Vorlage angefertigten Kupferstich nachzuweisen. Diese Vorlage entstand als Auftragsarbeit.

Sowohl durch Scheid als auch durch Bradley wird der „Familienbetrieb M.S. Merian" noch einmal verdeutlicht. So erfährt man durch Scheid von der Mithilfe der Tochter Dorothea Maria bei den Kolorierungsarbeiten an der *Metamorphosis* der Mutter; durch Bradley, daß M. S. Merian für ihre abwesende Tochter Johanna Helena tätig ist, indem sie deren Belegexemplare von dem Werk *Horti Medici Amstelodamensis* zum Verkauf anbietet. Mutter und Töchter arbeiten Hand in Hand.

Die Preisangaben bei Scheid und Bradley führten zu der Frage, ob es bei Merian jemals eine Preissteigerung bezüglich des Hauptwerkes gegeben hat. Es gab sie nicht. Trotz anders lautender Ankündigungen läßt sich bis heute an keiner Stelle nachweisen, daß Maria Sibylla Merian für die *Metamorphosis* einen anderen als

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141 Marperger (1707).

142 zum Vergleich: 1704 bezog ein Stadtschreiber in Frankfurt/M. ein Jahresgehalt von 100fl., dazu kamen

Extraleistungen: Elsas (1940), Bd.II-Teil A, S. 619; der Preis für 1 Pfund Butter betrug in der Zeit von 1705-

1714 (ebenfalls Frankfurt/M.) ca. 37 – 43 Pfennig, das sind etwa 5-6 Stüver: Elsas (1949), Bd.II-Teil B, S. 103.

den Subsriptionspreis verlangt hat. Wahrscheinlich ist es bis zu ihrem Tod nie zu einer anderen Forderung gekommen.

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# Gregor Kraus (1841–1915) – versatile botanist from Lower Franconia[1]

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**Abstract.** During his life as a scientist, Gregor Kraus (1841–1915) experienced different stages of botany as a rising and more and more extensive discipline. Under the guidance of August Schenk (1815–1891) he started with comparative studies on shape, form and microscopic structure of living and fossil plants. This approach was widened and intensified when Kraus was working with Heinrich Anton de Bary (1831–1888). Before, he joined Julius Sachs (1832–1897) in Bonn (and – later – in Freiburg im Breisgau) who then was the rising star of botany as an inductive science (a term introduced by Mathias Jacob Schleiden, 1804–1881). Kraus' position in the labs of Sachs and De Bary was what we now call a postdoc. During the end of his life, Kraus became one of the trendsetters of experimental field work thus cofounding ecophysiology, which was to become a central and highly sophisticated field of botanical research by a large group of scientists among whom Otto Ludwig Lange (*1927) is an outstanding personality. The chair of ecological botany that Lange held at Julius-Maximilians-Universität Würzburg (1967–1992) is one among three into which Kraus' former professorship was extended after World War II.

1 Thankfully dedicated to Otto Ludwig Lange on occasion of his 80th birthday in August, 2007

## Science in Germany after the war of 1870/71

Today, it is a matter of course to study a rather comprehensive topic that we usually call *life sciences*. Departments for teaching and research are named after *biology* or *life sciences*. Young biologists start with the basic instruction on what characterizes the living state in general, mainly by explanations on structure, metabolism, irritability and reproduction of cells. They get introductory training in physics, different branches of chemistry and physical chemistry in order to understand the numerous chemical reactions that are summarized by the general notion of metabolism and further aspects of organic matter. The fundamental metabolic processes are the same in all types of living beings. There is evidence that life on earth was monophyletic. From a single root comprising early preliminary stages of life a highly diversified set of descendents arose: prokaryotes and eukaryotes, heterotrophs and autotrophs (phototrophs, chemotrophs). All this amounts to what may be called *general biological thinking*, reflected by the current generalizing notion *life sciences*.

At the turn from the 19th to the 20th centuries, general understanding of the sciences of animals, humans and plants was rather different. Researchers in the different fields of life sciences only started some feeling of fellowship. The current misleading impression of *the* cell or *the* organism as overall standard models was already emerging, but not dominating. The specifity of every organism was still of fundamental importance in biology. Today, it is often pushed into the background in basic curricula of current instruction. To see both "paradox" aspects of organisms in a synopsis is a matter of dialectics. Since there are very similar basic processes in bacteria, fungi, plants and animals, we must conceive an organism as one of many examples of living beings. On the other hand, it just as well must be considered something unique and unreplaceable, different from all other living beings. We are always dealing with identity as well as with similarity. In general biology, we equalize non-identical organisms by only considering special aspects of the whole animal, plant, fungus or bacterium. Kraus equally appreciated both, case studies and comparative research.

Perception of speciation as an essential aspect of biology always goes up and down. There are periods of prevailing comparative (and morphological) approaches and others with main stress on one of the general aspects of life. Some biologists are gifted in one respect, others in another, often opposite respect. History of biology is fascinating since it aims at the personality as well as at life conditions which the scientist under consideration once experienced. An important component of botany is deeply rooted in its ancestral early form which, for many centuries, was *materia medica*. This special topic was essentially necessary for the basic training of physicians. As a young lecturer in Würzburg, Kraus had to teach this matter in classes. This kind of lectures formed a major part of his official duties (now labelled: pharmaceutical biology). From history botany inherited a rather big amount of applied science, i.e. knowledge of pharmaceutical merchandise. This may be one reason for the long-lasting influence of topics and concepts of natural history, especially the way of understanding plant morphology as configurationism. "Physiologization" of zoology was ahead compared with that of botany. Animal life was studied substitutionally for human life. However, in the 19th century, this narrow relationship turned out to be an obstacle for progress in animal physiology since then there was much aversion to the *must* to incorporate chemical ideas. Upcoming chemistry had a touch of pharmacy, manufactures, industry, mechanical and physical work, and thus appeared very different from the supposed essentials of vitality (*vis vitalis, élan vital* etc.). Therefore the German protagonists of human physiology, Emil du Bois-Reymond (1818 – 1896) and Hermann Helmholtz (1821 – 1894), were confronted with much more reservation from zoology than was Julius Sachs (1832 – 1897) with his program of experimental studies of plant life. Thus, plant physiology became a model science just when Kraus started his scientific career.

Reformation of scientific disciplines consists in redefining notions, subjects and borders. This process has an ambivalent character: regeneration on one hand, loss on the other. In periods of transition the weight of parts plaid by *actors on the stage of science* are changing. Some disappear without leaving traces, some escape to continue with tradition, newcomers enter the stage. Early in 1914/5 the Kaiser-Wilhelm-Gesellschaft established an "Institut für *Biologie"* in Dahlem near Berlin. In 1926, the publisher Julius Springer and the editorial board renamed a well-known reference journal *Berichte über die wissenschaftliche Biologie.* Both phenomena were programmatic. Thus a tendency emerging already at the turn of the 19th and 20th century became manifest. Attempts to bring together the very different approaches of medicine, botany and zoology in order so summarize current knowledge in a general pool called biology had a rather long "lag phase". It took a rather long time until consolidation of the whole of biology was accomplished, however, from now on "upside down". This opposite way was starting from the general notion of a living being, studied from a quite different point of view under general aspects, thus forming a true general biology. From this summit, in a top-down-manner, humans, animals, plants, microbes are taken all for living beings, each defined by what are their essential characters. Botany now was dealing with living beings whose special type of vitality is characterized by "harvesting the sun", growth and lack of motility. In the time of emerging of environmental concepts this required better understanding of the relation between the higher plant and its growth site. At the interface between higher animal and its environment we deal with movements that make up behaviour (psychology), at the interface between higher plant and its environment we deal with growth (physiology). However, in either case, current psychology, philosophy and general biology of that time helped to better understanding of this interrelation. The counterpart of Jakob von Uexküll's (1863–1941) idea to attribute a special environment to each given animal species was the definition of autecology in 1902 by the Swiss botanist Carl Schröter (1855 – 1939).

Gregor Kraus followed this in studying *Boden und Klima auf kleinstem Raum* (Soil and climate in the narrowest environment thus preparing what later was to become *Standortklima* or *microclimate* (or: *climate near the ground*). However, we shall see one further characteristic of periods of transition. We often meet radical iconoclasts and renegates side by side with deliberate people who respectfully make use of traditional knowledge. They do so in combining application of modern methods and experience surrendered by former generations. This can be seen from Kraus' book *Boden und Klima* containing many observations of different species in the field under a general new concept. Kraus was looking for the environments of different species of higher plants by carefully considering their structure and growth requirements. This mode to proceed to modern ecophysiology of plants was much more evolution than revolution. Autecological knowledge was essential for this approach. The way was paved by sociology, philosophy and zoology. Uexküll, referring to the philosopher and economist Werner Sombart (1863 – 1941) underlined: "It is not adequate to describe a forest as a statically determinate and objective environment. Instead, there are foresters', hunters', botanists', hikers', naturelovers', logger' and berry gatherers' woods, in addition to the fairy tale wood, in which Hansel and Gretel lost their way" (English version cited from Hauser 2004). Hans Kniep (1881– 1930) Kraus' follower on the botany chair in Würzburg, characterized his predecessor as follows: "He did not like to swim with the current, neither in science, not in private life. His leading principle was to do independently whatever he did. He was one among the few who mastered the art of living (Kniep 1916b: 178-9). With these fundamental characters Kraus is a good example for a case study on tradition and novelties in botany at the turn of the 19th to the 20th centuries.

In the era of the Emperor Wilhelm II, the German public considered university professors infallible authorities in highly specialized fields of research. Medical, zoological or botanical sciences were kept separate, while microbiology as a whole was not yet constituted, though there were preliminary stages in medicine, botany, and agricultural sciences. However, people then still profited from a traditional German all-round education. General biology and environmental studies were emerging but still matters of some few specialists. Humanities or liberal arts seemed to flourish, but what later was to be called the *phenomenon of the two cultures* cast its shadow before. However, the splitting of the whole set of educated people into two *subsets without common elements* was not yet fully performed. People of arts, letters and sciences met in multidisciplinary circles in an air favouring open-minded discussion on current intellectual matters on a rather high level. Even regional newspapers then included ambitious feuilleton pages. The whole of natural sciences was in an atmosphere of departure and full of belief in progress. Evolution was still mainly perceived as a phenomenon of natural history resulting in change of species. The biological paradigm of the theory of selection was of paramount importance. Laboratory equipment and research methods became more and more sophisticated offering excellent possibilities for the study of all kinds of organisms. All these innovations enabled biologists to perceive further phenomena and to approach problems whose solution appeared impossible before. Scientific aspects that we now altogether summarize under the nearly equivoque label "environment" were on the agenda in philosophy and in all types of social, economic, earth and life sciences, with biological and medical psychology right in front. Uexküll published his trail-blazing book *Umwelt und Innenwelt der Tiere* (*Environment and inner world of animals*) in 1909. In Austria in 1907, Julius Wiesner (1838–1916) was the first to study what later was called light-climate. Christian-Ernest (later: "Ernst") Stahl (1848–1919) in Jena, where he was full professor of botany since 1881, used the diversified and pittoresque surroundings of the town on the banks of river Saale as an open-air laboratory of early experimental plant ecology. He, too, had worked under Sachs in Würzburg where he wrote his application to become a lecturer ("Habilitationsschrift") and successfully synthesized a lichen in vitro.

In a similar way like in Jena, Würzburg was rather stimulating for a scientist. Obviously, it was a quiet provincial town with beautiful baroque scenery surrounded by vineyards. However, this type of quiet environment was the standard of most traditional German universities, probably with the exception of Berlin and Munich. Würzburg, though appearing provincial, nevertheless harboured rather vivid activities of arts, letters, medicine and science. So the town had become a centre of attraction for outstanding personalities of academic life, also from outside Germany. These attracted many highly qualified young scientists who assembled around August Schenk, Rudolph Albert Kölliker [later: "von Koelliker"] (1817–1905), Rudolf Virchow (1821–1902), Franz Leydig (1821–1908), Karl Semper (1832–1893), Wilhelm Konrad Röntgen (1845–1923), Julius Sachs, and later Theodor Boveri (1862–1915), many of them listed by Lehmann (1933), Schwarzbach (1981), Franke (1982), Otremba (1982) and Masuda (2001: 302). Ernst Haeckel (1834–1919) came to Würzburg as a medical student in 1852 and enthusiastically praised the atmosphere of the town (see Hemleben 1964: 23–31). One special pilgrim to Würzburg was Jacques Loeb (1859–1924) who studied with Kölliker and Sachs. He was deeply influenced by the modern way of research in plant physiology. With this background, he joined people with related opinions in the inspiring environment of the Zoologische Station Neapel (at Naples; or Stazione Zoologica Napoli), and later at Woods Hole, Mass. in the Marine Biological Laboratories. The intellectual atmosphere of Würzburg is well reflected in literature by authors who are not just world famous but well-known to those who like belles letters. Best known authors are Max Dauthendey (1867–1918), Leonhard Frank (1882–1961) and Margret Boveri (1900–1975). The famous writer Hermann Hesse (1877 – 1962) once confessed: „Wenn ich ein zukünftiger Dichter und gerade mit der Wahl meines Geburtsortes beschäftigt wäre, dann würde ich Würzburg sehr in Erwägung ziehen" (cited after Rottenbach 1972: 81). It is necessary to refer to this style of life that was cruelly finished by World War II and the destruction of classical Würzburg in 1945. University life today is characterized a little bit maliciously as site of academic work producing intellectuals who hope to profit from prosperity. It is thus less romantic, and only some exceptional cases remind the spirit of the former periods. Otto Ludwig Lange is one such example.

Gregor Kraus' lifetime was a period of vivid progress as well as one of radical change in biology (biographies were written by Kniep 1916a, 1916b, Mollenhauer 2005). Thus, he experienced an era of transition. When he was a student of botany in Würzburg, the chair of his professor Schenk was that of cameralistics. When he came back from abroad to take over this very chair as successor of Julius [von] Sachs, his discipline now forming part of the philosophical faculty had changed radically. Sachs, starting from scratch, set a new standard of research on the main problems of plant life with far-reaching consequences. Deeply impressed by Sachs' lifework some people from medicine and zoology paved the way to modern general physiology of all living beings (see also Höxtermann 1998, Penzlin 1998).

## Plant Physiology

Kraus was a distinguished plant physiologist of his time [see Kniep 1915: (76)-(82), Möbius 1968]. At the time when he obtained his doctor's degree, Kraus was deeply impressed by the "Handbook of the Experimental Physiology of Plants" (1865) and went, without much hesitation, to the lab of its author, Julius Sachs (1832–1897), whose attitude of quantifying physiological correlations guided his own further research. Kraus' did his first physiological experiments on *growth physiology*. Sachs was convinced that tropic curvatures and growth rotations were caused by differences in tissue tension. Kraus determined the tissue tension of stems – later (1867) using the results for his "habilitation" – and found, among others, day-periodic variations later generalized as universal "daily swelling periods" (1881). In diverse series he measured day-rhythmic volume changes of many plant organs with swelling maxima immediately before sunrise. The up and down of tissue swelling was demonstrated to be directly connected with water content and transpiration. Thus, Kraus turned to his second main research topic: *water balance* of plants. He studied the distribution of water in relation to organ growth, movement and swelling (1879, 1881) as well as to cell sap properties and components in solution (1880, 1884).

In his *cell sap analyses* he particularly focussed on free organic acids, beside carbohydrates and proteins, of which diurnal changes were also known. In 1875, the Heidelberg agricultural chemist Adolf Mayer (1843–1942) had observed accumulation of organic acids in some plants during the night. Kraus found this to be a more general phenomenon and drew special attention to the periodic in- and decrease of free acids in the metabolism of Crassulaceae (1886). The rise of acidity with a maximum in the early morning was shown to be dependent on oxygen (respiration) whereas light forced its fall. Kraus described diurnal reciprocal acid and carbohydrate cycles: malic acid, nightly produced and stored, would be oxidized during the day to "carbon acid" ($CO_2$) being again assimilated by "illuminated chlorophyll" to carbohydrates as a new acid source. Kraus had, with unbelievable intuition, anticipated the real relations, nearly a century before CAM (Crassulaceae Acid Metabolism) photosynthesis was finally outlined in the 1970s (cp. Black and Osmond 2003). – Kraus (1884) also semi-quantitatively analyzed the constituents of the phloem sap (of *Cucurbita*) and found, beside proteins, considerable amounts of carbohydrates, being an early evidence for the transport of assimilates in the sieve-tube system.

In the 1860s, Sachs had founded modern plant physiology with a new theory of $CO_2$ assimilation that convincingly combined and explained photosynthetic light action, chlorophyll function and starch formation having had been a mystery for a long time (see Höxtermann 2001). Initiated by Sachs, Kraus (1869) investigated the rate of $CO_2$ assimilation and showed that in *Spirogyra* cells the first starch granules emerged already five minutes after illumination. His further experiments on photosynthesis were focussed on the side of light action by means of a micro-spectral apparatus he first applied in botany. In 1872, a fundamental monograph on *chlorophyll* was published with many new findings: Kraus particularly studied the absorption behaviour of the photosynthetic pigments whose investigation so far had been a domain of physicists (see Höxtermann 1995). The Scotch physicist David Brewster (1781–1868) was the first to report on two main absorption bands of an alcoholic crude extract of chlorophyll in the red and blue spectral regions. In this respect, he had found no differences between the chlorophylls of varying species of green plants (Brewster 1834). Kraus finally differed, all in all, seven characteristic absorption bands of chlorophyll being identical in all green plants. His spectrum (Kraus 1872, Tab. I, Fig. 4) was later on reproduced and discussed in nearly all textbooks of botany for many decades. Furthermore, Kraus confirmed a small red-shift of the absorption bands of chlorophyll in living leaves compared with extracted chlorophyll – an early hint to the specific binding relations of chlorophyll *in vivo*. This red shift became popular with Kraus, but was discovered somewhat earlier (1870) by the physicist Eduard Hagenbach (1883–1910).

Particular attention found another result of Kraus' chlorophyll "bible": the evidence of the heterogeneity of the photosynthetic pigments in green plants by a simple separation technique. Already about 1800, French chemists had reported on alcoholic extracts of green leaf pigments which Pierre Joseph Pelletier (1788–1842) and Joseph Bienaimé Caventou (1795–1877) called "chlorophyle" [sic!] (Pelletier and Caventou 1817: 490). The green crude extract was, for a long time, considered to be a homogeneous solution, but already Pelletier (1832) suspected that it could be a non-uniform mixture of various components. His assumption was experimentally confirmed only by the physicist George Gabriel Stokes (1819–1903) who spectroscopically differed at least two green and two yellow pigments (Stokes 1864). But it stayed difficult to separate them. – In the first half of the 19$^{th}$ century the studies of chlorophyll were dominated by chemists. They tried to isolate and identify the pigments with concentrated acids, alkaline solutions, boiling media or other aggressive chemicals causing much confusion. The strategy changed in the 1860s when more physicists and botanists entered the field and combined gentle chemical separation procedures with spectroscopic analyses (Höxtermann 1980, 1992, 1995). In this context, Kraus (1872) developed a reliable technique to split green and yellow leaf pigments on the basis of differences in solubility. He divided an alcoholic crude extract from green leaves into a green and a yellow fraction by means of benzene (benzol):

"[Es] genügt, eine Portion alcoholischer Chlorophylllösung mit einer beliebigen Menge Benzol zu schütteln, um sofort eine Trennung der ursprünglich grasgrünen Lösung in 2 verschiedenfarbige Schichten zu veranlassen. – Fast augenblicklich sondert sich nach tüchtigem Schütteln die Mischung in eine unten stehende alcoholische goldgelbe Lösung, und eine darüber stehende Benzollösung von einem Grün, das eine deutlichen Stich in's Blaue hat." (Kraus 1872, p. 88) Kraus named the pigments in the green part "cyanophyll" and those in the yellow part "xanthophyll". The suitability of benzene for pigment differentiation was noticed when he, together with Alexis Millardet (1838-1902), applied the solvent in studies of algae pigments (Kraus and Millardet 1868) during their joint stay in the lab of Sachs in Freiburg/Breisgau in 1867. In 1877, Robert Sachsse (1840–1895) used benzine (petrol) instead of benzene. Both procedures became standard methods for the separation of chlorophylls and carotenoids and are still in use. Today, "Kraus' separation method" is an epitome for an easy and unfailing way to demonstrate the heterogeneity of green leaf extracts (cp., for instance, Brauner and Bukatsch 1973: 96).[2] – The partition of mixed photosynthetic pigments by non-mixable solvents was later perfected by Richard Willstätter (1872–1942) who determined the sum formulas of various chlorophylls and chlorophyll derivatives (Willstätter and Stoll 1913). He based his pigment analyses on differences in solubility whereas Michail Semënovič Cvet (1872–1919) developed a more efficient chromatographic technique on the basis of differences in the adsorption affinity (Tswett 1906).

In 1884, Kraus concentrated on another striking vegetable substance, the *tannin*. He summarized the results in his "Base Lines to a Physiology of Tannin" (1889) containing diverse news of great magnitude. Albert Wigand (1821–1886) and Theodor Hartig (1805–1880) had taken the view that tannins were "plastic" and reserve substances (Wigand 1862, Hartig 1869). Kraus, however, saw in tannins protective substances, especially against putrefaction and rottenness. The tannins would be synthesized in the leaves by light, but not primarily as a product of $CO_2$ assimilation; they rather were a kind of secondary plant material that

[2] In the early 1870s two botanists with the name Kraus dealt with chlorophyll. They are sometimes mistaken for each other. Carl Kraus (1851–1918) was a student of Carl Nägeli (1817–1891) in Munich and wrote his thesis (1875) on the nature of chlorophyll. Later he headed the agricultural academy at Weihenstephan (cp. Haushofer 1980).

couldn't be metabolised and would be disposed outside the leaves. – The idea of a protective function became important in plant pathology. At the end of the 19th century, plant pathologists were mainly concerned with the morphology of hyphae, the chemistry of fungous secretions and enzymes and the mechanisms of parasitic attacks (see Höxtermann 1998: 535). Kraus (1889) attracted the attention to the resistance of the host tissue against fungal infection being caused by preformed secondary plant substances. His belief was later experimentally confirmed in the case of tannic acid (Cook and Taubenhaus 1911) and phenol (Walker 1923).

The name of Kraus is further connected with another well-known plant physiological phenomenon, the *self-heating* of the spadix of *Arum italicum*. Kraus (1882, 1884) measured the daily course of the temperature of the flower cob, with a maximum in the afternoon, when the difference to the surroundings could increase up to 36 degrees – an astonishing fact that has often been quoted in botanical textbooks. The rising temperature was attributed to respiration processes closely connected with changing transpiration. Micro-chemical analyses of the spadices before and after the warming up brought interesting insights: starch and sugar entirely disappeared, while proteins, amides and ash substances stayed unchanged and free acids rose slightly. This meant for Kraus that carbohydrates were burned in the spadix of *Arum* – a conclusion being in contradiction to the prevailing view of the time that respiration would be coupled with a protein-breakdown.

Later, Kraus extended some of his crucial experiments to tropical plants when he travelled to India and Java, in 1893 and 1894. The blossoms of cycads and palms even showed several thermo-periods at succeeding days (Kraus 1896). Kraus gave an ecological interpretation: the flower warming would lure pollinating insects. By the way, during his travel to the tropics Kraus (1895) also measured the daily growth in length of the fast growing *Bambusa gigantea* and registered a spectacular increase of 57 cm per 24 hours.

## Ecology and ecophysiology, especially microclimate

Kraus was the first who defined microclimate as being the climate of the narrowest space. In his renowned work *Boden und Klima auf kleinstem Raum* (The Climate near the Ground, see Geiger 1911 and Geiger, Aron & Todhunter 1995), he was able to demonstrate, that the microclimate does not match the general climate profile, and sometimes might even represent the general climate at different degrees of latitude. This new discovery was essential for the understanding of plant life and led to a complete new field of research.

In the first edition of the textbook *Das Klima der bodennahen Luftschicht*, the meteorologist Rudolf Geiger (1894–1981) attributed Kraus as the *founder of microclimatology* (Geiger 1927;1933). The influence of Kraus' work on plant ecology can not be underestimated. When his work was published, it entailed a flood of studies, all articles pronouncing the importance of the knowledge the microclimate, describing the direct life conditions for plants and animals, and demonstrating the striking difference between microclimatic and general climatic conditions. Geiger (1933) stated, that the new field of microclimatology created an excellent example for interdisciplinary research in meteorology, botany, zoology and medicine, linking their common interest in order to understand how organisms cope with conditions of life in different environments. One of Geiger's students, Albrecht Vaupel (*1924), returned to Kraus' classic research site down-stream of Würzburg ("Kalbenstein") and wrote his doctoral dissertation in meteorology studying plant microclimate just there (Vaupel 1958). Eventually, this author became an employee of Deutscher Wetterdienst and – until his retirement – was on service as a meteorologist at the special station dedicated to studies of the climate near the ground in vineyards. It was situated at the famous site Würzburg-Stein. This is one example of the long-lasting effect of basic studies done by Kraus just in this area around the Capital of Lower Franconia.

Enlightened by his professor Stahl and by the basic work of Kraus, Alwin Schade (1881 – 1976) demonstrated in 1917 convincingly the value of microclimate for understanding the geographical distribution using the two mosses *Mylia taylori* (Hook.) S.Gray (as *Leptoscyphus taylori* [Hook.] Mitt.) and *Pohlia nutans* (Hedw.) Lindb. (as *Webera nutans* [Schreb.] Hedw.). The two mosses grew in a distance of 50 m to each other on rock boulders, but differently exposed. While *M. taylori* had a northern exposure with a mean annual temperature of 6.2 °C (min. – 6 °C, max. 17 °C), *P. nutans* grew in a southern exposure and experienced a mean annual temperature of 23.3 °C (min. – 9.7 °C, max. 56.8 °C). These mean annual temperatures represent general climatic conditions of a distance of 40° latitudes to each other. Transferred onto flat ground at sea level, this means, that if the one moss would grow near Rostock (Baltic coast, Germany), the other would grow near the northern coast of Lake Chad (Niger)!

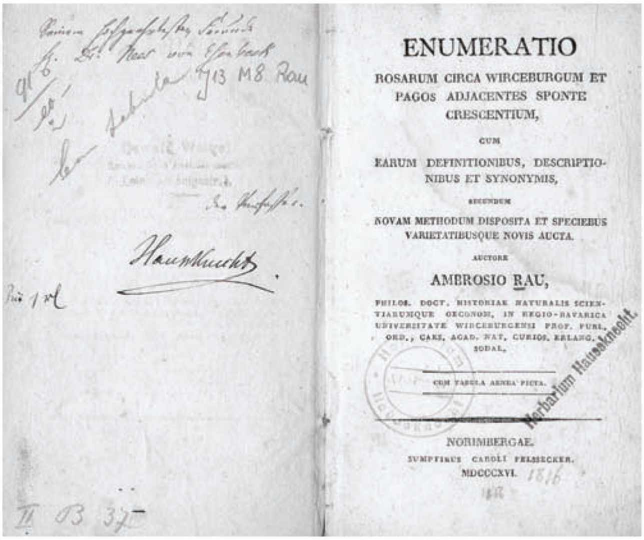
Considering the multitude of micro-climatically defined niches in tropical and temperate forests, it becomes immediately evident, that not only at the ecosystem level as seen before, but also at the level of biomes, microclimatic differentiation must be recognized as a major factor for biodiversity.

This is also reflected in the postulated *Gesetz der relativen Standortkonstanz* (= relative constancy of habitat[3]) by Heinrich (1898-1989) and Erna Walter (1893-1992): *Wenn innerhalb des Wohnbezirks oder Areals einer Pflanzenart das Klima sich in einer bestimmten Richtung ändert, so tritt bei dieser Art ein Wuchsort oder Biotopwechsel ein, durch den die Klimaänderung mehr oder weniger aufgehoben wird.*(H. & E. Walter 1953). This means that if the climate within the residential district or the area of distribution of

[3] It might be noted that the classic definition of „Standort" is not synonymous with „habitat". However, it became current practice to use the two words as synonyms. "Habitat" is a conjugated verb of "habitare" and means originally "he/she/it lives". Linnaeus used the term to describe the site where a plant grows. The distinction of site and condition of growth, easy in German, is difficult to express in English.

a plant species changes into a distinct direction, this species moves into a habitat or biotope that compensates for the climate change. For example, if the climate gets cooler in the residential district of a plant, it will make its way to sun exposed slopes to compensate cooling and disappear from other habitats in the district.

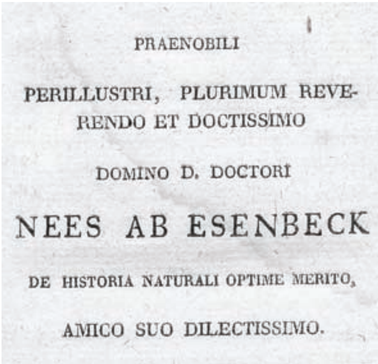
With the demonstration of the importance of the microclimate and the careful analysis of the plant habitat, Kraus showed the way to modern ecological research and can thus also be seen as one of the founders of eco-physiology. His scientific impact is still recognized in modern textbooks in plant ecology (e. g. Frey & Lösch 2004; Schulze, Beck, & Müller-Hohenstein 2002).


ENUMERATIO
ROSARUM CIRCA WIRCEBURGUM ET PAGOS ADJACENTES SPONTE CRESCENTIUM,
CUM
EARUM DEFINITIONIBUS, DESCRIPTIONIBUS ET SYNONYMIS,
SECUNDUM
NOVAM METHODUM DISPOSITA ET SPECIEBUS VARIETATIBUSQUE NOVIS AUCTA.
AUCTORE
AMBROSIO RAU,
PHILOS. DOCT. HISTORIAE NATURALIS SCIENTIARUMQUE OECONOM. IN REGIO-BAVARICA UNIVERSITATE WIRCEBURGENSI PROF. PUBL. ORD., CAES. ACAD. NAT. CURIOS. ERLANG. SODAL.
CUM TABULA AENEA PICTA.
NORIMBERGAE.
SUMPTIBUS CAROLI FELSSECKER.
MDCCCXVI.


## Wild Roses

One of the motivations for Kraus to accept the appointment in 1898 at the University of Würzburg was his desire to work on the impact of natural local conditions on the native flora. However, Kraus was not interested in the local problem itself, but in the underlying elements of general importance (Kniep 1916). Kraus, a profound botanist with good floristic knowledge was soon drawn on wild roses. At his time much more wild roses than today were present at the Würzburg area at all places Kraus worked with. Namely the limestone flora catched the eye of Kraus and in his book (Kraus 1911) he already presented detailed observations on flowering period and occurrence of wild roses and he admitted, that the flora of Ambrosius Rau (1784–1830) was his guide for determination of the species (Kniep 1917). In his last years, he had started to work on the *Enumeratio Rosarum circa Wirceburgum et pagos adjacentes sponte crescentium* (see fig 1-3) by Rau (1816) (Kraus 1910). As Kniep (1917) explained, Kraus aimed at a publication in five parts to honour Raus 100th anniversary of the publication and his

own 50th doctorate anniversary. When Kraus died in 1915, two parts were finished, a third part not complete. Two parts were planned, but not written, a part on "*Rosa aciphylla* Rau and relatives" and a part on "wild roses in culture" meaning the phenotypic plasticity expressed in culture experiments as later edited from the remained notices of Kraus by Kniep (1917). The two parts finished were about the history of roses around Würzburg prior to Rau's *Enumeratio*, and an analysis of herbarium specimen at the Wuerzburg herbarium believed to be original specimen from Ambrosius Rau. The part, which was present but unfinished concerns the occurrence and distribution of rose species at different localities. Kraus became a quite profound expert on wildroses, he mostly collected, herbarised and determined the species by himself and afterwards let them revise by Ernst Sagorski (1847–1929), the eminent authority of thuringian roses and well acquainted with morphotypes from the limestone slopes of the Saale with similar growth conditions as in Würzburg. All these parts by Kraus are sound and valuable studies on the rose flora of Würzburg. However, the most interesting part of his "rose-pentalogy" remained unwritten, the part on *Rosa aciphylla* Rau.


PRAENOBILI
PERILLUSTRI, PLURIMUM REVERENDO ET DOCTISSIMO
DOMINO D. DOCTORI
NEES AB ESENBECK
DE HISTORIA NATURALI OPTIME MERITO,
AMICO SUO DILECTISSIMO.

*Rosa aciphylla* was originally detected by Georg Heller, a court physician at Amorbach (not to be mixed up with his elder brother Franz Xaver Heller who published a Flora of Würzburg (Kniep 1917). Rau (1816: 70) acknowledged the initial finding by Heller and collected a number of samples with him at the type locality Hexenbruch. Rau wrotes: "*D. Georgius Heller, medicinae sudiosus* (sic!), *de flora wirceburgensi optime meritus hanc rosam primus invenit in colle saxoso Hexenbruch dicto, et ipse college cum eo plura specimina*" (Rau 1816: 70). In his *Enumeratio* Rau added only one illustration, *R. aciphylla* (fig. 4). In the *Conspectus specierum descriptarum* (Rau 1816: 35/36) the morphological differences are presented. *R. aciphylla* (*Tubo calycis globoso*) is separated from *R. canina* (*Tubo calycis oviformi*) only by the form of the hips, a character today known as being not useful for species delimitation. Interesting, Rau did not value the structure of the appendages on the sepals (*Laciniis calycinis appendiculatis*) as a

guiding character although it was known as a distinctive difference between species since medieval times (Stearn 1965). Rau subdivided *R. canina* into 4 varieties, including *R. ramosissima*. Kraus (Kniep 1917:12) compared the original Rau specimen of *R. aciphylla* with a surviving specimen of *R. ramosissima* from the Rau-collection and came to the same result as Rau (1816:75) already did: "*Per hanc varietatem rosae acyphyllae* (sic!), *excepto calycis tubo oviformi, similimam, rosa canina transire videtur in rosam aciphyllam*." This indicates, that *R. aciphylla* is a morphotype of *Rosa canina*, judging from the illustration and description presumably a xerophytic form from the dry limestone slopes around Würzburg, a good illustration of this xeromorphic form is given bei Redouté (1817-1824: 125) although it might be that Redouté did not work from a living plant (de la Roche & Rowley 1978). In the Monograph by Lindley (1830) *R. aciphylla* is recognized as a *R. canina* smaller in all parts and judged as "an appearance which is by no means uncommon in this country" (Lindley 1830: 102). Kraus already concluded that the taxonomic relevance of specific morphotypes needed to be validated by culture experiments (Kniep 1917: 19). Later rhodologists already summarized *R. aciphylla* into *R. canina* (e.g. Wallroth 1828, Keller 1931: 461), however, the experimental evidence for it is still lacking.

## Vineyards, viticulture, grapes, and wine

Before the disciplines oenology (science of winemaking) and ampelology (science of growing grapes) were well-established and could obtain chairs at some special universities or technical universities (France, Germany, Hungary), botanists contributed much to viticulture. Kraus was one of these pioneers. Maybe, the interest was raised and further developed when he met his colleague Alexis Millardet (1838–1902) in De Bary's lab in another centre of viticulture, in Freiburg im Breisgau. Both studied how to improve growth conditions, especially of the introduced grapes from North America, Kraus did it along with other studies, Millardet contributed much to pest control. Kraus' bought his special research ground in an area downstream of Würzburg. It consisted partly of vineyards, partly of natural vegetation; eventually it became a famous nature reserve (for details see Schönmann in Mollenhauer 2005).

Würzburg is the capital of Lower Franconia and a famous centre of viticulture. Franconian wine is for the connoisseur. In current German, "altfränkisch" or "fränkisch" are not just terms to design something highly apreciated, rather they refer to narrow-mindedness and retardation. However, the "Franconian" way of life also brings people together, a matter much facilitated by wine. Kraus was well familiar with this trait of character of the citizen of his home area. Though he was well acclimatized in Halle (Saale), he nevertheless decided to accept the botany chair of his former master after Sachs' death. To close, an anecdote might be added to show a very special case of uninterrupted tradition. Hans Burgefff (1883–

1976) who was the next but one follower on Kraus' chair lost all his private property on March 16, 1945, when Würzburg was destroyed by allied bombers. The Botanical Institute, too, was seriously damaged. So Burgeff had to look for provisional accommodation, both for his family and his work. He found this in a small vineyard cottage near Gambach, just where Kraus had studied microclimate. First provisional research work was done in a pigpen nearby.

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# Zwei lebensgeschichtliche Dokumente von Peter Kornmann (1907 – 1993)

*Reimer Kornmann*

**Abstract.** Peter Kornmann (1907-1993) was one of the most outstanding and influential german phycologists of the 20th century. Here two autobiographical scetches are presented for the first time, which were found in the bequest of his wife Hilde Kornmann.

## Einleitung

Bei der Sichtung des Nachlasses meiner Eltern – meine auf Helgoland lebende Mutter Hilde Kornmann war im Jahre 2006 plötzlich verstorben – stieß ich, für mich völlig überraschend, auf zwei Manuskripte meines Vaters: ein längeres handgeschriebenes mit dem Titel „Der ungewöhnliche Lebensweg eines Botanikers“ und ein kürzeres, maschinengeschriebenes, überschrieben mit „Autobiografische Notizen“ und mit Datum vom 11. 4. 1992. Letzteres war offensichtlich schon druckfertig – für Eingeweihte gut an den mit Tippex angebrachten Korrekturen zu erkennen, die der langjährige Mitarbeiter meines Vaters, Herr Paul-Heinz Sahling (1911 – 2002) stets im letzten Stadium der Manuskripterstellung vornahm. Wahrscheinlich hat mein Vater diesen Text deswegen nicht mehr zu einer Veröffentlichung eingereicht, weil er zur gleichen Zeit noch an weiteren Manuskripten arbeitete, deren schnelle Publikation ihm sicherlich wichtiger erschien (z.B. Kornmann 1993, 1994, Kornmann & Sahling 1994) Der handschriftlich hinterlassene Text ist nach 1987 entstanden, da mein

Vater im letzten Satz das internationale Forschungskolloquium erwähnt, das ihm zu Ehren anlässlich seines 80. Geburtstages an der Universität Hamburg veranstaltet wurde. Auch dieses Manuskript befand sich schon in einem recht fortgeschrittenen Arbeitsstadium, weil es keine Korrekturen mehr enthielt. Es fehlten lediglich die Inhalte von Klammern für entsprechende Verweise. Während die kurzen „ autobiografischen Notizen“ eine Rückschau allein auf wesentliche Aspekte des wissenschaftlichen Werks liefern, enthält der längere Text einen Abriss der Lebensgeschichte mit den wichtigsten Stationen. Sie machen den biografischen Hintergrund mit all seinen Zufällen und subjektiven Einflüssen verständlich, aus dem sich die Fragestellungen und Methoden für die Forschungsarbeit ergaben. Ich bin mir ziemlich sicher, dass mein Vater an eine Publikation auch dieses Textes in der vorliegenden Form gedacht hat, und so bin ich sehr dankbar für die gebotene Möglichkeit, ihn zusammen mit den „autobiografischen Notizen“ zu veröffentlichen. Irgendwelche Veränderungen an dem Text selbst schienen mir nicht notwendig zu sein. Die alte Rechtschreibung habe ich belassen. Die Inhalte der offenen gebliebenen Klammern beziehen sich mit einer Ausnahme auf frühe Arbeiten meines Vaters. Die Ausnahme betrifft die Klammer „ (vgl. Abschnitt ...)“ – damit ist sicherlich gemeint: „(vgl. den nächsten Abschnitt)“. Die übrigen Quellen lassen sich aus der Bibliografie meines Vaters (abrufbar unter www.ph-heidelberg.de/wp/kornmann.de) erschließen, und sie sind auch in der Biologischen Anstalt Helgoland zugänglich. Dort finden sich zu allen beschriebenen Lebensphasen recht eindrucksvolle und anschauliche Dokumente, die sich auch für eine Ausstellung und fachhistorische Auswertung eignen dürften.

## Peter Kornmann: Autobiographische Notizen (11. 4. 1992)

Ich darf mich wohl des zweifelhaften Ruhms erfreuen, das schlimmste „Enfant terrible“ der Algologie gewesen zu sein. Der Staub, den die Entdeckung des Lebenszyklus von Derbesia/Halicystis (1938) auf mir abgelagert hatte, wurde erst durch Feldmann (1950) abgewischt.

Ebenso wenig Glück war mir beschieden, als ich an den Grundfesten des so stabil angesehenen Chlorophyceen-Systems zu rütteln begann (Die Ulotrichales, neu geordnet auf der Grundlage entwicklungsgeschichtlicher Befunde, Phycologia 3, 1963). Ein weiteres, 1964 eingereichtes Manuskript wurde trotz erheblicher Bedenken für Phycologia 4 (1965) angenommen („Ontogenie und Lebenszyklus der Ulotrichales in phylogenetischer Sicht“). In seiner Beurteilung des eingereichten Manuskripts schrieb ein Gutachter: „Der Name Kornmann nimmt eine führende Stellung ein unter denen, die die Beweise lieferten, dass man die Lebenszyklen systematisch nur sehr begrenzt ernst zu nehmen darf. Um so mehr erscheint die vorliegende Arbeit daher als Schritt rückwärts in die Vergangenheit.“ Diese Prognose sollte sich als falsch erweisen.

Auf dem eingeschlagegenen Weg weitergehend konnte ich 1971 meine Vorstellung über die Codiolophyceae in Hamburg anlässlich der Verleihung der Ehrendoktorwürde vortragen, 1972 bei einer Tagung auf Helgoland (Codiolophyceae, a new class of Clorophyta, 1973). Das einer Intuition entsprungene Konzept wurde inzwischen durch elektonenmikroskopische und molekularbiologische Befunde untermauert und 1987 in einen erschöpfenden Festvortrag von Van den Hoek et al. einbezogen (Helgol. Meeresunters. 42, 339-383, 1988). Ergebnis: „This traditional hypotheses of chlorophytan evolution should now be abandoned. Dr. Kornmann`s idea that divergent organizational levels have been realized within one chlorophytan group is correct" (p. 375).

## Peter Kornmann: Der ungewöhnliche Lebensweg eines Botanikers

Kürzlich las ich die Autobiographie eines Botanikers, die dieser anlässlich einer akademischen Ehrung vortrug. Sein Lebensweg führte ohne besondere Hindernisse in stetigem Aufstieg auf die oberste Ebene beruflicher Erfüllung.

Die Lektüre regte mich an, meinen eigenen, so völlig anderen Werdegang darzustellen. Unsere angestrebten Ziele waren ganz verschieden, und auch meine Lebensarbeit blieb nicht ohne Anerkennung.

### *Kindheit und Schulzeit*

Mein oberhessischer Vater und meine badische Mutter – beide bäuerlicher Herkunft – gründeten ihren Hausstand 1905 in Frankfurt a. M. Am 23. Okt. 1907 wurde ich, Peter, geboren. Ich gedieh problemlos und entwickelte mich zu einem wahren Wonnepfropfen. 1914 wurde ich in die Mittelschule eingeschult; ich sollte ja einmal einen besseren Start haben als die Eltern. Mein Vater erhielt am 1. Mobilmachungstag den Stellungsbefehl zu seinem Truppenteil, der Infanterie. In die Kriegsjahre fiel meine erste Betätigung in „angewandter" Botanik: als kräftiger Junge nutzte ich eine kleine Fläche auf einem liegengebliebenen Bauplatz als Garten und konnte so zur Versorgung der Familie mit Gemüse beitragen. In allen Lebensabschnitten, in denen es möglich war, habe ich einen Garten bearbeitet.

Als mein Vater 1918 aus dem Feld zurückkehrte, war auch die Zeit gekommen, einen ordentlichen Garten in einem Kleingartenverein zu bewirtschaften. Gute Ernten an Gemüse und köstlichem Obst waren der Lohn für die in den ersten Jahren recht harte Arbeit.

Das Schuljahr 1920 begann mit einem unerwarteten Ereignis: der Rektor holte mich mitten aus dem Unterricht heraus, und ich ging mit meiner Mutter zur Liebig-Oberrealschule. Dort war eine Aufnahmeprüfung bereits im Gang. Vom nächsten Tage an war ich Untertertianer.

Die Schulzeit war hart, es musste gewaltig gearbeitet werden.1926 erhielt ich das Reifezeugnis, um Naturwissenschaften zu studieren.

### *Studienjahre 1926 – 1932*

Als Studienfächer wählte ich Biologie, Chemie und Physik mit der Absicht, einmal Studienrat zu werden. Schwerpunkt war aber zunächst die Biologie. Keine Wochenende wurde ausgelassen, um abwechselnd an botanischen oder zoologischen Exkursionen teilzunehmen. Den Zugang zur wissenschaftlichen Arbeit vermittelte mir eine ganz zufällige Begegnung. Ohne besonderen Anlaß machte ich eines Abends einen Spaziergang und traf in der Scharnhorststraße Prof. Laibach. Er blieb stehen, und in einem Gespräch fragte er mich, ob ich ihm bei seiner Arbeit mit dem Interferometer technische Hilfe leisten wolle. Ich nahm dieses Angebot gerne an, zumal es mir auch eine kleine Vergütung einbrachte. Am wichtigsten aber war der Zugang zu der Arbeit im Institut und die Kontakte zu anderen Institutionen und Persönlichkeiten wie R. E. Liesegang und dem Institut für Physikalische Grundlagen der Medizin. Wie von selbst ergaben sich aus der Tätigkeit am Interferometer auch die Themen für die Dissertation ( ) und eine praktische Arbeit für das Staatsexamen in Chemie ( ). Wie eine Vorgabe auf die Zukunft sollte sich eine Reise an die Zoologische Station in Neapel auswirken: Prof. Laibach hatte dort 1930 Permeabilitätsstudien an Valonia begonnen und konnte sie wegen des Semesterbeginns nicht abschließen. Ich durfte daher zwei Monate in Neapel verbringen, meinen 23. Geburtstag auf der Heimreise in Florenz erleben! Die erste Begegnung mit Meeresalgen und der Mittelmeer-Vegetation. Am Ende des Sommersemesters 1931 wurde ich mit dem Prädikat magna cum laude promoviert und konnte am 1. 4. 1932 eine Stelle als Hilfsassistent übernehmen. Mehr meiner wissenschaftlichen Arbeit als der Vorbereitung auf das Staatsexamen zugetan, legte ich 1932 diese Prüfung mit einem kümmerlichen Ergebnis ab.

### *Assistent in Frankfurt 1932 – 1936*

Zunächst mit der Wahrnehmung einer planmäßigen Stelle beauftragt wurde diese mir am 1. 5. 1934 zugeteilt. Die anderen Stellen hatten der Priv. Doz. Dr. Overbeck und Franz Firbas inne, mit dem mich bis zu seinem frühen Tode ein freundschaftliches Verhältnis verband. Die Episode des Interferometers war zu Ende gegangen, die Wuchsstofforschung war zu einer neuen Arbeitsrichtung von Prof. Laibach und seiner zahlreichen Schüler geworden. Mich aber faszinierten die Möglichkeiten, die mir die Riesenzellen von Valonia und ihre Verwendung als lebende Osmometer zu bieten schienen. Die Vorstellungen ließen sich realisieren: einem kürzeren Aufenthalt in Neapel im Herbst 1933 konnte ein längerer 1934 folgen. Das Ergebnis dieser Arbeiten, bei denen mir Hilde Koch (vgl. Abschnitt ...) technische Hilfe leistete, ist in zwei Arbeiten niedergelegt ( , ). Schien so auch der Weg in die wissenschaftliche Laufbahn vorgezeichnet, so stand dem doch ein Hindernis entgegen. Immer nur in Frankfurt studiert, promoviert und als Assistent wäre eine Habilitation kaum möglich gewesen. Auch hier schien sich ein Ausweg zu öffnen: in Helgoland war durch den Wegzug von Prof. Schreiber die Stelle des

Botanikers unbesetzt und es wurde ein auf ein Jahr befristeter Austausch meiner weiter von Frankfurt getragenen plm. Assistentenstelle gegen die Dienstleistung von Prof. Schreiber vereinbart. Meinem Umzug nach Helgoland am 1. April 1936 war die Verlobung mit Frl. Hilde Koch vorausgegangen.

### *Helgoland 1936 – 1939, Lebensbund*

Mein Plan, Wuchsstoffuntersuchungen an Meeresalgen fortzusetzen, lag nicht im Sinne des Direktors, Prof. Dr. Hagmeier. Vielmehr wünschte er, dass ich mich mit entwicklungsgeschichtlichen Studien, besonders an Kleinalgen, befassen sollte. Der Sommer 1936 war eine sorglos fröhliche Zeit im Kreise der jungen Kollegen und Gastforscher, zu denen sich noch meine Verlobte und der Student Hans Reimer Kuckuck gesellten. Dieser ordnete und inventarisierte den wissenschaftlichen Nachlass seines Vaters. Im Einvernehmen mit dem Direktor wurden im Laufe der nächsten drei Jahre die Ectocarpaceen für eine Veröffentlichung vorbereitet. Die Arbeit war bei Kriegsausbruch noch nicht fertig. Trotz Verlust der Originalzeichnungen und eines Manuskripts konnten die „Ectocarpaceen-Studien“ aus erhalten gebliebenen Resten in einer Serie von acht Teilen in der Zeit in der Zeit von 1954 – 1964 erscheinen. Die eingehende Beschäftigung mit dieser Familie war mir die Grundlage für eine Reihe von späteren Untersuchungen.

Die Arbeit in Helgoland sagte mir zu. Wichtiger war, dass sie auch dem Direktor gefiel. Sein Angebot, die Kustodenstelle zu übernehmen, nahm ich daher gerne an, zumal ich auf diese Weise von den politischen und sportlichen Schulungslagern für angehende Habilitanden verschont blieb. Mit der in Aussicht stehenden Beamtenstelle war die Grundlage für unseren Lebensbund gegeben: wir heirateten am 25. 3. 1937 in Frankfurt a. M. Einer der Höhepunkte des Jahres 1937 war der letzte der in zweijähriger Folge von Friedrich Oltmanns auf Helgoland abgehaltenen Algenkurse, dem ich dabei assistierte. Den Abschluß bildete wie stets zuvor eine längere Exkursion zu den nordfriesischen Inseln und das Wattenmeer, an der auch meine Frau teilnahm. Inzwischen hatte ich mich in die Kultur von Meeresalgen eingearbeitet und auch die siphonale Grünalge Derbesia marina einbezogen. Im Winter bis Frühjahr 1938 lag ein völlig unerwartetes Ereignis vor: der heteromorphe Generationswechsel von Halicystis ovalis war entdeckt! Mit dieser aufsehenerregenden Arbeit, die in Fachkreisen noch bis in die 50er Jahre umstritten war, stand meiner Ernennung zum Kustos am 1. 7. 1938 nichts mehr im Wege. Der Abteilung Botanik gehörte seit 1937 auch Dr. Kurt Beth an, der sich im Auftrag der Marine mit Anwuchsuntersuchungen beschäftigte.

Der Ausbau der Festung Helgoland machte eine unmittelbar verfügbare Befehlsübermittlung mit den einzelnen Kommandostellen notwendig. Dazu griff man auf ständige Einwohner zurück, die zunächst noch als Zivilisten eingearbeitet wurden. Um daraus richtige Soldaten zu machen, fehlte noch die militärische Grundausbildung. Sie erfolgte im Frühjahr 1939 in einem 4Wochen Lehrgang in der Kaserne am Mühlenweg in Wilhelmshaven.

Der erste Tag in der Kaserne wäre beinahe zu dem letzten meines Lebens geworden. Nur wenige Sekunden haben über Sein oder Nichtsein entschieden. Die eisernen Kojen in unserer Stube waren dreistöckig übereinander aufgebaut. Die Vorgänger hatten sich mit einem derben Scherz verabschieden wollen und die oberste Koje am Kopfende meines Bettenturms ausgehängt. Ich war schon dabei, in meine, die mittlere Koje, zu steigen, als Arthur Krüß von der Vogelwarte (Sperling!) in seine oberste Koje kletterte, die sofort herunterstürzte. Was hier abgewendet worden war, mag ein jeder nach Belieben deuten: als Zufall oder als eine höhere Fügung. Am 15. 6. 1939 wurde unser Sohn, Gerhard Reimer, auf Helgoland geboren, zehn Wochen vor Ausbruch des Kriegs.

### *Die Kriegsjahre*

Schon am 24. 8. 1939 erfolgte meine Einberufung als Marine-Artillerist zum Wehrdienst. Zweimal habe ich in die Automatik des Geschehens eingegriffen und dadurch zweifellos die Richtung im Laufe meines Lebens mitbestimmt. Gegen Weihnachten 1939 bat ich den Kompaniechef, von meiner Meldung zum Unteroffizierlehrgang abzusehen, die Voraussetzung für eine weitere Ausbildung zum Reserveoffizier war. Der Krieg zog sich in die Länge, und auf die Dauer hätte ich meine Stellung als Obergefreiter nicht halten können. Als der Marinewetterdienst Anwärter für die Ausbildung im technischen Dienst suchte, bat ich den Kompaniechef, mich dafür melden zu dürfen. Er gab meiner Bitte statt, allerdings nicht ohne die Bemerkung, dass ich mich ihm früher verweigert hatte. Am 11. 8. 1942 wurde ich wurde ich zur Ausbildungswetterwarte nach Gotenhafen abkommandiert. Als deren Leiter von meinem beruflichen Werdegang Kenntnis hatte, wurde ich nicht einem Techniker-Lehrgang zugeteilt, sondern konnte an einem Kursus für angehende Marine-Metereologen teilnehmen. Einige Tage später war ich in Gesellschaft von Prof. Albert Kolb (Geographie), Prof. Hermann Meusel, Halle, und dem Geographen und Arktisforscher Franz Messer, Wien. Der letztere und ich schlossen den Kursus als Hilfsregierungsräte ab. Nach dem Abschluß der wissenschaftlichen Ausbildung in Hamburg wurde ich im Marineobservatorium in Greifswald mit der Durchführung meteorologisch-wissenschaftlicher Aufgaben betraut. Am 5.6.1945 wurde ich als Reg. Rat der Reserve aus dem Dienst des Marineobservatoriums entlassen und in ein britisches Auffanglager in Marsch gesetzt.

### *Guxhagen, 1945 – 1950*

Am 9. 8. 1945 wurde ich in Meldorf/Holstein aus der Wehrmacht entlassen. Ein Flüchtling – Helgoland war zerstört. Meine Familie lebte in Dankerode, Kr. Rotenburg/Fulda. Dort hatte meine Frau in den letzten Kriegsjahren die Bewirtschaftung des Hofgut-Gartens und einen 14 Morgen großen Feldgemüsebau übernommen. Dadurch war sie nicht nur mit der Landwirtschaftskammer Bebra, sondern auch dem Samenhändler W. Rohde in Beziehung gekommen, dem ihre gediegene Sachkenntnis – sie wollte ursprünglich Gartenarchitektin werden, musste aber eine Gärtnerlehre in Dresden 1926 krankheitshalber abbrechen – nicht verborgen geblieben. Der nächste Abschnitt des Lebenswegs war sozusagen vorgezeichnet. Herr Rohde erkannte die Notwendigkeit, die Saatguterzeugung durch eigenen Anbau im hessischen Raum sicherzustellen. Dadurch wurde ein Zuchtgarten notwendig, der im Herbst 1945 nur aus einem leeren Acker in Guxhagen bestand. Arbeiter und technisches Personal wurden eingestellt und auch meine Frau und ich als wissenschaftlicher Leiter des Unternehmens in die Firma übernommen. Wegen meiner Zugehörigkeit zur Partei und ihrer Organisationen durfte ich bis zu meiner Einstufung als Mitläufer nur in gewöhnlicher Arbeit beschäftigt werden. Im Dezember 1945 zogen wir nach Guxhagen um. Ein großes Zimmer in einem Gutshaus war fünf Jahre unsere Bleibe. Es waren harte Jahre für die Familie, aber wir hatten unser Auskommen und die Arbeit sagte mir zu. Als Prof. Hagmeier die Möglichkeit einer Einstellung in den Dienst der Biologischen Anstalt in Aussicht stellte, traf ich eine folgenschwere Entscheidung. Nach einem Vergleich der äußerst dürftigen Lebensverhältnisse in List/Sylt und meiner Stellung in Guxhagen gab ich meine Beamtenstelle preis, ein Entschluß, den ich noch sehr bereuen sollte.

Infolge der Währungsreform geriet die Firma Rohde in wirtschaftliche Schwierigkeiten und mußte auch leitende Mitarbeiter entlassen. Am 30. 9. 1949 schied ich aus und erhielt Arbeitslosenunterstützung. Die Ahnenforschung Kornmann und Koch (meiner Frau väterlicherseits) auszubauen, war mir ein angenehmer Zeitvertreib. Erst am 1. 3. 1950 konnte ich eine Arbeit im Landesfürsorgeheim Fuldatal finden und eine Stelle als Gärtner übernehmen. Mit Befürwortung der Landwirtschaftskammer wurde ich als Meister entlohnt. Noch einmal war mir die Möglichkeit geboten„ meinen Lebensweg zu ändern: ich hatte mich in die inzwischen ausgeschriebene Botanikerstelle an der Biologischen Anstalt beworben und wartete sehnlichst auf eine Entscheidung. Wieder stand ich am Scheideweg: ich hätte auch wieder in die Firma Rohde eintreten können. Herr Rohde drängte, und in dieser verzwickten Lage wandte ich mich unter Umgehung jeglichen Dienstweges unmittelbar an das Ministerium. Die umgehende telegraphische Antwort lautete: „Einstellung verfügt, wünsche erfolgreiche Arbeit.“

### *List/Sylt, 1950 – Helgoland 1959*

Am 15. Juni 1950 übernahm ich meine frühere Stelle eines Kustos und Professor als wissenschaftlicher Angestellter. 42 Jahre war ich alt, als ich mit meiner Lebensarbeit beginnen konnte. Im Januar 1951 konnte die Familie erstmals in einer eigenen Wohnung, einem Reihenhaus, vereinigt sein. Es war eine glückliche Zeit in List/Sylt, schließt man eine schwere Erkrankung und Operation aus. Im Mai 1959 wurde die Biologische Anstalt auf Helgoland wieder eröffnet und damit die Rückkehr auf die Insel vollzogen. In rascher Folge konnte eine ansehnliche Zahl von Arbeiten veröffentlicht werden. 1971 wurde mir die Würde eines Ehrendoktors der Universität Hamburg verliehen. Das Ausscheiden aus dem aktiven Dienst als Abteilungsleiter für Meeresbotanik im Jahre 1972 brachte keine Unterbrechung meiner Forschungsarbeit mit sich. Mein 80. Geburtstag wurde in Hamburg mit einem internationalen Forschungskolloquium feierlich begangen.

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Contents

Kärin Nickelsen:
From Leaves to Molecules: Botany and the Development of Photosynthesis Research ....................................................................................................1

Rudolf Hagemann:
The reception of the Schimper-Mereschkowsky endosymbiont hypothesis on the origin of plastids – between 1883 and 1960 – many negative, but a few relevant positive reactions .............................................................................. 41

Vítězslav Orel:
Science studies and nature of Mendel´s paradigm ............................................... 61

Manfred D. Laubichler, Jane Maienschein, Grant Yamashita:
The Embryo Project and the Emergence of a Digital Infrastructure for History and Philosophy of Science ........................................................................... 79

Luminita Göbbel, Rüdiger Schultka, Lennart Olsson:
Collecting and dissecting nature: Meckel's Zootomical Museum at the University of Halle, Germany ................................................................... 97

Brigitte Wirth: Maria Sibylla Merian, Baltasar Scheid und Richard Bradley.
Die Künstlerin und Naturforscherin, ein Kaufmann und ein Botaniker............... 115

Dieter Mollenhauer, Burkhard Büdel, Ekkehard Höxtermann,
Volker Wissemann:
Gregor Kraus (1841–1915) – versatile botanist from Lower Franconia.............. 155

Reimer Kornmann:
Zwei lebensgeschichtliche Dokumente von Peter Kornmann (1907 – 1993).......173

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The name DGGTB (Deutsche Gesellschaft für Geschichte und Theorie der Biologie; German Society for the History and Philosophy of Biology) reflects recent history as well as German tradition. The Society is a relatively late addition to a series of German societies of science and medicine that began with the „Deutsche Gesellschaft für Geschichte der Medizin und der Naturwissenschaften", founded in 1910 by Leipzig University's Karl Sudhoff (1853–1938), who wrote: „We want to establish a ‚German' society in order to gather German-speaking historians together in our special disciplines so that they form the core of an international society…". Yet Sudhoff, at this time of burgeoning academic internationalism, was „quite willing" to accommodate the wishes of a number of founding members and „drop the word German in the title of the Society and have it merge with an international society". The founding and naming of the Society at that time derived from a specific set of historical circumstances, and the same was true some 80 years later when in 1991, in the wake of German reunification, the „Deutsche Gesellschaft für Geschichte und Theorie der Biologie" was founded. From the start, the Society has been committed to bringing studies in the history and philosophy of biology to a wide audience, using for this purpose its Jahrbuch für Geschichte und Theorie der Biologie. Parallel to the Jahrbuch, the Verhandlungen zur Geschichte und Theorie der Biologie has become the by now traditional medium for the publication of papers delivered at the Society's annual meetings. In 2005 the Jahrbuch was renamed Annals of the History and Philosophy of Biology, reflecting the Society's internationalist aspirations in addressing comparative biology as a subject of historical and philosophical studies.

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