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Editors

Elusive Phenomena, Unwieldy Things

Historical Perspectives on Experimental Control

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Controlling Nature in the Lab and Beyond: Methodological Predicaments in Nineteenth-Century Botany

Kärin Nickelsen

7.1 Introduction

In January 1872, the plant physiologist Julius Sachs gave a speech to the members of his university on "The State of Botany in Germany" (Sachs 1872). Sachs recounted how botany had undergone a radical transformation over the previous 40 years. The botanists of the 1870s, Sachs explained, were no longer obsessed with collecting plants and arranging them in systems, and they had liberated themselves from the harmful influence of the German *Naturphilosophie*. These days, botanists investigated the processes of cellular reproduction, the laws of growth and development, the effects of gravity and light, and the influence of climate on the distribution of plant species. Botany had become a scientific discipline—although, Sachs added, it could unfold its full potential only if there were more positions for academic botanists in Germany.

Sachs's speech clearly had a political agenda, and one should not take his rhetoric of awakening at face value. But the methodological shift that Sachs described was real. Botany changed dramatically in the nineteenth century, particularly in German-speaking countries. The rise of new microscopy techniques and other precision instruments opened new perspectives and prompted botanists to revisit almost

I am very grateful to all the organizers of the two workshops that gave rise to this volume. This goes especially to Jutta Schickore for insightful comments on earlier drafts of this paper, and for our productive and enjoyable conversations on the history of scientific methods and methodology over the past years. Comments by others were immensely helpful too, with special thanks to Klodian Coco, Claudia Cristalli, and Caterina Schürch. I also would like to thank Elizabeth Hughes, Berlin, and Louise R. Chapman, who carefully edited this essay.

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every area of the field, including the study of forms and functions, or morphology and physiology, where the influence of *Naturphilosophie* had been particularly strong (e.g. Mendelssohn 1964; Coleman 1971; Jahn 2000; Bowler and Morus 2005; Morange 2016).

An important part of this reform program was the development of new methodological principles for a "scientific botany," and this development is the focus of this essay. The new botany would no longer base itself in lofty imagination but in empirical facts, supporters of the agenda agreed. Hardly anybody used the term "control," or its equivalents in other languages, in this context (see the introduction to this volume). However, as this essay shows, many botanists were deeply concerned with making observations accurate, experimental design meaningful, inferences safe, speculations respectable, and interpretations reliable. In terms of this volume's categories, the chapter addresses general "methodological ideas," as well as "control strategies" and implemented "control practices."

Specifically, I shall examine the work of Julius Wiesner (1838–1916), acclaimed plant physiologist and protagonist of the new botany. Prior to his professorship in Vienna, Wiesner completed his doctoral dissertation in Jena with Matthias J. Schleiden (1804–1881), who was already famous for his *Principles of Scientific Botany* (first published in 1842). In this textbook, Schleiden called for a new beginning of botany as an "inductive science," with a set of rigorous control and validation strategies at its core. For many botanists, including Wiesner, this textbook became an important source of inspiration. I shall therefore begin this chapter by looking briefly at how methodology in general, and how different forms of "control" in particular, were discussed in Schleiden's *Principles.* I shall then note how this agenda unfolded in Wiesner's work, especially in his studies on the influence of light on plants. This influence occurred first in his botanical laboratory and then in the field, where he had to adapt his concepts and practices to entirely new conditions. The questions emerging from these studies led him far beyond plant physiology in its narrow sense, and demanded a different set of methodological principles and control strategies—so different, in fact, that Wiesner eventually helped to found a new sub-discipline: the "Biology of Plants."

7.2 Matthias J. Schleiden and *The Principles of Scientific Botany*

Matthias Jacob Schleiden (1804–1881) began his career as a lawyer but in 1835, after a personal crisis, dropped this profession and switched to botany (e.g., Möbius 1904; Jahn and Schmidt 2005). Four years later, Schleiden received a second PhD in this field, and in 1840 he became Associate Professor in Botany and Director of the Botanical Garden at the University of Jena.[1] In 1842, Schleiden published the first edition of a widely read (and celebrated) textbook, *Grundzüge der*

[1] In 1863, Schleiden became professor of botany at Dorpat (today's Tartu), which then was part of Russia; in 1864, he withdrew from this position and moved to Dresden as an independent scholar.

wissenschaftlichen Botanik nebst einer methodologischen Einleitung als Anleitung zum Studium der Pflanze ("Principles of scientific botany, with a methodological introduction as a guide to the study of plants"), which is the focus of the following sections.[2] Second and third revised editions were published in 1845 and 1849, the latter of which was reprinted in unaltered form in 1861.[3]

In this textbook, Schleiden introduced his groundbreaking theory of how plant cells developed and how they formed tissues and structures (first published as Schleiden 1838). In addition, he also addressed fundamental methodological questions of the field (e.g. Buchdahl 1973; Charpa 2003, 2010). Botany was in a deplorable state, Schleiden thought, especially in the German countries. Under the influence of Hegel, Schelling, and others, the field had degenerated into a "dogmatic science," Schleiden lamented, and "a widespread lack of orientation [prevailed] about the challenges to the human ability to gain knowledge, and about the means to meet them" (Schleiden 1861, 12).[4] For Schleiden, botany was urgently in need of a sound methodological and epistemological foundation, and this became the focus of his substantial, 100-page-plus introduction. He added methodological comments throughout the rest of the book.[5] The aim was to transform botany into an empirically based "inductive science," and Schleiden's textbook was to serve as an important step in this direction.[6] All of this is highly relevant for the question how the

[2] See Schleiden (1842, 1843) for the two volumes of the first edition. A second, revised edition was published as Schleiden (1845); this was again reworked for a third edition (Schleiden 1849a). The last of these was reprinted unaltered (albeit with a new preface) as Schleiden (1861). The English translation (Schleiden 1849b) was based on the 1845 edition. (This translation is not always reliable; if not stated otherwise, I am using my own translation for quotations from the German original.) In the English version of the text, the methodological introduction was omitted with a two-page "summary" in its place, while some of Schleiden's introduction to scientific microscopy was added as an appendix to the volume. The "Translator's Preface" explained that the methodological introduction was considered too long and also unnecessary, given that "two admirable works" on the principles of scientific inquiry had already been written in English by John Herschel and William Whewell.

[3] The introduction changed substantially from the first edition (1842) to the second (1845). Some of the personal attacks were dropped, while the discussion of general topics in epistemology was expanded. The subsequent third and fourth editions (1849, 1861) introduced only minor changes but they represent the most mature version of Schleiden's thoughts. In most cases, the 1861 edition is therefore used for quotations; relevant differences to earlier versions will be indicated. See, on these changes, e.g., Jost (1942), which also provides an overview of the textbook's reception.

[4] German original: "Es fehlt im Allgemeinen an einer richtigen Orientirung über die Aufgaben des menschlichen Erkenntnissvermögens und die Mittel zu ihrer Lösung." Schleiden vigorously promoted the post-Kantian philosophy of Jakob F. Fries, although in a slightly adapted version, and polemically criticized others, especially the protagonists of *Naturphilosophie*, with Hegel and Schelling as arch-villains.

[5] While the introduction grew over the years and through subsequent editions, the first version of 1842 already included 166 pages (and in smaller print format than later editions!).

[6] For a comprehensive treatment of how "induction" was understood by Schleiden (in the tradition of Fries), see, e.g., Apelt (1854). Apelt dealt primarily with the physical sciences but repeatedly cited Schleiden for topics related to biology *sensu largo*. In turn, Schleiden explicitly referred to Apelt in the 1861 edition and calls Apelt's contributions "the most important work of philosophy published in this century" (Schleiden 1861, Vorrede, VII).

notion of "control" gained a foothold in botany. The following sections, however, cannot do full justice to the program Schleiden pursued in this remarkable treatise. I restrict myself to a summary of Schleiden's comments on two areas that became so important for Wiesner and others: observation and experiment.

7.2.1 Observation

Schleiden treated the topic of observation in particular detail. This included a philosophical discussion of the faculty of "seeing," its weaknesses and its epistemological function, but also concrete recommendations of how to make observations reliable. In fact, large parts of the introduction were dedicated to the principles of scientific microscopy, where Schleiden was rightfully regarded an expert.[7] He believed that every botanist should master the techniques of microscopy, and he provided a comprehensive survey of how the microscope worked and how it should be used. But even without a microscope, Schleiden maintained, botanists ought to revisit their practices of observation, train their eyes, and refine their habits. "He who wishes to observe successfully," Schleiden lectured his readers, "must observe frequently and with the most profound attention, so that he gradually learns how to see, for *seeing* is a difficult art" (Schleiden 1861, 84).[8] Most importantly, botanists should observe the relevant phenomena themselves whenever possible, and always document them either in accurate sketches (which, according to Schleiden, helped to control and discipline one's observation) or, preferably, in durable preparations and slides that their colleagues would later be able to consult (e.g., Schleiden 1861, 85–91).[9] In cases where one had to rely on reports by others, Schleiden warned his readers to place their trust carefully.[10] There were so many pitfalls in the process, so many potential sources of error, and so many misguided minds, Schleiden thought, that one should always scrutinize another person's judgment carefully, even those made by alleged experts. He gleefully called out colleagues whose errors Schleiden found particularly outrageous.

[7] For an illuminating analysis of Schleiden's view of microscopy, see Schickore (2007).

[8] German original: "Wer mit Glück beobachten will, muss viel und mit angestrengter Aufmerksamkeit beobachten, damit er allmälig sehen lerne, denn *Sehen* ist eine schwere Kunst." Emphasis in original. The same phrase already appears in earlier versions of the introduction.

[9] Schleiden explains in detail how he thought observations should be documented in different forms and media, and control practices loom large in this context. However, as this essay focuses on control practices in experimental research, I do not discuss these interesting passages in more depth.

[10] On Schleiden's preference of "autopsia," see, e.g., Schleiden (1861, 54–55). On his warning against false authorities, and his plea to consider a person's character and scientific ethos in this context, cf. Schleiden (1861, 91–95).

Reliable observations and accurate descriptions were the foundations of everything. But Schleiden also reminded his audience that it was insufficient to observe a cellular configuration at one stage only. The risk for misinterpreting structures was high. The explanation of a plant's forms and functions had therefore to be rooted in observing the full process of cellular development, the "*Entwickelungsgeschichte*," as Schleiden called it (Schleiden 1861, 100–102).[11] This mantra was repeated many times over the course of the textbook. In fact, this "maxim of developmental history" was one of Schleiden's two guiding principles for inductive inferences in botany. (The other was the "maxim of the independence of the plant cell" which implied that cell physiology had to precede the physiology of the whole plant.)[12] Schleiden sharply criticized earlier traditions of morphology and physiology, which had violated this heuristic principle. They did so especially under the influence of *Naturphilosophie* and so had reached false conclusions. Diligent observation of the origin and development of a plant's cells and tissues were the only way to avoid such errors in the future.

In this context, Schleiden also called for a precise definition of the respective *explananda* and *observanda*. For him, conceptual clarity was as important as the accurate handling of the microscope. One of his most striking examples was the phenomenon of vegetable "growth," a central topic of investigation in nineteenth-century plant physiology. According to Schleiden, many of his colleagues failed to distinguish carefully between two processes: growth in its narrow sense, which was a division and multiplication of cells ("Zellvermehrung"), and growth in a more general sense, which was an effect of cellular elongation and the increase in cellular volume ("Zellstreckung"). These processes had similar effects but were very different in nature. Their widespread conceptual conflation, Schleiden argued, had led to flawed and nonsensical hypotheses on plant growth (Schleiden 1861, 574).[13]

Schleiden's message was clear: if botanists did not want to go amiss, they needed to control their observational practices. These included mastering the techniques of microscopy; taking into account the cells' developmental histories; documenting

[11] See also the textbook's concluding remarks (Schleiden 1861, 668): "Where should advice come from? From observing the external shapes, but not in the way it has been done up to now, without any principles and in a superficial manner. Instead, observation ought to be guided by the pursuit of morphology as a science which can only be founded on *developmental history*." German original: "Woher soll denn Rath kommen? Von der Betrachtung der äusseren Formen, aber nicht in der Weise, wie sie bisher principlos und oberflächlich getrieben, sondern von dem Erstreben einer Morphologie als Wissenschaft, deren Princip nur die Entwickelungsgeschichte sein kann." Emphasis in original. Apelt (1854, 53) also emphatically underlines this point.

[12] See Buchdahl (1873, esp. 36–39) for instructive and illuminating details. Some more "general maxims," Schleiden thought, were the same for all fields of inductive investigation; these included, e.g., parsimony, validity, unity, etc. Equally important were "specific maxims" for individual fields of study, such as botany. For Schleiden, induction and hypotheses without the guidance of these "maxims" would necessarily fail.

[13] The same point was already made in Schleiden (1842, §190, 458–59), although it was expanded and refined in subsequent editions of the textbook.

one's perception in accurate sketches and precise descriptions; double-checking the factual basis of claims made by others; and making sure that concepts and *explananda* were clearly defined. If botanists complied with these rules (or control strategies, as we may want to call them), Schleiden thought, botany might finally achieve something.

7.2.2 Experiment

Schleiden's remarks on experimental research were much shorter than his discussion of observation; in fact, the methodological introduction to the first edition of Schleiden's textbook does not even mention it. Experimentation appeared as a topic only in the second edition, that is, in 1845, and the respective passages remained unaltered in 1849 and 1861.[14] In contrast to observation, Schleiden found it difficult to formulate general guidelines for performing experiments, "because each one is modified differently according to the particular case" (Schleiden 1861, 85).[15] He also found experimentation more demanding. Anybody who was willing to receive adequate training and was ready to practice persistently could learn how to observe, but experiments required "innate talent." Probably as many as two out of three botanical experiments were inconclusive, Schleiden maintained, because their authors "did not have the gift to present questions to nature in an appropriate way, so that a clear yes-or-no response would be given" (Schleiden 1861, 85).[16] Besides talent, experimentation required comprehensive scientific and philosophical training, in order to develop one's power of judgment ("Urtheilskraft"). Only in rare cases could one rely on the felicitous instinct of a genius—Humboldt being the exception to the rule.

For Schleiden, the essence of experimentation was "placing natural bodies in a situation such that one can subject all aspects of their internal processes to measurement" (Schleiden 1861, 85).[17] Experiments, in other words, first and foremost required the design of a "controlled" setting—control in a broad sense (see the introduction to this volume)—which allowed for precise, quantitative examination. The measurements in question might entail the chemical analysis of the plant's organ but also the determination of the effects of physical forces on the plant's behavior, including temperature, light, gravity, magnetism, and electricity. To this end Schleiden distinguished two approaches:

[14] Cf. Schleiden (1845, 120–21) and Schleiden (1849a, b, 121–22).

[15] German original: "Für das Experiment dagegen lassen sich weniger allgemeine Vorschriften geben, weil jedes nach dem speciellen Fall sich verschieden modificirt."

[16] German original: "Es werden nur zu viele Experimente angestellt, die gar kein Resultat geben und geben können, weil ihre Urheber nicht die Gabe hatten, der Natur Fragen auf die zweckmässige Weise vorzulegen, so dass wirklich eine Antwort, Ja oder Nein, darauf folgen musste."

[17] German original: "Naturkörper in eine solche Lage zu versetzen, dass wir die an ihnen vorgehenden Processe in ihren einzelnen Elementen der Messung unterwerfen können."

(1) that the plant is deprived of the natural conditions needed for its growth as little as possible, and that it is made to grow in such a manner that the products of its vital processes, such as the emission of gas, the evaporation of water, and so forth, can be measured in terms of quantity and quality; (2) that one single, precisely defined condition of the plant's natural growth is excluded, or an alien condition is added, and the outcome is then compared with that state of a plant growing in natural conditions.[18]

These two types of experiment are intriguing, each in its own way. The first one (type 1) is unusual. Philosophically speaking, it might not be considered an experiment at all, because it involves as little intervention as possible, with procedures of measurement as the only exception. The botanist was to monitor the course of physiological processes under natural conditions and record carefully their manifestations, to better understand how these processes proceeded: a very valid intention, given the poor state of plant physiological knowledge at the time. The second type of experiment (type 2), in contrast, looks very familiar in its resemblance to Mill's "method of difference," where two specimens were compared, one of them as control (in a narrow sense), to test the effect of selected and possibly manipulated factors (Mill 1843).

Why Schleiden decided to include this paragraph in the introduction of 1845, as well as similar remarks in later chapters of his textbook, is not entirely clear. The timing is certainly suggestive—the passage was included only after John S. Mill's *System of Logic*, including its discussion of experimental methodology, was published in 1843. But there is no reference to Mill in Schleiden's textbook, neither in the introduction nor elsewhere, so that it is difficult to identify a specific connection. Given that the principal strategy was practiced long before Mill wrote his treatise (see, e.g., the chapters by Schürch and Coko in this volume), Schleiden's precise source of inspiration remains to be clarified elsewhere.

For Schleiden, a sufficiently "controlled" experimental setting included a thorough understanding of potential factors of influence and their reaction patterns:

These experiments can only bring us closer to our goal of understanding the phenomena of life if we at the same time subject all the individual substances and forces that might possibly affect the vital processes of plants, independently of the plant, to a careful examination and comprehensively investigate all their properties. (Schleiden 1861, 578).[19]

[18] German original: "(1) Dass man sie so wenig wie möglich den natürlichen Verhältnissen, unter denen sie wachsen, entzieht, dass man sie nur in denselben auf solche Weise wachsen lässt, dass man bestimmte Erfolge des Lebensprocesses, z.B. die Gasausscheidung, die Wasserausdünstung u.s.w. nach Quantität und Qualität dem Maass und Gewicht unterwerfen kann; (2) dass man eine einzelne genau bestimmbare Bedingung ihrer natürlichen Vegetation ausschliesst oder eine fremdartige hinzufügt, und den Erfolg dann quantitativ und qualitativ mit der unter natürlichen Bedingungen vegetirenden Pflanze vergleicht." See on this point also Schleiden (1861, 578), where it becomes clear that he really conceived of these as two separate approaches. (See, for the same remarks, Schleiden 1845, vol. 1, pp. 144–145 and Schleiden 1846, vol. 2, pp. 441.)

[19] German original: "Beide Arten von Versuchen können uns aber allein unserem Ziele, ein Verständniss der Lebenserscheinungen herbeizuführen, noch nicht näher rücken, wenn wir nicht gleichzeitig alle einzelnen, bei dem Pflanzenleben irgend in Frage kommenden Stoffe und Kräfte unabhängig von der Pflanze, für sich einer genauen Untersuchung unterworfen und in allen ihren Eigenschaften vollständig erforscht haben."

Schleiden demanded that, to assess the impact of certain substances and forces on life processes, botanists must first study these substances and forces *in vitro* and investigate their effects individually and in mutual interaction. Schleiden explicitly asked for preparatory trials to clarify reaction patterns outside the organism; only then the specificities of reactions in the living body could be identified. This was especially important if one wanted to minimize the role of so-called vital forces in the organism. Schleiden believed vital forces should be considered as explanatory factors only if it were utterly impossible to find a satisfactory physicochemical explanation. In most cases, Schleiden claimed, reference to vital forces was shorthand for "we do not know yet" (e.g. Schleiden 1861, 41–42).[20]

Plant nutrition was one such case. There were endless series of experiments about the alleged ability of plants to choose their nutrients, Schleiden lamented. "The theories based on them, the disputes about them fill a small library." All of this was pointless, he maintained, without prior investigation of reaction patterns outside the plant. If one knew the affinity of proteins, gums, sugars, and other elements of the plant toward minerals in solution, this might explain the mineral absorption of roots without any need to assume intentional action on plant's part. But experiments along these lines did not exist, and consequently almost nothing was known about the principles of plant nutrition (see Schleiden 1846, 442).[21]

Schleiden finally warned his readers that one should never jump to conclusions from only one set of experimental data, which may for arbitrary reasons not be entirely accurate. But one should also not be confused by persistent differences in the outcome. Individuals of the same species might very well behave differently and, therefore, yield different data in the same experimental set-up. These differences, however, were of no importance for the actual target of investigation, which for Schleiden was the general, characteristic, and lawful behavior of plant species.

This must suffice as a painfully brief survey of Schleiden's thoughts on the principles of observational and experimental methodology. His methodological introduction is a prime example of how control strategies and control practices (primarily in view of microscopy techniques *sensu largo*) were discussed at the time. Schleiden hardly ever used the term "control," but he had a clear concept of experiments in the sense of controlled intervention in otherwise stable settings (see on this point the

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[20] The only exception was the phenomenon of development, which Schleiden explained as the effect of the "nisus formativus" in organic matter, that is, the "instinct" of development.

[21] German original: "So z.B. sind seit *De Saussure* eine endlose Reihe von Versuchen über das Vermögen der Pflanzen, ihren Nahrungsstoff zu wählen, angestellt worden und die darauf gebauten Theorien, die darüber geführten Streitigkeiten füllen eine kleine Bibliothek. Ich dächte, wenigstens seit *Dutrochet's* Entdeckung wäre es gar leicht einzusehen, dass alles Reden darüber leer ist, so lange wir nicht untersucht haben, ob den organischen oder unorganischen in der Pflanze vorkommenden Stoffen nicht auch ausser derselben, unabhängig vom Leben der Pflanze, ein Wahlvermögen zukommt und welches, und in wiefern dieses mit dem bei der Pflanze beobachteten übereinstimmt." On the controversy around vital forces as explanatory factors for natural processes in nineteenth-century science, see also the chapter by Coko in this volume on the different explanations for Brownian motion.

introduction to this volume). He was familiar with experiments according to Mill's "method of difference" (although it is unclear whether Schleiden had actually read Mill's treatise), but also supported an alternative approach, which entailed monitoring life processes quantitatively under natural conditions. Generally, Schleiden demonstrated that he was keenly aware of the challenges inherent in experimentation with living organisms, although he did few experiments himself. Schleiden emphasized that experiments would be meaningful only if they were appropriately designed. He warned his readers that individual differences in outcome should not be overrated. He called for a deeper understanding of causal factors and their effects outside the organism before claims about their effects inside the organism. Finally, he emphasized the need for extensive scientific and philosophical training: the aspiring scientific botanist had to bring far more to the table than curiosity and good will.

In the following sections, I shall trace how this agenda unfolded in the work of one of Schleiden's former doctoral students, the plant physiologist Julius Wiesner.

7.3 Plant Physiology and Its Control Practices in the Laboratory

Julius Wiesner (1838–1916) was born in Moravia, then part of the Habsburg Empire. He spent most of his childhood in Brünn (today's Brno), but moved to Vienna for his university studies.[22] Wiesner attended classes in botany with Eduard Fenzl (1808–1879) and Franz Unger (1800–1870). But he was also attracted to the group of physicists and physiologists around Ernst Brücke (1819–1892), with whom Wiesner received his initial training in the use of precision instruments. Wiesner then moved to Jena, where he completed his PhD with Schleiden in 1860, that is, shortly before the reprint of the third edition of Schleiden's textbook. Thereafter Wiesner returned to Vienna and, in 1873, after various positions at the Technical University, he was appointed chair for "Anatomy and Physiology of Plants" at the University of Vienna. Wiesner became known for his expertise in microscopy and experimentation, for his sophisticated methods and techniques, and for his success as a discipline-builder.

A long-standing research interest of Wiesner's was the influence of light on plants' forms and functions. The question was as important as it was complex. By the 1860s, it was beyond doubt that many, if not all, characters and vital processes of plants were strongly influenced by their exposure to light. This was obviously true for photosynthesis, or "carbon assimilation," as it was called at the time, which depended upon illumination and ceased in darkness. But it was equally true for the processes of growth and development, for the shape and outer appearance of plants,

[22] On Wiesner see, e.g., Wurzbach (1888), Molisch (1916), and Wininger (1933). On Wiesner's research in old paper, see Musil-Gutsch and Nickelsen (2020).

and for their internal cellular and subcellular constitution. In many cases, light of different wavelengths and intensities seemed to prompt different effects, but the details of which kind of illumination led to which plant characteristics, and why, were obscure.

One of the issues Wiesner addressed in this context was the influence of light on chlorophyll, the green pigment of plants (e.g., Wiesner 1877). Very little was known about this substance at the time. It was known to be involved in assimilating carbon, but nobody knew exactly what the pigment did.[23] Wiesner decided that chlorophyll deserved more attention, and he started with the basics. In line with Schleiden's idea of botanical experiments, Wiesner first analyzed the elementary composition of chlorophyll and its chemical behavior *in vitro*. Only then did he begin investigating its reactions *in vivo*, that is, within the cell. One of Wiesner's specific interests was how and under which conditions chlorophyll developed in plant cells. It was known that this process depended on light: plants grown in darkness or shoots covered with earth remained pale or, botanically speaking, "etiolated." But there was no consensus on how and why these etiolated plant organs turned green upon illumination. One of the open questions for Wiesner was which part of the incident light prompted this greening process. Sunlight, the usual light source in nature, encompassed the full spectrum of rays, from very short to very long wavelengths. It therefore exposed the plant to two very different physical factors at the same time, namely, light and heat.[24] Wiesner wondered which of these were effective for the formation of chlorophyll. Was it necessary for a plant to receive light rays in the narrow sense, that is, comparatively short wavelengths, from the visible part of the spectrum? Or was it sufficient to provide plants with heat rays, which were also part of the spectrum but invisible to the human eye? (Wiesner 1877, 39).

The standard assumption at the time was based on an 1857 study by one of Wiesner's French colleagues, Claude Marie Guillemin, who claimed that heat rays were, in fact, as effective as light rays in prompting chlorophyll formation (Guillemin 1857). Guillemin had passed sunlight through a prism, so that the light split into its different components, and observed what happened to seedlings that grew under different parts of this light spectrum. He found that all seedlings developed chlorophyll, including those illuminated by the "dark" part of the spectrum, that is, by the range of heat rays. These experiments and their interpretation were favorably received by most of Guillemin's colleagues. Wiesner, however, considered them methodologically flawed in almost every respect. He objected that the hypothesis was based on only two experimental runs of limited duration; that the spectral rays were not sufficiently separated from each other, so that overlapping illumination could not be reliably excluded; and, finally, that insufficient methods had been used to detect the formation of chlorophyll, namely, visual inspection and external

[23] It would take another 80 years before this issue was fully resolved (cf. Nickelsen 2015).

[24] The nature of the multitude of different rays in nature, including their chemical and physical properties and effects, were widely debated at the time. On this topic, see, e.g., Hentschel (2007).

appearance. There was no doubt, Wiesner thought, that the case had to be revisited in a more appropriate experimental set-up.

To this end, Wiesner created an entirely controlled environment. He ordered the construction of double-walled glass jars, and had the space between the glass walls (9 mm) filled with a solution of iodine in carbon disulfide. If illuminated, this liquid layer fully absorbed all visible light but was permeable to heat rays. Hence the test factor was fully isolated, and its effect was monitored with a precision thermometer recording temperature changes within the jar. Wiesner then carefully grew sets of etiolated seedlings, which had not yet formed chlorophyll, and he transferred them to the jar in full darkness. Finally, Wiesner used gas light instead of sunlight as a source of illumination. In contrast to the sun's rays, the spectral composition and effects of gas light were well known. Gas light was also much easier to control. In Wiesner's own words:

> Now, I had it in my power to manipulate the incident radiation within a wide range of possibilities, by combining different gas flames, and by varying the distance between flames and test plants. I was able to operate under conditions of constant radiation; and there was the great advantage that I was in full command of how long the experiments would last. (Wiesner 1877, 43)[25]

The last point was especially important: being in command of the course and duration of the experiment. When Wiesner first tried these experiments with sunlight, he had to stop early because of unexpected clouding in the sky. He was not able to draw reliable conclusions. The new experimental set-up, in contrast, yielded crystal-clear results: not a single seedling in the jar turned green, and a sensitive fluorescence test confirmed that not even traces of chlorophyll had formed in any of them. Wiesner corroborated these findings with two control experiments. First, some of the etiolated seedlings were not placed in the jar but exposed to full gaslight. Wiesner found that these seedlings formed chlorophyll without problems, indicating that there were no inherent problems with the seedlings and light. Second, Wiesner exposed the jar seedlings after their heat experience to the full spectrum of the gaslight, where most of them recovered and turned green. Wiesner was satisfied, and concluded that heat rays were insufficient to induce the formation of chlorophyll in plants.

This meticulous care in experimentation characterized Wiesner's work in general. The double-walled glass jar is only one example of a sophisticated apparatus he specifically constructed to meet his standards. Others included a so-called Clinostat, which neutralized the gravitational pull by slow rotation, thereby allowing it to distinguish the influence of light on plants from the influence of gravitation. He was also responsible for innovative applications of the Auxanometer, a self-registering instrument that continuously monitored a plant's growth. Besides isolating the test factor, one also had to control and measure its impact as precisely as

[25] German original: "lch hatte es nunmehr in meiner Gewalt die Strahlung durch Combinirung von Gasflammen, Regulirung der Entfernung zwischen Gasflamme und Versuchspflanze innerhalb weiter Grenzen zu nuanciren, konnte bei constanter Strahlung operiren und hatte den grossen Vortheil, die Dauer der Versuche völlig zu beherrschen."

possible. In this latter respect, Wiesner thought, the then-current investigation of the influence of light was highly deficient:

> In physiology, one is not satisfied with the mere distinction between warm and cold but examines the surroundings of the plant with a thermometer, to the great benefit of the discipline. In a similar way, we must finally begin to measure the intensity of light that a plant receives in order to learn how much influence certain light intensities exert on the vital processes of plants. (Wiesner 1894, 1079)[26]

If light fundamentally affected a plant's growth and development, as everybody agreed that it did, it was high time to measure it in a way that allowed quantifying those effects. In other words, Wiesner was calling for a better-controlled method of recording.

In physics and chemistry these techniques already existed. In the 1860s, chemists Robert Bunsen (1811–1899) and Henry E. Roscoe (1833–1915) had developed a procedure to measure the intensity of the so-called "chemically active" rays, that is, rays of short wavelengths (blue-violet). They used standardized photographic paper and a color chart of blackness. The technique allowed the user to determine the intensity of these rays with high precision, but it was very demanding in practice. When Wiesner finally mastered the procedure, he used it in several experiments but almost immediately set out to develop a slightly adapted version (see Wiesner 1893). His version was less precise but easier to use and, as Wiesner emphasized, more reliable for high-light intensities. It was, therefore, more appropriate for measuring light conditions in nature, where Wiesner had taken his studies in the meantime.

7.4 New Concepts in the Field

After he had established the influence of light on the formation of chlorophyll, Wiesner also wanted to know how light affected the development of buds, the movement of tendrils, the shapes of leaves, the phenomena of differential growth, and other things. He first studied these questions in a series of greenhouse experiments. But when he started to investigate the same phenomena outside, Wiesner made two important observations. First, the intensity of chemically active light was dramatically higher outside than in the laboratory, so that the Bunsen–Roscoe method no longer worked. Apparently glass had a strong shielding effect, so that only a small part of the incident sunlight was actually effective behind windowpanes. One had to be careful, Wiesner concluded, in transferring the results of glasshouse experiments

[26] German original: "Aber so wie man sich in der Physiologie nicht mit der blossen Unterscheidung von warm und kalt begnügt, und die Medien, in welchen die Pflanzen sich ausbreiten, thermometrisch prüft, zu grossem Nutzen dieser Wissenschaft, so müssen wir endlich anfangen, die der Pflanze zu Gute kommenden Lichtstärken zu messen, um den Grad der Einwirkung der Lichtintensität auf die Lebensprocesse der Pflanzen kennen zu lernen."

to plants outside, living under natural conditions. Second, Wiesner found that in nature, plants received very different intensities of incident light, even if they grew almost side-by-side:

> The quantity of light that a plant receives is not only determined by the place on Earth where the plant grows but is also influenced by the specific characteristics of its location and, finally, by the form, number, and position of its organs. (Wiesner 1894, 1079)[27]

For Wiesner these differences were too important to be ignored, and had to be investigated through careful and precise measurements. This was an interesting move: inside the glasshouse, Wiesner might have tried to control this additional parameter (in the sense of Schleiden's type 2 experiments), but under natural conditions, in contrast, where full control was impossible, these differences became part of the research question and therefore had to be monitored (along the lines of Schleiden's type 1 experiments).

The relevant parameter under natural conditions, Wiesner concluded, was not the intensity of light incidence in general but the amount of light that a plant actually received ("factischer Lichtgenuss")—or, as Wiesner termed it, a plant's or a plant organ's "*specific* light reception" ("specifischer Lichtgenuss").[28] He defined this parameter as the fraction of light that a plant received at its specific location compared to the full amount of light, the "full daylight" ("gesammtes Tageslicht"), that a hypothetical plant would receive in the same place fully in the open. Only under exceptional circumstances were the two parameters identical: if leaves were growing on the surface of a pond in full sunlight, for example, or if desert plants developed in full exposure. But these cases were very rare. Wiesner was greatly surprised by this finding: "The influence of the specific location on a plant's actual reception of light is, according to photometric investigation, far more significant than one would assume at first glance" (Wiesner 1894, 1081–82).[29] Even within the crown of one tree, the specific light reception of different organs varied enormously, from very high intensities at the tip of the branches to very low intensities near the trunk.

For Wiesner, these differences in actual or specific light reception were possibly the most import factor of influence for the development of plants and the shape of their organs. As he reminded his colleagues, light acted on plants as a double-edged sword: it was an indispensable catalyst of vital processes, but too much light was also harmful, as Wiesner himself had confirmed in his chlorophyll studies. Plants were creative in providing their organs with the optimal balance of light and shade.

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[27] German original: "Das Lichtquantum, welches einer Pflanze zufliesst, ist nicht nur durch den Erdpunkt gegeben, auf welchem die Pflanze vorkommt, sondern wird auch mitbedingt durch die specifischen Eigenthümlichkeiten ihres Standortes, endlich durch die Form, Zahl und Lage ihrer Organe."

[28] This term is difficult to translate into English, as even Wiesner's colleagues from Anglo-Saxon countries acknowledged. "Specific light incidence" is an alternative term that was sometimes in use.

[29] German original: "Der Einfluss des Standortes auf die Grösse des Lichtgenusses der Pflanze ist, wie die photometrischen Untersuchungen lehren, viel beträchtlicher, als der Augenschein vermuthen liesse."

The morphology of leaves and stems, the patterns of arrangement and branching, the formation of buds, flowering periods, the growing of hairs and cuticle layers: Wiesner thought that all these phenomena, and many more, could be explained as reactions to a plant's specific light reception. And this might not only be true for individual specimens, but could also hold for the properties of plant species or for even larger patterns of vegetation.

But before these latter questions could be investigated, which all involved the long-term effect of certain kinds of illumination, Wiesner had to close a methodological gap. As mentioned earlier, his parameter of "specific light reception" was determined as a fraction of the hypothetical value of "full daylight" at the same location. The latter quantity, however, was not easy to determine. In some cases one could simply measure the light incidence nearby, in the open, but this was not always possible. Furthermore, given the daily and seasonal fluctuation of light incidence, one or two measurements were insufficient. A comprehensive investigation of "photochemical climates" was necessary, which became another new parameter Wiesner introduced (Wiesner 1897, 1907). It designated the average light conditions in a region, based on long-term data collected in one place under various conditions.

Wiesner emphasized that these investigations required commitment and persistence. He noted dismissively that some people had tried to extrapolate photochemical climates from just a few data and a set of equations. Wiesner found this approach not only careless but illegitimate and flawed: "With regard to the chemical intensity of light, as with temperature, the law of distribution on Earth can only be found by experiment" (Wiesner 1897, 75).[30] In collaboration with two assistants, Wiesner initiated more appropriate measurement series in Vienna, but he soon decided that he needed to investigate different climate zones to learn from their comparison. This was the main reason for Wiesner's extensive travel activities rather late in life: to the Botanical Station in Buitenzorg in the East Indies, Yellowstone National Park in the United States, Cairo in North Africa, and Tromsö in the Arctic.[31] Wiesner clearly had come a long way from his laboratory experiments on the formation of chlorophyll—geographically, methodologically, and intellectually. His new agenda was extremely innovative and ambitious. The question remains, however, whether it was still in line with the methodology so forcefully advocated by his famous teacher.

[30] German original: "Wie bezüglich der Temperatur wird also auch rücksichtlich der chemischen Intensität des Lichtes das Gesetz der Vertheilung auf der Erde erst durch das Experiment gefunden werden können."

[31] Fridolin Krasser and Ludwig Linsbauer contributed to the measurements in Vienna, Wilhelm Figdor worked with Wiesner on the climate in Buitenzorg, and Leopold Portheim travelled with Wiesner into Yellowstone National Park. See, e.g., Wiesner (1897, 75, 1898; 1907). On climate research in the Habsburg Empire (with a focus on the time before Wiesner), see Coen (2018).

7.5 A New Methodology for a New Field of Study: The Biology of Plants

The answer to this question is complex and requires a brief digression. For many scholars at the time, plant physiology, that is, Wiesner's discipline, was the embodiment of Schleiden's scientific botany, which inherently meant a strong commitment to empirical work and physicochemical explanations wherever possible. But there was no general consensus about what exactly this field entailed. In his keynote to the 1895 meeting of the American Association for the Advancement of Science (AAAS), the botanist Joseph C. Arthur circumscribed the field as follows:

> [V]egetable physiology […] is like a western or African domain, long inhabited at the more accessible points, more or less explored over the larger portion, but with undefined boundaries in some directions, and with rich and important regions for some time known to the explorer, but only now coming to the attention of the general public. In fact, our domain of vegetable physiology is found to be a diversified one, in some parts by the application of chemical and physical methods yielding rich gold and gems, in other parts coming nearer to every man's daily interest with its fruits and grains. (Arthur 1895, 360)

For Arthur, plant physiology covered a wide range of subjects and approaches; the boundaries with other fields of study were blurred and exciting discoveries still lay ahead, just as exciting as the discoveries waiting in Africa for the Europeans or in the Wild West for the Americans. Wiesner shared this broad understanding of the discipline, albeit without the dubious colonial metaphors. He thought that plant physiology "encompasses the study of everything regarding the plant's structure, development and life" (Wiesner 1898, 106). In his textbook of botany, first published from 1881 to 1884, he distinguished four divisions of plant physiology. They were: *first*, Anatomy and Physiology in the narrow sense, that is, the physicochemical explanation of vital processes (similar to Wiesner's own investigation of the formation of chlorophyll); *second*, Organography, the investigation of shape, development, and changeability of plant organs; *third*, a modernized Taxonomy and Systematics that also considered physiological and chemical properties of plants; and *fourth*, the "Biology of Plants" (Wiesner 1881b, 1884). This final division expanded substantially over time, and starting from 1899, it became a full separate volume of the textbook (Wiesner 1889).[32]

How are we to understand this "Biology"? The term is ambiguous and its history complex (see, e.g., Toepfer 2011). According to a still-popular narrative, it was in 1800 that the French naturalist Jean-Baptiste de Lamarck (1744–1829) and the German naturalist Gottfried Reinhold Treviranus (1776–1837) allegedly invented biology independently from each other, as the science of life. Over the course of the nineteenth century, the field then developed "from natural history to biology," to borrow a widely used expression, that is, in linear progression from a descriptive, old-fashioned enterprise into the scientific discipline we know today. Joseph Caron criticized this narrative already in 1988 and argued that the invention of a name

[32] For Wiesner's concept of "Biologie," see also Nickelsen (2023).

must not be confused with the founding of a discipline. He thought that the first real attempts at the discipline were made by Thomas H. Huxley, who in 1858 attempted to institutionalize a class in “Principles of Biology” in Cambridge, albeit with moderate success (Caron 1988). Kai T. Kanz then pointed out that we cannot extrapolate to other countries from this episode in England. With several examples Kanz showed how, from the eighteenth century, the term “biology” was used as either an umbrella term, a subordinate term, or a term synonymous with various others, and often the term was not used at all (see, e.g., Kanz 2002, 2006, 2007). Even more complicated is the combination of biology with other terms. Highly illuminating in this context is Eugene Cittadino’s (1990) discussion of “Biologie der Pflanzen” as precursor of evolutionary ecology for plants in the German-speaking countries.[33] This was exactly how Wiesner used the term in his pioneering textbook, as the following passage demonstrates:

> The word biology has very different meanings. Huxley, and probably most British naturalists (“*Naturforscher*”) with him, use the word in its broadest sense, as the study of organisms. Other naturalists have significantly limited the concept and regard biology as that part of science that deals with the way of life of plants and animals.
>
> The majority of today’s naturalists fall somewhere between these two extremes and see biology as the science of the habits, heredity, variability, adaptation, origin and natural distribution of organic beings. In this last sense, the word biology will be understood in the present book. (Wiesner 1889, 1)[34]

Wiesner obviously was aware of the terminological difficulties. He therefore tried to clarify his own usage in reference to the emergent field of study, which he introduced as part of “plant physiology”—understood widely but differently from “plant physiology” in a narrow sense (although he had to admit that the boundaries were blurred). Wiesner’s colleagues similarly struggled with defining this new field, including the Munich-based botanist Karl Goebel (1855–1932) in a paper of 1898:

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[33] See Cittadino (1990), esp. 149. Lynn K. Nyhart made a similar observation of a new “biological perspective” on animal life, albeit mostly beyond the circles of academic zoology; see Nyhart (2009). Already Arthur (1895) pointed to this German peculiarity and specifically cited Wiesner’s book as the first to have been published on the theme (the only other book that Arthur cited was the one by Friedrich Ludwig, see below; Arthur also declared that there was so far no analogous publication in English). Arthur acknowledged that the name “biology” was justified, yet given that Huxley had already used “biology” differently, he favored the alternative designation of this area as “ecology.” See Arthur (1895, 365).

[34] German original: “Man bezeichnet mit dem Wort *Biologie* sehr Verschiedenes. Huxley und mit ihm wohl die meisten britischen Naturforscher gebrauchen dieses Wort in seinem weitesten Sinne, als die Lehre von den Organismen. Andere Naturforscher schränken diesen Begriff wieder sehr stark ein und betrachten die Biologie als jenen Theil der Naturwissenschaft, welcher sich mit der Lebensweise der Pflanzen und Thiere beschäftigt. Die Mehrzahl der heutigen Naturforscher bewegt sich in der Mitte zwischen diesen beiden Extremen und begreift unter Biologie die Lehre von der Lebensweise, Erblichkeit, Veränderlichkeit, Anpassung, Entstehung und natürlichen Verbreitung der organischen Wesen. In dem zuletzt bezeichneten Sinne soll auch in diesem Buch das Wort Biologie verstanden sein.”

> We can compare the relationship between physiology and biology to that of two maps, one of which displays only the mountain ranges and rivers, the other also the political borders and settlements. How a country is populated clearly depends on its physical nature but also on the characteristic properties of its inhabitants and their varied history. Similarly, experimental physiology shows us a broad outline of the relationship of plants to their environment, but it does not reveal how the vital processes take place according to the plants' characteristic properties and history. On a general level, for example, the role of water is the same for all plant species. However, the ways in which plants go about meeting their demand for water, depending on their level of organization and the conditions of the environment, is infinitely different. (Goebel 1898, 4)[35]

Goebel, as we see here, drew the line between the two fields, physiology and biology, in terms of the questions being asked and the level of particularity being studied. Whereas physiology investigated the water balance of plants in general, biology studied the multiple adaptations of plant species, that is, their "manifold relationships to the outside world."[36] Like Wiesner, Goebel was in favor of the new field. He thought that the progress of physiology had come to a halt, while biology was on the rise, for two main reasons. First, the ongoing "exploration of tropical areas": botanists were no longer satisfied with lists of new species but had started to investigate the multitude and variability of vital processes on display in tropical climates. Second, the new approach of "Darwinism," which pointed to the interplay of an organism's morphology with its natural environment. Both had not only raised important questions but also opened paths to answer them (on these points, see Goebel 1898, 4–5).

For Goebel, "Darwinism" did not primarily refer to the transformation of species by means of natural selection: "In fact, if we look at today's botanical literature, we find that the actual Darwinism, that is, the theory in which natural selection is the main factor that causes adaptations, is hardly represented anymore, at least in Germany" (Goebel 1898, 10–11).[37] Goebel explained that even Darwin himself had increasingly downgraded the importance of natural selection. It might well be that it contributed to the transformation of species, but for Goebel, direct adaptation was

[35] German Original: "Das Verhältniss zwischen Physiologie und Biologie können wir etwa dem zweier Landkarten vergleichen, von denen die eine uns nur die Gebirgszüge und Flüsse, die andere auch die politischen Grenzen und Ortschaften gibt. Wie nun die Besiedelung eines Landes zwar abhängig ist von seiner physischen Natur, aber ausserdem auch von den charakteristischen Eigenschaften seiner Bewohner und ihrer wechselnden Geschichte, so zeigt uns auch die Experimentalphysiologie nur in grossen Zügen die Beziehungen der Pflanzen zur Aussenwelt, nicht aber, wie je nach der besonderen Eigenthümlichkeit und nach der Geschichte einer Pflanzenform ihre Lebensvorgänge sich abspielen. So ist die Bedeutung des Wassers im Wesentlichen für alle Pflanzenformen dieselbe, unendlich verschieden aber die Art, wie je nach der Organisationshöhe oder den äusseren Lebensbedingungen der Wasserbedarf gedeckt wird."

[36] German original: "mannigfaltige Beziehungen zur Aussenwelt."

[37] German original: "In der That, sehen wir uns in der heutigen *botanischen* Literatur um, so finden wir, dass der eigentliche Darwinismus, d.h. die Richtung, welche der natürlichen Zuchtwahl die *Haupt*rolle bei dem Zustandekommen der Anpassungen zuschreibt, in Deutschland wenigstens fast keine Vertreter mehr hat."

clearly the most significant factor. Its effects were also apparently transmitted to the next generation, which should remove the last lingering doubt about its significance, Goebel maintained. He predicted that biology would gain important insights along these lines in the near future, and concluded on a lyrical note: "The young biological science resembles the man which the poet sings about: 'There he goes without hesitation/His soul filled with dreams of harvest/And he sows and hopes.'" (Goebel 1898, 21).[38]

A slightly different relationship between physiology and biology was suggested by the botanist Friedrich Ludwig in his textbook on *Biologie der Pflanzen* (Ludwig 1895). In accordance with his Italian colleague Federico Delpino, Ludwig defined biology as "the doctrine of the external relationships of plants," whereas physiology was the "doctrine of the internal processes of plants."[39] To this Ludwig added a methodological observation: "While the latter amounts to physicochemical transformations, the former sneers at all attempts of mechanical explanation, as will always be the case with the mechanical explanation of life in general" (Ludwig 1895, V).[40]

For Ludwig, the difference between investigating (and explaining) the inner processes of plants, and investigating (and explaining) their relationship with the external world, was correlated with different types of explanation. Whereas physiology aimed at a "mechanical explanation," to be understood as causal explanation based on physicochemical factors, biology strove for non-mechanical, primarily teleological explanations. Ludwig left no doubt that he, like Delpino, considered "mechanical" approaches insufficient, and so supported a vitalist perspective on the manifestations of life. "Biology," thus understood, came dangerously close to *Naturphilosophie*, which many botanists at the time regarded as the epitome of "unscientific," and from which they had only just emancipated themselves. Wiesner was clearly getting into troubled methodological waters with his new area of interest.

[38] German original: "Da geht er ohne Säumen / Die Seele voll von Ernteträumen / Und sät und hofft". Goebel cites these verses, without any explicit reference, from a poem written by J. W. Goethe, "Ein zärtlich jugendlicher Kummer" (which approximately translates to "A tender adolescent sorrow").

[39] German original: „die Lehre von den äußeren Lebensbeziehungen der Pflanze“ vs. „die Lehre von den Vorgängen des inneren Pflanzenlebens.“

[40] German original: "Während die letzteren auf physikalisch-chemische Umwandlungen hinauslaufen, spotten die ersteren aller mechanischen Erklärungsversuche in dem Maße, wie dies mit der mechanischen Erklärung des Lebens überhaupt immer der Fall sein wird." Federico Delpino (1833–1905) pioneered the study of how floral morphology related to pollination. He also investigated the topic of "plant intelligence" and supported a teleological, spiritual interpretation of the processes of evolution.

7.6 A New Role for Speculation?

The introduction to Wiesner's textbook on the biology of plants is highly instructive in this respect. "Physiology" (in a narrow sense) and "biology" differed in their subject matter, he explained, and therefore necessarily also differed in methodology. Physiology focused on specific processes, such as transpiration and respiration, and sought to spell out the effects of isolated factors in this context. To this end, physiologists used the "*inductive* method" of chemistry and physics, which Wiesner understood as drawing inferences about causal links from experimentation. Biology, in contrast, focused on so-called "vitalistic" problems, "which we cannot yet resolve with exact scientific methods", and it aimed to understand the effect of all factors combined. It therefore "mostly arrives at the desired outcome by way of *speculation*" (Wiesner 1889, 2; both emphases in original).[41] As an example, Wiesner pointed to the complex relationship between insect behavior and flower morphology (which incidentally was an important research area of Delpino's, one of the authorities with a vitalistic inclination referred to by Ludwig). If one wished to illuminate these phenomena, Wiesner explained, it was not only practically impossible to separate the different factors from each other; it was also nonsensical, because the investigation aimed at the interplay of factors.

Given the ill repute of speculation at the time, in the wake of Schleiden's critical campaign against "speculative botany," this was dangerous ground. But the methodological schism between physiology and biology was not as radical as it might appear, Wiesner hastened to add: "For physiology too, like every other natural science, has to draw on speculation from time to time, to quickly open up new ways of induction, or to accelerate its often sluggish pace. And biology will only gain a sufficient basis for its speculation from the facts that have actually been ascertained" (Wiesner 1889, 2).[42] This commitment to an empirical basis implied that not all speculation was legitimate. In line with Schleiden, Wiesner insisted that vital forces or instincts were unacceptable as explanatory factors in biology as well:

> Overall, the assumption of a special vital force is only justified insofar as we have not yet succeeded in tracing all manifestations of life back to the effects of mechanical forces. However, since the assumption of a specific vital force loses its justification in proportion to the advances of the natural sciences, and since the assumption itself has turned out to

[41] German original: "*inductive* Methode"; "[vitalistische Probleme], welchen wir mit exacten naturwissenschaftlichen Methoden noch nicht beizukommen vermögen"; Biology arrives "vornehmlich auf dem Wege der *Speculation* zu den erstrebten Resultaten" (emphases in original).

[42] German original: "Freilich zeigt sich auch hier wieder die Zusammengehörigkeit beider; denn auch die Physiologie muss, gleich jeder anderen Naturwissenschaft, zeitweilig die Speculation heranziehen, um rasch neue Wege der Induction zu erschliessen, oder um den oft schleppenden Gang der Induction abzukürzen, und auch die Biologie wird nur aus dem thatsächlich Erhobenen eine zureichende Basis für ihre Speculation gewinnen."

> be absolutely unfruitful, […], one must approve the point of view delineated at the beginning of this paragraph: that the existence of a specific vital force cannot be accepted (Wiesner 1889, 14).[43]

And to make his position perfectly clear, Wiesner added, "The peculiarity of the life processes is not to be found in a principle independent of matter, or in a specific vital force, but in the combination of mechanical forces" (Wiesner 1889, 14).[44]

For Wiesner, biology was not an invitation to revitalize elusive forces. It was the attempt to include complexity and long-term effects into the realm of science. But translating this ideal of biological investigation into methodologically sound research practice remained a challenge. Wiesner's own research shows how he dealt with this dilemma, with examples as early as the 1870s. In his chlorophyll studies, Wiesner found that this pigment, which was essential for a plant's survival, was extremely sensitive to light and easily harmed in direct illumination. One should therefore expect to find in the organs and tissues of plants "special means of protection to preserve this substance," Wiesner explained, and this is exactly what he then identified (Wiesner 1875, 22).[45] Wiesner described in detail the striking differences between a plant's morphology in the sun and in the shade, including the shape and structure of stems, leaves, cuticles, and hairs, and also between patterns of vegetation, periodic movements, and other factors. On this basis, Wiesner explained the emergence of these characters in nature as a protection strategy against too much light—that is, with reference to their purposes and not their causes.[46] Wiesner conceded that the approach entailed methodological risk, but assured his readers that the risk was limited: "Since biology builds its speculations upon a broad factual basis, its hypotheses—notably Darwin's important doctrine, which in a way inaugurated the age of biological research—gain strength and support"

[43] German original: "Alles in allem genommen hat die Annahme einer besonderen Lebenskraft nur insofern eine Berechtigung, als es bisher noch nicht gelungen ist, alle Lebensäusserungen auf die Wirksamkeit mechanischer Kräfte zurückzuführen. Da aber die Annahme einer specifischen Lebenskraft desto mehr an Berechtigung verliert, je weiter die exacte Naturforschung vorwärtsschreitet, und da diese Annahme sich durchaus als unfruchtbar herausgestellt hat [...], so wird man den im Eingange dieses Paragraphen markirten Standpunkt, von welchem aus eine besondere Lebenskraft nicht zugestanden werden kann, nur billigen müssen."

[44] German original: "Das Eigenartige der Lebensprocesse ist also nicht in einem von der Materie unabhängigen Principe oder in einer specifischen Lebenskraft, sondern in der Combination mechanischer Kräfte zu suchen."

[45] German original: "besondere Schutzmittel zur Erhaltung dieser Substanz [waren] schon von vornherein [zu] erwarten."

[46] One anonymous reviewer of Wiesner's book on the "Lichtgenuss der Pflanzen," however, felt that Wiesner's research had yielded important insight but was methodologically problematic: "The book is by no means free from doubtful generalizations and generous assumptions; indeed, it seems that everyone who deals with adaptations must allow his imagination a rather loose rein. Withal there is in the work an important nucleus of no little value, and even an occasional flight of fancy may be permitted, if it stimulates interest" (C.R.B. 1908, 343).

(Wiesner 1889, 3).[47] While the speculation that *Naturphilosophie* employed had been unfounded and fruitless, speculation in biology was based on facts and therefore legitimate, Wiesner wanted to persuade his readers. It was, in a way, a "controlled" form of speculation.

The reference to Darwin in this context is significant.[48] Wiesner's biology included all the phenomena Darwin had wanted to explain, such as the "habits, heredity, variability, adaptation, origin and natural distribution of organic beings" (see Wiesner's definition of the field, quoted above). Darwin had likewise been accused of speculation: of presenting hypotheses insufficiently based on empirical evidence. In turn, Darwin had rejected this critique as unfounded and justified his speculation, like Wiesner, with reference to the explanatory power of his hypothesis: it simply had to be true because so many phenomena could be explained with his theory that otherwise would remain mysterious. This argument was common practice in other areas of nineteenth-century science, where it was impossible to provide experimental proof. The argument was always contested and certainly fallible but not illegitimate (see the chapter by Coko in this volume, in particular the discussion of how Gouy tried to explain Brownian motion). Wiesner now pointed to the same principle for the biology of plants.

However, there were limits to the speculation that Wiesner was prepared to accept. In 1880, Darwin published a comprehensive treatise on the movements of plants, in which he presented the result of studies (undertaken with his son Francis) on the question of how plants responded to external stimuli (Darwin 1880). Like others of Darwin's major publications after 1859, the book provided further evidence for his theory of transmutation and common descent. In a letter to his colleague Alphonse P. de Candolle, Darwin described his main finding with glee: "I think that I have succeeded in showing that all the more important great classes of movements are due to the modification of a kind of movement common to all parts of all plants from their earliest youth."[49] A second claim of Darwin's was that environmental factors, such as light, gravity, etc., acted like stimuli on certain areas of the plant with their effects transmitted to others, similar to transmission processes in the sensory and nervous systems of lower animals. Wiesner greatly admired Darwin's work, but in this particular case, he was unimpressed and found much to criticize:

> I soon recognized that Darwin had entered an area in which the methodology is just as powerful, and perhaps I do not exaggerate when I say, more powerful than the genius, namely the area of experimental plant physiology. In this field, no step forward can reliably be taken unless accurate physical or chemical methods are used to solve the problems,

[47] German original: "Da aber die Biologie ihren Speculationen eine möglichst breite thatsächliche Unterlage gibt, gewinnen ihre Hypothesen—namentlich die bedeutungsvolle Lehre Darwins, welche die Epoche der biologischen Forschung geradezu inaugurirte—Halt und Stütze."

[48] Arthur agreed with Wiesner on this point: "We may call Darwin the father of vegetable ecology, for had he not written, the field would have lain largely uncultivated and uninteresting" (1895, 368).

[49] Darwin to DeCandolle, 28 May 1880, in F. Darwin (2009 [1887]), 333).

> even if the question is precisely formulated. Darwin has not conducted his experiments with the required rigor, which is why many of his results are uncertain, even doubtful. (Wiesner 1881a, 3)[50]

Darwin, apparently, failed to meet Wiesner's methodological standards in experimental work. With this assessment, Wiesner fully agreed with his colleague from Würzburg, Julius Sachs, who not only rejected Darwin's conclusions as inaccurate but also ridiculed his experiments as unskillful and meaningless (e.g., Sachs 1882, 843). Soraya de Chadarevian (1996) has convincingly interpreted this strong reaction by Sachs as an attempt to maintain authority on the right way to do experiments, namely under fully controlled conditions in the laboratory. From this perspective, Darwin's naturalist approach could not possibly produce useful results, because it failed to meet the requirements of a scientific botany—which Sachs publicly championed as the only legitimate approach to plant science. De Chadarevian's claim is probably correct for Wiesner too, because he shared Sachs's methodological standards and worked in the same project of discipline-building. However, it is worthwhile to look at the specific targets of Wiesner's critique, in order to see where Wiesner tried to draw a line between the legitimate methodological approach of plant biology, which necessarily violated some plant physiological conventions, and illegitimate work, which yielded unreliable data and untenable conclusions.

Given the time and effort he invested, Wiesner clearly considered the issue important. To demonstrate where Darwin went amiss, Wiesner carefully replicated many of Darwin's experiments, compared the findings, and in most cases challenged Darwin's interpretation (and in many cases the experimental design as well). The result was devastating for Darwin. Wiesner presented his critique respectfully and with nuance, and he acknowledged that the work presented many interesting and valuable observations. Nevertheless, he fundamentally disagreed with its claims. "No man was ever vivisected in so sweet a manner before, as I am in this book," Darwin maintained in a letter to his friend and colleague Joseph D. Hooker (Chadarevian 1996, 38).[51]

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[50] German original: "*Darwin's* Buch enthält, wie ich mich alsbald überzeugte, wieder eine Fülle neuer interessanter Beobachtungen und geistreicher biologischer Bemerkungen über den Zweck der Bewegung für das Leben der Pflanze. Allein, ich musste bald erkennen, dass *Darwin* hier ein Gebiet betreten, in welchem die Methode ebenso mächtig, und vielleicht ist es keine Uebertreibung, wenn ich sage: mächtiger ist als das Genie, das Gebiet der experimentellen Pflanzenphysiologie, in welcher bei aller Schärfe der Fragestellung kein sicherer Schritt nach vorne gemacht werden kann, wenn nicht genaue physikalische oder chemische Methoden zur Lösung der Probleme in Anwendung gebracht werden. *Darwin* hat nun seinem Experiment nicht die erforderliche Strenge gegeben, wesshalb viele seiner Ergebnisse unsicher, ja zweifelhaft werden."

[51] De Chadarevian cites a letter by Darwin to Hooker of 22 October 1881. Darwin clearly did not take Wiesner's critique lightly but discussed the matter in a series of letters with his son Francis Darwin and also with Wiesner himself (de Chadarevian 1996, 38; see also the online edition of letters provided by the Darwin Correspondence Project at: https://www.darwinproject.ac.uk/letters). While Darwin conceded that Wiesner's critique was convincing in many points, he was not prepared to change his mind on the subject entirely.

Wiesner's main critique was that Darwin introduced hypotheses that were impossible to substantiate empirically—in other words, they were unfounded speculations. A prime exemplar, Wiesner thought, was Darwin's claim that all plant movements were derived from growth in the form of circumnutation. Wiesner admitted that there was something "tremendously appealing" about this idea (Wiesner 1881a, 23). However, in the absence of any conclusive evidence, it was just as likely that the exact opposite was true, namely, that all plant movements were derived from straight growth. Wiesner concluded that there was little value in Darwin's claim "because it is entirely based on speculation" (Wiesner 1881a, 23).[52] Wiesner, in contrast, tried to explain the same phenomena as the effect of a number of well-known factors combined, which for him made any additional hypothesis unnecessary.

The second target of Wiesner's critique was Darwin's claim that environmental stimuli were transmitted through the plant—from the sites of perception to adjoining tissues or organs, where then the reactions took place. This transmission would be similar to the functioning of sensory organs in animals. In particular, Wiesner set out to demonstrate that "the tip of the radicle did not have the peculiar and apparently mysterious properties, which Darwin attributed to it and which prompted him to claim that this part of the root directed all its movements and worked similarly as the brain of a lower animal" (Wiesner 1881a, 12–13). Wiesner firmly rejected the claim and was particularly critical of the comparative approach. The analogy between plant and animal characteristics was not illuminating at all, Wiesner maintained; even worse, he found it dangerous.[53] Wiesner acknowledged that Darwin had vital interest in drawing this analogy, because the unity of plants and animals was inherent to Darwin's theory of common descent. However, in this case Darwin went further than was compatible with a "rational investigation of nature" (Wiesner 1881a, 15). This was unfortunate and set a bad example:

> Darwin's comparison of plants and animals is always spirited and original, and it gives us intellectual pleasure even if we must disagree. However, these digressions raise concern since they encourage less talented students of nature to emulate this approach and

[52] As Wiesner wrote: "Allein man wird zugeben müssen, dass man auch den umgekehrten Fall setzen kann, d.h. dass man alle diese Bewegungen auch aus der einfachsten Form, dem geraden Wachsthum, ableiten könnte. So annehmbar dies klingt, so gering ist einstweilen der Werth dieser Anschauung, da sie doch nur auf Speculation beruht. Will man eine Grundlage für den Zusammenhang der Formen finden, so muss man den Weg der Beobachtung einschlagen. Es ist dies auch der Weg, den Darwin verfolgte, auf dem er aber zu Resultaten kam, die ich in der von ihm ausgesprochenen Allgemeinheit nicht bestätigen kann."

[53] This part of Darwin's work was also the main target of Sachs's criticism, as Soraya de Chadarevian (1996) has shown. Sachs even prompted one of his assistants, Emil Detlefsen, to replicate Darwin's experiments in order to refute their conclusion (de Chadarevian 1996, 29). Detlefsen notes that his experiments are in full agreement with Wiesner's findings, which he, however, only saw after he had already completed his studies (Detlefsen 1882, 627). German original: "Die kritische Studie von Wiesner […] erhielt ich leider erst, als meine Arbeit schon vollendet war, und ich konnte dieselbe daher nicht berücksichtigen. Es freut mich, constatiren zu können, dass ich in manchen wesentlichen Punkten zu Resultaten gelangt bin, die mit denen Wiesners übereinstimmen."

> steer them in a speculative direction, which turns away from strict investigation and proved to be a veritable impediment for the science of organisms not so long ago. (Wiesner 1880, 16)[54]

For Wiesner, drawing analogies between plants and animals was not only methodologically questionable but also implied a relapse into the aberrations of *Naturphilosophie*. Schleiden had specifically castigated Hegel, Schelling, and others for drawing analogies of this kind between the different realms of nature (e.g., Schleiden 1842, 46–47; see also Jahn 2006). In line with this assessment, Wiesner strictly rejected this type of reasoning, even when it came from Darwin. Speculation was only legitimate if it was principally possible to test the resulting claim empirically. Furthermore, no additional speculative factors or hypotheses were admissible if the phenomenon in question could be sufficiently explained by the effect of well-established factors. For Wiesner, this was where Darwin had failed, even in those cases where his experimental set-up was fine and the measurements beyond reproach.

7.7 Wiesner's Legacy

Wiesner's search for a methodologically sound experimental biology provided critical inspiration for the founding of one of the most interesting research institutions at the time: the *Biologische Forschungsanstalt* in Vienna, also known as the *Vivarium*. This remarkable institution was founded in 1903 by three scientists of Jewish origin: the zoologist Hans Leo Przibram (1874–1944) and the two plant physiologists Leopold von Portheim (1869–1947) and Wilhelm Figdor (1866–1938), who had been Wiesner's students.[55] All three had been unsuccessful in their attempts to gain academic positions in the anti-Semitic atmosphere of the Habsburg Empire at the time and, therefore, used private capital to set up their own research institution. Many people believe that this institution was called "biological" because it investigated questions from both botany and zoology, but a different interpretation is more convincing. The institute was called *Biologische Versuchsanstalt*, I propose, because it engaged in biological research in the sense of Wiesner and others.

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[54] German original: "Darwin's Vergleich der Pflanze mit dem Thiere zeichnet sich stets durch Geist und Originalität aus und gewährt uns auch dann einen geistigen Genuss, wenn wir ihm unsere Zustimmung versagen müssen. Allein diese Excurse haben auch ihre bedenkliche Seite, indem sie weniger begabte Naturforscher zur Nacheiferung anspornen und zu einer speculativen, von der strengen Forschung abgekehrten Richtung hinleiten, welche vor nicht allzu langer Zeit als ein wahrer Hemmschuh für die Wissenschaft von den Organismen sich gezeigt hat."

[55] Portheim had accompanied Wiesner on his journey to Yellowstone National Park. Figdor had travelled with Wiesner to Buitenzorg and Ceylon, and pursued Wiesner's research on the influence of light on leaf arrangements at the *Vivarium*. On the history of this institution, see, e.g., (Reiter 1999; Taschwer et al. 2016; Müller 2017). On the history of plant sciences in the *Vivarium*, see, (Nickelsen 2017).

This interpretation is supported by a remark in the first report of the institution written by Hans Przibram, one of the three founders:

> While it may be enough for the physiologist to keep his research organisms alive for as long as he wants to monitor a particular function, and then take fresh specimens for his further observations, the biologist is usually concerned with tracing the changes of form over a longer experimental period. (Przibram 1908, 235)[56]

This juxtaposition makes sense only if we assume that Przibram shared Wiesner's concept of biology. Biologists need more time than physiologists to complete their experiments, because they are doing biological research: they investigate, as Wiesner had detailed, the habits, heredity, variability, and adaptation of organisms, in interaction with their environment. The institute's particular focus was experimental morphology, the study of the causes of forms and functions of living organisms. This project the founders and their colleagues tried to establish in quantitative terms, but without necessarily aiming for mechanistic explanations. The institute was equipped with sophisticated light and dark chambers and hosted precision instruments of all kinds. Its members were trained in botany, zoology, physiology, chemistry, physics, and mathematics; and they investigated a wide range of vital processes, as well as their morphological basis, in long-term studies. From a certain perspective, the *Vivarium* had turned Wiesner's ambitious vision of a biological research program into reality. Its life span, however, was brief. It survived World War I and became highly successful thereafter, but the annexation of Austria by Nazi Germany in 1938 ended it. All three of its founders were murdered, and today almost nothing is left of this remarkable institution.

7.8 Concluding Remarks

Methodological considerations, statements, and critiques—in other words, "methods discourse" (Schickore 2017)—loomed large in nineteenth-century botanical research. The question of adequate control strategies and practices, the central focus of this volume, was an important part of this discourse, although the term itself was hardly used at the time. Control was even part of a programmatic change: Schleiden and Wiesner were both important protagonists in unfolding a "scientific" botany that ventured beyond descriptive taxonomy and set a comprehensive group of new methods, techniques, and approaches at its core. Wiesner was also instrumental in promoting the standards of a different field, plant biology. For both disciplines, plant physiology and plant biology, textbooks served as highly instructive sources for a reconstruction of methodological attitudes.

[56] German original: "Während es dem Physiologen genügen mag, seine Versuchsobjekte so lange am Leben zu erhalten, als er eine bestimmte Funktion verfolgen will, und dann zu weiterer Beobachtung frische Exemplare zu nehmen, kommt es dem Biologen meist auf Durchverfolgung der Formänderungen während einer längeren Versuchszeit an."

This essay began with Matthias Schleiden's agenda of a fundamental reform of botany, which he thought had been badly damaged by the dogmata of *Naturphilosophie*. To this end he provided his fellow botanists with clear guidelines. Schleiden touched on general issues, such as the principles of empirical work based on *autopsia* and reliable induction, but also gave detailed, hands-on introduction in how to use a scientific microscope. Schleiden reminded his colleagues that conceptual confusion would necessarily lead to unsatisfactory interpretations, and that explanations based on vital forces indicated factual ignorance. Interestingly, experimentation only started to appear in Schleiden's introduction from the second edition onwards, with two types he found permissible: type 1 was a sophisticated monitoring of life processes, and type 2 a difference test according to Mill's method. They required the full range of control dimensions for both physical and cognitive activities, and they were not for everybody. In contrast to observation, performing experiments required philosophical training and the talent to ask the right questions.

Schleiden's influence on subsequent generations of botanists was enormous, especially in the German-speaking countries. Even Julius Sachs, cited at the beginning of this essay, was full of praise: "The difference between this and all previous textbooks is like the difference between day and night," Sachs wrote in his otherwise hypercritical survey of the history of botany (Sachs 1875, 203). Julius Wiesner, one of Schleiden's former students, served as a case in point for this chapter. Wiesner was widely known as an excellent experimenter, and his research in the formation of chlorophyll is a model of Schleiden's type 2 experiments (and Mill's method of difference). Wiesner made painstaking efforts to create experimental set-ups that allowed reliable causal inferences. The careful description of how Wiesner separated potential factors of influence in the lab also demonstrated that, in botany, these measures were far from self-evident at the time. The influence of light on plants was a widely debated topic but few of his colleagues were as successful in studying it as Wiesner. In his research papers or in his textbook Wiesner never used terms such as "control" or "confounding factors," which we might expect in this context, nor did he refer to methodological treatises. But he clearly tried to exert control on all experimental circumstances.

This became impossible, however, when Wiesner moved these studies into the field, where his work started to resemble Schleiden's type 1 experiments in requiring the quantitative monitoring of vital processes of plants in reaction to their environment. Wiesner encountered difficulties of both a practical and conceptual nature. He responded by developing new techniques, such as an adequate procedure to measure light intensities in the field, and by defining new parameters, such as the new unit of specific light reception. But Wiesner increasingly became interested in questions that were impossible to answer in controlled experimental set-ups. He eventually decided that these questions required a subdisciplinary field of their own—the "biology of plants"—with its own methodological principles that deviated from established control strategies and practices. But Wiesner's discomfort in doing so was palpable. He was deeply worried that this approach would lead botany down on a slippery slope into the realm of wild speculation about vital forces; the attitude manifested by his colleagues Ludwig and Delpino confirmed that his worries were not unfounded. "Speculation" was necessary in biology, Wiesner argued, but only within boundaries: if it was based on facts and observation, if it was parsimonious, and if it had the potential to be tested empirically. In other words, if it was *controlled* speculation. The most ambitious attempt to put this program into effect was made by some of Wiesner's former students in the *Biologische Versuchsanstalt* in Vienna, but their institute was short-lived. The difficult transition from fully controlled physiological experimentation via field studies under limited control, to the challenges of methodologically sound research in the ecology and evolution of plants, would therefore be completed elsewhere.

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