Chemical Research on Plant Growth

A translation of Théodore de Saussure's Recherches chimiques sur la Végétation

Translated, with an Introduction,
by Jane F. Hill

Springer

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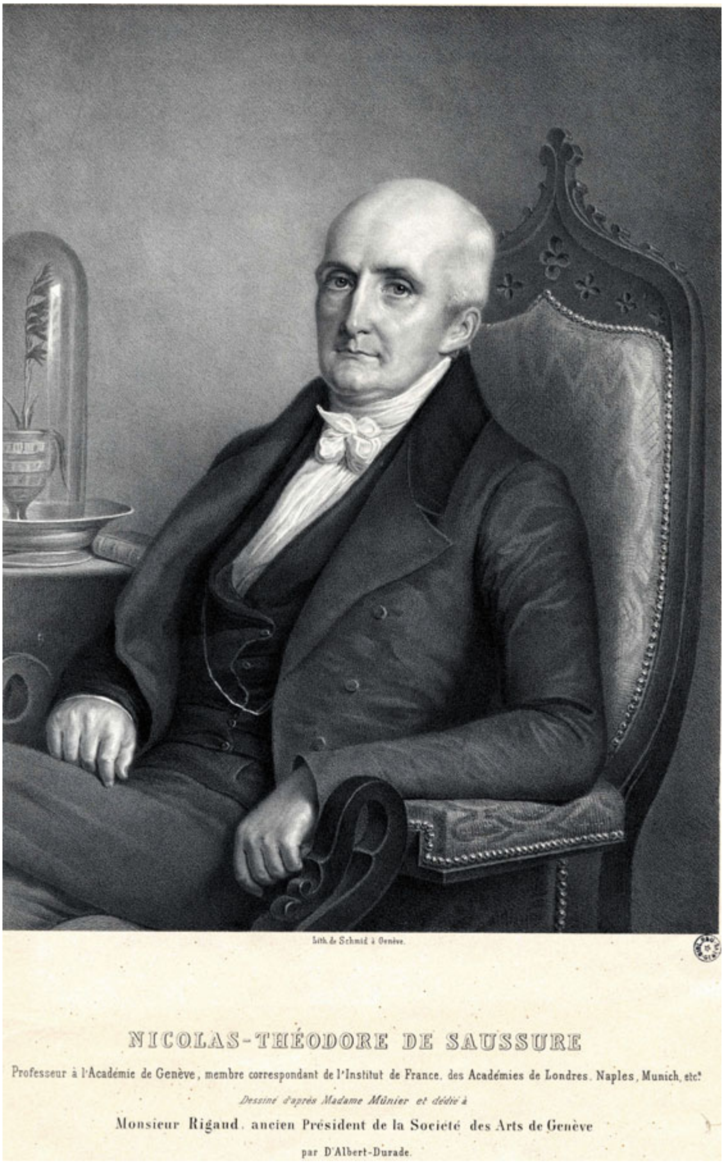

Portrait of Théodore de Saussure. On the table at his side is a "recipient", or receptacle, that encloses a plant; this was an experimental set-up that he used for studying gas exchange between plants and atmosphere of varying composition. (Image courtesy of Bibliothèque de Genève, Centre d'iconographie genevoise)

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Foreword

Théodore de Saussure was the last of the early pioneers of photosynthesis research. He filled in the gaps in the basic understanding of photosynthesis and presented the first concise summary of the field in his 1804 book *Recherches chimiques sur la Végétation,* which is translated into English here for the first time.

De Saussure's work was built on the achievements of his predecessors in plant-nutrition research. In the early 17th century, Jan van Helmont had shown that plants obtain very little of their dry matter from the soil, contrary to the prevailing view. Van Helmont concluded, instead, that plant dry matter is composed mostly of "transmuted" water rather than of soil. In the early 18th century, Stephen Hales demonstrated that "air" is fixed in plant matter, but, at the time he was working, knowledge of the atmosphere was so limited that he was unable to distinguish one kind of gas from another. In the early 1770s, Joseph Priestley discovered the fundamental fact that plants and animals exist in an interdependent relationship mediated by gases: Plants "purify" air that has been "vitiated", or corrupted, by the breathing of animals or burning of candles, and that animals, in turn, thrive in the restored air. In the last quarter of the 18th century, Antoine-Laurent Lavoisier and his colleagues made great strides in chemistry, which enabled rapid advances in knowledge about photosynthesis and plant nutrition during this period. The discovery that the atmosphere is not a homogeneous substance but instead is composed of a number of gases was of great importance for this unfolding understanding of plant nutrition. At about this same time, Jan Ingen-Housz established that light is necessary for plants to thrive and to produce oxygen, and Jean Senebier provided the insight that carbon dioxide is taken up in the production of oxygen by plants.

De Saussure showed that water is a component of plant dry matter (a fact that van Helmont had inferred but not conclusively demonstrated), thus enabling completion of the basic, overall equation of photosynthesis. De Saussure also showed that plant carbon comes mainly from atmospheric carbon dioxide rather than organic carbon in the soil. Further, he showed that plants obtain their minerals from the soil rather than from "transmutations" of other substances by a vital force within the plant, and that minerals are essential for plant growth.

Two German translations of de Saussure's book appeared during the 19th century. This first English translation now makes it readily accessible for English-speaking students, researchers, and other readers who are not familiar with French or German.

Jane F. Hill, known to me for several years as an accomplished historian of photosynthesis research, has written a beautiful introduction for this book (see Translator's Introduction) that places de Saussure's book in a historical context. This introduction, as well as additional supplementary material in the book, incorporates editorial and other suggestions that I made on draft versions of the manuscript. The introduction provides an account of the gradual replacement of primitive ideas about plant nutrition by the more accurate information supplied by the early pioneers, culminating in the work of the last of these early researchers, de Saussure. The fate of the book is also addressed, beginning with its positive initial reception, which was followed by neglect of its fundamental conclusions until their revival by Jean-Baptiste Boussingault and Justus von Liebig in the mid-19th century. The introduction describes how, despite some errors due to the relatively primitive state of knowledge and experimental methodology in de Saussure's day, the book is now widely recognized as a classic and a basic document in the development of the understanding of photosynthesis and plant nutrition. Finally, the life and scientific career of de Saussure are traced in detail.

In Chap. 10, Jane F. Hill presents succinct summaries of each of the nine chapters of de Saussure's book; in addition, she has provided at the end of the book three very important appendices, which provide conversions of units of measurements, a glossary of terms, and fleshed-out bibliography for most of the approximately 100 authors cited by de Saussure in his text.

I end this *Foreword* by making a reference to the Preface (pp. xxiii–xxvi), and three editorials (pp. 5–35) in a 2005 book *Discoveries in Photosynthesis*, edited by Govindjee, J. T. Beatty, H. Gest, and J. F. Allen, Springer, Dordrecht; they provide a link to the many discoveries and discoverers in photosynthesis research since de Saussure's pioneering work.

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Preface to the Original de Saussure Book

The object of the research that occupies me in this work is the influence of water, air, and humus on plant growth. I do not pretend, however, to fathom all aspects of this immense subject. I address the questions that can be decided by experiment and forgo those that can give rise only to conjectures. In natural history, the facts alone lead to the truth. In following this route, one is forced to acknowledge that the discovery of the means used by nature for the development of plants and for the composition of their substance must remain beyond our grasp for a long time. The solution of these questions often requires information that we do not have. It requires exact procedures for the analysis of plants and a full knowledge of their organization.

The functions of water and gases in the nutrition of plants, and the changes that plants produce in their atmosphere, are the subjects that I have investigated most deeply. The observations of Priestley, Senebier, and Ingen-Housz opened the road that I have traveled, but they did not attain the goal that I set myself. If imagination has sometimes filled the gaps that they left, it is by conjectures whose inadequacy has always been shown by their unintelligibility and inconsistency. For my eudiometric tests, I used either potassium sulfide [“*sulfide of potash*”] or phosphorus.[1] These procedures allowed me to achieve in my analyses an accuracy that the nitrous gas eudiometer used by my predecessors could not attain.

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[1] In this work, when I have given readings of the phosphorus eudiometer, they have always been cleared of the error that nitrogen gas can introduce through the expansion it undergoes in dissolving the phosphorus. Berthollet determined precisely the correction that this expansion requires. There is, however, a case in which this correction should be negligible: it is when the condensation of oxygen gas is observed at the precise moment at which it has reached its maximum, or when the phosphorus ceases to give off white vapors. In this operation, the nitrogen gas begins to expand only when it contains no more oxygen gas. In the first moments of the disappearance of the oxygen gas, the expansion of the nitrogen gas is undetectable. The expansion usually only reaches its extreme limit 24 hours after the complete absorption of the oxygen gas.
For these experiments, I used rapid combustion and the bent tube indicated by Giobert (*Analyse des Eaux de Vaudier*). I tilt the eudiometer when the phosphorus is melting, so that the phosphorus flows and spreads over the full length of the tube. With this procedure, the analysis of air is completed in less than half an hour, and at this time needs no correction.

My research leads me to conclude that water and air contribute more to the formation of the dry matter of plants growing in a fertile soil than does even the humus matter that they absorb in aqueous solution through their roots.

I also focus on a subject that has given rise only to hypotheses: the origin of the ash of plants. I investigate, through numerous experiments, the principles by which these ashes vary, in both amount and composition, depending on the season and the nature of the plants and their different parts. This work brought me several new observations, which show that all the questions that I have just stated can be resolved without attributing to vegetation creative forces and transmutations contrary to known observations.

In experiments on plant growth, so many diverse and unforeseen factors tend to influence the results that one should never fail to report all the accompanying circumstances. The details I provide on this subject will serve to determine the degree of confidence that can be placed in my research. They will prevent contradictions that arise from differences in method. They will explain errors that I cannot flatter myself to have avoided in a series of long, difficult experiments, whose results are perhaps applicable only to the species of plants that I examined. The path that I set upon is without doubt dry and fatiguing, but if one considers that the goal towards which it is directed is the improvement of agriculture, one will bear with its difficulties and excuse its shortcomings.

Acknowledgments

The translator gratefully acknowledges her deep indebtedness to a number of individuals who offered encouragement and support during the course of this project and who consented to read critically, review, or proofread the entire manuscript or parts of it. First, I want especially to thank Govindjee, Professor Emeritus, Department of Plant Biology, University of Illinois at Urbana-Champaign, Urbana, IL, USA, without whose encouragement and help this book would not have been possible, and whose advice, editing, and insightful comments were invaluable.

I am also indebted to Mary Ellen Bowden and Robert Kenworthy, of the Chemical Heritage Foundation, Philadelphia, PA, USA, for their generous assistance with chemical terminology.

Others to whom I am deeply grateful include Marilyn Schiff for her unfailing encouragement and for advice on French usage; and Anita Baker-Blocker, Dennis G. Baker, and Barbara Wolanin for critical readings and comments on various parts of the manuscript.

I could never have completed this project without the tremendous encouragement and support of my husband, William A. Hill, whose tireless and careful proofreading of drafts was of immeasurable value and is deeply appreciated.

The staffs of various libraries were of great assistance in helping track down reference works, especially older works, of limited accessibility, cited by Théodore de Saussure in his book. I am particularly indebted to Eric Frazier, Reference Librarian, Rare Book and Special Collections Division, Library of Congress, Washington, D.C., USA, for generous help in finding many of these obscure references. Staffs at a number of other libraries also provided valuable assistance: National Library of Medicine, Bethesda, Maryland, USA; American University Library, Washington, D.C., USA; United States Department of Agriculture, National Agricultural Library, Beltsville, Maryland, USA; Public Library System of Montgomery County, Maryland, USA; Bibliothèque de Genève, Geneva, Switzerland; Bibliothèque nationale de France, Paris, France; and Biblioteca Accademia delle Scienze, Turin, Italy.

Translator's Introduction

Summary: In his 1804 book, *Recherches chimiques sur la Végétation*, Théodore de Saussure amalgamated his own research results with the findings of earlier investigators of photosynthesis to produce the first concise summation of the basic facts of plant nutrition. Important advances that he contributed to the overall knowledge in the field were that water is incorporated into the dry matter of plants; that plant carbon is derived from the carbon dioxide of the air, not from humus or soil; that the minerals in plants are absorbed from the soil, not created by a vital force; and that minerals are essential to plant growth.

In an introduction written by the translator, de Saussure's book is placed in historical context and details of his life are presented. In addition, the translator has provided a glossary of terms and a list of de Saussure's more than 100 citations, with fuller reference details than were given in the original book.

Key words: Théodore de Saussure; photosynthesis; plant nutrition; history of science; early pioneers of photosynthesis research; plant mineral nutrition; roles of carbon dioxide, water, and minerals in plant growth and development; chemical revolution; agricultural chemistry; 18th- and 19th-century agricultural science, agronomy, and soil science; "humus" theory of plant nutrition; historical importance of *Recherches chimiques sur la Végétation*; reference details for literature cited by de Saussure; chemical equation of photosynthesis

Between the early 17th and early 19th centuries, pioneering researchers unraveled the fundamentals of photosynthesis and plant nutrition. The last of these pioneers, plant physiologist Théodore de Saussure (1767–1845), of Geneva, Switzerland, consolidated his own findings with the contributions of earlier researchers to provide the first concise, unified concept of photosynthesis and the physiology of plant nutrition in his book, *Recherches chimiques sur la Végétation* (de Saussure 1804a). De Saussure's most important findings were that plants incorporate water into their dry substance; that they obtain their carbon from the atmosphere, not from humus in the soil, as many others thought; and that their minerals come from the soil, not from "transmutations" of other elements within the plants, as was widely believed at the time. He also showed that minerals are essential to growth, and he performed the most detailed analyses of the minerals in plant ash to that date. De Saussure's demonstration that plants incorporate water enabled completion of the basic, overall chemical equation of photosynthesis, which states that, from the simple, inorganic substances water and carbon dioxide, plants, using the energy of sunlight, produce fixed carbon, with the liberation of oxygen gas.

Recherches chimiques sur la Végétation met with initial acclaim and was soon translated into German (de Saussure 1805), but the book's fundamental conclusions were then ignored for the next several decades. In the mid-19th century, the book's worth was re-established, ensuring de Saussure an enduring reputation.

Throughout the 19th century, even during the decades of eclipse, chemists and plant scientists, including some of the most illustrious in the field, cited the book extensively for various purposes (e.g., Thaer 1809–1812; Davy 1813; Keith 1816; Liebig 1840; Schleiden 1842; Boussingault 1860–1891; von Sachs 1875; Pfeffer 1881; Aikman 1894). Late in that century, after the book's conclusions had once again been recognized, it was translated into German a second time (de Saussure 1890). During the 20th and early 21st centuries, English-language writers on photosynthesis and other aspects of plant science, agriculture, and the history of chemistry praised the book highly, and many of them quoted passages that they had translated into English (e.g., Harvey-Gibson 1919; Sherman 1933; Browne 1944; Reed 1949; Nash 1952; Gabriel and Fogel 1955; Partington 1962; Aulie 1970; Morton 1981; Arnon 1991; Magiels 2010). A facsimile edition of the original French text was published in 1957, and a second facsimile edition appeared in 2010. Despite the book's widely acknowledged importance, however, the full text has not been available in English until now.

Plant Nutrition Concepts Before the Pioneering Research on Photosynthesis[1]

Advances in the knowledge of both chemistry and plant nutrition were long hampered by vague, untested assumptions, such as the idea that one substance could be "transmuted" into another. This concept had been invoked since the alchemical Middle Ages, not only in connection with the attempt to change base metals into gold, but also as an all-purpose explanation for a wide range of natural phenomena. Scientific advances were also hindered by the doctrine, which had persisted from the time of Aristotle (384–322 B.C.), that all matter is composed of just four elements: earth, air, fire, and water. In addition, a vital force was thought to control processes within organisms, operating differently from the principles that govern nonliving matter.

With knowledge so limited, plants were understandably thought to obtain all their nutrients from the soil through their roots, which were seen as somewhat analogous to animal mouths. This view persisted into the late 18th century in many quarters, even as experimental evidence was accumulating showing that air and possibly water, in addition to soil, contributed to plant nutrition. English agricultural writer and farmer Jethro Tull (1674–1741), in his widely read book *The New Horse-Houghing*

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[1] The term "photosynthesis" did not originate until the late 19th century (see Gest 2002).

Husbandry (1731 and many subsequent editions), popularized this view, with his advocacy of an agricultural system based on pulverizing the soil to such a fine powder that the particles could easily be taken in by plant roots.

In a detailed discussion of early ideas about plant nutrition, Browne (1944, p. 256) credits German physician and chemist Georg Ernst Stahl[2] (1660–1734) with making the first attempt at a scientific explanation of soil fertility, especially the benefits of organic manures. Stahl attributed soil fertility to an oily or fatty principle (a "phlogistic fatness"). Similarly, Külbel (1741) assumed that the main source of plant fertility was a fatty magma ("*magma unguinosum*") occurring in humus. Other promulgators of this concept of soil fertility included Home (1757), Wallerius (1761, per Browne 1944, p. 256), and Dundonald (1795).

The idea soon arose that humus had to be brought into solution by oxidation and fermentation before it could be used by plants (Browne 1944, p. 256), and out of this idea grew the "humus theory" of plant nutrition, which held that plants obtain their carbon from water-soluble organic matter in the soil (Waksman 1936; Russell 1976; Feller and Boulaine 1987; Boulaine 1994; van der Ploeg et al. 1999; Feller and Manlay 2001). Although some fertilizers, such as salts and lime, were acknowledged to promote plant growth, they were thought only to aid the decomposition of humus and the dissolution of organic matter in the soil water (van der Ploeg et al. 1999, p. 1057).

Often intertwined with the humus theory was the idea that a vital force in the plant created the minerals that had long been detected in plant ash. These minerals were thought to originate by transmutation of other constituents of plant matter (van der Ploeg et al. 1999, p. 1057), not from the external environment.

Contributions to Knowledge of Plant Nutrition and Physiology by the Photosynthesis Pioneers who Preceded Théodore de Saussure[3]

One hundred years before Külbel (1741) posited a fatty magma as the main source of plant fertility, Flemish physician Jan van Helmont[4] (1579–1644) took the first

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[2] Stahl is best remembered for his advocacy of the phlogiston theory of chemistry, which held sway during the 18th century before it was upended by the chemical revolution late in that century.

[3] Publications focusing on the work of the early pioneers of research on photosynthesis and plant nutrition include Rabinowitch (1945, Vol. 1, Chap. 1); Nash (1952); Rabinowitch and Govindjee (1969, see Introduction); Hill (1970); Rabinowitch (1971); Egerton (2008); Magiels (2010); and Hill (2012). The early pioneers are treated within the larger context of the history of chemistry by Partington (1957, 1962) and Ihde (1964); within the context of agricultural chemistry by Browne (1944); and within the context of soil science by Russell (1976). Overviews of photosynthesis research that go beyond the era of the early pioneers, up to various later dates, include von Sachs (1875, 1890 English translation); Spoehr (1919); Sherman (1933); Morton (1981); Höxtermann (1992); Huzisige and Ke (1993); Govindjee and Gest (2002); Govindjee et al. (2003); Govindjee et al. (2004); Govindjee and Krogmann (2004); Morton (2008); and Nickelsen (2010).

[4] For more on van Helmont, see Redgrove and Redgrove (1922), Harvey (1929), Pagel (1972), and Newman and Principe (2002), among many other sources.

major step towards a broader, more accurate view of plant nutrition. He was the first to demonstrate convincingly that soil could not be the only source of plant nutrients. He planted a 5-pound willow tree in an earthen pot in which he had placed 200 pounds of oven-dried soil, and covered this vessel with a perforated lead shield to exclude foreign matter. He added only water to the pot for 5 years. After removing the tree from the soil and re-weighing it (without the leaves that had been shed during the 5-year period), and also weighing the re-dried soil, he found that the tree had gained about 164 pounds and the soil had lost only 2 ounces. From these results, van Helmont concluded that the solid substance of plants is formed not of soil, but instead entirely of water (one of the four Aristotelian elements) that has been transmuted into solid matter, or "earth" (another of the Aristotelian elements). Van Helmont's experiment was briefly reported in a posthumously published collection of his writings (van Helmont 1648). Although he was correct in his conclusion that water is an important component of plant dry matter, he did not prove a role for water (an achievement belonging to Théodore de Saussure, 150 years later), and he mistakenly ruled out soil and the atmosphere as other sources of plant nutrients. He did establish, though, how little of their solid matter plants derive from the soil (Hill 1970).

The first experimental evidence showing that soil does play a role in plant nutrition was provided at the end of the 17th century, by English naturalist and geologist John Woodward (1665–1728). Woodward (1699) showed that plants grown in water containing impurities or soil gained more weight than did plants grown in distilled water. He concluded that the impurities or soil were the essential nutrient and that water was merely a vehicle carrying them into the plant.

Also in the late 17th century came evidence that air might be involved in plant nutrition, when the first-ever comprehensive studies of plant anatomy were undertaken, by Malpighi (1675–1679) and Grew (1682). They found that leaves contain minute pores (stomata) in their surfaces and have internal structural features that appeared capable of functioning as air ducts. Then, in the early 18th century, the first functional evidence was found for the involvement of gases in plant life: Stephen Hales[5] (1677–1761), an English physiologist, chemist, and cleric, reported in his classic book *Vegetable Staticks* (1727) that "air" becomes "fixt" (solidified) in plant dry matter and thereby loses its "elastick" (gaseous) state. Because Hales accepted the prevailing doctrine that all matter is composed of the four Aristotelian elements, however, he did not distinguish one kind of gas from another.

During the late 18th century, the understanding of photosynthesis progressed rapidly, as the chemical revolution yielded both new chemical knowledge and improved experimental techniques that could be applied to the study of plant nutrition and physiology. In turn, research in plant nutrition contributed to knowledge of chemistry (see Partington 1962; Ihde 1964). Of the many European natural philosophers who contributed to the dramatic advances in chemistry during that period, the

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[5] For biographies of Hales, see Clark-Kennedy (1929) and Allan and Schofield (1980).

French researchers and theorists, led by Antoine-Laurent Lavoisier[6] (1743–1794), stand out. Although Lavoisier did not advance the understanding of photosynthesis directly, his work helped make rapid progress in that field possible. For example, in 1770, Lavoisier took an early step towards overthrowing the doctrine of transmutation when he showed, through an experiment with water in a vessel, that the water was not transmuted into earth, or dry substance. He thereby cast doubt on van Helmont's conclusion that plant dry matter consists of transmuted water. Lavoisier also played a key role in establishing that there is a broad array of chemical elements, which are definable much more precisely than the four, all-encompassing, Aristotelian elements. To qualify as an element, a substance had to be shown to persist through complex chemical reactions and not be decomposed into more fundamental substances.

Another chemical advance important for photosynthesis research was the isolation of individual gases and the determination of their composition. This work was made possible by improved methods of catching gases, transferring them from vessel to vessel, and studying their properties (Rabinowitch 1971). Much of this work in "pneumatic" chemistry was done by British researchers during the third quarter of the 18th century (see Ihde 1964, Chap. 1). The work was greatly aided by the development of a piece of equipment, the pneumatic trough, which consisted of a receiver, or vessel, inverted in a bath of water or mercury to trap gases.[7]

The first gas to be isolated was carbon dioxide. It had originally been discovered by van Helmont (1648), who did not appreciate its significance. The gas was rediscovered in 1754 by Scottish physician Joseph Black (1728–1799), who called it "fixed air" (1756). Hydrogen was discovered by Henry Cavendish (1731–1810) in 1766, and nitrogen by three separate researchers in the early 1770s (see discussion by Partington 1962, p. 222, pp. 263–265).

English chemist and theologian Joseph Priestley[8] (1733–1804) broadened awareness that the atmosphere has a functional role in living organisms, with his startling discovery that plants "restore" air that has been "injured" by the burning of a candle or the breathing of an animal (1772, p. 168). Priestley, however, could not explain the relationship.

Priestley also developed a chemical technique, which he called the "nitrous air test" (but which actually used nitric oxide), to measure the degree of "purity" of a gas sample (1772, pp. 210–216).[9] The nitrous air test came to be widely used by

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[6] Biographies and other treatments of Lavoisier include McKie (1952); Guerlac (1961, 1975); Holmes (1985, 1998); Donovan (1993); Poirier (1996); and Bell (2005).

[7] For a history and description of the pneumatic trough, see Badash (1964) and Parascandola and Ihde (1969).

[8] For a comprehensive treatment of Priestley, see the two-volume biography by Schofield (1997, 2004).

[9] The nitrous air test was based on the reaction between oxygen and nitric oxide (NO), a colorless, insoluble gas. The solubility of the reaction product, nitrogen dioxide (NO_2), in water caused a decrease in the volume of the gas phase and thus provided a measure of the amount of oxygen in the original sample (see discussions by Conant 1950; Nash 1952; Partington 1962; Ihde 1964). De Saussure used the nitrous air test in some of his work with gases and plants, although by

18th-century chemists. Although Priestley did not realize it when he developed it, this was a test for oxygen, a gas that he himself was to discover only two years later, in a nonbiological context (see Priestley 1775). He named his new gas "dephlogisticated air" (as distinguished from "phlogisticated," or "bad," air). Lavoisier (1778) renamed the gas "*oxygine*"—i.e., oxygen.

The discovery of oxygen was the central event that enabled Lavoisier to overthrow the older, phlogiston theory of chemistry, which had dominated through most of the century (Conant 1950, p. 49), in favor of a new, more scientifically based, oxygen chemistry. The phlogiston theory, promulgated by Stahl (see Partington 1957, pp. 85–89), held that combustible materials contain an "inflammable principle," called "phlogiston," which is transferred to the atmosphere upon burning or, in the case of metals, upon calcination. Once the atmosphere had become "phlogisticated," or saturated with phlogiston, it could support no additional burning or calcination, nor could it support animal life, because respiration was presumed to saturate the air with phlogiston given off by slow burning within the animal body. In combustion and respiration, then, the role of phlogiston was essentially the converse of the role that oxygen was shown to play.[10] Armed with the new knowledge of oxygen, Lavoisier (1783) launched a vigorous attack on the phlogiston theory. He and his colleagues (de Morveau et al. 1787) revised and modernized chemical terminology, removing the phlogiston-associated language. Soon thereafter, Lavoisier set forth his new approach to chemistry in his pioneering book *Traité élémentaire de Chimie* (1789). Most of the chemical elements recognized in this book are still accepted today.

Late-18th-century chemical discoveries that were especially important for the understanding of plant and animal physiology included: the finding that fixed air (carbon dioxide) is composed of carbon and oxygen (Lavoisier 1781); that water, rather than being an element, is a compound of hydrogen and oxygen (see discussion by Partington 1962, p. 344 ff); and that fixed air is different from "mephitic" air (nitrogen) (see Partington 1962, p. 263). In physiology, animal respiration was demonstrated to be a process of oxidation that provides heat for the body (Lavoisier 1777; Lavoisier and de la Place 1780).[11]

Lavoisier developed the technique of analyzing organic substances by combustion (Holmes 1985, Chap. 9), and used it to analyze spirit of wine (alcohol), olive oil, and wax (1784). He noted (1789, 1790 English translation, p. 123) that the "true" constituent elements of plants are hydrogen, oxygen, and carbon.

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the mid-1780s, this technique of "eudiometry," or measurement of oxygen-gas content, had been largely abandoned as imprecise and unreliable (Golinski 1992, p. 93). In his research on plants, de Saussure preferred to use phosphorus or potassium sulfide in his eudiometric tests. For the history of the development of eudiometric methods, see Benedict (1912), Golinski (1992, pp. 117–128), and Magiels (2010, Chap. 4).

[10] See Conant (1950) and Nash (1952) for detailed discussion of the phlogiston theory and its upending by oxygen chemistry.

[11] For details about Lavoisier's findings in physiology, see Holmes (1985).

A major advance in the understanding of photosynthesis itself came in 1779, when, during a single summer of research, Dutch physician Jan Ingen-Housz (1730–1799) made the critical discovery that sunlight is essential for plants to thrive. Based on this pivotal finding, Ingen-Housz has been credited by some (e.g., Gest 1997; Magiels 2010) with the discovery of photosynthesis.[12] Ingen-Housz (1779) reported his results during the autumn immediately following his summer of work.

Ingen-Housz achieved his important insight about light partly because he used an innovative experimental set-up. Priestley's experiments were done on whole plants. Ingen-Housz, however, made use of a 1754 observation by naturalist Charles Bonnet (1720–1793) that bubbles form on leaves that are submerged in water. Bonnet had not recognized their significance but, with the benefit of the increase in knowledge of gases over the intervening years, Ingen-Housz was able to demonstrate that the bubbles consist of dephlogisticated air and that only illuminated leaves produce this gas. Ingen-Housz had no idea of the identity of the starting material for the production of dephlogisticated air, however, instead invoking a vague "transmutation," excited by light, within submerged leaves, and a different process—a withdrawal of phlogiston from the air—by plants in aerial environments.

Ingen-Housz also showed that the non-green parts of plants, and the green parts when not illuminated, produce fixed air. Thus he discovered respiration in plants, two years after Lavoisier (1777) had reported it in animals. Priestley, in contrast, with his use of whole plants, had difficulty obtaining clear results in his later experiments because his experimental material contained both non-green, carbon dioxide-producing tissues and green, oxygen-producing ones and because he did not understand the requirement for light in photosynthesis. Ingen-Housz noted that production of dephlogisticated air by plants during the day exceeded their production of fixed air by night, so that overall the improvement of the atmosphere during the day far outweighed its vitiation, or corruption, at night (1779, p. 47).

Swiss clergyman and naturalist Jean Senebier[13] (1742–1809) contributed the next major insight on photosynthesis. He repeated many of Ingen-Housz's experiments, with mostly similar results, and he did not always give Ingen-Housz due credit (Magiels 2010), but Senebier (1782) was the first to establish that fixed air is consumed in a plant's production of dephlogisticated air. Senebier (1782, 1783) also found that the amount of dephlogisticated air produced is roughly proportional to the amount of fixed air available to the plant. (This was a line of research that de Saussure was to expand upon and refine.) Further, Senebier (1782) pinpointed the green, fleshy parts of leaves (the parenchyma) as the sites where fixed air is transformed into dephlogisticated air.

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[12] Rabinowitch and Govindjee (1969, p. 7) conclude that each of the early pioneers made an invaluable contribution to the understanding of photosynthesis and that "there is fame enough to share among them".

[13] Kottler (1973) provides a comprehensive historical description of the scientific contributions of Senebier.

In contrast to Ingen-Housz, Senebier considered the production of fixed air by plants to be an indication of stress or disease caused by the unnatural growing conditions in experimental situations, rather than a natural process carried out by all plants. This subject became a major area of contention between the two men.

During the final two decades of the 18th century, Ingen-Housz (1787, 1789, 1796) and Senebier (1783, 1788, 1791, 1792, 1800) revised their views, speculated, and quarreled about various aspects of plant metabolism.[14] The two men agreed that carbon has a nutritional role in plants: Senebier (1791, p. 164; and 1800, Vol. 3, p. 151) stated that carbon combines with plant constituents, and Ingen-Housz (1796, p. 4) concluded that plants derive some of their most essential substances—such as their acids, oils, and mucilages—from the two elements that compose carbon dioxide (or carbonic acid, as it was then called). They disagreed about the source of plant carbon, however, right to the end: Ingen-Housz (1796) attacked the humus theory, conjecturing that plants growing in the open air obtain almost all of their carbon from the atmosphere, whereas Senebier (1800, Vol. 3, p. 148 ff) clung to the idea that they obtain it through their roots, from carbon dioxide dissolved in the soil solution. The quantitative data necessary to settle the matter would be supplied by de Saussure.

Senebier, in his writings on fixed air, initiated an error of interpretation that was to be perpetuated by Ingen-Housz (1796), de Saussure, and many others, for some 150 years. This was the idea that the oxygen liberated in photosynthesis comes from carbon dioxide rather than from water. This view seemed logical based on the close equivalence between the volumes of carbon dioxide absorbed and oxygen released. The idea is often attributed to Ingen-Housz (1796)—by, for example, Arnon (1971, 1991); and Raven, Evert and Eichhorn (1999)—but, although Ingen-Housz briefly stated this view in 1796, he confusingly also mentioned water as a source of oxygen, and, more importantly, it was not Ingen-Housz, but Senebier, who earlier had done many, repetitious studies and analyses that suggested carbon dioxide as the precursor of the oxygen released. Senebier's very verbosity may have eclipsed his findings, and Ingen-Housz, with his concise summary of the field in 1796, was more easily read and comprehended (Hill 2012, p. 792). It is now known that water, not carbon dioxide, is the source of the oxygen gas liberated in green-plant photosynthesis.

At century's end, unresolved questions also remained about the source of plant nitrogen and hydrogen. Lavoisier (1784) had found that plant matter contained abundant hydrogen, and Berthollet (1785) had found nitrogen in both plants and animals, but from what source did plants obtain these two important nutrients?[15] Because the most logical source of plant hydrogen was water, there was conjecture (by, e.g., Berthollet 1786; Senebier 1792) that plants obtain it from that liquid. Senebier's opinion (1792, p. 208) was based on his demonstration that plants transpire much

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[14] See Nash (1952) for a detailed, comparative account of the ideas of Ingen-Housz and Senebier during this period.

[15] Adequate concentrations of the four major nutrients in dry plant tissue are now considered to be, by weight, 45 % carbon, 45 % oxygen, 6 % hydrogen, and 1.5 % nitrogen (Stout 1961, as cited by Raven et al. 1999, p. 728).

less water than they take in through their roots. The contribution of water to plant dry matter was to be established by de Saussure.

For plant nitrogen, the most logical source seemed to be the large reservoir in the atmosphere, which is approximately four-fifths nitrogen gas, by volume. Ingen-Housz (1796) believed in an atmospheric source, but Senebier (1800, Vol. 3, p. 159) thought that it came from the soil, along with the carbonic acid that he also believed was absorbed from there. Senebier failed to establish that soil was the source, however, and de Saussure, although speculating correctly against gaseous nitrogen as the source, did not settle the matter.

In addition, neither Ingen-Housz nor Senebier clarified the source of plant minerals. The idea that a vital force within plants formed these substances was still prevalent when these two researchers completed their work. Senebier (1791, p. 252) pondered the question of vital force versus environment, but did not provide an answer.

Although both Ingen-Housz and Senebier clung to the phlogiston concept until late in life, both of them eventually re-interpreted their findings, to a large extent, in terms of Lavoisier's chemical system. Senebier probably adopted the new system about 1789 (Smeaton 1978); Priestley never did.[16]

Of the later publications by Senebier and Ingen-Housz, it was Ingen-Housz (1796) that was the most succinct and that, according to Nash (1952, p. 105), provided "a fair approximation to the conceptual scheme that we now hold to be correct.... [A] view representing a closer approach to our present conceptual scheme was not proposed until 1804" (in de Saussure's book). Ingen-Housz (1796), in an appendix to an obscure British government publication, presents the first formulation of plant nutrition and the photosynthetic process in accordance with the new chemical theory (Rabinowitch 1945). Although not entirely consistent in his terminology, Ingen-Housz mostly used "carbonic acid" rather than "fixed air," and "oxygen" or "vital air" rather than "dephlogisticated air." He used "carbon," but also sometimes "coal," for the element. His scheme, however, included only brief mention of the idea of the decomposition of water, and he paid scant attention to a possible nutritional role for minerals or to the question of how plants obtained minerals.

Thus, at the dawn of the 19th century, with the contributions of Priestley, Ingen-Housz, and Senebier completed, quantitative data on photosynthesis and plant nutrition were still incomplete and critical questions remained.

De Saussure Provides Extensive Quantitative Data to Address the Important Questions

The position of the Swiss plant physiologist Théodore de Saussure in the history of the early research on photosynthesis has been compared to that of Jan van Helmont, as both were transitional figures: Van Helmont was an alchemist, but he addressed

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[16] See Holmes (2000), however, for the view that Priestley's concept of phlogiston was only loosely allied with older phlogiston theories.

subjects that became major fields of study in the burst of chemical and plant physiological research of the late 18th century. De Saussure, in turn, completed the basic experimental work and provided a unified theoretical interpretation of the field; he also opened the way for further experiment and advances in knowledge (Nash 1952, p. 106).

Younger than his immediate predecessors, Théodore de Saussure did not have to labor under the weight of the phlogiston theory, because, by the time of his research, that theory had largely been replaced by Lavoisier's new chemistry. In accepting the new chemistry as a young man, Théodore was probably following the lead of his father, the noted scientist Horace-Bénédict de Saussure (1740–1799), who as early as 1788 adopted the new system and was probably the first scientist in Geneva to do so (Smeaton 1978).

With his more modern outlook, Théodore de Saussure recognized from the start that a facile recourse to the "transmutation" of one kind of substance into another as an explanation of natural phenomena was no longer tenable. Instead, he approached problem-solving analytically, designing experiments that were carefully targeted to answer important physiological questions of the day. He followed Lavoisier's lead and used a balance-sheet approach, carefully recording his quantitative results. He was thus able to build quantitatively on his predecessors' qualitative observations. Naturally a cautious man, not given to extravagant speculation as were his immediate predecessors in the field, he stuck to facts as revealed by experiments.

In many of his experiments, de Saussure used whole plants, which he was careful to maintain in a normal, healthy state. Unlike his predecessors, he did not simply observe whether the plants thrived or wilted, but measured their weight gain, and he did not merely determine whether the air "improved," but measured the amounts of its gaseous constituents. Also unlike his predecessors, he made liberal use of controls, comparable samples for determining quantities of substances in the initial plant material, and repetition of experiments with different plant species or plant organs. He made errors, some due to the primitive experimental techniques of his time, and others—of interpretation—due to the still-rudimentary state of knowledge. Nevertheless, he was able, through his thorough and wide-ranging investigations, to come close to identifying the sources of most of the major elements found in mature plants, and to determine the paths by which plants obtained them.

Some of the important questions de Saussure addressed—and the conclusions that he reached based on his experiments—were:

1. What is the quantitative relationship between the carbon dioxide taken in and the oxygen released by green plants in the light?
   De Saussure found that, under many conditions, there was a close equivalence between these quantities, but that in general somewhat more carbon dioxide was taken in than oxygen released.[17] This led him to conclude that plants retain, in their substance, a small amount of residual oxygen from the carbon dioxide. He

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[17] The finding that the carbon dioxide taken up exceeded the oxygen released may be attributable to a slow reaction of oxygen with the chemicals in the particular eudiometric tests de Saussure used (Morton 1981, p. 360, note 74).

often found, erroneously, that plants released a small amount of nitrogen gas to the atmosphere.

De Saussure's findings supported Ingen-Housz's view that, in ordinary air and with exposure to light, the uptake of oxygen and emission of carbon dioxide are, in a healthy plant as a whole, invariably exceeded by the uptake of carbon dioxide and emission of oxygen. These two processes (photosynthesis and respiration), although directly opposed, seemed to be inextricably linked in plant metabolism.

2. Do plants obtain their carbon mainly from the soil or the atmosphere?

   Using experimental plants grown with their roots in distilled water and their shoots in the open air, de Saussure provided the first convincing demonstration that plants assimilate carbon mainly from atmospheric sources. This was a major blow to the humus theory. He dealt that theory a further blow by showing that carbon is proportionally more abundant in the humus than in the plants whose decomposition gave rise to the humus, due to the loss of hydrogen and oxygen, in the form of water, from the humus. If plants were taking a large amount of carbon from the soil, he reasoned, there should be a decrease, not an increase, in the carbon in the humus. Further, he found that, in humus extracts, which are the soluble part of the humus and the only part available to plants, there is too little solid substance to account for the dry weight gained by plants that are growing and developing in the humus. Further, he found that, in atmospheres to which he had added a limited amount of extra carbon dioxide, plants in the sun gained more in dry weight than they did in ordinary air.
3. Does some of the oxygen released by green plants in the light come from the decomposition of water?

   De Saussure evaluated other authors' claims that some of the oxygen released by plants comes from water rather than carbon dioxide. He generally found no evidence for this. In the case of a plant growing in a normal day/night cycle, in an oxygen-free atmosphere, the plant may release several times its own volume of oxygen gas, but de Saussure did not consider this an indication of the breakdown of water, with release of the water's oxygen. Instead, he reasoned that the plant, having had no contact with oxygen gas during its early stages of growth, formed carbon dioxide entirely from the carbon and oxygen of its own substance and then decomposed it. In contrast, this same plant, at least if it was not fleshy and was growing in an atmosphere that already contained oxygen, did not form carbon dioxide entirely from its own substance but instead combined its carbon with the oxygen in the atmosphere. The plant's decomposition of this carbon dioxide simply returned to the atmosphere the free oxygen that had been there at the start. Because he found no net increase in atmospheric oxygen content under these conditions, he concluded that plants did not add oxygen to the atmosphere by a direct decomposition of water.
4. Does atmospheric oxygen combine directly with plant constituents other than carbon?

   De Saussure found that atmospheric oxygen generally reacts only with the carbon of the plant, forming carbon dioxide gas. These findings countered some previous researchers' assertions, such as the claim that oxygen combines directly with substances in a germinating seed to produce sugar. He based this conclusion on his finding of a near-equivalence between the volumes of oxygen taken up and carbon dioxide released, both in germinating seeds and in more-developed plants. Leaves exposed to an alternating day/night regime, in a receptacle filled with ordinary air, consumed oxygen at night but restored it by an approximately equal volume of carbon dioxide during the day. He also found that atmospheric oxygen gas is not fixed in the tissues of dead or fermenting plants or plant substances (such as wine that is fermenting to vinegar), during the early stages, nor does it combine with plant hydrogen to form water. He concluded that the sole role of oxygen in these stages is to combine with plant carbon, yielding carbon dioxide gas.

5. Is the presence of oxygen gas necessary for plants to break down carbon dioxide and to grow?
   Plants, de Saussure found, need oxygen gas in their atmospheres in order to be able to break down carbon dioxide (i.e., to photosynthesize) and to grow. Plants can survive in environments lacking oxygen gas only if they are able to produce free oxygen through the decomposition of carbon dioxide that they form entirely from their own tissues. Even plants that can survive in an atmosphere of pure nitrogen gas, however, die there if carbon dioxide is added to that atmosphere in concentrations that would have allowed plant growth in an ordinary atmosphere. (He also determined that plants do not absorb nitrogen, hydrogen, or carbon monoxide gases.)
   Like Ingen-Housz, de Saussure concluded that the union of atmospheric oxygen with plant carbon (i.e., respiration) is a normal plant process and not, as Senebier had thought, an indication of unnatural growing conditions or of plant stress or disease. He considered respiration to perform the same function in plants as in animals, that is, to bring about a release of heat, through the combination of atmospheric oxygen with the carbon of the organism. He noted that plants' release of heat often went undetected. De Saussure was unable to develop as clear an interpretation of the metabolic role of oxygen in plants as he was of carbon dioxide, as noted by Nash (1952, p. 110).
6. Do plants fix the elemental components of water in their dry substance?
   In regard to a possible role for water in plant nutrition, de Saussure reasoned—quite plausibly, but, as it turned out, incorrectly—that, because the amount of oxygen the plant loses to the atmosphere is approximately equal to the amount of oxygen in the carbon dioxide that the plant takes in, the most likely source for most of the oxygen assimilated by plants is water. He also reasoned—correctly, in this case—that water is probably the source of the hydrogen in plant dry matter. He showed experimentally that growing plants do, indeed, assimilate water (although not as much as he thought, since they assimilate only the hydrogen, the oxygen coming from the carbon dioxide). He thus confirmed van Helmont's guess that water is an important component of plant dry matter. Many consider de Saussure's recognition of the role of water to be his single greatest contribution to the understanding of plant nutrition (Nash 1952, p. 113). He provided the

additional, important insight that the weights of hydrogen and oxygen in a plant cannot be increased substantially unless the weight of carbon is simultaneously increased. By linking the incorporation of carbon and water, he foreshadowed the discovery of plant carbohydrates, but in his day, of course, there was no knowledge of organic carbon compounds (Naef 1987, p. 336).

7. Do plants form and release water?
   De Saussure found that water was formed and released during some processes within living plants, such as in seed germination, and also in some processes occurring in nonliving plants and plant products. For example, when atmospheric oxygen combined with the carbon of humus or of wood extracts and formed carbon dioxide, water was also produced. This water, however, was generally composed from hydrogen and oxygen already contained within the dry substance of the plant or plant product, not from the combination of atmospheric oxygen gas directly with the hydrogen of the substance. He believed that the later stages of fermentation presented a possible exception to this pattern, in that atmospheric oxygen combined directly with plant hydrogen.
8. Are the minerals that are found in plant ash created by a vital force within the plant or do they come from the soil?
   De Saussure demonstrated that mineral substances disappeared from test solutions in which plants were immersed by their roots, and that these substances reappeared, in roughly the same proportions as they were absorbed, in the incineration products (ash) of the plants. He thereby showed that the minerals in plants do not owe their presence to a transmutation of carbon or other elements by a vital force. Further, he demonstrated that roots do not absorb all solutes in the same proportions, and that they absorb more of the water of solution than they do any of its solutes.
   De Saussure's analyses of plant ash were the most accurate and extensive performed up to that time, and his tabulations of the percentages of the various minerals in the ash were the first of their kind (Browne 1944, p. 200). His analyses revealed all of the minerals now known to be plant macronutrients, except sulfur. He found traces of other minerals, in amounts too small for him to analyze quantitatively.[18]
   Another of de Saussure's important findings was that the composition of plant ash reflects the mineral composition of the soil in which the plants have grown. He thereby showed that humus does, after all, play a role in plant life—not as a carbon source, but as a mineral source. In addition, he found that mineral concentrations in plants vary among different species, among different tissues, and with plant

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[18] Nine essential plant macronutrients are now known: carbon, oxygen, hydrogen, nitrogen, potassium, calcium, magnesium, phosphorus, and sulfur. Adequate concentrations of the last four of these nutrients in plant dry tissue are 1 % or less. In addition, there are eight known essential plant micronutrients: chlorine, iron, boron, manganese, zinc, copper, nickel, and molybdenum (Stout 1961, as cited in Raven et al. 1999, p. 728).

age. For example, although all plants contain phosphorus,[19] young tissue contains more than old, and seeds are especially rich in it. Some minerals are lost from the plant due to leaching by rainwater.

9. Are minerals essential for plant growth?
   Although plants contain minerals in only very small amounts, these substances are essential for their growth, de Saussure concluded. Some of the elements absorbed may not be essential, he thought, but this had not been demonstrated for the elements that plants always contain.
   De Saussure did not trace the source of plant nitrogen convincingly, but he correctly concluded that the source of this nutrient is not atmospheric nitrogen gas, as Ingen-Housz (1796) and many others had believed. De Saussure thought, instead, that plant nitrogen came from extracts of plants and animals in the soil, or ammonia that these extracts released to the soil and atmosphere.

In his studies of gases, de Saussure generally considered the volume of his experimental plant to be a standard, or threshold, for determining whether oxygen or carbon dioxide gas was being taken in or released significantly. He considered that the "water of vegetation" in a plant's parenchyma could hold a volume of carbon dioxide approximately equal to, or slightly greater than, the volume of the plant, and thus could store it and sequester it, at least temporarily, from decomposition into carbon and oxygen by the plant's photosynthetic apparatus. This stored carbon dioxide could arise as a result of direct uptake of carbon dioxide by the plant from the atmosphere or as a result of the combination of oxygen from the atmosphere with the plant's own carbon—or even (in oxygen-free atmospheres) from the combination of carbon and oxygen that were both drawn from the plant's own substance. Thus, in order for plant uptake or release of a gas to be considered significant, the volume of this gas had to exceed considerably the volume of the plant. If it did not, the gas could be assumed to be in storage, as carbon dioxide, in the parenchyma.

The significance of one of Théodore de Saussure's discoveries regarding gas exchange was not understood by him and, in fact, came to be elucidated only in the 20th century. He found that the physiology of gas exchange in succulents, such as cactus (*Opuntia*), differed from that in thin-leaved plants. Although not relevant to the major physiological questions of his time, this was the first documentation (per Moyse 1990) of the photosynthetic pathway known as Crassulacean Acid Metabolism (CAM), an important variant of the usual photosynthetic pathway (see Black and Osmond 2005).

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[19] Analyzing phosphate content proved challenging for de Saussure. Although he showed that phosphates were important to plants, he had great difficulty in isolating them. Agricultural chemists who came after him also had difficulty in developing better tests for phosphates, which were often mistaken for alumina (Rossiter 1975, p. 25).

De Saussure's Work Enables Completion of the Overall Chemical Equation for Photosynthesis

The work of the early pioneers of photosynthesis research could, after Théodore de Saussure's contribution, be expressed in the basic, overall, chemical reaction[20] of photosynthesis that still stands today:

$$\text{Carbon dioxide} + \text{water} \frac{\text{Light}}{\text{Green plant}} \rightarrow \text{oxygen gas} + \text{carbon fixed in plant}$$

De Saussure was responsible for showing the participation of water in this reaction.

This equation does not incorporate the concept of energy, which was not fully developed until the mid-19th century, when the law of the conservation of energy was propounded: that energy can be neither created nor destroyed. Initially this law was applied only to physical systems, because a vital force was believed to create the energy that powered biological systems. German physician and physicist Julius Robert von Mayer[21] (1814–1878) contended in 1845, however, that energy conservation applied to biological systems, as well. He concluded, correctly, that photosynthesizing plants store, in the form of chemical energy, part of the light energy reaching them from the sun. The stored chemical energy is later broken down by the plants, or by organisms nutritionally dependent on the plants, and used as tissue building blocks and for doing cellular work.

Before von Mayer's insight, green plants could be understood only as chemical manufacturers of organic matter, but after von Mayer, plants were also seen as energy providers for life (Rabinowitch and Govindjee 1969, p. 9). The equation for photosynthesis could then be re-written to show both the material and the energy balance:

$$\text{Carbon dioxide} + \text{water} \ \frac{\text{Light}}{\text{Green plant}} \rightarrow \text{oxygen gas} + \text{carbon fixed in plant}$$
$$+ \text{chemical energy}$$

Critical Reception of *Recherches chimiques sur la Végétation*, its Legacy, and Historical Perspectives

Recherches chimiques sur la Végétation was favorably received upon publication. French chemist Claude-Louis Berthollet analyzed it and read extracts at the Institute of France (*l'Institut de France*[22]), and his review of the book was published in the

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[20] This reaction is not written in chemical formula notation here because that notation was not in use at the time de Saussure's book was published. It was not developed until 1808, by Swedish chemist Jöns Jacob Berzelius.

[21] For a biography of von Mayer, see Caneva (1993).

[22] Established in 1795, the Institute of France incorporated many of the former royal academies, including the Royal Academy of Sciences (*l'Académie royale des sciences*), which had been abolished 2 years previously during the French Revolution.

Annales de Chimie (1804). An English translation of the review soon appeared in the *Philosophical Magazine* (1805). Extracts of the book were published in the *Journal de Physique* (de Saussure 1804b), and a German translation of these extracts appeared in the *Annalen der Physik* (de Saussure 1804c). The year after the book appeared, biologist Friedrich Siegmund Voigt (1781–1850) published a German translation of the entire work (de Saussure 1805).

Despite its successful debut, however, the book's important conclusions on the source of plant carbon and mineral nutrients were largely ignored for about four decades, although various authors cited the book liberally. No revised edition of the book ever appeared.

Eminent German plant physiologist Julius von Sachs (1832–1897), although praising the book highly, identified a problem: the writing style. He commented (1875, 1890 English translation, p. 498):

> It is not every one who can follow a work like this, which is no connected didactic exposition of the theory of nutrition, but a series of experimental results which group themselves round the great questions of the subject, while the theoretical connection is indicated in short introductions and recapitulations, and it is left to the reader to form his own convictions by careful study of all the details.... [T]he writer seems to confine himself too anxiously within the limits of what is given in experience, and there is no doubt that many errors in later times might have been avoided if the inductive proof of de Saussure's doctrines had been accompanied with a deductive exposition of them of a more didactic character.

Von Sachs continued (pp. 501–502), "Unfortunately de Saussure neglected to state [*the results on minerals*] with due emphasis and to point out their fundamental importance, and consequently doubts were entertained even till after 1830 respecting the necessity of the constituents of the ash to vegetation". Grandeau (1879, p. 46) expressed similar regrets. Waksman (1936, p. 23) considered de Saussure's ideas regarding the origin and chemical nature of soil organic matter or humus too advanced for his times.

Another problem for the book was that some of the new research undertaken after it was published yielded unclear or inconsistent results. Further, closely following on the heels of the book were two highly influential works on agricultural chemistry that continued to promulgate the humus theory, despite referring to de Saussure's findings. These books were by German agronomist Albrecht Daniel Thaer (1752–1828), in his work of 1809–1812; and British chemist Humphrey Davy (1778–1829), in his work of 1813. Davy accepted (1813, p. 273) de Saussure's conclusions that plants cannot form any of their ash constituents by synthesis or transmutation, but Thaer did not (see Browne 1944, p. 180).

Other influential writers who clung to the humus theory during this period included Swedish chemist Jöns Jacob Berzelius (1779–1848), in a treatise he co-authored on animal chemistry (1815), and Swiss botanist Augustin Pyrame de Candolle (1778–1841), in a treatise on plant physiology (1832). De Candolle was more modern in his view on plant minerals, however, contending that they are derived entirely from the soil (1832, Vol. 1, p. 390) (See Browne 1944, Chaps. 5 and 6, for more detail on research and opinions during this period).

Another factor in the delay in general acceptance of de Saussure's conclusions was an error of interpretation that cost him his scientific reputation for a time. He mistakenly thought (his p. 56) that the pigment that gives a certain variety of *Atriplex hortensis* its red or purple color was associated with photosynthetic oxygen production, leading him to conclude, erroneously, that the green pigment was not essential. "This oversight on the part of a man so highly regarded engendered doubts which unfortunately persisted for a long time. . . ." (Morton 1981, p. 341).

De Saussure's conclusions on plant carbon and the soil minerals did find support in some new research studies, however, especially among German researchers. One of these was agricultural chemist Carl S. Sprengel (1787–1859), who, in a work on plant humus (1826), denied that plants could generate their own lime or other noncombustible constituents but obtained them instead from the environment. Sprengel also rejected Thaer's notion that plants assimilate humus (Browne 1944, pp. 231–239). Investigations by German researchers Wiegmann and Polstorff (1842), who used a balance-sheet approach, also supported de Saussure's findings on plant mineral matter.

De Saussure's ideas owe their real revival, however, chiefly to the influential German chemist Justus von Liebig[23] (1803–1873). Liebig (1840, 1847 English translation) stressed both the atmospheric source of plant carbon and the implications of de Saussure's findings on plant mineral nutrition for increasing crop yields through fertilization.[24] Liebig criticized the plant physiologists of his day for clinging, despite abundant evidence to the contrary, to the idea that plants obtain their carbon from the soil rather than the atmosphere.[25] He argued that, since the humus itself consists of the decayed matter of preceding plants, it was circular to contend that it was the original source of plant carbon (Aikman 1894). Liebig also pointed out that there is not enough soil humus to supply carbon to all of the plants growing in it. Based on de Saussure's ideas, manures could be seen in a different way: Animal dung provides silicates of potash and small amounts of nitrogen, ashes provide potash, and bone dust provides phosphates (Rossiter 1975, p. 25).

The renewed appreciation for de Saussure's contributions was also aided by the field research of French agricultural chemist Jean-Baptiste Boussingault[26] (1802–1887). Like Wiegmann and Polstorff (1842), Boussingault used a quantitative, balance-sheet approach, and, in extensive field trials on crop plants, which he reported in 1843–1844 and 1860–1891, he weighed and analyzed the materials he applied to the soil and the crops he obtained under the various regimes. His balance sheets showed the extent to which the fertilizers had met crop needs, and the extent to which air, rain, and soil had contributed. He demonstrated that plants can grow normally with nutrient salts alone, without any humus whatsoever (Waksman 1936).

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[23] For a biography of Liebig, see Brock (1997).

[24] Liebig later advocated and developed artificial fertilizers.

[25] See Werner and Holmes (2002) for the view that plant physiologists as a group did not deserve such singling out.

[26] For a biography of Boussingault, see McCosh (1984).

It was not until the 1850s that the source of plant nitrogen was traced to the soil. This accomplishment was also the work of Boussingault (1855, 1856, and 1857), as discussed in detail by Aulie (1970). Another three decades would pass, however, before Winogradsky (1890) isolated the soil bacteria that fix nitrogen.

Even after publication of Liebig's book and Boussingault's early research, however, the humus theory of plant carbon clung to life for a time. In 1845 plant physiologist Jean-François Macaire-Prinsep, of Geneva, noted that the theory was still controversial among physiologists. Evidence that de Saussure's ideas were taking hold, however, include the publication in 1842 (English translation from the German in 1849) of what Morton (1981, p. 377) called the first textbook of botany that is modern in form and spirit, by Matthias Jakob Schleiden (1804–1881), co-founder of the cell theory. Emphasizing plant physiology and its relationship to plant structure and development, Schleiden cited de Saussure numerous times and rejected both the humus theory and the concept of a vital force.

The renewed respect for *Recherches chimiques sur la Végétation* as the 19th century wore on is reflected in the appearance of the second German translation of the work, in 1890, by botanist Arwed Wieler[27] (b. 1858). This translation appeared in a two-volume set in the series Ostwald's *Klassiker der Exakten Wissenschaften*. Wieler stated (Vol. 1, p. 96) that he undertook the translation at the request of plant physiologist Wilhelm Pfeffer because of the rarity and high price of the original. Wieler's German is more modern than that of the 1805 translation by Voigt, and thus more accessible to a modern audience. (In 2009, the Wieler translation was reprinted in two paperback, "optical character recognition," volumes. In this reprint, Théodore de Saussure's name was, regrettably, rendered as "Théodore Laussure").

The continuing appreciation of de Saussure's book is also evident from the recognition and praise bestowed on it, from Liebig's time through the present, by numerous authors from a wide range of disciplines—chemistry, agriculture, agronomy, soil science, plant physiology, and plant nutrition.[28] Despite this renewed respect, however, French agronomist Paul Robin (2007, p. 47) laments, "Although it is constantly referred to by Liebig, de Saussure and the date 1804 are largely forgotten by today's agronomists".

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[27] Wieler subsequently published one of the first studies (1905) of the long-term effects of industrial air pollution on plants.

[28] These include, from the mid-19th through late-20th centuries: de Candolle (1846, p. IX); von Sachs (1875, 1890 translation, pp. 497–498); Pfeffer (1881, 1900 English translation, pp. 308–309); Aikman (1894, pp. 14–15); Brown (1899, p. 476); Harvey-Gibson (1919, pp. 79–82); Spoehr (1919, p. 36); Waksman (1936, p. 12); Browne (1944, pp. 192–193, p. 220); Rabinowitch (1945, Vol. 1, p. 23); Gabriel and Fogel (1955, p. 162); Leicester (1956, p. 232); Solovine (1957, p. VIII); Pilet (1975, p. 124); Russell (1976, p. 7); Morton (1981, p. 338); Buchs (1987, p. 172); Naef (1987, p. 334); and Boulaine (1994, p. 21). In the 21st century, de Saussure's life and work continue to garner attention, especially in the French-language literature on agriculture and agronomy: See Robin and Blondel-Mégrelis (2001), Candaux (2007), and Robin (2007). Magiels (2010), writing in English, addresses de Saussure in the context of Ingen-Housz's work.

Some Errors and Misinterpretations in *Recherches chimiques sur la Végétation*

Despite the achievement represented by de Saussure's book, there were, inevitably, flaws. As already noted, the writing style is, in places, opaque. The text is plagued with minor errors, including mistakes in conversion between Parisian and metric measurement systems, some mis-statements, and mis-citings of his own tables. There are also some more significant, conceptual errors, attributable to limitations in experimental techniques and the general state of knowledge of plant physiology and nutrition in de Saussure's day.

I have already pointed out de Saussure's erroneous conclusion that the green pigment in plants is not necessary for photosynthesis. In addition, he believed that, in the light, plants decompose carbon dioxide into carbon and oxygen, and that the carbon is then hydrated by a reaction with water (see p. 77 of his text), with most of the oxygen of the carbon dioxide released to the atmosphere. It is now known that, in green plants, water is the hydrogen donor in the photoreduction of carbon dioxide, yielding an organic building block (CH_2O), with the release of gaseous oxygen derived from the water. As already mentioned, the idea that the source of the liberated oxygen is the absorbed carbon dioxide rather than water seemed logical based on the near-equivalence between the volumes of carbon dioxide absorbed and oxygen released. In fact, this view of the source of the oxygen prevailed until the 20th century.

The possibility that the oxygen comes instead from water had been suggested by Lavoisier's chemical colleagues Antoine-Francois Fourcroy (per Moyse 1990) and Claude-Louis Berthollet (1786), but without experimental evidence. It was not until the work of van Niel (1932) and Ruben et al. (1941)[29] that water was shown to be the source of the released oxygen. Because excess water is almost always present in plant cells, however, and because oxygen is liberated from water only in proportion to the carbon dioxide absorbed by the plant, the net effect is as envisioned by de Saussure (Nash 1952, p. 119). The view that carbon dioxide is the source of the liberated oxygen persisted, however, among some workers[30] for more than a quarter-century after the work of Ruben et al. (1941).

De Saussure's erroneous conclusion that plants release nitrogen gas arose from limitations in experimental methodology (Gabriel and Fogel 1955, p. 164, fn. 2). In the early 19th century, gas analysis was still difficult (Partington 1962, p. 758) and the techniques were not noted for their accuracy (Ihde 1964, p. 291).

Methodological limitations also hampered de Saussure's chemical analyses of the minerals in plant ash, as indicated by the large "deficits" he obtained (i.e., the difference between the sum of the separate constituents he measured and the amount

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[29] Van Niel (1932) proposed, based on his research with bacteria, that water was the source of the oxygen liberated by green plants, and Ruben et al. (1941), using heavy oxygen (O^{18}), confirmed the hypothesis.

[30] For example, Warburg et al. (1969). (See Govindjee (1999) and Nickelsen and Govindjee (2011) for a challenge to the conclusions of this paper.)

of ash he analyzed). Nevertheless, his mineral analyses represent a major advance over previous work in this area. Liebig, who criticized de Saussure's techniques, also suggested (1840, 1847 English translation, p. 256) that, because all the analyses were done by similar methods, the results were of interest because they were comparable among themselves. (De Saussure's lengthy tables of incinerations and analyses are impressive, but, unfortunately, he made many mistakes when referring to the entries in them.)

De Saussure did not always identify elements clearly. Given the state of chemical knowledge in his day, this is understandable. It must be remembered that, in 1804, when *Recherches chimiques sur la Végétation* was published, elements and compounds had not been clearly differentiated. Although Antoine Lavoisier had propounded in 1789 that elements were substances that chemical analysis failed to break down into simpler substances, and very early in the 1800s English chemist and physicist John Dalton (1766–1844) had formulated an atomic explanation for the apparent indestructibility of Lavoisier's elements, Dalton's theory was not well publicized and accepted until 1808–1827.

Magiels (2010) criticized de Saussure's book on the grounds of historical inaccuracy, asserting that de Saussure failed to study the previous literature in the field thoroughly and thus mistakenly attributed to Senebier—who was a close friend of the de Saussure family—many contributions that were in fact made by Ingen-Housz. Magiels noted, for example, that de Saussure credited Senebier with the discovery that the nongreen parts of plants do not expel oxygen, when in fact it was Ingen-Housz (1779) who first made this observation. Magiels also criticized de Saussure for dissecting the humus theory without mentioning Ingen-Housz, who had attacked the theory 8 years previously. Ingen-Housz, however, stated his view briefly and without experimental data to support it, whereas de Saussure based his conclusion on detailed experimentation. It is true that, given the discord between Senebier and Ingen-Housz, Senebier may have tended to ignore Ingen-Housz's work and that de Saussure to some extent followed suit. Still, de Saussure's book, which is of only modest length, contains more than 100 citations, including six of Ingen-Housz, indicating that de Saussure examined a considerable amount of the previous literature (see Appendix 3: Bibliography of References Cited by de Saussure (as expanded by the translator)).

I have not thoroughly annotated de Saussure's text, but I have noted some clear errors in brackets within the translation.

The Life and Scientific Background of Théodore de Saussure

When *Recherches chimiques sur la Végétation* was published, Théodore de Saussure was 36 years old and well settled into a quiet, somewhat reclusive life as a plant researcher in his own private laboratory. In contrast to his staid middle and later years, however, de Saussure as a young man (see Fig. 1) had known adventure and scientific accomplishment with his father under arduous conditions in the high Alps,

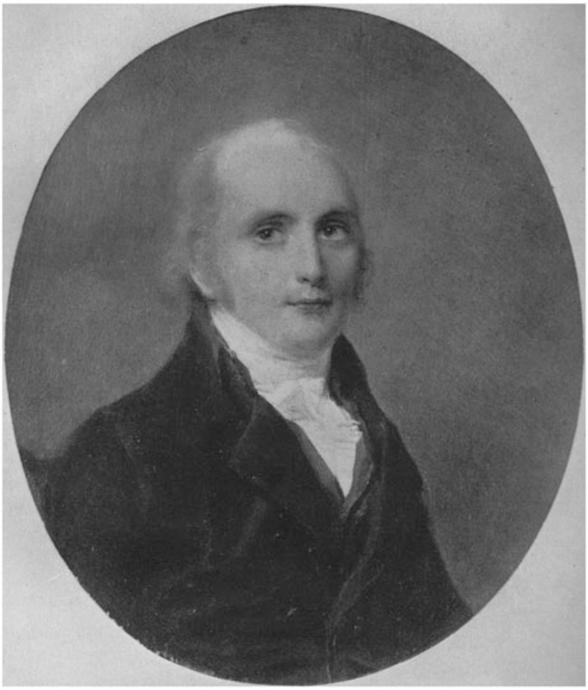

Fig. 1 Portrait of Théodore de Saussure as a young man

and he had suffered when the intense political turmoil of the French Revolution had reached his home city of Geneva.[31]

Théodore de Saussure was born in Geneva on October 14, 1767. His parents, Horace-Bénédict de Saussure (1740–1799) and Albertine-Amélie Boissier de Saussure (1745–1817), were wealthy, and Théodore grew up in a luxurious family townhouse (Fig. 2). His full name was Nicolas-Théodore, but he was usually called Théodore to distinguish him from his grandfather Nicolas de Saussure (1709–1790), a wealthy landowner and agriculturalist who had published a number of papers on agricultural subjects (Zumkeller 2001). Théodore was the second of three children. The first child was Albertine (1766–1841) and the third Alphonse (1770–1853). As his children were growing up, Horace-Bénédict, who was learned in geology, physics, meteorology, chemistry, and botany, was developing an illustrious career as a geologist, Alpine explorer, and professor of natural philosophy at the Academy of Geneva (*l'Académie de Genève*), the predecessor of the University of Geneva,[32] where he taught from 1762 until 1786 (Carozzi 1981).

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[31] The political upheaval in Geneva began in the early 1790s. In 1798, the French Republic annexed all Swiss territory. This territory became part of the Napoleonic Empire—which began in 1804, the year de Saussure's book was published—and remained a part of the empire until the end of 1813.

[32] For detailed treatments of Horace-Bénédict de Saussure's life and work, see Freshfield (1920), Sigrist (2001), Bungener (2002), and Carozzi (1981, 2005).

Fig. 2 Geneva townhouse where Théodore de Saussure grew up

Horace-Bénédict de Saussure and his son Théodore were among the natural philosophers responsible for a burst of scientific achievement in Geneva in the late 18th century (Sigrist 1990). The tradition of natural history in Geneva has been characterized as relying strongly on observation and experiment, in contrast to a more speculative and theory-based tradition that was prevalent in some other parts of Europe (Roger 1987). Photosynthesis pioneer Jean Senebier and plant researchers Charles Bonnet and Augustin Pyrame de Candolle were members of this scientific circle in Geneva.

Horace-Bénédict disapproved of the public education in Geneva and therefore directed the early schooling of his children at home. Macaire (1845) blamed Théodore's shy disposition on this private mode of instruction, because it was so isolating.

From 1782–1786, Théodore attended the Academy of Geneva, where he took courses in mathematics, science, and history (Robin and Blondel-Mégrelis 2001; Candaux 2007). He studied physics under the noted scientist Marc-Auguste Pictet (1752–1825) (Candaux 2007). At the outset, Théodore shared his father's interest in the physical sciences, especially geology and meteorology, and he began his scientific career as his father's companion and field assistant on Alpine expeditions. Horace-Bénédict devised hygrometers, eudiometers, and other precision scientific instruments and trained Théodore in their use (Browne 1944), eventually giving the young man responsibility for recording various physical data on the expeditions. Théodore first became involved, in a limited way, in 1787, when his father made the second-ever conquest of the summit of Mont Blanc (elevation 4,810 m), the highest peak of the Alps (Freshfield 1920; Rowlinson 1998). Judging that his 20-year-old son was not strong enough to make this climb, Horace-Bénédict left him in the village of Chamonix, at the foot of the mountain. There, Théodore recorded meteorological data for comparison with his father's readings on the summit (Macaire 1845).

In 1788, the year following the Mont Blanc ascent, Théodore de Saussure became a full partner with his father on an arduous expedition to the snowfields of the Col du Géant, a high glacial pass. On that expedition, which was the most ambitious and challenging of Horace-Bénédict's career (Freshfield 1920), father and son spent 17 days and nights under harsh, storm-wracked conditions at an elevation of 3,436 m. There, Théodore assisted in various physical, chemical, mineralogical, and meteorological experiments and analyses. He made numerous observations on the density and composition of the atmosphere (Briquet 1940, p. 425). To measure atmospheric density, he observed the oscillation of a pendulum—a method he found disappointingly inexact. He measured the oxygen content of the air using nitric oxide (Priestley's "nitrous-air" method), and tested for the presence of atmospheric carbon dioxide using limewater and caustic potash (Robin and Blondel-Mégrelis 2001). He also conducted experiments on the dissolution of metals in sulfuric acid (Macaire 1845).

The following year, Théodore published his first scientific paper, a physico-chemical analysis of the mineral "sappare" (aluminum silicate) (de Saussure "fils" 1789).[33] Also that year, he and his father made an expedition to Monte Rosa, where Théodore again measured the density of the atmosphere, this time using a large, empty, tightly closed glass flask, which he weighed at 70 different elevations. Théodore found that the differences in weight were exactly proportional to the differences in barometric pressure readings (Macaire 1845). Théodore thereby confirmed Boyle's Law (known as Mariotte's Law in France) by a new method (Hoefer 1867, p. 374; Briquet 1940, p. 425). Théodore's publication on this subject (de Saussure "le fils" 1790) brought him to the attention of the scientific community (Robin and Blondel-Mégrelis 2001). In each of the subsequent three years, he published a paper on mineralogy (de Saussure "fils" or "le fils," 1791, 1792, and 1793). These early chemical, mineralogical, and atmospheric experiments and analyses, although confined to the physical sciences, presage the studies that Théodore was later to conduct on the roles of gases and minerals in plant chemistry and physiology.

The French Revolution, which had begun in Paris the month of the de Saussures' Monte Rosa climb, soon diverted Horace-Bénédict's attention. He made only one further Alpine expedition—a visit, with Théodore, in 1792, to the St. Théodule glacial pass, to measure the height of the Matterhorn (Freshfield 1920).

During the early years of the revolution, while the de Saussure family fortune remained intact, Théodore was able to travel abroad to meet with foreign researchers

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[33] Following the custom of the day, the papers Théodore published during his father's lifetime were under the authorship of "de Saussure fils" or "de Saussure le fils," indicating that he was the son of a scientist of the same surname.

(Hart 1930). He made his first trip to London in the summer of 1791. In 1793–1794, once again in Britain, he met with Joseph Priestley, Joseph Black, and Henry Cavendish, all of them experts in chemistry and eudiometric methods (Robin and Blondel-Mégrelis 2001; Candaux 2007). Much of Horace-Bénédict's fortune was invested in France, however, and was lost during the revolution (Freshfield 1920), while his wife's income was drastically reduced (Carozzi 1981). In late December of 1793, Théodore's mother wrote to him in Britain of the financial difficulties, and advised him to seek a traveling tutorship (Hart 1930). Unable to find such work, Théodore returned to Geneva in the summer of 1794 (Candaux 2007).

The revolutionary turmoil in Geneva soon forced Théodore and his brother to flee to Rolle, about 30 km away, and, that same summer, his father suffered the first of a series of strokes. According to Freshfield (1920), Théodore spent a great deal of his time in Rolle during 1794 and 1795 correcting proofs of the last two volumes of his father's magnum opus, *Voyages dans les Alpes* (1779–1796). Théodore remained in Rolle at least until the summer of 1795 (Candaux 2007). In 1796, upon returning to Geneva, he married Renée Fabri (1767–1847) and began research in plant chemistry (Freshfield 1920; Hart 1930; Robin and Blondel-Mégrelis 2001; Candaux 2007).

When Théodore began his plant research, knowledge of biological chemistry lagged behind that of mineral chemistry, providing an opportunity for significant contributions (Macaire 1845). Other factors may also have propelled Théodore towards plant research. Eighteenth-century Geneva was an important center for botany, especially plant physiology (Naef 1987; Sigrist and Bungener 2008), and some of Théodore's family and their friends were deeply involved in botanical pursuits. Indeed, botany had been his father's first love (Freshfield 1920; Bungener 2001).[34] Further, Horace-Bénédict and his own father, Nicolas, maintained a botanical garden at the family estate at Frontenex (Fig. 3), on the outskirts of Geneva. There, Horace-Bénédict grew plants that he had collected, in order to observe them at different developmental stages for purposes of identifying and describing them (Bungener 2001; Sigrist and Bungener 2008). Thus, from early in life, Théodore would have been familiar with the scientific study of plant growth. In his experimental work, Théodore used a wide variety of plant material (Moyse 1990), including pea, beans, wheat, periwinkle, pellitory, loosestrife, mint, cactus, rhododendron, bilberry, pine, juniper, and fir. Some of these plants probably came from the family garden, although some came from the Alps.

Théodore de Saussure was profoundly influenced by pioneering photosynthesis researcher Jean Senebier, who was a close friend of Horace-Bénédict's and instrumental in Théodore's education. A major line of research pursued by Théodore was one that had intensely occupied Senebier—the absorption of carbon dioxide and liberation of oxygen by plant leaves. In addition, Théodore was undoubtedly influenced by his great-uncle Charles Bonnet, whose 1754 book on plant physiology had

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[34] Horace-Bénédict had launched his scientific career in 1760 by collecting plants near Geneva and from high Alpine areas for Albrecht von Haller (1708–1777), a Swiss physiologist and anatomist who was also a noted botanist and author of a major work (1768) on the Swiss flora. Horace-Bénédict had also published (1762) on plant anatomy.

Fig. 3 De Saussure family estate at Frontenex (on the outskirts of Geneva), where Théodore de Saussure's father and grandfather maintained a botanical garden

inspired Ingen-Housz's experimental methodology and who was a neighbor and close friend of the de Saussure family as Théodore was growing up (Freshfield 1920). Further, a number of the subjects Théodore chose to investigate—soils, humus, and plant mineral nutrition—indicate an agricultural impetus that probably reflected the influence of his agriculturist grandfather Nicolas. In his preface to *Recherches chimiques sur la Végétation*, Théodore acknowledged an agricultural motivation.

Théodore's early papers on plant nutrition provided the groundwork for *Recherches chimiques sur la Végétation*. The first was a major study of carbon dioxide metabolism, published in the *Annales de Chimie* (de Saussure "le fils" 1797) and reprinted in the *Journal de Physique* (de Saussure "le fils" 1798). This paper demonstrated that plants, like animals, continually form carbon dioxide from atmospheric oxygen and their own carbon, and that, if carbon dioxide production is not detectable in a plant, it is because the plant is decomposing this gas as it is formed. Further, he showed that, in sunlight, plants must decompose carbon dioxide in order to grow, and that, although growth in sunlight is promoted to some extent by elevated concentrations of this gas, such concentrations are harmful to plants in shade. This paper formed the nucleus for those chapters of *Recherches chimiques sur la Végétation* dealing with gas exchange and the metabolism of carbon and oxygen. De Saussure's second paper on plant physiology (de Saussure "fils" 1799), a study of the effects of oxygen on seed germination, laid the foundation for Chap. 1 of his book.

In the late 1790s, Théodore again left Geneva, this time with his wife. The couple remained abroad for several years. In the spring of 1800, on his first trip to Paris, Théodore took courses from the noted French pharmacist and chemist Louis-Nicolas

Vauquelin (1763–1829) and kept a journal (de Saussure 1800a) that provides information on his links to the world of Lavoisier (Robin and Blondel-Mégrelis 2001; Candaux 2007). In Paris, Théodore also presented a paper (de Saussure "le fils" 1800b) to the Philomatic Society (*la Société Philomathique*),[35] on his comparative analyses of the minerals contained in the ash of plant samples he had collected, starting in the summer of 1798, from calcareous and granitic Alpine mountains. His results showed that the mineral composition of plants varies according to the nature of the bedrock underlying the soils in which the plants are rooted. This paper contained the first data for those chapters of his book that address plant mineral nutrition. Robin and Blondel-Mégrelis (2001) express regret that, although this paper was the first application, on the ground, of the methods of the "new chemistry" and was therefore a significant contribution to knowledge of the role of soil in plant development and thus to agronomy, it has largely been forgotten.

In 1799, Théodore's father died. Soon thereafter, Théodore's papers began appearing under his own name, Théodore de Saussure. His early publications under this name were in the physical rather than the plant sciences. The first (de Saussure 1801) concerned alumina (aluminum oxide), and the second (de Saussure 1802) focused on carbon dioxide and the electric spark.

Théodore and his wife remained in Britain and France until 1802 (Hart 1930). Upon returning to Geneva, he expected to occupy a promised chair in natural history and chemistry at the Academy of Geneva, but instead was named honorary professor of mineralogy and geology. Acutely disappointed at not receiving an appointment in plant chemistry, he immediately requested and obtained an 18-month leave of absence in order to complete his book *Recherches chimiques sur la Végétation* and to prepare to teach a course in a field he had not chosen. Upon finishing the book, he taught a single course in mineralogy and geology, in 1805 (Candaux 2007), but then no longer taught, despite his presence on the faculty roster for 33 years (Borgeaud 1909). Macaire (1845) attributed his failure to teach to his shy nature.

Recherches chimiques sur la Végétation is Théodore de Saussure's sole book. The initial acclaim for the work brought him recognition as a major contributor to the understanding of plant physiology. He was elected a corresponding member of the Institute of France in 1805 (Haag and Haag 1859; Pilet 1975; Naef 1987).

After publication of the book, Théodore lived another 41 years—more than half his lifetime. During this period, he continued his research on plant chemistry and physiology, publishing numerous papers. He also conducted studies in organic chemistry. For example, he analyzed alcohol and ether by three different methods between 1807 and 1814 (Ihde 1964). De Saussure (1821) showed that, with regard to the air, green fruits behave as do leaves. His other significant contributions included the demonstration (de Saussure 1825) that seeds whose germination has been interrupted by drying can resume normal development if they are rehydrated. In 1841, in a notable presentation on plant nutrition to a scientific congress in France, he showed that

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[35] The year he made this presentation, Théodore was elected a member of the Philomatic Society, which had been founded in 1788 (Moyse 1990).

nitrogen must be absorbed by the roots, although he did not disprove the absorption of atmospheric nitrogen.

Théodore de Saussure was elected to the Royal Society of London as a foreign member in 1820. He served a number of times as a member of the representative council of Geneva. He died in Geneva on April 18, 1845, leaving no descendants.

De Saussure's archives are housed at the Library of Geneva (*Bibliothèque de Genève*; formerly called *Bibliothèque publique et universitaire de Genève*). Candaux (2007) lists the contents of these archives, which include a number of drafts of *Recherches chimiques sur la Végétation*. Further information about de Saussure's life and work can be found in Macaire (1845); Haag and Haag (1859, pp. 191–192); Hoefer (1867, pp. 374–375); Borgeaud (1909, pp. 55, 60, 76, 83, 112, 171, 177); Freshfield (1920); Hart (1930); Briquet (1940, pp. 425–428); Cannon (1961–1962, 1963); Pilet (1975); Robin and Blondel-Mégrelis (2001); and Candaux (2007).

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A Note on the Translation

In the mid-1790s, when Théodore de Saussure began the research that laid the ground work for *Recherches chimiques sur la Végétation*, the turmoil of the French Revolution had spread to his home city, Geneva. The Swiss Republic was annexed by the French Republic in 1798 and became part of the Napoleonic Empire in 1804, the year this book was published. In the upheaval of this period, many of the conventions of the *ancien régime* were replaced. The innovations included the metric system and a drastic redesign of the calendar system. In addition, the forms of address, or honorifics, changed. The text of de Saussure's book reflects the transitional nature of this period and adheres neither to the old nor to the new conventions completely. He compiled some of his book from papers he had written, or research he had undertaken, starting in the mid- to late-1790s, before the new systems had taken hold, and he did not complete the work until 1804, after these newer conventions had become more accepted.

De Saussure mostly, but not always, reported his measurements (linear, volume, weight) in units of both the old Parisian system and the newer metric system. The Parisian values that he reported are commonly more rounded than their metric equivalents, strongly suggesting that his laboratory equipment was calibrated in Parisian units. Although some of the Parisian units sound familiar to English speakers, such as inch (*pouce*), pound (*livre*), and grain (*grain*), these units are slightly different from their English counterparts. For the convenience of the reader, I have provided, in Appendix 1, conversions among the Parisian units, the English units (which de Saussure himself does not use), and the metric ones. De Saussure often rounds off his figures roughly, and his conversions between units of the Parisian and metric systems are not always exact.

In some places in his text, de Saussure used the familiar, pre-Revolutionary, January-December, calendar system, which was re-adopted in 1805 by Napoleonic France. At other places he used the French Revolutionary system (months Vendemaire through Fructidor, and years that began with *An* I (year 1, or 1793) and continued through *An* XIV (year 14, or 1805)). Some of the literature he cited was in French-language publications that appeared during the Revolutionary era and thus used this system. In Appendix 3, in which I list the authors de Saussure cited and as much information about the exact sources as I could find, I use only conventional dates.

Although Europe was roiled by the French Revolution during the time de Saussure was working on his book, the chemical revolution, which had begun some years before the French Revolution, was well advanced, and by the time de Saussure's book was published, the new chemical concepts and nomenclature were largely in place. Many of the chemical terms de Saussure used are the same as, or similar to, modern terms.

Chemical terminology has evolved since de Saussure's day, however, and some of his terms are now obsolete. This situation led to some complexity in the way I dealt with chemical nomenclature in the translation. For some of the older chemical terms for which there are unambiguous modern equivalents, the modern terms have been substituted throughout the translated text. For example, I used "carbon dioxide" in place of the now-archaic term "carbonic acid gas", and "chloride" in place of the now-obsolete "muriate". Where I have made such a substitution, I have provided de Saussure's term in square brackets at the first occurrence of the term in the text, and sometimes again later to remind the reader.

For some terms, however, the substitution of modern for older terminology was problematic, as in the case of "potash" and "soda"—two terms that de Saussure employed frequently. At the time he wrote his book, the elements potassium and sodium had not yet been isolated from potash and soda, respectively, and the terms "potash" and "soda" might refer to more than one chemical that contained potassium or sodium. In this translation, I have substituted the terms "potassium" and "sodium" for "potash" and "soda" only in cases where de Saussure used these terms as part of the name of a compound that is still valid today, and only where the general context did not make such substitution confusing for the reader. In cases where it could be confusing, I retained the older terminology, even in the names of still-valid chemical compounds. For example, in Chap. 9, which contains detailed descriptions of complex chemical manipulations and is heavily laden with chemical terminology, de Saussure frequently used the terms "potash" and "soda", both by themselves and to refer to components of compounds (for example, "phosphate of potash", which, in modern terminology, is potassium phosphate). The substitution of "potassium" for "potash" in the names of chemical compounds but not for the term "potash" when used by itself would have resulted in a disconnected narrative that adhered neither to the new nor to the old terminology, and could have been confusing for the reader. Therefore, in that chapter, I have generally retained the older names for all terms (including lime and magnesia, because the elements calcium and magnesium, respectively, had not yet been isolated from these compounds). I have provided a glossary of terms (Appendix 2) that gives the older terms and their modern equivalents.

In addition to the changes in chemical terminology, I have made some changes where a literal translation of the original sounded awkward in English. Further, for passages in which the flow of ideas seemed difficult to follow, or where unusual terms were used, I have inserted, in italicized type in square brackets, explanations or clarifications that I thought would be helpful to the reader. (This translation is not a thoroughly annotated version of the text, however). I have also inserted, in bold type in square brackets, the page numbers of the original French text, for ease in consulting that text. All footnotes are de Saussure's, as is text that is set off in ordinary parentheses.

De Saussure made a number of clear errors, mostly minor, in his text. These include mistakes in conversion between the old, Parisian measurement units and their metric equivalents; misinterpretation of another author's writing; some misstatements; and mis-citings of his tables. I have generally corrected, or pointed out, the errors that I have spotted, giving, where appropriate, de Saussure's original wording or numerical figures, or an explanation for my change, in italicized type in square brackets after the corrected text. (Errors were a particular problem in de Saussure's references in Chap. 9 to his extensive tables of incinerations and ash analyses.) I have also corrected the four errata that de Saussure listed at the end of his text.

Although de Saussure's book is of only modest length, it includes more than 100 citations, drawn from diverse fields. As was common in his day, however, he supplied no bibliography and provided only sketchy details for many of his sources—sometimes no more than an author's name. I have traced these references as far as I could, obtaining nearly all authors' full names and, for most of them, the full titles of books, journals, and papers. For many of them, I found relevant page numbers. The fleshed-out list is provided in Appendix 3. Some of the entries in this list are, necessarily, only plausible guesses on my part about his sources (and I note these as such), but the list should provide the reader an entrée to the broad range of literature de Saussure consulted in his research.

In the text itself, I have left the references largely as de Saussure cited them, except for a few changes related mainly to forms of address and name renderings. For authors in French-language publications in the older, pre-Revolutionary era, the honorifics used were "M." and "MM." (for *Monsieur* and *Messieurs*), whereas in the Revolutionary-era publications, "C." and "CC." (for *Citoyen* (Citizen) and *Citoyens* (Citizens)) were commonly used instead. For the sake of simplicity, I have deleted all honorifics in the translation itself and in the fleshed-out citations listed in Appendix 3, as well as in the reference list that I compiled for my introduction. In the text of the translation, I have italicized foreign words and the titles of all books and journals cited by de Saussure.

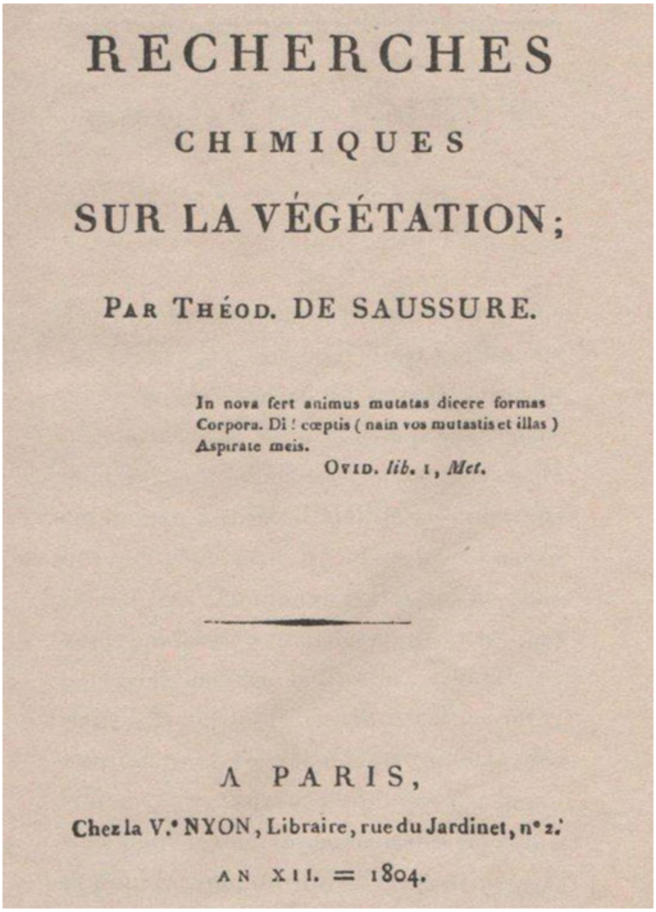
In addition, in the translation and in Appendix 3, I have made the authors' surnames conform to generally accepted spellings or forms. The name changes I made are as follows:

My rendering of the name	De Saussure's original
DeCandolle	Decandolle
Haüy	Hauy
Ingen-Housz	Ingenhoutz
Lefebure	Le Febvre
Marggraf	Margraff
Perthuis	Pertuis
Rückert	Ruckert
Rumford	Rumfordt
van Helmont	vanhelmont

I have also standardized the scientific (Latin) names of the plant species mentioned in the text. De Saussure was not consistent in this regard. In some places he used a lower-case initial letter for genera, other times a capital letter. Sometimes he italicized the Latin names, other times not. For the sake of consistency and to conform to modern nomenclature standards, I have italicized all Latin names and capitalized the first letter of all Latin generic names. Otherwise, I have preserved the scientific names as he presented them, even though many of these have been changed since his time.

De Saussure's sentences tended to be long, with a much more liberal use of commas, semicolons, and colons than is usual today. I have taken the liberty of removing many of his commas, and of substituting periods for many of his semicolons and colons, because periods seem more natural by modern standards.

Hill, J. F.



RECHERCHES

CHIMIQUES

SUR LA VÉGÉTATION;

PAR THÉOD. DE SAUSSURE.

> In nova ſert animus mutatas dicere formas
> Corpora. Di ! cœptis (nam vos mutastis et illas)
> Aspirate meis.
>
> OVID. *lib.* 1, *Met.*

A PARIS,

Chez la V.ᵉ NYON, Libraire, rue du Jardinet, n° 2.

AN XII. = 1804.

Title page of *Recherches chimiques sur la Végétation*

Contents of Original de Saussure Book

De Saussure’s Plate of Figures

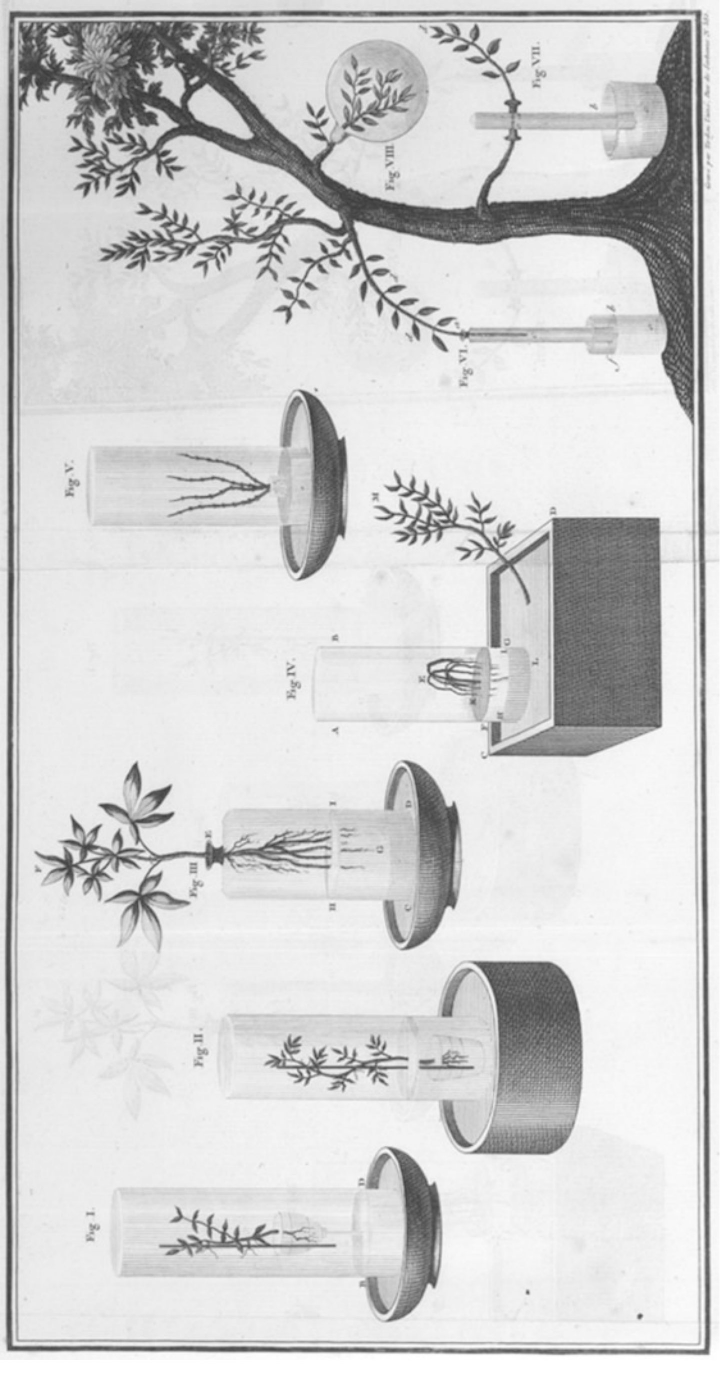


Plate from de Saussure’s 1804 book, *Recherches chimiques sur la Végétation*, showing Figures I–VIII.

[p. 1] Chapter 1
Influence of Oxygen Gas on Germination

1.1 Of Germination

Oxygen gas and water are two agents that nature uses simultaneously to stimulate the development in seeds known as *germination*. Water alone, by entering their cellular tissue and filling it, increases their volume but does not cause them to germinate. The [*appearance of the*] radicle is, for many but not all seeds, an **[p. 2]** indication of germination. Some seeds reveal this organ by a swelling due to the imbibition of water and not due to an act of plant growth. We cannot doubt it, knowing that certain seeds that are dead or have lost, through decrepitude, the ability to germinate nonetheless sprout their radicles as if they were capable of new development.

Coffee, for example,[1] which can sprout only if sown 5 or 6 weeks after being harvested, produces the phenomenon in question after an indefinite number of years. It produces the effect not only in cold water, without contact with air, but also in liquids certainly incapable of eliciting or sustaining growth, such as boiling water, vinegar, and some saturated saline solutions. This very small increase, the end of which can be anticipated before the swelling that produces it, does not change **[p. 3]** the taste, odor, or other properties of the seed. The imbibition of water without contact with air has no other effect than to dispose the seed to rapid decay.

Some seeds, such as peas, lentils, and aquatic seeds, can germinate under water, but only in water that is impure or contains oxygen gas foreign to its composition. I passed boiling water under a receptacle [*de Saussure used, almost exclusively, the term "recipient" for this vessel; see Glossary (Appendix 2)*] filled with mercury, and, after this water had cooled, peas, lentils, and seeds of *Alisma plantago* and *Polygonum amphibium*. They gave no sign of germination when the amount of boiled water was only seven to eight times the weight of the seeds, but when this amount was 100 or 200 times greater, they germinated, and the elongation of their radicle was proportional to the amount of water surrounding them. It is perhaps unnecessary to explain the reason for these differences, or to recall that boiling does not deprive

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[1] Valmont de Bomare, *Dictionnaire d'Histoire naturelle* (*article Café*), made this phenomenon known, but it has been ascribed, baselessly, to a true germination. Lentils that have lost their germinative ability have the same property in a less striking manner.

J. F. Hill, *Chemical Research on Plant Growth*,
DOI 10.1007/978-1-4614-4136-6_1,

water of all the oxygen gas that is contained in it in a dispersed or dissolved state, and that in increasing the amount of fluid, I added enough air to elicit a very feeble development in the seed. It has been claimed that peas that were swollen under water **[p. 4]** germinated when they were then submerged in oil. I repeated this experiment several times but was unable to obtain the reported result.

Humboldt found that seeds germinated in oxygenated muriatic acid [*chlorine; see Glossary*] that was very diluted with water. This experiment, repeated in darkness with strong oxygenated muriatic acid dissolved in boiled water and without contact with external air, gave me the same results. It shows that it is not really necessary that oxygen gas be free or uncombined to elicit germination. [*A misinterpretation by chemists of that day; chlorine, or "oxygenated muriatic acid," does not contain oxygen; see Glossary*] But this acid is, up to now, at least according to my experiments, the only substance that can produce this effect. Dilute nitric acid, to which the same influence had been attributed, does not exercise it without contact with external air. I likewise tried, unsuccessfully, sulfuric acid and several other acids taken from the class of acids in which oxygen appears to be the least tightly bound. I was no more successful in subjecting to the same tests, and without contact with external air, a mixture of boiled water and different metallic oxides, such as black oxide of manganese, red oxide of mercury, and red oxide of lead. **[p. 5]** I carried out these experiments in glass flasks that I filled with liquid heated to the boiling point. When the liquid had cooled to 30° Réaumur, I introduced the seeds. [*In the Réaumur temperature scale, the freezing point of water is 0°, the boiling point 80°; see Conversions of Units of Measurement, Appendix 1*] The flasks were then carefully closed [*see "closed", in Glossary, for explanation*] and plunged under mercury. The amount of water used in each experiment was no more than seven or eight times the weight of the seeds. The seeds tested were lentils, haricots, fava beans, peas, *Alisma plantago*, and *Polygonum amphibium*.

A seed of fava, pea, or haricot, swollen by water without contact with oxygen gas, shows a marvelous structure [*"organization"*] when its integuments are removed. The microscope reveals, in the thinnest lamina that can be detached from the surface of the cotyledons, a regular tissue formed of angular cells. This tissue is speckled, on the back of the seed and on the edges of the cotyledons, with the openings or glandular pores [*presumably stomata*] that produce such a beautiful effect on the surface of mature leaves.[2] They are **[p. 6]** very abundant on the opposing surfaces of the cotyledons of pea seeds.

When seeds begin to germinate through contact with oxygen gas, the elongation of their radicle beyond the length it could have attained by a purely aqueous swelling is the only indication of their growth. But, other than the lack of a growth spurt, no essential difference could be seen between the structure of a seed swollen by pure water and the structure of a newly germinated seed. It is only by more prolonged growth that the changes undergone by the seed become detectable to us. The seed

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[2] This observation, which I am merely confirming, was made by Sprengel (*Anleitung zur Kenntniss der gewachse*, Vol. 1. p. 396). It shows, along with other, similar observations, that these openings do not form through contact with air.

does not decay as it would if it had been deprived of contact with oxygen gas, but its taste changes. It becomes bitter or sweet, according to the nature of the plant. The cotyledons turn green through exposure to light and empty into the developing plant by means of branched ducts that can be revealed by injected dye.

1.2 Use of Oxygen Gas in Germination [p. 7]

Our means are too limited to discover the causes of plant development, to see the ultimate basis for their marvelous organization, and to explain how their growth protects them from the decay or destruction to which they would seem to be destined by the respective affinities of their elements. We have seen that oxygen gas was necessary for germination to take place. We will limit ourselves to investigating whether oxygen gas changes the seed by combining with it or rather by removing some element from it. Oxygen gas could also act as a stimulant, as an irritant, but I will not concern myself with this vague and hidden effect, which cannot be conclusively tested here.

When a seed is placed in oxygen gas to germinate, the oxygen gas disappears and is at the same time replaced by carbon dioxide [*de Saussure used the older term, "carbonic acid gas", throughout the book*]. Rollo,[3] who observed these effects **[p. 8]** in barley seeds, believed that the oxygen gas consumed was (1) for the most part absorbed by the seed; [*to de Saussure, the consumption of a gas meant its uptake, whereas absorption meant assimilation into the plant's dry matter*] and (2) formed, with the carbon of the seed, carbon dioxide. This opinion is only conjecture, not based on any precise analysis.

It seemed to me that we could clarify what happened in this regard during germination only by comparing the amount of oxygen gas consumed with the amount of carbon dioxide gas produced at the same time. If the amount of oxygen gas consumed exceeds the amount entering into the carbon dioxide formed in the process, we could conclude that the seed, in fact, absorbed oxygen gas, but if these two quantities are always equal, we must infer that the oxygen gas was not absorbed by the seed but was used only to remove carbon from the plant, by forming, with this carbon, carbon dioxide gas.

I gave details of my research on this subject, in the *Journal de physique*, year 7 [*of the French Republican calendar, i.e., 1799; see Note on the Translation*]. I carried out my research in bell-jars filled with ordinary air and closed by mercury. I introduced only a very small amount of water, just enough **[p. 9]** to elicit germination, in order that this fluid would not cause an appreciable loss [*via solution in water*] of the carbon dioxide produced in germination. I performed these experiments on seeds of pea, fava, haricot, barley, wheat ["*blé*"], lettuce, purslane, and garden cress. They all provided me the result that I have communicated [*presumably, in his paper of 1799*].

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[3] *Annales de Chimie*, Vol. 25, p. 37.

Carbon, in combining with oxygen gas through combustion, should not appreciably change the volume of the oxygen gas, for we find, by calculating according to the data of Lavoisier, that 100 cubic inches of carbon dioxide gas contain 49.67 grains of oxygen, which are 98 cubic inches of oxygen gas.[4] [*Note: In the Paris measurement system, which preceded the metric system and which de Saussure uses along with the metric system in this text, the units of measurement, such as inch, pound, grain, differ from the comparable English units; see Appendix 1.*] Germinating seeds, as well as burning carbon, do not appreciably change the volume of the oxygen gas that they transform into carbon dioxide. If, for example, we germinate seeds **[p. 10]** in 100 cubic inches of ordinary air containing 21 cubic inches of oxygen gas and 79 cubic inches of nitrogen gas [*de Saussure used the term "azote", which is still used for nitrogen in French*], we find that if germination produced 14 cubic inches of carbon dioxide, only 7 cubic inches of free oxygen gas remain in their atmosphere; or if 7 cubic inches of carbon dioxide gas are produced, 14 cubic inches of oxygen gas remain in the receptacle. The volume of oxygen gas consumed is thus equal to the volume of carbon dioxide gas that is produced at the same time. This result, which so far has admitted no exception, is an observation that should clarify for us the use of oxygen gas in germination, whatever opinion one adopts on the proportions of the constituent elements of carbon dioxide.

To observe the effects that I have communicated, the seeds must be in direct contact with oxygen gas. If they are deprived of it by a layer of water or by being too heaped up, they decay, and then yield hydrogen and carbon dioxide gases, whereas in germinating, they lose only carbon.

Rollo, who saw that moistened seeds in a medium free of oxygen gas **[p. 11]** yielded carbon dioxide, concluded that seeds develop this gas, or its two constituents, when they are in the presence of oxygen gas, but there is no comparison, no analogy to be drawn between a seed that germinates under the influence of oxygen gas and a seed that decomposes in a place totally devoid of it.

The effect of oxygen gas on cotyledons, even when they remain underground, as do those of favas and haricots, is useful to the growth of the plant after it germinates, for, when these seeds are germinated in water, the plant that arises can thrive in air only insofar as the cotyledons are above the surface of the liquid.

Seeds that germinate with the aid of water, under a receptacle full of pure oxygen gas, produce, under equivalent conditions, much more carbon dioxide gas than under a receptacle full of atmospheric air. Carbon dioxide, in all proportions, appears to be detrimental to the initiation of germination. If moistened quicklime is placed under the air-filled receptacle in which seeds are being germinated, in such a way that the seeds are not in contact with this earth, the growth **[p. 12]** of the radicles is a little accelerated. Carbon dioxide, however small the amount, added to the ordinary air in which seeds are being germinated retards germination more than does a similar amount of hydrogen gas or nitrogen gas.

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[4] Lavoisier found, in his experiment on the combustion of charcoal in oxygen gas, that the oxygen gas underwent a small decrease, equivalent to around 5/100 of its volume. This difference arises from the fact that charcoal cannot be entirely freed of hydrogen; therefore, the purer and drier the charcoal that was used, the less the difference (*Mémoires de l'Académie des Sciences,* 1781).

I could see no difference in the time of germination of seeds placed at the same time in ordinary air and in pure oxygen gas. Nor have I found that pea, wheat ["*blé*"], or cress seeds placed in the open air on two sponges, one of which was wetted with water strongly oxygenated by a compression pump and the other with boiled water, germinated at different times. When I prolonged the experiment, keeping the radicles immersed in the boiled and oxygenated waters, they always elongated less in the oxygenated water. This effect may be attributed to two different causes: one, that pure oxygen gas removes too much carbon from the seeds; the other, that carbon dioxide, if more abundant, is harmful to their early development. In general, carbon dioxide seems useful to plants only insofar as they can decompose it. Seeds, before the **[p. 13]** development of their plumule, do not seem able to bring about this decomposition.

It may be superfluous to note that seeds, depending on their species, require different amounts of oxygen gas to begin germinating. The seeds of haricot, fava, and lettuce consume, at equal weights, more of it than do peas, and peas more than wheat ["*blé*"], barley, and purslane. It is very difficult to determine these amounts accurately, by numbers, because there is always some uncertainty about the precise time of germination. The amount of oxygen gas consumed to elicit the beginning of germination appeared to me to be the same for haricot, fava, and lettuce, around a hundredth part of their weight. The oxygen gas consumed by wheat ["*froment*", i.e., *Triticum sativum*], barley, and purslane seeds is equal to a thousandth or two thousandth part of their weight. The carbon that these seeds lose at the same time is only about a third of these amounts.

The amount of oxygen gas used by the same [*kinds of*] seeds for germination is, other things being equal, proportional to their weight and not to their number. I chose four large favas whose weight was equal to that of 23 other, smaller ones. I germinated **[p. 14]** the large and small favas at the same time, but separately, in two receptacles closed by mercury. In this operation, the four large favas consumed as much oxygen gas as the 23 other, smaller ones. A large seed thus requires more oxygen gas to develop than does a small seed of the same species. The small seed can germinate more deeply in the ground than the former. If experiment demonstrates that large seeds develop at a depth where small ones perish, it is not because the small ones do not germinate there [*i.e., it is not because oxygen levels deep in the ground are too low*], it is because their plumules, which are weaker, cannot lift the soil that covers them.

1.3 Of the Changes that a Seed Undergoes Through Germination

In germination, we see no convincing evidence of the decomposition of water, for the seeds emit neither hydrogen gas nor oxygen gas. Oxygen makes up part of the carbon dioxide gas that the seeds produce, but the oxygen belonged to **[p. 15]** the atmosphere of the seeds before germination.

Seeds submerged in water or placed with this fluid in pure nitrogen gas emit carbon dioxide, methane [*de Saussure's term for methane is "carburetted hydrogen gas"*], and nitrogen gas, but these emissions are of constituents that have separated from the substance of the seed itself when it ferments. They are observed only when the seed begins to putrefy, never during its growth or development, or even during fermentation when that process occurs in direct contact with oxygen gas.[5]

[p. 16] Some seeds, when germinating in atmospheric air, pass from a mucilaginous to a sweet state. Rollo noted that the seeds do not undergo this change of taste in pure water or in oxygen-free environments, and analysis showed long ago that sugar contains more oxygen than does mucilage. It was therefore concluded that sugar does not form in a germinated seed unless the seed combines with the oxygen gas in the atmosphere or decomposes the surrounding water.

These inferences or explanations are unnecessary, for the mucilage of seeds is formed of hydrogen, oxygen, and carbon, and the proportion of oxygen in this compound can as well be increased by the removal of another element (the carbon) as by the addition of foreign oxygen gas.

When, with the aid of pure water in a closed vessel, I germinated any seed that had been fully dried before the experiment, **[p. 17]** and then dried this same seed after its germination [*the second drying kills the seed*], I always found that this germinated, dried seed weighed less than the same seed before germination, although I added back to it, by calculation, the carbon that the seed had lost in carbon dioxide either during growth or during drying, and also the small amount of mucilage or extract that it had left in the water in which it had developed.

This singular result can, I believe, be explained only by accepting that the seed has lost water that was previously fixed in its own substance. It remains to be determined whether this water is lost at the time of germination or after the death of the seed during drying. It seemed to me that it was only during this last operation, because when I doubled or tripled the growing time, the weight loss always remained the same, whereas when I prolonged the drying time, the loss was always greater. Seventy-three pea seeds, harvested 5 years earlier and placed for several weeks in a drying-oven heated continuously to 20° Réaumur, **[p. 18]** had a combined weight of 200 grains. They were placed, with five times their weight of distilled water, in a large flask filled with atmospheric air, closed by a glass stopper, and inverted in mercury. I withdrew these seeds after 2 days. They had almost all germinated, and during this development they had formed 4-1/2 cubic inches of carbon dioxide, which contained, following

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[5] I say in direct contact because the contact of atmospheric air with the water in which dead seeds are submerged singularly augments the development of hydrogen gas. Seeds placed under a little water covered by a layer of oil, or without oil under a receptacle filled with a small amount of water and sealed by mercury, can emit very little gas there, or at most three or four times their volume, and the plant does not deteriorate noticeably. But if the mercury or oil is removed, the emission of hydrogen gas is limited only by the almost complete disappearance of the seed. The mechanical pressure of the oil or mercury could stop this release, but in my experiments it was not great enough to produce this effect, because a column of water equal in weight to that of these external fluids did not stop the emission of the gas. I believe that the carbon dioxide that the water retains in greater quantity when the water is not in contact with the atmosphere serves as an antiseptic to the seed.

Lavoisier, 0.85 grains of carbon. The water remaining in the flask, evaporated at a gentle heat, left a dry residue of 0.75 grains of mucilage or extract.

I dried these seeds on a flat vessel in the same drying-oven in which they had germinated, and I examined, by comparative experiments made on other germinated seeds, the changes that they caused in their atmosphere during drying. I found that they produced no hydrogen gas and that they formed, as in germination, carbon dioxide gas with the surrounding oxygen gas, without producing carbon dioxide entirely from their own substance. The amount of carbon lost by the dead seeds appeared to me less, during the same time and at the same temperature, than during growth. They took 2 days to dry, or the same length of time as to germinate. **[p. 19]** For brevity, I will suppose that the amount of carbon they lost in these two operations was the same. The seeds, in drying, gave no indication of putrefaction or deterioration, either by odor or other products, or by external appearance. Their volume alone diminished appreciably. These seeds, based solely on the results that I have reported, should have had a dry weight of $200 - 0.85 \times 2 - 0.75 = 197.5$ grains. Yet I found that they weighed only 189 grains. They could have lost only water, other than the substances I have mentioned, and this lost water amounted to about 8 grains.

I repeated this experiment, at the same temperature, on identical seeds, leaving them to grow or germinate in the flask for 4 days instead of 2. They lost very nearly twice as much carbon, but only 8 grains of water, as in the preceding experiment. The drying time remained the same. The amount of lost water increased when I dried them for a longer time, exposing them to a lower temperature and in a more humid place.

These experiments were repeated on haricots and favas, with similar results. **[p. 20]** The dry, germinated seed weighed less than the dry, ungerminated seed, and this difference was much greater than should result from the loss of pure carbon and of extract.

As the seed, in drying and germinating, does not change the volume of its atmosphere, and as the oxygen gas that the seed causes to disappear from the atmosphere is recovered in an exactly equal amount in the carbon dioxide gas produced, we must accept that atmospheric oxygen gas has no direct part in the formation of this water, or, in other words, that the oxygen does not combine with the hydrogen of the plant to form the water. The water arises entirely from the substance of the seed itself.

The seed thus loses, in the form of water, a part of its oxygen and hydrogen, and this loss should increase the proportion of its other substances, particularly carbon. Indeed, I found that 100 parts by weight of dried seeds, which had germinated in a closed vessel in pure oxygen gas, contain more carbon than 100 parts of dry, ungerminated seeds. Taking the mean of several observations, 100 pounds of pea seeds yielded, by carbonization, 17-3/4 pounds of charcoal, **[p. 21]** whereas 100 pounds of the same seeds, dry, that had germinated in a closed vessel in oxygen gas, produced 18 pounds of charcoal.

This loss of water and the resulting increase in the proportion of carbon occurs only after the death of the plant. A seed growing in a closed vessel in an atmosphere of pure oxygen gas does not lose water that was previously fixed; neither does it fix it. It only loses carbon. The direct effect of oxygen gas on the dead seed[6] and on the germinating seed is essentially the same. In both cases, it is limited strictly to the removal of carbon. But the results are different: The dead seed loses fixed water, namely, oxygen and hydrogen, and the germinating seed retains them.

1.4 Of the Influence of Light on Germination

Senebier is the first natural philosopher who has been led to admit that light is **[p. 22]** harmful to germination. This opinion was confirmed by the comparative experiments carried out by Ingen-Housz in the shade and in sunlight. Moreover, it has perhaps also been confirmed by the practice of gardeners, who have found it advantageous to protect their seedbeds from the direct effects of solar rays. [*It is now known that some seeds, especially small ones, require light to germinate*].

It remains to be determined if the harmful effect of this star is attributable to its heat, which could destroy the plant, or to the light alone, considered separately.

It has been believed that the influence of light was seen here, because the comparative experiments, in shade and sun, were performed at the same temperature according to the thermometer. But it must be noted that this instrument, placed under a receptacle in the atmosphere of the seeds, does not indicate the real heat that the seeds experience on their surface due to the effect of solar rays. This heat is so promptly dispersed by the surrounding bodies that it eludes our instruments. It may be brought to the level of incandescence, as Rumford observed.[7] A small plant should be especially affected by it, **[p. 23]** as all its organs are conjoined in a smaller space, it transpires less, and it decomposes less carbon dioxide.[8] Now, experiment shows that young plants transpire less, as they are less developed, and that young leaves decompose, at the same volume, less carbon dioxide than do adult leaves.

I tried to germinate precisely weighed seeds, simultaneously, under two equivalent receptacles, one opaque and the other completely transparent, but the seeds under the transparent receptacle received only diffuse sunlight, such as reaches us through a thick layer of clouds. The temperature was exactly the same in the two experiments, according to the reading of a very sensitive thermometer. I could see no difference in the time of germination of the seeds placed under these two vessels. When the trial was prolonged on the germinated seeds, the seeds under the transparent receptacle gained more weight and their growth was more vigorous and more advanced. The plumules of the seeds **[p. 24]** in darkness were more slender and elongated due to the effect of etiolation. I believe I can conclude from these experiments that there is no evidence that light has, apart from the heat that accompanies it, a detrimental influence on the germination and growth of young plants.

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[6] I am not speaking here of effects that occur in the final stages of putrefaction. I will return to those later.

[7] *Essais politiques, économiques, et philosophiques*, Vol. 2, p. 273.

[8] The decomposition of carbon dioxide should produce cold, since composing it yields heat.

Lefebure[9] notes that it is not by drying the seeds that the sun is detrimental to their germination, because he observed that germination occurs more readily under water in the shade than in sunlight. Although the reasons that I have given for the difficulty of estimating the very intense heat that the surface of seeds placed in air in the sun may acquire could be applied here, they are not the only cause of the results obtained by that author. Growth under water may also be retarded in the sun because the air that is disseminated in the water, and with whose aid germination could have occurred in the shade, is partly disengaged through the influence of the solar rays.

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[9] *Expériences sur la Germination des Plantes*, by Lefebure, p. 136.

[p. 25] Chapter 2
Influence of Carbon Dioxide Gas on Plant Growth

I would perhaps have followed a more methodical order if I had addressed the influence of oxygen gas on mature plants before proceeding to an examination of the effect of carbon dioxide gas, which is only secondary and which is exerted only in conjunction with oxygen gas, but a knowledge of the effects of oxygen gas assumes preliminary data that force me to invert the natural order.

2.1 Influence of Carbon Dioxide Gas on Germination

Seeds do not germinate in pure carbon dioxide gas. A small amount of this gas (such as one-twelfth), which, when mixed with atmospheric air, promotes the **[p. 26]** growth of developed plants in the sun, is detrimental to germination and retards it, both in the light and in the shade, more than does the same amount of hydrogen gas or nitrogen gas. If we introduce, under a receptacle in which seeds have been germinated in pure water and atmospheric air, some potash or a [*i.e., another*] substance capable of absorbing the carbon dioxide gas that the seeds form with the surrounding oxygen gas, their development is accelerated somewhat. It always seemed to me that germination occurs sooner in moist sand or between two wet sponges than in humus, and humus produces carbon dioxide gas. In general, this gas seems useful to plants only insofar as they can break it down, and seeds, at the earliest stage of their development, do not seem to carry out this decomposition appreciably. We note, however, that, as germinating seeds produce too much carbon dioxide gas for us to deprive them of it entirely, it is impossible to decide if its complete absence is harmful or helpful to them.

2.2 Influence of Carbon Dioxide Gas on Developed Plants [p. 27]

When recently sprouted seeds are nourished with water lightly impregnated with carbon dioxide gas, the water appears to be less advantageous to them than it is at later stages of their growth. In two jars, one of which was filled with distilled water

J. F. Hill, *Chemical Research on Plant Growth*,
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and the other with acidulated water,[1] I floated two plates pierced with 24 holes, which were intended to support the same number of pea seeds germinated in distilled water. When the experiment began, their radicles were 6 millimeters (2 1/2 lines) long.

After 10 days, the roots in contact with the distilled water had elongated 1.3 decimeters (5 inches) more than had those **[p. 28]** in the gassed water. The stems and leaves had developed in the same proportion. But when, after a month, the plants nourished by the acidulated water were more developed, they no longer differed from those that had grown with the aid of pure water and that had attained their maximum growth several days previously. The latter, in turn, were even surpassed by the former, for, after 6 weeks, the pea plants in the gassed water had gained 46.4 grams (12 *gros* + 10 grains), [*"gros" is an obsolete French unit of measurement, equal to 1/8 French ounce; see Appendix 1*] whereas those grown in pure water had gained 45.5 grams (11 *gros* + 66 grains).[2] [*Note: De Saussure made an error in decimal placement here: He wrote 4.55 grams instead of 45.5. Also, he referred to footnote 1, when he clearly meant footnote* 2]. It is important to note that Senebier found that young leaves decompose, at equal volume and in the same time, less carbon dioxide than do adult leaves.

[p. 29] In the experiments just communicated, the stems of the plants grew in the open air and received almost entirely through the roots the extra carbon dioxide that was provided to them dissolved in water. It remains for me now to consider whether this gas is useful to plants when it serves as their atmosphere.

Percival (*Memoirs of the Manchester Society*, Vol. 2) observed that a mint plant nourished by water and exposed to a stream of atmospheric air mixed with carbon dioxide fared better than did a similar plant exposed to a stream of pure atmospheric air.

I tried to verify this initial observation and to determine the amount of carbon dioxide that, when mixed with atmospheric air, may promote plant growth.

I germinated peas with the aid of water, until each plant had reached about 1 decimeter (4 inches) in height and weighed 1 gram (20 grains). I then placed three plants for each experiment in a stemmed glass filled with water, such that the roots alone were immersed in this liquid, and I presented them with various mixtures of ordinary air and carbon dioxide in receptacles closed by water that was **[p. 30]** covered on the interior by a layer of oil if the receptacles contained more than half their volume of carbon dioxide. The three pea plants in each experiment had an atmosphere of 990 cubic centimeters (50 cubic inches) and they did not displace 1/400 part of it. Each day, for 5–6 hours, they received direct sunlight, which was moderated when

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[1] At first this water contained about a quarter of its volume of carbon dioxide, but as it was exposed to the open air in the sun, it retained only a much smaller amount during the general course of the experiment, which lasted 6 weeks. During this period, the gassed water was renewed four times.

[2] Rückert (*Annales de Crell*) found that favas and violets planted in pots full of garden humus fared better when they were sprinkled with water containing a third of its volume of carbon dioxide than when they were sprinkled with distilled water. I did not find a detectable difference, in performing the same experiment on wheat [*blé*]. It is possible that my humus, which was richer in manure, provided the plants excess carbon dioxide.

too intense. At the same time and in the same place, I set up similar apparati exposed to a weak and diffuse light. I call the latter manner of placement *exposure to shade.*

2.2.1 Results in Sunlight

The mean weight gain of the plants exposed to sunlight for 10 days in pure atmospheric air was 425 milligrams (8 grains) per pea.[3]

[p. 31] These plants, at the same exposure, wilted as soon as they were in contact with pure carbon dioxide.

They met the same fate in the atmospheres containing 3/4 and 2/3 of their volume of carbon dioxide.

They grew for 7 days in the vessel containing 1/2 its volume of carbon dioxide. After this period, they stopped growing.

The plants whose atmosphere contained 1/4 of its volume of carbon dioxide survived for the 10 days devoted to the experiment but they did not thrive. Each pea gained only 265 milligrams (5 grains).

With 1/8 carbon dioxide, the mean gain was 371 milligrams (7 grains).

Finally, the mean gain of each plant in an atmosphere of ordinary air in which carbon dioxide occupied 1/12 part was 583 milligrams (11 grains). I repeated this experiment several times, and **[p. 32]** the plants consistently fared better than in pure atmospheric air. The plants that grew in pure atmospheric air did not change it perceptibly, either in purity or in volume, but those that grew in the artificial mixture changed nearly all the carbon dioxide gas into oxygen gas.

I performed another experiment, which confirms this one and shows directly that humus is useful to plants, not only through the nutrients that the plants may draw from it through their roots, but also through the influence that humus has on the atmosphere (influence that, as we know, consists largely of forming carbon dioxide). I suspended, at the upper part of a receptacle containing about 3 liters (150 cubic inches) of atmospheric air, 61 grams (2 ounces) of moistened humus. With this receptacle, closed by water, I covered some partially developed pea plants, whose roots were immersed in pure water during the experiment. After 10 days in the sunlight, these plants, which did not touch the humus at all, had grown one-third more than other, similar plants placed at the same time, without humus, under a receptacle similar to the preceding one. **[p. 33]** But I must note that I renewed the air of the receptacles twice in 24 hours, for without this precaution the plants growing with the humus would have fared less well, either because the humus releases too much carbon dioxide or because it produces vapors or miasmas that, in an unrenewed air and through an unknown cause, are highly detrimental to plant growth.

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[3] This weight gain was due largely, and maybe even entirely, to the introduction of liquid water, that is, water of vegetation, into the leaves that developed during the experiment and that drew their solid substance from the cotyledons, which were still large and attached to the plant. These cotyledons contained three or four times less water of vegetation than the leaves to whose development they contributed. I will return elsewhere to this subject, which is unrelated to what occupies me here.

2.2.2 Results in the Shade

In the apparatus exposed to shade, the smallest amount of carbon dioxide added to ordinary air was detrimental to plant growth. In the atmosphere containing 1/4 of its volume of carbon dioxide, the plants were dead after the sixth day. They survived, at the same exposure, for 10 days in an atmosphere in which carbon dioxide occupied 1/12 part, but their gain there was only 159 milligrams (3 grains), whereas it was 265 milligrams (5 grains) in pure atmospheric air.

We have just seen that carbon dioxide added artificially in very small proportions to the atmosphere of plants **[p. 34]** is useful to their growth in the sun, but it exerts this beneficial effect only insofar as this atmosphere contains free oxygen gas. Thus, plants that can maintain their growth in nitrogen gas die in it even in the sun when it is combined with the proportion of carbon dioxide gas that would have promoted their development in atmospheric air.

2.3 Elaboration of Carbon Dioxide Gas by Leaves is Necessary to Their Growth in the Sun

The experiments that I have detailed on the growth of peas in pure atmospheric air yielded the same results when I washed this air in limewater and thus removed the very small amount of carbon dioxide that this air contains naturally. But the results were very different when I introduced into the atmosphere of the plants a substance that absorbs the carbon dioxide that the plants contribute to forming. I suspended, at the top of the receptacles covering the peas, 7 or 8 grams (2 or 3 *gros*) of lime slaked with water **[p. 35]** and then dried quickly by the heat of boiling water. I rested the openings of these receptacles on saucers filled with limewater.

From the second day, the atmosphere of the plants exposed *to the sun* in this apparatus decreased in volume. The third day, the lower leaves began to turn yellow, and between the fifth and sixth days, the stems died or were completely defoliated. The atmosphere of the plants, examined at this time, was found to be corrupted [*"vitiated"*]. It contained no more than 16/100 oxygen gas. [*Ordinary air is approximately 21 % oxygen, 79 % nitrogen, by volume*]. Peas that had grown at the same time without lime, under receptacles filled with ordinary air, had changed the air neither in purity nor in volume, and all their parts were healthy and vigorous. We see by the experiment with lime that there was absorption [*by the lime*], and consequently formation of carbon dioxide gas, for the absorbing substance acted only on this gas. We see, further, that the presence, or rather, the elaboration, of carbon dioxide is necessary to plant growth in the sun.[4] Finally, we **[p. 36]** find that when we do not

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[4] One might think that the withdrawal of the part of the atmospheric oxygen gas retained by the carbon dioxide in the lime was the reason why growth ceased, but developed pea plants can survive in pure nitrogen gas. Lime and potash exert their full deleterious influence on marsh plants, which thrive as well in pure nitrogen gas as in atmospheric air.

see carbon dioxide production by plants growing without lime in ordinary air, it is because they decompose it as they form it with the surrounding oxygen.

In the shade I obtained a different result. Not only did the plants not die in the receptacle containing the lime and limewater, but they fared better there than in a similar receptacle lacking these substances.

The mean weight gain of each plant growing with the lime was 371 milligrams (7 grains) in the period of 10 days. The air of the receptacle contained 3/100 carbon dioxide after the experiment. But in the ordinary air without **[p. 37]** lime, each pea acquired [*on average*] only 212 milligrams (5 grains). Limewater showed 11/100 carbon dioxide in this atmosphere.

It is evident from these results that one cannot judge the effect of the complete deprivation of carbon dioxide on plant growth in the shade because too much of this gas is produced in this case for the lime to absorb it all as it is formed, but that the effect of a partial deprivation is to promote growth.

For the same reason, lime does not cause plants that are growing in the sun in an atmosphere of pure oxygen gas to lose their leaves. This atmosphere has a superabundance of carbon dioxide gas that the alkaline earth did not have time to absorb as quickly as it was formed.

The above observations were made only on plants growing in pure water, and it was important to make sure that the same results would be obtained with plants rooted in plant mold. But this earth could not be placed under a receptacle because the earth produces too much carbon dioxide for the lime, which acts only at a distance, to be able to remove it before its elaboration by the plant.

I put 31 grams (1 ounce) of the same lime that I used in the preceding experiments into a glass globe. **[p. 38]** I moistened the lime slightly to remove any doubt as to its dehydrating properties. Into the globe, which had a capacity of about 4 liters (200 cubic inches), I then placed a woody branch,[5] covered with leaves, that was exposed to the sun and whose roots were in plant mold. I took care that the leaves touched neither the lime nor the walls of the globe, whose neck was carefully luted [*cemented*] to the branch. I fitted a similar apparatus, but without lime in the globe, to a branch located next to the preceding one. This branch retained its freshness for more than 2 months, but it was not so for the branch growing with the lime. Its leaves remained green for 12 days, then began to dry and, after 3 weeks, they had all fallen. The branch was not dead. A month later, it sprouted new leaves in the globe, which **[p. 39]** had not been unsealed. But at this time, the lime no longer had an effect on the surrounding air. Its surface was saturated with carbon dioxide. I withdrew it and found that it effervesced with acids.

I must note that quicklime and potash have no very noticeable effect on the growth of succulent plants, because, with their very thick parenchyma and their epidermis that is less porous than that of other plants, they retain carbon dioxide more tenaciously. For the same reason, the stems of all plants are much less affected than are the leaves in these experiments.

[5] The plants on which I performed these experiments were honeysuckle (*Lonicera caprifolium*), plum (*Prunus domestica*), privet (*Ligustrum vulgare*), and peach (*Amygdalus persica*).

This new growth shows that the leaf shedding was due not to deprivation of oxygen gas, which could be retained in the lime by the absorption of carbon dioxide gas, but only to the absence of carbon dioxide. If the effects occurred more slowly in these experiments than in the previous ones, it is partly because the plants rooted in soil were not deprived of the effects of the carbon dioxide that they received from the plant mold through the roots, but only of the external effects of this gas on the leaves. [*Although de Saussure says here that plants obtain some carbon via carbon dioxide in the soil, he generally considers the main source of plant carbon to be atmospheric carbon dioxide.*]

2.4 Of the Decomposition of Carbon Dioxide Gas by the Green Parts of Plants

Priestley was the first to recognize that leaves have the property of improving air corrupted by combustion or respiration, but he did not trace the cause of this phenomenon. Senebier discovered that leaves **[p. 40]** decompose carbon dioxide by appropriating its carbon and eliminating its oxygen. He noted that fresh leaves exposed to the sun, in spring water or water lightly impregnated with carbon dioxide gas, produced oxygen gas as long as some carbon dioxide remained in the water. He saw that when this gas was used up, and when leaves in distilled water were exposed [*to the sun*], they did not produce more air than could be interposed in their own volume. But no one has yet analyzed the effects of the decomposition of carbon dioxide, or seen if the amount of oxygen gas eliminated is greater than, less than, or equal to the amount in the carbon dioxide gas. The following experiments were intended to answer this question. I will go into this subject in long and minute detail, but without it the results would be almost meaningless.

2.4.1 First Experiment

2.4.1.1 On Periwinkle (*Vinca minor* L.)

I composed, from carbon dioxide gas and ordinary air that **[p. 41]** the phosphoeudiometer [*see "eudiometer" in Glossary and discussion in footnote 9 of Introduction to the translation*] showed to be 21 hundredths oxygen, an artificial atmosphere occupying 5.746 liters (290 cubic inches [*original text erroneously says 290 cubic centimeters*]). Limewater showed it to be 7-1/2 hundredths carbon dioxide. [*Note: The amount of carbon dioxide in ordinary air is so small that virtually all the carbon dioxide de Saussure measured in these experiments is the added gas.*] The gaseous mixture was confined in a receptacle closed by mercury that was wetted, or covered with a very thin layer of water, to prevent contact of this metal with the air surrounding the plants, for I have observed that such contact, as reported by the Dutch chemists, is harmful to plant growth in prolonged experiments.

Under this receptacle I placed seven periwinkle plants, each 2 decimeters (8 inches) tall. They displaced a total of 10 cubic centimeters (1/2 cubic inch). Their roots were immersed in a separate vessel containing 15 cubic centimeters (3/4 cubic inch) of water. The amount of this liquid under the receptacle was insufficient to absorb appreciable carbon dioxide, especially at the ambient temperature, which was never less than + 17° Réaumur [*about 21 °C, or room temperature*].

This apparatus was exposed to direct sunlight for 6 consecutive days, from 5 to 11 o'clock in the morning, and shaded whenever the light became too intense. On the seventh day, I removed the plants, **[p. 42]** which had not deteriorated in the least. Their atmosphere, all corrections made [*presumably for temperature and pressure*], had not changed in volume, at least as far as one could judge in a receptacle of 1.3 decimeters (5 inches) diameter, in which a difference of 20 cubic centimeters (1 cubic inch) is almost imperceptible; but the error can be no greater than this.

Limewater showed no remaining carbon dioxide gas. The eudiometer indicated 24-1/2 hundredths oxygen gas. I set up a similar apparatus with pure atmospheric air and the same number of plants at the same exposure. This atmosphere changed neither in purity nor in volume.

From the eudiometric observations noted above, it follows that, before the experiment, the mixture of ordinary air and carbon dioxide contained:

4199 cubic centimeters (211.92 cubic inches) of nitrogen gas
1116 cubic centimeters (56.33 cubic inches) of oxygen gas
431 cubic centimeters (21.75 cubic inches) of carbon dioxide gas
5746 cubic centimeters (*290 cubic inches*)

After the experiment, the same air contained:

4338 cubic centimeters (218.95 cubic inches) of nitrogen gas
1408 cubic centimeters (71.05 cubic inches) of oxygen gas
0 cubic centimeters (0 cubic inches) of carbon dioxide gas
5746 cubic centimeters [*290 cubic inches—but de Saussure omitted this figure*]

[p. 43] The periwinkles therefore elaborated, or removed, 431 cubic centimeters (21-3/4 cubic inches) of carbon dioxide gas. Had the plants eliminated all of the oxygen gas from it, they would have produced a volume of oxygen equal to that of the carbon dioxide that disappeared. But they released only 292 cubic centimeters (14-3/4 cubic inches) of oxygen. Therefore they assimilated 139 cubic centimeters (7 cubic inches) of oxygen in decomposing the carbon dioxide, and produced 139 cubic centimeters (7 cubic inches) of nitrogen gas [*an error; plants do not release nitrogen gas*.]

A comparative experiment showed me that the seven periwinkle plants that I used had a dry weight of 2.707 grams (51 grains) before the decomposition of the carbon dioxide, and yielded, through carbonization by fire in a closed vessel, 528 milligrams (9.95 grains) of charcoal. The plants that decomposed the carbon dioxide were dried and carbonized by the same technique and yielded 649 milligrams (12.23 grains) of charcoal. The decomposition of the carbon dioxide therefore caused a gain of 120 milligrams (2.28 grains) of charcoal.

I similarly carbonized periwinkles that had grown in atmospheric air freed of carbon dioxide, and I found that the **[p. 44]** proportion of their carbon had decreased rather than increased during their stay under the receptacle.

2.4.2 *Second Experiment*

2.4.2.1 On Aquatic Mint (*Mentha aquatica* L.)

The mixture of ordinary air and carbon dioxide that served as atmosphere for two mint plants, each 3.5 decimeters (13 inches) tall, and together displacing 10 cubic centimeters (1/2 cubic inch), occupied 6.5 liters (328 cubic inches). Limewater indicated 7-1/4 hundredths carbon dioxide. Before the addition of this gas, the ordinary air contained 21 hundredths oxygen gas. The arrangement of the apparatus was the same as in the previous experiment.

After 10 days the plants had elongated by 1 decimeter (4 inches) and had sprouted long roots, but the volume of their atmosphere had not changed. Limewater indicated no more than 2-1/2 hundredths carbon dioxide gas in the artificial mixture at this time. After removal of the carbon dioxide, this atmosphere contained 23-1/2 hundredths oxygen gas.

[p. 45] Ordinary air without mixture, in which two mint plants were grown at the same time, did not change, either in purity or in volume.

The mints in the preceding experiment therefore removed 309 cubic centimeters (15.6 cubic inches) of carbon dioxide gas. They eliminated 224 cubic centimeters (11.26 cubic inches) of oxygen gas from it. They retained 86 cubic centimeters (4.34 cubic inches) of oxygen gas in elaborating the carbon dioxide gas, and replaced the oxygen gas absorbed by a nearly equal amount of nitrogen gas.

Through carbonization, I found that these plants had increased their charcoal content in this experiment, and that this increase was no greater in those that had grown under a receptacle filled with pure atmospheric air.

2.4.3 *Third Experiment*

2.4.3.1 On Loosestrife (*Lythrum salicaria*)

The mixture of atmospheric air and carbon dioxide gas used for this experiment occupied 1.486 liters (75 cubic inches). Limewater indicated 10/100 carbon dioxide in it. Before the addition **[p. 46]** of the carbon dioxide, the ordinary air contained 21/100 oxygen gas. The loosestrife displaced 2.8 cubic centimeters (1/7 cubic inch). The arrangement was the same as in the previous experiments. The receptacle that I used here was 9 centimeters (3.5 inches) in diameter, and I could not be mistaken by more than 5 cubic centimeters (1/4 cubic inch) in the estimation of volumes.

After 7 days devoted to this experiment, not a single leaf of the loosestrife had turned yellow. The atmosphere had decreased by 10 cubic centimeters (1/2 cubic inch). It then no longer contained any carbon dioxide gas, and the eudiometer indicated 27-1/4 hundredths oxygen gas in it.

Another loosestrife, grown at the same time under the same conditions, in pure atmospheric air, changed the air neither in purity nor in volume.

Based on the eudiometric results reported above, the plant that stayed in the artificial mixture removed 149 cubic centimeters (7-1/2 cubic inches) of carbon dioxide gas, and eliminated from it 121 cubic centimeters (6.13 cubic inches) of oxygen gas. It assimilated 27 cubic centimeters (1.37 cubic inches) of oxygen gas from the carbon dioxide gas, **[p. 47]** and it produced 21 cubic centimeters (1.1 cubic inches) of nitrogen gas.

2.4.4 Fourth Experiment

2.4.4.1 On Pine (*Pinus genevensis*)

The mixture of ordinary air and carbon dioxide occupied 5.549 liters (280 cubic inches). Limewater indicated 7/100 carbon dioxide. For 18 days, I left in it a young pine, 2.4 decimeters (9 inches) tall and 10 cubic centimeters (1/2 cubic inch) in volume. After this period, the atmosphere had decreased by 39 cubic centimeters (2 cubic inches), as far as I could estimate in a receptacle that was 1.6 decimeters (6 inches) in diameter.

The eudiometers indicated 1-1/2 hundredths carbon dioxide gas there, and after this was removed, 24-1/4 hundredths oxygen gas.

Another pine, grown for the same time under a receptacle filled with pure atmospheric air, made no detectable change in it.

The plant in the artificial mixture removed 306 cubic centimeters (15-1/2 cubic inches) of carbon dioxide gas. The plant disengaged **[p. 48]** 246 cubic centimeters (12-1/2 cubic inches) of oxygen gas and retained 60 cubic centimeters (3 cubic inches) of oxygen in the decomposition of the carbon dioxide. Finally, the plant produced 20 cubic centimeters (1 cubic inch) of nitrogen gas.

2.4.5 Fifth Experiment

2.4.5.1 On Raquette (*Cactus opuntia*)

The mixture of ordinary air and carbon dioxide gas occupied 3.012 liters (155 cubic inches), and limewater indicated 10/100 carbon dioxide gas. The cactus displaced 22 cubic centimeters (1-1/10 cubic inches). It remained for 8 days under the receptacle, exposed to the sun's full intensity. I had moderated this effect for the other plants, which would have been harmed without this precaution, but here there was not the same risk, and without strong light, the carbon dioxide would have decomposed too slowly.

When I withdrew the plant, the volume of its atmosphere had not changed detectably. The eudiometers indicated 4/100 carbon dioxide gas in it, and after the removal of this gas, 24/100 oxygen gas. A cactus identical to the **[p. 49]** preceding, grown for the same time in an equal volume of pure atmospheric air containing 21/100 oxygen gas, made no detectable change.

The plant therefore removed 184 cubic centimeters (9.3 cubic inches) of carbon dioxide gas from the artificial mixture. It eliminated from this gas 126 cubic centimeters (6.4 cubic inches) of oxygen gas. The plant assimilated 57 cubic centimeters (2.9 cubic inches) of oxygen gas in decomposing the carbon dioxide and replaced the oxygen gas absorbed by a nearly equal amount of nitrogen gas.

It follows from all these experiments that plants, in decomposing carbon dioxide gas, assimilate a part of the oxygen that it contains.

2.5 Plants Grown with Pure Water in the Open Air Take Carbon from the Small Amount of Carbon Dioxide Gas that Occurs Naturally in Our Atmosphere

The previous observations show that plants in closed vessels decompose **[p. 50]** carbon dioxide gas when it is mixed with atmospheric air in much larger proportions than occur naturally.

It is now appropriate to see whether plants carry out this decomposition in the open air, which contains hardly more than 1/500 carbon dioxide by volume. Hassenfratz, in a memoir on plant nutrition (*Annales de Chimie*, Vol. 13), sought to establish that plants growing in pure water and the open air increase in volume with the aid of the water alone, and that after they develop they contain an absolute amount of carbon less than that which was present in the seed. On this subject, I did several experiments, which yielded results contrary to those of this author. I will report two examples.

First Experiment I immersed the roots of several peppermint plants (*Mentha piperita*) in bottles filled with distilled water, and I grew these plants in the sun, on an exterior window sill protected from the rain. By drying some similar plants that had been uprooted at the same time and place, **[p. 51]** I ascertained[6] that 100 parts by weight of those that I grew in distilled water contained 40.29 parts of dry plant substance, from which 10.96 parts of charcoal were withdrawn by carbonization.[7]

The 100 parts of mint, after 2-1/2 months of growth in the open air, had a green weight of 216 parts, but this weight gain revealed nothing at that point, since it

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[6] The absolute weight of the plants grown in distilled water was 7.6 grams (3 *gros*).

[7] For the procedure followed in this operation, see the note at the end of Chap. 5.

could have been due to the addition of water of vegetation, which always occurs in plants when they are transplanted to a place that is wetter than the one in which they had grown previously. By drying them at atmospheric temperature, they were reduced to a weight equivalent to 62 parts. The plants had therefore increased their dry substance, with the aid of air and water, by 21.71 parts. These 62 parts yielded, by carbonization, 15.78 parts of charcoal, or 4.82 parts more than they would have yielded before having grown in the distilled water. When I grew the same [*kind of*] plants **[p. 52]** under similar conditions in a weakly lit place, I found that they lost a small amount of their carbon. Lack of light may be the reason for the results obtained by Hassenfratz.

Second Experiment I placed four fava beans weighing 6.368 grams (120 grains) among flint pebbles in glass capsules. I wetted them with distilled water. After 3 months of growth in the sun, under the open sky, the fava plants, immediately after flowering, had a fresh weight of 87.149 grams (1,642 grains). They were reduced by drying to 10.721 grams (202 grains). Thus they had almost doubled their dry plant matter by growing in the open air. These plants yielded, by carbonization in a closed vessel, 2.703 grams (51 grains) of charcoal. But four fava beans of the same weight as those used in this experiment yielded 1.209 grams (22-3/4 grains) of charcoal. The favas had thus more than doubled the amount of carbon in their substance by developing with the aid of water in the open air, **[p. 53]** and there is no doubt that they accomplished this by the decomposition of the carbon dioxide that they found in the atmosphere, for we saw in the previous section that plants grown under receptacles filled with air that was pure and not renewed did not increase their carbon.

2.5.1 Further Remarks on the Decomposition of Carbon Dioxide Gas by Plants

Ingen-Housz[8] observed that green plants to which he had applied, in darkness, an artificial heat that was ostensibly the same as that received from the sun by other, similar plants yielded an impure air, while those exposed to the sun produced the opposite effect. He concluded that the production of oxygen gas by the sun-exposed plants was attributable to the light alone, abstractly considered, and not to the accompanying heat. But this conclusion may be premature, because the heat that accompanies light, **[p. 54]** and dark heat, act in very different ways in these experiments.

Luminous heat warms only the plant, due to the plant's opacity. This heat hardly warms the plant's surrounding medium at all, because of the medium's transparency. The medium removes from the plant some of the heat that the sun conveys to it and moderates an effect that, considered by itself, could destroy the plant.

Dark heat warms the transparent fluid and the plant to the same degree. The plant is harmed because the medium surrounding it can no longer rid it of the heat that has been conveyed to it.

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[8] *Experiences sur les végétaux*, Vol. 1, p. 39 [*de Saussure cited p. 36, but it should have been p. 39*], and Vol. 2, p. 167.

It is very likely that plants in atmospheric air, without the intervention of light, decompose a part of the carbon dioxide gas that they form themselves with the surrounding oxygen gas. But this effect cannot be shown directly. I have seen marsh plants, such as *Polygonum persicaria* and *Lythrum salicaria*, emit oxygen gas into an atmosphere of nitrogen gas in a low, diffuse light, but they never produced this effect in **[p. 55]** total darkness. It may be presumed, however, that plants decompose carbon dioxide gas there, based on the following observations. I grew some peas, loosestrifes, and fleabanes [*"inules"*] in profound darkness, beneath two identical receptacles filled with atmospheric air. I renewed [*presumably means replaced*] these plants each day so that they would not suffer harm. These experiments were done in duplicate. One of the receptacles contained quicklime or potash, the other neither of these. After 4 or 5 days, both atmospheres were corrupted, but I always found that the receptacles provided with lime or potash contained less oxygen gas than those that lacked alkali, and one imagines that it is because these plants found less carbon dioxide gas to decompose in the receptacles that lacked lime or alkali. [*This seems to be a mis-statement; the plants should have found less carbon dioxide to decompose in the receptacles that were provided with lime or alkali.*]

Another effect also seems to indicate the decomposition of carbon dioxide in darkness. It is the light green hue assumed by etiolated leaves that develop there. Acording to Senebier, this color is a result of the decomposition of carbon dioxide. [*Senebier was mistaken; the decomposition of carbon dioxide requires the green pigment plus light.*] But as these signs are very weak and very indirect, we will not generally accept, **[p. 56]** in the following, that the decomposition of carbon dioxide can occur anywhere but in the light.

Senebier recognized that non-green plant parts, such as wood, roots, most petals, white blotches on leaves, and leaves that have turned completely red or yellow in autumn through deterioration of their juices, do not exhale oxygen gas. It is not necessary, however, to infer from this that green color is essential to the plant parts that decompose carbon dioxide gas, or a necessary result of this decomposition, for the variety of *Atriplex hortensis* in which all the green parts are replaced by red or dark purple parts produced for me, under spring water, in 5 or 6 hours, seven or eight times its volume of oxygen gas, which contained only 0.15 of its volume of nitrogen gas. The variety of the same plant having green leaves did not provide me oxygen gas that was any purer or more abundant. [*Wrong conclusion; the red or purple pigment in the vacuoles of the mesophyll cells merely masks the green of the chlorophyll in the chloroplasts, where photosynthesis takes place as usual.*]

Not all kinds of leaves are able to decompose **[p. 57]** carbon dioxide gas to the same degree. In several experiments, *Lythrum salicaria* was able in one day to decompose up to seven or eight times its volume of it. *Cactus opuntia* and other succulents could decompose only a fifth or a tenth of this amount. Without wishing to identify all the causes of these differences, I will note that the green parts decompose carbon dioxide in proportion to their surface area, but hardly at all in proportion to their volume. Very thin leaves, and ones that are deeply cut and thread-like, decompose the most carbon dioxide, under equivalent conditions. Fleshy plants, stems, and fruits, which present little surface area, decompose, at the same volume, much less gas. [*Further,* *plants using the Crassulacean Acid Metabolism (CAM) pathway, which include many succulents, absorb and store carbon dioxide at night and close their stomata by day, so there is less gas exchange in the daylight; see Translator's Introduction.*].

Leaves, in exhaling oxygen gas, always release nitrogen gas. They produce nitrogen in appreciable amounts only in the light, and nearly in proportion to the carbon dioxide that they decompose. [*An error: plants do not release nitrogen gas.*] Leaves that can tolerate a very humid atmosphere the longest without damage are the ones that produce the purest oxygen gas. Marsh plants are among these. **[p. 58]** Whatever plant is placed in the unnatural conditions of these experiments, the oxygen gas produced at the beginning of the experiment is always less corrupted than that at the end. These observations indicate that the nitrogen gas arises from the very substance of the plant. I am inclined to believe that this gas, which has an affinity for pure oxygen gas, is removed in the interior of the leaf by oxygen gas in greater quantity the more listless the growth is. It is understandable why leaves develop much less nitrogen gas in atmospheric air to which carbon dioxide gas has not been added. It is because the leaves there have much less contact with pure oxygen gas. The oxygen gas contained by atmospheric air is saturated with the nitrogen gas that occurs there naturally.

2.6 Summary

Carbon dioxide gas added in certain proportions to atmospheric air promotes plant growth, but only insofar as the plants can decompose this gas. It is detrimental to germination in the same proportions in which it is useful to developed plants. It promotes the growth **[p. 59]** of developed plants in the sun and becomes detrimental to them in darkness.

The presence, or rather the elaboration, of carbon dioxide gas is indispensable to the growth of green plant parts in the sun. Sun-exposed plants die when the carbon dioxide gas that they form with the surrounding oxygen gas is removed.

Green plants growing with the aid of distilled water in open atmospheric air acquire a large amount of carbon there.

Plants, in decomposing carbon dioxide, assimilate a part of the oxygen contained in the carbon dioxide.

[p. 60] Chapter 3
Influence of Oxygen Gas on Developed Plants

3.1 Of the Changes that Oxygen Gas Undergoes Through its Contact with Leaves

In Chap. 6, I will treat the growth of green plants in environments previously depleted of oxygen gas. I will show that plants develop there only because they emit this gas there. The chemical results of the gas's effects on green, herbaceous parts of plants are the focus of my present research.

Some healthy leaves, cut after a serene summer day and placed for one night under a receptacle filled with atmospheric air, caused the surrounding oxygen gas to undergo changes that seemed to vary **[p. 61]** according to the nature of the plant.

1. The leaves of oak (*Quercus robur*), horse chestnut, false acacia, *Sedum reflexum*, and most plants diminish the volume of their atmosphere. Its oxygen gas disappears and it forms a volume of free carbon dioxide gas that is smaller than the volume of oxygen consumed during the experiment.
2. The leaves or green parts of *Cactus opuntia*, *Crassula cotyledon*, *Sempervivum tectorum*, *Agavé americana*, and *Stapelia variegata* diminish the volume of their atmosphere by absorbing oxygen without forming appreciable carbon dioxide. In this case, as in the previous one, it seems at first glance that the reduction in the volume of the atmosphere is due to the assimilation of oxygen by the plant.

In order to observe, consistently, a condensation in the air around the leaves in darkness, the leaves must be perfectly healthy and occupy between a 7th and a 20th of the volume of the air in which they are enclosed. If they occupy a smaller space, the effects will sometimes be **[p. 62]** undetectable; if they occupy a larger space, their environment will be too depleted of oxygen for them to thrive. They must also be placed in the experiment immediately after being cut at sunset and must not remain under the receptacle more than 12 hours. Since my plants did not wither in any of these trials, I cannot believe that plant deterioration affected my results. I should not suppose that it did, especially in the case where no carbon dioxide was formed, for all wilting leaves produce a certain amount.

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We may ask whether the oxygen gas absorbed by healthy leaves at night is assimilated effectively by them for augmenting their dry matter, for we will see that, if those leaves that absorbed this gas are exposed to the sun, they restore an approximately equal amount of the gas, and that if the leaves have enough growth force to remain healthy for several consecutive days under the same receptacle, they diminish their atmosphere each night and augment it each day in approximately the same proportion.

[p. 63] I give to these alternating effects the names expiration and inspiration. These expressions are perhaps inappropriate because they indicate in plant organs a contraction and expansion that has not yet been established by observation. I accept these designations only because the gaseous imbibition and emission by plants produces, in the final analysis, results analogous in some ways to those of respiration in animals, although the appearance and the means are very different. Anatomical comparision of plants and animals is misleading, but if only their overall physiological traits, such as nutrition, secretions, reproduction, and the influence of oxygen gas or respiration on their existence, are considered, without regard to the means by which these functions are carried out, a striking analogy must be admitted between these beings.

On matters that do not lend themselves to rigorous proof, accurate accounts of observations are more instructive than are general conclusions, which often depend on the observer's point of view. For this reason, I will detail my experiments **[p. 64]** on *Cactus opuntia*. Their results can be applied, with slight, easy-to-conceive, modifications, to the leaves of other plants and even to all herbaceous, green plant parts. But these parts do not all have enough growth force to withstand the same tests.

3.2 Experiments on Inspiration [this section title supplied by translator]

3.2.1 Inspiration by Cactus opuntia

I suspended, after sunset, some branches or leaves of cactus displacing 119 cubic centimeters (6 cubic inches) in a receptacle containing 951 cubic centimeters (48 cubic inches) of atmospheric air freed of its carbon dioxide. There was no water[1] in this vessel, which was closed by mercury.

[p. 65] The next day at sunrise I found, after correcting for changes in temperature and pressure, that the atmosphere of the plant had diminished by 79 cubic centimeters

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[1] Here is an example of the tremendous growth force of this plant. A cactus branch that had served for 3 weeks in trials similar to those that I have just reported, either in the sun or in the shade, was placed in darkness, without water or soil, in a cupboard for 14 months. There it endured cold of $-8°$ Réaumur in winter, and heat of $+22°$ in summer. At the end of this period, it was wrinkled and thinned from the loss of half of its water of vegetation, but it had sprouted stems and roots all over its surface. No part of it had lost its growth force. When it was taken out of there, it produced, under distilled water, in the sun, a third of its volume of an air containing 34/100 oxygen gas and 66/100 nitrogen gas. And when I planted it in vegetable mold, all its former substance swelled, recovered its original dimensions, and continued to produce new shoots.

(4 cubic inches). I then examined this air. It contained only 14/100 oxygen, whereas before the introduction of the cactus, it had contained 21/100 of this gas. Limewater showed not an atom of carbon dioxide in the remaining air [*presumably because the plant retained the carbon dioxide it had formed with the surrounding oxygen gas during the night*]. Further, I will say that when I placed some limewater beside the cactus under this receptacle for one night, no calcium carbonate [*"carbonate of lime"*] was formed. It follows from the eudiometric observations communicated above that the atmosphere of the plant contained, approximately:

Before inspiration	198 cubic centimeters (10 cubic inches) oxygen gas
After inspiration	119 cubic centimeters (6 cubic inches) oxygen gas
Inspiration	79 cubic centimeters (4 cubic inches) oxygen gas

[p. 66] The reduction in volume of the atmosphere during the night was thus precisely equal to the reduction in oxygen gas. The plant therefore did not appreciably absorb nitrogen gas, but only an amount of oxygen equal to three-quarters of its volume. Not all cacti make such large inspirations. Inspirations during a night often amount to only half the volume of the plant. These differences are due not only to the condition of the plant, which absorbs more the more vigorous it is, but also to the temperature at which the experiment is conducted. Inspiration is greater in a given time at a temperature of 20° or 25° Réaumur than at 10° or 15°.

If these plants are kept in darkness for more than one night, they continue to absorb oxygen, but always more slowly, until they contain about 1-1/4 times their own volume of it. After this time, which arrives in 36 or 40 hours they neither decrease nor increase the volume of their atmosphere as long as there is free oxygen in it, whatever the capacity of the receptacle and the duration of the experiment, even if it is prolonged for a month or until the death of the plant.

3.2.2 Leaves Saturated with Oxygen Gas Form Carbon Dioxide Gas in Darkness [p. 67]

A cactus that is saturated with oxygen gas by inspiration is not, however, without an effect on its surroundings. When the plant is at the point that it can absorb no more oxygen, it begins to form, without deteriorating in the least, free carbon dioxide gas from its own carbon and the surrounding oxygen. This combination, as I have observed several times, does not change the volume of the atmosphere. It is noteworty that leaves thus saturated consume, in a given period, by the formation of carbon dioxide, around half as much oxygen as they consumed by inspiration.

Most leaves, particularly nonsucculent ones, form free carbon dioxide at the same time as they inspire oxygen. They generally make lesser inspirations, but never larger than that of the cactus.

3.2.3 A Cactus that has Inspired Oxygen Gas Cannot Expire it Through the Effect of the Vacuum of an Air Pump

The oxygen gas inspired by a cactus or any other leaf is retained in the leaf by an **[p. 68]** attraction so powerful that suppression of the weight of the atmosphere does not release the gas appreciably.[2] Six cubic inches of cactus that, in one night, had inspired 4 cubic inches of oxygen were able to expire, in darkness, under a small amount of water in a vacuum, only 1 cubic inch of air, containing 15/100 oxygen gas and 85/100 nitrogen gas, and no or 1/100 carbon dioxide gas. The preceding day, before the inspiration, this plant had yielded the same amount of air by this means, but the air had been composed of 19/100 oxygen gas and 81/100 nitrogen gas.

The small difference in purity seen in the airs extracted before and after inspiration supports the idea that the air contained in the leaves is always related to the purity of the surrounding atmosphere. As the air in the vessel where the plant had stayed in the dark was corrupted by inspiration, so it was in the plant. It is very likely that **[p. 69]** the air pump obtains little except the free air present in the plants. I have always found it, at all hours and exposures, to be less pure than atmospheric air, in plants growing in the open air. When a cactus is completely saturated with oxygen gas, the effect of a vacuum will extract from it an air containing two or three hundredths carbon dioxide gas, which does not amount to a 50th of the oxygen that the plant has inspired.

A dark heat, moderate enough that it did not destroy the plant, was no more effective than a vacuum at releasing the inspired gas. Six cubic inches of cactus that had absorbed 4 cubic inches of oxygen gas at 15° Réaumur could not expire any gas in the dark, under receptacles filled with atmospheric air heated to either 30° or 35°. In both cases, they only combined their carbon with the surrounding oxygen, without changing the volume of their atmosphere. They turned yellow at 40° and the results then became meaningless. When I placed, in the shade, some oxygen-saturated leaves, in water heated to the preceding temperatures, they released an amount of air equal to a sixth of their volume. **[p. 70]** This air contained 89/100 nitrogen gas and 11/100 carbon dioxide gas.

3.2.4 Leaves do not Inspire Appreciably in Gaseous Environments that Lack Free Oxygen

A cactus (or any other leaf) cut at sunset and placed for one night in nitrogen gas, hydrogen gas, or pure carbon dioxide gas does not decrease the volume of its atmosphere. On the contrary, it expands the volume by adding carbon dioxide gas to it. This increase, which is less the more vigorous the plant, ordinarily amounts, in one night, to a third or a fourth of the volume of the cactus. There is no doubt, however,

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[2] Using this procedure, I removed from green fruits, such as pears and apples, even before they ripened, an air containing a very large amount of carbon dioxide, but leaves never yielded a significant amount, unless they had stayed in environments devoid of oxygen gas.

that these atmospheres penetrate in very small amounts into the interior of the plant, for when I subjected cactuses that had passed the night in pure hydrogen gas to the effects of the air pump, they yielded an amount of gaseous fluid equal to a sixth of their volume. This air was composed of 40 parts hydrogen gas, 40 parts nitrogen gas, and 20 parts carbon dioxide gas.

A cactus cut at sunset and **[p. 71]** placed for one night in hydrogen gas does not, as I just said, make any detectable inspiration. But when it is removed from this atmosphere and placed in darkness in a receptacle filled with ordinary air, it inspires its own volume of oxygen gas, or very nearly the same amount as it would have if it had not been in the hydrogen gas.

If carbon dioxide gas is substituted for hydrogen gas in this experiment, the plant, as soon as it is placed in darkness in a receptacle filled with ordinary air, begins to expand its atmosphere, emitting to it a small amount of carbon dioxide, equivalent to around a sixth of the plant's volume. The plant then inspires an amount of oxygen gas about equal to the preceding emission, after which it changes the volume of its atmosphere no further. It modifies the atmosphere only by combining its carbon with the surrounding oxygen gas. This plant can remain 2 or 3 days in pure carbon dioxide gas without perishing.

3.2.5 A Cactus Inspires Carbon Dioxide Gas in the Same Proportion as Oxygen Gas, When the Carbon Dioxide is Mixed with the Oxygen in a Small Amount [p. 72]

The experiments that I reported in Sect. 3.2.1, on the inspiration of oxygen gas, were done in an air that had previously been freed of carbon dioxide gas, and the question arises whether, in natural, atmospheric air, leaves absorb carbon dioxide gas in preference to oxygen. Consequently, I placed a cactus under a receptacle filled with ordinary air to which I had added carbon dioxide gas, and I found that the carbon dioxide and oxygen gases were absorbed in the same proportion, but that the inspiration was more rapid than it would have been if there had been no carbon dioxide gas. Here are the details of one of these experiments: 153.6 cubic centimeters (7-3/4 cubic inches) of cactus were placed after noon in 43-1/2 cubic inches of air confined by mercury. This air, which I set equal to 100 parts, contained 74 parts nitrogen gas, 19 parts oxygen gas, and 7 parts carbon dioxide gas. After 12 hours in darkness, the volume of air was reduced to 81 parts. The leaves had thus made an inspiration **[p. 73]** equal to around 1-1/4 times their volume [*actually, closer to their exact volume; 1-1/4 times would be 9.69 cubic inches, whereas this inspiration is 8.265 cubic inches*]. I found by analyzing the remaining air that the cactus had inspired 13-1/2 parts oxygen gas and 5-1/2 parts carbon dioxide gas. They had thus inspired the two gases in the same proportion, or 3/4 of each one. After this inspiration they were saturated. They could not continue to reduce their atmosphere through a prolonged darkness.

3.2.6 A Cactus Never Appears Saturated with Oxygen Gas After Staying for Several Days in Darkness, or an Indefinite Time in the Open Atmosphere

I have indicated in Sects. 3.2.1 and 3.2.4 that a cactus placed in darkness in atmospheric air, under a receptacle closed by mercury, absorbs in the period of 30 or 40 hours 1-1/4 times its volume of oxygen gas, [*in Sect. 3.2.4, he does not give a figure of 1-1/4; he says "its own volume"*] and that after that time it does not reduce the volume of its atmosphere further, however long the experiment. Based on this observation, we should expect that a cactus cut at the same time as the previous one and exposed to darkness for several days in the open air or outside a receptacle would not diminish its atmosphere when subsequently placed, without water in darkness, under a receptacle filled with atmospheric air. But **[p. 74]** in these circumstances the plant makes an inspiration equal to a quarter of its volume in 24 hours. It is then saturated and inspires no more. If the cactus is taken out of confinement and exposed to darkness in the open air, and then returned to darkness under the receptacle, it makes an inspiration equal to the previous one. By repeating the same trials several times, I was able to make the same leaf absorb in a fortnight, in continuous darkness, several times its volume, or an indefinite amount, of oxygen gas. In the following [*i.e., in Sect. 3.2.8*], I will give an explanation of this phenomenon, which is common to all plants or at least to all their succulent parts.

3.2.7 A Cactus Whose Structure has been Destroyed Makes no Appreciable Inspiration in Atmospheric Air

Six cubic inches of cactus collected at sunset were cut into pieces and quickly reduced to a paste by pulverization. Immediately afterwards, they were placed, for one night, under a receptacle filled with atmospheric air closed by mercury. They did not change the volume of their atmosphere. They did not absorb oxygen gas. But they **[p. 75]** combined their carbon with this gas, forming carbon dioxide gas, and the juice of the plant coagulated. The amount of oxygen gas consumed in the formation of carbon dioxide gas in this experiment was much less[3] than the amount that would have disappeared through inspiration, for a healthy cactus would have consumed, under conditions otherwise the same, at least 3 or 4 [*cubic*] inches of oxygen, whereas the cactus whose structure had been destroyed removed only a quarter of this amount in the formation of carbon dioxide gas.

A cactus reduced to paste and placed immediately afterwards either in pure nitrogen gas or hydrogen gas responds as an intact cactus does. It expands these gases by emitting carbon dioxide gas. At later stages, it adds hydrogen and nitrogen gases.

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[3] This assertion is true only in the period immediately after the death of a plant. When it putrefies, it consumes much more oxygen gas than through inspiration.

3.2.8 Inferences from the Preceding Observations: The Oxygen Gas Inspired by Leaves in the Dark is Converted into Carbon Dioxide Gas [p. 76]

It is very likely that the oxygen gas absorbed by cactus and by leaves in general is retained in their parenchyma in the form of carbon dioxide gas. Although this view has not been demonstrated by direct experiments, we must accept it because it appears to be the only explanation for the principal facts that we have just observed.

The ability of leaves to expire and inspire oxygen gas appears to depend on their ability to decompose carbon dioxide gas. The green parts of plants, which are in general the only parts that carry out this decomposition, are also, as will be seen in the following, the only ones that make successive inspirations and expirations through the effects of sunlight and darkness. These two functions appear to be alternately cause and effect, one of the other, as the fall of an oscillating pendulum seems to be the cause of its ascent and this ascent the cause of a new fall.

A leaf that is placed in darkness **[p. 77]** immediately after being exposed to sunlight contains no carbon dioxide gas because that gas was decomposed in the light. Air freely penetrates the leaf, as we saw hydrogen gas do (Sect. 3.2.4). Atmospheric oxygen gas is seized in passing by the carbon of the plant and forms carbon dioxide gas, which loses its gaseous [*"elastic"*] state and its volume through its union with the water of vegetation. Due to plant structure, the carbon dioxide undergoes a compression, which is limited since the plants that absorb the most gas cannot acquire more than 1-1/4 times their volume of it. The effect of the oxygen gas thus only apparently changes before and after saturation of the leaf. Before saturation (Sect. 3.2.1), the gas appears to be absorbed, as long as the leaf can condense or absorb the carbon dioxide gas produced by this oxygen gas; and after saturation (Sect. 3.2.2), the oxygen gas forms free carbon dioxide gas because the leaf, being saturated, can accept no more carbon dioxide gas.

But why are leaves never saturated with carbon dioxide gas when they have been in darkness for an unlimited time in the open air (Sect. 3.2.6)? Why does alternating exposure under a receptacle and in open air cause them to absorb **[p. 78]** many times their volume, or an indefinite amount, of carbon dioxide gas in continuous darkness? I respond that the latter phenomenon is only an illusion, produced by the attraction that the open atmospheric air exerts on the carbon dioxide contained in the leaves. Atmospheric air has a real chemical affinity for carbon dioxide gas [*this is a misinterpretation*], and, like most combinations, has limits of saturation. It may not be superfluous to recall here the evidence.

When pure water is placed under a receptacle filled with carbon dioxide gas, this water becomes saturated, and it takes up neither more nor less than its own volume of it. This water, saturated and confined in flasks with different amounts of atmospheric air, sheds its gas in proportion to the amount of air contained in each flask, and the air expands in proportion to the amount of carbon dioxide gas that it removes from the water. If it is a question of destroying this gaseous combination, the substances used will remove more or less carbon dioxide gas, according to their degree of affinity for it. Limewater will remove more than distilled water, and potash more than limewater.

These effects and their causes have been indicated by Berthollet in his fine **[p. 79]** memoir on affinities. (*Mémoire de l'Institut National, sciences et arts*, Vol. 3).

Pure atmospheric air, not being in equilibrium of saturation with a leaf that is saturated, that is, contains 1-1/4 times its volume of carbon dioxide, removes only a small amount of the leaf's carbon dioxide. [*This is confusing; the next sentence suggests that the pure air should remove more than just a small amount of carbon dioxide from the leaf.*] Therefore we see that, as soon as a cactus that is saturated with oxygen or carbon dioxide gas is put under a new receptacle filled with pure atmospheric air, the atmospheric air undergoes an immediate increase in volume (Sect. 3.2.4), due to an emission of the carbon dioxide gas that saturates the cactus. After this emission, the plant forms, with the surrounding oxygen gas, new carbon dioxide gas that it can inspire, because the saturated atmospheric air poses no more obstacle to saturation of the leaf. When the leaf is removed from its confinement, the open atmospheric air removes from the leaf the part of the carbon dioxide that the leaf just acquired and puts the leaf in a condition to absorb anew, under a receptacle, that which it has lost to the open air. The leaf thus does not accumulate carbon dioxide or oxygen gas in this experiment.[4]

[p. 80] We cannot compare the phenomena produced in darkness by a plant that enjoys all its growth capabilities in atmospheric air with those that it shows in oxygen-free environments (Sect. 3.2.4).

In the latter case, plant growth remains suspended. All the effects that the plant could produce as an organized body are interrupted. The plant begins to putrefy, to decompose, producing from its own substance the two elements of carbon dioxide. The water of vegetation, which is less than the weight of the plant, then imbibes this gas only in very small amounts, because it is not retained by the pressure that the growth force can exert on it. We have no idea of the limits of this compression. The experiments **[p. 81]** of Hales show that the compression may exceed the weight of our atmosphere. It is for this reason that the air pump was unable to extract the carbon dioxide that the cactus had absorbed (Sect. 3.2.3). The disorganized cactus did not inspire carbon dioxide gas or oxygen gas appreciably because this compression no longer existed. The very small amount of carbon dioxide required to saturate the water of vegetation under these conditions had been formed and absorbed in the act of pulverization.

It generally follows from these considerations that leaves in darkness do not assimilate the atmospheric oxygen gas that they have absorbed, at least insofar as they do not decompose the carbon dioxide that results from this absorption. [*He is presumably saying that if they decomposed carbon dioxide, they would assimilate a small*

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[4] The affinity that I have assumed between atmospheric air and carbon dioxide is contested by Dalton. But let us confine ourselves to experiment and note that when 1 cubic inch of water is introduced into a mixture composed of 90 cubic inches of atmospheric air and 10 cubic inches of carbon dioxide, or any other amount, and this water is withdrawn, exposed to the open air in darkness, and put back into the gaseous mixture, and this maneuver is repeated several times with the same water, the atmospheric air is almost entirely depleted of the 10 cubic inches that had been added to it. Thus the water produced precisely the same effect here as the cactus: It appeared to have absorbed ten times its volume of carbon dioxide, although it was clear that the water never accumulated one-tenth of this amount.

amount of the resulting oxygen.] If this decomposition occurs in these circumstances, it is only in a proportion that escapes any direct experiment. These conclusions would doubtless be risky if they were based only on the results that cactus provided me, but I confirmed them on more than 60 other plants of very different kinds. Several of them consumed more oxygen gas in the same time, leaving more free carbon dioxide gas in their atmosphere, but, in **[p. 82]** darkness that was prolonged indefinitely, none was able to diminish the volume of this atmosphere by an amount greater than the volume of the plant, and the oxygen gas that disappeared was always, with the exception of this small difference, directly proportional to that which entered into the composition of the carbon dioxide gas produced.

3.3 Experiments on Expiration [this section title supplied by translator]

3.3.1 Expiration by Cactus in Atmospheric Air

The 119 cubic centimeters (6 cubic inches) of cactus that were used in the experiment in Sect. 3.2.1, and that had corrupted and diminished their atmosphere during a single night by inspiring 79 cubic centimeters (4 cubic inches) of oxygen gas, were placed without water in another receptacle and exposed to the morning sun, without being in contact with the walls of the receptacle or with the liquid that closed it. This air contained 951 cubic centimeters (48 cubic inches) of atmospheric air freed of carbon dioxide gas. In the evening, this atmosphere had increased by 87 cubic centimeters (4.4 cubic inches). It contained no carbon dioxide gas. The eudiometer indicated 27-1/4 hundredths oxygen gas there, whereas before the introduction of the plants, **[p. 83]** it had indicated only 21 hundredths. [*Even though the cactus is a CAM plant, it is releasing gas during the day.*] It follows from these observations that the air of the receptacle contained:

Before inspiration	200 cubic centimeters (10.1 cubic inches) of oxygen gas +751 cubic centimeters (37.9 cubic inches) of nitrogen gas
After expiration	283 cubic centimeters (14.28 cubic inches) of oxygen gas +755 cubic centimeters (38.1 cubic inches) of nitrogen gas
Difference, or expiration	83 cubic centimeters (4.18 cubic inches) of oxygen gas +4 cubic centimeters (0.2 cubic inch) of nitrogen gas

The following night, I placed these same leaves in a new atmosphere of ordinary air. There they inspired 74 cubic centimeters (3-3/4 cubic inches) of oxygen gas, and the next day, in the sun, they expired 79 cubic centimeters (4 cubic inches) of oxygen gas and 6 cubic centimeters (1/3 cubic inch) of nitrogen gas. When these trials were continued for 7 days, the inspirations and expirations of oxygen gas steadily decreased and the expirations of nitrogen gas steadily increased. I found, by adding all the inspirations on the one hand and all the expirations on the other, that the same cactus inspired in 7 nights 331 cubic centimeters (21-3/4 cubic inches) [*conversion not right; 21-3/4 cubic inches = 431 cubic centimeters; 331 cubic centimeters = 16.7* *cubic inches; the correct figure seems to be the 21-3/4 cubic inches*] of oxygen gas, and expired, in the sun, during the 7 intervening days, 584 cubic centimeters (29-1/2 cubic inches) of air **[p. 84]** composed of 23-1/4 cubic inches of oxygen gas and 6-1/4 cubic inches of nitrogen gas. The final expiration was more than half nitrogen, whereas the first yielded only 1/16 of its volume of this gas.

In this experiment, the emissions of pure oxygen gas exceeded the inspirations [*he wrote "expirations" here but presumably meant "inspirations"*] a little, but the difference is too small, and too insignificant compared with the volume of the plant, to allow us to conclude anything about the decomposition of water.[5]

3.3.2 Expiration by Cactus Under Distilled Water and in Nitrogen Gas

I varied the experiments on expiration by submerging, during the day, under a receptacle filled with distilled water, a cactus that had passed the night without water in a receptacle filled with ordinary air depleted of carbon dioxide. The general results were the same **[p. 85]** as in the previous experiment. In this case, though, the plant, since it was under water that was cooler than the air and, moreover, in an uncongenial medium, carried out all its functions more slowly. It did not have time to decompose, in one sunny day, all the carbon dioxide that it had formed during the night. Moreover, its expirations were corrupted by a considerable amount of nitrogen gas, whereas the earlier expirations [*i.e., those in the preceding experiment*] in ordinary air were of almost pure oxygen gas and were never, in this regard, less than the inspirations. Six cubic inches of cactus absorbed, in 7 nights, 17 cubic inches of oxygen gas, and expired into [*original text erroneously says "inspired" into*] the water, in the form of bubbles, 19 cubic inches of air that, according to an analysis with potassium sulfide [*"hydrosulfide of potash"*], was composed of 13.3 cubic inches of oxygen gas and 5.7 cubic inches of nitrogen gas. The first expiration contained 80/100 oxygen gas and 20/100 nitrogen gas. The seventh was composed of 39/100 oxygen gas and 61/100 nitrogen gas. We cannot help but see that the nitrogen arises from the decomposition of the plant. The plant produces it in a significant amount only when it is suffering damage, either from the **[p. 86]** length of the experiment or from an uncongenial medium.

A cactus that passes the night in atmospheric air and the day in nitrogen gas in the sun makes larger expirations than it does under water, but less pure than in ordinary air.

3.3.3 Expiration is Proportional to Inspiration

First Proof When cactus plants have inspired all the oxygen gas they can absorb by being held for 36 or 40 consecutive hours in darkness under a receptacle filled

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[5] I will report other results on this subject in Chap. 7. The experiments will be much lengthier. The plants will be nourished, in atmospheric air, by a small amount of water. They will not be exhausted by the lack of this nutrient or plagued with repeated transfers.

with atmospheric air, and they are then exposed to the sun, they make a much larger expiration in 7 or 8 hours than when they have spent only a single night in ordinary air. I performed this experiment several times, and in a manner that leaves no doubt. I will give an example here: 6 cubic inches of cactus, having inspired 4 cubic inches of oxygen gas during 12 hours in darkness, expired [*original text erroneously says "inspired"*] immediately afterwards, in the sun, in 7 hours, 4.2 cubic inches of the same gas. I then placed the same plant under a receptacle in the dark **[p. 87]** for 36 consecutive hours. It absorbed 7-1/2 cubic inches of oxygen gas. Next it was exposed to the sun, and it exhaled, in 7 hours, the 7-1/2 cubic inches that it had inspired in the previous 36 hours. The expiration is thus augmented by the amount of the inspiration. I believe this result important because it shows that the emission of oxygen gas, which is an effect of the inspiration, should not be confused with an emission that might be attributable to the decomposition of water.

Second Proof I exposed to the sun, in atmospheric air freed of carbon dioxide gas, some cactus plants that had been cut the previous evening and had passed the night in either pure nitrogen gas or pure hydrogen gas. They exhaled oxygen gas into the atmospheric air, but about half as much as they would have produced if they had passed the night in a receptacle filled with ordinary air. The following night, I placed these plants again in nitrogen gas, and the next day in atmospheric air. They produced half as much oxygen gas as the day before. The third day they produced still less. The fifth day they were dead and **[p. 88]** putrefying. The total oxygen gas that they were able to yield in the sun in this entire experiment was less than their volume. The result was the same when the plant was nourished with a small amount of water and, after passing the night in nitrogen gas, was exposed under water to the sun. Cactus thus can produce only a very limited amount of oxygen gas, less than its own volume, when it has passed the night in a medium lacking oxygen gas and the day in atmospheric air.

I obtained very different results when I performed the converse experiment, namely, placing the cactus during the night in a receptacle filled with atmospheric air and during the day in a different receptacle, filled with nitrogen gas. I could then continue the experiment for a fortnight or more without the cactus wilting. The amount of oxygen gas that the plant produced exceeded five or six times the volume of the plant. The amount was, as it were, unlimited. These observations confirm the heading of this section. Moreover, they prove that it is only during the night that leaves especially require contact with oxygen gas. [*In CAM plants, the stomates are generally open only at night.*] The following inference can be drawn from this: When plants must **[p. 89]** stay in a place that is dimly lit and in which the atmosphere is always more or less corrupted, as in some greenhouses, they should be arranged so that, the darker the place they occupy, the more frequently the air they receive is renewed.

3.3.4 Effect of Quicklime or Potash on Expiration by Cactus

Quicklime or potash, which will kill thin leaves growing in the sun in a few days if it is placed in their atmosphere, does not have this effect on cactus or succulents in general because, with their very thick parenchyma and less-porous epidermis, they more tenaciously retain the carbon dioxide gas that they form. If limewater is put under a receptacle full of atmospheric air in which a cactus passes a single night, no calcium carbonate [*"carbonate of lime"*] is formed. But it is different if limewater is placed in contact with the air in which a cactus is staying in the sun. The limewater becomes covered with carbonate. [*But stomates of CAM plants are usually closed in the sun.*] In this case, and especially when dry quicklime slaked with water **[p. 90]** or a solution of potash is placed under the receptacle, the expiration of oxygen gas is noticeably reduced. It is then never greater than the inspiration.

3.3.5 The Results Obtained on Expiration by Cactus are Applicable to the Leaves of Other Plants

Experiments on expiration can give results that are dramatic and can withstand scrutiny only with green plants that contain a large amount of green herbaceous matter in a small volume and that have a strong enough growth force that they can displace, in the sun, a tenth or a twentieth of the capacity of the receptacle without crumpling and without touching the vessel walls, which are then warm enough to destroy a plant that presses on them. Succulents are almost the only plants that are in this category. Thin leaves are so delicate, so expanded, that they can grow in the sun under a receptacle only if the plant bearing them displaces a 400th or 500th of the receptacle's capacity. They can be shown to be subject to the same laws, however, by comparing the following two experiments: (1) If a thin-leaved plant, **[p. 91]** such as a mint, an epilobe, or a *Lythrum salicaria*, is grown for a fortnight, in the alternating effects of sunlight and night, under a receptacle full of ordinary air, this atmosphere will be found to have changed neither in purity nor in volume after the allotted time. These plants occupy too little space for the changes that they should produce there to be appreciable. A cactus of the same volume and under equivalent circumstances would not have produced more marked effects. (2) If the growth of thin leaves is maintained in two receptacles, one designated for the stay of the plant in the sun and the other for its stay in darkness, we find, after the successive transfers, that at the end of a fortnight the air of the receptacle kept in darkness decreased in purity and volume and that of the one in the sun improved.[6] The opposing effects are too small **[p. 92]** for their respective amounts to be very accurately compared, but the general results cannot be doubted, and we can understand based on the first experiment that the improvement obtained in the second is due to the oxygen gas transported by the plant from the atmosphere in darkness into the atmosphere in sunlight.

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[6] This trial was performed by Ingen-Housz with cress plants (*Lepidium sativum*). *Experiences sur les Vegétaux*, Vol. 2, p. 144. But this author did not investigate the cause of the result he obtained. He states that the improvement is greater than the deterioration. I repeated this experiment on the same plant. The deterioration of the air appeared to me to be greater than its improvement. My eudiometric methods were more precise than his.

Thin leaves always leave significant amounts of free carbon dioxide gas in their atmosphere in darkness. Due to their large surface area, part of the gas that they contain is removed from them by the surrounding air more abundantly than in fleshy plants.

3.4 Plant Growth in Pure Oxygen Gas

Although all plants require oxygen gas in order to develop, they fare less well in the shade when their atmosphere is composed only of oxygen than when the oxygen is mixed with a certain amount of nitrogen or hydrogen gas. The latter gases do not, by themselves, seem to have any noticeable effect on plant growth. Most plants do not assimilate them if the gases are isolated or in a **[p. 93]** gaseous state. But the gases can be useful to a plant by reducing its points of contact with oxygen gas.

In most of the experiments that I performed on partly developed pea plants (*Pisum sativum*), the plants, during 10 days *in the shade*, with the aid of water and of nourishment provided by their cotyledons, gained half as much weight in pure oxygen gas as in ordinary air confined by receptacles. In pure oxygen gas, the plants formed much more carbon dioxide gas, which, in itself, is always harmful to plant growth in the shade. Moreover, the excess oxygen gas could be harmful to them by removing too much of their carbon.

When the pea plants were exposed, in pure oxygen gas, *to the direct effects of solar rays,* their weight gain was almost the same as in ordinary air, but their stems seemed to be longer and thinner in the ordinary air. The shortened form of the plants in the oxygen gas cannot be attributed with certainty to the purity of this gas, however, because it seemed to me that this effect occurred in an artificial atmosphere composed in the same proportions as ordinary air, **[p. 94]** with the oxygen gas drawn from manganese [*i.e., manganese dioxide*], and the nitrogen gas from beef muscle by nitric acid. The eudiometer revealed the same composition as for our atmosphere.[7]

3.5 Relative Amounts of Oxygen Gas Consumed in Darkness by Different Leaves

I tried to determine whether the amount of oxygen gas that different leaves of the same volume consume, or remove, in darkness, either by inspiration or by the formation of free carbon dioxide gas, is related to other circumstances of their growth.

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[7] Does our atmosphere contain substances that elude the eudiometer and that favor plant growth? Or do artificial gases retain some ingredient of the substances from which they were extracted and that inhibits this elongation? I cannot decide this. It is certain that well-washed nitrogen gas has an animal odor if it was extracted from beef muscle, and a sulfurous odor if it was obtained from ordinary air by a sulfide.

The leaves of succulent plants **[p. 95]** consume less oxygen gas than do most other leaves. They retain it more tenaciously, or, in other words, they leave less free carbon dioxide gas in their atmosphere. These effects occur because these leaves offer the surrounding air many fewer points of contact, and their surface is, other things being equal, pitted with many fewer pores, as microscopic examination reveals. Because succulent plants growing under the influence of daylight and night-time can retain almost all the carbon dioxide they form in darkness, they may lose only a very small amount of their carbon and consequently can endure the lack of this nutrient for a longer time, or require less of it through the roots for their growth. [*Here is another example of de Saussure's view that plants do not obtain all of their carbon from the atmosphere.*] These plants grow naturally in sand, clay, or barren soil. Because they consume very little oxygen, they can grow in a rarefied atmosphere. The large number of sedum, saxifrages, and sempervivum growing in the mountains provide examples. [*Not a correct interpretation*]

The leaves of deciduous trees generally (though there are numerous exceptions) **[p. 96]** lose the most carbon, or consume the most oxygen gas. On high mountains, we see that the dominion of trees ends long before that of herbs.

The leaves of evergreen trees consume less oxygen gas than do the leaves of deciduous trees. Evergreens such as pines, juniper, and rhododendron can grow in a sterile soil and in a rarefied atmosphere.

Marsh plants grow in an environment in which they are deprived of free access to oxygen gas by the surrounding vapors. Experiment shows that, under equivalent conditions, they consume less oxygen gas than do most other plants having herbaceous stems. From this, we can understand why herbaceous plants of the mountains are often found in marshes of the plains.

We see that, in general, the amount of oxygen gas that leaves remove is related to the site. Leaves growing in an unproductive soil, or in a rarefied atmosphere, or in low, humid places consume, under equivalent conditions, less oxygen gas **[p. 97]** than do leaves that grow only in a fertile soil, with ample access to atmospheric air.

I am attaching here the table of the observations from which I deduced these results. The experiments were done in elongated receptacles closed by mercury that was lightly wetted on its surface. The receptacles contained 991 cubic centimeters (50 cubic inches) of atmospheric air and 20 cubic centimeters (1 cubic inch) of leaves. The nonfleshy leaves filled, at this volume, almost the entire capacity of the receptacle. I was obliged to keep them in the experiment for 24 hours in the dark because, in a shorter time, several of them would not have produced, at the volume that they occupied, effects appreciable enough to be measured exactly. I should note that they did not wither or lose any of their general appearance or freshness in these experiments, which were all done at a temperature of 15° to 16° Réaumur.

The numbers in the table are reported with the volume of the leaf taken as unity. Thus, when I represent by the number 5.5 the amount of oxygen gas consumed by oak leaves, I mean that, in 24 hours, the leaves **[p. 98]** caused 5-1/2 times their volume of oxygen gas to disappear, partly by the formation of free carbon dioxide gas, partly by inspiration.

In none of these experiments did the inspirations appreciably exceed the volume of the leaves, and most often they were less.

Relative Amounts of Oxygen Gas Consumed by Different Leaves (Tables 3.1–3.5) [p. 99]

Table 3.1 Leaves of evergreen trees and shrubs

Names of leaves	Time of experiment	Oxygen gas consumed during 24 hours in darkness; reported with the volume of the leaf taken as unity
Holly (*Ilex aquifolium*)	September	0.86
Box (*Buxus sempervirens*)	September	1.46
Cherry-laurel (*Prunus laurocerasus*)	May, new leaves	3.2
	September	1.36
Laurustinus (*Viburnum tinus*)	September	2.23
Ivy (*Hedera helix*)	September	1
Periwinkle (*Vinca minor*)	June	1.5
	September	0.93
Spruce (*Pinus abies*)	September	3
Hare's ear (*Bupleurum fruticosum*)	May	4
Savin juniper (*Juniperus sabina*)	June	2.6
Common juniper (*Juniperus communis*)	June	2.4

Table 3.2 Leaves of deciduous trees and shrubs **[p. 100]**

Names of leaves	Time of experiment	Oxygen gas consumed during 24 hours in darkness; reported with the volume of the leaf taken as unity
Beech (*Fagus sylvatica*)	August	8
Hornbeam (*Carpinus betulus*)	May	5
	September	6
Oak (*Quercus robur*)	May	5.5
	September	5.5
Horse chestnut (*Aesculus hypocastanum*)	September	4.8
Poplar (*Populus alba*)	May	6.2
	September	4.36
Apricot (*Prunus armeniaca*)	September	8
Peach (*Amydalus persica*)	June	6.6
	September	4.2
Walnut (*Juglans regia*)	May	6.6
	September	4.4
Plane tree (*Platanus occidentalis*)	September	3
Acacia (*Robinia pseudo acacia*)	May	5
	September	6.7
Lilac (*Syringa vulgaris*)	May	3.36
	September	2.2
Ash (*Fraxinus excelsior*)	May	4.32
	September	3.71
Pear (*Pyrus*)	May	5.2
	September	3.4
Rose (*Rosa centifolia*)	June	5.4
Chestnut (*Fagus castanea*)	July	5.6

Table 3.3 Leaves of nonaquatic, herbaceous plants **[p. 101]**

Names of leaves	Time of experiment	Oxygen gas consumed during 24 hours in darkness; reported with the volume of the leaf taken as unity
Potato (*Solanum tuberosum*)	September, before flowering	2.5
Cabbage (*Brassica oleracea*)	September, young leaves	2.4
	September, old leaves	2
Nettle (*Urtica urens*)	September	2
Mercury (*Mercurialis annua*)	September, during flowering	2.33
Carrot (*Daucus carotta*)	September, during flowering	1.9
Fava bean (*Vicia faba*)	Before flowering	3.7
	During flowering	2
	After flowering	1.6
Lily (*Lilium candidum*)	May, before flowering	0.66
	September, after flowering	0.5
Nasturtium (*Tropaeolum majus*)	September, during flowering	3
Foxglove (*Digitalis ambigua*)	July	2
Turnip (*Brassica rapa*)	September, during flowering	1.25
Oats (*Avena sativa*)	June, before flowering	2.7
Wheat (*Triticum aestivum*)	May, before flowering	5
Pea (*Pisum sativum*)	May, during flowering	3.72
Rue (*Ruta graveolens*)	August	2

Table 3.4 Leaves of marsh or aquatic plants **[p. 102]**

Names of leaves	Time of experiment	Oxygen gas consumed during 24 hours in darkness; reported with the volume of the leaf taken as unity
Water plantain (*Alisma plantago*)	August	0.7
Fleabane (*Inula dissenterica)*	September	1.6
Epilobe (*Epilobium molle*)	September, during flowering	1.9
Watercress (*Sisymbrium nasturtium*)	September	1.6
Polygonum persicaria	September, during flowering	2
Veronica (*Veronica beccabunga*)	September	1.7
Creeping buttercup (*Ranunculus reptans*)	September	1.5
Loosestrife (*Lythrum salicaria*)	May, before flowering	2.3
Caltha palustris	May	1
Carex acuta	May	2.25

Table 3.5 Leaves of succulent plants **[p. 103]**

Names of leaves	Time of experiment	Oxygen gas consumed during 24 hours in darkness; reported with the volume of the leaf taken as unity
Raquette (*Cactus opuntia*)	August	1
Agave americana	August	0.8
Sempervivum tectorum	July	1
Sedum globosum	September	1.5
Saxyfraga colyledon	September	0.6
Sedum reflexum	June	1.7
Stapelia variegata	July	0.63
Mesembryanthemum deltoides	July	1.7

3.6 Influence of Atmospheric Oxygen Gas on Plant Roots [p. 104]

Plant roots that are buried in humus are not entirely deprived of atmospheric oxygen gas. A certain amount of the gas can be withdrawn simply by boiling water that has filtered through plant mold. If we pour water into a hollow made in the soil surface and collect the air that the water displaces in penetrating the earth, we find that it is less pure than atmospheric air but that it still contains a considerable amount of oxygen gas. Hales already said it, but it seemed interesting to me to see whether contact of this gas with roots is useful to plant growth.

I uprooted some young, leafy, horse chestnut trees, each weighing about 23 grams. They had long roots of 2.5 decimeters, and stems of about the same length. I introduced the roots of chestnut FG (Fig. III [*see Plate, p. lix*]), into the neck E of a 1.2-liter receptacle, and I luted its stem precisely with the neck E. **[p. 105]** After I had filled this vessel with distilled water, I introduced, through its opening CD, 594 cubic centimeters of nitrogen gas, which was in contact with the entire upper part of the roots, whose tips were submerged in water HI. The receptacle rested on a bowl filled with mercury. I set up a similar apparatus with hydrogen gas and another with carbon dioxide gas, which I supplemented every day because it was absorbed by the water HI. Three other horse chestnuts were set up separately, in the same way, with atmospheric air.

The plant whose roots were in contact with carbon dioxide gas died first, after 7 or 8 days. The plants whose roots were in contact with nitrogen gas and hydrogen gas died nearly simultaneously, after 13–14 days. By adding carbon dioxide gas during the entire time they were growing, they had increased the volume of their atmosphere, which, after subtracting the carbon dioxide, had not decreased. The horse chestnuts whose roots were growing in ordinary air were still vigorous after 3 weeks, when I terminated the experiment. Their roots had reduced the volume **[p. 106]** of gaseous fluid in the receptacle, either by absorbing the carbon dioxide that they had formed, or because of the absorption of this same gas by the water HI. The nitrogen gas of this atmosphere did not decrease.

One may conclude from this that contact of oxygen gas with the roots is useful to vegetation. Several other, less direct, observations support this contention:

1. Duhamel noted (*Physique des arbres*, Part 1, Chap. 5) that the "lateral roots are stronger and more vigorous the closer they are to the soil surface: so that if in planting a tree in a uniform soil, several layers [*de Saussure transcribed this last term as* "plants*" ("plants" in English) but Duhamel's original says* "plans*," one of the meanings of which is "planes," which I have interpreted as "layers"*] of roots are retained, the layer that is closest to the soil surface will almost always be more vigorous than one which is deeper." Is this not because the deeper level has less contact with atmospheric oxygen gas than the more superficial one? One might believe that it is because the upper roots find more nutritive juices at the soil surface, but the following observation shows that the effect cannot be attributed to this cause alone: If common soil is piled on the roots and around a tree that is thriving in a good soil, **[p. 107]** this tree will be harmed. If it does not die, the lower roots, although planted in a better soil, will decay, and the tree will sprout new roots upon the old ones, in the newly added soil. It is not necessary to mention the benefits that accrue from opening or raising the surface of soil used by vegetation.
2. Tap-rooted plants—so-named from their structure, as they have stout vertical roots almost devoid of root hairs—fare better, other things being equal, in a dry soil than in a damp one, and also better in a light soil than in a compact one, probably because these roots, which have little surface relative to their mass, require more contact with oxygen gas than do fine roots, which may have this contact even in a heavy soil.
3. Tree roots that penetrate manure, mud, or water pipes divide indefinitely and form foxtails because they can grow only to the extent that they multiply their points of contact with the very small amount of oxygen gas that they find in these environments.
4. A plant whose roots are **[p. 108]** suddenly covered by stagnant water is harmed much more quickly than if it had met this fate in running water. The oxygen gas of standing water will soon be used up, but that of running water will not be exhausted. It should be noted that standing waters are more useful to plants than pure water if the plants receive the impure waters by sprinklings that are managed so as to provide contact with oxygen gas. From these observations, one may conclude that most plants will be harmed if the soil is too wet, not only because water as a nutrient is provided to them in too large a quantity, but also because their roots have almost no contact with the external air.

Some writers have believed that infusions of humus and manure do not themselves serve as plant nutriment, because plants that they uprooted and immersed by the roots in these liquors did not fare as well as they did in pure water. But I doubt very much that this observation is true as a generality. When infusions are not rich, or approach spring water in richness, plant growth is better and the plants gain **[p. 109]** more weight than in distilled water or in filtered rainwater. I repeated the very precise experiments of Woodward on this subject, and my results are consistent with his. Observations to the contrary were made with little regard for accuracy and with richer infusions than can be obtained by means of a press from a naturally moist, fertile plant mold. But observations should be made on more dilute infusions, because the interstices of the soil allow air to reach the roots, whereas in our total immersions, the oxygen gas that could have reached the roots through the medium of distilled water and of a less saturated infusion is entirely consumed by the extractive substances in a more concentrated infusion.

3.7 Of the Changes that Oxygen Gas Undergoes Through its Contact with Roots

If a healthy root, lacking its stem and having **[p. 110]** enough growth force that the root will not decompose by putrefaction, is placed under a receptacle filled with atmospheric air closed by mercury, the root decreases the volume of this air, apparently by absorbing oxygen gas. Moreover, it forms carbon dioxide gas with the surrounding oxygen gas. But whatever the capacity of the receptacle and the duration of the experiment, the amount of oxygen gas that seems to be absorbed by the root is always less than the volume of the root. If a root thus saturated is transferred immediately into another receptacle filled with ordinary air, it does not change the volume of this air, but simply forms carbon dioxide with the oxygen gas. But if, before the transfer, the root is exposed for some time to the open atmosphere, it absorbs the same amount of oxygen gas under the receptacle as it did in the first experiment.

A recently uprooted yellow carrot root consumed its own volume of oxygen gas in 24 hours, and absorbed a hundredth of this amount.

A potato tuber consumed, during the same period, 0.4 of its volume of oxygen gas and appeared to absorb 0.08 of its volume of this gas. A lily bulb having true roots consumed **[p. 111]** 0.39 of its volume of oxygen gas and absorbed 0.19 of its volume.

A turnip [“*rave*”], in the same amount of time, always consumed its own volume of oxygen gas. Absorption was a quarter of this amount.

The inability of a root to absorb more than its own volume of oxygen gas under a receptacle from which it is not removed, and the ability it acquires to absorb additional oxygen gas by exposure to the open air, shows that this organ does not permanently assimilate the oxygen gas that it absorbs, but only converts it into carbon dioxide gas, which atmospheric air, due to its affinity for carbon dioxide gas, can remove from the root.

Roots behave, both in sunlight and in shade, very nearly as leaves growing in darkness do, but the inspirations of roots are much less noticeable because, since they do not decompose carbon dioxide gas during the day, they are never entirely depleted of it.

In the experiments I have reported up to now, the roots were lacking stems, but the results are very different when the roots have their stems and the experiment is set up so that the root **[p. 112]** is confined in a receptacle filled with atmospheric air, while the stem and leaves project into the open air. The roots then appear to absorb several times their own volume of oxygen gas. I used the following apparatus for this purpose with *Polygonum amphibium* or *P. persicaria*. It is uncommon, I believe, to find plants that, through the length and suppleness of their stems and their growth force, lend themselves to this arrangement, for lutes must be avoided as far as possible. Lutes will not hold reliably enough on plants whose stems are always more or less subject to contraction and expansion. On the shelf of a mercury bath CD [*he is evidently referring here to Fig. IV of the Plate, p. lix*], I placed a narrow receptacle AB filled with atmospheric air. Onto the surface of the mercury HI contained in the receptacle, I passed a layer of water FG, 7 to 8 lines thick. Finally, I introduced the root EK of the *Polygonum* LM across the mercury into the receptacle. The layer of water FG, which nourished the plant, was soon drawn up by it. I then added water to the receptacle for several days in a row. From the ascent of the mercury in the bell jar, I saw that the air in this vessel gradually decreased. **[p. 113]** When all the added water had been absorbed and the air of the receptacle had decreased by nine or ten times the root volume, I ended the experiment, and I found that nitrogen gas had not been absorbed, that the oxygen gas alone had disappeared, in proportion to the decrease in the air volume.

The oxygen gas was not assimilated by the plant but was returned to the atmosphere by the leaves, for I saw that, when I placed a *Polygonum* having its leaves and roots under a receptacle filled with ordinary air, such that the roots were immersed only by their tips in the water contained under the receptacle and were mostly in contact with the atmospheric air that was confined there, the oxygen gas no longer decreased, because the leaves restored on the one hand that which the roots had absorbed on the other. In these experiments, the plant displaced only about a 300th of its atmosphere.

When one introduces, into a globe that is carefully closed and full of atmospheric air, the tip of a leafy branch occupying around a 400th or 500th part of the volume of the globe and is rooted in plant mold (Fig. VIII), the branch improves **[p. 114]** its atmosphere very noticeably after a fortnight or 3 weeks. But this change does not occur perceptibly when the same plant is completely enclosed, with its roots, in the globe.

To study accurately the influence that plants exert, with the help of pure water, on their surrounding atmosphere, it is necessary, as I indicated in Fig. I, that the roots not be immersed in the water BD that is used to close the receptacle, for, if this water (Fig. II) is considerable in amount, and if it is renewed often, the results obtained are analogous to those given by a branch (Fig. VIII) of a plant rooted in humus. The atmosphere provides to the water, and the water to the roots, an unlimited amount of oxygen gas, which is partly released by the leaves into the receptacle.

It follows in general from these observations that oxygen gas is not assimilated directly by the roots, but forms, with their substance, carbon dioxide gas that is absorbed by the roots and elaborated by the leaves.

3.8 Of the Changes that Oxygen Gas Undergoes Through Woody Stems [p. 115]

In the springtime, I placed some woody branches, just before bud opening, under a receptacle filled with ordinary air (see apparatus, Fig. V [*of the Plate, p. lix*]), with a tip of each immersed in a small amount of water to nourish them. The branches leafed out as in open air. But this development could not take place under a receptacle filled with nitrogen gas or hydrogen gas. They rotted in these gases, emitting nitrogen and carbon dioxide gases, without giving any sign of growth. Defoliated woody branches, arranged as I have just described, corrupt ordinary air, both in the sun and in the shade, without changing (other than by the volume of the stem) the volume of this atmosphere. They always replace the oxygen gas that they cause to disappear by an equal volume of carbon dioxide gas. Thus they do not assimilate oxygen gas. Their green parts undoubtedly assimilate a small amount in the sun, in decomposing **[p. 116]** the carbon dioxide gas that they form themselves with the surrounding oxygen gas, but this effect is not detectable by these experiments.

Branches of willow (*Salix alba*), oak (*Quercus robur*), poplar (*Populus nigra*), and hornbeam (*Carpinus betulus*), 7 millimeters (1–3 lines) in thickness, consumed, in the spring and summer, at 15° Réaumur, an amount of oxygen gas equal to at least half, and at most all, of their volume in 24 hours. Pear and apple, under the same conditions, consumed two or three times their volume.

Defoliated woody stems show the phenomenon of inspiration of oxygen gas in darkness, and, undoubtedly due to the green substance contained in their bark, that of expiration of this gas in the sunlight. This emission is not detectable in atmospheric air by eudiometric tests because the stems form, with the surrounding oxygen gas, more carbon dioxide gas in sunlight than in shade, and because their green parts do not have enough surface area to decompose all the carbon dioxide that they form in the sun. It is very clear, though, that the atmosphere of the branch **[p. 117]** decreases during the night and resumes its original volume in the light.

Woody stems do not replace the oxygen gas that they consume by an equal volume of carbon dioxide gas when the plant has its roots in soil, its leaves in the open air, and its stem alone confined in an atmosphere of ordinary air, as shown in the apparati in Figs. VI and VII, which are sealed by luting and mercury. The carbon dioxide gas that is left by the branch in its atmosphere is less, by an indefinite amount, than the oxygen gas that the branch consumed. The carbon dioxide that is missing here follows the course **c a d** of the branch, to be decomposed by its leaves in the open air. I will elaborate on the details of one of these experiments.

At the beginning of July, at 7 o'clock in the morning, I luted (Fig. VI) to the opening *a* of the tube *ab*, of which the opening *e* was immersed in mercury, the tip of an apple branch from which I had removed the leaves and the last, purely herbaceous, ramifications. This branch was 5 mm (2 lines) in diameter at its section *c* [*author mistakenly wrote* e *rather than* c *here*], and it occupied 2 cubic centimeters (0.1 cubic inch) in the apparatus. The tube itself contained 125 cubic centimeters (6.3 cubic inches) of atmospheric air **[p. 118]** in part *ab*, for I made the mercury climb to *b* in the tube so that the confined air would be at the same pressure as the external air.

Two hours after sunset, the mercury had risen, after corrections were made, by 3-1/2 millimeters (1-1/2 lines) inside the tube. This rise corresponded to about half the volume of the branch. The next day, at daybreak, the mercury had dropped a little, and 2 hours after sunrise, the air had regained the volume that it had had before the experiment. This air contained 3/100 carbon dioxide gas, and after deduction of that, 13/100 oxygen gas. The branch thus consumed, in 24 hours, 5-1/3 times its volume of oxygen gas.[8] The branch replaced it by barely three times its volume of free carbon dioxide gas, and produced **[p. 119]** a large amount of nitrogen gas, which prevented any volume change due to absorption of oxygen or carbon dioxide. I will return soon to this development of nitrogen gas, which is a necessary consequence of the experimental set-up. When I prolonged the experiment for 48 and 96 hours, the effects were doubled and tripled. There is therefore no doubt that the confined part of the stem absorbs unlimited quantities of the carbon dioxide gas that it produces. But in none of these trials, however prolonged, did the mercury climb more than 1-1/2 lines, and it always descended the next morning to the point where it had been the previous evening. I repeated these experiments, with the same results, on myrtle and *Lonicera xylostheum*, with the apparatus arranged as shown in Fig. VII.

I did the same experiments (Fig. VI) by closing with water [*rather than mercury*] a tube containing a column of atmospheric air 1.4 decimeters (5 inches) in height. After 3 or 4 days, the water had climbed 2.7 centimeters [*author erroneously wrote "decimeters"*] (1 inch) inside the tube. It did not climb higher when I prolonged the experiment. The air in the tube, examined at this time, contained only nitrogen gas (with the exception of about 2 or 3 hundredths **[p. 120]** carbon dioxide). Thus the air had undergone the same decrease in volume as it would have if subjected to the effects of a sulfide [*which would have absorbed oxygen*]. No nitrogen gas was produced. Hales performed this last experiment[9] and obtained the same results, but they prove nothing regarding our subject because the carbon dioxide gas that was produced, since it is absorbed by the water, cannot be compared with the oxygen gas that disappeared at the same time.

This famous natural philosopher had an opinion that must be contested: He believed that woody branches absorbed a large amount of air during the day and expired it during the night.[10] He cemented, as in the experiment in Fig. VI, the defoliated tip of an apple branch to the opening *a* of tube *ae*. But he filled this tube entirely with water and immersed its lower opening in mercury. The branch, by rapidly sucking in

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[8] A branch of the same dimensions as the previous one was cut or separated from this apple tree and placed for 24 hours under a receptacle closed by mercury, in the same exposure as the branch that was connected with the ground. It caused four times its volume of oxygen gas to disappear and replaced it with four times its volume of carbon dioxide gas. It produced no nitrogen gas. It follows from these two experiments that a branch connected with the ground consumes more oxygen gas than one that is separated from it.

[9] *Statique des végétaux*, French edition, p. 130.

[10] *Ibid.*, pp. 76 and 77.

this water, caused the mercury to climb several inches above [*de Saussure mistakenly wrote "below"*] its level. This ascent soon stopped because air bubbles were disengaged from the enclosed branch, **[p. 121]** preventing a further rise. This air came (and the author, moreover, saw it correctly) from atmospheric air introduced from *d* into *c* through the suction or vacuum that tended to make the mercury rise in the tube, which was imperfectly sealed by the porous wood of the branch. He noted that the mercury rose during the day and descended at night, and it was on this occasion that he expressed his opinion on the diurnal inspiration and nocturnal expiration of stems. But he wrongly attributed to vegetation an effect that was dependent only on the arrangement of his apparatus and on the greater and lesser water uptake during the day and at night.

During the day, when the tree transpired through its leaves, the volume of water drawn up by the branch exceeded the volume of atmospheric air that was introduced from *d* to *c* in the tube, and this air exited as it had entered, that is, through the pores of the wood. At night, when transpiration was less or nonexistent, so was the suction, and the air, penetrating the apparatus in greater proportion than the water was aspirated, caused the mercury to descend.

About the same thing happens when the tube is full of atmospheric air, but the effects are almost imperceptible **[p. 122]** to the eye because the branch draws in carbon dioxide gas much more slowly than it does water. No sooner has the branch, by drawing in some of this gas, made the mercury climb by 1-1/2 lines above its level, than the mercury draws into the apparatus atmospheric air, from which the branch consumes the oxygen gas and leaves only the nitrogen gas. We must attribute to this cause the production of nitrogen gas in the first experiment.

No nitrogen gas is produced in a column of air 1.4 decimeters (5 inches) in height, closed by water, because the rise of 2.7 centimeters [*author erroneously wrote "decimeters"*] (1 inch) of water is insufficient to draw atmospheric air into the apparatus. But when the column of water is higher, it produces the same effect as mercury.

I luted the tip of an apple branch to a tube *ae* (Fig. VI) completely full of mercury, and I dipped this tube deep enough in a mercury bath that the luted neck *a* was submerged in it. The branch emitted no air into the tube in 24 hours, either in the sun or in the shade. But when, by lowering the level of the external bath, I made the branch support a column of mercury 2.7 decimeters **[p. 123]** (10 inches) in height, the branch then produced, especially through its section *c*, countless air bubbles, which lowered the mercury about 1 decimeter (3 or 4 inches) in 24 hours. After 60 hours, the mercury had fallen to the level of the mercury in the outer vessel. The air that was produced contained 86/100 nitrogen gas, 9/100 oxygen gas, and 5/100 carbon dioxide gas, but in calculating the amount of oxygen gas that the branch consumed in the apparatus, it seemed to me that the air produced must have originally been about as pure as atmospheric air.

For 8 days, I exposed to the sun a young apple tree bearing its leaves, stem, and roots, under a receptacle full of atmospheric air. It did not change its atmosphere noticeably. The oxygen gas that the stems appeared to assimilate in some of the preceding experiments thus circulated [*as carbon dioxide*] within the plant, to be exhaled by the leaves.

I removed the bark from parts *ac* of the branches contained in the apparati in Figs. VI and VII. These branches also absorbed and decomposed through their leaves the carbon dioxide that they produced. When I performed this experiment under a receptacle, and on branches **[p. 124]** cut as in Fig. V, they left in this vessel an amount of carbon dioxide gas precisely equal to the amount of oxygen gas that they removed, and they produced, at the same volume and surface, the same effects one to two times faster than the nondebarked wood. The hardness acquired by debarked trees thus is not attributable, as several authors have believed, to the absorption of oxygen gas by the wood. It is due instead, as I will show below, to a superabundance of carbon.

In doing these experiments, I observed wood, especially that of oak, that at first was perfectly white turn dark yellow or brown within a few hours. This coloration was, as others have observed, more intense in the sun than in the shade. The conversion of oxygen gas into carbon dioxide gas by debarked wood was also faster in the light than in darkness.

When I placed fresh, recently debarked oak wood in nitrogen gas or under water, in the sunlight, it retained its whiteness fully, and then colored immediately in the sunlight under receptacles full of ordinary air. This coloration is thus an effect of contact with oxygen gas. It is based, **[p. 125]** as Bethollet thought (*Art de la teinture*, Vol. 1, p. 48), on a precipitation of carbon and formation of water. But this water formation does not arise from a combination of atmospheric oxygen gas with the hydrogen of the wood. This explanation would contradict experiment, which shows that the oxygen gas consumed is found in an exactly equal amount in the carbon dioxide gas produced. The precipitation of carbon is due to the fact that the wood, in losing a small amount of its carbon through its removal by oxygen gas, loses, at the same time, in much greater proportion, its oxygen and hydrogen in the form of water (see Chap. 4, Sect. 4.3). The loss of oxygen and hydrogen leaves carbon predominant in the woody residue.

Senebier made an observation similar to the one I just reported. He saw that the green color of leaves, when prepared by alcohol [*"spirit of wine"*], turns yellow in the sun only when in contact with oxygen gas. (*Physiologie végétale*, Vol. 3, p. 144.)

3.9 Of the Changes that Oxygen Gas Undergoes Through its Contact with Flowers

According to my experiments, flowers, even those of aquatic plants, do not **[p. 126]** develop in pure nitrogen gas. Their buds, ready to open, are as if paralyzed there, and, whether opened or not, putrefy more rapidly than under receptacles filled with ordinary air. Flowers such as white lilies or double roses that were able to displace, without deteriorating, a seventh or an eighth of their surrounding atmosphere during 24 hours under a receptacle filled with ordinary air, replaced, except for the volume of the flower, the oxygen gas that they removed, by carbon dioxide gas. I say, except for the volume of the flower, because the flower retained in its parenchyma an amount of carbon dioxide gas smaller than its volume. But when the flower was transferred immediately afterwards into another receptacle filled with ordinary air, it did not make this absorption. I will not return to the explanation for this observation.

It is rare for recently picked flowers confined in a vessel full of ordinary air to change the volume of this atmosphere noticeably, because they very nearly replace the carbon dioxide gas that they absorb [*presumably, oxygen gas that the flowers convert to carbon dioxide and retain*] by nitrogen gas. They differ in this respect from other plant parts, which, when growing in darkness, exhale much less nitrogen gas **[p. 127]** and consequently almost always reduce the volume of the surrounding air.

Might not the production of nitrogen gas in darkness by flowers that do not appear to be injured be related to the production of their odors? Even if we found that very fragrant flowers produced this gas as abundantly as less fragrant ones, we could conclude nothing against this supposition, because it is possible that not all flower scents affect our sense of smell. It is regrettable that flowers live so briefly that we can hardly prolong experiments beyond 36 hours, and that we must fear that the slightest disruption of such a delicate structure will influence the results.

In the experiments that I am going to report, the flowers seemed not to have deteriorated in the slightest. They lost nothing of their appearance or freshness. Those in the bud stage bloomed under the receptacle. When the green parts could not be removed, they were left on, but these parts were too small relative to the volume of the petals to have any noticeable effect in the experiments on rose, lily, wallflower, nasturtium, **[p. 128]** and chestnut catkins. The white lilies consumed [*i.e., took in*] their own volume of oxygen gas in 24 hours in the shade in ordinary air, under a receptacle closed by mercury, at a temperature of +17° Réaumur. They absorbed [*presumably, they converted to carbon dioxide and retained*] an amount of oxygen equivalent to 15/100 of their volume, and they replaced it by 15/100 of their volume of nitrogen gas. [*The rest of the oxygen that the flowers consumed presumably was converted to carbon dioxide and released as such.*]

Based on this account, we can understand the following data:

	Oxygen gas consumed in the shade in 24 hours	Absorption	Nitrogen gas produced
Flowers of lily (*Lilium album*)	1.1	0.15	0.15
Umbels of carrot (*Daucus carotta*)	2.1	0.3	0.3
Flowers of nasturtium (*Tropaeolum majus*)	2.17	0.5	0.5
Raceme of false dittany (*Dictamnus albus*)	2.6	0.7	0.7
Raceme of gillyflower, double (*Cheyranthus incanus*)	1.2	0.7	0.4
Cabbage rose (*Rosa centifolia*)	1.8	0.43	0.43
Male catkins of chestnut (*Fagus castanea*)	4.7	0.22	0.22

[p. 129] Petals consume more oxygen gas in the sun than in the shade. They give off no hydrogen gas. I tried in vain to spark an explosion in the pure oxygen gas in which, for 8 days, I had grown false dittony racemes, which I renewed [*replaced*] often. The inflammation that can be produced in this plant appears to arise strictly from the combustion of its essential oil.

I did the same experiments with the same results, either in the sun or in the shade, on nasturtiums and marigolds. It has been said that these flowers spontaneously produce flashes, but this phenomenon has been seen by only two observers, even though these flowers are widespread in gardens. It is all the more doubtful because their brightness could easily create an illusion.

3.10 Influence of Oxygen Gas on Fruits

The experiments that can be done with fruits, with regard to the questions that have occupied me up to now, are far less reliable than are those with other plant parts, because fruits retain almost no growth force **[p. 130]** when they are separated from the plant that produced them.

I introduced some unripe grapes, whose vine was rooted in soil, into a globe that was carefully closed and exposed to the sun. Before ripening, they improved their atmosphere of ordinary air, in a fortnight, without emitting a detectable amount of carbon dioxide, and they ripened. But when I put into the same globe some quicklime, slaked and dried quickly by the heat of boiling water, they corrupted their atmosphere under the same conditions and were unable to ripen. I obtained the same results with unripe berries of *Solanum pseudocapsicum*. Green fruits thus seem to yield the same results as leaves.

When I placed unripe grapes, *Solanum* berries, pears, and apples separated from the tree under receptacles full of ordinary air, these fruits corrupted their atmosphere in 24 hours, although they spent the day in the sun. Their inspirations and expirations were hardly noticeable. During the day they could not decompose all the carbon dioxide gas that they formed during the night. When they **[p. 131]** remained in continuous darkness for several weeks, they assimilated no oxygen gas. The oxygen that they removed reappeared in the same amount (except for the volume of the fruit) in the carbon dioxide produced.

3.11 Utility of Oxygen Gas in the Nutrition of Plants

To assemble here the principal effects of oxygen gas on plant nutrition, I will anticipate some functions that will be discussed in Chaps. 7 and 8. Humus contains extractive juices that penetrate plants and promote their growth. When these juices are exhausted, oxygen gas, by removing carbon from the humus, develops a new extract that replaces the first one. The effect of oxygen gas on the soil is not limited to this one influence. When the humus loses its carbon, it yields at the same time, in the form of water, its oxygen and hydrogen. Absorption of this liquid may be useful to plant growth under certain conditions.

Plants that develop with the aid of **[p. 132]** distilled water, under a receptacle full of atmospheric air, do not have humus for nutriment, but their own substance, at whose expense they grow, replaces the humus. The inner parts of the stem, or a part of the roots or lower leaves, wither and empty their extractive juices into the developing parts. All the influences of oxygen gas on humus relate to the case that we will now examine.

I have shown in Chap. 2 and in this one that green plants combine their carbon with the surrounding oxygen gas to form carbon dioxide gas, and that they wither if they are prevented from decomposing carbon dioxide, by the removal of the gas with lime or potash. The extractive juices thus probably assimilate their carbon into the green parts only insofar as it is converted into carbon dioxide gas. For this compound to be formed, oxygen gas is an absolute necessity to plant growth.

Oxygen gas itself serves as a nutrient to the green parts of plants. They do not assimilate it directly, but once they have converted it into carbon dioxide, they retain, in decomposing the carbon dioxide, a part of its oxygen. This effect is so small that it **[p. 133]** can be appreciated only when they are given a large amount of carbon dioxide gas to decompose. (See Chap. 2, Sect. 2.4, and Chap. 7, Sect. 7.3)

An atmosphere composed only of nitrogen and carbon dioxide gases is not favorable to plant growth (Chap. 2, Sect. 2.2). Free oxygen gas must be present. Oxygen gas thus has an effect beyond that of presenting to plants, in the form of carbon dioxide gas, elements that they can assimilate. One can presume that this second influence consists not only in developing a nutritive extract and water in the humus or plant, but also in producing a release of caloric [*a precursor idea to the concept of energy; see Glossary*] by the union of oxygen gas with the carbon of the plant. The release of caloric is a necessary result of this combination. If it usually escapes our notice, it is because of its small amount and the opposing effect of evaporation.

3.12 Summary

Green plants that are exposed in atmospheric air to the successive effects of day and night make alternating inspirations and expirations **[p. 134]** of oxygen gas mixed with carbon dioxide gas. The oxygen gas that green plants inspire is not directly assimilated by them. It is metamorphosed during inspiration into carbon dioxide gas. The plants decompose the carbon dioxide gas in the act of expiration, and it is only through this decomposition, which is only partial, that they are able to assimilate the oxygen gas that they find in the atmosphere.

The roots, wood, sapwood, petals, and all parts that in general are not green do not make successive inspirations and expirations. They do not assimilate atmospheric oxygen gas either directly or indirectly. They transform it into carbon dioxide gas, which is retained or dissolved in small amounts in their succulent parts, as it would be in pure water. Otherwise, they do not change it.

I showed how deceptive it can be to perform these experiments on a single plant part. The carbon dioxide gas that the roots, trunk, and woody branches form with the surrounding oxygen gas follows the longitudinal course of the plant to be decomposed by the leaves.

[p. 135] I have shown that contact of oxygen gas with the roots and the nongreen parts in general is essential to plant growth. I will return to this subject in Chap. 6.

The leaves of marsh plants, succulents, and evergreen trees generally consume, other things being equal, less oxygen gas than do the leaves of other plants.

The most noticeable influence that oxygen gas exerts on plants is to form carbon dioxide gas and to present to plants, in this form, elements that they can assimilate.

[p. 136] Chapter 4
Influence of Oxygen Gas on Some Plant-Derived Substances [*“proximate constituents of plants”*]

Several plant compounds that come in contact with oxygen gas during the early stages of fermentation show changes whose origin we are going to trace. This investigation should clarify for us the changes that these compounds undergo during plant growth, should it be true that we can sometimes compare the effects of a disorganized substance to the effects occurring in a plant enjoying all its growth capabilities.

4.1 Use of Oxygen Gas in the Precipitation of Extracts

Fourcroy[1] noted that, through exposure to air, extracts dissolved in water allow **[p. 137]** pellicles or insoluble material to precipitate in the liquid containing them. He attributed this precipitation to the combination of oxygen gas with the extract itself, but I do not know if the assertion of this learned author is based on an analysis of this precipitate or on eudiometric tests.

I placed some capsules containing extracts of cinchona, oak wood, and various humuses under jars filled with atmospheric air and closed by mercury. After a few days I found that a precipitate had formed in the solutions and that the air in the receptacles had undergone a small reduction in volume. Eudiometric examination of this atmosphere showed me that the oxygen gas that had disappeared had not completely reappeared in the free carbon dioxide gas that was produced at the same time. But an important fact struck me, namely, that the volume reduction of the oxygen gas in which the experiment was done was always less than the volume of the extractive solution, whatever its degree of liquidity or thickness. If 10 volume-parts of liquid extract are placed in contact with 1,000 volume-parts of atmospheric air, the air undergoes a decrease in volume that does not exceed 10 parts **[p. 138]**. This invariable result made me suspect that the oxygen gas does not combine with the extract, but that the oxygen removes carbon from the extract by forming carbon dioxide gas, of which the liquid can absorb only its own volume.

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[1] *Annales de Chimie*, Vol. 5.

J. F. Hill, *Chemical Research on Plant Growth*,
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When, after this experiment, I subjected these extractive juices, which were apparently saturated with oxygen gas, to the effect of a vacuum, I withdrew from them an amount of carbon dioxide gas nearly equal to the volume of oxygen gas that they had absorbed, whereas this release did not occur before saturation. But this test was not exact enough to be decisive. It is not so with the experiment I am now going to report.

I passed a stream of carbon dioxide gas through the extractive liquids before I put them in the experiment. They were then exposed for a few moments to the open air (to rid them of excess carbon dioxide). Immediately afterwards, they were introduced under a receptacle full of ordinary air, which rested on mercury. If they did not cause their atmosphere to undergo any expansion in several hours, the test was continued, but in the case of expansion, the air of the receptacle **[p. 139]** was renewed. The extracts thus prepared and well depleted of excess carbon dioxide underwent the same changes as they would have if they had absorbed nothing. They became covered with an insoluble pellicle but they did not change the volume of their atmosphere, did not absorb oxygen gas, and did not remove it without replacing it with a volume of carbon dioxide gas exactly equal to the oxygen gas consumed.

It thus follows from my experiments that extracts, in the first stages of fermentation and the formation of pellicles, assimilate no oxygen gas, and that the effect of oxygen is limited to removal of carbon.

When the extracts lose a small amount of their carbon due to the effects of oxygen gas, they give up a larger amount of their oxygen and hydrogen in the form of water. I will give here but one example of the experiments I performed on this subject. They all yielded similar results. I mixed 1.91 grams (36 grains) of dry extract of oak wood with 30 grams (1 ounce) of water impregnated with carbon dioxide gas, at the precise point at which this liquid caused no expansion of the atmosphere of ordinary air **[p. 140]** in which I placed it. This atmosphere, which occupied 792 cubic centimeters (40 cubic inches), contained 21/100 oxygen gas and negligible carbon dioxide. A fortnight later, a thick pellicle had formed but the air of the receptacle had not changed in volume. It then contained 5/100 carbon dioxide, and, after deduction of that, 16/100 oxygen. It must be concluded from these results that 40 cubic centimeters (2 cubic inches) of oxygen gas disappeared and were replaced by 2 cubic inches of carbon dioxide gas, which contain, according to Lavoisier, 0.4 grains of carbon. The extract from which this element [*"principle"*] had been removed weighed no more than 30 grains after being dried in a water bath. Thus it seemed to have lost 5-1/2 grains of water from its own substance. But it was possible that this water was lost during drying. Therefore I dissolved 36 grains of dry extract of oak wood in the same amount of water as was used in the previous experiment. This solution, dried immediately afterwards for the same length of time and at the same degree of fire as the extract subjected to the effect of air, yielded 34 grains of dry extract. The drying process **[p. 141]** therefore volatilized 2 grains of extract, or rather 2 grains of water belonging to the dry substance of the extract. It follows from all these trials that 36 grains of extract were divested of 1/2 grain of carbon by the oxygen gas under the receptacle, and that they lost or formed, simultaneously, an amount of water equal to 3-1/2 grains. I would not report such modest results with confidence had I not confirmed them by a very large number of other observations.

The loss of oxygen and hydrogen from the extracts, in the form of water, in much larger proportion than the carbon that the oxygen gas removes from them, tends to increase the proportion of carbon in the extractive residue. In fact, I found that 100 parts of dry extractive pellicles yield, through the residue of their distillation in an open fire, more charcoal than 100 parts of extract that was not separated from its pellicles. Thus, these insoluble flakes, far from being an oxygenated extract, are an extract that is deoxygenated, or supersaturated with carbon.

The juices of green plants, very rich in the extractive material that Fourcroy called plant albumin **[p. 142]** because it may show some characteristics of animal albumin, do not absorb oxygen gas. The juices of *Cactus opuntia* and *Sedum telephium*, upon contact with oxygen gas, coagulate into a gelatinous and elastic substance, without changing the volume of this gas, only transforming it into carbon dioxide gas.

But care must be taken not to put the extractive juices in vessels in which they have little access to the surrounding air. The solution must be spread over a large area. Without this precaution, the parts that are too far removed from contact with oxygen gas could form carbon dioxide gas entirely from their own substance.

When the temperature at the end of the experiment is very different from that at the beginning, it is necessary, before ending the experiment, to return the extractive liquor directly back to the temperature it had had at the moment it was introduced into the receptacle, and not to make, by calculation, the correction that is usual in such a case, because the ability of water to combine with carbon dioxide gas increases with the lowering of temperature.

In none of the experiments that I have described **[p. 143]** have the plant substances begun to putrefy or reached the stage of fermentation required for them to generate, under certain conditions, hydrogen and nitrogen gases.

4.2 Use of Oxygen Gas in the Conversion of Wine to Vinegar

Through the ages, it has been recognized that contact with air markedly favors the conversion of wine to vinegar [*"acetification"*]. Lavoisier[2] found this acid fully formed, without the intervention of oxygen gas, in the fermentation products of sugar with yeast, but as experiment shows that a very small amount of oxygen gas is enough to give vinous liquor pronounced signs of acidity, I do not know but what the oxygen gas that was dissolved or interposed in the distilled water used in this experiment contributed substantially to the formation of the acid. We also cannot know if acetic acid exists **[p. 144]** fully formed in yeast, combined either with ammonia or with the glutinous substance contained in the yeast. Berthollet has already noted that this experiment was not conclusive.[3]

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[2] *Traité élémentaire de Chimie*, by Lavoisier, pp. 143 and following.

[3] *Statique chimique*, Vol. 2, p. 525.

Rozier found that oxygen gas was absorbed during the conversion of wine to vinegar. He concluded this from the decrease in volume undergone by atmospheric air in this process. But I showed in the preceding section that this decrease is not a proof of the assimilation of oxygen gas with plant matter, if the volume of gas absorbed is not compared with the volume of the liquid. I showed that the oxygen gas absorbed may be limited to forming carbon dioxide gas, which remains dissolved or interposed in the liquor.

For a full year, I held wine in contact with oxygen gas in receptacles closed by mercury. This wine turned to vinegar, but I never found that the reduction in volume of the oxygen gas exceeded the volume of the wine. It was always less than that.

When I performed this experiment with wine artificially impregnated with carbon dioxide gas, **[p. 145]** at the point where the mixture no longer expanded atmospheric air,[4] this wine also turned to vinegar, but without changing the volume of its atmosphere and by replacing the oxygen gas (which disappeared) by exactly the same volume of carbon dioxide gas.

Three cubic inches of Bordeaux wine, lightly impregnated with carbon dioxide gas and placed for 5 months in contact with 80 cubic inches of atmospheric air confined by mercury in an oven heated to a constant 20–23° Réaumur, became acidic to the point of being undrinkable and of eliciting a slight effervescence in potash. The wine underwent this change without changing the volume of its atmosphere, but by forming with oxygen gas about 6 cubic inches of carbon dioxide gas and causing an equal volume of oxygen gas to disappear. Four experiments done under the same conditions on different wines gave me similar results. In a fifth experiment, **[p. 146]** I found a small volume reduction and a corresponding absorption of oxygen gas, but, in this particular case, the wine had not been placed in, and withdrawn from, the experiment at the same temperature. It was at +20° Réaumur when the experiment began and 0° when it ended. I do not doubt that the attraction of wine for carbon dioxide gas, which increases with the lowering of the temperature, was the cause of this anomaly.

A freshly prepared dough of wheat flour does not assimilate oxygen gas when acidifying in atmospheric air. The effect of this gas is limited to removal of carbon. This experiment, done in the summer, lasted around 60 hours.

Three cubic inches of alcohol placed for 5 months in contact with 80 cubic inches of atmospheric air underwent no change, nor did the surrounding air.

My experiments on acetic acid formation do not undermine the generally accepted view that wines become acidified due to a larger share of oxygen, for they are formed largely of oxygen, hydrogen and carbon, and the removal of carbon increases the proportion of oxygen.

[p. 147] One could suppose that wines, on the one hand, assimilate oxygen, and, on the other hand, return an exactly equal volume of carbon dioxide gas entirely from their own substance; but admitting such a consistently exact compensation would perhaps be giving too much weight to this hypothesis. Besides, the result would be

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[4] Wine by itself, deprived of air, always expands atmospheric air a little, due to the alcohol that is dissolved in it and that assumes a gaseous state.

the same under this supposition: Wines become oxygenated or acidified only through loss of their carbon, since they would lose, in the elimination of carbon dioxide gas, as much oxygen gas as they would take from the atmospheric air.

4.3 Influence of Oxygen Gas on Dead Wood

Wood, moistened and placed in oxygen-free environments, add to them carbon dioxide gas formed entirely from its own substance. The formation of this gas does not have the same origin when the wood is in the presence of oxygen gas.

I mixed 15.29 grams (1/2 ounce) of sawdust of an oak trunk, dried at a set reading of the thermometer and hygrometer, with water so as to make a paste. **[p. 148]** The mixture, placed in a glass capsule for 5 weeks under a capacious receptacle full of ordinary air and closed by mercury, was exposed to a constant temperature. After the allotted time, this air had not changed in volume. There were formed 198 cubic centimeters (10 cubic inches) [*de Saussure wrote 1.98 cubic centimeters, but presumably 10 cubic inches is correct, so the metric equivalent should be 198 cubic centimeters*] of carbon dioxide gas, which took the place of the same volume of oxygen gas. This sawdust, which was then exposed to the open air under conditions otherwise the same as those under the receptacle, was dried after 3 weeks, at the same readings of the thermometer and hygrometer as before it was moistened. Its dry weight had decreased by 796 milligrams (15 grains). Yet during its confinement and drying, it could have lost only 159 milligrams (3 grains) of carbon, at most. It had thus lost, in addition to this element, an amount of oxygen and hydrogen represented by at least 637 milligrams (12 grains) of water. The sawdust was covered, on the surface exposed to air, by a dark brown crust. One hundred parts of this crust yielded,[5] by carbonization, 20-1/2 parts of charcoal. **[p. 149]** One hundred parts of sawdust, before its alteration by air, produced only 17-1/2 parts of charcoal.

From these experiments and several others, it follows: (1) that wood and sapwood, after their death, do not assimilate atmospheric oxygen gas; (2) that the effect of atmospheric oxygen gas is limited to removing carbon from them; (3) that these plant substances lose oxygen and hydrogen, in the form of water, at the same time and in greater proportion than carbon; (4) that the removal of oxygen and hydrogen increases the proportion of carbon in the wood residue.

Woods that decompose through the effects only of water, without contact with oxygen gas, as occurs in dead branches that retain a thick, tight bark, undergo very different changes. Their substance whitens rather than darkening, and their proportion of carbon decreases rather than increasing. I believe that this change is due largely to the loss of their extractive and coloring substances, which are richer in carbon than is the wood itself.

One hundred parts of oak wood that had decomposed to a white color without contact with air **[p. 150]** yielded 16 parts of charcoal, excluding the ash.

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[5] The procedures followed in this operation are indicated in the note on carbonizations at the end of Chap. 5.

One hundred parts of oak wood, rotted to a black color with contact with air, yielded 25-1/2 parts of charcoal, excluding the ash.

It is impossible to reduce wood, by multiple decoctions, to the point where it loses no more extractive substances to water. Each decoction or maceration in contact with air produces an extract that had not existed before, or that becomes soluble in water through a change of substances brought about in the process of boiling or maceration. I boiled 92 grams (3 ounces) of oak wood sawdust for a half-hour in 24 times its weight of distilled water. The decoction, filtered and evaporated at a gentle heat, yielded a dry extract weighing 4.7 grams (90 grains). A second decoction, similar to the first, gave an extract weighing 1.5 grams (29 grains). These amounts decreased continually up to the 9th decoction, which yielded an extract weighing 212 milligrams (4 grains). The 11th decoction yielded a similar amount, and so did the 12th. The moistened sawdust that had been subjected to these twelve operations **[p. 151]** was exposed to air, but protected from dust, for 2 months. I ascertained that the wood only converted oxygen gas into carbon dioxide gas, without assimilating the oxygen, during this period. After the allotted time, the sawdust yielded, by a 13th decoction similar to the previous ones, an amount of extract that weighed 292 milligrams (5-1/2 grains), and that was, consequently, larger than the product of the 9th decoction. A 14th decoction yielded an extract weighing 4 grains, but after 2 months of exposure to air, the amount of extract produced by a single operation returned again to 5-1/2 grains. Wood that is the most exhausted of its extractive substances always yields, when macerated in cold water in contact with air, infusions that are rich in extracts. These extracts are all soluble in a small amount of water. In a concentrated state, they are clouded by limewater, potassium carbonate ["*carbonate of potash*"], and metal solutions.

Oxygen gas has less effect on wood lacking its extractive substances than on wood that has them.

Wood freed of its extract by decoction in a large amount of water yields **[p. 152]**, at the same weight, less charcoal than does natural wood.

The sap, in forming sapwood [*sap was believed at that time to give rise to sapwood*], probably undergoes changes somewhat similar to those of an extract that is carbonized by the effects of oxygen gas and that becomes partly insoluble in water. But there is undoubtedly this difference: In the change in the extract, carbon that is more or less impure precipitates or separates, in a black form, from the rest of the compound, whereas in the sap, through a particular disposition of this substance or by the effects of plant growth, the carbon remains in combination to form the sapwood. In this compound, insoluble in water, the proportion of carbon is at a maximum. But when the sapwood is exposed to the effects of air, the combination is destroyed. The combination parts with its carbon, which combines in part with oxygen gas and in larger part precipitates in the form of humus or very impure charcoal. The wood, through the separation of this carbon, returns towards its origin by passing to a state of water-soluble juice. Fully formed wood itself is often perhaps no longer a homogeneous combination, but a very condensed sapwood mixed with charcoal or humus. See the note on carbonizations at the end of Chap. 5.

4.4 Condensation of Oxygen Gas by Oils [p. 153]

Oils cause oxygen gas to disappear, but not in the way that extractive juices, vinous liquors, and wood do, solely by forming carbon dioxide gas. I found that turpentine oil could absorb, in 4 months, twenty times its volume of oxygen gas, producing a volume of carbon dioxide gas that was four times less than the volume of oxygen gas absorbed. The results were the same when the oil being tested had been saturated with carbon dioxide beforehand.[6]

Linseed oil could absorb more than twelve times its volume of oxygen gas in **[p. 154]** 4 months without leaving a detectable amount of carbon dioxide in its atmosphere. Of all the substances, either animal or plant, that I tested, it is the only one that gave me this result. This property could make it suitable for use in eudiometric tests. The linseed oil that I used in my experiments did not condense nitrogen gas and did not emit any gas at room temperature in gaseous environments devoid of oxygen gas.

The occurrence of water in oils, both animal and plant, that have been exposed for a long time to the effects of air has been established by a very large number of observations. It can be presumed that the oxygen gas absorbed is not assimilated by these substances. I have not noticed the formation of water in the experiments that I have detailed, but perhaps they were not continued long enough and the oil was able to hold in solution or suspension the water that it contributed to forming. We must therefore await further observations before deciding whether the formation of resins by oils, through the effects of air, is due to a removal of substances or to an addition of foreign oxygen gas, or to both effects together. We will be content **[p. 155]** to note that the removal of carbon contributes little or nothing to this formation of resins.

Oils do not appear to absorb or assimilate oxygen gas in the process of plant growth. They seem to have this property only when they have become a secretion, or a body external to the plant that produced them. Thus the seeds of flax and hemp, from which oils that condense oxygen gas can be expressed, do not absorb this gas noticeably in the process of germination. They modify it, as do mucilaginous and starchy seeds, only by converting it into carbon dioxide gas.

4.5 Condensation of Oxygen Gas by Putrefying Plant Substances

Plants, regardless of their stage of fermentation, never increase the volume of the atmosphere with which they are in direct contact if it contains a large proportion of oxygen gas. But if it contains little or no oxygen, they increase it, in the early stages

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[6] In doing these experiments, I had occasion to observe a fact reported by Priestley: that turpentine oil absorbs nitrogen gas. But it seemed to me that the volume of nitrogen gas that was condensed never exceeded the volume of the oil. Priestley recognized that the oil lost this property over time and regained it after being exposed to a vacuum. It therefore seems very likely that the nitrogen does not lose its caloric in this combination.

of fermentation, **[p. 156]** by pure carbon dioxide gas, and, during putrefaction or the final stage of fermentation, by hydrogen or other inflammable gases mixed with carbon dioxide and nitrogen gas.

I have, on several occasions, observed a fact already reported by Berthollet,[7] with his customary accuracy, namely, that a putrefying body, all of whose parts are in contact with atmospheric air, never emits either hydrogen or nitrogen gas.

It seemed to me that as long as fermenting plant substances are not emitting hydrogen gas into an oxygen-free atmosphere, they will not change the volume of an atmosphere of oxygen gas, but that if they are put in an atmosphere of oxygen gas at a time when they can emit hydrogen gas under water or in nitrogen gas, they cause a condensation of the oxygen gas. I deprived some pea seeds of their growth ability by submerging them for several moments in boiling water. These peas, swollen and submerged in water, at first did not emit any gas. **[p. 157]** Carbon dioxide appeared first, followed by hydrogen. Immediately after their death, I placed them in an atmosphere of ordinary air. After 4 days the atmosphere had not changed in volume. Six cubic inches of oxygen gas had disappeared and been replaced by 6 cubic inches of carbon dioxide gas. After the experiment, I submerged these same peas in water. After 2 days they began to release inflammable gas. I then put them in direct contact with 100 cubic inches of atmospheric air closed by mercury. After 3 days, this atmosphere had been condensed by 1 cubic inch, and I found by eudiometric examination that 7 cubic inches of oxygen gas had disappeared and been replaced by only 6 cubic inches of carbon dioxide gas. This atmosphere contained no hydrogen gas. Haricots, favas, and potato roots provided, during the period when they emitted hydrogen gas, the same results as the peas.

I placed 3.82 grams (1 *gros*) of fresh gluten under a receptacle containing 1,070 cubic centimeters (54 cubic inches) of atmospheric air. After 8 days at a constant temperature **[p. 158]** of 10–12° Réaumur, and the gluten appearing to be softened, the air of the receptacle had not changed in volume. It had lost 59 cubic centimeters (3 cubic inches) of oxygen gas, which had been replaced by the same volume of carbon dioxide gas. The gluten then had the sour smell of wheat dough beginning to ferment. By drying for 3 hours, it was reduced to 1.35 grams. But before the experiment, it had been reduced by drying to 1.48 grams (28 grains). Thus it had lost 2 grains of water from its dry substance.

I continued the experiment, under the same conditions, on 3.82 grams (1 *gros*) of the same gluten, brought to the stage of fermentation at which I had left that of the previous experiment. When placed under water, it began to release hydrogen gas. After 8 days, the 54 cubic inches of atmospheric air had decreased by 3/4 cubic inch. The air then contained 8/100 carbon dioxide, and after the deduction of this, 11-1/2 hundredths oxygen. Nine and one-half parts of oxygen gas had disappeared and been replaced by only eight parts of carbon dioxide gas. **[p. 159]** The gluten floated in the water that it had formed. I dried it at a gentle heat. It was reduced by this operation to 1.38 grams (26 grains). It had thus produced 46 grains of water. This liquid contained ammonia, but an insignificant weight of it. It did not turn plant dyes [*used as reagent*] green, probably because the ammonia was combined either with

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[7] *Statique chimique*, Vol. 2, p. 546.

the gluten itself or with acetic acid. During putrefaction, the gluten was covered by a black dust that did not appear when it putrefied under water without contact with air.

4.6 Summary

It follows from the observations reported in this chapter that, in general (with the exception of the oils), during the earliest stages of fermentation, oxygen gas is not fixed in dead plants nor does it unite with their hydrogen to form water, and that it removes only carbon from them. Thus, in the conversion of wine to vinegar, in the precipitation of extracts, and in the coagulation of plant albumin, oxygen gas is used only to remove this carbon. The proportion of carbon, however, **[p. 160]** is not decreased in the combined residues from this removal, because the plant substances that are concerned release, at the same time, a part of their oxygen and hydrogen in the form of water.

But oxygen gas combines either with the hydrogen of dead plants, or with their whole substance, when they begin to putrefy and at the stage of fermentation at which they can release hydrogen gas. Since the time of this combination is exactly that of the development of hydrogen gas, since hydrogen gas is no longer found in the oxygen gas with which the putrefying plant is in direct contact, and, finally, since a much greater amount of water appears at this stage of fermentation, we must presume that the oxygen gas that has disappeared and that is not found in the carbon dioxide gas produced at the same time is not assimilated by the plant substance but is used to form water.

There is an important difference to note between the products of fermentation that occurs without contact with oxygen gas and the products of fermentation that occurs with this contact. In the first case, the carbon dioxide that is disengaged draws **[p. 161]** its two elements from the fermenting plant, whereas, in the second case, the carbon dioxide seems to draw only one of its elements (the carbon) from the fermenting substance.

[p. 162] Chapter 5
Of Plant Humus

5.1 Research on the Composition of Humus

I mean by the term humus the black substance with which dead plants become covered when exposed to the combined effects of oxygen gas and water. The experiments that I reported in the preceding chapter tend to show that this substance is not the result of the combination of oxygen gas with the dead plant, but that it is the residue of the removal of some of the elements of this plant.

For most of my research, I used almost pure humus that was freed, by a fine sieve, of most of the undecomposed plants that are **[p. 163]** always mixed with it. This humus contained little but the mineral components that arose from the plants that produced it. I collected the humus from high rocks or from within tree trunks, where it could not have been modified by the foreign substances that are ordinarily introduced into the soil through livestock access, manures, and deposits by spring water. This humus seemed fertile to me, especially when mixed with a certain amount of sand or gravel, which serves as support for the roots and provides access to oxygen gas. I excepted, however, the humus that forms in the trunks of certain trees, such as oak. When the water has no outlet, the humus is rich with excess extractives, which block the vessels of the plants. These soluble constituents do not arise in this case entirely from the humus itself, but partly from the living tree, and thus are not suited to the nutrition of all plants.

The following operations provide an overview of the differences that may generally be seen between the composition of a humus and that of the plants from which it originates.

Distillation of Oak Wood [p. 164] 10.614 grams (200 grains) of dry oak wood, distilled at a heat up to red hot, in a luted glass retort, yielded 2,293 cubic centimeters (116 cubic inches) of methane, + 575 cubic centimeters (29 cubic inches) of carbon dioxide, + 4.25 grams (80 grains) of water holding in solution pyrolignate of ammonia with excess crude acetic acid [*de Saussure's term for crude acetic acid was "pyroligneous acid"*], + 589 milligrams (13 grains) of bituminous empyreumatic oil [*liquid oil obtained from the distillation of some organic substances*]. The charcoal remaining in the retort weighed 2.23 grams (42 grains). It contained 26 milligrams (1/2 grain) of ash.

J. F. Hill, *Chemical Research on Plant Growth*,
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Distillation of Brown Humus of Oak Wood 10.614 grams (200 grains) of dry humus of oak wood, distilled as the previous wood was, yielded 2,456 cubic centimeters (124 cubic inches) of methane, + 673 cubic centimeters (34 cubic inches) of carbon dioxide gas, + 2.81 grams (53 grains) of water holding in solution pyrolignate of ammonia and carbonate of ammonia, + 530 milligrams (10 grains) of bituminous empyreumatic oil. The charcoal remaining in the retort weighed 3.13 grams (59 grains). It contained 424 milligrams (8 grains) of ash.

Distillation of Whole *Rhododendron ferrugineum* Plants [p. 165] 10.614 grams of dry plants, distilled at a heat up to red hot in a luted glass retort, yielded 1,982 cubic centimeters (100 cubic inches) of methane, + 634 cubic centimeters (32 cubic inches) of carbon dioxide gas, + 3.34 grams (63 grains) of water rich in pyrolignate of ammonia with excess crude acetic acid, + 1.7 grams (32 grains) of bituminous empyreumatic oil. The charcoal remaining in the retort weighed 2.813 grams (53 grains). It contained 159 milligrams (3 grains) of ash.

Distillation of Black Humus of the Above *Rhododendron* 10.614 grams (200 grains) of dry humus, distilled at a heat up to red hot, yielded 2,040 cubic centimeters (103 cubic inches) of methane, + 673 cubic centimeters (34 cubic inches) of carbon dioxide gas, + 3 grams (57 grains) of water rich in pyrolignate of ammonia and carbonate of ammonia, + 557 milligrams (11 grains) of bituminous empyreumatic oil. The charcoal residue of the distillation weighed 3.45 grams (65 grains). It contained 689 milligrams (13 grains) of ash.

Distillation of the humus of grass and spruce, **[p. 166]** and of these plants themselves, yielded products showing the same differences [*between plant and humus*]. It follows that undecomposed plants contain, at the same weight, more oxygen and less carbon than do their humuses. We do not know whether the carbon of these humuses is completely combined with their other constituents.

Nitrogen occurs in greater proportion in humus than in the undecomposed plant. This result is not surprising since plants that ferment in contact with air evolve almost no nitrogen gas. We cannot, however, attribute to this cause alone all the carbonate of ammonia that I obtained from the distillation of humus. It undoubtedly arises partly from the insects that live in the humus and that leave their remains in it.

The famous Klaproth obtained, by the distillation of peat, products[1] that undoubtedly contained very little nitrogen, since crude acetic acid was very abundant in it. But peat cannot be considered a true humus. It is the residue of plant decomposition **[p. 167]** occurring in stagnant water and partly without contact with air. In this case, the plants lose nitrogen in a gaseous form. Peat appears to contain less carbon than is found in a finished humus. In the table at the end of this chapter, compare the carbonizations of humus nos. 8, 15, and 19, with the carbonizations of peat nos. 32 and following.

Acids show no noteworthy effect when mixed with humus. They produce no effervescence; they do not completely dissolve the humus; and they take up some of the iron and earthy substances that the humus contains, but very little of the

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[1] *Beiträge zur Chemischen Kenntniss*. Vol. 3.

plant component. Concentrated hydrochloric and sulfuric acids reduce the plant component to charcoal with the aid of heat, and they disengage acetic acid from it, according to the comment of Vauquelin.

Alcohol does not dissolve humus. It usually separates from the humus a small amount of extractive substances and resin, equivalent at most to two or three hundredths of the weight of the humus.

Potash and soda dissolve humus almost completely. The humus releases ammonia through their effects. This solution is decomposed by acids. **[p. 168]** They precipitate from it a combustible, brown powder, which is not abundant compared with the weight of the humus devoted to the operation.

5.2 Extractive Substances of Humus

Humus is essentially insoluble in water. This liquid separates from it extractive substances that are not the humus itself. Via the following experiments, I will give an overview of the amount of extract taken up by pure water that falls on a fertile soil.

I filled a large vessel with nearly pure grass humus and watered it with distilled or rain water until it could absorb no more. After 5 days, it was subjected to the action of a press. Ten thousand parts, by weight, of expressed and filtered fluid yielded, on evaporation to dryness, a dry extract weighing 26 parts.

I performed the same experiment, for the same length of time, on the heavy soil of a vegetable garden that had been amended with manure. Ten thousand parts, by weight, of expressed water yielded a dry extract weighing 10 parts.

[p. 169] The same experiment, repeated under identical conditions on the light soil of a field that bore a fine harvest of wheat [*"blé"*], yielded, from 10,000 parts of water, 4 parts of extract.

The humuses were dry before the experiment, and the water used to moisten them contained no carbon dioxide. But that was no longer the case when the water was withdrawn. It then clouded limewater with calcareous carbonate, although not much more than ordinary spring water does. One hundred cubic inches of water from the humus, expressed into a retort in which I boiled the water immediately after expression, yielded an air containing at most 2 cubic inches of carbon dioxide gas. This determination may not be very precise, but other observations indicate that the amount of carbon dioxide that roots draw from common soil is not substantial.

When the entire upper part of a green plant rooted in humus is put into a globe, and the neck of this vessel is carefully closed at the origin of the stem, one may see a small improvement in the air of the flask only after several days or even several weeks, **[p. 170]** although a very large volume of water is given off by the leaves.

The amount of extract that boiling water can separate from humuses that are pure, natural,[2] and formed in the countryside is not considerable. I subjected such humuses

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[2] I mean by these terms a humus that leaves only a small amount of ash after it is burned, or no more than a tenth of its weight. I also assume that the humus was not amended with manure, or formed by an artificial accumulation of plants that died at the same time.

to twelve successive decoctions, each lasting a half-hour, with an amount of water weighing more than twenty-four times the weight of the humus. The amount of extract that I could collect by all these operations was no more than an eleventh of the weight of the humus, and often much less. It seemed to me that a pure humus that yielded, by the twelve decoctions I mentioned, an amount of extract equal to an eleventh of its weight, was, under the same conditions, less fertile for favas and peas than the same humus containing only a half or two-thirds of that amount.

[p. 171] If the amount of extract that a humus should contain in order to support a good crop must not be too large, it also must not be too small. I subjected an almost pure humus to twelve successive decoctions, with renewal of the water. In two garden pots full of the humus, I sowed seeds of fava, pea, and barley, and sprinkled them with rainwater, comparable in purity to distilled water. Other, similar seeds were sown at the same time in two pots like the preceding ones, and filled with the same humus, except that this humus had not been depleted of its extract. The plants in both experiments bore fertile seeds, but the weight of these plants and their seeds was one-quarter greater in the humus provided with its extract than in the exhausted humus. The action of decoction, however, had not changed the external characteristics of the exhausted humus. It could not be distinguished, by sight or feel, from the humus that still contained its extract. It seemed to me only that the exhausted humus was able to retain or imbibe more water.

One hundred parts of humus, dry and depleted of **[p. 172]** most of its soluble matter, were able to hold 477 parts of water.

Dry, unwashed humus could hold at most 400 parts.

Humus, like wood, cannot be entirely freed of its extractive substances by water, at least detectably and in contact with air. The first macerations or decoctions remove more extract than do the following ones, but soon a point is reached at which it yields an amount that is constant or no longer decreases appreciably. If the moistened humus, after it has reached this maximal exhaustion, is exposed for a long time to the effects of external air, it changes in such a way that it can produce more extract than it did by the preceding decoction. Ten thousand parts of dry, black humus of *Rhododendron ferrugineum*, which yielded by combustion 65 parts of ash, were boiled with twenty-four times their weight of distilled water. This decoction, after filtering, yielded an extract weighing 250 parts. The extract of the 9th decoction weighed 40 parts. The 10th and 11th decoctions each separately provided a similar amount. Humus exhausted to this degree, and moistened, was exposed **[p. 173]** to the effects of air, but protected from dust, for 3 months. After this period, it was subjected to a 12th decoction similar to the preceding, and it yielded an extract weighing 58 parts. Maceration in cold water produces similar effects. These macerations, if conducted for a long time and repeated more than 50 times, on the same humus already exhausted by decoction, always produced, at least in contact with air, infusions stained by a highly soluble extract.

Humus that has been partially freed of its extractive substances by decoction yields, at distillation, very nearly the same products as unexhausted humus, but the amount of charcoal residue from this operation is a little greater in the partly exhausted humus. One hundred parts of partly exhausted humus yielded 33-1/4 parts of charcoal, containing 5-1/4 parts of ash. One hundred parts of the same humus, provided with its extract, yielded, by the same operation, 31 parts [*de Saussure wrote "grains" but clearly meant "parts"*] of charcoal, containing 6-1/2 parts of ash.[3] This increase in the proportion of carbon in the humus is confined within very narrow limits. **[p. 174]** When I subjected this same humus, exhausted by several successive decoctions, to new decoctions, I could not continue to increase the proportion of carbon, although I removed a large amount of extract by these operations.

Humus extract is not deliquescent. It yields carbonate of ammonia at distillation. The aqueous solution of this extract, which is close to syrup in consistency, is neither alkaline nor acid. It has a sweet taste; precipitates in air; and is clouded after a few moments by limewater, carbonate of potash, and most metallic solutions. When mixed with alcohol, the alcohol dissolves a small part of it and separates another that is insoluble. The substance soluble in alcohol is very deliquescent.[4] [*Footnote 4, which appears at the bottom of p. 174, is not cited in text; it seems to belong here.*] The extract produced by the first macerations of humus in water contains, at equal weight, a greater proportion of the deliquescent substance than does the extract produced by the subsequent macerations.

5.3 Of the Salts Contained in Humus [p. 175]

Reagents do not usually reveal, if simply mixed with the infusion of a natural humus that formed in the countryside, significant amounts of potash, chlorides [*called "muriates" by de Saussure*], or alkaline sulfates, if the substratum on which the humus rests has added none. Most of the alkaline salts contained in plants manifest themselves only in the residue of their combustion. It is the same for the salts contained in humus.

Some writers have believed that plants themselves create the salts that they contain, because the ash of most naturally occurring humus does not lose salts to boiling water. This conclusion is undoubtedly premature. All the humus that I examined contained alkaline salts, although the ash of the humus was often resistant to water. But these salts were retained in the ash by semi-vitrification with the earthy substances, if the latter were very abundant. One hundred parts of grass humus yielded, **[p. 176]** by combustion, 50 parts of sand or ash that did not lose saline substances to boiling water. But 100 parts of dry extract of this same humus yielded 14 parts of ash, and 100 parts of this ash formed, with boiling water, a lye containing 25 parts of salts composed of free potash, chlorides, and alkaline sulfates. Further analysis showed me that the water had extracted only half of the salts contained in this same ash.

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[3] For the procedures used in these carbonizations, see the note at the end of this chapter.

[4] I do not know whether this deliquescent extractive substance is enclosed by the nondeliquescent substance, which is superabundant, such that the deliquescent substance is prevented from drawing humidity from the air, or whether the alcohol brings about a new combination in the extractive substances.

One hundred parts of *Rhododendron* humus contained 6-1/2 parts of ash. One hundred parts of this ash lost to water only 1/2 part of alkaline salts. But 100 parts of the ash of the extract of this same humus released to water one-third of its weight of alkaline salts, and this liquid was far from extracting the salts completely.

I did the same experiments on six other, very different, kinds of humus. They all provided me similar results.

5.4 Of Changes that Oxygen Gas Undergoes by its Contact with Humus [p. 177]

Humus is the residue of a putrefied substance but is itself no longer subject to putrefaction. It may even be considered an antiseptic, for the extractive substances that it contains are likely to proceed to putrid fermentation if they are isolated, but not if they remain united with the humus. For 1 year, I kept pure, unexhausted humuses in receptacles full of water and closed by mercury. They did not emit any gas, except perhaps for the small amount of carbon dioxide with which the water covering them became charged.

There is no doubt that humus, except for its saline and earthy constituents, is entirely destructible through the combined effects of air and water. Without recourse to detailed research to prove it, I can do no better than to transcribe here the observations of my father (*Voyages dans les Alpes*, § 1319) on the plant mold that covers the plains between Turin and Milan and **[p. 178]** whose cultivation dates back to remote antiquity.

> The thin layer of topsoil seen in these plains seems to me also to show that the amount of this soil cannot be regarded as a measure of the time that has elapsed since the land began to produce crops, for in this space of 10 leagues between Turin and Saint-Germano, I nowhere saw a layer as thick as a foot. Now in my opinion the shallowness of this layer shows that this soil is subject to a decomposition that sets a limit on its increase, for otherwise, why would a flat land, fertile and cultivated for more than 3,000 years, not have a thicker layer? This destructibility of the topsoil is a fact beyond exception, and farmers who have wished to replace manures by too-frequent plowing have had a sad experience. They have seen their soil gradually impoverished and their fields become barren by the destruction of the topsoil. Thus, since this soil is destructible, the amount destroyed must be, **[p. 179]** up to a certain point, proportional to its absolute amount, and as, on the other hand, only a limited amount is produced annually, its increase must necessarily have certain limits.
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> The limits of this increase must vary according to the climate; the nature and location of the substrate that serves as a base for the topsoil; the plants that grow there; the kind of cultivation they are given; and, finally, the fertility of the land. But even if all the forces that tend to increase the thickness of this soil layer were conjoined, this layer undoubtedly would finally reach a certain maximum beyond which the destructive forces, having become equal to the constructive forces, would allow no further increase.

Pure humus, imbibed with distilled water and enclosed under receptacles filled with atmospheric air or oxygen gas confined by mercury, formed carbon dioxide gas there, in causing oxygen gas to disappear. But the humus was never able to decrease the volume of this atmosphere by an amount greater than the volume of the water used to moisten it, whatever the amount of humus and **[p. 180]** the duration of the experiment, which was sometimes prolonged to more than a year. When this water was impregnated with carbon dioxide beforehand, the humus did not change the volume of its atmosphere. The oxygen gas consumed reappeared, in an exactly equal amount, in the carbon dioxide gas produced, and the humus released neither hydrogen gas nor nitrogen gas.

It is evident from these experiments that humus neither fixes nor assimilates atmospheric oxygen gas. The effect of the oxygen gas is limited strictly to removing carbon from the humus.

For the results given above to be obtained, the humus must not be rich in iron or clay deposits. Incompletely oxidized iron contained in these deposits combines effectively with oxygen gas, but this effect is not produced by humus or by pure earths, or even appreciably by iron and manganese that are combined with the plant part of the humus and that are revealed there by incineration.

Oxygen gas removes more carbon from humus that retains its extractive substances than from humus that is **[p. 181]** depleted of them by decoctions. The latter, at equal weight, formed half as much carbon dioxide gas with atmospheric oxygen gas as did the same, but unexhausted, humus.

Moistened humus, contained in vessels that do not allow its extractive substances to escape, loses, if considered in the dry state, some of its weight through contact with oxygen gas, and this loss is greater than the weight of the carbon that is removed by this gas. In a glass capsule, I mixed 30.57 grams (1 ounce) of *Rhododendron* humus, dried[5] in the shade at a set reading of the thermometer and hygrometer, with water until it could imbibe no more. I put this capsule under a large receptacle full of atmospheric air. This air was renewed several times and examined eudiometrically each time. The experiment lasted 4 months. The humus passed 3 of them under a receptacle, and 1 drying in the shade in open air to the same degree as it had been before being mixed with water. I then found that its weight **[p. 182]** had decreased by 849 milligrams (16 grains). During its confinement under the receptacle, it had removed 476 cubic centimeters (24 cubic inches) of oxygen gas and replaced them by the same volume of carbon dioxide. Thus, during the entire experiment, it had formed about 32 cubic inches of carbon dioxide gas, assuming that the production of carbon dioxide was the same during confinement and during drying.[6] Now, as 32 cubic inches of carbon dioxide contain, according to Lavoisier, 6 grains of carbon, the humus must have lost, besides this element, an amount of oxygen and hydrogen represented by 10 grains of water.

The proportion of carbon increases by the removal of water from the residue of plants that are being reduced to humus, but I believe that carbon is not increased by

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[5] For a long time this humus was completely dry to the touch, but although it was not deliquescent, its weight varied with the readings of the thermometer and hygrometer.

[6] Humus probably forms less carbon dioxide, other things being equal, under a receptacle than in open air. But as it has almost no effect on oxygen gas during the last stages of desiccation, which takes a long time, I believe that the amount of carbon dioxide that I attribute to drying is in excess rather than default.

this process in a finished humus. A finished humus must lose, due to the effects of **[p. 183]** air and water, its oxygen, hydrogen, and carbon in very nearly the same proportion. If it lost its oxygen and hydrogen in greater proportion than its carbon, we would often find, in a soil long denuded of vegetation, residues of nearly pure carbon or of charcoal. But we do not find this. They all yield, by distillation, products of which carbon makes up half, at most.

Carbon is a very powerful antiseptic, and the state in which it occurs in humus seems suited, in part, to give it this property. I filled several equivalent vessels with various kinds of humus, pure or almost pure. In each of them, I placed the same amount of beef flesh. The same experiment was done, under conditions otherwise the same, with sawdusts of the woods from which these humuses had arisen; with pure charcoal; with carcareous sand; with siliceous sand; with clayey sand; and in the open air. The flesh stayed fresh a little longer, without deteriorating, with the charcoal than with the humus, but much longer with the humus than with the sawdusts or with the various sands, or in the open air. It is partly **[p. 184]** because of this antiseptic property of humus that, for maintaining plant growth, there is a big difference between nourishing plants with extractive solutions, isolated and not renewed, and supplying them with humus. The insoluble part of the humus prevents the undecomposed plant substances from furnishing plants with fermenting juices that are always harmful to growth.

5.5 Summary

Carbon occurs in greater proportion in humus than in the plants from which the humus arises. The proportion of carbon that a humus contains, however, does not seem able to be increased significantly by the continued action of the causes that formed it.

Humus that is moistened, but considered in the dry state, loses weight at room temperature through contact with oxygen gas. Oxygen is not fixed in the humus, and does not unite with the hydrogen of the humus to form water. The oxygen gas removes only carbon from the humus. The humus, in losing this element, loses at the same time, in the form of water, its oxygen and its hydrogen, and an extract soluble in this liquid. **[p. 185]** Humus thus appears to be completely destructible at room temperature, by the joint action of oxygen gas and washings.

The extractive juices of humus contribute to a certain extent to its fertility: Their ash contains all the substances of plant ash.

Pure humus is antiseptic.

5.6 Note on the Carbonization of Various Plant Substances

When a plant or a substance derived from it [*a "proximate constituent"*] is distilled in a luted glass retort, the degree of fire that the retort can withstand without melting is often insufficient to expel all the hydrogen that remains combined with the charcoal and that could be disengaged at a higher heat. This procedure would not, however, be inexact for estimating the relative amounts of charcoal contained in non-volatile plant substances, if the various charcoals all retained the same proportion of hydrogen at the highest degree of fire that the retort can withstand. **[p. 186]** But it is not so. Plant substances such as certain seeds, that are dense and capable of softening through the effects of fire, retain a larger proportion of hydrogen than do those that are less dense and do not soften, as most woods.

In the results that I am going to give, the degree of fire used for carbonization was higher than that which melts silver, and, as the weight of the various charcoals did not change when they were subjected to a higher temperature, I have reason to think that they are comparable among themselves. I was consistent in the way I managed the fire and in the time taken to bring it to the highest level.

To carry out these carbonizations, I wrap the dry plant material in paper and form it into a compact ball that is introduced into a cylindrical iron box, or a gun barrel that is closed on one side and open on the other. It is 9 centimeters high and 4 centimeters wide. I lay a small iron disk, with a vertical handle, on the ball. This handle is used to remove the disk. The disk fits the inside diameter of the cylinder precisely. This plate is luted into this position with clay. **[p. 187]** It is covered with a layer of charcoal dust, and a second layer, of ash.

After the operation, I weigh the charred ball without defoming it, while it is hot, and I subtract the weight of the charred paper, which is known from an earlier operation.

The precautions taken in collecting, drying, and incinerating the plants are the same as those indicated in Chap. 9.

In some cases, the relative amounts of charcoal produced by different substances can be judged only by supposing them free of ash. It is this result that is entered in the fifth column of the table of carbonizations. I will give an example: 100 parts by weight of humus of spruce leaves (no. 19) produced 28 parts of ash; 100 parts of the same humus produced 52-1/2 parts of charcoal provided with ash. By subtracting the ash from the humus and from the charcoal, and setting up the proportion $100 - 28 : 52.5 - 28 = 100 : x$, I find that 100 parts of this humus devoid of ash would have yielded 34 parts of charcoal [*i.e.,* $100 - 28 = 72$; $52.5 - 28 = 24.5$; $72 : 24.5 = 100 : x$; *therefore* $x = 34$] (Table 5.1).

Table 5.1 Table of Carbonizations [**pp. 188–192**]

Carbonization No.	Name of substance	Weight of charcoal supplied by 100 parts of dry substance	Weight of ash contained in 100 parts of dry substance	Weight of charcoal supplied by 100 parts of dry substance, excluding ash
1	Wood of oak (*Quercus robur*) separated from the sapwood	19.75	0.2	19.69
2	Sapwood of the preceding wood	17.5	0.4	17.16
3	Wood and sapwood of the preceding oak, freed of their extract by repeated decoctions	16.75	0.2	16.54
4	Bark of the trunk of the preceding oak	26	5	22.1
5	Phloem of the preceding bark	24.8	6.2	19.82
6	Bark of oak branches 1 centimeter (5–6 lines) in diameter	26.6	6	21.92
7	Debarked oak branches 1 centimeter (5–6 lines) in diameter	17	0.4	16.66
8	Brown humus of oak wood	28.5	4	25.44
9	Oak wood that had become white due to decay without air contact	20	6.8	14.1
10	Oak leaves collected in May	30	5.3	26.1
11	Oak leaves collected in Sept.	26	5.5	21.69
12	Levant gall-nuts	30	2	28.57
13	Cork	22	0.87	21.31
14	Entire *Rhododendron ferrugineum* plants	23.5	1.6	22.5
15	Black humus formed by the preceding plant	31	6.5	26.2
16	Same humus stripped of its extract by repeated decoctions	33.25	5.25	28.58
17	Debarked wood of spruce (*Pinus abies*)	20	0.34 (according to Kirwan)	19.72
18	Leaves of the preceding spruce	24.5	3	22.16
19	Humus formed by leaves of the preceding spruce	52.5	28	34
20	Wood of mulberry (*Morus nigra*) separated from the sapwood	23.25	0.7	22.7
21	Sapwood of the preceding wood	17.25	1.3	16
22	Bark of the preceding tree	25	8.9	17.68
23	Phloem of the preceding bark	18.1	8.8	10.19

Table 5.1 (continued)

24	Debarked branches of hazel (*Corylus avellana*); they were 1 centimeter (5 lines) in diameter	16.5	0.5	16.08
25	Barks of the preceding branches	25.6	6.2	20.68
26	Leaves of the preceding hazel, collected in May	29.25	5.8	24.9
27	The same, collected in June	29.5	6.2	24.84
28	The same, collected in Sept.	28	7	22.58
29	Wood of hornbeam (*Carpinus betulus*)	17.75	0.6	17.25
30	Sapwood of the preceding wood	17.5	0.7	16.92. In this tree, the sapwood was very indistinct from the wood
31	Bark of the preceding tree	30.5	13.5	19.74
32	Peat analyzed by Klaproth	38.75	18.5	23.62. This carbonization, done in a glass retort, could not be completed
33	Holland peat	34.25	21.3	16.45
34	Wheat plants collected May 1, a month before flowering	25	7.9	18.56
35	The same, in flower, June 14	25	5.4	20.71
36	The same, bearing mature seeds, July 22	26.6	3.3	19.95
37	Straw of the preceding plants, separated from the seeds	23.6	4.3	20.13
38	Seeds of the preceding plants	20.5	1.3	19.45
39	Wheat flour	19.5	0.82	18.83
40	Bran	23.25	5.2	19.04
41	Starch	10.75	0.16	10.6
42	Gluten	22.75	1.25	21.77
43	Gum arabic	17.75	2.5	15.64
44	Gum tragacanth	16.75	2	15.05
45	Crystallized sugar	18.5	0.1	18.41
46	Unsized white paper	11	0.88	10.2
47	Holland linen washed several times	14	3	11.34

[p. 193] Green plant parts appear to differ from the other parts in having a larger proportion of carbon.

The proportion of carbon in the green parts decreases in autumn. They then deplete themselves of their glutinous and extractive juices. These substances are very rich in carbon, since, as we see, washed wood, linen, and paper contain only a very small proportion of this element.

The wood contains more carbon than does the sapwood.

Bark usually contains more carbon than does the wood or sapwood. This result is not consistent among all trees because the bark is not a homogeneous substance. Its epidermis alone is carbonized by contact with air. The phloem and the inner parts of the cork are not often changed by this cause, and the proportion of their carbon varies in different plants, according to circumstances that it is impossible for us to appreciate.

[p. 194] Chapter 6
Of Plant Growth in Oxygen-Free Environments

In focusing, in the two preceding chapters, on the decomposition that plants may undergo after their death, I interrupted the train of my research on plant growth, but this digression was necessary so that effects of a dead plant or its fermentation could be distinguished from those of plant growth.

The development of some plants in oxygen-free environments presents phenomena arising from a combination of these two effects.

6.1 Of Plants that Cannot Maintain their Growth in Nitrogen Gas

Plant growth seems able to maintain itself, with the aid of water, in pure nitrogen gas only **[p. 195]** through the oxygen gas that the green parts of the plants exhale there.

Plants lacking green parts or having them only in small amount cannot grow in this atmosphere. Thus, seeds do not germinate in it, and if apparent exceptions to this rule are found, it is because of the use in these experiments of too much water, which, because it cannot be entirely depleted of the oxygen gas that is dissolved or interposed in it, provides enough gas for development to begin. Not only does germination fail to occur in nitrogen gas, but seeds that have already germinated and are placed in it always die if they have sprouted only their radicles before being put into this gas. I performed these experiments on seeds of pea, garden and water cresses, and *Polygonum amphibium*. They all rotted without developing. But most of the plants that developed from these seeds were able to survive and elongate indefinitely in this gas, if they were placed in it after they had abundant green parts or leaves.

Woody branches **[p. 196]** of poplar (*Populus nigra*) and willow (*Salix alba*), with leaf buds ready to open, could not bring about this development with the aid of water, in nitrogen gas, either in the sun or in the shade. They began to putrefy after a fortnight. These same branches leafed out after 3 or 4 days when they were placed, in conditions otherwise the same, under receptacles filled with ordinary air, and they maintained their growth there for several weeks.

J. F. Hill, *Chemical Research on Plant Growth*,
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A wilted plant placed in a weakly lit spot, in a receptacle filled with ordinary air or oxygen gas confined by water, always becomes covered with mold, but it does not acquire mold in nitrogen gas. When I placed these molds, fully formed, in nitrogen gas, they did not develop further. But the gas must be completely pure, for the least amount of oxygen gas suffices to maintain the growth of these very small plants.

Roses, lilies, and carnations that were picked 2 or 3 hours before full bloom and that did in fact bloom at the end of this period under receptacles filled with ordinary air, could not accomplish this, with the aid of water, in nitrogen gas. They rotted there, at the **[p. 197]** same developmental stage as when they had been cut, and more rapidly than in ordinary air. These effects also occurred in a vacuum.

A rose has been said to last longer in a vacuum than in ordinary air, but appearances are deceiving. It is true that the rose drops its petals sooner in ordinary air, but this loss, which is a natural process in plant growth, does not indicate any decomposition in the plant. The fallen petals give off a weak but agreeable odor. The opposite occurs in a vacuum or in nitrogen gas: A rose appears to preserve its form and color there for a long time, but when, after a fortnight, we expect to withdraw it still fresh, it gives off a stench. Its petals are rotten, and it is clear that this apparent life conceals a veritable death.

6.2 Of Plants that can Grow in Nitrogen Gas

As I have said, only plants that are amply provided with green parts can grow in nitrogen gas, **[p. 198]** and even these are not all equally successful. It seemed to me that they had to present a lot of surface area, and that they had to belong to the class of plants that consume the least oxygen gas when growing in atmospheric air in darkness.

Cactus opuntia, nourished by water, was able to maintain its growth in the sun for 3 weeks in nitrogen gas, but it was greatly harmed. In the shade it died in 5 to 6 days. It was about the same with *Sedum telephium*. These plants grow indefinitely under receptacles full of ordinary air.

Pea plants that were able to withstand the first 4 or 5 days in an atmosphere of nitrogen gas (which is not always the case) continued to grow there, in the sun, for months, but only sluggishly.

I will report an experiment made on these plants. The result can be considered an average taken from several observations.

Three partially developed pea plants, which together weighed about 3 grams, gained, under a receptacle full of ordinary air, in the sun, **[p. 199]** with nourishment from pure water, 1.27 grams (24 grains) in 10 days. Similar plants that withstood the effects of nitrogen gas, in the sun, for the same length of time gained no more than 106 milligrams (3 grains). [*There is a conversion error here; 106 milligrams = 2 grains*.] These plants, when placed in nitrogen gas in the shade, always died within the first 4 days. They survived for several weeks in ordinary air.

Smaller periwinkles survived, both in the sun and in the shade, for as long in nitrogen gas as in ordinary air, namely, about 3 weeks. In both cases, they died only because they could not tolerate an excessively humid atmosphere.

Lythrum salicaria, *Inula dysenterica*, *Epilobium hirsutum*, *E. molle*, *E. montanum*, and *Polygonum persicaria*, which are all more or less marsh plants, grew admirably in nitrogen gas and made good development there for an indefinite time, similar to that under receptacles full of ordinary air. They could even sustain their growth **[p. 200]** in nitrogen gas for months, if exposed to a weak light or protected from the direct effect of sunlight. I turn now to the changes these plants make in this atmosphere.

I grew *Lythrum salicaria* in the sun in 65 cubic inches of nitrogen gas, which showed no decrease by nitric oxide [*a test for oxygen; see "nitrous air test," in Glossary*]. This plant displaced about 1/8 cubic inch, and it was immersed, only by its roots, in an ounce of water. This liquid had no contact with the water that closed the receptacle. In the course of the experiment, I had to renew [*replace*] the plant five or six times because, in elongating, it pressed and burned against the walls of the vessel that covered it. After 2 months, its atmosphere had increased by 3.4 cubic inches. The eudiometer then indicated 5/100 oxygen gas there. I prolonged the experiment for another month in this improved atmosphere but the plant did not continue to add oxygen gas to it. *Polygonum* and other plants gave me similar results. I ascertained, by several experiments, that in general the amount of oxygen gas that is developed in nitrogen gas is not proportional to the length of stay of the plant under the **[p. 201]** receptacle, but that the amount of gas usually produced in the first weeks does not increase further in the following weeks, although growth is equally vigorous at all times.

Similar plants that I grew for the same time under receptacles filled with ordinary air never added oxygen gas there.

When I placed these plants in nitrogen gas in complete darkness,[1] renewing them every 12 hours so that their growth would not languish, they produced no oxygen gas. They augmented their atmosphere with carbon dioxide gas that they formed entirely from their own substance.

If the same experiment was performed in ordinary air, carbon dioxide gas was also produced, **[p. 202]** but the volume of the atmosphere was neither increased nor decreased by it. This gas thus had another origin. It was formed by the carbon of the plant and the surrounding oxygen gas.

These observations show us the source of the oxygen gas that plants eliminate in nitrogen gas. The oxygen comes from the decomposition of the carbon dioxide gas that the plant forms entirely from its own substance. When the plant has thereby

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[1] *Lythrum salicaria*, *Polygonum persicaria*, and other marsh plants exposed to a weak or diffuse light do not leave carbon dioxide in their atmosphere of nitrogen gas. They add oxygen gas to it. But for them to produce this effect and maintain their growth for a long time at this exposure, the temperature must not be excessively elevated, for plants, like animals, require and consume less oxygen gas, the lower the temperature.

made itself an atmosphere having sufficient oxygen gas, it releases no more of it, because the carbon dioxide gas that it then produces and decomposes is the result of the union of its carbon with oxygen gas that is already formed. Then it makes almost all the oxygen that it has caused to disappear during the night reappear during the day.

The small amount of oxygen gas that *Lythrum* and *Polygonum* formed in nitrogen gas was necessary for the development of these plants, but it was much more than they needed to maintain their growth without developing.

I suspended, at the upper part of a receptacle containing 60 cubic inches of nitrogen gas, a mixture of 1 part iron filings, 2 parts flowers of sulfur [*sublimed sulfur*], and 1-1/2 parts water.[2] [*This mixture would remove oxygen gas*] **[p. 203]** At the same time, I introduced into this atmosphere confined by water two *Lythrum salicaria* plants, which together displaced 1/8 cubic inch. Their roots alone were immersed under the receptacle, in a small vessel containing 2 centiliters of water. The apparatus was exposed, in a room, to the effects of sunlight moderated by window glass. Ten days later, one of the plants was dead. The other continued to grow or maintain itself without suffering harm, without the wilting of any leaf, for 4 months, from the third of thermidor [*11th month (July–Aug.) of the French republican calendar*] to the second of frimaire [*3rd month (Nov.–Dec.) of that calendar*], at which time I withdrew the plant, as healthy as before its introduction. The air of the receptacle underwent no reduction by nitric oxide. The plant had made no development whatsoever during its confinement. Its growth remained, somehow, suspended. This is the only effect produced by the sulfide in this experiment. The same plant elongated by 5 or 6 inches after 10 days **[p. 204]** in nitrogen gas in which this substance was not present. The removal of the oxygen gas developed by the *Lythrum* presented the only obstacle to its development. The sulfide vapors had no role in this effect, for similar plants elongated considerably with iron sulfide under a receptacle filled with ordinary air that I renewed every 3 days.

For 5 weeks, a *Polygonum persicaria* plant responded very nearly as the *Lythrum* had, under a receptacle filled with nitrogen gas in which I had suspended concentrated potassium sulfide [“*hydrosulfide of potash*”]. The plant did not develop at all and lost two leaves from the vicinity of its roots. It died after the allotted time, only from the effects of too-strong sunlight, from which I had not taken the precaution of protecting it.

Plants that I grew in nitrogen gas in the sun died much more quickly than did plants in ordinary air, from the effects of quicklime or potash suspended in their vicinity.

It is strange to see marsh plants withstand the effect of a sulfide, which removes oxygen gas from them, and not withstand the effect of lime, which removes **[p. 205]** carbon dioxide gas from them. But it should be noted that the sulfide removes oxygen gas only after the oxygen is formed, whereas lime or potash removes this same gas

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[2] Each part here is equivalent to 11.5 grams (3 *gros*.) The correct proportion of water is essential for the mixture to exert a strong effect on oxygen gas.

from them before its release. [*Lime and potash remove carbon dioxide before the plant has a chance to break it down and release the oxygen contained in it.*]

A superabundance of carbon dioxide gas is much more harmful to plants growing in nitrogen gas than to ones growing in ordinary air. I have said elsewhere that carbon dioxide gas mixed in the proportion of one-twelfth with atmospheric air, in which I grew pea plants in the sun, favored their development. It did not harm marsh plants in the same circumstances. But marsh plants were never able to tolerate this mixture, in the proportions indicated, with pure nitrogen gas. They died there in a few days, as did the pea plants. The elaboration of a certain amount of oxygen gas thus appears necessary for the elaboration of a certain amount of carbon dioxide gas. Carbon dioxide always becomes harmful to plants when they cannot decompose it.

Priestley believed that he had found that some plants have the ability to absorb the nitrogen gas in which they are growing. He reported that an *Epilobium hirsutum* plant, **[p. 206]** placed in a receptacle 10 inches high and 1 inch wide, absorbed, after a month, seven-eighths of the atmospheric air that it contained.[3]

Ingen-Housz did not limit this faculty to a small number of plants. He observed[4] that all the plants grown in nitrogen gas caused an appreciable decrease in the amount of this gas in a few hours. I followed, with great care, the growth of *Epilobium hirsutum*, both in ordinary air and in pure nitrogen gas, using the procedures indicated by Priestley for this experiment,[5] and prolonging it greatly, but I did not see any decrease in the nitrogen gas after the removal of the oxygen gas that was formed there. It was the same with all the other plants that I tested **[p. 207]**. Thus plants do not condense nitrogen gas appreciably. The experiments of Senebier and Woodhouse confirm this assertion.

If nitrogen is a simple substance, if it is not an element of water, we are forced to recognize that plants assimilate it only from plant and animal extracts and from ammonia vapors,[6] or from other water-soluble compounds that they can absorb from the soil and atmosphere. We must acknowledge that when they grow in an unrenewed atmosphere, with the aid of a small amount of pure water, the parts that develop obtain nitrogen only at the expense of the nitrogen that the other parts of the plant contained before the experiment.

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[3] *Experiments and Observations on Different Kinds of Airs*, Vol. 3, p. 332.

[4] *Expériences sur les Végétaux*, Vol. 2, p. 146.

[5] The procedure consists of planting the plant in a pot full of plant mold, submerging this pot and the origin of the stem in water underneath the shelf of a basin, and covering the rest of the plant with a receptacle full of air. The plant then undergoes much greater development than it does when it has its roots in pure water. For this reason, I was obliged to replace the plant several times.

[6] The presence of ammonia vapors in the atmosphere cannot be doubted, when we observe that pure aluminum sulfate [*"sulfate of alumina"*] is eventually converted, in the open air, into ammonium aluminum sulfate [*"ammoniacal sulfate of alumina"*]. The superiority of animal manures to plant manures seems, in large part, due only to a greater proportion of nitrogen in the former.

6.3 Of Plant Growth in Carbon Monoxide [*"oxide of carbon"*] Gas[7] (Berthollet's oxycarburetted hydrogen) [p.208]

I prepared this gas by distilling, in a gun barrel, at a red heat, a mixture of equal parts calcite ["*calcareous spar*"] and iron filings. The gaseous fluid that was obtained after having been freed of carbon dioxide contained 1/100 oxygen, which I removed by potassium sulfide [*"hydrosulfide of potash"*].

Plants grew in the carbon monoxide gas as in nitrogen gas. Those lacking green parts died in it. The growth of developed pea plants languished in it in the sun **[p. 209]** and could not maintain itself in the shade. *Epilobium hirsutum*, *Lythrum salicaria*, and *Polygonum persicaria* thrived in it as in ordinary air. After having grown in this gas for 6 weeks, in the sun, they had not decomposed it. They had increased its volume, as they had that of nitrogen gas, by a corresponding amount of oxygen gas. In total darkness, they increased this atmosphere by carbon dioxide gas.

6.4 Of Plant Growth in Hydrogen Gas

All of the seeds that I tested, without exception, failed to germinate in hydrogen gas, when placed in it with a small amount of water. Senebier observed that they bring about a very considerable reduction in the volume of this gas, by putrefying. The gaseous fluid that is a residue of this condensation is carbon monoxide gas. The carbon dioxide gas that the seeds form from their own substance is decomposed by the hydrogen gas, with the aid of the caloric **[p. 210]** disengaged in fermentation. Water forms, and the carbon dioxide, freed of a part of its oxygen, is converted into carbon monoxide gas.

Green plants grow in hydrogen gas very nearly as they do in nitrogen gas. Plants that languish in nitrogen gas also languish in hydrogen gas, and those that thrive in nitrogen thrive in hydrogen. If there is some difference in the vigor of plants growing in these two gases, it seemed to me that it was in favor of growth in nitrogen gas. Plants grown in hydrogen gas have been said to develop a darker green color, but I did not notice this effect.

I consistently observed that marsh plants, such as *Lythrum salicaria* and *Polygonum persicaria*, that I grew in the sun for 5 or 6 weeks in hydrogen gas left little or no oxygen gas there, whereas in the same period in nitrogen gas they always left fifteen or twenty times their volume of oxygen gas. This result is very likely due to the fact that, in hydrogen gas, the plants cannot completely decompose **[p. 211]**

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[7] The idea that hydrogen is an essential component of oxide of carbon gas may be based on evidence that is still too indirect to be recognized as certain.—I will note, however, in favor of oxycarburetted hydrogen, that it is odd that plants do not decompose carbon monoxide gas and that they never reduce carbon dioxide into carbon monoxide gas directly, or without the presence of hydrogen gas. [*Even after it was established that hydrogen was not a component of this gas, Berthollet continued to believe that it was.*]

all the carbon dioxide gas that they form there, because a large part of this carbon dioxide is itself decomposed by the hydrogen gas. As I have already observed, water and carbon monoxide gas are the products of this decomposition. The oxygen gas that the plants would have emitted without the hydrogen gas is concealed in these two compounds. One hundred parts (60 cubic inches) of hydrogen gas that had served as atmosphere for 5 weeks to a *Lythrum salicaria* plant exposed to the sun could not be diminished appreciably by nitric oxide. The gaseous fluid then contained no carbon dioxide, but when I ignited it by an electric spark, with the correct proportion of oxygen gas, it left a residue of water, 3 parts carbon dioxide gas, and 4 parts nitrogen gas. Hydrogen gas in which there had been no plants and that had been placed alongside the preceding, under the same conditions, did not produce a detectable amount of carbon dioxide gas when burned.

The volume of the atmosphere of the *Lythrum* decreased during growth, but to the same degree as hydrogen gas that was confined **[p. 212]** by water[8] and not in contact with *Lythrum*. Considering that this plant formed carbon monoxide gas and that the addition of this gas was not detectable through an increase in the volume of the gaseous fluid contained by the receptacle, there must have been a compensation here, and the hydrogen gas was diminished by the effect of the plant growth. It does not appear that the plants absorbed this gas. They condensed it by forming water through an indirect route.

6.5 Of Plant Growth in a Vacuum

In the vacuum produced by the best air pumps, some seeds may give signs of the beginning of germination. This result is not surprising, as it has been shown that this vacuum cannot **[p. 213]** be complete, and because, moreover, even the most perfect of these machines are never joined exactly enough to prevent the ingress of external air entirely. The pump that I used produced a vacuum in which the barometer at first stayed at 3/4 of a line, when there was no water in the receptacle. In 24 hours, it climbed by 1 line, through the imperceptible introduction of external air. Peas germinated in it after 12 days, even when I renewed the vacuum every day, but the development never went beyond the first appearance of the radicle.

Fully developed, leaf-bearing pea plants, as well as favas and haricots, always died after 3 days in a vacuum, either in the sun or in the shade. They also perished in nitrogen gas in the shade, but they often survived in it in the sun. No thin-leaved plant seemed to me able to maintain its growth in a vacuum in the sun. The thickest joints or leaves of *Cactus opuntia* survived more than a month in a vacuum in the sun. Only their epidermis dried out, in part. After this experiment, these leaves regained

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[8] For a year I kept hydrogen gas in a receptacle containing water and resting on mercury. The water absorbed approximately its own volume of gas, but no more. When I withdrew the metallic fluid, there was no limit to the absorption of the hydrogen gas by the water. It is quite probable that, as Guyton conjectured, this gas was released by the water to the atmospheric air.

their vigor **[p. 214]** when I planted them in plant mold. The thinnest cactus leaves were dead after several days in a vacuum in the sun.

A one-foot-tall *Polygonum persicaria* plant, with its roots immersed in 1 ounce of water, was placed in the vacuum that I mentioned and that I renewed each day. The plant elongated several inches. It was only withdrawn after 6 weeks, as healthy as it was before the experiment except for two or three leaves that had yellowed, in the vicinity of the roots. I obtained the same results with *Epilobium molle*, *E. hirsutum*, *Lythrum salicaria*, and *Inula dysenterica*. All of these plants thrived as well in a vacuum as under a receptacle full of ordinary air. Their transpiration was the same in both cases.

The experiments just described were carried out in full daylight but protected from the direct effects of sunlight. The plants wilted as soon as they were exposed to it, even if the rays were weak and produced no effect on similar plants confined in receptacles filled with ordinary air or pure nitrogen gas. **[p. 215]** It is likely that plants survive in a vacuum only with the aid of the oxygen gas trapped in their parenchyma, and that the sun harms the plants in expelling this gas by the expansion it causes in the gas. Solar rays do not exert the same effect on plants in nitrogen gas because the oxygen gas that the plants contain is compressed by the full weight of the atmosphere.

Plants do not seem to survive and develop in a vacuum, except with the aid of the oxygen gas eliminated by their green parts. Seeds that have only their radicles die in it. Woody plants in the springtime were unable to open their leaf buds in it.

The buds of rose, lily, and carnation were as if paralyzed there. It is evident that, in many respects, plants behave in a vacuum as in nitrogen gas, hydrogen gas, etc. The suppression of the weight of the atmosphere, or the expansion that the plant necessarily undergoes by this suppression, does not appear to have a very appreciable effect on its growth. Only the removal of oxygen gas is harmful to it.

6.6 Summary [p. 216]

Only plants provided with their green parts seem to be able to grow in oxygen-free environments, because these plants release this gas there. When this gas is removed as the plants form it, their development is arrested. The amount of oxygen gas that some require to survive, without developing, is inappreciable.

Plants do not absorb nitrogen gas. Neither do they absorb hydrogen gas. They reduce the amount of the latter a little, but this decrease occurs because the hydrogen gas decomposes the carbon dioxide gas formed by the plant. The result of this decomposition is water and carbon monoxide gas.

The green parts emit less oxygen gas in hydrogen gas than in nitrogen gas.

Green plants growing in carbon monoxide gas in the sun do not decompose this gas. They add oxygen gas to it.

Green plants grow in the vacuum produced by an air pump as in nitrogen gas, provided that the experiment takes place away from direct sunlight.

[p. 217] Chapter 7
Of the Fixation and Decomposition of Water by Plants

7.1 Investigations on the Fixation of Water by Plants Growing in Atmospheric Air Freed of Carbon Dioxide Gas

Those authors who have addressed the question of the decomposition of water by plants have advanced this subject only by conjectures unsupported by any direct experiment. Senebier never saw a plant that had been deprived of contact with carbon dioxide gas produce a volume of oxygen gas exceeding the volume of the plant itself (*Physiologie végétale*, Vol. 3, pp. 228 and following). This effect seemed to him insufficient to prove the decomposition of water. Moreover, he recognized that this small amount **[p. 218]** of oxygen gas, less than the volume of the leaf, was due to the carbon dioxide gas lodged in its parenchyma. This learned physiologist does not, however, reject the decomposition of water by plants. He considers it likely based on very scholarly considerations and on inferences drawn from the germination of some seeds in pure water without apparent contact with oxygen gas. But this observation, even if correct, would still not be a proof. I have shown that this result should be attributed to the air dissolved in the water and foreign to its composition. Ingen-Housz regarded water as a simple substance. Nevertheless some of his observations seem to suggest the decomposition of this fluid. He observed succulent plants ameliorate an atmosphere of ordinary air that was not renewed, but, as his eudiometric methods were unable to give absolute amounts, and as the volume of the plant and that of the gas produced are unknown, it is still conceivable that the volume of oxygen gas eliminated is less than the volume of the plant used in the experiment. Spallanzani,[1] who obtained the same results **[p. 219]** with succulents submerged in limewater, also leaves us uncertain about the volume of the plant. Senebier, in analyzing and repeating these last experiments, showed clearly that they were not conclusive, for the same reasons as I gave above.

The famous Berthollet, whose opinion always carries such great weight, accepted the decomposition of water in vegetation, but by reasoning rather than by new

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[1] *Journal de Physique*, 1799. [*De Saussure cited the date as* "pluviose, an 7" *(the 5th month of year 7 of the French Republican calendar).*]

J. F. Hill, *Chemical Research on Plant Growth*,
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experiments. Several authors, whom it would be superfluous to cite, accepted this decomposition, but without proofs and without even discussing the question.

Plants growing with the aid of pure water, in oxygen gas or in ordinary air that was washed in limewater before the experiment, may gain in fresh weight under these conditions, without losing or letting dry any of their parts. This result proves neither the decomposition of water, nor even the fixation of the oxygen and hydrogen of water in the plant. The plant may gain weight solely by the introduction of liquid water into the sap vessels, or of water of vegetation into the cellular tissue, for experiment **[p. 220]** showed long ago that this water may increase with soil moisture and with etiolation.

We can judge whether the dry or solid substance of plants is increased through the fixation of the constituent parts of water, by drying at room temperature a plant similar to, and of the same weight as, the one grown in a closed vessel with the aid of pure water and oxygen gas, and noting whether the dry weight of the plant grown under the experimental conditions is greater than it would have been if the plant had been dried before the experiment, as was the dried plant that serves as a comparison. It is superfluous to add that the two plants must be collected at the same stage of maturity, from the same soil, and that they must always be weighed at the same readings of the thermometer and hygrometer.

The numerous experiments that I conducted using these procedures showed me that plants grown with the aid of water alone, in a closed vessel, in atmospheric air freed of its carbon dioxide, do not increase their dry weight appreciably, and that, if they do increase it, it is by a very small, very limited amount, or, in other words, by an amount that cannot **[p. 221]** be further increased if their growth is continued for a longer time. From the numerous experiments I conducted on this subject, I will select those that gave me the most marked results.

In the month of June, in a receptacle containing 4.95 liters (250 cubic inches) of ordinary air freed of its carbon dioxide, I placed three loosestrife plants (*Lysimachia vulgaris*), whose roots were immersed, under the receptacle, in 1 centiliter of distilled water. They were exposed to the alternating effects of darkness and sunlight that was moderated when too intense. These plants weighed, in the green state, 6.96 grams (129-1/4 grains), and they displaced 10 cubic centimeters (1/2 cubic inch).

I collected other loosestrifes, of the weight I just indicated, and I dried them at room temperature. Their dry plant substance weighed 2.05 grams (38-1/2 grains) at a set reading of the thermometer and hygrometer. These plants had been uprooted at the same time and place as those grown under the receptacle.

After 8 days I removed the experimental plants from their confinement. They were perfectly healthy. **[p. 222]** They had elongated, and they had not detectably changed the air around them, either in purity or in volume. They then weighed, in the green state, 7.43 grams (141 grains) and, after being dried to the same degree as the previous plants, 2.159 grams (40-1/2 grains). They had thus increased their dry matter by $2.159 - 2.05 = 109$ milligrams (2 grains).[2] If these 109 milligrams had

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[2] These 2 grains include the oxygen that the plant assimilated from atmospheric air, but the amount of carbon dioxide that the plant formed and decomposed for this assimilation is so small that I cannot count this increase in my results.

been formed only from the hydrogen of water, the plants would have had to eliminate, under the receptacle, all the oxygen with which these 109 milligrams of hydrogen were combined, that is, at least 436 cubic centimeters (22 cubic inches) of oxygen gas, an amount that would have been very obvious, either through an increase in the volume of the air or by a eudiometric test. Now, as they did not emit an appreciable amount of oxygen gas in this experiment, it follows that they assimilated, in addition to the hydrogen **[p. 223]** of the water, almost all of its oxygen.

The experiment that I just reported was repeated for double and triple the length of time. That is, the plant was grown in a closed vessel for a fortnight and for a month. But loosestrifes of the same weight as the previous ones were never able to increase their substance by more than 2 grains, and often did not increase it at all, although they elongated by several inches.

Seven periwinkle plants (*Vinca minor* L.), having a combined weight of 7.855 grams (148 grains) and containing 2.375 grams (44-3/4 grains) of dry plant matter, were placed under a receptacle, in the same circumstances as those in the preceding experiment. They displaced 10 cubic centimeters (1/2 cubic inch). These plants did not deteriorate in the least. They did not change their atmosphere, either in purity or in volume. After drying, they weighed 46-1/2 grains. Their dry plant matter had therefore increased by 93 milligrams (1-3/4 grains), which could not be attributed solely to the assimilation of hydrogen gas from the water, since this result would have presumed the **[p. 224]** elimination of 19 cubic inches of oxygen gas, which did not occur. I repeated this experiment several times, prolonging it greatly, but the assimilation of water by the plant was never any greater.

Two aquatic mint plants, whose green weight was 157 grains, increased their green weight by 17 grains by growing in a closed vessel, with the aid of pure water, in atmospheric air [*presumably freed of its carbon dioxide, as in the preceding experiments*]. But they did not increase their dry plant matter by more than 1 grain. When the experiment was prolonged, on other, similar plants, the increase was no greater. In this experiment, as in the previous ones, the atmosphere of the plant changed neither in purity nor in volume.

I must note that I have considered only the results in which the plants did not suffer harm, for if growth languishes, the plants, far from gaining weight, lose it.

7.2 Fixation of Water by Plants Growing in a Mixture of Ordinary Air and Carbon Dioxide Gas [p. 225]

The previous experiments on the fixation of water yielded such trivial results that they are scarcely beyond the limits of experimental error, but I believe that this is not hard to explain. It is very likely that the amounts of oxygen and hydrogen in plants cannot be increased beyond certain limits without correspondingly increasing the proportion of their carbon. Consequently, I grew plants in a mixture of ordinary air and carbon dioxide gas, so that they could assimilate carbon. In all of the cases in which the plants were not harmed, the results were then more pronounced. The plants clearly increased their dry matter by an amount exceeding what they drew from the elements of the carbon dioxide.

I will now return **[p. 226]** to the experiments that I reported in Sect. 2.4, on the decomposition of carbon dioxide gas. I did not give all the details then. We saw in the First Experiment that seven periwinkle plants, grown with the aid of pure water in a mixture of ordinary air and carbon dioxide gas, assimilated the carbon contained in 431 cubic centimeters (21-3/4 cubic inches) of carbon dioxide, or an amount of carbon equal, according to Lavoisier, to 217 milligrams (4.2 grains). The plants also appropriated, from the same gas, 139 cubic centimeters (7 cubic inches) of oxygen gas, but I must disregard this last assimilation, because they lost, in turn, an equal amount of nitrogen gas. These two opposing effects nearly cancel each other. Before the experiment, these plants weighed, green, 8.955 grams (168-3/4 grains) and contained 2.707 grams (51 grains) of dry plant matter. After decomposition of the carbon dioxide, they yielded, upon drying, 3.237 grams (61 grains). They thus increased their dry matter by 531 milligrams (10 grains), of which only 217 milligrams (4.2 grains) can be attributed to the carbon dioxide. **[p. 227]** The periwinkles in this experiment thus fixed or solidified 315 milligrams (5.8 grains) of water.

Two aquatic mint plants (Sect. 2.4, Second Experiment) decomposed 309 cubic centimeters (15.6 cubic inches) of carbon dioxide gas, which contain, according to Lavoisier, 159 milligrams (3 grains) of carbon. They yielded, before the experiment, 1.698 grams (32 grains) of dry plant matter, and after the experiment, 2.016 grams (38 grains) of the same substance. They thus increased their dry plant matter by 318 milligrams (6 grains), of which 159 milligrams (3 grains) must be attributed to the fixation of water.

These results require conditions that are rarely encountered: The plant must remain healthy in all its parts. If a single leaf wilts under the receptacle, or if the roots suffer harm, the dry weight of the plant often decreases, even though the plant has decomposed a large amount of carbon dioxide gas.

7.3 Of the Decomposition of Water by Plants [p. 228]

Since plants appropriate the oxygen and hydrogen of water, we may suppose that, under certain conditions, they exhale the oxygen that was a component of this liquid. Dead plants in a heap, fermenting without contact with air, form carbon dioxide entirely from their own substance. In this process, and with the aid of the heat produced during fermentation, the oxygen of the water that was fixed or solidified during growth unites with the carbon to produce carbon dioxide gas. And growing plants, in decomposing the latter, eliminate the oxygen that had originally belonged to the water. But in no case do they appear to decompose water directly, appropriating its hydrogen and immediately releasing its other element in the form of oxygen gas.

Green plants growing day and night in nitrogen gas emit to it several times their volume of oxygen gas because, having lacked **[p. 229]** contact with oxygen gas during the first stages of their growth, they form, entirely from their own substance, the carbon dioxide gas that they decompose. But the same plants, or at least those that are not fleshy, do not add oxygen gas to an atmosphere of ordinary air or of oxygen gas in which they are growing day and night, and it is only in these atmospheres that we can judge whether they decompose water directly, because the carbon dioxide gas that they form there results from the combination of their carbon with the surrounding oxygen gas and is not a product entirely of their own substance.[3]

[p. 230] I grew *Lythrum salicaria* with the aid of water for 4 months in the sun, in 40 cubic inches of atmospheric air closed by water and mercury. I renewed [*i.e., replaced*] this plant several times so that it would not deteriorate. After this lengthy trial, the air of the receptacle contained 1/100 less oxygen gas than it had before having been subjected to the effects of plant growth, and yet growth was still very vigorous there.

Likewise, I grew *Polygonum persicaria* plants under equivalent conditions for 3 months, and the ordinary air surrounding them did not improve or change detectably.

All the other thin-leaved plants that I subjected to the same trials gave me the same results.

The only result that might be surprising is that the plants did not corrupt their atmosphere more appreciably by absorbing its oxygen gas in the decomposition of the carbon dioxide gas that they formed there. But it must be noted **[p. 231]** that the roots, submerged in water that was not renewed, being deprived of contact with oxygen gas, must produce, entirely from their own substance, a small amount of carbon dioxide gas, and that the decomposition of the latter by the leaves may very nearly offset the absorption of oxygen gas.

Indeed I often noted in these experiments that the roots released a small amount of air, especially when they were exposed to the sun. In the shade, this gas was absorbed by the plant in the proportion in which it was produced.

If plants found excess oxygen gas in the water, they would, in decomposing the carbon dioxide gas, eliminate all of the oxygen of the latter gas, but the experiments reported in Chap. 2 showed that they assimilated a part of this oxygen. Thus the oxygen found in the water that nourished them did not suffice for their nutrition.

At first glance, *Cactus opuntia* and probably other succulents[4] seem **[p. 232]** to give results that are inconsistent with those I just reported. Cactus growing day and

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[3] The results of the following experiments do not contradict those I just mentioned. I grew two aquatic mint plants in the sun for 3 weeks, with the aid of water, under two identical receptacles filled with ordinary air. In each receptacle, beside the growing plant, I placed a mint plant that was wilted but not desiccated and not completely dead. But, in one of the receptacles, this wilted mint was covered by the water that served to close the receptacle, whereas in the other receptacle, the wilted mint was suspended in the air contained therein. The plant growing near the wilted plant in air did not improve its atmosphere, whereas the mint growing with the submerged plant added several times its volume of oxygen gas to the ordinary air surrounding it. The difference in the origin of the carbon dioxide produced by the wilted plants under these two circumstances explains these results. The submerged plant produced carbon dioxide entirely from its own substance, whereas the one that was in air supplied only the carbon component of the carbon dioxide.

[4] I did the same experiments on *Sempervivum tectorum*, *Stapelia variegata*, and *Sedum telephium*, but they could not tolerate a very humid atmosphere long enough without being harmed.

night under the same receptacle filled with atmospheric air previously freed of its carbon dioxide gas may emit several times its volume of oxygen gas. I performed the following experiment on this plant. I repeated it four times under the same conditions and with the same results. The details that I am going to report will show the exactness of my methods.

At sunset, in June, I cut a leaf or joint of cactus that displaced 15 cubic centimeters (3/4 cubic inch). It was placed in a stemmed glass containing, at its bottom, 10 cubic centimeters (1/2 cubic inch) of distilled water. The leaf touched the surface of this liquid only by its tip, or wedge-shaped extremity.

All of this was covered by a sufficiently elongated receptacle that a change of 1/4 cubic inch in the volume of the atmospheric air it contained would be detectable. This air, freed of carbon dioxide gas, occupied 847 cubic centimeters (42-3/4 cubic inches). When the water in the glass was so depleted that it was no longer in contact with the cactus, I was able to replenish the water, without **[p. 233]** removing the cactus, by dipping the receptacle in boiled water and lifting it out. The receptacle was closed by this liquid, which rose inside it, and the bottom of the basin on which it rested was filled with mercury. After 31 days, I removed the plant, which seemed as healthy as when I had placed it in the experiment. It had grown roots 2.7 centimeters [*text erroneously says "decimeters"*] (1 inch) in length, and its atmosphere had increased by 69 cubic centimeters (3-1/2 cubic inches). The eudiometer indicated, in the 917 cubic centimeters (46-1/4 cubic inches) of air (taking the mean of five observations), 25/100 oxygen gas, and, before the introduction of the plant, 21/100 of this gas. This atmosphere contained no carbon dioxide. It follows from these data that the cactus eliminated, in 1 month in the sun, 3-1/2 times its volume of oxygen gas, which might be attributable to the decomposition of water.

But the plant does not appear to have brought about this decomposition directly, or to have immediately appropriated to itself the hydrogen of the water, in eliminating its oxygen. An in-depth examination leads us to believe **[p. 234]** only that it produced, in the sun, carbon dioxide entirely from its own substance, and that it decomposed this gas.

It should be noted (1) that the inner parts of the cactus, due to the plant's small surface area and the low porosity of its epidermis,[5] are not in direct contact with oxygen gas when the sun has expelled the free air that had penetrated there. After this expulsion, they are in about the same condition as they would be if they had been subjected to distillation in a closed vessel or had grown in nitrogen gas. They form carbon dioxide gas entirely from their own substance. It is not the same with thin leaves. All their parts are in contact with the surrounding air, not only because their parenchyma is not as thick, but also because their epidermis is, other things being equal, pitted with many more pores. The improvement made by the cactus in its atmosphere is very slight. **[p. 235]** Under the best circumstances, it amounts to only 1/13 of the volume of the leaf in 24 hours.

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[5] The epidermis of the leaves of succulent plants has many fewer pores than that of the leaves of other plants. We owe this curious observation to DeCandolle.

(2) A direct experiment showed me that cactus emitted oxygen gas only through the decomposition of carbon dioxide gas. I grew some cactuses for 1 month under the same conditions as those under which they had produced 3-1/2 times their volume of oxygen gas, but I suspended at the upper part of the receptacle a vessel full of potash. The cactus then added no more oxygen gas to its atmosphere. It did not change the atmosphere at all, and the potash became effervescent. The plant, however, was not harmed at all. It grew roots, as in the previous experiment. The potash, in this trial, did not remove from the plant the carbon dioxide gas that the plant formed in darkness with the surrounding oxygen gas. The cactus retained this carbon dioxide in its thick parenchyma. The potash removed from the plant only the gas that the plant formed from its own substance. When I put limewater under the receptacle, I saw that the latter carbon dioxide was formed only in the sunlight.

(3) I showed in Sect. 2.4, Fifth Experiment, that a cactus growing with the aid of **[p. 236]** water, in a mixture of ordinary air and carbon dioxide gas, retains, in decomposing the latter gas, a considerable part of the oxygen it contains, as do other plants. This plant thus does not find enough oxygen in water alone. Therefore, under the conditions most favorable to its growth, the plant is far from having to reject this element of water as superfluous.

(4) I investigated, with the utmost care, whether cacti that had emitted 3-1/2 times their volume of oxygen gas in growing for a month in atmospheric air freed of carbon dioxide gas had, in this process, increased their dry matter,[6] but I found no increase. It even seemed to me that the weight of this substance always decreased slightly.

7.4 Summary

Plants appropriate the oxygen and hydrogen of water, causing the water to lose its liquid state. **[p. 237]** This assimilation is pronounced only when the plants incorporate carbon at the same time.

Water fixed or solidified by plants probably cannot lose its oxygen in a gaseous form except after the death of the plant or one of its parts. When plants that have assimilated the oxygen and hydrogen of water begin to ferment without having free contact with oxygen gas, they form carbon dioxide gas entirely from their own substance. The oxygen of the fixed water can unite with their carbon to form this carbon dioxide gas, and plants or their growing parts, in disengaging the oxygen from this latter gas, indirectly release an element that had originally belonged to water.

Thus, through the combination of plant growth and fermentation that proceeds without contact with air, the water may release its principal element in the form of oxygen gas.

But in no case do plants decompose water directly, assimilating its hydrogen and eliminating its oxygen in a gaseous state. They exhale oxygen gas only through the direct decomposition of carbon dioxide gas.

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[6] To dry the cactus, I immersed it for several moments in boiling water and then exposed it to the sun.

[p. 238] Thin-leaved plants, growing with the aid of pure water in a mixture of oxygen and nitrogen gases, under the alternating effects of sunlight and darkness, do not add oxygen gas to the atmosphere and give no external sign of the direct decomposition of water. The oxygen gas that the plants emit in pure nitrogen gas or under water cannot be attributed to the direct decomposition of water because they form carbon dioxide gas entirely from their own substance whenever they are in an oxygen-free environment.

Some succulents, when growing in ordinary air freed of its carbon dioxide gas, add to this atmosphere an amount of oxygen gas that exceeds several times their volume. But this gas, although it could have originally been a component of water, arises, in the final analysis, only from the decomposition of carbon dioxide that the plants form, in the sun, entirely from their own substance. For when a substance that can absorb this carbon dioxide is placed near them, they no longer add oxygen gas to the environment in which they are growing day and night. They no longer give any indication of the direct decomposition of water, although their growth **[p. 239]** is vigorous. The exclusive property that succulents have of forming carbon dioxide gas from their own substance arises from the low porosity of their epidermis, or the limited contact of their inner parts with the surrounding oxygen gas.

It cannot be doubted that most of the hydrogen that annual plants acquire in developing in the open air, with the aid of distilled water, originates in this liquid, which the plants solidify. We can say as much of their oxygen, for we can judge, whether by the amount of carbon dioxide gas that these plants can decompose in a given time, or by the small change that they make in ordinary air, that the amount of oxygen that they obtain from atmospheric gases is insufficient to account for the oxygen that they acquire in the short period of their development. We must not forget that water is the most abundant product of the decomposition of most dry plants, or that oxygen is their main element.

[p. 240] Chapter 8
Of the Absorption of Solutions by Plant Roots

8.1 Water and Gas are Insufficient Nourishment to Bring about the Full Development of Plants

Plant roots, as filters, are too fine to be able to absorb substances other than fluids. If they admit solids, these must be so diluted, so divided, that their dispersion in the liquid has all the characteristics of a true solution.[1] **[p. 241]** Research on the absorption of solutions by plants is very important for the theory of their nutrition because these studies help us assess the amount and kind of nourishment that plants can assimilate from the soil through their roots.

Tull, van Helmont, and even some modern naturalists have tried to show that plants draw only water from plant mold,[2] and that manures **[p. 242]** only act on the soil, by providing plants a support more or less suitable for retaining heat and moisture. These authors supposed that the vital force, animal as well as plant, could, by decomposing or combining atmospheric air and water in various ways, produce all the substances, even the salts, earths, and metals, that analysis and incineration reveal in plants. This confused idea is no more provable than is that of making gold from substances that contain none. Before we have recourse to transmutations that are unintelligible, miraculous, and contrary to all known observations, we must make certain that plants are not finding these substances, already formed, in the

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[1] For a month I nourished thirty *Polygonum persicaria* and peppermint plants with distilled water to which I had added a certain weight of finely divided silica, which remained partly suspended with the aid of a small amount of sugar dissolved in the liquid. After the experiment, I did not find, either by incinerating the plants or by closely examining the residue of the absorbed liquor, that this earth had penetrated the plant appreciably. Bonnet caused some plants to absorb ink, but only an imponderable amount of the undissolved coloring matter was absorbed. It would have passed much more profusely through the most perfect filters we can make.

[2] I think it is unnecessary to list here the experiments of van Helmont, Tillet, Bonnet, and Duhamel, who were the main supporters of this theory. The flaws in their methods were sufficiently demonstrated by Bergman, Kirwan, and Hassenfratz. They noted especially that the vessels in which Tillet and van Helmont grew plants were porous and buried in plant mold, which could influence the results these authors obtained. Duhamel's oak was moistened with water naturally rich in extractive material. Bonnet's plants had for support plant substances more or less soluble in water.

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environments in which the plants develop, and that they do not draw the substances from that source.

Other authors have come closer to a plausible explanation in supposing that all plant constituents, except water, were provided to plants in a gaseous form. But, based on the results of direct observation, this hypothesis is not wholly tenable. Oxygen and carbon dioxide are the only known gaseous substances with which plants can **[p. 243]** nourish themselves in our atmosphere. Experiment shows that most plants do not assimilate nitrogen gas, yet nitrogen composes an essential part of plants. It is consistently found in wood, extracts, and the green coloring matter. Plants contain earths that can, as in animals, contribute to forming their solid or bony parts. We cannot prove that these substances exist in a gaseous form in our atmosphere, although we see them there in a suspended or vaporous state. But the earths have been shown to be present in springs that have washed over plant molds and that enter the roots. It has also been shown that these same spring waters hold in solution extractive substances of which nitrogen is an esssential part, and that soil fertility is, in some respects and within certain limits, dependent on the amount and nature of the water-soluble substances it contains. Finally, it is evident that, by appropriating these substances, plants eventually exhaust the soil or make it sterile.

Plants that grow naturally on dry rocks or in pure sand find, in the remains of plants and animals **[p. 244]** floating in our atmosphere, the nutriments that the gases composing the atmosphere cannot provide. These bodies settle on the leaves, dissolve in the water that condenses there, and penetrate the interior. Rain water absorbed by the roots is rich in the same materials.

Very few plants can develop fully by taking all their sustenance from our atmosphere in this way. Such scant nutrition suffices only for those whose structure is adapted to their situation, only for those that are perennial and whose very slow development is proportionate to the small quantity of nutrients assimilated. Most of them, such as the mosses, ferns, succulents, and pines, are evergreen. Their leaves decompose carbon dioxide gas all year round. They continuously offer stopping places for the corpuscles floating in our atmosphere. They transpire little. They are either leathery or succulent, and in these two forms they lose only a small amount of their carbon through the effects of the surrounding oxygen gas. But almost no annual plants occur in a soil **[p. 245]** that lacks plant mold. They must grow or develop too quickly to be able to find in our atmosphere the large amount of nutrients that they consume.

Annual plants languish as soon as they have exhausted the nourishment contained in the substance of their seeds. I tried to make seeds of fava, haricot, pea, and cress develop by giving them for support pure sand or horsehair contained in funnels that drained away the excess distilled water with which I wetted them. They usually bloomed, but their seeds were never able to mature. Nevertheless, I carried out variations of these experiments with all possible care for 5 consecutive years. Giobert, Hassenfratz and other naturalists preceded me in this research without obtaining any better results.

Those who believe that atmospheric air and water are the sole constituents and nutrients of plants object that sand, hair, and other, similar supports are not suitable media for providing the amount of water needed to maintain plant growth.

This objection would be justified if **[p. 246]** many observations had not shown that a soil may be largely stripped of its growth-promoting properties yet still be indistinguishable from the most fertile soil by physical qualities, namely, retention of heat and moisture, root penetrability, and serving as root support. Thus, it is well known that a soil that is washed-out by spring or rain waters loses its fertility, yet retains the same appearance, color, weight, and consistency. When I (as described in Sect. 5.2) stripped an almost-pure humus of most of its extractive materials, it retained its external characteristics. Its fertility, however, decreased. If it were possible to divest a humus of its soluble plant parts entirely, it might be compared to pure sand with respect to its growth-supporting properties, but complete despoliation is impossible, and we can reason here only by approximation.

If manures promoted plant nutrition principally by the gases that they develop, a field that is unsown and produces no crop should exhaust itself to the same extent as one that nurtures a rich harvest. Farming operations, however, all demonstrate the contrary. **[p. 247]** We find that harvests impoverish the soil, and do so more or less according to their nature. In general, annual plants that are rich in plant substance and transpire profusely exhaust the soil more than do perennials, which develop less rapidly, and more than annuals having succulent leaves, which transpire little, such as peas, favas, and buckwheat.[3]

We can make another observation, which is a consequence of the foregoing and which should serve to prove it. It is that, other things being equal, the plants that are the most exhausting are those that require soil that is richest in nutrients.

8.2 Do Plants Absorb the Substances that are Dissolved in Water in the Same Proportion as they Absorb the Water?

I prepared several solutions, each containing, in 793 cubic centimeters (40 cubic inches) of distilled water, **[p. 248]** 637 milligrams (12 grains) of the substance indicated below. I will assume these 12 grains equal to 100 parts.

1. Solution containing 100 parts potassium chloride [“*muriate of potash*”]
2. Solution containing 100 parts sodium chloride [“*muriate of soda*”]
3. Solution containing 100 parts calcium nitrate [“*nitrate of lime*”]
4. Solution containing 100 parts powdered sodium sulfate [“*powdered sulfate of soda*”]
5. Solution containing 100 parts ammonium chloride [“*muriate of ammonia*”]
6. Solution containing 100 parts calcium acetate [“*acetate of lime*”]
7. Solution containing 100 parts copper sulfate
8. Solution containing 100 parts crystallized sugar

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[3] *Bibliothèque Britannique. (Mémoires sur la culture du Blé. Agriculture,* Vol. 5, p. 499).

9. Solution containing 100 parts gum arabic
10. Solution containing 100 parts 25 parts humus extract[4]

I soaked root-bearing plants of *Polygonum persicaria* or *Bidens cannabina* in each of these solutions. I primarily used marsh plants in these studies, because they would be less harmed by **[p. 249]** the superabundant water that nourished them. I will add that, before putting them into the experiment, I kept them for several days in distilled water, until their roots began to lengthen.

The *Polygonum* plants grew in the shade for 5 weeks in the solutions of potassium chloride, calcium nitrate, sodium chloride, sodium sulfate, and humus extract, developing their roots in these solutions. The plants always languished in ammonium chloride, without any development. They could maintain themselves in sugar water only if the solution, which putrefied very rapidly, was renewed. They died in 8 or 10 days in the gummy water and in the solution of calcium acetate. They could survive no more than 2 or 3 days in the solution of copper sulfate.

The *Bidens* plants showed very nearly the same responses in these different solutions. In general, the *Bidens* tolerated them less well than did the *Polygonum*.

When I investigated the proportions in which the solutes were absorbed relative to the water by root suction, **[p. 250]** I used the same solutions, but I ended the experiment as soon as the plants had taken up precisely half of the liquid nourishing them, that is, 397 cubic centimeters (20 cubic inches) of solution. The plants were numerous enough that this absorption was completed in 2 days. If absorption had continued for a longer time, the roots would have putrefied in the solutions that were unsuited to them. Moreover, the sugar, gum, and humus extract could have lost most of their constituents through fermentation.

Analysis of the 397 cubic centimeters (20 cubic inches) of solution that the plants left in each vessel after suction showed me the amount of salts[5] that they had taken up. **[p. 251]** They would have taken up 50 parts if they had absorbed the saline

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[4] I did not dissolve previously prepared and dried extract in water because, if this is done, the solution still holds in suspension the part of the extract that was precipitated during evaporation. Instead I made a cold infusion of humus in water. The evaporation of a part of this infusion showed me that the infusion in which I grew the plants contained 25 parts of extract. The results obtained with salts are more accurate than those with extract, sugar, and gum because these plant compounds are always a little corrupted by contact with the roots.

[5] The saline solutions were analyzed more accurately by reagents than by any other method. I ascertained that the 100 parts of potassium chloride that I used formed, by silver nitrate, a

precipitate equal to	187-1/2 parts
100 parts of sodium chloride, by the same reagent	232-1/2 parts
100 parts of ammonium chloride, by the same reagent	270 parts
100 parts of calcium nitrate, by potassium oxalate [“*oxalate of potash*”]	78 parts
100 parts of calcium acetate, by the same reagent	81-1/4 parts
100 parts of powdered sodium sulfate, by barium acetate [“*acetate of Baryta*”]	167-1/3 parts
100 parts of copper sulfate, by the same reagent	94-1/2 parts

The solutions of gum, sugar, and humus extract were evaporated to dryness and the residue was weighed.

substances in the same proportion as the water. But I found that the *Polygonum*, in taking up half the liquid in the experiments, absorbed only

14.7 parts	Potassium chloride
13 parts	Sodium chloride
4 parts	Calcium nitrate
14.4 parts	Sodium sulfate
12 parts	Ammonium chloride
8 parts	Calcium acetate
47 parts	Copper sulfate
9 parts	Gum
29 parts	Sugar
5 parts	Humus extract

The *Bidens* absorbed:

16 parts	Potassium chloride
15 parts	Sodium chloride
8 parts	Calcium nitrate
10 parts	Sodium sulfate
17 parts	Ammonium chloride
8 parts	Calcium acetate
48 parts	Copper sulfate
32 parts	Sugar
8 parts	Gum
6 parts	Humus extract

[p. 252] It is evident that, in general, the plants absorbed all the substances that I presented to them, but that they took in water in much greater proportion than the solutes. It is also evident that the plants did not always take from this water more of those nutrients that were most beneficial to them. Copper sulfate, which was the most harmful, was absorbed the most. Gum and calcium acetate, which were very unfavorable to growth, entered the plants only in small amounts.

I repeated these experiments several times, either in the same proportions or in different ones, and I always obtained the same general results. The plants always absorbed more chlorides and alkaline sulfates than calcium acetate or calcium nitrate. They always absorbed more sugar than gum. But the results varied in detail. The absolute amounts of salts, gum, and sugar that were absorbed were never the same in two similar experiments. I soon noticed that the anomalies arose from the different conditions of the roots, which absorbed more solutes if they were less vigorous. I tried cutting off the roots, and the plants then very quickly suffered harm **[p. 253]** in all the solutions. They absorbed two or three times more solute than in the previous experiments.

I think that we must attribute the very great absorption of copper sulfate, especially, to the disorganization that it causes in the roots. From the following results, in fact, it will be evident that the addition of this salt to a solution of calcium acetate or calcium nitrate causes these latter substances to penetrate the plants in larger amounts than they would by themselves or if mixed with a salt less harmful to growth than copper sulfate.

8.3 Do Plants Absorb Certain Substances in Preference to Others, from a Liquid Containing Several Substances in Solution?

So far, I have presented to the same plant only a single salt in solution. I will now present several of them and see if the plant will make particular secretions of them. [*"Secrétion" is used here in the sense of absorption of substances by a plant; see Glossary, Appendix 2.*] I dissolved, **[p. 254]** in 793 cubic centimeters (40 cubic inches) of water, two or three different salts, each weighing 637 milligrams (12 grains). As in the preceding experiments, I set these 12 grains equal to 100 parts, and I analyzed the residue of the solution when it had been reduced by suction to precisely half its volume. The amount of salt contained in this residue, subtracted from the amount that the liquid contained before the plants were introduced, indicated to me the amount of salt that the plants had absorbed. The *Polygonum* absorbed 11.7 parts of sodium sulfate and 22 parts of sodium chloride, in absorbing up to half of a solution containing 100 parts (637 milligrams) of each of these salts. From a similar solution, the *Bidens* absorbed 7 parts of sodium sulfate and 20 parts of sodium chloride. Based on this example, the results that I present in the following table can be understood.

[p. 255] Weight of substances dissolved in water before the experiment	Weight of substances taken up by *Polygonum* in absorbing half the water of solution	Weight of substances taken up by *Bidens* in absorbing half the water of solution
100 parts of powdered sodium sulfate ["*powdered sulfate of soda*"]	11.7	7
100 parts of sodium chloride ["*muriate of soda*"]	22[6]	20
100 parts of powdered sodium sulfate	12[7]	10
100 parts of potassium chloride ["*muriate of potash*"]	17	17
100 parts of calcium acetate ["*acetate of lime*"]	8-1/4	5
100 parts of potassium chloride	33[8]	16
100 parts of calcium nitrate ["*nitrate of lime*"]	4-1/4	2
100 parts of ammonium chloride ["*muriate of ammonia*"]	16-1/2	15
100 parts of calcium acetate	31	35
100 parts of copper sulfate	34	39
100 parts of calcium nitrate	17	9
100 parts of copper sulfate	34	36
100 parts of sodium sulfate	6[9]	13
100 parts of sodium chloride	10	16

Weight of substances dissolved in water before the experiment	Weight of substances taken up by *Polygonum* in absorbing half the water of solution	Weight of substances taken up by *Bidens* in absorbing half the water of solution
100 parts of acetate of lime	Inapprec.quant.	Inapprec.quant.
100 parts of gum	26	21
100 parts of sugar	34[10]	46

[6] I precipitated the residue of root suction by barium acetate ["*acetate of Baryta*"]; the decanted liquor was precipitated by silver nitrate

[7] The analysis was done by the same procedure. It is evident that I made plants take in salts that mutually decompose. But this decomposition was not carried out here because the solutions were too dilute. Berthollet showed that the exchange of substances took place only when caused by the force of crystallization, or, what amounts to the same thing, by the insolubility of the new combination

[8] The residue of the root suction was divided into two equal parts; one was precipitated by potassium oxalate ["*oxalate of potash*"] and the other by silver nitrate

[9] The residue of root suction was divided into two equal parts; one was precipitated by potassium oxalate, and the other by barium acetate. After the separation of the barium sulfate ["*sulfate of Baryta*"], the decanted liquor was precipitated by silver nitrate

[10] I evaporated the residue to the consistency of syrup and then threw on it alcohol, which dissolved the sugar and precipitated the gum. A comparative experiment showed me that the precipitated gum obstinately retained about a quarter of its weight of sugar. The results that I have given were corrected according to this principle.

[p. 256] I repeated some of these experiments on peppermint, Scotch pine, and common juniper. The results were generally the same. The salts that were absorbed the most by *Bidens* and *Polygonum* also were by these other plants. There were differences in the absolute weights of salts absorbed. Differences are to be expected for plants that are dissimilar, but plants of the same species present, in this regard, such frequent anomalies that I could not attribute these differences with certainty to the kind of plant that I was testing.

The excision of roots, their decomposition, and a general languor of growth favor the introduction of solutes from the water into the plant.

I let plants whose roots I had cut absorb solutions. They then carried out the above-mentioned [*differential*] secretions in a less marked way. They almost indiscriminately absorbed all the salts that I presented to them. They absorbed all of them in large amounts and nearly in the same proportion as the water of solution. Gum, calcium acetate, and copper sulfate killed the plants in 7 or 8 hours, **[p. 257]** and I was able to complete the trial in the other solutions only by repeatedly replacing the wilting plants with new ones. Thus, root-bearing plants absorb, from the same solution, certain substances in preference to others. For example, they always absorb more sodium chloride and potassium chloride than calcium acetate and calcium nitrate. From a solution of sugar and gum, they take more sugar than gum, etc. These substances do not all enter the plant in proportion to their influence on growth. They are absorbed in a much smaller proportion than is the water in which they are dissolved.

I am inclined to think that the plant, in absorbing one substance rather than another from the same liquid, does not do this by virtue of a kind of affinity, but rather based on the degree of fluidity or viscosity of the different substances [*an erroneous interpretation*]. In fact, we find that distilled water is more fluid, or passes more easily or rapidly through a filter, than does water in which chloride **[p. 258]** or sulfate of sodium [*"muriate or sulfate of soda"*] is dissolved.[11] We find that calcium acetate and calcium nitrate form, with the same amount of water, more viscous solutions, which pass less easily through a filter than do the chlorides and alkaline sulfates. These last were always absorbed more abundantly by the plant than were the first. Gum, which is more viscous than sugar, was absorbed in a smaller amount. But we must assume that roots are exceedingly fine filters, much more perfect than the filters that we commonly make. For if a solution containing calcium acetate and potassium chloride is poured on a filter made of several double layers of paper, and this filtration is interrupted as soon as half the liquid has drained through, **[p. 259]** this half is not found to contain less calcium acetate than the half that remains on the filter. The same is true for solutions of sugar and of gum.

The roots did not noticeably decompose the salts that I let them absorb. I never found that an acid or an alkali had become free in the residue of the root suction.

Through incineration, I convinced myself that the salts had actually penetrated the plant substance. In distilled water, I grew some root-bearing *Polygonum* plants weighing, in the green state, 173.13 grams (3,262 grains). I grew other *Polygonum* plants, of exactly the same weight, in 1.58 liters of water in which 3 grams (56-1/2 grains) of potassium chloride were dissolved. As the plants used up the solution, I replaced it with distilled water. The plants fared as well in the solution as they did in the pure water. After 3 weeks I ended the experiment, and I found, by analyzing the residue of the suction after drying it at a red heat, that the plants had removed 1.59 grams (30 grains) of potassium chloride.

After drying, **[p. 260]** the plants weighed 39.8 grams (750 grains), and they produced, by combustion, 4.246 grams (80 grains) of ash.

The *Polygonum* plants grown in distilled water weighed, after drying, 38.851 grams (733 grains) [*the author made an error in conversion: he wrote 132 grains; since the context strongly suggests that the "38.851 grams" figure is correct, the conversion should be 733 grains*] and produced 2.76 grams (52 grains) of ash [*another conversion error; the author wrote 151 grains instead of 52*].

The plants thus increased their ash by an amount very nearly equal to that of the potassium chloride ["*muriate of potash*"] that disappeared from the solution. I analyzed these ashes and found, by silver nitrate, all the hydrochloric acid contained in the added salts. The potassium chloride had not been decomposed. The absolute amount of free potash was no greater in the ash of the *Polygonum* that had absorbed pure water than in the ash of the *Polygonum* that had absorbed potassium chloride. [*De Saussure probably meant to say this the other way around: that the absolute amount of free potash was no greater in the ash of the Polygonum that had absorbed*

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[11] For these effects to be detectable, the solutions must be more concentrated than the ones that I let plants absorb. It seems likely to me that a dilute aqueous saline solution is not always a homogeneous combination, but a mixture of saturated and unsaturated water. It is well known that a dilute aqueous solution of sodium chloride that is left completely at rest becomes denser and saltier at the bottom of the vessel than at its surface. (See the memoir of Leblanc on *Cristallotechnie*. *Journal de Physique*, year 11 [*of the French Republican calendar;* i.e., 1802].

potassium chloride than in the ash of the Polygonum that had absorbed pure water.] This experiment, repeated on *Bidens*, yielded very similar results. It is clear that the same [*kinds of*] plants may contain very different amounts of salts and that we cannot rely too closely on constructed tables for assigning to each [*kind of*] plant a certain proportion of ash or salts. These determinations **[p. 261]** can be exact only insofar as the plants grew in the same soil, for the same time.

8.4 Considerations on the Saline or Mineral Substances that Enter into the Composition of Plants

Some authors have argued that the mineral substances found in plants are only there accidentally and are not at all necessary to their existence, because the plants contain them only in very small amounts. This opinion, undoubtedly true for substances that are not always found in plants of the same kind, has not been demonstrated for those substances that are invariably present. Their small amount does not indicate that they are useless. The calcium phosphate [*"phosphate of lime"*] contained in an animal perhaps composes not 1/500 of its weight, yet no one doubts that this salt is essential to the constitution of its bones. I have found this same salt in the ash of all the plants in which I have looked for it, and we have no reason to assert that they can live without it.

[p. 262] It has often been concluded, from the fact that some salts, in certain proportions, are harmful to certain plants, that all salts, in all proportions, are harmful to plant growth. But observation rarely confirms these systematic and general ideas. Observation shows that many plants require a saline nutrient but that the nutrient must be modified in its amount and characteristics, according to the nature of the plant that must absorb it. Duhamel recognized that marine plants languish in a soil that lacks sodium chloride [*"muriate of soda"*]. This salt is harmful to wheat [*blé*] in the same proportions in which it is useful to marine plants. Pellitory, nettle, and borage thrive only where they find calcium nitrate [*"calcareous nitrate"*] or potassium nitrate [*"nitrate of potash"*]. Calcium sulfate [*"sulfate of lime"*] accelerates the development of alfalfa, clover, and sainfoin. It has no effect on some other plants. Salts were believed to favor plant growth only because they hastened the putrefaction of dead plant matter spread on the soil or because they drew humidity from the air. But calcium sulfate is not deliquescent, and if it were useful in accelerating putrefaction, its salutary effect would not be limited to **[p. 263]** such a small number of plants. Farmers use too little of it for it to produce the septic effects that have been attributed to it or to be able to change the physical properties of the soil, considered simply as a support for plants.

Thouvenel and Cornette believed they had found that saline solutions did not enter plants and that the salts acted as astringents or corrosives, by closing the vessel openings and resisting the absorption of water. Observation has shown, however, up to now, that plants contain salts that occur in the soil in which they are growing. Pellitory, nettle, etc., are rich in nitrate. Marine plants are rich in sea salt and no longer yield it if they are forced to languish far from the seaside. De Bullion (*Mémoires d'Agriculture*, 1791) sowed some seeds of sunflower (*Helianthus annuus*) in a sandy soil lacking niter. The plants arising from the seeds yielded, at analysis, no trace of this salt. He watered other, similar plants in this same soil with a solution of potassium nitrate [*"nitrate of potash"*], and they became enriched in it.

Although experiment shows that certain **[p. 264]** saline substances are favorable to the growth of some plants, it reveals at the same time that this nourishment suits them only if provided in very small amount. Calcium sulfate probably owes its salutary influence on the development of some plants partly to its low solubility. The ineffectiveness of salts that are more soluble, such as potassium nitrate, potassium carbonate, and sea salt, when used in their pure state and applied directly to the soil, is recognized for most of the common plants. The usefulness of ash that contains some of these salts is not disputed, but in the ash they are combined with earths through a semi-vitrification that decreases their solubility and makes them penetrate the plants slowly and in repeated doses, rather than all at once. In certain plants, excess salts accumulate on the surface of the leaves and form there an incrustation that kills them by impeding their transpiration. This, in large part, is the origin of the white malady that attacks the Cucurbitaceae[12] and many garden vegetable plants.

Plants do not obtain all of their mineral substances from saline solutions similar to those that can be formed artificially by dissolving, for example, chlorides of lime, iron or manganese in pure water, but they absorb them largely in combinations that we cannot compose, such as ones in which the elements of these salts are chemically united with oxygen, hydrogen, nitrogen, and carbon **[p. 266]** in humus extract, and in which the salts can be detected only by incineration of the extract.

8.5 Application of the Foregoing Observations to Research on the Amount of Nutrients that the Humus Substance Alone Provides to Plant Roots

We are too far from knowing all the effects of the plant on humus, and of humus on the plant, to be able to calculate and weigh all the elements that the plant takes from humus. In the study of nature, we can do no better than to follow this maxim[13] of the famous Haüy: *Things are supposed to be such in themselves as they present*

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[12] In gourd plants, this malady begins with viscous beads that appear especially on the upper surface of the leaf, near the petiole. These beads dry and form prominent, powdery white blotches that spread and multiply successively, out to the perimeter of the leaf. I detached such an incrustation. It was only partially soluble in water and in alcohol. These solutions, evaporated to dryness, yielded a deliquescent salt that had all the properties of calcium chloride combined with a negligible amount of magnesia. The salt was precipitated in abundance by silver nitrate, potassium oxalate, and alkaline carbonates, but not at all by barium hydroxide fully dissolved in water ["*water of baryta*"], and it was almost unalterable by fire. The saline and earthy part of the incrustation made up about a third of its weight. It was enveloped by a white plant substance insoluble in water and alcohol, and abundant enough that the incrustation itself did not noticeably draw humidity. This malady especially attacks old plants growing in soil that is very rich in animal manure, and in hot-beds where the leaves are not washed by rainwater.

[13] *Traité de Minéralogie*, by Haüy, Vol. 1, p. 7.

themselves to our observation. If we rely on the observations made either in this chapter or in previous ones, we see that humus extract, carbon dioxide gas, oxygen gas, and all the water-soluble compounds that enter from the soil into a green plant through its **[p. 267]** roots are not nearly sufficient to make up the greater part of the weight of this plant, considered in the dry state.[14]

In Sect. 5.2, we saw that rain water, after having remained for several days in well manured garden soil, formed an infusion that contained 1 part of dry extract in 1,000 parts of water. We saw that a plant that absorbed this infusion would take only a quarter of the solid extract that it contained. If the humus extract were its sole nourishment, this plant would then gain only 1/4 pound of dry weight **[p. 268]** in absorbing 1,000 pounds of infusion. An annual plant, such as a sunflower, that grew in this garden would be able to acquire, in the 4 months after it germinated, a weight of 4 kilograms (8 pounds) in the fresh state, or 1/2 kilogram (1 pound) in the dry state.[15] If we accept the experiments of Hales showing that the amount of water taken up and transpired in 24 hours is equal to half the fresh weight of the sunflower, then, according to my weighings of the sunflower at different stages of its growth, it could not have transpired and absorbed more than 100 kilograms (200 pounds) of water or infusion in 4 months. The amount of dry extract contained in these 100 kilograms is 100 grams, or 1/5 pound. The plant took only one-quarter, or 25 grams, of it, but the carbon dioxide contained in 100 kilograms of infusion must be added to it. [*Here de Saussure is again saying that some carbon dioxide enters plants by way of the roots.*] I estimate that this gas weighs 3.7 grams (70 grains), based on the experiments **[p. 269]** reported in Sect. 5.2. Only half of the weight of this gas was assimilated by the sunflower, which, in decomposing it, exhaled a part of its constituents. This plant thus drew from the humus, disregarding the water, $25 + 1.85$ grams of plant substance, which makes up only about 1/20 of the dry weight that the sunflower gained, based on direct observation.

The calculation that I have just made is undoubtedly far from exact, but, supposing that my estimate of the amount of nutrients that the plant draws through its roots from the soil is two or three times too large or two or three times too small, the essential, general results that I have in mind nevertheless remain the same. They show equally that the humus extract, the gases, and all the water-soluble substances that come from

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[14] I could rely on the experiment of van Helmont, who, after having grown a willow for 5 years in 200 pounds of humus, found that this plant had gained, in the green state, 164 pounds and that the humus, which was dried in an oven before and after the experiment, had lost only 2 ounces. But Kirwan made some very valid reflections on these results: He observed (1) that the humus was placed in an unglazed, porous earthen vessel, which was itself buried in plant mold, and that this last conveyed extractive juices to the plant; (2) that the oven drying might not have been the same before and after the experiment; (3) that the willow left, in the soil, root fibrils whose weight could not be estimated; (4) that the rain water that was used for watering the plant must, through its impurity, have contributed to the plant's nutrition. (*Mémoire sur les Engrai*s, by Kirwan.)

[15] Hales says that a sunflower in full growth loses only a quarter of its weight by drying. This result is certainly incorrect, as are the inferences that this author deduced from it. [*De Saussure misinterprets Hales, who wrote, on p. 8 of Vegetable Staticks (1727), that the sunflower, upon drying, lost 3/4 of its weight as water.*]

the soil and enter the roots of a green plant do not compose the major part of the plant's dry weight, if the water itself is excepted. These [*i.e., the water-soluble substances from the soil*] will always, however, be found to enter the plant appreciably and, to have, as nutrients, a very powerful influence on growth, despite their small amount. It is clear that the water **[p. 270]** acquired and solidified by the plant, whether from the soil or from the atmosphere, makes up the greater part of the plant's dry matter, by weight; that the carbon is provided to the plant in a gaseous form, by the atmosphere more abundantly than by any other source; but that the nitrogen, the salts, and the earths, which are the least abundant elements in the plant, come from: (1) extractive and saline solutions drawn by the roots from the humus; and (2) plant and animal matter held in suspension in the atmosphere and deposited on the plant.

8.6 Summary

1. Plant roots absorb salts and extracts, but in smaller proportions than the water in which these salts and extracts are dissolved.
2. The cutting away of roots, their decomposition, and sluggish plant growth in general promote the introduction of salts and extracts into plants.
3. A plant does not absorb, in the same proportion, all the substances contained simultaneously in the same solution. It makes particular secretions from it. In general, it absorbs **[p. 271]** more of those substances whose separate solutions are less viscous.
4. If the weight of the extract that the most fertile soil can produce is compared to the weight of a dry plant that developed there, it is evident that the plant could have drawn only a very small part of its substance from that source.

[p. 272] Chapter 9
Observations on the Ash of Plants

9.1 Observations Made by Various Authors on the Amounts of Ash Produced by Plants

Experiment has long shown that plants of the same weight vary, by species, in the amount of ash that they can produce. But the general reason for this phenomenon has not been traced. The origin of this ash and the reason for the differences have not been investigated. Before telling of my observations, I will briefly review what is already known.

The inspector-commissioners of saltpeter manufacture in France, **[p. 273]** and Kirwan and Rückert,[1] found that, after drying, herbaceous plants yield more ash than the same weight of woody plants. This result has been confirmed by all who have been concerned with incineration products. Perthuis provided a beautiful demonstration of this principle, when he reported that the trunks of trees produce less ash than the branches, and the branches less than the leaves (*Annales de Chimie*, Vol. 19). But we must stop there, regarding the results of this author. His other assertions, (1) that plants that are burned when they are dry produce less ash than when they are burned green; (2) that decayed wood produces less ash than healthy wood; (3) that plants burned when mature produce more ash than when burned before or after maturity; and (4) that plants generally yield more water-soluble salts the more ash they contain, seemed to me to need to be modified. It is regrettable that, of the 60 incinerations carried out by Perthuis, only four were **[p. 274]** done on plants that were dry and not mixed with unknown plants. The uncertainty left by incomplete drying, estimated vaguely by fractions, such as one-half, one-quarter, and three-quarters, limits the inductions that can be drawn from these experiments, especially if, as must be suspected based on the results, the plants were not protected from the rain after being cut. Despite these minor inaccuracies, which are inevitable in the course of a lengthy work, we cannot disregard the usefulness of the research of this author, whose object was not physiological observations but the large-scale extraction of alkaline salts.

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[1] *Mémoire sur les Engrais*, by Kirwan, *Société royale d'Irlande*, Vol. 5, p. 129; and Rückert's *Feld-Bau.*

J. F. Hill, *Chemical Research on Plant Growth,*
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9.2 Principle by Which Ash Varies in Amount in Woody and Herbaceous Plants

In a subject as new and complicated as the one that occupies me now, the explanations that I give will, undoubtedly, very often be guesswork. But I have reason to believe that the observations on which they are based are not so, at least for the species **[p. 275]** that I examined, for although my incinerations were numerous, they are perhaps still not sufficient for us to draw general conclusions.

I will consider only plants that are dry and of the same weight. The water of vegetation varies so greatly according to the particular constitution of each individual of a species, the age of the plant, and the climate, that it is advisable, above all, to remove this source of error.

The research of the authors that I cited in the preceding section, and my own (as can be seen in the table of incinerations (Table 9.1) at the end of this chapter), are consistent in showing that woody plants contain less ash than do herbaceous plants. If, as we saw in Chap. 8, plants allow earthy and saline substances into their interior only in a liquid state, they should contain more ash the more abundant the suction or transpiration is, for these two functions are always dependent, one on the other. Hales[2] and Bonnet[3] demonstrated **[p. 276]** that herbaceous plants transpire more water than do woody plants. The latter should therefore contain less ash.

According to the observation of Hales, the leaves of evergreen trees transpire less than do those of deciduous trees. The latter are, for this reason, richer in ash. Compare, at the end of the chapter, the incinerations of plants burned in the dry state, nos. 1, 2, 16, 31, with nos. 67, 71, 74.

The leaves of evergreen trees transpire year-round, it is true, but in winter this effect is very slight or almost nonexistent, and at this season they probably lose, due to washing by rain water, as much ash as they acquire.

When a saline solution is contained in a vessel that allows evaporation through its pores, more salt is deposited on the parts where evaporation is most abundant. The distribution of ash in a plant generally follows this principle. Transpiration being less abundant through the stem than through the leaves, the leaves are richer in ash. Compare incineration no. 29 with the following one; no. 67 with no. 69; and no. 70 or 71 with the following one [*this last should be no. 71 with no. 73*].

[p. 277] Leaves, according to the observation of Hales, transpire more than do fruits. Fruits yield much less ash. Compare, in the incinerations, nos. 30, 31, or 32 with no. 34; nos. 51, 52, or 53 with no. 54 [*this last presumably should be no. 55, not no. 54*]; and nos. 57, 58, or 59 with no. 61.

The bark is the immediate site of the transpiration of the trunk and contains much more ash than the inner parts. Compare, in the incinerations, nos. 5 and 6 with no. 7; see nos. 14 and 15, nos. 20 and 21, nos. 22, 23, 24, and nos. 26, 27, 28.

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[2] *Statique des végétaux*, French edition, pp. 3 and 43.

[3] *Recherches sur l'usage des feuilles*, p. 77, 8° edit.

Ash steadily increases in tree leaves from the moment they emerge from the bud until they turn yellow and fall off. See nos. 1 and 2, nos. 12 and 13, nos. 16, 18, and 19, and nos. 30, 31, 32. Plants should accumulate ash in all the parts that do not change their form or growth force. At all times, I chose leaves that were entirely green and healthy. In summer and autumn, however, I took care to collect only those that appeared the oldest.

The results that I have just noted **[p. 278]** cannot be applied to annual plants collected and burned in their entirety, with any dead parts that they may have. The ash of these plants diminishes as their growth advances. See, in the table of incinerations (Table 9.1), nos. 35 and 36, nos. 37, 38, 39, nos. 43, 44, 45, nos. 46, 47, 48, nos. 51, 52, 53, and nos. 57, 58, 59. The reason for this difference is that annual plants, as they age or produce new growth, lose their lower leaves, which are the oldest and consequently the most laden with ash. If these leaves do not drop off, they languish or dry, and in this withered state they give up their soluble substances to rain water, dew, and even to their transpiration. If we could monitor the proportion of ash in a single leaf of an annual plant before the leaf deteriorated, we would probably see an increase, as in the leaves of the trees that I cited.

Ash appears to increase in annual plants as they age, if the plants are considered in the green state. See nos. 37 and 38, and nos. 47 and 48. But this is **[p. 279]** an illusion produced by the water of vegetation. The greater loss of this water with advancing growth appears to increase the proportion of ash in the plant as the plant approaches the dry state.

Ash does not accumulate indefinitely in tree trunks. The sapwood contains more ash than the wood. See, in the table of incinerations (Table 9.1), nos. 5 and 6, nos. 22 and 23, and nos. 26 and 27. When the layers of sapwood harden and become wood, they lose, to the ascending juices, the ash that they had accumulated during their growth.

I took fresh hazelnut leaves, dried some of them and washed the rest several times in cold, distilled water. The latter were dried as the former were. One hundred parts, by weight, of the washed leaves yielded less ash than 100 parts of the unwashed leaves. This result may serve, in part, as support for the preceding interpretation.

A putrefied plant yields, at the same weight, more ash than the same plant unputrefied. One hundred parts, by weight, of rotten wood provided me more ash than did 100 parts of healthy wood. But this result presumes, in some cases, **[p. 280]** that the plant has not been washed by running water during putrefaction.

9.3 General Composition of Ash; Influence of Soil

The alkaline salts of potassium [*"potasse"*] or sodium [*"soude"*], the earthy phosphates of lime or magnesia, free or carbonated lime, silica, and the oxides of iron and manganese make up, together or separately, the most notable components of the ash and the only ones with which I will be concerned. Ash contains many others that we do not usually notice, due to their small amount. Ash holds perhaps all the known substances that cannot be volatilized by the effects of the fire used for combustion.

For it is possible that our atmosphere holds in suspension all the elements, and that a thorough analysis would reveal traces of them in all the soils.

Analysis shows that all the dominant substances in the ash are contained in humus, and that its soluble part, which alone penetrates the plant, contains these substances in greater proportion than does **[p. 281]** the insoluble part. (See the table of analyses (Table 9.1), nos. 10 and 11, nos. 76 and 77, and nos. 78 and 79.) [*The pair 78/79 does not illustrate his point, because the table shows much more ash in the humus than in its extract. He notes, in the table, that the large amount of ash in this humus is due to sand and detritus of gneiss mixed with it.*] Their presence in the plant is therefore only natural; their absence would be more surprising.

The nature of the soil has, other things being equal, an appreciable influence on variations in the amount of ash in most plants. I gave (Chap. 8) details of an experiment in which I grew *Polygonum persicaria* with a saline solution, and other, similar plants with distilled water. The plants in the saline solution yielded, at equal weight, almost twice as much ash as the ones in distilled water. I allowed fava seeds to develop by three different methods. The first ones were protected from the rain and nourished with distilled water. One hundred parts, dry, of the plants that these seeds gave rise to contained, at flowering time, 3.9 parts of ash. Other, similar seeds were sown in glass capsules filled with gravel and placed on the ground under the open sky. They were watered, both naturally and artificially, with rain water. One hundred parts, dry, of these plants yielded, at flowering, 7-1/2 parts of ash. **[p. 282]** Finally, fava plants in flower, grown in open ground in a vegetable garden, yielded 12 parts of ash.

The proportion of the elements in the ash is almost always related to that of the elements composing the soil. Plants grown in a humus originating from a siliceous mountain yield, other things being equal, ash that contains less lime and more silica than plants grown in a carcareous humus. (See, in the table of analyses (Table 9.1), nos. 67 and 68, nos. 71 and 72, nos. 74 and 75.) The soil of le Bréven [*modern spelling "Brévent"*], the granitic mountain [*in the Alps*] on which I made the siliceous harvests, was much richer in iron oxide than was the soil of the calcareous mountain [*presumably la Salle, in the Jura; de Saussure (1800) was an analysis of the minerals in plants of various mountain soils*]. The same difference was seen in the ash. All the plants were harvested at the same season, but growth was a little retarded in the siliceous soil, and this circumstance tended, as I will show in the following, to decrease the proportion of silica and iron oxide in the ash of the plants that grew there.

The differences between the productions of calcareous and siliceous soils are detectable only insofar as the extractive substances that **[p. 283]** nourish the plants contain different amounts of silica and lime. Plants grown in a calcareous sand and in a granitic sand, amended by the same manure or by the same humus, would contain similar ash. One may judge by the experiment of Lampadius, recorded in the *Journal des Mines*, no. 65 [*actually, no. 55; see Appendix 3, Bibliography of References Cited by de Saussure (as expanded by the translator)*]. This author prepared five garden beds, each 4 [*square*] feet in surface area and 1 foot deep. They were separated by boards. Each square was filled with a very pure earth and 8 pounds of cow manure. These earths were alumina, silica, lime, magnesia, and garden humus. Rye was sown there, and its ash was found to contain the same substances in the five different earths. The author concludes from this that silica forms during plant growth, regardless of the nature of the soil in which the plant is grown. But as Lampadius did not analyze the ash of the cow manure, which alone nourished the rye, we cannot draw this conclusion. The only certain conclusion to which this experiment can lead is that earths, if not in a soluble state, do not penetrate plants. **[p. 284]** Furthermore, I will show in the following that the open atmosphere introduces a small amount of ash into plants.

I will now investigate why plants of different species, growing in the same soil, do not contain all their ash constituents in the same proportion, and why this ash also varies in the different parts of the same plant. The explanations that I can give on this subject are sometimes far from fully satisfactory. They often depend on a knowledge that I lack, namely, that of plant structure. But they are less absurd than those that ascribe to plants a creative force for all of its elements, and, if my reflections are incorrect, I hope at least to draw attention to new observations. I will examine separately the principal ash constituents.

9.4 Of the Alkaline Salts in Ash

First Observation The alkaline salts[4] are, without comparison, the most abundant constituent **[p. 285]** in the ash of an herbaceous green plant, all of whose parts are growing. I was very surprised to find that the ash of young goldenrod plants, no. 43, grown in a barren, uncultivated terrain, and the ash of fava no. 37, sunflower no. 46, wheat [“*froment*”] no. 51, and maize [*Zea mays*] no. 57, contained at least three-quarters of their weight as alkaline salts. [*The entries in the water-soluble-salts column are actually only 55–69 %, but adding in the major part of the “deficit” (see discussion in Sect. 9.11 G), which is listed in a separate column, they mostly reach three-quarters or more*.] We can say almost the same about the ash of tree leaves emerging from their buds: At least one-half and sometimes three-quarters of their weight consists of these salts. I found one-half in the ash of leaves of hazelnut and poplar trees on 26 May; and three-quarters in oak leaves on 10 May, before their full development. And all these trees grew in a soil that was uncultivated and of poor quality. This result should not be surprising, since we have seen that the ash of an extract of an uncultivated humus yielded at least half its weight as alkaline salts. (See Sect. 5.3, and the table of analyses (Table 9.1), nos. 76 and following.) [*He probably means, instead of no. 76, no. 77, with the alkaline salts that are included in the “more precise analysis” and the “deficit” columns added in*.] **[p. 286]** But even if this product composed much less of the extract, there could be much more in the plant, if, as we must presume, the alkaline salts are the finest, least viscous of all the fixed parts of the extract.

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[4] I adopt here the oldest accepted meaning, which confers the name of alkaline salts only on those in which potash, soda, or ammonia is one of the constituents. We must exclude only ammonia, because it cannot enter into the composition of ash.

I showed in Chap. 8 that the different substances that could be contained in the same solution did not enter the plant in the same proportion, but in inverse relation to their viscosity.

Second Observation The proportion of alkaline salts never increases appreciably, and most often decreases, as the plant develops and ages in the same soil. This observation applies not only to annual plants, but also to tree leaves during the course of a season. A plant emerging from the ground, a leaf emerging from its bud, contains ash that is as rich, and usually richer, in alkaline salts at this time than at any other stage of its growth. See, in the table of analyses, nos. 1 and following; nos. 12 and following; nos. 16, 18 and following; nos. 30 and following; nos. 35 and following; nos. 43 and following; 46 and following; 51 and following; 57 and following. The more advanced the growth, the greater is this removal of saline substances. **[p. 287]** A plant emerging from the ground in May loses less salt between May and July than between July and September.

When a fresh plant is washed with water (table of analyses, nos. 16 and 17), the liquid removes the alkaline salts in greater proportion than all the other constituents of the ash. The liquid removes less of the salts, or the plant retains them more tenaciously, the greater the plant's growth force or the less advanced its development. The water that circulates around the roots, the rain water that falls on the leaves, and indeed the water that the leaves transpire carry away the salts, as I have just said, and in greater proportion the more water the plant receives.

Third Observation The ash of bark contains a much smaller proportion of alkaline salts than does the ash of wood or sapwood. See no. 4, no. 7, no. 15, no. 21, no. 24, no. 28. [*These are all entries for barks, which are to be compared to entries for wood or sapwood listed nearby in the table.*] The bark renews itself only very slowly. It is exposed year-round to washing by rain and dew. It is provided with a dead substance, the epidermis or cork. More than any other part, it must be depleted of its **[p. 288]** most soluble salts. It does not seem, however, that this effect can be attributed solely to the causes I have indicated.

Fourth Observation The ash of fully formed wood is almost as rich in alkaline salts as the ash of the sapwood that is adherent to it. See, in the table of analyses, nos. 5 and 6, nos. 22 and 23, nos. 27 and 28. [*This last should be 26/27; also, pairs 5/6 and 26/27 show the fully formed wood having slightly more alkaline salts than the sapwood, even after the deficit is added back in.*]

This result is peculiar. It is contrary to the reduction of ash in the wood and to the other components of this same ash. [*But the table shows, for 5/6, 22/23, and 26/27, that some ash components are less abundant, some more abundant, in the sapwood than in the fully formed wood.*] The well formed woods in healthy, vigorous trees such as the woods that I burned are not a dead substance. They serve as channels or conduits for the sap, and it is likely that these channels, which are much narrower or denser than those of the sapwood, admit only the most fluid juices, such as those carrying saline substances, and that these same channels are an insurmountable obstacle for the other substances. Moreover, the wood is far removed from atmospheric influences, and loses through these influences almost none of the salts that it acquires.

It seems to me that, other things being equal, the harder a wood is, the richer it should be in saline substances, **[p. 289]** because the ducts of hard wood are narrower and because all their substance allows less rain water to penetrate. But the slow growth rate of trees having hard wood undoubtedly requires frequent modifications of this principle. If soft woods are more quickly depleted of the salts that they acquire, they receive, on the other hand, a greater quantity of salts in the same time due to their rapid absorption and growth.

Fifth Observation The ash of seeds is richer in alkaline salts than is the ash of the plant that bears them; see, in the table of analyses, nos. 29, 32, 34 [*compare these three as a group*], 40, 41 [*these are all right as a pair, if the "more precise analysis" in the far right column is taken into consideration*], 54, 55 [*these are all right as a pair*], 60, 61 [*no. 61 shows no value for any substance; he probably means no. 62 instead, but the total for alkaline salts in these seeds is less than in the stem, no. 60, whereas it should be more*], 63, and 64 [*these are all right as a pair if the deficit and "more precise analysis" are taken into consideration*]. Most seeds are enclosed in integuments of very low porosity, which protect them from atmospheric influences. They transpire little and should therefore retain their salts. Moreover, they are attached to the rest of the plant only by very fine, very slender channels, which admit only the most fluid juices. The famous Vauquelin made an observation contrary to the one I just reported. He observed (*Annales de Chimie*, Vol. 29), in comparing the ash of oat grains with the ash of a stack of grain-bearing oat plants **[p. 290]** accidentally burned at Ecouen [*a canton on the outskirts of Paris*], that the seed alone contained no alkaline salts, but that the whole plant did contain them. I will note, in general: (1) that the time of harvest introduces such large variations in the composition of the ash that the different parts of the same [*kind of*] plant cannot be compared unless they were collected at the same time and from the same soil; (2) that this chemist did not burn pure oat seed, but oats with its chaff, for I obtained, as he did, 3.1 parts of ash in 100 parts of unhulled oats, but I found only 1.7 parts of ash in 100 parts of oats stripped of its chaff or calyx. The chaff composes about one-third of the weight of unhulled seed, and 100 parts of this envelope contain about three times more ash than do 100 parts of naked seed. Thus, in the ash that Vauquelin examined, more than three-quarters of the weight was associated with the chaff.

I analyzed the ash of oat seeds bearing this envelope. This ash yielded almost no alkaline salts by a purely aqueous leaching, but when I dissolved the ash **[p. 291]** in nitric acid, separated the silica by a filter and the earthy phosphates by ammonia, and then subjected the remaining liquor to a heat-driven evaporation pushed by degrees to the highest temperatures, I obtained, as a residue, 15 parts of alkaline salts in 100 parts of ash of oats provided with its chaff.

The difficulty of obtaining enough naked oat seed, without spoilage, prevented me from making a precise analysis of its ash compared with that of its straw, but I have reason to believe that the results would confirm those that I obtained with other seeds, and would show that the ash of naked oat seed contains more than half its weight as alkaline salts. I have devoted more time to this discussion than it needs. The results of Vauquelin are no doubt completely accurate, but the timing of the harvests, or even the exposure of the seed to rain, are sufficient to explain the differences in our analyses.

The ash of seeds contains very nearly the same amount of alkaline salts as the ash of the plant that arises from them, in the same soil, at the time when the plant yields the most, that is, in the first stages of growth. **[p. 292]** I obtained only very slight differences in this regard, and these are largely attributable to the fact that my analyses were made on entire seeds. Their whole substance is not used as nutriment by plants. Only the inner part of the seed is used for this function. Now, I found that the bran, or the outer part of the wheat grain, contained a somewhat smaller amount of alkaline salts than the grain itself, and this bran was mixed with a certain amount of meal.

I went into great detail about the distribution of alkaline salts in plants because the distribution of the other solid substances in plants is almost entirely subordinate to it.

9.5 Of Earthy Phosphates in Ash

Earthy phosphates, namely, those of lime and magnesia, form a water-soluble combination in humus extracts, as only nature can produce. We do not know whether these salts are essential to the extract. Be that as it may, when I speak of the solubility of these phosphates in water, I mean **[p. 293]** only the solubility of the extractive part that contains them. It is the same with silica, lime, and the metallic oxides.

The earthy phosphates are, after the alkaline salts, the most abundant constituent of the ash of an herbaceous green plant all of whose parts are growing and developing. We must presume that, after the alkaline salts, they are the finest, least viscous part of the solid substance of the extract. These two kinds of salts almost always follow, except for their amount, and by the same causes, the same course in the ash.

When a plant is washed with water, the earthy phosphates are removed by this liquid in greater proportion than all the other constituents of the ash except for the alkaline salts. See, in the table of analyses, nos. 16 and 17.

A tree leaf contains ash that is richer in earthy phosphates when it emerges from its bud than at any later stage of growth. See, in the table of analyses, nos. 1, 2, etc. [*The pair 1/2 shows this pattern, but the immediately following ones are not relevant.*]

The proportion of earthy phosphates decreases in the ash of annual plants from the time of germination up to **[p. 294]** that of flowering. See nos. 37, 38, 43, 44, 46, 47, 51 and 52. But these phosphates seem to increase at the time of seed maturation, if the seeds are abundant. (See the numbers that follow all those I just cited.) This reverse course is an illusion produced by the effect of a change of substances that occurs during the process of incineration. The phosphates are decreased in the plant and increased in its ash. The ash of seeds is abundantly provided with phosphate of potash and lacking in carbonate of lime. The ash of the stems and leaves, at the time of fruit maturation, contains little or no phosphate of potash, and, on the other hand, some carbonate of lime. See analyses nos. 40, 41, 54, 55, 60, 62, 63 and 64. When the ashes of the stems and seeds are mixed during incineration, the free or carbonated lime decomposes the phosphate of potash, forming earthy phosphate. It changes an alkaline salt into an earthy salt, and thus makes the earthy salt seem more abundant in the ash than it is in the plant. This result is very striking in the analyses of the ashes of fava plants having mature seeds; of these plants **[p. 295]** without their seeds; and of the seeds themselves: nos. 39, 40, and 41.

The amount of earthy phosphate that the seed alone contains in greater proportion than the stem is not sufficient to explain the increase in earthy phosphate obtained by the incineration of the entire, seed-bearing plant, without adding the earthy phosphate that comes partly from the decomposition of the phosphate of potash contained in these same seeds.

The ash of bark contains a much smaller proportion of earthy phosphate than does the ash of sapwood. See, in the table of analyses, nos. 6, 7, 14, 15, 20, 21, 23, 24, 27 and 28. The reason for this is the same as for the alkaline salts (Sect. 9.4).

The ash of the sapwood contains more earthy phosphate than does the ash of the wood. This result is consistent with the reduction of ash in the wood (see the table of incinerations), but is contrary to the presence of alkaline salts in this same wood. I tried (Sect. 9.4) to give an explanation for this anomaly. I do not consider the stated reason as fully satisfactory, however. It is possible that the parts that stop growing lose, **[p. 296]** or allow the decomposition of, their phosphoric acid and retain only the phosphate earth.

The discovery of a large amount of phosphorus in seeds is very old. It dates to the time of Pott, and Marggraf confirmed it.[5] Vauquelin recognized that the stems of wheat and oats contained less phosphate than did the seeds. These results were confirmed by my observations on several other, very different, plants. This famous chemist, jointly with his colleague Fourcroy, withdrew magnesium ammonium phosphate [*"magnesian ammoniacal phosphate"*] from some seeds before burning them. The action of potash on this phosphate probably led to the formation of the phosphate of potash that my analyses revealed so abundantly in several ashes. Phosphate of potash is surely also produced by the action of potash on simple phosphates of lime and magnesia, as will be seen in detail in Sect. 9.11.1.

9.6 Of Free or Carbonated Lime in Ash [p. 297]

When I washed leaves with water (table of analyses, nos. 16 and 17), the proportion of carbonated lime in their ash increased. This result occurred only because of a larger withdrawal of the other fixed substances, namely, the alkaline salts and the phosphates. For this reason, it is understandable why, in all my analyses nos. 1, 2, 12, 13, 14, 16, 18, 35, 36, etc. [*no. 14 does not seem to belong in this list*], I found that the proportion of carbonate of lime increased as the plant grew. It is because the plant lost its salts and phosphates in much greater measure than the lime.

We only rarely find plants that, through a particular disposition, retain their salts in very nearly the same proportion during their growth. In these plants, carbonate of lime does not increase appreciably. This is the case in, for example, fava plants.

In herbaceous green plants all of whose parts are **[p. 298]** growing, carbonate of lime is present only in very small amounts. These plants contain barely more than ten or twelve hundredths of it, and I believe that ash composed of nearly equal parts

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[5] *Opuscules chimiques* of Marggraf, Vol. 1, p. 68.

carbonate of lime and alkaline salts would be an impossible product for one of these plants, at least on a natural site. This is because the alkaline salts that are present in a green plant are composed largely of alkaline phosphates, which are changed into earthy phosphates by incineration.

We will therefore not conclude from the fact that certain plants (such as the grasses before they are fully developed) yield no free or carbonated lime through incineration that the substance that yields this earth in other plants is not present in those with which we are concerned. It is very likely that it is.

The ash of barks contains a large amount of carbonate of lime, much more than the sapwood, because barks are depleted of their alkaline salts and earthy phosphates. See nos. 6, 7, 14, 15, 23, 24, 27 and 28.

The ash of well formed woods contains **[p. 299]** more carbonate of lime than does the ash of the sapwood because the formed woods are depleted of their phosphates.

The ash of most seeds, and even of all those that I examined, contains no carbonate of lime because the seeds are rich in phosphate of potash. It is possible that, due to the presence of this salt, plants that are heavily laden with seeds yield ash that is richer in carbonate of lime at the time of flowering than at fruit maturity.

The seeds of some *Lithospermum* produce a slight effervescence with acids. They contain carbonate of lime or magnesia before combustion, but it is possible that their ash does not produce these earths in a free or carbonated form.

9.7 Of Silica in Ash

Silica is never present in large amounts in ash unless the plants are depleted of their salts and phosphates. Large amonts of alkaline salts and of **[p. 300]** silica are incompatible in the composition of the ash of a young, herbaceous green plant, all of whose parts are growing.

When a fresh plant is washed with water, the proportion of silica in the ash increases. See table of analyses, nos. 16 and 17.

The ash of young plants, and of leaves emerging from their buds, contains very little silica. But the proportion of this earth increases as the plant develops and is depleted of its alkaline salts. The purest plant molds, exposed to atmospheric influences, are, for this reason, very rich in this earth.

The proportion of silica does not increase appreciably in plants, such as fava, that retain the same proportion of alkaline salts throughout their growth.

I sowed seeds of maize [*Zea mays*] and wheat [*"froment": Triticum sativum*] at the same time, in the same soil. When I examined these plants a month after germination, or a month before flowering, I found that all the visible parts of the maize were growing. Its ash **[p. 301]** then contained 8/100 silica and 80/100 alkaline salts. The radical leaves of the wheat at this time, however, had already become dry or yellow, although the plant was thriving. Its ash then contained 12/100 silica and 65/100 alkaline salts. At flowering time, or a month after the preceding observations, all parts of the maize were growing as before and it still yielded ash containing 8/100 silica and 80/100 alkaline salts. But the wheat, although vigorous, had an increased number of dried or yellowed leaves. Its ash then contained 32/100 silica and 54/100 alkaline salts. These observations show how, through very basic circumstances that vary according to the growth of the different plants, they must contain dissimilar ashes, even when the plants are assumed to absorb identical nourishment. But it is very likely that the nourishment is not the same in the same soil, and that the roots modify it when it enters, based on the width of their pore openings.

Most grasses are distinguished from other plants by a larger silica content. We may conclude from this that **[p. 302]** these grasses take up and lose much more. We must believe that they absorb a more abundant nourishment and that they deplete themselves of part of it. The plants that are richest in silica should, other things being equal, be the most exhausting [*of the soil*].

I will not say at present that these grasses differ by having root pores that are more open. It is possible, on the contrary, that their pores are narrower, since these plants, when young, contain less carbonate of lime and often more alkaline salts than other plants. It must be acknowledged only that they have a much greater suction force and much more abundant excretions. Indeed we see in many of these plants an appreciable, very unusual, transpiration (*Physiologie végétale* of Senebier, Vol. 4, p. 87).

Seeds lacking their external coats contain less silica than the leafy stem bearing them.

I do not pretend, however, to be able to explain everything. In the distribution of silica in trees, there are effects whose cause I do not understand. The trunks of trees **[p. 303]**, their bark, sapwood, and well formed wood often contain almost no silica, whereas the leaves are rich in it, especially in autumn. It is by the periodic dropping of leaves that trees cast off this earth. I examined barks of five different trees—those of poplar, young and old oak, hazelnut tree, mulberry, and hornbeam—and I found a substantial amount of silica only in the bark of mulberry. I found almost none in the well formed wood of any these trees. Their leaves contained a notable amount, four or five times more than the wood or bark.

9.8 Of Metallic Oxides in Ash

The proportion of oxides of iron and manganese in ash increases as growth advances. Tree leaves yield ash that is richer in these substances in autumn than in springtime. It is the same with annual plants. Seeds contain metals in smaller proportion than does the stem that bears them. When a plant is washed with water, **[p. 304]** the proportion of its metallic oxides increases. The purest humuses are always very rich in metallic oxides, much more so than the plants that formed them.

9.9 Influence of the Atmosphere on the Ash of Plants

We can determine whether the atmosphere introduces earths and salts into plants by germinating seeds with the aid of distilled water and seeing whether the weight of the ash of the plants that are developed in this way exceeds the weight of the ash that a preliminary experiment indicates in the seed.

Forty-one fava beans (*Vicia faba*), also known as marsh beans, weighing, together, 79.135 grams (2-1/2 ounces + 51 grains), were placed in the flared necks of 41 vials, each filled with 594 cubic centimeters (30 cubic inches) of distilled water. I exposed them to the open air, in sunlight, on an exterior window sill protected from rain. These plants reached a height of a foot and even more. **[p. 305]** Their stems were weak and could not support themselves without several fastenings. They flowered, but these flowers were small, imperfect, and almost unrecognizable. I ended the experiment after 2-1/2 months, during or immediately after flowering, because at this stage the plants had reached their greatest development and because their growth had become weak. The leaves and stem tips collapsed under their own weight.[6] They had absorbed 1.7835 decaliters (900 cubic inches) of distilled water. After the experiment, the vials contained 3.963 liters (200 cubic inches) of water rich in conferva [*a green alga*]. The liquid, after evaporation, left a dry residue weighing 5.8 decigrams (11 grains), which produced by combustion 1-1/2 grains of ash. This ash **[p. 306]** seemed to be composed of 3 parts lime, 1 part silica, and an imponderable amount of alkaline salts.

Weight of fava seeds before the experiment	79.135 grams	(2-1/2 ounces + 51 grains)
Approximate weight of the green plants that arose from the preceding seeds	611	(20 oz.)
Dry weight of the same plants	84.337	(2-3/4 oz. + 5 gr.)
Weight of the ash of the fava seeds before they developped	2.601	(49 gr.)
Weight of the ash of the plants that arose from the seeds	3.025	(58 gr.)
Difference, or weight of the ash deposited by the atmosphere	424 milligrams	(9 gr.)

The 49 grains of ash of the seeds, before the experiment, contained		The 58 grains of ash of the fava plants that developed in distilled water contained	58 grains of ash of flowering fava plants, grown in plant mold, contained
Potash	10.9	13	33.2
Potassium phosphate [*"phosphate of potash"*]	21.5	19.25	0
Chlorides [*"muriates"*] and alkaline sulfates	1.4	2.5	7
Earthy phosphates	13.68	17.5	8.7
Earthy carbonates	0	0	3
Silica	0	Imponderable amount	1.2
Metallic oxides	0.25	0.25	0.25

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[6] I would have been able to obtain much more vigorous growth, no different from that in open ground, at least up to the fruiting stage, if I had sown these seeds in pots full of pure sand or gravel, but I do not know whether the juices of the roots would have attacked the stones themselves. The erosion that lichens sometimes seem to cause on rocks suggests this. Moreover, the prodigious number of insects that, in open country, leave their remains on leaves, and the debris of all kinds that the winds transport there, would cast doubt on the results of the experiment.

[p. 307] From the foregoing, we see that the plants gained 424 milligrams (9 grains) [*conversion is a little off: 424 milligrams equal 8 Paris grains; 9 grains equal 477 milligrams*] of ash and that the composition of the ash of the entire plant that developed in distilled water differs very little from the products of the seed.

The atmosphere, I believe, deposited on these plants all the common constituents of ash, but principally lime. This earth decomposed a part of the phosphate of potash and formed phosphate of lime. During growth, a small amount of alkali was evaporated. The difference between the ash of the fava plants that developed in distilled water and the ash from plants grown in humus shows us how great is the influence of the soil on their composition.

I do not believe that we can conclude, from the small gain that we have just seen in plants grown in distilled water, that they themselves formed, with gases and water, these additional substances. If we consider how quickly a body of any kind exposed to the open air is covered with dust due to the immense number of corpuscles floating in our atmosphere, and if we note that the 41 fava plants **[p. 308]** presented to these corpuscles, for about 2 months, a stopping place of more than a square foot of surface area, we should be less surprised that this gain occurred than if it had not occurred. Its amount suggests nothing supernatural here. We note further that distilled water itself is never pure. It contains foreign substances[7] whose weight we cannot estimate because they escape with the water during evaporation.

9.10 Details on the Procedures used for Incineration

Annual plants were collected whole, including any dead leaves that they might have. I cut away only those roots **[p. 309]** that were still impregnated with soil. The lower parts of the stem were carefully freed of soil splatterings. When I collected annual plants having mature seeds, I did so at the precise moment of this maturity and not after the death of the plant. Tree leaves, even those of autumn, were always chosen green and in a perfectly healthy condition. Care was taken at this season, however, not to take the leaves of the most recent shoots, but of the oldest.

All these plants were sheltered from the rain immediately after they were collected. When they seemed dry, they were placed for several weeks in an oven heated to 20° Réaumur. Their green or soft parts mostly became friable and brittle there, and in this dry state the plants were weighed. Despite this precaution, however, the plants were not all brought to the same degree of dryness. The dryness that they can reach at the same temperature varies not only according to their species, but also within the same plant, depending on the stage of growth. It seemed to me that, in general, green plants collected **[p. 310]** when young could be dried more completely than

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[7] Karsten and Westrumb noted that water that is distilled with the greatest care and, if recently prepared, is not at all modified by reagents, undergoes, after exposure for a fortnight to light or 4 weeks to darkness, changes that have all the characteristics of fermentation products. It then somewhat clouds solutions of lead and silver. It gives slight indications of ammonia and, at other times, of acid. (*Kleine Physikalisch-chemische Abhandlungen von Westrumb. Zweites Heft* [*spelling of title corrected by translator*]).

plants harvested at a more advanced age. The latter often retain, at the temperature indicated above, a semi-flexibility, which is a certain sign of the presence of water in substances, such as those that I am concerned with, which become stiff or brittle at a higher temperature. The impossibility of estimating the amount of this water, liquid and external to the essential composition of the plant, must always put a little uncertainty into the products of plant analysis.

The plants dried by this method were burned on a large iron slab. The residue of this operation was incinerated anew in a crucible heated to a dark red, until no parts of the charcoal were susceptible to burning. There are plant substances, such as the straw and seed of wheat [*Triticum sativum*], that I could not incinerate completely or reduce to gray or white ash. But sugar, starch, gum, maize plants and their seeds, likewise those of fava, barley, and oats, could be reduced to white ash.

[p. 311] The more time devoted to the operation, and the gentler the heat of the furnace, the more complete the incineration. Care must be taken, especially in the always very difficult combustion of seeds, not to stir the ashes often. Such action, by crushing the salts and charcoal, by condensing it while it is somewhat fluid, prevents its combustion and sometimes makes it impossible. The yields of my incinerations are, in general, lower than those of the investigators who preceded me. But most of them, having had in mind only the large-scale extraction of salts, did not complete, in a crucible, the incineration of the residue of the first combustion in the open air.

According to Perthuis, plants yield more ash if they are burned when green than if they are burned when dry. But he probably reported this result so that plants would not be exposed, after harvest, to the effects of water [*presumably because water would deplete them of salts, which Perthuis sought to maximize*], for, green or dry, they provided me the same amount of ash.

All the products of my incinerations were weighed hot, upon removal from the crucible, and analyzed similarly.

9.11 Details on the Procedures used for Analyzing Ash [p. 312]

The goal I set myself was to understand the order in the composition of the ash, not only in different plants, but also in their different parts and according to their developmental stage. My lifetime would have been insufficient for this work had I obliged myself to make very detailed analyses. The procedures that I followed seemed precise enough for the general research that I had in mind. They consist of the following operations:

A. The ash, dried at a red heat and pulverized if it was agglutinated, was boiled with 20,000 times its weight of distilled water. The filtered lye was evaporated to dryness at a heat up to red hot. The weight of its residue indicated to me the weight of the water-soluble salts that the ash contained. It is this result that was recorded in the first column of the table, when a new solution of this residue, in a small amount of water, left no **[p. 313]** insoluble material. But, in the contrary case, the solution was filtered and evaporated anew, until the salts were completely dissolved. The part that became insoluble by evaporation is the carbonate of lime in ash, such as that of barks, which contains a large amount of this earth. It is formed by a phosphate of potash and lime or magnesia [*de Saussure's term was "phosphate potassé de chaux ou de magnésie*"] in the ash of grasses and seeds that contain no free or carbonated lime, but, on the other hand, a great deal of earthy phosphate and potash. Silica occurs only in insignificant amounts, at least if one uses, as I always tried to do, only the lowest degree of fire possible to produce incineration. My yields of alkaline salts are, in general, much larger than those obtained from the same plants by my predecessors. I can attribute this difference only to my more complete incinerations and to the care that I took in drying my plants at harvest. The water is far from removing all the alkaline salts from the ash, but the amount obtained is always very nearly proportional to that which the ash contains, if there is no phosphate of potash. **[p. 314]** When this salt was encountered in the ash, I almost always made a more detailed analysis, which I will discuss in the following [*see G, below*].

B. The washed, water-insoluble ash was boiled with seven or eight times its weight of rectified nitric acid in a porcelain capsule. The residue was treated, at a red heat, with six times its weight of powdered soda in a platinum crucible. The [*resulting*] glass [*presumably water glass, sodium silicate*] was dissolved in water and then mixed with acid. The solution was evaporated to dryness. The residue was put in digestion with acid and filtered. The insoluble product of this operation was, after being dried at a red heat, recorded under the term silica.

C. The nitric solution of ash was precipitated by potassium ferrocyanide [*"prussiate of potash"*] and filtered. The solution was then concentrated by evaporation and diluted with a small amount of water and acid. It was precipitated a second time by potassium ferrocyanide. The separation of the precipitates was accomplished by a double filter, and the difference between the incineration products of each single filter indicated the amount of metallic oxide contained in the ash, after subtraction of the weight of the metallic oxide **[p. 315]** in the potassium ferrocyanide.

D. The nitric solution, freed of metallic oxides and concentrated by evaporation, was precipitated by ammonia. The precipitate, dried at a red heat, indicated very nearly, after deducting the alumina from it, the weight of the earthy phosphates. By this term is meant the phosphates of lime and magnesia.

E. I dissolved these phosphates in nitric acid; they were precipitated by potash in excess; and the mixture was boiled. The filtered liquor was saturated by acid and precipitated by ammonia, which revealed the alumina. This earth was rectified by a new solution and precipitation. After being dried at a red heat and pulverized, it was put in digestion in vinegar to remove the earths that were combined with it. But the amount [*of alumina*] was extremely small, and often none, and in more than 40 ashes in which I looked for it, I found that it did not exceed 1/100 of their weight. I believed that I had found a large amount of it in the analyses I published several years ago in the *Journal de Physique* [*i.e., de Saussure (1800); see Translator's Introduction*], on the incineration of some plants **[p. 316]** grown in different soils. But I have since recognized that my error arose from the impurity of the ashes and the solubility of the earthy phosphates in potash. The same kinds of plants were burned again and analyzed more carefully.

F. The nitric solution D, freed of metallic oxides and earthy phosphates, was precipitated by crystallized carbonate of soda. The mixture was boiled for a long time and filtered after cooling. This operation separates the earthy carbonates, that is, those of lime and magnesia. I note the latter earth so as to omit nothing, for it seemed to me that pure or carbonated magnesia was never present in ash in more than a negligible amount. It is not the same with phosphate of magnesia, which was discovered in ash by Fourcroy and Vauquelin.

G. The column headed *deficit* in the table indicates the difference between the sum of the separate constituents and the amount placed in the experiment. This difference is very large and, at first glance, must cast an unfavorable light on the results. But I am satisfied that this deficit should not be **[p. 317]** apportioned among all the products of analysis. It concerns only the water-soluble alkaline salts, which, during incineration, formed with the earths and earthy phosphates a combination that could not be completely destroyed by decoction in water. This deficit must therefore be added to the salts recorded, deducting from it four or five hundredths for the unavoidable loss during manipulations, in order to find very nearly the absolute sum of the alkaline salts that the ash contains. I always found these lost salts when, after treating with nitric acid the ash washed in water, and after precipitating the acid solution by ammonia and carbonate of ammonia, I subjected the filtered liquor to evaporation at the highest heat in a platinum crucible. This evaporation requires, from the moment the salts begin to congeal, a very graduated fire and around 7 or 8 hours, so that the salts are not lost by explosions of boiling. If the fire has not been intense enough, the salts retain a little nitrous [*sic*] acid, but it can be reliably eliminated by adding a little charcoal dust while they are in fusion.

I followed these procedures [*he is referring to the following procedures rather than the preceding ones*] in those analyses **[p. 318]** recorded as more precise [*i.e., in the observations column of his table, in the boxes headed "more precise analysis of the same ash"*]. I did not always treat the ash with boiling water [*presumably he is referring to the initial treatment with boiling water, described in A*] but I dissolved it, with its salts, in nitric acid. The latter procedure is quicker and much more exact because, if the lyes contain phosphate of potash and lime, filtration or even precise decanting is almost impossible because of the viscosity[8] of the liquor. It holds in solution a large amount of lime or rather phosphate of lime, whose amount can be estimated only by dissolving the salts in an acid and precipitating them by ammonia. I will give here the procedures that I followed in analyzing salts rich in phosphate of potash and depleted of earthy phosphates. It is unnecessary to look for phosphate of potash in ash containing **[p. 319]** more than one or two hundredths pure or carbonated lime. Phosphate of potash is present in ash containing a larger proportion of lime, but in such small amount that it can be ignored.

a. I mix the salts with vinegar and evaporate almost to dryness. The residue is mixed with alcohol, which, in dissolving the acetate, becomes enriched with the greater part of the excess potash contained in the ash. This product is set aside.[9]

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[8] Viscosity of the lye of ash, and its reduction to gel by evaporation, is an almost certain indication of the presence of phosphate of potash and lime or magnesia. However, this salt may be present without this characteristic, if there is a large excess of potash. Phosphate of potash is not viscous or gelatinous by itself, whatever the proportions of its components. It has this property only when it contains lime or magnesia without excess potash.

[9] Perhaps it would be more exact to eliminate this first operation (a), which separates, by vinegar and alcohol, only a part of the excess potash and removes a little phosphate of potash. I used this

b. The alcohol-insoluble salts, which are phosphate of potash, the chlorides, the sulfates, and a little free potash, are dissolved in water and mixed with acetate of lime in excess. The latter decomposes the phosphate of potash. The mixture is boiled strongly and evaporated almost to dryness. The magma is diluted with a large amount of water, separated by a filter, and dried at a high heat.[10] **[p. 320]** It is mixed with vinegar (c) until this acid removes nothing more from it. The insoluble residue of this operation is pure phosphate of lime, resulting from the decomposition of the phosphate of potash. The pure phosphate of lime, augmented in the ratio of 100–129, gives the weight of the alkaline phosphate (d) contained in the ash without excess potash.

All the acetous solutions (a) and (c), including the wash water, are combined, evaporated at a red heat, and filtered. They are then evaporated again at a red heat. This residue (e) is weighed. It contains all the potash in the ash, including that which is a component of the potash of phosphate, and also the chlorides and alkaline sulfates. All of these salts are dissolved in nitric acid rather than in water because the potash contains a little carbonic acid, whose quantity is estimated from the weight loss of the mixture. This nitric solution is precipitated successively by barium nitrate and silver nitrate. The weight of each of these precipitates gives, by known calculations, **[p. 321]** the chlorides and alkaline sulfates. The sum of these last is subtracted from the weight (e), or from that comprising the potash, chlorides, and sulfates. The difference gives the weight of all the potash contained in the phosphate of potash and of the potash that was in excess. Now, we know the weight of the potash that is a component of the potash of phosphate, already given in (d). One hundred parts of this salt contain 65 parts of potash [*as explained in the first paragraph of the note below*]. Therefore, to this salt is attributed the potash belonging to it. The rest is the potash in excess that is contained in the ash.

9.11.1 Note on the Combination of Phosphate of Potash with Lime

I formed phosphate of potash by slowly combining, by the wet method [*i.e., using solvents, as opposed to the dry method, which uses fire*], phosphoric acid with potash, until the mixture was just neutralized and did not cause any change in the plant dyes used as reagents. I dried this compound at a red heat and weighed it. **[p. 322]** It was dissolved in water and decomposed by calcium chloride [*"calcareous muriate"*]. The mixture was evaporated to dryness, and, after being dissolved in water, was filtered. I deduced from the principles recognized by Klaproth for phosphate of lime

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procedure only because it makes the rest of the analysis less cumbersome and saves much acetic acid and calcium acetate.

[10] Vinegar dissolves phosphate of lime a little, if it has not been dried after its precipitation.

that 100 parts of dry phosphate of potash contain, at the degree of saturation indicated above:

Potash	65
Phosphoric acid	35

It is a phosphate of potash thus constituted that is referred to in the analyses of ash. It is such that 129 parts of this salt are capable of forming only 100 parts of phosphate of lime. [*Note: See (c) and (d) under G., in Sect. 9.11, above*].

Aqueous solutions of phosphate of potash are not, as has been said, viscous and gelatinous. Even concentrated, they pass freely through the finest filters.

If this solution is mixed with 20 or 30 times its volume of limewater, the mixture, which forms a soluble phosphate of potash and lime, retains all its transparency. It undergoes no apparent change other than becoming viscous and passing only extremely slowly through ordinary filters. **[p. 323]** If more limewater is added to it, a precipitate begins to form. This is not phosphate of lime, it is a phosphate of potash and lime that has become insoluble in water, through an excess of earth. When we dissolve it in nitric acid, we withdraw from it only a quarter or a half of its weight as phosphate of lime. Very nearly the same effects occur with magnesia, but the compounds of this seemed less viscous to me.

The same, but less pronounced, results can be obtained by substituting a solution of calcium acetate [*"calcareous acetate"*] for the limewater. If the solution is dilute, the first drops do not decompose the phosphate of potash, or at least the precipitate that is formed is redissolved by stirring the liquor. This observation shows that the calcium acetate found so abundantly by Vauquelin in plant saps can exist there with the phosphate of potash, which is completely decomposed by calcareous salts of any kind only by evaporating the mixture to dryness. I say the same thing for magnesia.

The aqueous solution of phosphate of potash and lime is not clouded by potash, soda, ammonia, **[p. 324]** or phosphoric acid, but it is partially decomposed by all the alkaline carbonates, which precipitate carbonate of lime from it. It is also partially decomposed by oxalic acid. Finally, it is partially decomposed by simple drying at a red heat. In this residue is found phosphate of lime, which has become insoluble in water through an excess of earth, and phosphate of potash and lime, which is soluble in water but which contains less lime than in the first solution. This effect occurs more or less in all the lyes of ash that are evaporated to dryness and redissolved in water.

Action of Potash on Phosphate of Lime Fourcroy and Vauquelin saw clearly (*Annales de Chimie*, Year 11 [*i.e., 1803*]) that when liquid potash is boiled with phosphate of lime, a very small amount of lime is deposited. But this decomposition, which is contrary to the old order of affinities, of which Berthollet provided so many examples in other, analogous compounds, is not the only noteworthy result in this experiment. I observed that a very large part of the phosphate of lime is completely dissolved in the potash, and that a phosphated potash of lime [*de Saussure's term is "potasse* *phosphatée de chaux*"] then forms. **[p. 325]** I boiled, for an hour, 300 parts of potash (dissolved in about twice its weight of water) with phosphate of lime in paste form and recently precipitated by ammonia. Before the experiment, the phosphate, dried at a red heat, weighed 25 parts. The decoction was filtered. The residue that was undissolved by the potash weighed no more than 9 parts. It was dissolved in nitric acid and precipitated by ammonia. The precipitate, dried at a red heat, weighed only 6 parts. The filtered solution was mixed with carbonate of soda or ammonia, which separated from it 1/2 part of carbonate of lime. In this experiment, the potash thus dissolved 3/4 of the phosphate of lime, and really decomposed only 1/50 of it.

I obtained almost exactly the same results by treating, with a melting fire in a platinum crucible, dry phosphate of lime with dry potash. The phosphate weighed 20 parts, and the potash 80. The opaque glass that resulted from this mixture was dissolved in water and filtered. The undissolved residue in this fluid weighed 12 parts, which were dissolved in nitric acid and precipitated by ammonia. The ammonia separated **[p. 326]** only 6-1/2 parts of phosphate of lime from it. The filtered solution was precipitated by carbonate of soda, which separated 2-1/2 parts of carbonate of lime from it.

The compounds of potash in excess with phosphate of lime that were obtained by the procedures I have just described differ in some properties from the compound obtained by mixing limewater with phosphate of potash.

These properties show that the potash in excess is not inessential and acts on the lime by a very strong affinity. I have said that the aqueous solution of phosphate of potash and lime was decomposed by oxalates. But the phosphated potash of lime is not clouded by these reagents. They reveal lime there only if the potash that appears in excess has been saturated very precisely with acid. It still requires that the oxalic acid precipitate all the lime, even in this circumstance. A quadruple combination is probably formed here [*i.e., lime, potash, phosphate, and oxalate*].

Liquid phosphate of potash and lime, or a mixture of limewater with phosphate of potash, is partly decomposed by drying, but liquid phosphated potash of lime is not significantly changed **[p. 327]** by this operation. The phosphate of potash and lime forms, with water, a viscous or gelatinous solution, which does not pass easily through a filter. The phosphated potash of lime passes very freely through a filter and does not form a gel by concentration.

Table 9.1 Tables of Incinerations and Analyses

Table of Incinerations

Incineration number	Name of plants, time of their collection	Ash from 1000 parts of the plant green	Ash from 1000 parts of the plant dry	Water of vegetation in 1000 parts of the plant green	Observations
1	Leaves of oak (*Quercus robur*), May 10	13	53	745	In a forest; soil gravelly and nearly sterile
2	The same, Sept. 27	24	55	549	*Ibidem*.
3	Debarked stems or branches of young oak, May 10		4		*Ibidem*. These stems or branches were about 1 centimeter (5-6 lines) in diameter
4	Bark of the preceding branches		60		This bark includes the bast and epidermis
5	Oak wood, separated from sapwood		2		This was part of a trunk that was about 2 dm (8 inches) in diameter
6	Sapwood of the preceding oak wood		4		
7	Bark of the preceding oak trunks		60		This bark includes the bast and epidermis

Table 9.1 (continued)

Table of Analyses

Constituents of 100 parts of the ash

Analysis No., corresponding to incineration No.	Water-soluble salts [alkaline salts]	Earthy phosphates [phosphates of lime or magnesia]	Earthy carbonates [carbonates of lime or magnesia]	Silica	Alumina	Metallic Oxides	Deficit	Observations
1. Oak leaves, May	47	24	0.12	3	Less than 1/100 of the weight of the ash	0.64	25.24	In this analysis and the following ones, the deficit is attributable almost entirely to water-soluble salts
2. The same, Sept.	17	18.25	23	14.5		1.75	25.5	
3. Debarked stems or branches of young oaks, May	26	28.5	12.25	0.12		1	32.58	
4. Bark of the preceding branches	7	4.5	63.25	0.25		1.75	22.75	
5. Oak wood, separated from sapwood	38.6	4.5	32	2		2.25	20.65	
6. Sapwood of the same oak wood	32	24	11	7.5		2	23.5	The silica may have been accidental in this sapwood, for I did not find it in young branches of oak
7. Bark of the preceding oak trunks	7	3	66	1.5		2	21.5	

Table 9.1 (continued)

Table of Incinerations

Incineration number	Name of plants, time of their collection	Ash from 1000 parts of the plant green	Ash from 1000 parts of the plant dry	Water of vegetation in 1000 parts of the plant green	Observations
8	Bast of the preceding bark		73		
9	Extract of the wood of preceding oak		61		
10	Humus of oak wood		41		
11	Extract of the preceding humus of oak wood		111		
12	Leaves of poplar (P*opulus nigra*), May 26	23	66	652	In a meadow; clay soil
13	The same, Sept. 12	41	93	565	On the same branch as the preceding leaves

Table 9.1 (continued)

Table of Analyses								
Constituents of 100 parts of the ash								
Analysis No., corresponding to incineration No.	Water-soluble salts [alkaline salts]	Earthy phosphates [phosphates of lime or magnesia]	Earthy carbonates [carbonates of lime or magnesia]	Silica	Alumina	Metallic Oxides	Deficit	Observations
8. Bast of the preceding bark	7	3.75	65	0.5		1	22.75	
9. Extract of the wood of the preceding oak	51				Less than 1/100 of the weight of the ash			Sawdust of oak, boiled for a half-hour in distilled water: the filtered decoction was evaporated to dryness at a gentle heat
10. Humus of the preceding wood	24	10.5	10	32	1	14	8.5	This nearly-black humus was taken several feet above the ground, from the trunk of a growing oak; it contained small, white lumps of silica
11. Extract of the humus of oak wood	66							The humus was boiled for a half-hour in distilled water: the filtered decoction was evaporated to dryness, at a gentle heat
12. Poplar leaves, May	36	13	29	5		1.25	15.75	In this analysis and the following ones, the deficit is attributable almost entirely to water-soluble salts
13. The same, Sept.	26	7	36	11.5		1.5	18	

Table 9.1 (continued)

Table of Incinerations

Incineration number	Name of plants, time of their collection	Ash from 1000 parts of the plant green	Ash from 1000 parts of the plant dry	Water of vegetation in 1000 parts of the plant green	Observations
14	Debarked trunks of the preceding poplars, Sept. 12		8		In the same soil. Trunk 2 dm (8 inches) in diameter
15	Bark of the preceding trunks		72		This bark includes the bast and epidermis
16	Leaves of hazel (C*orylas avellana*), May 1		61		On the uncultivated edge of a woods. Soil gravelly and almost sterile
17	The same, washed with cold, distilled water		57		
18	Hazel leaves, June 22	28	62	655	*Ibidem*.
19	The same, Sept. 20	31	70	557	*Ibidem*.
20	Debarked branches of the preceding hazel, May 1		5		These branches were 1 centimeter. (4 lines) at their greatest diameter
21	Bark of the preceding branches		62		

Table 9.1 (continued)

Table of Analyses

Constituents of 100 parts of the ash

Analysis No., corresponding to incineration No.	Water-soluble salts [alkaline salts]	Earthy phosphates [phosphates of lime or magnesia]	Earthy carbonates [carbonates of lime or magnesia]	Silica	Alumina	Metallic Oxides	Deficit	Observations
14. Debarked poplar trunks	26	16.75	27	3.3		1.5	24.5	
15. Bark of the preceding trunks. May	6	5.3	60	4		1.5	23.2	
16. Hazel leaves, May	26	23.3	22	2.5	Less than 1/100 of the weight of the ash	1.5	24.7	In this analysis and the following, the deficit is attributable almost entirely to water-soluble salts
17. The same, washed	8.2	19.5	44.1	4		2	22.2	These fresh leaves were immersed eight times in cold, distilled water; they remained under water 1/4 hour at each immersion
18. Hazel leaves, June	22.7	14	29	11.3		1.5	21.5	
19. The same, Sept.	11	12	36	22		2	17	
20. Debarked hazel branches	24.5	35	8	0.25		0.12	32.2	
21. Bark of the preceding branches	12.5	5.5	54	0.25		1.75	26	

Table 9.1 (continued)

Table of Incinerations

Incineration number	Name of plants, time of their collection	Ash from 1000 parts of the plant green	Ash from 1000 parts of the plant dry	Water of vegetation in 1000 parts of the plant green	Observations
22	Wood of so-called Spanish mulberry (*Morus nigra*) separated from sapwood, November		7		In a vegetable garden, clay soil. Trunk 2 dm (8 inches) in diameter
23	Sapwood of the preceding mulberry		13		
24	Bark of the preceding mulberry		89		This bark included the bast and epidermis
25	Bast of preceding bark		88		
26	Wood of hornbeam (*Carpinus betulus*) separated from sapwood, November	4	6	346	In a meadow, clay soil. Trunk 1.6 dm (6 inches) in diameter
27	Sapwood of the preceding hornbeam	4	7	390	In this tree, the sapwood was very indistinct from the wood
28	Bark of the preceding hornbeam	88	134	346	This bark included the bast and epidermis

Table 9.1 (continued)

Table of Analyses

Constituents of 100 parts of the ash

Analysis No., corresponding to incineration No.	Water-soluble salts [alkaline salts]	Earthy phosphates [phosphates of lime or magnesia]	Earthy carbonates [carbonates of lime or magnesia]	Silica	Alumina	Metallic Oxides	Deficit	Observations
22. Mulberry wood, separated from sapwood	21	2.25	56	0.12		0.25	20.38	
23. Sapwood of the preceding mulberry	26	27.25	24	1		0.25	21.5	
24. Bark of the preceding mulberry	7	8.5	45	15.25		1.12	23.13	
25. Bast of preceding bark	10	16.5	48	0.12	Less than 1/100 of the weight of the ash	1	24.38	In this and the following analyses, the deficit is attributable almost entirely to water-soluble salts
26. Wood of hornbeam, separated from the sapwood	22	23	26	0.12		2.25	26.63	
27. Sapwood of the preceding hornbeam	18	36	15	1		1	29	
28. Bark of the preceding hornbeam	4.5	4.5	59	1.5		0.12	30.38	

Table 9.1 (continued)

Table of Incinerations					
Incineration number	Name of plants, time of their collection	Ash from 1000 parts of the plant green	Ash from 1000 parts of the plant dry	Water of vegetation in 1000 parts of the plant green	Observations
29	Trunks and leafless branches of horse chestnut (A*esculus hypocastanum*), May 10		35		In a meadow, fertile soil
30	Leaves of horse chestnut, May 10	16	72	782	*Ibidem.*
31	The same, July 23	29	84	652	On the same branch
32	The same, Sept. 27	31	86	636	On the same branch
33	Flowers of the preceding horse chestnut May 10	9	71	873	On the same branch

Table 9.1 (continued)

Table of Analyses

Constituents of 100 parts of the ash

Analysis No., corresponding to incineration No.	Water-soluble salts [alkaline salts]	Earthy phosphates [phosphates of lime or magnesia]	Earthy carbonates [carbonates of lime or magnesia]	Silica	Alumina	Metallic Oxides	Deficit	Observations
29. Trunks and leafless branches of horse chestnut	9.5							
30. Leaves of horse chestnut, May	50							
31. Leaves of horse chestnut, July	24							
32. The same, September.	13.5							
33. Flowers of horse chestnut, May	50				Less than 1/100 of the weight of the ash			

Table 9.1 (continued)

Table of Incinerations

Incineration number	Name of plants, time of their collection	Ash from 1000 parts of the plant green	Ash from 1000 parts of the plant dry	Water of vegetation in 1000 parts of the plant green	Observations
34	Ripe fruits of the same horse chestnut, Oct. 5	12	34	647	On the same branch
35	Pea (Pi*sum sativum*) plants in flower		95		In a vegetable garden, clay soil
36	The same, bearing mature seed		81		In the same garden bed as the preceding
37	Fava (marsh) bean (*Vicia faba*) plants before flowering, May 23	16	150	895	In a vegetable garden, clay soil
38	The same, during flowering, June 23	20	122	876	In the same garden bed as the preceding

Table 9.1 (continued)

Table of Analyses

Constituents of 100 parts of the ash

Analysis No., corresponding to incineration No.	Water-soluble salts [alkaline salts]	Earthy phosphates [phosphates of lime or magnesia]	Earthy carbonates [carbonates of lime or magnesia]	Silica	Alumina	Metallic Oxides	Deficit	Observations
34. Ripe fruits of the same horse chestnut	75	10.5		0.75		0.5	13.25	*More precise analysis of the same ash:* Potash 51 Phosphate of potash 28 Chlorides & alk. sulfate 3 Earthy phosphates 12 Earthy carbonates 0 Silica 0.5 Metallic oxides 0.25 Loss 5.25 100
35. Pea plants in flower	49.8	17.25	6	2.3		1	24.65	
36. Pea plants bearing mature seeds	34.25	22	14	11		2.5	17.25	
37. Fava plants, before flowering	55.5	14.5	3.5	1.5		0.5	24.50	
38. The same, during flowering	55.5	13.5	4.12	1.5	Less than 1/100 of the weight of the ash	0.5	24.38	*More precise analysis of the same ash:* Incompletely carbonated potash 57.25 Chloride and sulfate, alkaline 12 Earthy phosphates 15 Earthy carbonates 5 Silica 2 Metallic oxides 0.5 Loss 8.25 100

Table 9.1 (continued)

Table of Incinerations					
Incineration number	Name of plants, time of their collection	Ash from 1000 parts of the plant green	Ash from 1000 parts of the plant dry	Water of vegetation in 1000 parts of the plant green	Observations
39	The same, bearing mature seeds, July 23		66		*Ibidem.*
40	The same, separated from the mature seeds		115		*Ibidem.*
41	Seeds of the preceding plants		33		*Ibidem.*

Table 9.1 (continued)

Table of Analyses

Constituents of 100 parts of the ash

Analysis No., corresponding to incineration No.	Water-soluble salts [alkaline salts]	Earthy phosphates [phosphates of lime or magnesia]	Earthy carbonates [carbonates of lime or magnesia]	Silica	Alumina	Metallic Oxides	Deficit	Observations	
39. The same, bearing mature seeds	50	17.75	4	1.75		0.5	26	The water-soluble salts do not contain phosphate of potash	
40. The same, separated from the mature seeds	42	5.75	36	1.75		1	12.9	*More precise analysis of the same ash:*	
								Incompletely carbonized potash	31
								Chloride of potash	14
								Sulfate of potash	2
								Earthy phosphates	6
								Earthy carbonates	37.5
								Silica	2.75
								Metallic oxides	0.75
								Loss	6
									100
41. Seeds of the preceding plants					Less than 1/100 of the weight of the ash			*Analysis:*	
								Potash	22.45
								Phosphate of potash	43.93
								Chloride of potash	0.9
								Sulfate of potash	2
								Earthy phosphates	27.92
								Earthy carbonates	0
								Silica	0
								Metallic oxides	0.50
								Loss	2.30
									100

Table 9.1 (continued)

Table of Incinerations

Incineration number	Name of plants, time of their collection	Ash from 1000 parts of the plant green	Ash from 1000 parts of the plant dry	Water of vegetation in 1000 parts of the plant green	Observations
42	Blooming fava plants grown in distilled water and arising from the preceding seeds		39		
43	Goldenrod *(Solydago vulgaris)*, before flowering, May 1		92		On the uncultivated edge of a woods; gravelly soil
44	The same, ready to flower, July 15		57		*Ibidem*.
45	The same, bearing mature seeds, Sept. 20		50		*Ibidem*.

Table 9.1 (continued)

Table of Analyses

Constituents of 100 parts of the ash

Analysis No., corresponding to incineration No.	Water-soluble salts [alkaline salts]	Earthy phosphates [phosphates of lime or magnesia]	Earthy carbonates [carbonates of lime or magnesia]	Silica	Alumina	Metallic Oxides	Deficit	Observations	
42. Blooming fava plants grown in distilled water and arising from the preceding seeds								*100 parts of ash contain:*	
								Potash	22.4
								Phosphate of potash	33.4
								Chlorides and alkaline sulfates	4.3
								Earthy phosphates	30
								Earthy carbonates	0
								Silica	inapprec. quant
								Metallic oxides	0.5
								Loss	9.4
									100
43. Goldenrod, in May	67.5	10.75	1.25	1.5		0.75	18.25	These plants had only their radicle leaves	
44. The same, in July	59	8.5	9.25	1.5	Less than 1/100 of the weight of the ash	0.75	21	In this and the following analyses, the deficit is attributable almost entirely to water-soluble salts	
45. The same, bearing mature seeds	48	11	17.25	3.5		1.5	18.75		

Table 9.1 (continued)

Table of Incinerations

Incineration number	Name of plants, time of their collection	Ash from 1000 parts of the plant green	Ash from 1000 parts of the plant dry	Water of vegetation in 1000 parts of the plant green	Observations
46	Sunflower (*Helianthus annuus*) plants, June 23, a month before flowering		147		In a vegetable garden, clay soil
47	The same, beginning to flower, July 23	13	137	877	*Ibidem*.
48	The same, Sept. 20, bearing mature seeds	23	93	753	*Ibidem*.
49	Blooming wheat plants				In a fertile field; gravelly soil
50	The same, bearing mature seeds				*Ibidem*.
51	Wheat (*Triticum sativum*) plants, May 1, a month before flowering		79		*Ibidem*.
52	The same, in flower, June 14	16	54	699	*Ibidem.*
53	The same, July 28, bearing mature seeds		33		*Ibidem*.

Table 9.1 (continued)

Table of Analyses

Constituents of 100 parts of the ash

Analysis No., corresponding to incineration No.	Water-soluble salts [alkaline salts]	Earthy phosphates [phosphates of lime or magnesia]	Earthy carbonates [carbonates of lime or magnesia]	Silica	Alumina	Metallic Oxides	Deficit	Observations
46. Sunflower plants, a month before flowering	63	6.7	11.56	1.5		0.12	16.67	
47. The same, beginning to flower	61	6	12.5	1.5		0.12	18.78	
48. The same, bearing mature seeds	51.5	22.5	4	3.75		0.5	17.75	
49. Blooming wheat plants	43.25	12.75	0.25	32		0.5	12.25	Year 1802
50. The same, bearing mature seeds	11	15	0.25	54		1	18.75	The same year, seeds abundant and of the finest quality
51. Wheat plants a month before flowering	60	11.5	0.25	12.5	Less than 1/100 of the weight of the ash	0.25	15.5	Year 1803, in the same soil as the preceding
52. The same, in flower	41	10.75	0.25	26		0.5	21.5	*Ibidem*
53. The same, bearing mature seeds	10	11.75	0.25	51		0.75	23	Seed compressed and not abundant

Table 9.1 (continued)

Table of Incinerations					
Incineration number	Name of plants, time of their collection	Ash from 1000 parts of the plant green	Ash from 1000 parts of the plant dry	Water of vegetation in 1000 parts of the plant green	Observations
54	Straw of the preceding wheat, separated from the seeds		43		
55	Wheat seeds selected from the preceding		13		

Table 9.1 (continued)

Table of Analyses

Constituents of 100 parts of the ash

Analysis No., corresponding to incineration No.	Water-soluble salts [alkaline salts]	Earthy phosphates [phosphates of lime or magnesia]	Earthy carbonates [carbonates of lime or magnesia]	Silica	Alumina	Metallic Oxides	Deficit	Observations	
54. Straw of the preceding wheat, separated from the seeds	9	5	1	61.5		1	22.5	*More precise analysis of the same ash:*	
								Potash	12.5
								Phosphate of potash	5
								Chloride of potash	3
								Sulfate of potash	2
								Earthy phosphates	6.2
								Earthy carbonates	1
								Metallic oxides	r.
								Loss	7.8
									100
55. Wheat seed, selected from that borne by the preceding straw	21	38	0	0.5	Less than 1/100 of the weight of the ash	0.25	40.25	*More precise analysis of the same ash:*	
								Potash	15
								Phosphate of potash	32
								Chloride of potash	0.16
								Sulfate of potash..impond. cloud	
								Earthy phosphates	44.5
								Earthy carbonates	0
								Silica	0.5
								Metallic oxides	0.25
								Loss	7.59
									100

Table 9.1 (continued)

Table of Incinerations

Incineration number	Name of plants, time of their collection	Ash from 1000 parts of the plant green	Ash from 1000 parts of the plant dry	Water of vegetation in 1000 parts of the plant green	Observations
56	Bran		52		
57	Maize (*Zea mays*) plants, June 23, a month before flowering		122		In a vegetable garden; clay soil
58	The same, in flower, July 23		81		*Ibidem*.
59	The same, bearing mature seeds		46		*Ibidem*.

Table 9.1 (continued)

Table of Analyses

Constituents of 100 parts of the ash

Analysis No., corresponding to incineration No.	Water-soluble salts [alkaline salts]	Earthy phosphates [phosphates of lime or magnesia]	Earthy carbonates [carbonates of lime or magnesia]	Silica	Alumina	Metallic Oxides	Deficit	Observations	
56. Bran								Potash Phosphate of potash Chloride of potash Sulfate of potash Earthy phosphates Earthy carbonates Silica Metallic oxides Loss	14 30 0.16 0 46.5 0 0.5 0.25 8.59 100
57. Maize plants, before flowering	69	5.75	0.25	7.5		0.25	17.25		
58. The same, in flower	69	6	0.25	7.5		0.25	17		
59. The same, bearing mature seeds					Less than 1/100 of the weight of the ash				

Table 9.1 (continued)

Table of Incinerations

Incineration number	Name of plants, time of their collection	Ash from 1000 parts of the plant green	Ash from 1000 parts of the plant dry	Water of vegetation in 1000 parts of the plant green	Observations
60	Stems of the preceding maize plants,separated from their ripe ears		84		
61	Ears of the preceding stems		16		
62	Seeds of the preceding maize		10		

Table 9.1 (continued)

Table of Analyses

Constituents of 100 parts of the ash

Analysis No., corresponding to incineration No.	Water-soluble salts [alkaline salts]	Earthy phosphates [phosphates of lime or magnesia]	Earthy carbonates [carbonates of lime or magnesia]	Silica	Alumina	Metallic Oxides	Deficit	Observations
60. Stems of the preceding maize plants, separated from their ripe ears	56	5	1	18		0.5	19.5	*More precise analysis of the same ash:* Potash 59 Phosphate of potash 9.7 Chloride of potash 2.5 Sulfate of potash 1.25 Earthy phosphates 5 Earthy carbonates 1 Silica 18 Metallic oxides 0.5 Loss 3.05 100
61. Ears of the preceding stems								
62. Seeds of the preceding maize	24	34	0	1		0.12	40.88	*More precise analysis of the same ash:* Potash 14 Phosphate of potash 47.5 Chloride of potash 0.25 Sulfate of potash 0.25 Earthy phosphates 36 Earthy carbonates 0 Silica 1 Metallic oxides 0.12 Loss 0.88 100

Table 9.1 (continued)

Table of Incinerations

Incineration number	Name of plants, time of their collection	Ash from 1000 parts of the plant green	Ash from 1000 parts of the plant dry	Water of vegetation in 1000 parts of the plant green	Observations
63	Straw of barley (*Hordeum vulgare*), separated from its mature seeds		42		In a field; calcareous soil
64	Barley seeds of the preceding straw		18		This seed was such as is used for sowing, i.e., provided with its inner husk
65	Barley seeds				

Table 9.1 (continued)

Table of Analyses

Constituents of 100 parts of the ash

Analysis No., corresponding to incineration No.	Water-soluble salts [alkaline salts]	Earthy phosphates [phosphates of lime or magnesia]	Earthy carbonates [carbonates of lime or magnesia]	Silica	Alumina	Metallic Oxides	Deficit	Observations
63. Straw of barley, separated from its mature seeds	14	7	12.5	57	Less than 1/100 of the weight of the ash	0.5	9	Potash 16 Sulfate of potash 3.5 Chloride of potash 0.5 Earthy phosphates 7.75 Earthy carbonates 12.5 Silica 57 Metallic oxides 0.5 Loss 2.25 100
64. Barley seeds of the preceding straw	7	31	0	36		0.25	25.75	*More precise analysis of the same ash:* Potash 18 Phosphate of potash 9.2 Sulfate of potash 1.5 Chloride of potash 0.25 Earthy phosphates 32.5 Earthy carbonates 0 Silica 35.5 Metallic oxides 0.25 Loss 2.8 100
65. Barley seeds	22	22	0	21		0.12	29.88	These seeds, although capable of germinating, were collected a fortnight before full maturity. I found, in looking for the [water-soluble] salts remaining in the acid solution, that they, along with the 22 parts that are listed, weighed 47 parts. Both in this analysis and the preceding one, a large part of the silica must be attributed to the husk, from which the seed was not entirely freed.

Table 9.1 (continued)

Table of Incinerations					
Incineration number	Name of plants, time of their collection	Ash from 1000 parts of the plant green	Ash from 1000 parts of the plant dry	Water of vegetation in 1000 parts of the plant green	Observations
66	Oats		31		This seed was provided with its husk
67	Leaves of rosage (*Rhododendron ferrugineum*), grown on the Jura, calcareous mountain. June 20		30		
68	The same, grown on the Bréven, granitic mountain. June 27		25		
69	Stems and branches of rosage grown on the Jura. June 20		8		These stems, and the following ones, were stripped of leaves
70	Stems of rosage grown on the Bréven. June 27				

Table 9.1 (continued)

Table of Analyses

Constituents of 100 parts of the ash

Analysis No., corresponding to incineration No.	Water-soluble salts [alkaline salts]	Earthy phosphates [phosphates of lime or magnesia]	Earthy carbonates [carbonates of lime or magnesia]	Silica	Alumina	Metallic Oxides	Deficit	Observations
66. Oats	1	24	0	60		0.25	14.75	By a more precise analysis, I found, besides the same products, 10 parts of potash and 5 parts of chloride and alkaline sulfate in the same ash
67. Rosage leaves, calcareous. June 20	23	14	43.25	0.75	0.12	3.25	15.63	In this and subsequent analyses, the deficit is attributable almost entirely to water-soluble salts
68. Rosage leaves, siliceous. June 27	21.1	16.75	16.75	2	0.12	5.75	31.53	Growth was more retarded in the siliceous than in the calcareous soil. This observation also applies to all the following harvests
69. Rosage stems, calcareous. No. 67. June	22.5	10	39	0.5	0.12	5.4	22.48	
70. Rosage stems, siliceous. June	24	11.5	29	1		11	24.5	

Table 9.1 (continued)

Table of Incinerations

Incineration number	Name of plants, time of their collection	Ash from 1000 parts of the plant green	Ash from 1000 parts of the plant dry	Water of vegetation in 1000 parts of the plant green	Observations
71	Needles of spruce (*Pinus abies*) grown on the Jura. June 20		29		
72	The same, grown on the Bréven, June 27		29		
73	Spruce branches stripped of needles, June 20		15		
74	Bilberry *(Vaccinium myrtillas)* grown on the Jura. August 29		26		
75	The same, grown on the Bréven. August 20		22		

Table 9.1 (continued)

Table of Analyses

Constituents of 100 parts of the ash

Analysis No., corresponding to incineration No.	Water-soluble salts [alkaline salts]	Earthy phosphates [phosphates of lime or magnesia]	Earthy carbonates [carbonates of lime or magnesia]	Silica	Alumina	Metallic Oxides	Deficit	Observations
71. Spruce needles, calcareous. June	16	12.27	43.5	2.5		1.6	24.13	
72. Spruce needles, siliceous. June	15	12	29	19		5.5	19.5	In this analysis and the following ones, the deficit is attributable almost entirely to water-soluble salts
73. Spruce branches stripped of needles	15							
74. Bilberry, calcareous. August 29	17	18	42	0.5		3.12	19.38	
75. Bilberry, siliceous. August 20	24	22	22	5		9.5	17.5	

Table 9.1 (continued)

Table of Incinerations					
Incineration number	Name of plants, time of their collection	Ash from 1000 parts of the plant green	Ash from 1000 parts of the plant dry	Water of vegetation in 1000 parts of the plant green	Observations
76	Humus of rosage, calcareous, of Nos. 67 and 69		65		This black humus was taken from a rock of pure carbonate of lime, which livestock could not reach; it was exposed in the open air to all atmospheric influences
77	Extract of the preceding humus		140		This dry extract, black and semi-transparent, was prepared by repeated decoctions of the humus in distilled water renewed at each decoction. The decoctions were cloudy and could be filtered only by a straining-bag, which did not clarify them
78	Humus of rosage, siliceous, of Nos. 68 and 70		620		The large amount of ash contained in this humus was caused by its mixture with the sand or detritus of the gneiss that underlay it
79	Extract of the preceding humus		142		

Table 9.1 (continued)

Table of Analyses

Constituents of 100 parts of the ash

Analysis No., corresponding to incineration No.	Water-soluble salts [alkaline salts]	Earthy phosphates [phosphates of lime or magnesia]	Earthy carbonates [carbonates of lime or magnesia]	Silica	Alumina	Metallic Oxides	Deficit	Observations
76. Humus of rosage, calcareous, of Nos. 67 and 69	0.5	6	29	28	3	18	15.5	I found 12 parts of alkaline salts in the acid solution of the ash freed of earths and oxides. The deficit is thus only 3 parts. This humus did not effervesce with acids
77. Extract of the preceding humus	27	16.75	21	3	0.12	3	29.13	*More precise analysis of the same ash:* Carbonated potash 14 Chloride of potash 23 Sulfate of potash 16 Earthy phosphates 17.25 Earthy carbonates 21.5 Silica 3.25 Alumina 0.12 Metallic oxides 3 Loss 1.88 100
78. Humus of rosage, siliceous	0							
79. Extract of the preceding humus	24	13	17	14	0.12	10	21.88	

Chapter 10
Chapter Summaries

By the Translator

Chapter 1. Influence of oxygen gas on germination. Seeds cannot germinate without water and oxygen gas. Oxygen does not unite directly with the substance of the seed. All germinating seeds give off carbon dioxide, and the volume emitted is nearly the same as the volume of oxygen absorbed; thus oxygen's influence seems to be limited to removing a part of the carbon that the seed acquired when it was formed. Seeds will not germinate in atmospheres of pure nitrogen, hydrogen, or carbon dioxide. The carbon dioxide produced during germination inhibits continued germination more than does a similar amount of nitrogen or hydrogen gas. For a given species of plant, the amount of oxygen required for germination varies in proportion to the size of the seeds and is, overall, very small. Putrefying seeds give off carbon dioxide, methane, and nitrogen gases. During germination, seeds produce sugar, but not as a result of the direct combination of oxygen gas with a substance in the seed. Seeds have a lower dry weight after germination, even when corrected for the carbon lost as carbon dioxide. This additional weight loss is attributable to the formation and release of water from the seeds' dry substance. Light is not detrimental to germination.

Chapter 2. Influence of carbon dioxide gas on plant growth. Green plants exposed to sunlight must break down carbon dioxide in order to grow. Germination and the stages of growth immediately following it, however, are inhibited by this gas. Extra carbon dioxide added to a green plant's environment stimulates plant growth, up to a point. Plants grown in sunlight, with their roots immersed in water that was lightly impregnated with carbon dioxide, thrived and gained weight. Plants grown with their roots in distilled water and with atmospheres containing added carbon dioxide gas showed various responses. If they were exposed to the sun, in an atmosphere that was 8 percent carbon dioxide, they thrived, but at higher levels of added carbon dioxide, their growth declined, and, at levels of 50 percent and above, they died. For plants grown in the shade, growth was inhibited even in atmospheres containing only 8 percent carbon dioxide. For plants grown in sunlight, if quicklime was present to absorb the carbon dioxide of the air and water, the plants soon died and vitiated their atmosphere.

Plants grown with their shoots in open air, in the light, and their roots in distilled water assimilated carbon, proving that these plants must be obtaining their carbon solely from the atmosphere, miniscule though that source is, because they could not

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be drawing it through their roots. Plants of the same weight, grown in darkness, lost carbon. The amount of carbon dioxide decomposed by the green parts of plants is approximately equal to the amount of oxygen they liberate, but a small percentage of the oxygen is retained by the plant, as is all of the carbon. Plants release nitrogen gas.

Chapter 3. Influence of oxygen gas on developed plants. The most evident effect of oxygen gas on developed plants is to elicit the production of carbon dioxide from the combination of this oxygen with the plants' own carbon. At night, leaves "inspire" atmospheric oxygen and partly replace it by "expiring" carbon dioxide, whereas during the daytime the reverse occurs, and overall the atmosphere in a closed space remains about constant in volume. If the leaves are very thick, however, as in cactus, they absorb the oxygen without releasing carbon dioxide. The production of carbon dioxide gives back to plants a substance that they can decompose and thereby regain the carbon that they have lost. The formation of carbon dioxide from atmospheric oxygen and plant carbon was confirmed for leaves of 57 species.

Oxygen is essential to plant growth and development. For example, germinating seeds require it; roots need it and die if put in nitrogen, hydrogen, or, especially, carbon dioxide, even if the shoot is exposed to air and light; and shoots survive in oxygen-free environments only if their green parts are exposed to light, allowing them to break down carbon dioxide that they have formed from their own internal oxygen and carbon, thereby producing and releasing enough oxygen to sustain themselves. Plant parts that are growing or otherwise physiologically active require more oxygen than do less active parts. The oxygen inspired cannot be recovered by an air pump. In non-green plant parts, the carbon dioxide produced by the combination of atmospheric oxygen with plant carbon probably ascends with the sap to be decomposed by the leaves rather than being fixed directly in the absorbing organ.

Chapter 4. Influence of oxygen gas on some plant-derived substances. In plants that are in the early stages of fermentation, or have just died, oxygen gas is not fixed in the tissues nor does it combine with their hydrogen; it only removes a part of their carbon, forming carbon dioxide. This effect is evident in the precipitation of plant extracts, the formation of acetic acid from wine, changes in wood, coagulation of plant albumin, oxidation of turpentine oil, and putrefaction. Olive oil, in contrast, releases no carbon dioxide. As carbon dioxide is released from substances, some of the water that had previously been fixed in the plant is also removed. As fermentation of plant substances proceeds, more water is disengaged, and the oxygen absorbed does not appear entirely in the carbon dioxide produced but probably combines directly with the hydrogen in the plant to form water. Thus, the proportion of carbon in the residue is not reduced after carbon is withdrawn through its combination with oxygen.

Chapter 5. Of plant humus. Humus is the black substance into which dead plants are converted by the combined action of oxygen and water. Oxygen gas penetrates humus and combines with its carbon to produce carbon dioxide gas, which is released, thereby becoming a nutrient for plants. Carbonization of 47 plants, plant parts, or plant substances showed that humus contains more carbon per unit weight than the plants that gave rise to it. The proportion of carbon contained by a humus, however, does not seem to be increased significantly by the continued action of the causes that formed it. In a finished humus, although continuous exposure to air and rain leads to continued weight loss, there is little change in the proportions of carbon, hydrogen, and oxygen because the hydrogen and oxygen are removed, in the form of water, in proportion to the removal of carbon as carbon dioxide. After multiple extractions of humus, the humus still yields a small amount of extract. Acids do not dissolve the organic matter of humus, but potash and soda do. Humus is entirely destructible at room temperature due to the joint action of oxygen and water. As a humus loses carbon and water, a small amount is also being lost as extract to the soil drainage water. These changes explain why there is little accumulation of humus, even in places where vegetation has long been established. Incineration of humus yields a semi-vitreous ash which is not very soluble in water. The ash of the extractive juices of a humus, however, contains all the substances of plant ash. Thus, the extractive juices contribute to the fertility of the humus. Humus contains a larger proportion of nitrogen than do undecomposed plants. Humus has an antiseptic action, which retards fermentation and putrefaction.

Chapter 6. Of plant growth in environments lacking oxygen gas. Plants can survive without oxygen in their atmospheres only if they have enough area of green tissue to produce free oxygen through the decomposition of the carbon dioxide that they form entirely from their own tissues. Oxygen seems to aid photosynthetic assimilation of carbon dioxide. Thus, in environments lacking oxygen, the elaboration of a certain amount of oxygen appears necessary for the decomposition of a certain amount of carbon dioxide. If the oxygen is removed as it is formed, plant growth is arrested. Seeds will not germinate in oxygen-free environments, and newly germinated seeds, before they have developed green parts, will die in these environments. Plants do not assimilate nitrogen, hydrogen, or carbon monoxide gases. They survive in these gases as they do in a vacuum, by means of the oxygen released by their leaves, and they can do so only if they are not in direct sunlight. The amount of oxygen needed to sustain a plant's life is small; more is needed for growth. Carbon dioxide always becomes harmful to plants if it is present in amounts too large for them to decompose. Excess carbon dioxide is more harmful to plants in an atmosphere of nitrogen than in one of ordinary air. In pure hydrogen gas, the carbon dioxide formed by the leaf from its own substance is decomposed by the hydrogen, with production of water and carbon monoxide gas. In an atmosphere of carbon monoxide, green plants, in the sun, do not decompose this gas but do add oxygen.

Chapter 7. Of the fixation and decomposition of water by plants. Plants do not decompose water directly, appropriating its hydrogen and immediately releasing its other element in the form of oxygen gas. Instead, they exhale oxygen gas only through the decomposition of carbon dioxide gas. They retain a small amount of oxygen from this decomposition, but it does not account for all the oxygen that plants contain. Rather, the assimilation of the hydrogen and oxygen of water is the source of most of the oxygen and all of the hydrogen in plant dry substance. The amounts of hydrogen and oxygen in plants cannot be increased significantly without correspondingly increasing the amount of their carbon.

Dead plants in a heap, decaying in the absence of oxygen, form carbon dioxide gas entirely from their own substance. The oxygen component of this carbon dioxide is the oxygen of the water that was previously fixed during growth. Growing plants, in decomposing this newly formed carbon dioxide, eliminate oxygen that had originally been part of the water.

Plants with thick leaves, such as cactus, liberate oxygen in air lacking carbon dioxide. They do this by breaking down carbon dioxide that they form from their own substance. If the carbon dioxide is removed as it is formed, by alkali, no free oxygen is produced. The ability of these plants to form carbon dioxide from their own substance is a result of the low porosity of their epidermis and their large volume, which protects much of their substance from exposure to the outside atmosphere.

Although a plant adds small amounts of oxygen to atmospheres initially lacking both oxygen and carbon dioxide, this does not prove that the plant has decomposed water. Rather, the plant has decomposed carbon dioxide that it forms entirely from the carbon and oxygen it already contains.

In contrast, in atmospheres that contain oxygen, plants do not form carbon dioxide entirely from their own substance, but only from a combination of their carbon with the atmospheric oxygen. The decomposition of this carbon dioxide by the plant would then simply return to the atmosphere the free oxygen that the atmosphere had previously contained. This process should result in no net increase in atmospheric oxygen. Since experiments show no increase in oxygen content, there is no direct decomposition of water by plants.

Chapter 8. Of the absorption of solutions by plant roots. In addition to water and gases, developing plants need minerals from the soil, even though these substances occur in very small amounts in plants. From ten test solutions, each containing the same amount of a different mineral constituent, roots took up varying amounts of minerals, but absorbed none of them in greater proportion than the water of solution. Variations in assimilation also occurred for solutions containing several mineral solutes. Roots absorbed toxic as well as beneficial substances. The minerals that were absorbed varied with the plant species. The dry weight of the extract that the most fertile soil can produce is much less than the dry weight of a plant that develops there, so the plant clearly can be taking only a very small percentage of its dry matter from that source, even though such substances are essential for growth.

Copper sulfate is toxic, killing the roots and thus destroying their selective power of absorption. Sluggish growth, and the cutting off of roots, also promote the unrestricted absorption of solutes.

Variations in assimilation are attributable largely to differences in viscosity. The less viscous a substance, the more easily it penetrates the root. Gum, which yields a very viscous solution, is assimilated to a much lesser extent than is sugar.

Most of a plant's substance is derived from carbon dioxide and water. The roots of green plants absorb only a very small amount of salts and other constituents from the extractive and saline solutions of the humus, yet these constituents have a very pronounced effect on plant growth.

Chapter 9. Observations on the Ash of Plants. The ash left after plants or plant derivatives are incinerated consists of the minerals that the plants had contained. Measurements of ash content and mineral analyses of the ash were made on 79 different plants or plant parts, plant products, and humuses or extracts, including plants of different ages or ones grown in different soils. Results are presented in tables of incineration and mineral analyses. The table of mineral analyses lists the percentages of water-soluble salts, insoluble phosphates and carbonates, silica, alumina, and metallic oxides in the ash.

In general, herbaceous plants contain more ash than do woody plants. Ash is more abundant where transpiration is profuse. Leaves yield more ash than stems or fruits, and bark more than wood. The leaves of deciduous trees, which have greater transpiration than the leaves of evergreen trees, yield more ash. The ash of herbaceous annuals increases at first, but declines as leaves gradually die and are shed or minerals are lost to leaching by rain. A decayed plant that has not been exposed to heavy leaching yields more ash than a living plant of the same kind.

Mineral amounts are presented as percentages. Thus, after the leaching or other loss of certain minerals, the remaining, less easily leached minerals occur in apparently greater abundance, although they are not so in absolute terms. For example, the alkaline salts are easily leached, so, as plants age and are exposed to weathering, the relative proportions of other minerals increase.

The proportions of the elements in plant ash are almost always related to the soils in which the plant grew. Thus, plants growing in a soil rich in silica contain more silica and less lime than the same kinds of plants growing in a calcareous soil. Plants grown in the open air, with their roots immersed in distilled water, develop abnormally because they can obtain minerals only from the dust that settles from the atmosphere. The ash of these same kinds of plants, grown in humus, contains significantly more saline and earthy materials, including minerals that could not have come from the seed.

Minerals are essential to plants. Certain minerals that are taken in might indeed be inessential, but this has not been demonstrated for those minerals that are always present in plants of the same species.

The text concludes with a lengthy description of the chemical methods used to analyze the mineral composition of ash.

Appendices

Appendix 1: Conversions of Units of Measurement

Before the French Revolution, many systems of measurement were in use in different regions of France, but the Paris system was generally used by Parisian scientists such as Antoine Lavoisier and by other scientists writing in French, including Théodore de Saussure. During the French Revolution, the metric system supplanted the Parisian system. Although the units of measurement in the Paris system, when translated into English, sound familiar (*pouce* is inch, *livre* is pound), the quantities represented by these units are not quite the same in the two measurement systems: The Paris pound was slightly smaller than the English avoirdupois pound, and the Paris inch was slightly longer than the English inch. De Saussure wrote *Recherches chimiques sur la Végétation* during the transition to the metric system and reported most of his experimental findings in units of both the metric and the older, Parisian systems. (He did not use any English units, but I have provided conversions, for general information.)

Weight measurements, Paris and English systems:
1 Paris pound (*livre*) = 16 Paris ounces (*onces*) = 9216 Paris grains (*grains*)
1 Paris ounce (*once*) = 8 *gros* (*gros* is an obsolete French unit that has no equivalent term in English.)
1 *gros* = 72 Paris grains (*grains*)
1 Paris pound = 0.926 English avoirdupois pound
1 Paris grain = 0.820 English Troy grain

Length and Volume Measurements, Paris and English Systems:
1 Paris inch (*pouce*) = 12 lines (*lignes*) = 1.066 English inches
1 Paris cubic inch (*pouce cube*) = 1.211 English cubic inches

Metric Equivalents for Paris Units of Weight, Length, and Volume:
1 Paris pound = 489.506 g
1 Paris ounce = 30.594 g
1 *gros* = 3.824 g
1 Paris grain = 0.053 g

J. F. Hill, *Chemical Research on Plant Growth*,
DOI 10.1007/978-1-4614-4136-6,

1 Paris inch = 2.7 cm
4.4 Paris lines = 1 cm
1 Paris cubic inch = 19.84 cm^3

Réaumur Temperature Scale:
In his book, de Saussure exclusively used the Réaumur (abbreviated "R") temperature scale, in which the freezing point of water is 0° and the boiling point 80°, assuming a standard, 1 atmosphere of pressure. The Réaumur scale was named for French scientist René Antoine Ferchault de Réaumur. To convert from the Réaumur to the Celsius scale, multiply the Réaumur degrees by 5/4. To convert from the Réaumur to the Fahrenheit scale, multiply the Réaumur degrees by 9, divide the product by 4, and add 32. (1° Réaumur = 2.25 °F).

Amount of Carbon and Oxygen in Carbon Dioxide:
De Saussure stated in his book that he used Lavoisier's figures for determining the proportions of the weights of carbon and oxygen in carbon dioxide: 28 parts of carbon and 72 parts of oxygen. To determine the weight of carbon and oxygen in a given volume of carbon dioxide gas (at set readings of temperature and pressure), de Saussure used conversion factors of approximately 0.19 Paris grain of carbon and 0.49 Paris grain of oxygen, per Paris cubic inch.

Appendix 2: Glossary of Terms[1]

Absorption (*absorption*) de Saussure often used this term to refer to the assimilation of a gas into a plant's dry substance; distinguished from "consumption" (q.v.)

Acetic (*acétique*) relating to, or containing, acetic acid, or vinegar ($C_2H_4O_2$)

Acetous (*acéteux*) tasting or smelling like vinegar; acetous acid (*acide acéteux*), which is impure acetic acid, is an older name for acetic acid (*acide acétique*); although de Saussure did not use the term acetous acid, he did mention acetous solutions

Aeriform (*aériforme*) gaseous

Alkaline salts (*sels alcalins*) salts of potash (potassium, K) or soda (sodium, Na), but potassium and sodium had not been isolated in de Saussure's day; the salts included carbonates, chlorides (e.g., NaCl, KCl), sulfates, and phosphates; de Saussure also referred to these as "the water-soluble salts"

Alkaline sulfates (*sulfates alcalins*) Na_2SO_4, K_2SO_4, but sodium and potassium had not yet been isolated

Alumina (*alumine*) aluminum oxide (Al_2O_3)

Ammonium aluminum sulfate (*sulfate ammoniacal d'alumine*) also called ammonium alum

Antiseptic (*antiseptique*) a substance that arrests the process of putrefaction or putrid fermentation

Baryta (*Baryte*) barium hydroxide ($Ba(OH)_2$); de Saussure used acetate of baryta ($Ba(C_2H_3O_2)_2$), nitrate of baryta ($Ba(NO_3)_2$), and water of baryta (that is, fully dissolved in water, $Ba^{+2} + 2OH^-$) as reagents for precipitation; the element barium was not isolated from baryta until 1808, by Humphrey Davy[2]

Calcareous spar (*spath calcaire*) calc-spar, or calcite

Caloric (*calorique*) also called "matter of heat"; a precursor to the concept of energy; chemists, impressed by the enormous amount of heat liberated during the combustion of inflammable bodies in oxygen, assumed that oxygen gas was a combination of a very large amount of caloric with a "base" of oxygen or an "oxygenous principle"; all gases were considered to consist of a combination of caloric with a

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[1] This glossary is organized based on the literal translations of terms used by de Saussure. His original French terms are given in parentheses, then modern equivalents are provided and, where appropriate, expanded explanations of the terms, including modern chemical formulae, although these formulae had not been developed in his day. (For an explanation of the handling of chemical terminology in the text of the translation, see the Note on the Translation.)

[2] Davy, H. 1808. Electro-chemical researches, on the decomposition of the earths; with observations on the metals obtained from the alkaline earths, and on the amalgam procured from ammonia. Phil. Trans. Roy. Soc. London 98:333–370.

principle or base of the gas; de Saussure thought that the caloric released by the combination of oxygen gas with the carbon of the plant was useful to the plant

Carbonate of lime (*carbonate de chaux, carbonate calcaire*) calcium carbonate ($CaCO_3$), but the element calcium was not isolated until 1808, by Humphrey Davy[3]

Carbonic acid gas (*gaz acide carbonique*) carbon dioxide (CO_2)

Carburetted hydrogen gas (*gaz hydrogène carburé*) methane (CH_4)

Closed (*fermé*) sealed off or separated from the external environment; de Saussure typically applied this term to inverted receptacles (vessels) that were partially filled with a liquid, either water or mercury, with the mouth of the vessel resting in a saucer or basin of water or mercury; such a system is “closed” with respect to the external air

Consumption (*consumption*) de Saussure often used this term to refer to a plant’s uptake of a gas, as distinct from absorption, or assimilation of a gas into a plant’s dry matter

Decoction (*décoction*) extraction of substances by more or less prolonged contact with a solvent or by boiling

Earth (*terre*) when used in a chemical sense, this usually refers to a carbonate, oxide, or hydroxide; may be calcareous, magnesian, or siliceous

Earthy carbonates (*carbonates terreux*) a collective term for calcium carbonate ($CaCO_3$), which de Saussure referred to as “carbonate of lime”, and magnesium carbonate ($MgCO_3$), which he referred to as “carbonate of magnesia”

Earthy phosphates (*phosphates terreux*) calcium or magnesium combined with phosphate in various forms; de Saussure called these compounds “phosphate of lime” or “phosphate of magnesia”

Elaboration (*élaboration*) decomposition, as of carbon dioxide by green plant tissue in the presence of light

Elastic (*élastique*) in a gaseous, as distinct from a fixed or solid, state

Element, principle (*élément, principe*) the concept of “element” was not well defined in de Sausure’s day, although Lavoisier[4] had held, correctly, that elements were substances that chemical analysis failed to break down into simpler substances; de Saussure sometimes used the older term “principle” in the sense of “element” or “substance”, and, in this translation, depending on the context, one of these latter terms is used instead of “principle”

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[3] Davy 1808. Electro-chemical researches.

[4] Lavoisier, A. 1789. Traité Élémentaire de Chimie, présenté dans un ordre nouveau et d’après les découvertes modernes. 2 vols. Paris: Cuchet. (1790 English translation by Robert Kerr, as “Elements of Chemistry”. Edinburgh: Printed for William Creech; reprinted 1965, Dover Publications, New York).

Empyreumatic oils (*huiles empyreumatiques*) liquid oils obtained by distilling various organic substances at high temperatures; these oils are acidic and soluble; they do not retain the taste and odor of the substance from which they were obtained, but instead have a taste and/or odor of burnt plant or animal matter[5]

Eudiometer (*eudiomètre*) a kind of gas burette used to measure the "goodness" of air, that is, its oxygen content; there were various eudiometric techniques, using different chemicals—for example, nitric oxide (a method called the "nitrous air test" by Priestley, who first developed it); phosphorus; and potassium sulfide (K_2S, de Saussure's "hydrosulfide of potash"); de Saussure considered the phosphorus and potassium sulfide methods superior to the nitrous air test; another technique involved sparking a mixture of hydrogen and the air being tested

Fava bean (*fève*) *Vicia faba*; an Old World bean also known as faba bean or broad bean; compare with "haricot"

Fermentation (*fermentation*) a process not well understood in de Saussure's day, as yeasts and bacteria were not yet known; chemists generally referred to three kinds of fermentation—spiritous (which produced alcohol); acid (which produced vinegar, or acetic acid); and putrid (which produced ammonia and was also known as putrefaction)

Flowers of sulfur (*fleurs de soufre*) sublimed sulfur in flower-like crystalline form; flakes or powder

Haricot (*haricot*) the seed or unripe pod of the kidney bean (*Phaseolus vulgaris*), a NewWorld, climbing string bean; compare with "fava bean"

Humus, plant mold *(terreau, humus, terre végétale)* de Saussure used all these terms, without clear distinctions, but he mostly used "*terreau*", which, along with "*humus*", is translated here as "humus", whereas "*terre végétale*" is translated as "plant mold"; Feller and Manlay[6] maintain that de Saussure ascribed a broad meaning to the word "humus" (i.e., the whole vegetative cover undergoing decomposition) and a narrower one to the word "mold" (i.e., the black substance in which plants are embedded); "terre végétale" can also be translated as topsoil

Hydrogen (*gaz hydrogène*) H_2; see also "inflammable gas"

Hydrosulfide (*hydrosulfure*) older term for sulfide; in some of his eudiometric tests, de Saussure used "hydrosulfide of potash" (potassium sulfide, K_2S)

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[5] Eklund, J. 1975. The incompleat chemist: being an essay on the eighteenth-century chemist in his laboratory, with a dictionary of obsolete chemical terms of the period. Smithsonian Stud Hist Tech 33. Washington, DC: Smithsonian Institution Press.

[6] Feller, C., and Manlay, R. 2001. Humus, fertility and sustainability of cropping systems: concepts over the past three centuries. Publication No. 147. Université Laval, Faculté de foresterie et de géomatique, Département des sciences du bois et de la forêt, Groupe de coordination sur les bois raméaux. Quebec, Canada.

Inflammable gas (*gaz inflammable*) older term for hydrogen gas, but was also used for a variety of hydrocarbons

Lime, slaked (*chaux éteinte à l'eau*) calcium hydroxide ($Ca(OH)_2$); formed by mixing unslaked lime (CaO), also called quicklime, with water

Limewater (*eau de chaux*) a solution of calcium carbonate ($CaCO_3$); de Saussure used limewater to test for the presence of carbon dioxide, which was detected by the formation of an insoluble, white precipitate of calcium carbonate; he also used this technique to remove carbon dioxide from gas mixtures

Lute (*lute*) to seal or cement, as with clay, to make an impervious fitting

Magnesia (*magnésie*) magnesium carbonate ($MgCO_3$)[7]; modern magnesia is magnesium oxide (MgO); magnesium was not isolated until 1808, by Humphrey Davy[8]

Metallic oxides (*oxydes métalliques*) de Saussure mentions oxides of iron, manganese, and lead

Muriates (*muriates*) chlorides; de Saussure referred to sodium chloride (NaCl) as "muriate of soda", and potassium chloride (KCl) as "muriate of potash"

Muriatic acid (*acide muriatique*) hydrochloric acid (HCl)

Niter (*nitre*) potassium nitrate (KNO_3), also called "saltpeter"

Nitrogen (*azote*) in French, the term "azote" is still used for nitrogen

Nitrous air test (*gaz nitreux*) a test developed by Priestley for the presence of oxygen gas; actually uses nitric oxide (see Translator's Introduction, footnote 9)

Oxalate (*oxalate*) the anion $C_2O_4^{2-}$

Oxide of carbon gas (*gaz oxyde de carbone*) carbon monoxide (CO); in de Saussure's day, hydrogen was often confused with a number of other inflammable gases, and the possible presence of hydrogen in oxide of carbon gas was still a subject of debate; thus, the chemist Berthollet called oxide of carbon gas "oxycarburetted hydrogen", as mentioned by de Saussure on p. 208 of his 1804 book; de Saussure set forth his own views on the matter in footnote 7 on this same page; oxycarburetted hydrogen is now known to be a water gas mixture, that is, hydrogen, carbon monoxide, and carbon dioxide[9]

Oxygenated muriatic acid (*acide muriatique oxygené*) chlorine (Cl_2); in Lavoisier's chemical nomenclature, there was some confusion: hydrochloric acid was called "muriatic acid", and chlorine was called "oxygenated muriatic acid"

Pellicle (*pellicule*) a thin saline crust that forms on a solution

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[7] Eklund 1975.

[8] Davy 1808. Electro-chemical researches.

[9] Eklund 1975.

Phosphate of potash and lime or magnesia (*phosphate potassé de chaux ou de magnésie*) salts that de Saussure obtained while trying to isolate phosphates; Boussingault[10] noted this phrase in de Saussure's book as indicating that de Saussure recognized that some crystalline substances dissolve as if they were two salts (i.e., in modern notation, $Ca\,K(PO_4) \cdot K\,Mg(PO_4)$)

Phosphated potash of lime (*potasse phosphatée de chaux*) $Ca\,K(PO_4)$; a salt that de Saussure obtained while trying to isolate phosphates

Plant growth (*végétation*) see "vegetation"

Plant organization (*organisation végétale*) older term for plant structure

Platina (*platine*) platinum (Pt); the term platina is still sometimes used today to refer to the natural form of platinum, with its impurities

Potash (*potasse*) usually, potassium carbonate (K_2CO_3), but sometimes other compounds of potassium; the element potassium was not discovered until 1808, when Humphrey Davy[11] isolated it from potash, but the "basis of potash" was suspected before the discovery of potassium, as is evident in terms such as caustic potash (KOH), vitriol of potash (K_2SO_4), and muriate of potash (KCl)[12]; in this translation, "potassium" is substituted for "potash" in cases where de Saussure is using the term as part of the name of a compound that is still a valid compound today, or where the general context does not make substitution confusing for the reader

Powdered or effloresced (*effleuri*) describes a substance that is changed from a crystalline to a powdery form through loss of the water of crystallization when exposed to air, or that develops a powdery crust as a result of evaporation or chemical change; de Saussure mentioned powdered soda and powdered sulfate of soda

Proximate constituents of plants (*principes immédiats des végétaux*) refers to plant components obtained through chemical analysis; these components are compounds, such as acids, oils, camphor, and gum, that are presumed to be simpler than the original substance[13]

Prussiate of potash (*prussiate de potasse*) potassium ferrocyanide ($K_4[Fe(CN)_6] \cdot 3H_2O$), a reagent used by de Saussure in analyzing for metallic oxide in ash

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[10] Boussingault, J.-B. 1843. Économie rurale considérée dans ses rapports avec la chimie, la physique et la météorologie. Vol. 1, p. 102. Paris: Béchet jeune. (Translated into English in 1845 by George Law, as Rural Economy in its Relations with Chemistry, Physics, and Meteorology. London: H. Bailliere).

[11] Davy H. 1808. The Bakerian Lecture, on some new phenomena of chemical changes produced by electricity, particularly the decomposition of the fixed alkalies, and the exhibition of the new substances which constitute their bases; and of the general nature of alkaline bodies. Phil Trans Roy Soc London 98:1–44.

[12] Ihde, A. J. 1964. The Development of Modern Chemistry. P. 94, footnote 2. New York, Evanston, and London: Harper & Row.

[13] Eklund 1975.

Putrefaction (*putrefaction*) also known as putrid fermentation; produces ammonia

Pyroligneous acid (*acide pyroligneux*) crude acetic acid from wood ($HC_2H_3O_2$)[14]; de Saussure obtained this acid as a product of wood distillation

Quicklime (*chaux vive*) calcium oxide (CaO); also called unslaked lime; mixing it with water yields calcium hydroxide ($Ca(OH)_2$), or slaked lime (see "lime, slaked")

Recipient (*récipient*) a vessel or receptacle that was an important piece of equipment used by de Saussure and other plant physiologists of his day for studying the relationship between gases and plant life; so-called because chemists used these vessels to "receive" substances produced by distillation; de Saussure's recipients were generally of glass and cylindrical in shape; the experimental plant was introduced from below, into the inverted recipient, which contained either ordinary air or an experimental atmosphere; water or mercury (or both) in the lower part of the recipient and in a saucer beneath the mouth of the recipient served to seal the apparatus from the external air; the gases that remained at the end of the experiment were analyzed by means of eudiometric or other techniques; examples of recipients are shown in de Saussure's plate of figures

Red heat (*chaleur rouge*) refers to a high temperature used for carrying out chemical reactions

Saltpeter (*salpêtre*) potassium nitrate (KNO_3), also called "niter"

Sap, juice (*sève*, *suc*) sap or juice of a plant

Sapwood (*aubier*) de Saussure considered sapwood to be formed by a tree's sap; distinguished from older wood (see "wood, fully formed or well formed")

Sea salt (*sel marin*) sodium chloride ($NaCl$)

Secretion (*secrétion*) de Saussure used this term both in the sense of absorption of a substance by a plant, as through the roots (see his pp. 253, 256), and of excretion of a substance by a plant (see his p. 155)

Septic (*septique*) causing or resulting from putrefaction

Silica (*silice*) silicon dioxide (SiO_2)

Soda (*soude*) this term, like the term "potash", was not well defined in de Saussure's day; it could refer to sodium oxide (Na_2O), sodium bicarbonate ($NaHCO_3$), sodium carbonate (Na_2CO_3), or sodium hydroxide ($NaOH$); the element sodium was not discovered until 1808, when Humphrey Davy[15] isolated it from soda, but, as with potash, the "basis of soda" was suspected before the isolation of sodium; in this translation, "sodium" is substituted for "soda" in cases where de Saussure is using the term as part of the name of a compound that was known at the time and is still valid

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[14] Eklund 1975.

[15] Davy 1808. The Bakerian Lecture.

today, or in cases where the general context does not make substitution confusing for the reader

Spirit of wine (*esprit du vin*) ethyl alcohol

Sulfate of lime (*sulfate de chaux*) gypsum, or calcium sulfate ($CaSO_4$), but calcium was not isolated until 1808, by Humphrey Davy[16]

Vegetation (*végétation*) in the 18th century, this term referred to the active processes of plant growth and development (in contrast to today's connotation of inactivity); the term included such processes as germination, flowering, and fruiting; in this translation, the term "plant growth" is generally substituted for "vegetation"

Vitiated air (*air vicié*) corrupted air, that is, air that is depleted of, or reduced in, oxygen content

Wet, or moist, method (*voie humide*) in chemical analysis, a method in which solvents are used; contrasted with the dry method, in which high temperature is applied instead

Wheat, grain (*blé*) there is some ambiguity in this French term; it may refer to wheat (known as *Triticum sativum* in de Saussure's day) or more generically to grain; de Saussure specifically refers to *froment* as *Triticum sativum*, however

Wood, fully formed or well formed (*bois tout formé*, *bois bien formé*) wood that is older than sapwood (q.v.)

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[16] Davy 1808. Electro-chemical researches.

Appendix 3: Bibliography of References Cited by de Saussure (as expanded by the translator)

Some of the references in this list are only plausible guesses or provide only a brief mention of the finding or claim de Saussure attributed to the author. Where he listed only an author's name, it is often impossible to know which work he is citing. If the work was published in both English and French and de Saussure did not explicitly cite the French version, the English one is given.

Page no. in *Recherches chimiques sur la Végétation*	Footnote no.	References
v	Footnote	Berthollet, Claude-Louis. 1803. Essai de statique chimique. Vol. 1, pp. 513–514. Paris: Chez Firmin Didot
v	Footnote	• Giobert, Jean-Antoine. 1793. Des eaux sulphureuses et thermales de Vaudier, avec des observations physiques, économiques et chimiques sur la vallée de Gesse et des remarques sur l'analyze des eaux sulphureuses en général. Chapter 4, §. 3, pp. 64–68. Turin: De l'Imprimerie de Jaques Fea
2	Footnote 1	• Valmont de Bomare, Jacques-Christophe. 1791. Dictionnaire raisonné universel de l'histoire naturelle. Vol. 2, p. 549, footnote (a). Lyon: Chez Bruyset Frères
4		von Humboldt, Alexander. 1799. Versuche über die chemische Zerlegung des Luftkreises und über einige andere Gegenstände der Naturlehre. Chapter 10 (pp. 236–249). Braunschweig: Friedrich Vieweg
5	Footnote 2	• Sprengel, Kurt. 1802. Anleitung zur Kenntniss der gewachse, in briefen. Vol. 1, p. 396. Halle: Karl August Kümmel
7	Footnote 3	• Guyton de Morveau, Louis-Bernard. 1798. Expériences et observations sur le sucre; extrait de l'ouvrage du docteur Rollo, sur le diabète sucré. Annales de chimie 25:37–50
8		• de Saussure, Théodore. 1799. Recherches sur l'influence du gaz oxygène sur la germination des graines. Journal de physique, de chimie, d'histoire naturelle et des arts 49:92–98
9	Footnote 4	• Lavoisier, Antoine-Laurent. 1781. Mémoire sur la formation de l'acide, nommé air fixe ou acide crayeux, & que je désignerai désormis sous le nom d'acide du charbon. Mémoires de l'Académie royale des sciences, 1781, pp. 448–467. (Also in Oeuvres de Lavoisier (1862) 2: 403–422. Paris: Imprimerie impériale)

• Indicates de Saussure provided some bibliographic information in addition to an author's name, even if sketchy and often with no page numbers, although for some authors (e.g., van Helmont and Hales), there was only one relevant publication (but two French translations of Hales) to which de Saussure could have been referring

Page no. in *Recherches chimiques sur la Végétation*	Footnote no.	References
10		• Guyton de Morveau, Louis-Bernard. 1798. Expériences et observations sur le sucre; extrait de l'ouvrage du docteur Rollo, sur le diabète sucré. Annales de chimie 25:37–50
16		• Guyton de Morveau, Louis-Bernard. 1798. Expériences et observations sur le sucre; extrait de l'ouvrage du docteur Rollo, sur le diabète sucré. Annales de chimie 25:37–50
21		Senebier, Jean. 1782. Mémoires physico-chymiques, sur l'influence de la lumière solaire pour modifier les êtres des trois règnes de la nature, & sur-tout ceux du règne végétal. Vol. 3, pp. 330–344 (see especially p. 342). Geneva: Chez Barthelemi Chirol
22		Ingen-Housz, Jan. 1789. Expériences sur les végétaux, spécialement sur la propriété qu'ils possèdent à un haut degré, soit d'améliorer l'air quand ils sont au soleil, soit de le corrompre la nuit, ou lorsqu'ils sont à l'ombre; auxquelles on a joint une méthode nouvelle de juger du degré de salubrité de l'atmosphère. Revised edition. Translated from English by the author. Vol. 2, p. 447. Paris: Chez Théophile Barrois le jeune
22	Footnote 7	• Thompson, Benjamin (Comte de Rumford). 1799. Essais politiques, économiques, et philosophiques. Vol. 2, p. 273. Translated from English by L. M. D. C. Geneva: Chez G. J. Manger
24	Footnote 9	• Lefebure, E. A. 1801. Expériences sur la germination des plantes, p. 136. Paris and Strasbourg: Chez Levrault, Frères; Strasbourg: De l'Imprimerie de Louis Eck
28	Footnote 2	• Rückert, Georg Christian Albrecht. 1788. Beiträge zur Beantwortung der Preisfrage über den Einfluss der kunstlichen Luftarten auf die Vegetation. Chemische Annalen 2:394–405 (see especially p. 399)
28		Senebier, Jean. 1782. Mémoires physico-chymiques sur l'influence de la lumière solaire pour modifier des êtres des trois règnes de la nature, & sur-tout ceux du règne végétale. Vol. 1, p. 112. Geneva: Chez Barthelemi Chirol
29		• Percival, Thomas. 1789. Speculations on the perceptive power of vegetables. Memoirs of the Literary and Philosophical Society of Manchester. Second edition. Vol 2, pp. 130–146
39		Priestley, Joseph. 1772. Observations on different kinds of air. Philosophical Transactions of the Royal Society of London 62:147–264

Page no. in *Recherches chimiques sur la Végétation*	Footnote no.		References
40			Senebier, Jean. This is a general finding, reported in many of his works, including: 1782. Mémoires physico-chymiques sur l'influence de la lumière solaire pour modifier les êtres des trois règnes de la nature, & sur-tout ceux du règne végétal. 3 vols. Geneva: Chez Barthelemi Chirol
50			• Hassenfratz, Jean-Henri. 1792. Sur la nutrition des végétaux, Premier Mémoire. Annales de chimie 13:178–192
53	Footnote 8		• Ingen-Housz, Jan. 1787. Expériences sur les végétaux, spécialement sur la propriété qu'ils possèdent à un haut degré, soit d'améliorer l'air quand ils sont au soleil, soit de le corrompre la nuit, ou lorsqu'ils sont à l'ombre; auxquelles on a joint une méthode nouvelle de juger du degré de salubrité de l'atmosphère. Revised edition. Translated from English by the author. Vol. 1, pp. 39–43 [not p. 36, as cited by de Saussure]. Paris: Chez Théophile Barrois le jeune
53	Footnote 8		• Ingen-Housz, Jan. 1789. Expériences sur les végétaux, spécialement sur la propriété qu'ils possèdent à un haut degré, soit d'améliorer l'air quand ils sont au soleil, soit de le corrompre la nuit, ou lorsqu'ils sont à l'ombre; auxquelles on a joint une méthode nouvelle de juger du degré de salubrité de l'atmosphère. Revised edition. Translated from English by the author. Vol. 2, pp. 167–170. Paris: Chez Théophile Barrois le jeune
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About the Author and the Translator

Théodore de Saussure (1767–1845) Plant physiologist Théodore de Saussure was born in Geneva, Switzerland, where he lived all his life. He was the son of the famous geologist, meteorologist, and mountain explorer Horace-Bénédict de Saussure. Théodore de Saussure completed the fundamental, pioneering research on photosynthesis and presented the first concise summary of the field in his 1804 book, *Recherches chimiques sur la Végétation*. His major contributions to the knowledge of plant nutrition were his finding that water is incorporated into the dry matter of plants; that the source of plant carbon is atmospheric carbon dioxide, not dissolved organic matter in the soil; that minerals are absorbed by the plant from the soil, not created in the plant by transmutation of other substances; and that minerals are essential for plant growth. His discovery of the incorporation of water enabled completion of the basic, overall chemical equation for photosynthesis. Although he continued to conduct research for 40 years after his book was published, the book represents his major contribution to science.

Jane F. Hill received an undergraduate degree in English from Carleton College, Northfield, Minnesota, USA, in 1968, and earned a master's degree in biology and a doctorate in botany from The George Washington University, Washington, D.C., USA, in 1976 and 1980, respectively. Before entering graduate school, she worked as a writer and editor for the United States Department of Agriculture (USDA) Soil Conservation Service, where she developed an interest in plant growth, agronomy, and soil science. As a graduate student, she worked in dendroclimatology research at the United States Department of the Interior, United States Geological Survey, Reston, VA, and later was a volunteer collaborator at plant research laboratories at the USDA Beltsville Agricultural Research Center, Beltsville, MD, USA. She has written extensively on a variety of subjects involving many aspects of botany and biology, including encyclopedia essays on pioneering photosynthesis research and other 18th and 19th century history-of-science topics. She wrote a chapter on the early photosynthesis pioneers for Volume 34 in the series *Advances in Photosynthesis and Respiration* (J.J. Eaton-Rye, B.C.Tripathy, and T.D. Sharkey, eds.; Govindjee and Sharkey, series eds., 2012, Springer).

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In addition, Dr. Hill has co-authored a 500-page book on structure, function, and development of woody plants (J. A. Romberger, Z. Hejnowicz, and J. F. Hill 1993, Springer-Verlag; re-issued 2004, Blackburn Press). Also, she has published original research papers on dendroclimatology and wood structure in the American Journal of Botany.