Chapter 3
One Myrtle Proves Nothing: Repeated Comparative Experiments and the Growing Awareness of the Difficulty of Conducting Conclusive Experiments

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3.1 Introduction: From a Proven Truth to a Controversial Physical Problem

By the mid-1780s, physicists across Europe considered the view that electricity accelerated vegetation to be a proven truth (van Troostwyk and Krayenhoff 1788, 134; Rouland 1789, 4). Some 40 years earlier, an Edinburgh teacher had suggested that electricity could be applied "towards the improvement of vegetation" (Demainbray 1747a, 3). His suggestion was soon confirmed by famous electrifying philosophers (Priestley 1767, 140–141), and experiments in the 1770s and early 1780s provided further evidence of the growth-enhancing effect of electricity (D'Ormoy 1791, 29). These seemingly unambiguous results left no doubt as to the correctness of the assumption that electricity promotes vegetation (Senebier 1791, 63).

This certainty disappeared, however, when Jan Ingen-Housz (1786) questioned the validity of the earlier experiments. His criticism convinced many of his contemporaries, who felt compelled to agree that artificial electricity had no influence on vegetation (Senebier 1791, 64). Tiberius Cavallo (1803, 357), for one, concluded in *The Elements of Natural or Experimental Philosophy* that "with respect to vegetation, the most impartial, diversified, and conclusive experiments have shewn, that

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electrization does neither promote nor retard vegetable life."[1] For Alexander von Humboldt (1794, 77–79), on the other hand, the matter was less clear-cut. He suspected that there was no physical problem on which scholars were as divided as about the effect of electricity on vegetation. According to experimental physicists involved in the debate, the case showed how difficult it is to make truly demonstrative experiments (Rouland 1789, 4) and how easy it is to err (Ingen-Housz 1789, 225–226). This chapter takes a closer look at these difficulties. Physicists studying the influence of electricity on plant growth were very well aware that a number of factors could prevent them from drawing correct experimental conclusions. I examine their reports to discover what kinds of errors this group of physicists tried to avoid, and what strategies they used to do so.

The protagonists of this chapter were not attempting to uncover the cause of a puzzling phenomenon, as was the case for the subjects covered by Cristalli and Coko, Chaps. 6 and 8 in this volume, or by Schickore (2023). Rather, they wanted to see whether a well-known factor, electricity, actually affected the growth of plants. To this end, they performed comparative experiments. We will see that they did this as a matter of course, without making their methodology explicit. In line with what Schickore (Chap. 1, this volume) refers to as the "narrower" notion of experimental control, they considered comparative experimental design a prerequisite for drawing safe conclusions. In their experiments they also attended to control practices in the broader sense. They kept the two experimental settings stable to exclude other factors from interfering with the experimental outcomes. They tried to expose their plants to exactly the same conditions, except for electrification, and noted the values for conditions they could not or did not want to control, such as the weather or the temperature. In addition, they detailed their intervention and how they assessed plant growth. Some also measured the intensity of the electricity applied and varied the amount to see if the effect changed accordingly.

While they agreed that comparative tests must support causal conclusions, they disagreed on what exactly would ensure a safe basis for concluding that electricity promotes plant growth. Many felt that a single run of a comparative experiment was sufficient. If the electrified plants grew faster than the non-electrified ones, they took the difference to indicate a vegetation-enhancing electrical effect. Others, however, refused to draw conclusions from single experiments and insisted on many repetitions of the same experiment. Only when they observed the difference consistently were they prepared to assume a causal relationship. The experimenters also disagreed on the number of test objects to be used per experiment. While most physicists compared the growth of a small number of plants, Ingen-Housz monitored thousands of cress seeds. This was intended to compensate for the individual variability of the experimental objects. However, he seems to have been the only one at the time who considered this control measure necessary.

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[1] Cavallo (1782, 38) had changed his mind on this subject, having previously stated: "By increasing the perspiration of vegetables, Electricity promotes their growth; it having been found, after several experiments, that such plants, which have been often and long electrified, have shewed a more lively and forward appearance, than others of the same kind that were not electrified."

In contrast, many physicists warned against the expectations of experimenters as a source of error. Some even argued that the view of electricity as a growth promotor had only prevailed because it had been reported by famous physicists, and subsequent generations of physicists had not tested it thoroughly enough. Ingen-Housz (1789, 217) therefore urged his colleagues not to rely on authorities, but rather to examine each other's experiments in search of errors.

The chapter is structured as follows. Section 3.2 presents some experiments carried out between 1746 and 1748 comparing the growth of electrified and non-electrified plants. In Sect. 3.3, we will encounter contemporary views on how experimentalists can learn about hidden causes through comparative trials. In addition, we shall devise a list of error sources that physicists associated with experimentation of this type, as well as their suggestions for control practices to avoid them. Section 3.4 examines contributions to the controversy published between 1757 and 1789, in the light of the sources of error and control practices discussed in Sect. 3.2. This includes the elaborate but little-known experiments of Runeberg and Köstlin, as well as the contributions of Ingen-Housz and reactions to them. Finally, Sect. 3.5 summarizes the results of this investigation and suggests: concurrent comparative experimentation was the procedure of choice if the process under study is temporally extended and/or cannot be observed twice on the same object—for example, because it is a directed developmental process.

3.2 Comparing the Growth of Electrified and Non-electrified Plants, 1740s

Stephen Demainbray (1710–1782), a French teacher living in Edinburgh in the 1740s, based his claim that electricity improves vegetation on the following experiment:

> On the 20th of December last I had a Myrtle from Mr. Boutcher's Green-House, which since that time I have electrified seventeen times, and allowed the Shrub half an English Pint of Water each fourth Day, which you'll please to observe was kept in the Room the most frequented of my House and consequently the most exposed to the Injuries of the Air, by the Doors and Windows being oftenest opened.
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> This Myrtle hath since by Electrization produced several Shoots, the longest measuring full three Inches; whereas Numbers of the same Kind and Vigour left in the said Green-House have not shewn the least Degree of Increase since that Time.[2]

He compared the growth of several shrubs, one of which he had electrified and the others not. He found that the electrified myrtle, unlike similar non-electrified examples, had produced several new shoots. However, Demainbray was not content with this trial. He set out to perform "a further and more satisfactory Experiment of

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[2] Demainbray 1747a, 3. According to advertisements in the *Caledonian Mercury*, e.g. Nr. 3245, of Tuesday 13 January 1741, 3, William Boutcher Jr. was a Nursery-man and Seeds-man at Comely Garden near the Abbey-hill, Edinburgh.

the same Nature" and promised to communicate "soon to the Publick some Proofs still more evident of the present Hint."[3]

3.2.1 Demainbray Stabilizes Experimental Conditions to Secure his Discovery

Exactly 1 month after announcing his first experiment, Demainbray made good on his promise and sent another letter to *The Caledonian Mercury*. In this second attempt, he made sure to treat the two plants as similarly as possible, except for electrification:

> On the 17th of January last, Mr. Boutcher favoured me with *two Myrtles* of the greatest Equality of Growth, Vigour, &c. he could chuse; these I placed in the same Room, and allowed them each an equal Quantity of Water.
>
> On electrifying one of them, it hath produced several Shoots full three Inches. The other Shrub (which I did not electrify) hath not shewn any Alteration since I first had it.[4]

By treating the electrified and non-electrified myrtles as equally as he could, Demainbray had anticipated what an anonymous commentator on the editorial board of *The Gentleman's Magazine* had criticized about his first report. The commentary, following an excerpt from Demainbray's first letter, reads:

> This account is deficient, and, perhaps, no certain inference can be made in favour of the great increase of the plant by electrising only; because it might be occasioned (at least in part) by its having water; which the plants in the greenhouse (by what appears) had not. (Anonymous 1747a, 81)

Both the commentator and Demainbray acknowledged the importance of the experimental actions in drawing conclusions. The commentator observed that the electricity supply was likely not the only difference between what we can call the test-myrtle and the control plants; while the test-myrtle was brought into a house, the others remained in the greenhouse, with unknown amounts of water supplying them. This inequity matters because water promotes plant growth. Because the setup could not rule out other growth factors, the conclusion that the new shoots resulted from electricity does not safely follow from the experiment.

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[3] Despite the mentioned shortcomings of the experiment, Demainbray's report was reprinted under the titles "An application of electricity towards the improvement of vegetation" in *The Scots Magazine* 9, no. 2: 40; "A remarkable Experiment in Electricity" in *The London Magazine* 16, no. 2: 87; and under "Electricity, effects of, on vegetation" in *The Gentleman's Magazine* 17, no. 2: 80–81.

[4] Demainbray (1747b, 2–3, emphasis in original). This second letter was reprinted as well, e. g. in the *Ipswich Journal* of Friday 21 February 1747: 3, and in *The Scots Magazine* 9, no. 3: 93.

To avoid various sources of experimental error, Demainbray tried to stabilize the conditions. He relied on the resources and expertise of his neighbor, who gave him two myrtles as identical in appearance as possible. Demainbray placed them in the same room and gave them the same amount of water. With these control practices he was confident that electrification was the only difference between the test plant and the control, and he was thus more certain than before that the growth came from electricity. Satisfied with his work, Demainbray concluded his second letter as follows:

> As the Business of my School does not allow me the necessary Time of attending this *now certain* Discovery, I submit it to those whose Leisure will permit them to pursue a Hint which may hereafter be highly beneficial to Society.[5]

Thus, for Demainbray, the certainty of the conclusions depended on the details of the experimental design and its implementation. The commentator of *The Gentleman's Magazine* shared this understanding and applauded the adjustments.[6]

To determine the effect of his intervention on the myrtle, Demainbray counted the number of new shoots and measured their length. On the other hand, the brief report does not mention how he electrified the plant. From his letters of November 1746, we know that Demainbray had become familiar with the latest literature on electricity, in the work of Desaguliers (1742), Hausen (1746), and Bose (1744). His former teacher John Theophilus Desaguliers (1683–1744) had resumed his electrical experiments in the late 1730s, and Demainbray witnessed some of them.[7] After moving to Edinburgh in 1740, he had "all the apparatus that Gravesand [sic] describes made" and reproduced "all the experiments of Hawksbee, and all those which are described in the Brochure by which

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[5] Emphasis in original. This was Demainbray's last publication on the subject. For his further career, see footnote 37.

[6] A follow-up comment in the same issue reveals that the "same ingenious Scotchman" had repeated his "experiment concerning the effect of Electricity on Vegetation" with "two Myrtles of equal growth, both which are expressly said to have been supplied with an equal quantity of water"; and that the electrified plant "produced several shoots 3 Inches long, and the other remained without alteration" (Anonymous 1747b, 102).

[7] Demainbray told Erasmus King in his letter of 8 November 1746: "I had some general Joys of Electricity before my Arrival in Scotland. By the small Number of Trials I had seen Doct. Desaguliers make of it." Records assembled by the State Paper Office, SP 36/89/1, folios 71–72. National Archives, Kew. For Desaguliers' electrical experiments of the late 1730s, see Desaguliers (1739). Demainbray and his wife came to Edinburgh in 1740 to run "a Boarding-School for Ladies in Bishop's Land." In his letter to Abraham de Moivre, 8 November 1746, Demainbray proudly introduced himself as "a pupil of the late Doct. Desaguliers." Records assembled by the State Paper Office, SP 36/89/1, folios 65–66. National Archives, Kew. Desaguliers instructed Demainbray in mathematics and natural philosophy in the 1720s (Anonymous 1795, 317–318). At the age of seventeen, Demainbray allegedly left London to study in Leyden. Macray (1888, 330), however, noted that Demainbray's name cannot be found in the Leyden *Album Studiosorum*.

M. Desaguliers won the prize in Bordeaux." He further claimed to "have by the means of Hawksbee's Globe set fire to spirits of Wine."[8]

None of the many authors who later mentioned Demainbray's experiments criticized their design or his lack of detail about how he conducted them. This, however, tells us less about contemporary standards for good experiments than it does about the reception of Demainbray's original reports: hardly anyone seems to have read them (see footnote 15). The only author who explicitly responded to them was Stephen Hales (1677–1761). *The Gentleman's Magazine* for April 1747 states that "[n]otwithstanding what has been inserted of the efficacy of electricity on plants" in an earlier issue, "the Rev. Dr Hales finds his suspicion, that electricity will not promote vegetation, confirmed by several experiments made by Mr King, at his experiment room, near the king's Meuse, London, and by Mr Yeoman at Northampton." We know even less about these trials than we do about Demainbray's. The experiments of Erasmus King, a lecturer in natural and experimental philosophy, and Thomas Yeoman, an engineer, did not become part of the accepted body of studies on vegetation.[9] In contrast, news (though not the exact wording) of Demainbray's experiments reached Jean Jallabert in Geneva and Jean-Antoine Nollet, the dean of French electricians of the time (Heilbron 1979, 254), in Paris.

3.2.2 *Nollet's, Jallabert's, and Menon's Comparative Experiments*

Two months after Demainbray's second letter appeared in print, another experimental philosopher began a similar study. Jean Jallabert (1712–1768), then professor of experimental philosophy and mathematics as well as curator of the Geneva public library, spent "a part of the month of April and the whole month of May in regularly electrifying for 1 or 2 h, each day, various plants; among others, a yellow wallflower placed in a box full of earth."[10] Like Demainbray, he attended to how the electrified

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[8] Demainbray to de Moivre, 8 November 1746, loc. cit. In his letter to King, 8 November 1747, loc cit., Demainbray specified that he "made a Wheel with a Treadle to whirl a Globe" and thereby "fired Spirits of Wine etc." In vain, he had "attempted Beatification," an experiment described by Bose (1744). Demainbray saw "the wavering Fire round the Feet but no more." For a sketch by Père Chabrol of the electrical machine used by Demainbray at Bordeaux in 1753, see Fig. 2 in Morton and Wess (1995, 174). The device is similar to the electric globe machine (object nr. 1927-1186 Pt1) in the King George III collection at the Science Museum. The object consists of a glass sphere that can be rotated around a vertical axis by means of a gear inside the brass casing.

[9] The trials were most likely conducted after Hales wrote to the Rev. Mr. Westly Hall on February 23, 1747, see Hales (1748). This letter, which was read before the Royal Society 16 months later, contains no information about the two experiments mentioned in the *Gentleman's Magazine* (Anonymous 1747c, 200). For King and his cooperation with Hales, see Appleby (1990).

[10] Jallabert (1748, 80). The French original reads: "Une partie du mois d'Avril, & tout le mois de Mai, furent employés à électriser régulièrement une ou deux heures, chaque jour, diverses plantes; entr' autres, un giroflier jaune ou violier placé dans une caisse pleine de terre."

and non-electrified plants grew. He observed that "[a]ll these plants increased considerably in stem & branches; & in particular the wallflower made beautiful sprays & flowered."[11] But in contrast to Demainbray, Jallabert found the difference between the electrified and non-electrified plants too small to take as an effect of electricity:

> [T]he progress of these electrified plants, compared with that of other plants of the same age, raw in vases full of the same soil &c., did not seem to me to be sufficiently considerable to dare to conclude that the material of electricity was capable of accelerating vegetation.[12]

According to Jallabert, his first experiments gave no evidence that electricity accelerated vegetation. Minor differences did not warrant such a conclusion.[13]

Just as Jallabert (1748, 81) was about to repeat these experiments in the fall of 1747, he heard that myrtles electrified in Edinburgh had grown sprays three inches long within a few days. This growth occurred in a season when other myrtles had not yet budded. Shortly afterwards, his friend Jean-Antoine Nollet (1700–1770) informed him of "some very curious experiments" he had made with mustard seeds. Nollet had also heard about the electrified plants:

> I learned that in England, plants and shrubs had been electrified and felt in such a way as to make people believe that electric virtue promotes or hastens vegetation; but as no details of these experiments have come down to us, I was unable to draw any advantage from them, other than to embolden myself in my intention to devote myself to these tests.[14]

Nollet reported hearing "that two myrtles had been electrified, and that they had grown a few buds; but I don't know what was done to be entitled to attribute this effect to electric virtue."[15] Thus, Nollet, like Demainbray and his commentator,

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[11] Jallabert (1748, 81): "Toutes ces plantes augmentèrent considérablement en tige & en branches; & en particulier le giroflier fit de très beaux jets & fleurit."

[12] Jallabert (1748, 81): "Cependant les progrès de ces plantes électrisées, comparés à ceux d'autres plantes de même âge, crues dans des vases pleins de la même terre &c. ne me parurent pas assés considérables pour oser en conclure que la matière de l'électricité étoit capable d'accélérer la végétation."

[13] Jallabert seems to have had a quantitative expectation of how big the difference between the test plants and the controls would have to be to warrant attributing the effect to electricity. But he did not specify what would have been a sufficient difference.

[14] Nollet (1749, 356): "[…] j'appris qu'en Angleterre on avoit électrisé des plantes & des arbustes, qui s'en étoient ressenti de maniéré à faire croire que la vertu électrique favorise ou hâte la végétation; mais comme il ne nous est venu aucun détail de ces experiences, je n'ai pu en tirer d'autre avantage, que celui de m'enhardir dans le dessein où j'étois de me livrer à ces épreuves."

[15] Nollet (1752, 172): "J'ai oui dire depuis, qu'on avait électrisé deux myrthes, & qu'ils avoient poussé quelques boutons; mais j'ignore ce que l'on a fait pour être en droit d'attribuer cet effet à la vertu électrique." In a footnote he added that he had since—that is, since reading the Mémoire before the Académie Royale in April 1748—learned that this experiment was made "in Edinburgh by Mr. Mambray, that two myrtles having been electrified during the whole month of October 1746, grew at the end small branches & buds; which similar non-electrified shrubs did not." This footnote probably served as the template for most of the later references to Demainbray's work. It looks as though few of the naturalists who referred to Demainbray's experiments read his original reports. Soon the erroneous view crept in that the experiment (singular) was carried out in *October*; that *two* myrtles were electrified at the same time, and that they began to *bloom*. Priestley (1767, 135), for instance, wrote: "Mr. Maimbray at Edinburgh electrified two myrtle trees, during the

distinguished several dimensions of experimentation. The physical activities for conducting the experiment were one, and the mental activities for interpreting the results were another. Demainbray's former teacher Desaguliers (1727, 264) referred to this distinction when he noted that "a Mechanical Hand, and a Mathematical Head are the necessary Qualifications of an Experimental Philosopher: The first alone may enable a Man to make a great many Experiments, but not to judge of them."

To judge whether growth-promoting effects could be attributed to electricity, Nollet (1748, 189), like Demainbray and Jallabert, compared the growth of electrified and non-electrified plants. He otherwise treated them as equally as possible:

> I took two Garden-Pots, filled with the same Earth, and sowed with the same Seeds; I kept them constantly in the same Place, and took the same Care of them, except that one of the two was electrified for fifteen Days running, for two or three, and sometimes four Hours a Day. This Pot always shewed its Seeds raised two or three Days sooner than the other, a greater Number of Shoots, and those longer, in a given Time.

This result made him "believe, that the electrical Virtue helps to open and display the Germs, and facilitates the Growth of Plants." However, Nollet said that he advanced this "only as a Conjecture, which deserves further Confirmation" (189–190). His letter was read at a meeting of the Royal Society of London in February 1748. Two months later, a more detailed account was read at the Académie Royale des Sciences at Paris. From this mémoire, we learn that on Monday, October 9, 1747, he took two similar tin bowls filled with the same soil, and sowed in each an equal quantity of mustard seed taken from the same packet.[16] After leaving them in the same place for two days, Nollet and his collaborators electrified one of the bowls (Fig. 3.1):

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whole month of October 1746; when they put forth small branches and blossoms sooner than other shrubs of the same kind, which had not been electrified." Anonymous (1752, 75–76) reads: "This acceleration in plants was tested in Edinburgh, by Mr. Mambrai. Two myrtles having been electrified during the whole month of October 1746, grew at the end small branches & buds, which similar non-electrified shrubs did not." According to Sigaud de Fonds (1771, 372), the conclusion that electricity "must hasten the effects of vegetation […] was confirmed by a number of observations made with care by several famous Physicists. Doctor Mimbray was one of the first who applied himself to this research. As early as October of the year 1746, he found that two electrified myrtles grew small branches and buds, which similar non-electrified shrubs did not." De la Cepède (1781, 175) wrote: "Mr. Mambrai having electrified two myrtles in Edinburgh during the month of October 1746, saw them grow small branches and put on buds, which was not the case with other myrtles to which no attempt was made to give a new quantity of fluid." Bertholon (1783, 152) wrote: "Dr. Mainbrai electrified two myrtles in Edinburgh, during the whole month of October 1746; they grew small branches and buds at the end, which similar non-electrified shrubs did not. The shoots they gave on this occasion were even three inches long, which is astonishing in a season when the other trees were not yet budding." See also Runeberg (1757, 15), Gardini (1784, 15), Duvarnier (1786, 94), and Rozieres (1791a, b, 351). Miller (1803, 23–24) wrote: "The application of electricity to growing vegetables was first made by Mr. Maimbray, of Edinburgh, who found that, in certain cases, it expedited the progress of vegetation." As the great exception, Anonymous (1795, 317) cited Demainbray's first letter published in *The Caledonian Mercury*.

[16] Nollet (1752, 173): "Le 9 Octobre 1747, je fis remplir de la même terre deux petites jattes d'étain toutes semblables; je semai dans chacune une égale quantité de graine de moutarde prise au même

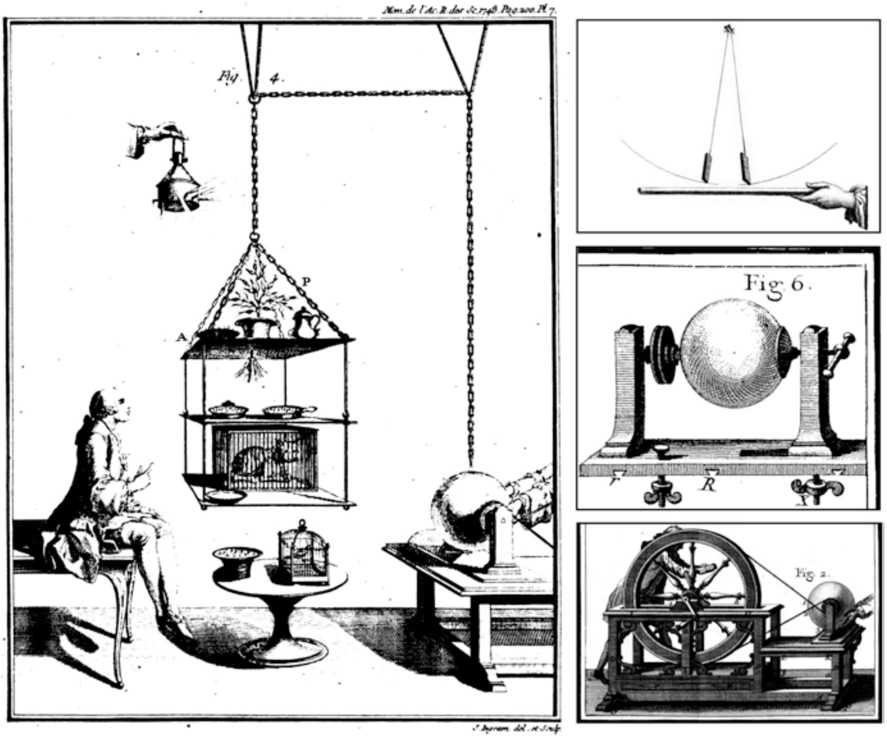

Fig. 3.1 Left: Plate 7 in Nollet (1752, 200). Nollet placed objects to be electrified (e.g. the mustard seeds in bowl A) in a cage made of three large sheets of cloth, held at the four corners by iron mounts. The cage was suspended by two metal rings on a large silk cord stretched horizontally. An iron chain conducted the electricity, generated by rubbing a glass globe, to the cage. Two "strong men," replaced by two others from time to time, turned this ball while a third person rubbed it with their hands (316–317). Right, top: Fig. 4 from plate 2 in Nollet (1749, after 162), showing an electrometer. Right, middle and bottom: Plate 1 from Nollet (1765, after 24) showing the glass globe and the apparatus by which it is turned

> I placed one of the bowls marked with the letter A in the tin cage where it was electrified for ten hours, namely, in the morning from seven o'clock until noon, and in the evening from three o'clock until eight. During this time, the other bowl was kept apart, but in the same room, where the temperature was quite uniformly 15 ½ degrees according to M. de Reaumur's thermometer.[17]

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paquet; je les laissai deux jours dans le même lieu, sans y faire autre chose que les arroser & les exposer aux rayons du soleil, depuis environ dix heures du matin jusques à trois heures après midi."

[17] Nollet (1752, 173): "[…] je plaçais une des jattes marquette de la lettre A dans la cage de tôle où elle fut électrisée pendant dix heures, savoir, le matin depuis sept heures jusqu'à midi, & le soir depuis trois heures jusqu'à huit. Pendant tout ce temps-là, l'autre jatte était à l'écart, mais dans la même chambre, où la température était assez uniformément de 15 degrés $^1/_2$ au thermomètre de M. de Reaumur."

The following day the bowls were exposed to the sun and watered equally. When they entered the house in the evening, Nollet still did not see anything. But on October 13 at nine o'clock in the morning, he "saw three seeds in the electrified bowl, whose stems were three lines above the ground" while "the non-electrified bowl had none." Nollet and his assistants continued the experiment: "We took the same care of one & the other as the previous day, & in the evening electrified the one intended for this test for three hours."[18] The next morning, the difference between the plants in the two bowls was even more marked: "the electrified bowl had nine stalks out of ground, each of which was seven to eight lines long, & the other one had still absolutely nothing raised; but in the evening, I saw one in this one starting to show."[19] That afternoon the first bowl was electrified again for 5 h. Nollet summarized the rest of his experiment as follows:

> Until the 19th of October, I cultivated these two small portions of seeded land similarly, continuing to electrify one, & always the same one, for several hours every day; […] after eight days of experiments, the electrified seeds were all raised & had stems from fifteen to sixteen lines high, while there were barely two or three of the others out of the ground with stems of three or four lines at most.[20]

With this result, Nollet initially feared that he had not adequately controlled for all relevant experimental conditions: "This difference was so marked, that I was tempted to attribute it to some accident that escaped my knowledge." He first imagined the accident to be a factor inhibiting plant growth in the non-electrified bowl. But a few days later, when he noticed that all the seeds in the control bowl had sprouted, he began to "believe with some confidence, that electricity had truly accelerated the vegetation & growth of the others."[21] Nollet emphasized that he "only came to this conclusion after several repeated tests on different seeds, with

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[18] Nollet (1752, 173–174): "Le 12, ces deux jattes furent exposées ensemble au soleil & arrosées également; on les rentra de bonne heure le soir, & je n'y aperçus encore rien de levé. Le 13 à neuf heures du matin, je vis dans la jatte électrisée, les trois graines levées dont les tiges étaient de trois lignes hors de terre; la jatte non électrisée n'en avait aucune. On eut de l'une & de l'autre le même soin que le jour précédent, & l'on électrisa le soir pendant trois heures celle qui était destinée à cette épreuve."

[19] Nollet (1752, 174): "Le 14 au matin, la jatte électrisée avait neuf tiges hors de terre, dont chacune était longe de sept à huit lignes, & l'autre n'avait encore absolument rien de levé; mais le soir, j'en aperçus une dans celle-ci qui commençoit à se montrer: la première fut encore électrisée ce jour-là pendant cinq heures de l'après-midi."

[20] Nollet (1752, 174): "Jusqu'au 19 d'Octobre, je cultivai également ces deux petites portions de terre ensemencées, en continuant d'en électriser toûjours une, & toûjours la même, pendant plusieurs heures tous les jours; & qu'au bout de ce terme, c'est-à-dire, après huit jours d'expériences, les graines électrisées étaient toutes levées & avoient des tiges de quinze à seize lignes de hauteur, tandis qu'il y en avait à peine deux ou trois des autres hors de terre avec des tiges de trois ou quatre lignes au plus."

[21] Nollet (1752, 174): "Cette différence était si marquée, que je fus tenté de l'attribuer à quelque accident qui aurait échappé à ma connoissance: mais au retour d'un petit voyage que je fus obligé de faire, je trouvai toutes les graines levées dans la jatte qui n'avait pas été électrisée; & je commence à croire avec quelque confiance, que l'electrité avait accéléré véritablement la végétation & l'accroissement des autres."

more or less similar results: I almost always saw a considerable difference between electrified seeds and those that were not; the former sprouted more quickly, in greater numbers in a given time, and grew more rapidly."[22]

Motivated by the results of Demainbray and Nollet, Jallabert returned to his experiments on the influence of electricity on vegetation in the last days of 1747. He put several daffodil, hyacinth, and narcissus bulbs on water-filled carafes. Most of the plants had already sprouted roots and leaves, and some even had advanced flower buds. Jallabert measured the length of their roots, stems, and leaves, and then placed the carafes on resin cakes. This last measure was a "precaution or preparation" to ensure that the carafes were ready to receive electricity.[23] To electrify the plants he used archal wires that, starting from an electrified bar, plunged into the water of each carafe. Thus, from December 18 to 30, except for December 24 and 25, he electrified the bulbs for 8 to 9 h a day. Like Nollet, Jallabert had a thermometer from Mr. de Réaumur in his cabinet, which throughout the tests stayed between the 8th and 10th degree above freezing.[24] This time the difference between the test and control plants was marked:

> The difference in the progress of the electrified onions, compared to that of other onions of the same species equally advanced & situated & treated the same except for electrification, was very noticeable. The electrified onions increased more in leaf, & in stem; their leaves spread more; & their flowers bloomed more promptly.[25]

According to Jallabert, this experiment suggested that electricity hastens plant growth. From another trial he argued that electricity also promotes transpiration: electrified daffodil bulbs lost more weight than non-electrified ones.[26] In a third experiment, Jallabert put cress and mustard seeds on the outer surface of a vessel. He reported that at "the end of the second day of 8 to 9 h of electricity each day,

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[22] Nollet (1752, 174–175): "Quoique cela parût assez clairement indiqué par l'expérience que je viens de citer, je ne me suis rendu à cette conséquence qu'après plusieurs épreuves réitérées sur différentes graines, & suivies de résultats à peu près semblables: j'ai presque toûjours vû une différence assez considérable entre les semences électrisées & celles qui ne l'étaient pas; les premières ont levé plus promptement, en plus grand nombre dans un temps donné, & leur accroissement s'est fait plus vîte."

[23] Nollet (1752, 67–68): "Il est essentiel d'indiquer quelques précautions ou préparations nécessaires pour les [corps] mettre en état de recevoir la vertu électrique. Us doivent être isolés de tout autre corps non électrique. On les en sépare, soit en les suspendant à des cordons de soye exempts de toute humidité; ou, en les posant sur des gateaux de résine, sur des caisses pleines de poix, sur des guéridons de verre séchés exactement." Nollet used this second form of insulation by hanging the electrified cage from silk cords.

[24] Jallabert (1748, 82–83): "Depuis le 18 jusq'au 30 Décembre, excepté le 24 & le 25, j'électrisai de cette maniéré plusieurs oignons 8 à 9 heures chaque jour; & pendant toute cette opération, un thermomètre de Mr. de Reaumur fut, dans mon cabinet, entré le 8^{me} & 10^{me} degré au-dessus de la congélation."

[25] Jallabert (1748, 83): "La différence du progrès des oignons électrisés, comparé à celui d'autres oignons de même espèce également avancés & situés & traités de même à l'électrisation près, a été très sensible. Les oignons électrisés ont plus augmenté en feuilles, & en tige; leurs feuilles se sont étendues davantage; & leurs fleurs se sont épanouïes plus promptement."

[26] See Jallabert (1748, 83–85).

several mustard sprouts had grown. And, without electricity, by the 4th day, only a few had sprouted. The stems of the electrified sprouts rose, and their first two small leaves opened much more rapidly."[27] Later in his monograph, Jallabert argued that the results compelled a certain conclusion: electricity increases the speed of moving fluids. "Experience demonstrates it, and this is enough to account for the rapid vegetation of electrified plants."[28]

The basic design of the experiments of Demainbray, Nollet, and Jallabert was the same: they compared the growth of electrified plants with that of non-electrified ones. They intended to attribute differences in plant growth to the action of electricity. At the same time, another naturalist conducting analogous experiments was the priest and Doctor of Theology at the University of Angers, François Menon (?–1749). On December 2, 1747, Menon wrote René Antoine Ferchault de Réaumur, Nollet's former teacher and collaborator:

> I sowed and electrified lettuce seeds that I watered before electrifying them. They sprouted three days earlier than those I had sown at the same time and which I watered with the same quantity of water and at the same times. I put in the ground some ranunculus and I tried to plant them equally. Eight days after I electrified them for an hour each day, they are as advanced as the ones that are a month old, if they continue to prosper, I'll have flowers in January at the latest.[29]

Giovanni Battista Beccaria (1753, 125), professor of physics at the University of Turin, summarized the situation as follows: at about the same time, Demainbray in Edinburgh, Nollet in Paris, Menon in Angers, Bose in Wittenberg, and Jallabert in Geneva had been experimenting on the same subject. In Beccaria's view, these researchers, "with their different experiences, had known the same truth."[30] In fact, we have seen that their studies differed in many ways. Not only did they electrify the plants differently, but they also

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[27] Jallabert (1748, 85): "De la semence de cresson, & de moutarde, appliquée le 26 Décembre à la surface extérieure de ce vase de terre poreuse […] a germé plus promptement sur ce vase électrisé, que lors qu'il ne l'est pas. A la fin du 2^{d} jour d'une électricité de 8 à 9 heures chaque jour, plusieurs germes de moutarde avoîent poussé. Et, sans électricité, à peine le 4^{me} jour en parutil quelques-uns. Les tiges des germes électrisés s'élevèrent, & leurs deux prémiéres petites feuilles s'épanouïrent aussi beaucoup plus promptement."

[28] Jallabert (1748, 196): "[…] [on est] forcé de convenir que l'électricité augmente la vitesse des fluides qui se meuvent déja. […] l'expérience le démontre; & cela suffit pour rendre raison de la promte végétation des plantes electrisées."

[29] Menon to Réaumur, 2 December 1747, Fonds Réaumur 69 J, 67/24, Académie des Sciences, Paris. The French original reads: "J'ay semé et electrisé des graines de Laitue que y'arrosois avant de les electriser. Elles ont levé trois jours plutôt que celles que j'avoit semé en même temps et que j'arrosois avec la même quantité d'eau et aux mêmes heures. J'ay mis en terre des renoncules et j'ay essayé de les rendres égales. Celles qui y etoient depuis plus d'une mois et qui etoient desja bien avancées il y a huit jours que j'ay les electrisé une heure chaque jour, et je suis prêt de les voir aussi avancées que Celles d'un mois, si elles continuent a prosper j'auray des fleurs dan le mois de janvier au plûstard." For the last 18 months of his life, Menon worked as demonstrator of Réaumur's cabinet (Birembaut 1958, 167).

[30] In January 1748, Nollet received a letter from Georg Matthias Bose (1710–1761) informing him that he had electrified several species of plants and shrubs, and that the vegetation appeared to him to be constantly accelerating (Nollet 1749, 356–357).

used different types of plants at different stages of development and focused on different aspects of growth.[31] In one respect they even came to different conclusions: unlike Jallabert, Nollet did not find thicker stems in the electrified plants than in the non-electrified ones. Rather, it seemed to him "that the grains whose germination and growth had been accelerated by electricity had grown smaller and softer stems than those that had sprouted on their own." But he was cautious and "would not dare to say for sure", as he had "not had a large enough number of experiments to be sure."[32]

Finally, the experimenters also had different motivations for working on electricity and plants. Demainbray realized that electricity was the "modish Topick of all Europe" and "the subject of conversation for all the Savants, half-Savants and Ignorants." He therefore "endeavoured to strike out some few Things on this Subject" himself.[33] Menon started electrifying plants because he learned that Mr. Bose (1747) had written a treatise entitled *Tentamina Electrica Tandem Aliqvando Hydravlicae Chymiae Et Vegetabilibvs Vtilia*.[34] Jallabert's and Nollet's experiments, in turn, grew out of their extensive study of electrical phenomena. Both had studied electrical effects in inanimate bodies and wondered whether electricity affected organized, living beings.[35] After noticing that electricity spreads easily in plants, Jallabert (1748, 80) wanted to investigate whether it helped or hindered development. Nollet (1749, 363), on the other hand, found that electricity accelerated the flow of liquids through narrow channels, so he suspected that it had some effect on plant sap. Knowing about this possible influence seemed useful, "especially now that so many people have electrified themselves, & that anyone can easily do so."[36]

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[31] Demainbray worked with growing myrtle shrubs, Nollet and Menon (and Jallabert, in his last experiment) worked with mustard, cress, and lettuce seeds, while Jallabert used bulbous plants. Some of those had already formed roots, stems, and leaves. Demainbray reported the number of newly formed shoots and their length, while Jallabert initially assessed whether the plants increased in stem, branches, and twigs, and whether they flowered. Later, he also recorded how extensively the leaves spread and how quickly they flowered, how much the plants weighed, how many of them sprouted in a given time, and how quickly they emerged and opened their first leaves. Nollet again measured the number and height of seedlings rising from the ground after a certain period of time. He and Menon paid attention to how quickly the plants germinated and sprouted.

[32] Nollet (1752, 175, footnote): "Il m'a semblé que les grains don't l'électricité avoit accéléré la germination & l'accroissement, avoient poussé des tiges plus menues & plus foibles que celles qui avoient levé d'elles-mêmes; mais je n'oserois l'assurer, n'ayant point eu un assez grand nombre d'éxpériences pour m'en rendre bien certain."

[33] Demainbray's letters to Erasmus King and Abraham de Moivre, both written on November 8, 1746. Records assembled by the State Paper Office, SP 36/89/1, folios 71–72 and 65–66. National Archives, Kew. Heilbron (1979, 261) agrees with Demainbray's assessment that in the second half of the 1740s, nothing was more fashionable than electricity.

[34] Menon to Réaumur, 2 December 1747, Fonds Réaumur 69J, 67/24, Académie des Sciences, Paris.

[35] Nollet had been introduced to this field by his teacher Charles François de Cisternay du Fay (Benguigui 1984, 11).

[36] Nollet (1752, 172): "[…] l'électricité entraîne les liquides qui sont obligés de passer par des canaux fort étroits, je commencai à croire que cette vertu […] pourroit avoir quelque effet sur la sève des végétaux […]. Soit qu'on en dût craindre de mauvaises suites, soit qu'on en dût attendre

3.3 The Purpose of Comparative Experiments and the Need for Control

We have seen that many natural philosophers from across Europe conducted comparative experiments to determine whether electricity promotes plant growth. This section explores their broader attitudes toward how nature should be investigated. Demainbray, Nollet, and Jallabert were among those offering lecture series on Experimental Philosophy, and they advocated the approach of investigating nature by experiment.[37] In what follows, we see that physicists associated experimentation with finding causes. They also used several strategies to avoid drawing erroneous conclusions from comparative experiments.

3.3.1 Discovering Causes from their Effects

In his inaugural lecture as professor of experimental philosophy and mathematics, delivered at the University of Geneva in 1739, Jallabert placed himself in a tradition with Nollet and Demainbray's teacher Desaguliers. A central idea of this tradition, he said, is that good experiments can discover the works of nature:

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de bonnes, il me paroissoit également utile de le savoir, présentement sur-tout, que beaucoup de personnes se sont électriser, & que tout le monde le peut aisément." Jallabert (1748, 236) followed this reasoning: "The acceleration of the course of the water […] through the capillary pipes by the action of the electric matter & the phenomena which electrified plants give are a strong prejudice that the electric fluid increases the movement of the liquors which the plants contain & that it consequently contributes to pushing & introducing into their extremities the juices necessary to develop them, extend them & increase them."

[37] Jallabert taught as professor of experimental philosophy and mathematics at the University of Geneva. Nollet published his *Leçons de la philosophie expérimentale* in six volumes and read lessons in experimental philosophy to the Duke of Savoy, the Duke of Penthievre, and the Duke of Chartres (Nollet 1743, xiii–xiv). He also reported that several Colleges and Oratories as well as the University of Rheims adopted his plan of introducing experimental proofs into their public exercises. In 1744 and 1745, he taught physics to Prince Louis, son of King Louis XV, and his wife Marie-Thérese at Versailles. Demainbray started his career as an itinerant lecturer in natural philosophy in the winter of 1748/9. In *The Caledonian Mercury* of June 28, 1748, no. 4324, he "proposeth to give a Course of Experimental Philosophy, consisting of 51 Lectures, and to begin on Monday 7th of November next." Besides all the experiments of his former teacher John Theophilus Desaguliers, he promised "some additional in Mechanicks, Hydrostaticks, Pneumaticks, Opticks, and Astronomy: The Properties of Magnetism will be examined, and the Doctrine of Fire, with the Nature of Electricity attempted." Gentlemen and ladies were asked to pay two guineas for the whole season, or one shilling for a single lecture. After lecturing in Edinburgh, Demainbray travelled through the north of England before moving on to Ireland and France (Morton 1990). Late in 1754 he returned to London, where he began to lecture in 1755. During that year, he gave a course of lectures to the Prince of Wales, the future King George III, and Prince Edward (Morton 1990, 420). By the end of the 1750s he gave up lecturing and became an official of the Excise, and was later superintendent of the observatory built for King George III at Richmond. He was also librarian for Queen Charlotte and instructed her in experimental philosophy and natural history (Rigaud 1882, 281).

> You will see there [i.e. in the most famous lyceums of Europe] how successfully the famous Desaguliers's Gravesande, Muschenbroek, and the expert Nollet, who is closely associated with me, teach physics, while whatever they bring forward to the medium, they confirm with their own experiments; […] [their disciples] want all the Works of Nature to be discovered by certain experiment.[38]

Nine years later, he spoke of what it takes to get a glimpse of "the mechanism by which Nature operates." According to Jallabert, this could only be achieved "by gathering a great number of facts, and by considering them in all their circumstances."[39] Thus, in his *Expériences sur l'électricité*, he set out to "describe the main electrical phenomena accurately, & to arrange them in an order that would facilitate the deduction of the resulting consequences."[40] From the consequences, he hoped to identify the causes and then develop a theory: "For such is, & especially in Physics, the slow but necessary gradation of our knowledge; it is only by the consequences that we can go back to the causes, & arrive insensibly at a theory."[41]

For Demainbray's teacher, too, discovering causes was also a central goal of scientific activity. Desaguliers (1745, iii) wrote that the "business of science" was to "contemplate the Works of GOD, to discover Causes from their Effects, and make Art and Nature subservient to the Necessities of Life, by a Skill in joining proper Causes to produce the most useful Effects." Nollet, for his part, warned against confusing effect with cause, adding that it is easier to recognize the former than the latter.[42]

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[38] Jallabert (1740, 8): "Perlustra, quaeso, celebriora Europae Lyceae: Videbis & ibi quam feliciter Physicam doceant Celeberrimi *Desaguliers, 's Gravesande, Muschenbroek*, mihique conjunctissimus peritissimus *Nollet*, dum quaecunque in medium proferunt, suis experimentis confirmant; videbis & ibi, quantos faciant profectus in perscrutatione rerum naturalium beati tantorum virorum discipuli, dum Magistrorum exemplo omnia Naturae Opera certo experimento comperta esse volunt." In 1734, Nollet visited Desaguliers in England as well as 's Gravesande and Musschenbroek in the Netherlands (Anstey and Vanzo 2016). Later, Nollet (1770, xiii) referred to the textbooks of both 's Gravesande and Desaguliers. Morton (1990, 413) and Schofield (1970, 81) describe Desaguliers as the doyen of lecturers in natural philosophy in England.

[39] Jallabert (1740, v): "Ce n'est qu'en rassemblant un grand nombre de faits, & en les considérant dans toutes leurs circonstances, qu'on peut entrevoir le mécanisme par lequel la Nature opére." Home (1756, 7): "The operations of bodies are to be accounted for only from their known qualities ascertained by experiment. Reasoning on any other plan, can never certainly lead to truth."

[40] In collecting, organizing, and reflecting on experiments and observations, Jallabert (who described the work of "Nature Historians") thus claimed to be doing what, according to Anstey and Vanzo (2016), corresponded to central activities of the Baconian method of natural history.

[41] Jallabert (1748, iii–iv): "Je ne me suis proposé que de décrire avec exactitude les principaux phénomènes électriques, & de les ranger dans un ordre qui facilitât la déduction des conséquences qui en résultent. Car telle est, & surtout en Physique, la lente mais nécessaire gradation de nos connoissances; ce n'est que par les conséquences que nous pouvons remonteraux causes, & arriver insensiblement à une théorie." He thus endeavored "to describe the main electrical phenomena accurately, and to arrange them in an order that facilitates the deduction of the resulting consequences."

[42] According to Nollet (1743, xxxv), the effect could be known by the least educated peasant, while the cause would not be known by the most learned philosopher.

Now the goal of the experiments described in Sect. 3.1 was not so much to discover unknown causes for mysterious phenomena. Rather, the physicists wanted to decide whether any extraordinary or diminished plant growth could be “attributed” to the action of electricity. To decide this question, they conducted comparative experiments: they performed sets of simultaneous parallel experiments, where one set showed the unperturbed course of nature, or the plant’s normal growth. The other set was used to determine how changing one variable, electricity, affected the growth outcome.

The use of comparative experiments to test assumptions about causal relations seems to have been a widespread practice: neither Demainbray nor the anonymous commentator felt the need to introduce or defend the procedure in detail. Neither did Nollet, Jallabert, or any of the other experimentalists discussed here. The only exception is M. d’Ormoy (1789), who sowed an equal number of electrified and non-electrified seeds and assured that everything else “was completely equal, as should always be observed in experiments of this nature.”[43] The others did not comment on the design of their trials, perhaps because, like William Marshall (1745–1818), they thought that “the Mode of making Experiments—requires little explanation” (1779, introduction).[44] To make a comparative experiment, according to Marshall, one needs to observe “an identity of *place, time, element*, and *process*, […] in every particular, excepting only the *intended difference* which constitutes the Experiment” (1779, introduction, emphasis in original). Half a century later, the botanist Augustin-Pyrame de Candolle (1778–1841) stated that to perform an experiment with any certainty means to do so in a comparative manner (1832, 1130). He explained that

> a test proves nothing, as long as another comparative test is not placed next to it […]. We must place the beings we want to study comparatively in all similar circumstances, except for one, which we will establish as positive in one case, negative in the other. Then we can conclude on one point.[45]

Marshall and de Candolle agreed that the processes to be compared should occur at the same time and place, and should differ in only one factor. The experimentalists mentioned earlier tried to follow this rule. Apart from electrifying the test plants, they treated their tests and controls as equally as possible. In line with Schickore’s (Chap. 1, this volume) narrower notion of experimental control, they considered the

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[43] D’Ormoy (1789, 162): “[E]n un mot, tout a été entièrement égal, ce qu’on doit toujours observer dans les expériences de cette nature.”

[44] Schickore (2017), Bertoloni Meli (2009), and Boring (1954) have identified a few experimentalists who applied this strategy in the seventeenth century.

[45] De Candolle (1832, 1535): “[U]n essai ne prouve rien, tant qu’on ne place pas à côté de lui un autre essai comparatif; je m’explique: une expérience ne peut donner qu’un seul résultat. On doit placer les êtres qu’on veut étudier comparativement dans toutes les circonstances semblables, sauf une seule, qu’on établira positive dans l’un des cas, négative dans l’autre. Alors on pourra conclure sur un point.” See also de Candolle (1832, 1130): “[J]e demanderdai, […] si l’expérience a jamais été faite avec quelque degré de certitude, c’est-à-dire d’une manière comparative.”

comparative experimental design a prerequisite for making “certain” inferences or discoveries, or for concluding “safely” that electricity promotes vegetation.

3.3.2 Potential Errors and Strategies to Avoid them

In their comparative experiments, the physicists studying electricity and vegetation used what Schickore (Chap. 1, this volume) calls control practices in the broader sense: they kept the two experimental settings stable to rule out as much as possible that the differences in growth were due to factors other than electrification. Nollet and Jallabert not only implemented these strategies in their work, but also discussed them in writing. Nollet (1749, 104) reminded his readers that “we can be fooled by a fact, because we will have changed the circumstances without knowing it, or without paying attention to it.” He thus urged that “we must have great regard for these circumstances” known to influence the result, “since they can be an occasion of error, for anyone who neglects to pay attention to them” (127). Jallabert (1748, ix) explained that electrical experiments were particularly susceptible to minute changes in setup—their outcomes can vary infinitely due to slight differences in performance or external circumstances. Nollet’s and Jallabert’s comments are consistent with Marshall’s (1779) more explicit methodological discussion of comparative experiments. He stressed that experimenters have to act prudently and accurately, guarding against any dissimilarity of factors that should be kept constant such as the soil or the seeds.[46]

In the following, we focus on individual elements of experimentation and how experimenters tried to control them. In this way we can identify differences between the methodological views of individual authors and thus have a template for later discussion of further developments.

3.3.2.1 Stabilize (and Monitor) Experimental Conditions

The physicists kept their experimental plants under similar conditions: they moved them together from room to room, to expose them to the same air and equal amounts of water. In addition to ensuring that the control and test plants were at similar temperatures, Nollet and Jallabert also reported the results of their temperature measurements. Jallabert was particularly interested in temperature because of his understanding of the mechanism of plant growth. One of his experiments suggested

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[46] Marshall (1779, introduction). Home (1756, 3) blew the same horn when he warned: “What a disagreement from a small difference in one of these circumstances!” Another author of an agronomical textbook, Thaer (1809, 9), similarly emphasized that one must prevent as far as possible the interference of anything foreign or unknown that might influence the outcome. For more on Thaer’s and Marshall’s conception of comparative experiments and their implementation in the agronomical sciences, see Schickore (2021).

that electricity increases transpiration in plants. This increase initially leads to a loss of substance, he thought, but ultimately to growth because the loss is "repaired by food." According to Jallabert, "nourishing juices make plants and animals grow" and electricity "accelerates the flow of fluids and the movement of plant juices."[47] Plant sap and its movement thus played a central role in Jallabert's understanding of how electricity promotes plant growth. Aware that "the sap seems to be in total inactivity in winter," Jallabert admitted that the "experiment made in England on myrtles" seemed to "combat his conjectures." He therefore regretted that he had not been able to determine the temperatures at which Demainbray's myrtles thrived:

> It would have been desirable that in publishing these curious observations the degree of the thermometer in the place where they were made would have been marked. However diligent I may have been to find out about this fact, I was unable to do so, and I do not know if this precaution was not neglected.[48]

Jallabert thought it likely that the sap in Demainbray's electrified myrtles was not not entirely without movement, both because it is warmer indoors than out and because "perhaps the myrtles that the electric virtue had caused to bud were handled before the experiments & then surrounded by spectators." This would have further warmed the room. Jallabert was referring to plant-specific knowledge when he added that "it is certain that the myrtle does not need as much heat to grow as most of the plants that are removed during the winter in greenhouses." He quoted Hales (1727, 62) that pineapples thrive at 29 degrees on John Fowler's thermometer, aloes at 19, Indian figs at 16, orange trees at 12, and myrtles at 9.[49] This episode shows nicely that the decision about which experimental conditions are relevant and worth measuring depended on how the experimenters conceptualized the process under investigation. In Sect. 3.4 we shall meet a physicist who decided to record the weather because he believed that it affected the strength of electricity.

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[47] Jallabert (1748, 266–267): "[...] les sucs nourriciers qui les font croitre, la dissipation de leur substance causée par la transpiration & que la nourriture répare [...]." And: "On ne doit donc pas trouver étrange que l'électricité qui accéléré le cours des fluides & le mouvement des sucs des plantes, exerce encore son action sur les êtres animes." On pp. 236–237, Jallabert made sense of his and Nollet's observations: "[A]s the nourishing juice flows more easily & more abundantly in the tender organs of a young plant than in those of a plant already strong by the ease it finds in passing through vessels which yield & expand easily, it is doubtless the cause of the rapidity with which the seeds germinated in the ground by the Abbé Nollet and those with which I covered the vase of porous earth of which I spoke. It is apparently by the same mechanism that electricity noticeably hastens the blossoming of the flowers which make all the parts of the plant the most delicate & those where the juices are carried most easily & in greatest abundance. The leaves and the petals that electrification seemed to revive seem to lend a new force to these conjectures since the juice made more abundant in their fibers must, by swelling them, shorten them and consequently straighten them."

[48] Jallabert (1748, 239): "Il auroit été à souhaiter qu'en publiant ces curieuses observations on eût marqué le degré du thermomêtre dans le lieu où elles ont été faites. Quelque diligence que j'aye apporté à m instruire de ce fait je n ai pû y réussir & j'ignore si cette précaution n'a point été négligée."

[49] Jallabert (1748, 240). Jallabert added that this 9th degree at Fowler's thermometer does not quite correspond to the 5th above zero of de Reaumur's thermometer.

3.3.2.2 Stabilize Experimental Objects

Demainbray and Nollet attempted to stabilize their experimental plants by picking myrtles of similar shapes and by using seeds from the same package. None of the experimenters, however, explained why they had chosen myrtles, wallflowers, or cress seeds in the first place. Nor did they specify the exact number of experimental plants (except for Demainbray, who compared one myrtle to another). Jallabert and Menon performed the same experiments on different plant species, and Nollet emphasized that he had repeated his tests on different seeds. We will see that, four decades later, Ingen-Housz called for better control of individual variability in experimental plants.

3.3.2.3 Control Intervention and Detection

Nollet (1749, 103–104) warned about a fundamental source of error when he explained that “in Electricity, as in all other matters of Physics, it is on the report of our senses that we judge things.” Because our senses could deceive us, he advised suspending judgment “until we have sufficiently verified the fidelity of their testimony.” As a control strategy, he committed to the maxim of making an observation “several times & in the same circumstances” and having “other eyes agree” with his: “Why not hear all the witnesses who can testify to a fact, if the unity of their voices should give more certainty to our knowledge?”

Nollet called for more than just multiple observers to witness an experimental result. He also wanted the same result to occur multiple times. This goal sets him apart from the other physicists. Demainbray was confident that comparing one electrified myrtle with one non-electrified myrtle was sufficient to draw a firm conclusion. Jallabert and Menon made several experiments with different plants, but they did not repeat the same experiment. Nollet, on the other hand, emphasized the need for many experiments to consistently show the same effect. After making repeated tests with mustard seeds, he still felt that more experiments were needed to determine with greater certainty the effect of electricity on vegetation. In experiments preceding his trials with organized bodies, Nollet (1752, 168–169) claimed to have repeated each experiment at least three or four times, adding that the results were identical or differed only slightly. He therefore felt that he could draw safe conclusions. Hales (1727, vii), for his part, maintained that to pry on the operations of Nature, physicists must take the “pains of analysing Nature, by numerous and regular series of Experiments.”[50]

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[50] Desaguliers (1727, 266) picked up on this rhetoric and praised Hales for following in Newton’s footsteps, “averting nothing but what is evidently deduc’d from those Experiments, which he has carefully made, and faithfully related; giving an exact Account of the Weights, Measures, Powers and Velocities, and other Circumstances of the Things he observ’d; with so plain a Description of his apparatus, and manner of making every Experiment and Observation, that as his Consequences are justly and easily drawn, so his Premises or Facts may be judg’d of by any Body that will be at the Pains to make the Experiments, which are most of them very easy and simple.”

One must avoid errors in the determination of experimental results, for these results are the basis for causal conclusions (Desaguliers 1745, i). If physicists do not measure the effects of their interventions accurately, then they risk drawing false conclusions.[51] For the same reason, we might expect Demainbray and the others to have tried to control their intervention, perhaps even to varying the amount of electricity, to see if the effect varied accordingly. But apparently this was not a concern for most of the physicists introduced so far. Nollet and Jallabert gave more detailed information about how and for how long they electrified their plants. But they did not further quantify the electricity applied.[52] They decided to compare two scenarios—the growth of electrified and non-electrified plants—rather than considering a third group, which they might have electrified twice as long, for example. The one exception was Erasmus King. He conducted what today we would call a sensitivity analysis. He "electrified 12 new laid eggs, three thrice, three 5 times, three 15, and the other three 20 times." However, the experiment was inconclusive.[53]

In contrast to Marshall (1779) and de Candolle (1832), other authors writing about comparative experiments required that the factors vary in order to examine them for their effects. Albrecht Daniel Thaer (1809), for example, maintained that "in order to investigate the effect of a thing under our control," we must

> add and omit, quantitatively and qualitatively change, only this *single* thing in various experiments, set up at the same time and next to each other, but keep everything else as constant as possible. The success will then tell us what part the *single* altered circumstance played in it.[54]

Later, we will meet physicists who tried to measure and vary the electricity supplied to their plants. But only one, Köstlin (1775), made a quantitative argument by

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[51] Desaguliers (1719, 2) warned that "we must not go about to define a Cause, unless we know its Effects" and advised experimental philosophers to "measure the Quantity of the Effects produc'd, compare them with, and distinguish them from each other" in order "to find out the adequate Cause of each single Effect, and what must be the Result of their Action." In his review of Hales' *Vegetable staticks*, Desaguliers (1727, 264) warned that "without being able to observe, compare, and calculate the exact Quantity of Weight, Force, Velocity, Motion, or any other Change to be taken notice of in making Experiments; Effects may be attributed to Causes which are not adequate to them, and sometimes expected to be produc'd even without a Cause."

[52] Nollet (1749, 157–158) did describe a procedure for measuring the decreases or increases of electricity. His electrometer consisted of a linen thread on an iron rod suspended horizontally, the two ends of which hung parallel to each other (see Figure 3.1). He explained that "as long as the two ends of the wire diverge from each other, it is certain that the body from which they hang is electric, and the angle they form, moving away from each other, is a kind of compass that marks more or less electricity."

[53] Anonymous (1747c, 200). The report continues: "One of these latter eggs produced a chick, and in all there were but 7 chickens hatched, six being addled eggs, among which was one unelectrified egg; so that nothing can be inferred from the experiment." It is unclear whether the one egg remained unelectrified by design or by accident.

[54] Thaer (1809, 10, emphasis in original). The German original reads: "[...] so müssen wir, um die Wirkung eines in unserer Gewalt stehenden Dinges zu erforschen, nur dieses *einzige* in verschiedenen zugleich und neben einander angestellten Versuchen zusetzen und weglassen, quantitativisch und qualitativisch verändern, alles übrige aber möglichst gleich erhalten. Der Erfolg wird uns dann über den Antheil, den der *einzige* veränderte Umstand darauf hatte, belehren [...]."

comparing the amount of electricity with the amount of effect (i.e. the speed of germination).

3.3.2.4 Neutralize Expectations and Report Accurately

A third potential source of error was the experimenter's expectations about test outcomes. Nollet (1748, 191–192) presented his experimental outcomes as confirmation of his theoretical reasoning. He wrote that his conception of plants as hydraulic machines led him to perform the experiments and made him "foresee their Success." This claim is remarkable in light of the fact that authors such as John Keill—"the first who publickly taught Natural Philosophy by Experiments in a mathematical Manner" (Desaguliers 1745, v)—saw this same point as a weakness of the experimental method. According to Keill (1700, 3), experimenters had "too often distorted their Experiments and Observations, in order to favour some darling Theories they had espoused." He therefore urged that comparisons of ratios with the phenomena of nature be made with great caution:

> [W]e are well apprised how fond these Gentlemen are of their Theories, how willing they are that they should be true, and how easily they deceive both others and themselves, in trying their Experiments. Such therefore as are produced by all, and which succeed upon every trial, we receive as undoubted Principles or Axioms: as likewise we ought sooner to give credit to those Experiments that are more simple and easy to be shewn, than to those that are more compounded, and difficult to be performed. (Keill 1700, 7)

In order to proceed with "greater safety, and, as much as possible, avoid all Errors," Keill advocated presupposing only those definitions necessary to arrive at the knowledge of things, and concentrating on one problem, ignoringall irrelevant aspects. He also urged others to start with the simplest cases (7–9).[55] However, neither Nollet nor Jallabert claimed to have considered particularly simple cases of plant growth. In contrast to Keill, Nollet emphasized the advantages of a hypothesis-driven approach. In his view, it was "useful […] in Physics to form a point of view early on, & to establish on first discoveries a system of explanations that one is nevertheless always ready to abandon, as soon as it is contradicted by sufficient reasons […]."[56] Ultimately, Nollet made it clear that physicists should submit only to facts and not to opinions: "When it comes to physics, one shouldn't

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[55] Keill (1700, 2–3) described the philosophers who proceed upon experiments as one of four most eminent "sects of Philosophers that have wrote on physical subjects" (the others being the Pythagoreans and Platonists, the Peripatetics, and the mechanical philosophers). The Experimenters, according to Keill, "make it their sole business, that the Properties and Actions of all Bodies may be manifested to us, be the means of our Senses." When describing his own "Manner of Proceeding, in the investigating the Causes of Natural Things," Keill proposed to combine useful elements of all four "ways of Philosophizing."

[56] Nollet (1752, 166): "On jugera par les expériences que je vais rapporter, [...] combien il est utile en Physique de se former de bonne heure un point de vûe, & d'établir sur les premières découvertes un système d'explications que l'on soit cependant toûjours prêt d'abandonner, dès qu'il sera démenti par des raisons suffisantes [...]."

be a slave to authority; one should be even less of a slave to one's own prejudices […]."[57]

Accordingly, physicists were careful to report unexpected results. Jallabert, for example, initially hoped that a certain experiment would "serve to show more clearly the way in which the electric fluid accelerates vegetation." But when it seemed to prove the opposite of his suspicion, Jallabert did "not conceal the fact": "although [the experiment] did not yield what I expected of it, I must nevertheless relate it so as not to omit any fact that has some influence on the discovery of the cause of such interesting phenomena."[58] He (1748, viii) reminded his readers that "honesty and accuracy in the detail of observations should be the main characteristics of the Nature Historian." Hales (1727, vi) had also emphasized the importance of accurate description when he asserted: "I have been careful in making, and faithful in relating the result of these Experiments, and wish I could be as happy in drawing the proper inferences from them."

3.3.2.5 Conclude Safely

According to Nollet, natural philosophers question nature by experiment, study nature's secret by assiduous and well-considered observations, and allow as knowledge of only that which appears to be obviously true.[59] According to Nollet (1749, 189–190; 1752, 168–169), Desaguliers (1727, 264), and Hales (1727, vii), this method is only reliable when physicists draw conclusions about causal relations from numerous, well-confirmed comparative experiments with consistent results.

3.4 Comparing the Growth of Electrified and Non-electrified Plants, 1750s–80s

Almost 40 years after Demainbray wrote to *The Caledonian Mercury*, Ingen-Housz caused a stir by denying that electricity affected plant growth at all. His contemporaries believed that overwhelming evidence for this view had accumulated over the previous four decades. But a closer look at the experiments from the 1750s to the

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[57] Nollet (1743, xxi): "[E]n matière d'Physique, on ne doit point être esclave de l'autorité; on devroit l'être encore moins-de ses propres préjugés, reconnoître la vérité; partout où elle se montre, & ne point affecter d'être Newtonien à Paris, & Cartésien à Londres."

[58] Jallabert (1748, 237–238). Ultimately, Jallabert suggested that "from the fact that the electric fluid could not in this experiment overcome the resistance occasioned by the gravity of the water & the friction of the walls of the tube, it should not be concluded that in still narrower pipes such as those of plants the electric fluid cannot lift and set in motion the liquors they contain."

[59] Nollet (1743, ix): "[O]n prit le parti de l'interroger par l'expérience, d'étudier son secret par des observations assidues & bien méditées, & l'on se fit une loi de n'admettre au rang es connoissances, que ce qui paroîtroit évidemment vrai."

early 1780s reveals that not all naturalists were convinced that electricity promoted plant growth. The Swedish naturalist Edvard Fredrik Runeberg (1757, 15), for example, said that "far too much" had been concluded from Demainbray's experiments, "for although two electrified myrtle twigs [sic!] have grown more rapidly than unelectrified ones, one cannot be sure that electricity causes the same rapid growth, or that it has the same effect on all twigs of the same kind."

This section examines contributions to the controversy published from the 1750s to the late 1780s in light of the errors and control measures highlighted in Sect. 3.3.2.

3.4.1 No Causal Inference on the Basis of a Single Experiment

Runeberg (1721–1802) was not convinced that electricity caused extra growth in Demainbray's test-myrtle, nor that electricity promoted plant growth in general. In his own experiments, he emphasized details of the intervention and a condition, the weather, that had not previously been considered. Perhaps most remarkably, he refused to draw any causal conclusions.

On July 4, 1754, at 9 o'clock in the morning, Runeberg distributed 22 almonds in four containers of equal depth. He placed eight almonds in each of two nearly identical wooden boxes and three almonds in each of two unglazed stone pots. Of these, one wooden box and one stone pot were electrified, while the other wooden box and the stone pot stood next to each other without being electrified.[60] He stored the electricity generated by rubbing a glass ball in a Leyden jar and transferred it to iron bolts near the almonds (Fig. 3.2). Of his detection regime he wrote: "At 12 o'clock every day, both the electrified and non-electrified plants are measured with a yardstick set up for that purpose and divided into decimal inches and lines" (17). In a table (Fig. 3.3), he noted the weekly growth of the plants, with the electrified plants marked with Latin letters and the non-electrified with Greek letters.

Runeberg decided to run his experiment longer than those of his predecessors, and to electrify the test plants more extensively.[61] He monitored growth over 16 weeks, during which time he electrified the eleven test plants between five and seventy-four times per week. He also built an electrometer to "take into account the relative strength of the electricity" (16), and for each of the 16 weeks he checked the strength (strong, medium, weak) of the electricity applied. In addition to the intensity and frequency of the intervention, he also registered the weekly weather, regretting that he had to do without a barometer and thermometer. Based on his experimental results, Runeberg concluded,

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[60] Runeberg (1757, 17–18). In the same year that he published his experiments, Runeberg, who was the inspector of weights and measures in Stockholm, became a member of the Royal Swedish Academy of Sciences in Stockholm.

[61] Runeberg (1757, 15).

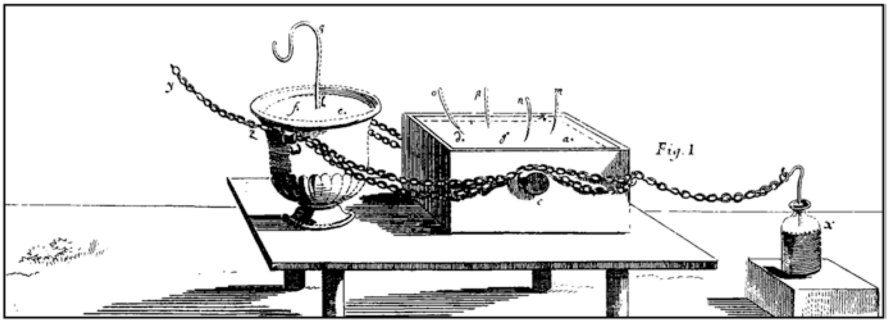
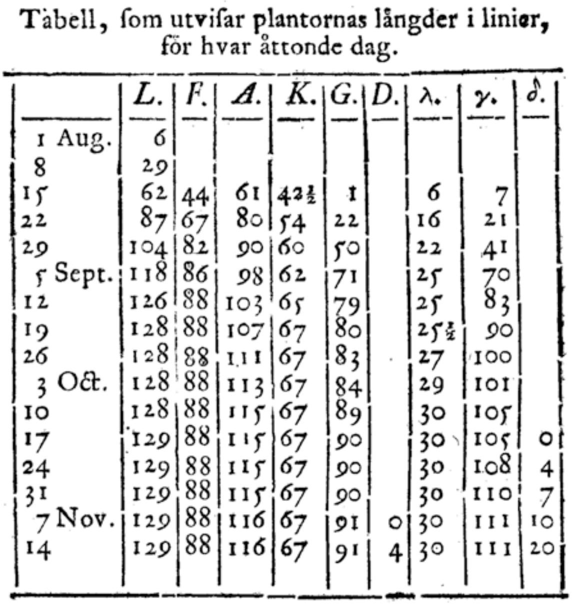


Fig. 3.2 Plate from Runeberg (1757, after 78), depicting the setup to electrify the iron bolts (m, n, o, p, and q) in the stone pot and the wooden box. The bolts are connected to C, the head of an iron bolt of 4 lines in diameter. This bolt receives electricity from the iron chain connected to a Leyden bottle. The position of the almonds is given by the letters a, b, k, d, g, e, f, l



Tàbell, ſom utviſar plantornas långder i linier, för hvar åttonde dag.

	L.	F.	A.	K.	G.	D.	λ.	γ.	δ.
1 Aug.	6								
8	29								
15	62	44	61	42½	1		6	7	
22	87	67	80	54	22		16	21	
29	104	82	90	60	50		22	41	
5 Sept.	118	86	98	62	71		25	70	
12	126	88	103	65	79		25	83	
19	128	88	107	67	80		25½	90	
26	128	88	111	67	83		27	100	
3 Oct.	128	88	113	67	84		29	101	
10	128	88	115	67	89		30	105	
17	129	88	115	67	90		30	105	0
24	129	88	115	67	90		30	108	4
31	129	88	115	67	90		30	110	7
7 Nov.	129	88	116	67	91	0	30	111	10
14	129	88	116	67	91	4	30	111	20

Fig. 3.3 Table from Runeberg (1757, 28) showing the lengths of the plants in lines, for every 8 days. The almonds L, F, A, K, G, and D were electrified while α, γ, and δ were not. The seven almonds that did not germinate at all are not included in this table nor in Runeberg's calculated averages of growth

> (1) that the electrified plants came up first, and probably fastest, but if the electricity caused that speed, several rounds must show. (2) That more of the electrified than of the unelectrified almonds came up. (3) That none of the unelectrified almonds made as many shoots per day as the plant L, namely 8 lines. (4) That none of the unelectrified plants have reached the height of two of the electrified ones. (5) That since the cold began and the strength of the electricity was reduced, the growth of the electrified plants was slowed down. (6) That the electrified plants did not lose in coarseness and steadiness against the unelectrified ones, even as the plant L, which grew strongest of all, shot even from its root a shoot, which was trimmed by violence, and was on the 2nd of September 40 lines long, and as thick as any of the unelectrified ones of the same length. (7) That the average of the growth of the electrified plants was 82 ½, while the average of the unelectrified plants was only 53 $^{2}/_{3}$, at the same time as the former were electrified 491 times.[62]

Runeberg distinguished finely between various effects of electricity: (1) the rate of upcoming, (2) the proportion of upcoming plants among those planted, (3) the maximum growth length of a shoot within a day, (4) the height attained, (5) the growth rate of electrified plants as a function of weather, (6) the coarseness and firmness of the plants, and (7) their average height. Although he described marked differences between electrified and non-electrified almonds, Runeberg did not draw any causal conclusions from the differences. His comment suggests that he would not do so until he had repeated the experiment several times with the same result.

3.4.2 Inferring Causes from Constant Effects Rather than Single Observations

Runeberg was cautious and avoided drawing conclusions from a single comparative experiment. At the opposite extreme, we find authors who considered mere observations to indicate the influence of atmospheric electricity on plant growth. Carl Heinrich Köstlin (1755–1783), a student of medicine at the University of Tübingen, stated that we know the positive influence of storms on vegetation "by common experience, especially if with rain the [electric] material of the lightning melts away"; "We see that after such storms the plants, which were previously weak, recover new strength, and the next generation grow happier plants" (1775, 34). He also observed that "the regions subjected to more storms are known as the most

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[62] Runeberg (1757, 26–27): "Haͤraf finner man 1), at de electricerade plantorne vaͤl kommo foͤrst up, och skoͤto maͤstadels fortast, men om electriciteten foͤrorsakat den skyndsamheten, måfte flera roͤn utvisa. 2) At flera af de oelectricerade, aͤn af de oeletricerade mandlarna upkommit. 3) At ingen af de oelectricerade mandlarna gjordt så stora skott om dygnet, som plantan L, naͤmligen 8 linier. 4) At ingendera af de oelectricerade plantorna hunnit til den hoͤgd, som tvaͤnne af de electricerade. 5) At sedan koͤlden tog til och electricitetens styrka af, saktades de electricerade plantornas tilvaͤxt. 6) At de electricerade plantorna ej foͤrlorat i grofhet och stadighet emot de oelecticerade; haͤllt som planten L, hvilken vaͤxte starkast af alla, skoͤt aͤfven ifrån sin rot en telning, som trifdes vaͤl, och var. den 2 Sept. 40 linier lång, och raͤtt så tjoek som någondera af de oelectricerade af samma laͤngd. 7) At medium af de electricerade plantornas vaͤxt aͤr 81½, då medium af de oelektricerade aͤr allenast 53 $^{2}/_{3}$, linea, på lika tid, då de foͤrre blifvit electricerade 491 gångor."

fertile regions." Louis-Hyacinthe d'Everlange-Witry (1719–1791) similarly referred to the "observation confirmed by Gardeners, that natural rain, being more or less impregnated with a certain portion of electric fire, is better suited to plants than watering made with other water: You will judge by this the effect of the appreciably electrified rain that is observed at all times" (1777, 18). Based on this observation, he was willing to ascribe a growth-promoting effect to natural electricity.[63]

Jan Ingen-Housz (1730—1799) rejected this argument. He commented that one "cannot doubt the fact" that stormy rains revive vegetation, "but one could doubt whether these rains would not produce the same effect, if they were not electric" (1789, 202). Another four decades later, de Candolle (1832, 1090–1091) noticed similarly that such observations "always leave a little doubt, because it is difficult to isolate by thought the effect of electricity from that of heat and humidity, which more often than not combine with it." Moreover, these events are difficult to observe, "because we are surprised by thunderstorms, and seldom have the presence of mind to measure them exactly." Nevertheless, de Candolle believed that these observations "tend to prove, at least vaguely, the influence of atmospheric electricity on vegetation." The naturalists therefore differed in their view of the value of observations in assessing causal relationships.

Another oft-quoted observation concerned the growth of wild jasmines in the garden of Senator Quirini. Pierre Bertholon de Saint Lazare (1741–1800), who taught physics in Montpellier, quoted from a letter he had received from the priest, physicist, and professor of astronomy at the University of Padua, Giuseppe Toaldo (1719–1797):

> Two of these jasmines which are contiguous to the chain of the conductor […] rose to an extraordinary height, and after two years one saw them surpass the roof of the house, at thirty feet in height; while the other jasmines which are cultivated with the same care, have hardly four feet in height. These two shrubs which are twisted to the mast & to the chain of the conductor, are of a triple size of the others & give flowers before them & in much greater quantity; they still continue to give some several days & several weeks after the others.[64]

Toaldo wrote to Bertholon that "this confirms what you say in your book [*De l'électricité des végétaux*] that the plants grow better and are more vigorous around the lightning conductors, when there are some of them." Indeed, Bertholon

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[63] We find similar reasoning in Gardini (1784, 25), who stated that "[o]bservations of natural atmospheric electricity further show that the greatest influx into vegetation originates from the same. For the plants begin to develop, grow and flourish, while in spring many stormy clouds begin to appear scattered everywhere, which possess and give the greatest amount of electricity to the air, but this development and vigor of the plants continues until in the autumn such clouds cease; in fact, at the end of the summer the storm clouds decrease in frequency and number."

[64] Bertholon (1787, 371–372): "Deux de ces jasmins qui se trouvent contigus à la chaîne du conducteur dans l'endroit où il s'enfonce en terre se sont élevés à une hauteur extraordinainaire, & au bout de deux ans on les a vus surpasser le toit de la maison, à trente pieds de hauteur tandis que les autres jasmins qui sont cultivés avec le même soin, ont à peine quatre pieds de hauteur. Ces deux arbrisseaux qui se sont entortillés au mât & à la chaîne du conducteur sont d'une grosseur triple des autres & donnent des fleurs avant eux & en beaucoup plus grande quantité ils continuent encore à en donner plusieurs jours & plusieurs semaines après les autres."

considered observations to be decisive even when they did not come from comparative experiments. In his view, there was "nothing more decisive than this beautiful observation."[65] Ingen-Housz disagreed. For him, the debate provided a moment to write about the effects of chance and how to control them:

> In order to decide on the existence of a law of nature of this kind [*i. e.* that atmospheric electricity accelerates vegetation], it is necessary that a large number of direct and comparative facts demonstrate its reality by a uniform result. Now, the fact in question is an isolated one, and consequently does not decide anything as such. Pure chance could have produced it among the jasmines, as chance sometimes produces a giant among men.[66]

Ingen-Housz (1789, 330) reminded his readers that "from a particular case of this nature, we cannot legitimately deduce a general consequence." Quirini's experiment "would not decide the question, as long as other similar experiments repeated and observed with care have not had the same effect constantly and obviously" (328). Ingen-Housz suggested that rigorous comparative trials would more accurately identify the cause of the jasmine's extraordinary growth. If "a similar shrub had been planted near a pole which was not topped by a conductor," one would probably have observed a similar effect. According to Ingen-Housz, Quirini would have had a basis for causal conclusions only if he had repeatedly compared the growth of jasmines near lightning rods with that of jasmines near ordinary poles (225–226).

Ingen-Housz asked his fellow physicists "that they have the goodness not to allege single, isolated facts, or such as they hold from hearsay, second or third hand." In his view, the idea of electric force as an accelerator for vegetation had "already served as a basis for endless works & theories, & for costly practices (see Fig. 3.4), which could find themselves quite uninstructed, if, unfortunately, the foundation of the system itself were found to be lacking" (217). He argued that "the public, in a matter of such superior importance", must be able to base decisions on "well-detailed and carefully observed reports of experiments, made by those who present them."[67]

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[65] Bertholon (1787, 372).

[66] Ingen-Housz (1789, 222): "Pour décider de l existence d une loi de la nature de ce genre, il faut qu'un grand nombre de faits directs & comparatifs en démontrent la réalité par un résultat uniforme. Or le fait dont il s'agit est un fait isolé, & qui par conséquent ne décide rien comme tel. Un pur hasard auroit pu le produire parmi les jasmins, comme un hasard produit quelquefois un géant parmi les hommes." Ingen-Housz (1789, 220–221) met a scholar who had visited Quirini's garden in 1786 and corrected Toaldo's account as follows: "There are not two jasmines, [...] but only one, which is contiguous to the mast surmounted by a lightning rod." The scholar moreover assured Ingen-Housz that this jasmine is "at least three times as big as all the others, [...] leaned against the mast" and that "Senator Quirini, & all those who witnessed this fact, attribute the extraordinary height of this jasmine to the fact that the conductor supplied him with an extraordinary quantity of electric fluid."

[67] Ingen-Housz (1789, 219). Regarding the letter of Toaldo to Bertholon, for example, Ingen-Housz (1789, 219) complained that "some articles of this letter seem to me to lack clarity, which probably comes from the little care that the one who copied or translated it from the original employed there."

Fig. 3.4 Plate II, Fig. 1, from Bertholon (1783, after 468), depicting a means to spread electric rain on trees, in order to increase their vegetation. Bertholon (1783, 406–407) expected that the electric rain would carry to all plants "a principle of fecundity, a particular virtue which has the greatest influence on all the vegetable economy." Contact between a man and an electric machine set in motion is established by means of a chain E, attached to the conductor (D). Standing on a large insulating stool, the man waters the tree (G) by pushing the piston (C). In order to communicate electricity to the tray (B) filled with water, he places one foot on a small plate of tin F

3.4.3 Sensitivity Analyses and Varying the Amount of Electricity Applied

Although many physicists of the 1770s also cited observations to support the view that electricity promoted plant growth, they primarily conducted comparative experiments. Among these efforts, the experiments of Köstlin (1775) stand out as particularly elaborate. Köstlin, one of the few who had read Runeberg's article (in its German translation), studied how electricity affected the development of chicken and butterfly eggs, as well as the growth of certain plants. He listed the "precautions which are to be observed in experiments, if I wished to elicit anything certain from them." These precautions all concern the fact that the test and control plants should be treated as equally as possible.[68]

To get a sense of Köstlin's elaborate experiments, let us look at one that lasted more than two weeks. Köstlin sowed seeds of *Cheiranthus cheiri* into twenty

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[68] Koestlin (1775, 7) used equal vessels, and, as far as possible, filled them with the same kind of earth in the same quantity. Each vessel was irrigated at the same time, in the same way, and with the same quantity. The seeds were placed deep in the ground and were, as far as possible, equal seeds. The same number were placed in the electrified and non-electrified vessels. The pots placed outside the windows were positioned next to each other so that they were exposed in the same way to temperature, sunlight, and other factors. Moreover, those not electrified were kept in the same room for the same time while the others were electrified.

vessels. The vessels themselves were of different materials and filled with different substances, and those that were electrified received the electricity in different ways.[69] The "system of electrification" from June 8 to June 16 was as follows:

> On the 8th of Jun. in the morning immediately after sowing, at noon and in the evening, they were electrified, Nr. 2, 3, 5, 9, 10, 12, 14, 16, & 18. for 30 min. Nr. 4. but for 45. min. From Nr. 7. five times in the morning, at noon, and in the evening, as many simple sparks were fired as they could fire after 50 revolutions of the wheel. And to Nr. 8. were applied 5. spark concussions three times a day, so that the individual concussions succeeded each other after 50. rotations of the ball. Simple sparks and concussions were evoked from the surface of the ground and usually in one and the same place.[70]

Köstlin recorded the effects of these treatments by noting the order of the vessels in which the seedlings (not single seeds, but many) germinated:

> June 10 evening in nr. 4.
> D. 11. — morning in nr. 12. at noon in nr. 3. and in the evening in nr. 14.
> D. 12. — morning in nr. 5. at noon in nr. 13. & 16.
> D. 13. — morning in nr. 18. & 15. & evening in nr. 2.
> D. 14. — morning in nr. 5. & 10. noon in nr. 1. & evening in 17.
> D. 15. — morning in nr. 19. & 20. evening in nr. 9.
> D. 16. — morning in nr. 11.
> Note 1.) in nr. 10 & 11. not all the seeds germinated, but more did germinate in nr. 10. than in 11. Furthermore, the seedlings in both pots were very weak.
> Note 2.) in nr. 7. & 8. No seedlings sprouted in those places, from which simple electric sparks and concussions were elicited. Versus the walls of the vessels sprouted indeed some, but they seemed to be weak, and burned immediately if sparks were drawn from them.
> The seedlings grew in the ratio of germination, so that the difference was always noticeable until the 22nd, for on that day the vessels were emptied.[71]

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[69] From 15 plate vessels, 9 were filled with humus. Vessels Nr. 1. and Nr. 6 were treated in the ordinary way, i.e., they were not electrified. Electrical material was passed through vessel Nr. 2. Vessels Nr. 3. and Nr. 5. were exposed to simple electrification (positive, without sparks). Nr. 4. likewise received simple electrification, but the electrical material was communicated over a longer period of time. The ground of vessels 5 and 6, in addition, was manured with cow dung. Vessel Nr. 7 was treated with simple electric sparks, Nr. 8 with electric shocks, and Nr. 9 was exposed to negative electrification. Plate vessels Nr. 10. and 11. contained clay, 12. and 13. river sand, and 14. and 15. groves of wood. Two glass vessels (16. and 17.) and three shell vessels (18., 19., and 20.) were filled with earth. Vessels 10., 12., 14., 16., and 18. received simple electrification, while vessels 11., 13., 15., 17., 19., and 20. were treated in the ordinary way.

[70] Koestlin (1775, 21–22): "Die 8 Jun mane statim post sationem meridie & vesperi electrisabantur. nro 2, 3, 5, 9, 10, 12, 14, 16, & 18. per 30 min. nro. 4. autem per 45. min. Ex nro. 7. eliciebantur quinquies mane, meridie & vesperi tot scintillæ simplices, quot post 50. gyrationes rotæ elici potuerant. Et nro. 8. adplicabantur 5. scintilla concussoria ter quotidie ita, ut singulæ concussiones post 50. gyrationes globi se invicem succederent. Evocabantur scintillæ simplices & concussoriæ ex superficie humi & plerumque in uno eodemque loco. Continabatur hæc electrisandi ratio ab 8. Jun.—16. Jun."

[71] Koestlin (1775, 22): "Die 10. Jun. vesperi in nro. 4. D. II. — mane in nro. 12. meridie in nro. 3. & vefperi in nro. 14. D. 12. — mane in nro. 5. meridie in nro. 13. & 16. D. 13. — mane in nro. 18. & 15. & vesperi in nro. 2. D. 14. — mane in nro. 5. & 10. meridie in nro. 1. & vesperi in 17. D. 15. — mane in nro. 19. & 20. vesperi in nro. 9. D. 16. — mane in nro. 11. Not. 1.) in nro. 10. & 11. non omnia semina progerminabant, progerminabant vero plura in nro. 10. quam in 11. Plantulæ porro in utroque vase erant valde debiles. Not. 2.) in nro. 7. & 8. plantulæ nullæ in illis locis pro

Köstlin repeated these experiments three more times—twice with *Cheiranthus cheiri*, and once with *Cheiranthus incano*. Seeds of the latter were sown into six equal-sized vessels filled with the same earth, and again they were electrified in different ways.[72] Vessels Nr. 1. and 3., 5, were electrified at the same hours over the same intervals. Köstlin found that the seedlings in Nr. 1. and 3. sprouted after two and a half days, and the seedlings in Nr. 5. sprouted 1 day earlier than those in Nr. 2., 4., and 6 (23).

From these and other experiments, Köstlin concluded that electric matter influences the development of certain plants in the germination of freshly sown seeds. He was convinced that germination was accelerated *by* the passage of electrical material. In contrast to everyone else considered so far, Köstlin was able to argue that germination was accelerated in proportion to the quantity of electrical matter applied. Experiment §. 28., according to Koestlin, shows that electricity is able

> (α.) to accelerate germination *in several types of soil* […]; (β.) with respect to *vessels* that contain earth; a.) mostly in plate vessels, b.) less in glass vessels, c.) not at all in shell vessels. (γ.) germination seems to be more accelerated by electric material than by the *fermentation of cow dung*. (δ.) […] *in free air and in a closed room* […] (ε) even if the *soil*, including the seeds, *is not watered*. (ζ.) to hasten germination if the earth containing the seeds is irrigated by means of *water*, to which *previously the electric charge* has been communicated.[73]

Köstlin further concluded that “negative electrification retards germination,” and that seedlings do not grow in places where electric sparks are applied. His experiment §. 29 showed that the electrical material also affects vegetation in plants already germinated.[74] Overall, Köstlin was pleased that some of his results agreed with the “notable” experiments of Runeberg, Jallabert, and Nollet. In his opinion,

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germinabant e quibus eliciebantur scintillæ electricæ simplices & concussoriæ. Versus vasorum parietes progerminabant quidem aliquot sed debiles esse videbantur & torrebantur statim, si ex illis ipsis scintillæ eliciebantur. Crescebant plantulæ in ratione progerminationis, ita, ut differentia usque ad diem 22. semper effet notabilis, nam illo die vasa evacuabantur.”

[72] Koestlin subjected pot nr. 1. to “simple electricity” and kept it “in closed air.” Pot nr. 2., was kept in the open air and treated “in a common manner.” Nr. 3. was subjected to simple electricity. Nr. 4 was treated in a common way. Vessels nr. 3. & 4. received no water or any other fluid. He irrigated the soil in vessel nr. 5., with water, to which he had previously communicated simple electricity. Finally, he irrigated nr. 6. at the same times at nr. 5, but with non-electrified water.

[73] Koestlin (1775, 29): “α.) accelerare progerminationem *in pluribus terræ speciebus* 1.) in humo, 2.) argilla, 3.) arena, & 4.) scobe lignorum. β.) respectu *vasorum*, quæ terram continent & quidem a.) in vasis bracteatis maxime, b.) in vasis vitreis minus, c.) in vasis testaceis minime. γ.) progerminationem videri magis accelerari a materia electrica, quam *stercoratione simi bubuli*. δ.) accelerare progerminationem *in aëre libero & in clauso*, scilicet in conclavi, cui liber aditus aëris non patet. ε) accelerare progerminationem, quanquam *terra*, quæ semina includit non irrigetur. ζ.) accelerari progerminationem, si modo *aqua*, cui *antea simplex electricitas* est communicata terra semina continens irrigetur” (emphasis in original).

[74] In annual plants, Koestlin (1775, 30) found vegetation to be accelerated by means of electrical stimulation “in such a way that out of two equal plants, if the size of one is increased by electrification, the other becomes equal again to the former; when the former’s electrification ceases, the latter is electrified.”

the experiments together all "show in a similar manner the earlier germination of the seeds of certain fresh crops, and the increased vegetation of certain plants by the aid of electrification." However, the question of *how* electric matter produces these effects remained open.[75]

One of the few physicists to respond to Köstlin's work was Francesco Giuseppe Gardini (1740–1816), a former student of Beccaria practicing medicine. In 1782 Gardini was appointed professor of philosophy at the college in Alba.[76] Gardini (1784) repeated Köstlin's experiments with cotton and observed "that the seeds of cotton impregnated with electricity germinated more quickly than others, which I kept for comparison under the same circumstances" (18). Like Köstlin, Gardini studied the influence of electricity on annual plants, and "used and changed electricity in many and various ways and observed its influence" on vegetation. He claimed to have obtained the same results as Köstlin, except that he "could not observe a notable difference between positive and negative electrification" (19). To Gardini, from his experiments and those of other authors, it seemed "sufficiently proven" that "artificial electricity influences the life of plants, and that this influence is different in different circumstances and promotes their vegetation" (25).

3.4.4 *Less Intricate Comparative Experiments*

Other experimental reports were far less intricate. D'Everlange-Witry (1777, 18), for one, merely opined rather than reported an experiment.[77] Bernard Germain Etienne de La Ville-sur-Illon, comte de la Cepède (1756–1825) claimed that the electric fluid's action on vegetation had been "proven by incontestable experiments." But he remained vague in describing his own experiments.[78] Jean-Paul Marat (1743–1793) compared the growth of electrified and non-electrified lettuce seeds:

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[75] Köstlin (1775, 30–31) wrote that "this is a question to which fate can never give a definite answer, since so much about the very nature of electricity is still hidden from us. For the labors of the natural researchers, as most of their writings sufficiently testify, have certainly made little progress in this matter so far. It must therefore be emphasized in the probable explanation of such phenomena by the other effects of electricity known to us."

[76] Bertholon (1783, 154) outlined an experiment of a certain Édouard-François Nuneberg (sic!), reported by the physical and economic society of Stuttgart. He must have read the strongly abbreviated and erroneous second-hand report (Anonymous 1777), but not Runeberg's (1757) original account or its German translation, Runeberg (1759).

[77] D'Everlange-Witry, a noble canon of Tournai, was the superintendent of the cabinets of rarities at the Court of Brussels of Prince Charles Alexander of Lorraine. In 1773, he became a member of the Imperial and Royal Academy of Sciences and Belles Lettres of Brussels.

[78] De la Cepède (1781, 175–176). He maintained: "Whenever I have electrified a plant, I have also seen it grow and rise more strongly than usual, and I have always succeeded perfectly in hastening the vegetation of plants whose onions are made to germinate and grow in vases full of water." On de la Cepède, see (Schmitt 2010).

> On December 3, 1780, I filled six fayance pots with moist soil. I sowed lettuce seed picked on the same stem, and I maintained the fresh soil by watering it. Three of these jars were placed at the bottom of a very large jar on an insulator with a high glass column & in the middle of a chamber, where the thermometer was at two degrees above freezing. The other three were placed on an insulator in the middle of an adjacent room, equally exposed, equally without fire, and where the air was at the same temperature. For fifteen consecutive days, I kept the jar constantly loaded for seventeen hours out of twenty-four, and all this time, the thermometer only varied by one degree.
>
> Now, from the seventh day, we could see the beginning of vegetation in the first ones: it continued to grow little by little; and at the end of the fortnight, the little plants were as advanced as those of another pot which had been sown at the same time and kept in a room where the thermometer was constantly nine degrees above zero. But in the last three pots, there was no appearance of vegetation.[79]

Marat worked with the same experimental plants as Menon and said that the seeds came from the same stem. As with other experiments considered so far, it is unclear how many seeds were sown and how many germinated.

Finally, Bertholon (1783, 166) sowed poppy seeds in two identical vases and electrified one of them "from time to time." He observed "an acceleration in the germination and growth of the parts of the plant, […] and also a multiplication of small branches, leaves, flowers, capsules and seeds, which the poppies in the non-electrified vase did not show, although the cultivation and everything connected with it were the same on both sides."[80]

Bertholon repeated these experiments on tobacco plants with equal success. He found that "the ratios varied, but the plant multiplication in the electrified individuals was always constant" (167). He considered his experiments to be "decisive":

> Having electrified some plants for a certain time, & having observed […] that their branches, twigs, and leaves were considerably multiplied, by comparing them with plants of the same

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[79] Marat (1782, 359–360): "Le 3 Décembre 1780, je remplis de terreau humide six pots de fayance. J'y semai de la graine de laitues cueillie sur la même tige, & j'entretins la terre fraîche en l'arrosant. Trois de ces pots furent placés au fond d'une fort grande jarre sur un isoloir à haute colonne de verre & au milieu d'une chambre, où le thermomètre étoit à deux degrés au dessus de la congélation. Les trois autres furent posés sur un isoloir au milieu d une chambre voisine également exposée également sans feu, & où l'air étoit à la même température. Pendant quinze jours consecutifs je tins la jarre constamment chargée dixsept heures sur vingt-quatre; & tout ce tems thermomètre ne varia que d'un degré. Or, dès le septième jour, on appercevoit un commencement de végétation dans les premiers: elle continua à se faire peu à peu; & au bout de la quinzaine, les petites plantes étoient aussi avancées que celles d'un autre pot qui avoit été ensemencé en même tems & tenu dans une chambre où le thermomètre se soutint constamment neuf degrés au dessus de zero. Mais on n'apperçut dans les trois derniers aucune apparence de végétation." At the time, Marat was still serving as a physician in the household of the Comte d'Artois, but already focused on his career as an experimental physicist (Conner 2012, 23).

[80] Bertholon calculated "average numbers," suggesting to him that the "ratios of multiplication," or the "differences in excesses," were "for the branches of eight more; for the leaves, of thirty; for the flowers & fruits, of six; for the seeds contained in the capsules, of ten."

> species in the same circumstances, I always noticed that the roots of the electrified plants were larger, more abundant, better supplied with radicles & hair.[81]

Bertholon reported that, when he examined this object carefully, he found that the ratios of multiplication of the roots and hairs were about the same as that of the branches and leaves, namely 8 to 30. It is striking how precisely he measured the effects of his interventions, while saying little about what the intervention was.

In contrast, Franz Karl Achard (1753–1821), director of the physical classes of the Royal Academy of Sciences in Berlin, distinguished between the application of positive and negative electricity, just as Köstlin and Gardini did. He "filled three Leyden bottles to the half with moistened garden soil, & after having equalized it, I covered it with wet flannel, on which I put cress seed: one of these bottles was not electrified, the other was positively electrified, and the third negatively." However, he did not vary the electricity applied to his plants and did not report the duration of the experiment. He only stated that "every hour, [he] gave back to the bottles their electricity charge, and observed":

1. That the cress seed in the two electrified Leyden bottles germinated more than the one in the non-electrified bottle;
2. That the growth of the germ took place in the two electrified bottles with the same speed.
3. That the plants increased more in height in these two bottles than in the non-electrified bottle.[82]

Note that Achard's goal was not to evaluate the influence of electricity on plant growth; rather, he wanted to compare the effects of positive and negative electricity. He found that the value of the charge did not change the rate of growth.

Experiments in the 1770s and early 1780s by Marat, Achard, Bertholon, and Gardini seemed to confirm the view that electricity stimulates plant growth. And these experiments were not considered questionable at all; on the contrary. Bertholon and Gardini were awarded the *prix de physique* by the Académie des Sciences, Belles-Lettres & Arts de Lyon in 1782.[83] Moreover, the Société Royale des Sciences de Montpellier concluded that Bertholon's monograph (1783) deserved the praise of

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[81] Bertholon (1783, 167–168): "Ayant électrisé quelques plantes pendant un certain tems, & ayant observé, comme je l'ai dit, que leurs branches, leurs rameaux, leurs feuilles, &c. étoient considérablement multipliées, en les comparant à des plantes de même espece dans les mêmes circonstances, j'ai toujours remarqué que les racines des plantes électrifées étoient plus grandes, plus abondantes, mieux fournies de radicules & de chevelus."

[82] Achard (1784, 432): "1°. Que. la semence de cresson, dans les deuæ bouteilles de Leyde électrisées, germa plutôt que celle qui étoit dans la bouteille non électrisée; 2°. Que. l'accroissement du germe se fit dans les deux bouteilles électrisées avec la même vîtesse. 3°. Que. les plantes augmentèrent plus en hauteur dans ces deux bouteilles que dans la bouteille non électrisée."

[83] In 1782, the Académie des Sciences, Belles-Lettres & Arts de Lyon had formulated the following questions for the *prix de physique*: Does the electricity of the atmosphere have any influence on the plants? What are the effects of this influence? And if it is harmful, what are the means to remedy it?

the public and the approval of the Société because it contained “a large number of interesting, ingenious & decisive observations & experiments.”[84]

3.4.5 No Difference and no Reason for Inferring Causal Relevance

In the early 1780s, someone else was studying electricity and plant growth: Jan Ingen-Housz, the court physician to the Austrian empress Maria Theresa. After reviewing other experiments and finding them ill-judged, he sought to test the assumption that electricity promotes vegetation “very carefully by repeated facts.”[85] This decision was welcomed by the businessman and chemist Adriaan Paets van Troostwyk (1752–1837) and by the physician Cornelis Rudolphus Theodorus Krayenhoff (1758–1840). These two believed that, all in all, only a few physicists had been concerned with the subject. They also believed they knew why: the experiments that had already been done “seemed to be sufficient in the eyes of the majority of physicists.” Instead of repeating the experiments, which seemed useless, physicists simply accepted them—because “the names of Nollet, Jallabert, Menon, Achard, and a few others were authoritative enough to place the acceleration of vegetation by electricity among the best established principles.”[86]

After eight years of study, Ingen-Housz (1789) concluded that “the experiments that have so far been offered to show that the electric force accelerates vegetation are not decisive.” Crucially, he did not criticize the design of the earlier experiments. His trials were also comparative, but he found no consistent differences between electrified and non-electrified plants. Like the others, Ingen-Housz saw experiments as a means of substantiating causal hypotheses. He described the goal of physics as “the contemplation, in detail, of the intermediate causes & phenomena whose examination is within its reach, or which it produces, by combining different agents” (197). He illustrated this abstract account with the following example: “Rains prodigiously speed up the vegetation.” For Ingen-Housz, there was little doubt about this causal relationship: “We see the obvious effects of this. We imitate them by artificial watering which produces the same effect, without ever missing it.” But the situation is rather different with the electric fluid, he argued:

> Its influence on plants […] does not yet seem to me to be specially demonstrated; and I believe that from my experiments I shall be able to conclude that by artificially sprinkling

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[84] Extrait des Registres de la Société Royale des Sciences de Montpellier du 1er Juin, 1783, at the end of Bertholon de Saint-Lazare (1783).

[85] Ingen-Housz (1789, 183). Ingen-Housz criticized the work on *mimosa* for attributing to electricity an effect whose occurrence is in fact independent of the presence of electricity (see Ingen-Housz 1786, 92).

[86] Van Troostwyk and Krayenhoff (1788, 134).

> plants with this fluid […], an effect has been attributed to the electric force which, in reality, was produced by the faintness of the light.[87]

Ingen-Housz did not doubt that other physicists had found growth differences, but he did not believe that they were due to electricity. He offered an alternative explanation, attributing the differences to the different light intensities to which the test and control plants were exposed—those less exposed to light grew faster.

But let us start from the beginning. In the spring of 1781, Ingen-Housz (1789, 183–184) placed some daffodils and hyacinths on an insulator, electrified them continously during the day, and placed other similar plants some distance away but did not electrify them. He found no difference in growth. These preliminary trials, Ingen-Housz recalled, showed him that the effect of electricity on vegetation "was not so evident" as he had believed, "according to the writings of the physicists who had established or confirmed this system." Over the next two years, he repeated the experiments but never obtained the same results as the other physicists.

3.4.6 Compensating Individual Variability with Many Experimental Objects

Determined to judge the matter more carefully, Ingen-Housz decided not to work with daffodils or hyacinths. He considered these bulbous plants to be unsuitable test objects "because of the great difference which one often observes in the progress of their vegetation; in such a way that one rarely finds three in a row which grow in a uniform way" (184–185). From this we can deduce that, for Ingen-Housz, suitable test objects should exhibit the target behavior consistently. In other words, the experimental plants should grow uniformly under similar conditions. After all, any differences in growth would provide the basis for causal inferences. Instead of bulbous plants, Ingen-Housz used seeds of mustard and cress, which are plants that grow much more uniformly.

He sprinkled 60 to 100 seeds on a "floating island" made of slices of cork wrapped with pieces of fog paper or linen. He then used different methods to electrify the seeds. At "the same time, in a place far from all electricity," he performed "an equal number of *experiments of comparison*, exactly uniform to those

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[87] Ingen-Housz (1789, 197): "Son influence sur les végétaux, dont on ne sauroit douter ne me paroît pas encore spécialement démontrée; & je crois que d'après mes expériences je pourrai conclure qu'en arrosant s'il est permis de'm exprimer ainsi, artificiellement les plantes de ce fluide, on a attribué à la force électrique un effet qui, en réalité, étoit produit par la foiblesse de la lumière." Ingen-Housz (1789, 188) explained in more detail: "I have observed that sunlight, so beneficial to adult plants, is very harmful to the development of seeds, and to the growth of very young plants. This is why the seeds of mustard, cress, and probably any other plant, develop better when placed at the bottom of a room, than when they are placed near the windows; and it is probably for lack of this attention, that we have made an erroneous judgement (if it is an erroneous judgement) on the cause of the sudden growth of electrified plants."

mentioned above" (186, emphasis added). "The constant result" was that the electrified plants, "placed in exactly the same circumstances as the others," did not grow faster. Ingen-Housz assured his readers:

> [I]n all these experiments, varied in every way I could imagine, it was evident that the electric force had no effect in advancing vegetation; it was evidently from the greater or lesser degree of light, and in no way from the electric force, that the difference in vegetation acceleration depended. Also no difference could be found between electrified & non-electrified plants, when both were placed at exactly the same distance from the windows.[88]

But he was still not satisfied, and so proceeded to make "infinitely more conclusive" experiments by sowing mustard and cress seeds on the largest fayence dishes he had. This experiment was supposed to be more conclusive because it involved more plants. Each dish contained more than 1000 seeds. Although Ingen-Housz "kept the dishes electrified night and day," the vegetation "was always more or less precocious, […] and the electricity did not contribute in any way to make them grow more rapidly." He thus summarized:

> Seeing that vegetation was always at least as good in the non-electrified jar as in the one that was constantly electrified, it seemed quite clear to me that it was the weakness of the light and not at all the electric force that was the cause of the early growth of the seeds placed in these electrified jars.[89]

3.4.7 *The Need for Perfectly Equal Conditions*

Van Troostwyk and Krayenhoff (1788) supported Ingen-Housz's findings. They also found no consistent difference between electrified and non-electrified plants. During their study, they observed nothing "that could provide the slightest reason to defend the influence of electricity on vegetation" (140). In the summer and fall of 1786, they experimented with Turkish beans, cress, and horseradish. Unlike Ingen-Housz, however, they compared the growth of individual seeds. On August 3, for example, they chose from many Turkish beans "four beans which appeared to the eye to be exactly alike" and treated them equally with the exception of electrifying

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[88] Ingen-Housz (1788, 324): "En un mot, dans toutes ces expériences variées de toutes les manières que je pouvois imaginer, il étoit évident, que la force étectrique n'avoit aucun effet pour avancer la végétation; c'étoit évidemment du degré de lumières, & nullement de la force électrique, dont la différence dans l'accélération de la végétation dépendoir. Aussi ne pouvoit-on trouver aucune différence entre les plantes électrisées & non-électrisées, lorsque les unes & les autres étoient placées exactement à là même distance des fenêtres."

[89] Ingen-Housz (1786, 92): "En voyant que la végétation se faisoit toujours au moins aussi-bien dans la jarre non électrisée que dans celle qui l'étoit constamment, il me paroissoit assez décidé, que c'étoit la foiblesse de la lumière & nullement la force électrique, qui étoit cause de l'accroissement précoce des semences placées dans ces jarres électrisées."

two of them.[90] The two electrified beans germinated first and continued to grow faster than the non-electrified ones:

> [O]n the 26th of August, when we finished this experiment, one of the electrified plants had a height of 16 inches and a quarter: the other of 21 inches and a quarter: while one of the non-electrified plants was only 8 inches and a quarter, and the other of 10. Since the beginning, 455 hours of electricity had been used.
>
> […] although the two electrified plants surpassed the other two in height, they did not appear to be more advanced in other respects, nor more vigorous: for they grew their second and third stems at about the same time as the other two; and all four resembled each other in this respect.[91]

While this first experiment suggested that electricity positively affects vegetation, other attempts yielded different results. On September 1, van Troostwyk and Krayenhoff took three small bean plants, left one "in its natural state," and electrified the others for 76 h. Twelve days later the three plants were "perfectly in the same state, which continued without the slightest difference until the 20th." On the same day they started an experiment with three vases and five beans in each—one vase was not electrified, with the other two positively and negatively electrified respectively. This time the non-electrified plants grew best. Further experiments with cress seeds sown on pieces of wool also showed no difference between electrified and non-electrified plants: "vegetation was equal in all directions." After van Troostwyk and Krayenhoff cut these stems to the same height, "the vegetation started again with an equal vigor without being able to notice the least difference." Repeating this experiment with negative instead of positive electricity gave the same result: "Expansion, germination, growth, and the production of new stems, after the first ones had been cut off: everything, in a word, happened on one of the two pieces of wool as on the other, without us being able to notice the slightest difference."

The two were puzzled that their experiments were "so diametrically opposed to those that were made before" them, since those experiments had been done by physicists whose names will be "forever celebrated in the history of electricity and will always have much authority" (141). They did not doubt the "good faith" of those

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[90] Van Troostwyk and Krayenhoff (136–137) reported that they "placed each [bean] in a glazed earthenware pot, filled with an equal quantity of the same earth, provided with a hole in the lower part, & placed on a saucer which contained the same quantity of water. We suspended two of them (with their saucers) from metal wires: & two others from silk cords: in order to raise them by a bottle of Leide [...]."

[91] Van Troostwyk and Krayenhoff (137): "[...] le 26 d'Août, que nous terminâmes cette éxpérience, une des plantes électrifées avoit une hauteur de 16 pouces & un quart: l'autre de 21 pouces & un quart: tandis qu'une des plantes non électrisées n'étoit que de 8 pouces & un quart, & l'autre de 10. On avoit employé depuis le commencement 455 heures d'électricité. Nous croyons devoir ajouter, que, quoique les deux sèves électrifées surpassassent les deux autres en hauteur, elles ne paroissoient cependant pas plus avancées à d'autres égards, ni plus vigoureuses: car elles poussoient leurs secondes & troisiémes tiges à peu près dans le même temps que les deux autres: & toutes quatre se ressembloient à cet égard."

figures, who were "endowed with all the talents necessary to observe Nature well, & who so often gave proofs of their genius & of their exactitude." Van Troostwyk and Krayenhoff maintained that it was difficult to explain their results, "since there are a great number of circumstances which can accelerate or retard the vegetation of plants," and, hence, there are many potential confounders. Nevertheless, they had a suspicion: "[It] seems likely to us that not enough care and precaution was taken in the first experiments on this subject to make all the circumstances of the plants that were electrified and those that were not electrified perfectly equally" (141). They followed Ingen-Housz in suspecting that "perhaps care was not taken to provide the same degree of light to these two types of plants: a circumstance which nevertheless has the greatest influence on vegetation."

3.4.8 No Other Authorities beside Comparative Experiments

Ingen-Housz (1788, 337) did not conclude that the electric fluid has no influence on plants. But he claimed that the experiments hitherto thought to establish electricity's growth-promoting effect "do not have all the authenticity that has been attributed to them." Still, Ingen-Housz hoped that his experiments would motivate other physicists "to imitate them, or to imagine new ones, in order to be able to judge whether, and to what extent, I have been mistaken in my observations." He urged his peers to examine his work and said that nothing would give him more pleasure than to "see my experiments invalidated by others more conclusive." He looked forward to embracing the growth-promoting effect again as soon as a physicist presents "to the court of the public an exact detail of experiments analogous to" his own, or others which would have had a "constant success" opposite to his findings.

Ingen-Housz (1789, 191) demanded that objections be based on observation and not, like the criticism of M. Duvarnier (1786), on "the respectable authority of all the nations and of the most famous physicists they have produced." But Duvarnier was not alone in his position. Thomas Nicolas Jean de Rozieres (1791a, 352), too, considered it legitimate to decide the question "according to recommendable & respectable authorities." Rozieres was concerned that many people "were put off by the numerous contradictions of the scholars, in their writings, which make that after having read a lot, one is often not more informed," and thus wished for more unity among scholars (354, fn 2).

Compelled to defend the call for rigor, Ingen-Housz (1789, 225–226) assured that he was "by no means guided by the spirit of contradiction or criticism […] but by a sincere desire to discover the light in the middle of darkness; by a desire to lift the veil under which nature often likes to hide herself." He emphasized how difficult it is to discover nature's secrets, and how easy it is to err. This was precisely the lesson that Jean-Claude de la Métherie drew from the controversy.[92] The editor of the *Journal de Physique*, where both Ingen-Housz's and Duvarnier's articles appeared, suggested that the experiments "must still be repeated to know finally on which side the truth lies." Van Troostwyk and Krayenhoff (1788, 142), for their part, wished physicists to follow Francis Bacon's lesson in not imagining or supposing, but *discovering*, what nature does or may be made to do.[93] This lesson requires that one avoid the influence of previous studies when experimenting—a demand for which Nollet (1743) had already advocated.

According to Ingen-Housz (1789, 182–183), the problem with the work on electricity and plant growth in the 1770s was the physicists' expectations. He assumed that they were convinced that electricity accelerated vegetation, and so wanted to see this influence confirmed by outdoor experiments. Four decades later, de Candolle revisited the problem. According to de Candolle (1832, 1535), "most of those who make experiments like to see them succeed." As a result, experimenters "always tend, by a very forgivable inclination of the mind, to exaggerate the favorable results of their trials, and to conceal the contrary results." But if naturalists fail to report experiments that find no effect, while exaggerating effects when they do find them, then they distort the facts. Hence one sees a multitude of procedures praised by authors and unchecked by newspapers—a situation that in reality "cannot be sustained in practice, nor enlighten the theory."[94]

De Candolle proposed two countermeasures. First, experimenters should take systematic notes to prevent their expectations from inflating their observations. "Without precise notes, without rigorous labels, without exhibits," de Candolle (1832, 1536) believed, "the most exact minds are prone to strange illusions about long-lasting phenomena." Second, de Candolle criticized learned societies and journal editors, because they were supposed to be the institutional bodies of control. They should act as gatekeepers, publishing only reports that meet certain standards. He deemed it "desirable that this mass of agricultural and horticultural societies which cover Europe today, accept in principle to give some attention only to those experiences which are really comparative and expressed by formal figures" (1535).

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[92] The editor's note on Duvarnier (1786, 94) reads: "Such opposite results in experiments made on the one hand by Physicists as famous as those quoted here by Mr. Duvarnier, and on the other by Physicists no less famous, Messrs. Ingen-Housz, Schwankhardt, etc., must surprise, and show all the difficulties that the art of experiments presents."

[93] After Bacon (1620, Liber Secundus, Aphorismus X).: "Primo enim paranda est historia naturalis et experimentalis, sufficiens et bona; quod fundamentum rei est: neque enim fingendum, aut excogitandum, sed inveniendum, quid natura faciat aut ferat."

[94] De Candolle (1832, 1534–1535) complained: "Every day one reads in the books on cultivation, and hears in conversation, the use of such and such a process, and proclaims it good or bad, without an exact term of comparison. The product is related to an approximate average that each one has thought of the product of his fields; and when one comes to a more careful examination, one recognizes that this average is almost arbitrary within large limits; that, consequently, the vague assertion that a process has succeeded well or not so well is very often due to the personal character of the observer."

3.5 Comparative Experimentation in the Eighteenth Century and beyond

Psychologist Edwin Boring (1954, 589) suggested understanding the methodological status of control as check or comparison with reference to John Stuart Mill's (1806–1873) method of difference. Mill (1843, 459) praised this method as the only way to "arrive with certainty at causes" through direct experience. The method shares the essential features of a strategy we have seen in the writings of various naturalists—two settings are kept perfectly equal except for one intended difference (Marshall 1779); a single circumstance is altered (Thaer 1809); or one is established as positive (de Candolle 1832). One then evaluates how the two settings compare. Other authors who discussed comparative trials were Matthias Jacob Schleiden (1804–1881) (see Nickelsen, Chap. 7, this volume) and Claude Bernard (1813–1878). According to Bernard (1865, 224), comparative experimentation allows physiologists to isolate a phenomenon to be studied from all the complications surrounding it. It does so by adding to a comparison organism all the experimental modifications except one, which is the one they wish to identify.[95] In Mill's (1843, 455) words, experimentalists strive to bring about two instances—one where the phenomenon occurs, and one where it does not—that have all circumstances in common except one. The circumstance "occurring only in the former; the circumstance in which alone the two instances differ" is the "effect, or cause, or a necessary part of the cause, of the phenomenon."

This section reinforces Boring's proposal by summarizing the main findings about eighteenth-century control practices that emerge from the controversy on electricity and vegetation. We shall further discuss the connection between comparative experimentation and the study of biological phenomena.

3.5.1 Comparative Experimentation and Strategies of Control

We have seen that the concept of comparative experimentation was not only mentioned in methodological discussions, but also guided physicists in the design of their experiments. Between the 1740s and 1780s, physicists compared the growth of electrified and non-electrified plants in the kingdoms of Great Britain, France, Savoy-Piedmont, Sweden, Prussia, the Netherlands, the Republic of Geneva, the

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[95] Bernard (1865, 224): "[I]l nous suffira de bien isoler le seul phénomène sur lequel doit porter notre examen en le séparant, à l'aide de l'expérimentation comparative, de toutes les complications qui peuvent l'environner. Or, l'expérimentation comparative atteint ce but en ajoutant dans un organisme semblable, qui doit servir de comparaison, toutes les modifications expérimentales, moins une, qui est celle que l'on veut dégager." For the differences between Mill's method of difference and Bernard's comparative experimentation, see Schickore (2017, chapter 7).

Archduchy of Austria and the Holy Roman Empire. Many of the authors earned their living as itinerant lecturers or university professors of experimental or natural philosophy, or of physics (Demainbray, Nollet, Jallabert, Beccaria, Gardini, Bertholon). Others funded their research through employment as (court) physicians (Marat, Ingen-Housz, Krayenhoff), and a third group belonged to the clergy (Menon, d'Everlange-Witry). Given this diverse group of experimentalists, it is remarkable that they all conducted comparative experiments: they agreed that the method was essential to draw conclusions about cause-effect relationships. The practice seemed so familiar to physicists (and to journal editors) that they rarely defended it explicitly.[96]

As the following summary shows, it is instructive to consider the control strategies of these physicists in light of their goal of inferring causes from differences.

3.5.1.1 Stabilize (and Monitor) Experimental Conditions

A key control strategy was to treat the test and control plants as equally as possible except for the intervention. The physicists spent many words testifying that they had indeed maintained equal treatment except for electrification. If the test and control plants were consistently different, they felt justified in identifying electricity as the cause of extraordinary plant growth. But if there were no consistent differences, they concluded nothing about the causal role of electricity.

While everyone seemed to agree on this, they differed on how many experimental runs they needed to draw conclusions. Some were content to draw far-reaching conclusions based on single comparative trials. Nollet (1749) and Runeberg (1757), on the other hand, insisted that multiple replications were necessary and that the test and controls should be consistently different before any conclusions could be drawn. After Ingen-Housz's unexpected findings, the problem of control took on new urgency. Ingen-Housz (1789) and van Troostwyk and Krayenhoff (1788) again emphasized the need for many rounds of comparative experiments with consistent results in order to draw reliable causal inferences. Otherwise, one runs the risk of attributing an effect to the intervention when in fact it occurred by chance. In contrast to what Schickore (2011, 516–520; 2017) found in her analysis of seventeenth- and eighteenth-century snake venom experiments, the strategy of many repetitions of the same experiment was less firmly anchored in the minds of the physicists considered here.

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[96] The practice was by no means limited to the circle of "electrifying philosophers." Schickore (2021, 487) found the same for practitioner-authors in agricultural science for the same period.

3.5.1.2 Stabilize Experimental Objects

There was no extensive debate about the choice experimental plants. Runeberg (1757) tested whether electricity would also benefit nut growth, after it had been shown to do so for shrubs, onion plants, and seeds. Köstlin (1775) and Gardini (1784) were careful to study both annual and perennial plants. Ingen-Housz (1789) was the only author to discuss the growth characteristics of different plants, and he argued that cress was more suitable for growth studies than other species because it grew uniformly under the same conditions. Other physicists chose seeds from the same stem, or took shrubs or beans that looked as identical as possible on the outside. Their goal was to minimize the risk that different results were caused by individual variability in the plants.

The task of stabilizing the experimental objects has challenged physiologists ever since. Bernard (1865, 225–226) noted that "no animal is ever absolutely comparable to another." In his view, experimentalists can therefore only assume that the "two animals being compared are sufficiently similar" so that the "difference observed in them as a result of the experiment cannot be attributed to a difference in their organism." Decades later, botanist F. A. F. C. Went (1863–1935) admitted that "the material being experimented with, the living plant, cannot be kept completely constant" (Went 1931, 173). This was a problem because, according to Went (1933, 137), in order to examine the "influence of any factor on a life process," one "needs to keep all other factors constant, let only one change and then wait for the result." Went's own son Frits Went (1903–1990) struggled with the same problem. When his experimental plants showed different responses in reaction to a given intervention, he suspected that they were "not all equal." Went (1928, 27–28) determined the reaction of a "larger number of reaction plants" and was thus able to "arrange the obtained numbers in the form of a binomial curve." This approach, without the statistical model, was exactly what Ingen-Housz (1788) had chosen 140 years earlier. Instead of comparing a few plants like other physicists, he followed the growth of thousands of cress seeds. His contemporaries did not follow suit. Van Troostwyk and Krayenhoff (1788), for example, compared five electrified beans with five non-electrified beans. Bernard spoke of comparing *two* animals.

3.5.1.3 Control Intervention and Detection

Some physicists described how they supplied electricity to their test plants, although we learn little about whether and how they tried to control the intervention. Only one, Köstlin (1775), intentionally varied the amount of electric force in order to evaluate whether the putative effect also varied. Nor did the physicists make fine distinctions in conceptualizing the problem. They took their trials all as contributions to one and the same problem, even though they were investigating different aspects of vegetation such as the formation of new branches (Demainbray), opening of seeds in a given time (Nollet), formation of additional leaves (Jallabert), amount of growth in length during a certain interval (Runeberg), order of germination (Köstlin), strength of growth (de la Cepède), or the recovery of weak plants (Bertholon).

According to the historian of biology Brigitte Hoppe (2010, 107), the plants were electrified without measuring the amount of static electricity in most of the experiments. This fact points to the role the experiments played for the experimenters. In Hoppe's view, they demonstrated the wonders of nature in an entertaining way. In contrast, she credited Ingen-Housz with a genuine interest in plant physiological mechanisms, which would explain his more careful experimentation.

On closer inspection this explanation is not valid. For one, Ingen-Housz's methodological ideas coincided quite closely with those of Nollet (apart from his using thousands of plants). For another, Nollet and Jallabert were actually interested in how electricity promotes plant growth (see Sect. 3.3.2). What did change between the 1740s and 1780s, however, were the conceptions of plant growth and of nutrition. In the 1770s, the "simple" view of plant growth, on which plant material was no more than transmuted water, was undermined (Nash 1957, 344). New studies emphasized the role of light and the atmosphere on vegetation (350). Under these circumstances it is hardly surprising that, in the 1770s, a new generation of physicists attempted to prove that atmospheric electricity promotes vegetation. It is equally understandable that Ingen-Housz, after his studies on the influence of light on plants (Ingen-Housz 1779), was prepared to give a central role in plant development to light.

3.5.1.4 Neutralize Expectations, Report Accurately, and Conclude Safely

For the protagonists considered in this paper, the details of experimental procedure were crucial for assessing the safety of the experimental conclusions. At the same time, though, they often did not have access to those details. In all likelihood, none except for Hales had read Demainbray's reports. Jallabert's experiments were probably known to many through the writings of Nollet, and the same is definitely true for the experiments of Bose and Menon.[97] Another contribution rarely read in the original was that of Runeberg (1757). Nevertheless, van Troostwyk and Krayenhoff (1788) felt free to criticize it.[98]

Similarly, several authors worried about how physicists' expectations affected their work. Keill (1700) warned that experimental results are often distorted by physicists wishing to confirm their favorite theories. Van Troostwyk and Krayenhoff (1788, 141)

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[97] In any case, no one mentioned that Jallabert at first could not find any clear effect.

[98] Despite the fact that Runeberg was careful not to draw any conclusions at all, van Troostwyk and Krayenhoff (1788, 141) were "astonished that some Physicians & especially MM. Achard & Nunebert [sic!], have dared to decide a question of such importance on the basis of so few facts." Most probably, they were referring to Anonymous (1777, 436), an erroneous secondhand report on Runeberg's trial.

suggested that knowing others' conclusions about the same issue might lead physicists to interpret experimental results too hastily, such that they confirm earlier findings. Since famous and capable experimenters had found that electricity positively affected plant growth, their successors were well advised to find the same. Because the physicists believed the matter to be "sufficiently decided by the experiments of their predecessors," they were "satisfied with a single experiment which by chance succeeded in confirming them in the feeling for which they were so strongly advised." The two admitted that "the same thing could have happened to us, if we had wanted to be satisfied with a small number of observations: since our first experiments seemed to confirm the doctrine of electricity in plants." To counteract this dynamic, Ingen-Housz asked his colleagues for a rigorous review of his experiments. Van Troostwyk and Krayenhoff suggested, referring to Bacon, that physicists should not expect experimental outcomes in a way informed by earlier experimental findings. Rather, they should investigate without bias what nature does or can be made to do. In the 1830s, de Candolle advised naturalists to systematize their note-taking. He suggested that journal editors accept only those contributions that met certain methodological standards. We can understand these suggestions as attempts to discipline the community of experimentalists and to ensure the quality of their experiments.

3.5.2 Controlling Complex Systems

De Candolle (1832, 1534–1535) claimed that the "logical method" of "rigorously comparative experiments" was "well known in all the other sciences." Marshall (1779, 17) similarly considered "comparative Experiments" to be the hallmark of science, and necessary for the acquisition of knowledge. In contrast, Albrecht Daniel Thaer (1752–1828), another author of an agronomic textbook who characterized comparative experiments, considered the comparative method appropriate for many, but not all, empirical sciences. According to Thaer (1809, 9–10), comparative experiments are the strategy of choice when experimenters do not have full control over all conditions—for example, when they cannot introduce or remove conditions at will, or even measure and weigh them. In contrast, in an isolated room such as the chemist's laboratory, Thaer thought it possible to perform completely perfect and pure experiments.[99] Bernard explained that it would never be possible, on the other hand, "to experiment with any degree of rigor on living animals" because physiological phenomena are so complex.[100] Comparative experimentation,

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[99] Thaer (1809, 9–10). In the chemical laboratory, naturalists "allow known and measured substances and potencies to interact, cut off the influence of other substances and potencies, and note the success of the experiment."

[100] Bernard (1865, 223): "Les phénomènes physiologiques sont tellement complexes, qu'il ne serait jamais possible d'expérimenter avec quelque rigueur sur les animaux vivants, s'il fallait nécessairement déterminer toutes les modifications que l'on peut apporter dans l'organisme sur lequel on opère."

however, can reduce this complexity and "eliminate en bloc all known or unknown causes of error." In other words, the great advantage of comparative experimentation is that experimenters do not need to have control nor stabilize many potentially relevant conditions at all. Conditions such as the weather or temperature in the plant experiments, can vary as long as they do so in the same way for test and control objects. Thus, the variation poses no threat to the validity of the experiment.

This technique greatly facilitates the study of animals and plants, but it was also used in studying less complex and inanimate systems. For example, Nollet (1749, 140–141) used the comparative approach to study the process of cooling liquids and the influence of electricity on it.[101] Presumably he thought it would be less work to study the process in two separate vessels simultaneously than to control the room temperature precisely during the two successive cooling processes. In another case, Nollet compared the velocity of electrified and non-electrified water streams. In this case, the experiments to be compared did not run parallel, but one after the other. After measuring the flow of electrified water, Nollet used "the same water and the same vase" when he repeated the experiment without electrification. He noted the duration of this flow "for comparison with that of the first" (346). Since little time passed between the test and control instances, Nollet could assume that the environmental conditions had not changed much. We can thus conclude that simultaneous, comparative experimentation is the procedure of choice in two situations: when the process takes a long time, and/or when the process cannot be observed twice on the same object (as in the case of a directed developmental process, such as plant growth).

3.6 Conclusion: The Need for Rigor

This chapter has examined physicists from across Europe who, between the mid-1740s and the mid-1780s, investigated whether electricity promoted plant growth. Reports of their experiments were presented at the meetings of illustrious societies like the Académie Royale in Paris, the Royal Society in London, and the Royal Swedish Academy of Sciences in Stockholm. The controversy attracted attention even beyond the circle of practicing experimental philosophers.[102] Ingen-Housz's experiments by no means settled the question. De Candolle (1832, 1097),

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[101] Nollet described this experiment as follows: "I filled two cylindrical glass vases of the same height and capacity with water; I plunged the ball of a very sensitive thermometer into one and the other, so that it did not reach the bottom of the vessel; I put the whole thing in a hot water bath until the liquor of the two thermometers had risen to 40 degrees; then I placed one of the two vessels on the metal cage to be electrified and I put the other one on a table a little apart, but in the same place. I observed the two thermometers, whose constant reading on both sides taught me that electricity neither delayed nor accelerated the cooling."

[102] Theologian Samuel Miller (1803, 27), for example, wrote that "the correction of former errors, with respect to the influence of electricity on vegetables, by Dr. Ingenhouz, may be considered among the most interesting of recent improvements" in the study of electricity.

who thought it probable that electricity stimulated plant life, suggested that the subject "must be elucidated by precise experiments under the direction of a physicist familiar with the phenomena of plant life." However, he warned that "such comparative experiments are difficult to rid of all causes of error" (1094).

This impression was shared by Ingen-Housz and his contemporaries. The controversy reminded them how error-prone experimental work is and demonstrated the difficulty of systematically investigating causes and effects (Schickore 2021, 502; Schickore 2023). Some authors used the opportunity to call for stricter methodological standards, hoping that increased rigor would help to uncover the secrets of nature more efficiently.[103] The example illustrates that practicing experimentalists have given a lot of thought to sources of error. They incorporated these considerations into their study designs and into the organization of their scientific communities. These are compelling reasons for further study of historical practices of experimentation to improve our understanding of how these discussions and practices have evolved.

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[103] According to Senebier (1772, 40), it is necessary for observers to shorten their labours by prescribing a rigorous method for the study of their subjects.

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