【2026 Fu Ssu-Nien Lectures (I)】

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Discussion Session:

Topic: Observation as a Way of Life

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※ **advance reading**

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The Empire of Observation, 1600–1800

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By circa 1600, as the previous essay by Gianna Pomata shows, observation had become an epistemic genre, especially among astronomers and physicians but also among jurists and philologists: an increasing number of book titles proudly announced their contents as "observations," understood as the results of empirical inquiry. Characteristic of the emergent epistemic genre of the *observationes* was, first, an emphasis on singular events, witnessed firsthand (*autopsia*) by a named author (in contrast to the accumulation of anonymous data over centuries described by Cicero and Pliny as typical of *observationes*); second, a deliberate effort to separate observation from conjecture (in contrast to the medieval Scholastic connection of observation with the conjectural sciences, such as astrology); and third, the creation of virtual communities of observers dispersed over time and space, who communicated and pooled their observations in letters and publications (in contrast to passing them down from father to son or teacher to student as rare and precious treasures). By circa 1750, observation had also become an *epistemic category*, that is, an object of reflection that had found its way into philosophical lexica and methodological treatises.[1] Observation had arrived, both as a key learned practice and as a fundamental form of knowledge. As the Genevan naturalist Charles Bonnet wrote in 1757 to his fellow observer, Bern anatomist and botanist Albrecht von Haller: "I have often revolved in my mind the plan of a work that I would have entitled *Essay on the Art of Observing.* I would have collected as in a tableau the most beautiful discoveries that had been made since the birth of philosophy. . . . I would have demonstrated that the spirit of observation is the universal spirit of the arts and sciences."[2]

The consolidation of an epistemic genre primarily linked to astronomy and medicine in the sixteenth century into an epistemic category essential for all the arts and sciences by the early eighteenth century was the result of remarkable innovations in the making, using, and conceptualizing of observation: new instruments like the telescope and microscope; new techniques for coordinating and collating the information produced by far-flung observers ranging from the questionnaire to the synoptic map; new thinking about the relationship between reason and experience—or rather, about new forms of reasoned experience, most prominently observation and experiment. As an epistemic category, "observation" took its place among a throng of other early modern innovations in the realm of disciplined experience.[3] The most important of these was "experiment," whose meaning shifted from the broad and heterogeneous sense of *experimentum* as recipe, trial, or just common experience to a concertedly artificial manipulation, often using special instruments and designed to probe hidden causes. By the early seventeenth century, "observation" and "experiment," seldom coupled in the Middle Ages, as Katharine Park notes in her essay in this volume, had become an inseparable pair, and have defined and redefined each other ever since. In the period from the early seventeenth to the mid-nineteenth century, the relationship between observation and experiment shifted not once, but several times: from rough synonyms, as in the phrase "observations and experiments" that had become current by the early seventeenth century, to complementary and interlocking parts of a single method of inquiry throughout much of the eighteenth and early nineteenth centuries, to distinct procedures opposed as "passive observation" and "active experiment" by the mid-nineteenth century. The relationship between observation and conjecture was also in motion during this period, evolving from deliberate segregation in the late sixteenth and seventeenth centuries to equally deliberate interaction by the latter half of the eighteenth century, when observation became an "art of conjecture."

The emergence of observation as a recognized form of learned experience in early modern Europe did not, however, alter a fundamental aspect of observation that had been prominent since the Middle Ages, if not earlier, and is amply documented in the other essays in part 1 of this volume: observation and observance remained tightly intertwined. Although the kinds of observances required by new contexts and modes of observation did change dramatically, observation remained a way of life, not just a technique. Indeed, so demanding did this way of life become that it threatened to disrupt the observer's other commitments to family, profession, or religion and to substitute epistolary contacts with other observers for local sociability with relatives and peers. The metaphorical "family" developed among observers in the context of the emergent epistemic genre of the *observationes* in the late sixteenth century threatened by the mid-eighteenth century to displace the observer's literal family—as when French naturalist Louis Duhamel du Monceau depleted not only his own fortune but that of his nephews on scientific investigations.[4] By the late seventeenth century, the dedicated scientific observer who lavished time and money on eccentric pursuits was a sufficiently distinctive persona in sophisticated cultural capitals like London or Paris to be ridiculed by satirists and lambasted by moralists.[5] In the course of the seventeenth and eighteenth centuries, scientific observation was theorized and practiced, disseminated and celebrated with missionary-like enthusiasm, as its adherents opened up a veritable empire of observation.

Observation and Experiment

How did the term "observation" broaden its meaning and significance to become an essential aspect of both the theory and practice of natural knowledge by the late seventeenth century? The obverse of this question is how the widely diffused, all-purpose word "experiment" during the same period narrowed its scope to denote a carefully designed human intervention into the ordinary course of nature. Although Francis Bacon's own vocabulary did not fix the fluid meanings of *observatio*, *experimentum*, and *experientia*,[6] avowed Baconians played a key role in the rise of the terminology of observation and experiment in mid-seventeenth-century scientific circles. The academies (and some private groups, such as the circle around Samuel Hartlib in London[7]) founded in northern Europe during the middle decades of the seventeenth century, in imitation of earlier Italian academies like the Roman Accademia dei Lincei (established 1603), seem to have provided the crucible that fused the Baconian program for a natural philosophy grounded in an enlarged and improved natural history with the earlier medical project of collecting *observationes*.

The earliest of these transalpine academies, the Academia Naturae Curiosorum (Academy of Those Curious about Nature; later known as the Leopoldina) established in the imperial city of Schweinfurt in 1652 by a handful of German physicians, was perhaps the clearest example of this fusion.[8] In the late 1660s the officers of the Academia Naturae Curiosorum issued an invitation to the "learned all over Europe" to submit their "observations and experiments" on anything "rare and hidden in physic or medicine" to be collected and published with the names of the contributors in an annual volume, variously known as the *Ephemerides* or the *Miscellanea curiosa*, with the academy's imprimatur.[9] The early volumes reported on the activities of sister academies in Florence, London, and Paris (which were sent copies), and Bacon's House of Salomon, an imagined institution for lavishly funded scientific research, was explicitly held up as a model.

Although the Academia Naturae Curiosorum's official focus was on medicine, it self-consciously emulated the Royal Society of London (established 1660) and the Paris Académie Royale des Sciences (established 1666) in the form and scope of its publications: short firsthand reports submitted by—as the preface to the 1669 volume of the *Philosophical Transactions* put it—"all Ingenious Men, and such as consider the importance of Cementing Philosophical Spirits, and of assembling together Ingenuities, Observations, Experiments and Inventions, scattered up and down the World;. . . ."[10] As the wording of this invitation suggests, the vocabulary of the *Philosophical Transactions* was not as influenced by the medical model of *observationes* as that of the *Miscellanea curiosa.* In a 1665 letter to Breslau physician and Academia Naturae Curiosorum member Philip Jacob Sachs von Lowenhaimb, Henry Oldenburg, editor of the *Philosophical Transactions* and secretary to the Royal Society, emphasized the Society's more sweeping ambitions: "I understand that your Academy is composed of medical men only . . . But our Society, aiming at other things, is composed of men of all ranks who are distinguished in letters or by their experience [*tum literis tum experientia*], and enrolls mathematicians, physicists, mechanicians, physicians, astronomers, opticians, etc. It is about to reconstruct philosophy, not as it pertains to medicine alone, but as it concerns all that pertains to the usefulness and convenience of human life . . . to this end it is busy with nothing so much as building up a store and treasury of observations and experiments [*Observationum et Experimentorum*]."[11]

The titles of the articles published by the *Philosophical Transactions* in its first decades reflect this broader constituency and less-specialized vocabulary: many but by no means all titles relating some event or object investigated firsthand contained the word "observation" (and variants such as "observables"); of these, only some followed the medical format of numbered items. Yet these articles nonetheless bear witness to a meaning of the term "observation" that had at once expanded and sharpened: "observations" on everything from may dew to silkworms joined examples in astronomy and medicine, but the sense of "observation" in the late seventeenth-century context was now explicitly linked to *autopsia*, as opposed to remarks upon someone else's observations or hypotheses, which were designated as "considerations" or "animadversions."[12]

A parallel consolidation of term and meaning appears to have taken place in the annals of the Paris Académie Royale des Sciences in the 1660s and

1670s. Like the Royal Society of London, the Paris Académie aimed to be more comprehensive in its membership and inquiries than the medical Academia Naturae Curiosorum. But as in the case of the Royal Society, medical men were prominently represented among its members and correspondents.[13] Several of the works, including books and especially pamphlets, published under its auspices during this period, are presented as "observations."[14] After the *Histoire et mémoires de l'Académie royale des sciences* began regular publication from 1699 on, "diverse observations," under which individual short observations were presented in numbered lists with the names of the observers, became a regular feature of the *Histoire* section.[15] The term was used often in the manuscript minutes of the Académie from the pre-1699 period, almost always where astronomical or meteorological information was presented, frequently for anatomical reports and occasionally for accounts of botanical, chemical, and physical phenomena.[16] In all cases, *observation* denoted a firsthand report in which the time and place were scrupulously noted. Even those observations that were not presented in a numbered list, after the fashion of the medical *observationes*, were of well-circumscribed objects or events, including those observations that were routinely repeated (e.g., daily thermometer and barometer readings). By the turn of the eighteenth century, "observation" had become an essential practice in almost all of the sciences, not just astronomy, meteorology, and medicine—and the complement and supplement of "experiment."

In Latin and in the vernacular, the terms *experientia/experimentum* appear to have undergone an analogous focusing in the latter half of the seventeenth century, which fixed their meanings well into the eighteenth century. In the medieval period through the early seventeenth century, these words were often used interchangeably, covered a broad range of empirical procedures ranging from experience in general to the artisanal trial or medical recipe, and occurred with considerably greater frequency than *observatio* and its variants,[17] at least in texts about natural knowledge. Probably the most celebrated seventeenth-century use of the word *experimentum*, Bacon's *experimentum crucis* that decided between rival hypotheses, was introduced in the context of a sifting and comparison of observations.[18] English natural philosopher Robert Hooke, for example, perpetuated this sense when in 1679 he described the observation of stellar parallax as the *experimentum crucis* with which to test the Copernican hypothesis.[19]

Yet in the *Novum organum* (1620) and especially in his histories of various natural phenomena, Bacon occasionally and consequentially became more specific in his usage: *experimentum* referred to a deliberate manipulation that would shed light on causes inaccessible to the unaided senses and intellect, not just produce an effect. In addition to exhorting natural philosophers to pay greater heed to "all the experiments [*experimenta*] of the mechanical arts and all the operative parts of the liberal [arts],"[20] he proposed several specific "experiments" of his own, for example, regarding the rarefaction and compression of air, described in considerable detail: "We took a glass egg, with a small hole at one end. . . ."[21] These were "artificial experiments," as opposed to those provided by the ordinary course of nature, and imitated nature's "sports and wantonings": for example, gunpowder was an "artificial experiment" that explained the cause of lightning.[22] In explicit contrast to the trials of the workshop or the marvels of nature, these Baconian operations on nature were to be first and foremost experiments of "light" rather than of "fruit": only once nature had been understood could it be commanded.[23]

What Bacon called "artificial experiments" became the model for "experiment" tout court by circa 1660. The language of artifice, intervention, manipulation, demonstration (both in the sense of proof and spectacle), and causal inquiry increasingly defined the *experimentum* (known, however, as *expérience* in French and *esperienza* in Italian, a lingering echo of the medieval twins *experientia*/*experimentum*).[24] By the late seventeenth century, the nice-minded were drawing distinctions between *experimenta* and *observationes* on the basis of whether one intervened in the course of nature to produce an effect or studied effects as they occurred in the course of nature: according to German natural philosopher Gottfried Wilhelm Leibniz, "there are certain experiments that would be better called observations, in which one considers rather than produces the work."[25] Other distinctions emphasized that observation examined nature as presented to the senses (with or without the aid of instruments), while experiment revealed hidden effects or causes.[26] By the mid-eighteenth century, usage in English, French, and German had crystallized around some form of this distinction.

The terms nonetheless remained intertwined, if distinct, throughout the eighteenth century, as countless titles of the form "Observations and Experiments" testify. In 1756, French mathematician and *philosophe* Jean Le Rond d'Alembert characterized the interaction between observation and experiment as a never-ending loop: "Observation, by the curiosity it inspires and the gaps that it leaves, leads to experiment; experiment returns to observation by the same curiosity that seeks to fill and close the gaps still more; thus one can regard experiment and observation as in some fashion the consequence and complement of one another."[27] The English natural philosopher Joseph Priestley, author of one of the most celebrated eighteenth-century collections of "observations and experiments," similarly emphasized how experiments ramified into observations, which in turn led to new experiments, yielding further observations, stoked by endless curiosity.[28] Although various eighteenth-century accounts valorized either one of the terms at the expense of the other, almost all viewed the two forms of inquiry as working in tandem.[29]

Coordinated Observation

Since ancient times, observation had been understood as collective, as the slow accumulation of anonymous observations over generations, centuries, even millennia. But when observation was reconceived in early modern Europe as the province of doctors, scholars, naturalists, and other literate elites, the nature of that collectivity changed radically: authored observations were systematically made and recorded, exchanged in letters, published in books, and gathered by individuals, governments, mercantile corporations, and scientific societies. Some of these new collectives of observers were informal, albeit crucial to the development of sciences like botany: adopting the epistolary habits of Renaissance humanists, learned naturalists such as Conrad Gessner in sixteenth-century Zurich or Carolus Clusius in Leiden exchanged observations (both in word and image) just as they exchanged specimens and seeds of plants.[30] By the late seventeenth century such letters were sent to and solicited by the editors of learned journals such as the *Philosophical Transactions* and the *Miscellanea curiosa*, who transformed them into the first scientific articles simply by deleting the opening greetings and concluding compliments.[31] But such publications did not replace the personal correspondence of savants, which remained an important means of collectivizing observation throughout the eighteenth century and could rival the networks of major academies in their number of correspondents and geographic reach, as in the case of the Swedish naturalist Carl Linnaeus.[32]

Other early modern observer collectives were more formal and centralized, depending on paid labor and hierarchies of command rather than voluntary contributions from self-declared citizens of the Republic of Letters. The Holy See and the Spanish Council of the Indies issued voluminous questionnaires to solicit the observations of missionaries and colonial administrators, respectively, in foreign lands; trading companies such as the Dutch East India Company instructed their functionaries to file detailed reports on their travels.[33]

Although formal and informal observer collectives were differently organized, the boundary between them was often blurred: the Royal Society resorted to questionnaires and eagerly interrogated merchants about the natural history of faraway lands; well-traveled Jesuits published accounts of their missions abroad that were reviewed in learned journals and plundered for observations; botany, imperialism, and commerce were braided together in the global trade in new pharmaceuticals; humanist travel for personal edification shaded imperceptibly into official travel in the service of the crown, deploying similar observational grids. Bacon's imagined "Merchants of Light," described in his utopian fragment *The New Atlantis* (1627), were supposed to sail the world's seas as spies in order to supply the "Interpreters of Nature" at the pinnacle of the House of Salomon with "knowledge of the affairs and state of those countries to which they were designed, and especially of the sciences, arts, manufactures, and inventions of all the world"[34]—a neat and prescient conflation of the diplomatic, mercantile, and scientific models of early modern collective observation.

The explosion of collective observational activity created a new challenge of integration: how to coordinate observers, standardize instruments and regimens, and correlate results? When observations had been rare and costly to make, as in medieval astronomy, or left uncollected and untransmitted in doctors' personal notes or individual diaries, as Katharine Park describes in her essay in this volume, or confined to local phenomena such as the weather and farming conditions, integration had posed few problems. But as observations multiplied, diversified, and diffused and the ambitions of observational programs like those of imperial powers or transcontinental trading companies swelled, ways of collecting and sorting out the results became urgently needed.

Compendia were a typically humanist response to the problem: adapting the techniques of commonplace books, erudite compilers with well-stocked libraries combed the work of ancient and modern authors to assemble thick volumes of selected, indexed observations on all manner of topics. This was the bookish method plied by medical authors such as Johann Schenck and also by naturalists such as Gessner in his *Historiae animalium* (1551–60)[35] or Bacon in his unfinished *Sylva sylvarum* (1627).[36] The collective empiricism encouraged by seventeenth-century periodicals like the *Miscellanea curiosa* and the *Philosophical Transactions* modified the humanist compendium model to solicit new observations made by named contemporaries, substituting eyewitness testimony for bookish scholarship. But the use of the library to construct series of observations, sometimes reaching back to antiquity, continued to be an important observational technique.

The limitations of compendia soon became evident, especially as methods of observation were refined and standards raised: from the standpoint of naturalists increasingly skeptical about the reliability of classical authors like Pliny,[37] observations attributed to authors of varying credibility or to no authors at all and made under diverse or unspecified conditions heaped up helter-skelter seemed unlikely to supply the solid foundation for a reformed natural philosophy.[38] Even compendia of observations freshly made by trustworthy reporters were too heterogeneous to be summed into generalizations or sifted for regularities: despite the efforts of some editors to append "scholia" or "histories" to individual observations in order to bring out their connections to other observations and larger significance, in the spirit of Bacon's "major observations,"[39] the contents of mid-seventeenth-century scientific journals remained stubbornly miscellaneous—and therefore a disappointment to those who, like Oldenburg, hoped to use them to mobilize the Republic of Letters for a program of coordinated, global observation.

Several attempts were made to counter the dispersion of observations, both before and after the fact. Since the mid-sixteenth century (and well before, in the case of Venetian ambassadors),[40] states and mercantile enterprises trained their representatives in foreign parts to observe and report according to standardized schemes: questionnaires, synoptic tables, Ramist branching charts. Observational grids ranged from curt instructions like Sir William Petty's unpublished lists ("Get the best map of the country." "The value of fruites in winter and somer.") to voluminous lists of questions like the two hundred published by the diplomat and humanist Heinrich Rantzau, which covered everything from the exact point of sunset to musical instruments to the salaries of local clergy.[41] Starting with the Swiss encyclopedist Theodor Zwinger's *Methodus apodemica* (1577), manuals aimed at scholars, young gentlemen, ambassadors, missionaries, merchants, colonial administrators, and other travelers instructed readers on what to look at and how in foreign climes.[42] By the early seventeenth century, observation had become a named practice that travelers were exhorted to cultivate, as in the revised 1630 English translation of Giovanni Botero's *Relationi universali* (1597–98), which added a section "Of Observation."[43]

The questionnaire format was adopted by the Royal Society, which eagerly sought information from travelers in order to compile its Baconian natural histories, despite the problems of verifying marvelous tales from distant lands.[44] Robert Boyle recommended the preparation of a compendium of travel reports to Oldenburg in 1666 and in the first volume of the *Philosophical Transactions* published a natural history questionnaire for any "Countrey, Great or Small."[45] In his fragmentary "The General History of the Air," Boyle had also called upon everyone "who hath leisure, opportunity, and time" to keep a diary of "his own observations of the change and alteration of the air from day to day," emphasizing the utility of such mundane "histories."[46] Instead of the questionnaire format, tables or "schemes" like that proposed by Hooke in 1663 were intended to make the weather observations sent in by correspondents all over Europe commensurable and comparable with one another.[47] In 1723, James Jurin, in his capacity as secretary to the Royal Society of London, went one step further in his Latin invitation to potential observers, offering to provide instruments and giving detailed instructions as to when, where, and how to deploy them.[48]

In such dragnet calls for observations to be sent in from far and wide, the scientific societies of the late seventeenth and early eighteenth centuries shifted the emphasis from observation as individual self-improvement, a prominent theme in earlier humanist travel guides, to observation as a collective, coordinated effort in the service of public utility. As English philosopher John Locke wrote when he published his own weather observations: "I have often thought that if such a Register as this, or one that were better contriv'd, with the help of some Instruments that for exactness might be added, were kept in every County in England, and so constantly published, many things relating to the Air, Winds, Health, Fruitfulness, & c. might by a sagacious man be collected from them, and several Rules and Observations concerning the extent of Winds and Rains, & c. be in time establish'd, to the great advantage of Mankind."[49]

Questionnaires, schemata, and instruments supplied by a central authority—princely, ecclesiastical, or scientific—aimed to press observations into a uniform grid. But these preliminary standardizing measures (which also included supervised drawings, as Daniela Bleichmar describes in her essay in this volume), even when followed scrupulously by roaming merchants, officials, missionaries, and dispersed savants, did not suffice for the smooth integration of the observations that accumulated. There were too many observations, too variously taken, and too obscurely correlated with other observations. As J. Andrew Mendelsohn documents in his essay in this volume, the predicament for the networks of seventeenth- and eighteenth-century weather watchers was dramatic: repeated efforts to sift the piles of reports in search of reliable correlations between rainfall and barometric data, wind and rain, temperature and illness, and any number of other hypotheses failed to yield the desired "Rules and Observations" of the weather.[50] Early modern statesmen were confronted with similar challenges: how to collate the stacks of reports and questionnaires sent in by ambassadors and local officials? As a late-seventeenth-century response to the problem of integrating observations by far-flung correspondents, a vogue for "synopses," "calendars," "registers," "tables," and other visual digests edged out the indices and *loci communes* devised by humanist compilers a century earlier. Tables that correlated two or more observed variables, used since ancient times in astronomy, spread in the late seventeenth and early eighteenth century to meteorology, experimental natural philosophy, and natural history.[51] In an unpublished memo probably intended for one of the rulers he served, Leibniz proposed a "State Table" that would digest all the oral and written accounts of well-traveled informants into a compact summary that the prince could "look over in a moment" and thereby grasp "the connections of things."[52]

Leibniz compared his handy table to "maps of land and sea," and one of the most successful efforts to integrate the results of collective observation was a world map showing prevailing wind patterns prepared by the English astronomer and natural philosopher Edmond Halley in 1686 (fig. 3.1). On the basis of published accounts, conversations with mariners, and his own seafaring observations, Halley discerned a few general "rules" (albeit with exceptions) in the direction of the trade winds above and below the equator and the seasons of regional storms such as Caribbean hurricanes and Indian monsoons. Like Leibniz's table, Halley's map or "Scheme" showed "at one view all the various Tracts and Courses of these Winds."[53] Although Halley's synoptic map and general explanation of global wind patterns was an all-too-rare triumph of collective observation in the seventeenth and eighteenth centuries, it can stand as an emblem for the ambitions of such programs. Inspired by Bacon's project for a "history of the winds," Halley's synthesis drew, as Bacon had hoped, on "a Multitude of Observers, to bring together the experience requisite to compose a perfect and compleat History of these Winds," including not only natural philosophers but navigators and travelers. But in contrast to Bacon's vision of a centralized, state-financed, hierarchically organized corps of observers subordinated to the "Interpreters of Nature" in Salomon's House, Halley's informants were volunteers, and he himself was a seafaring observer, a "Merchant of Light" as well as an "Interpreter of Nature." The merging of these two roles of roaming observer and discoverer of "greater observations, axioms, and aphorisms"[54] was to prove consequential for the practices of learned observation: the eye of the body and the eye of the mind had to be taught to work in harmony.

Observational Practices

By the late seventeenth century, special procedures, carried out by specially qualified people under special circumstances, distinguish the scientific observation from the all-purpose remark. At the very least, scientific observers were expected to exercise unusual care, sometimes as a group cross-checking its individual members. In his preface to the third year of the *Philosophical Transactions,* Oldenburg expressed the hope that "our Ingenious Correspondents have examin'd all circumstances of their communicated Relations,

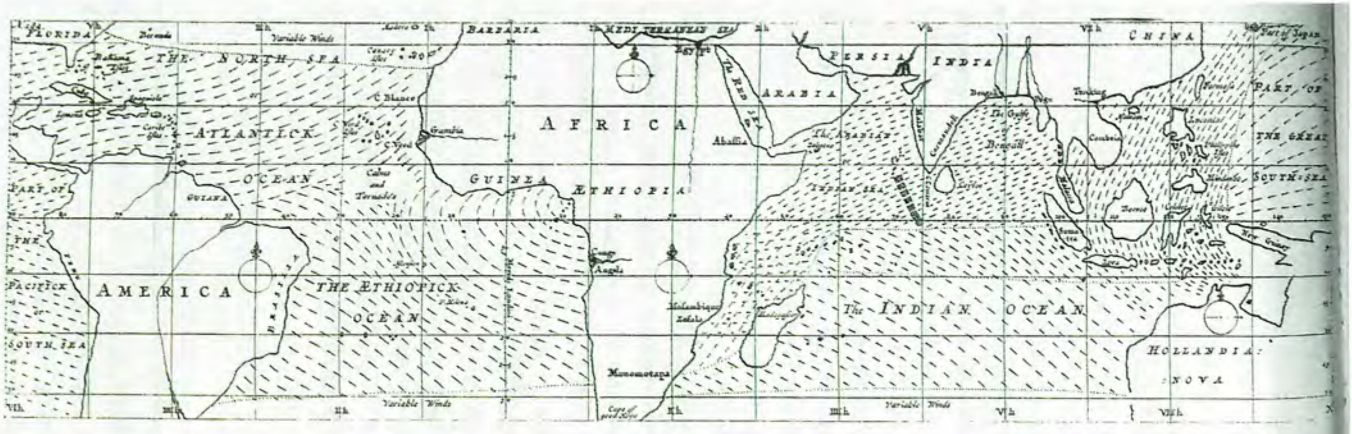

FIGURE 3.1. Map of the world winds. Edmond Halley, "An Historical Account of the Trade Winds, and Monsoons, observable in the Seas between and near the Tropicks, with an attempt to assign the Phisical cause of said Winds," *Philosophical Transactions of the Royal Society of London* 16 (1686–92): 153–68, foldout between pages 49 and 51.

with all the care and diligence necessary to be used in such Collections."[55] These sentiments were echoed in the Paris Académie's *Histoire naturelle des animaux*, which purportedly contained "no facts that have not been verified by the whole Company, composed of people who have eyes for seeing these sorts of things, in contrast to the majority of the rest of the world. . . ."[56] Scientific observers in the seventeenth and eighteenth centuries self-consciously developed novel practices that schooled perception, attention, judgment, and memory. Tools such as the notebook and the magnifying glass were enlisted in these practices. As observation became repetitive as well as collective, the challenge of synthesizing the sequence of notes made by an individual complemented that of integrating the ensemble of reports produced by a community.

REPETITION

Although sustained observation over generations had since ancient times been considered characteristic of the ways in which astronomers, farmers, sailors, and shepherds discovered regularities about the stars and the weather, regimens of repetitive observation of the same object were rare before the early modern period. The example of astronomy, as the most ancient of the observational sciences (and the one longest and most consistently associated with the term), is instructive concerning how a cumulative observational tradition became a repetitive one.

When the French astronomer Jean Picard journeyed to the Danish island of Hven in 1671 to conduct astronomical observations from the ruins of Danish astronomer Tycho Brahe's castle Uraniborg and bring back Tycho's manuscript observations to Paris, he bore witness to the strong sense of continuity that bound even the most boldly innovative early modern astronomers to their predecessors.[57] Part of the care with which late sixteenth- and seventeenth-century astronomers preserved and transposed past observations stemmed from the superhuman temporal scale along which some celestial events, such as the precession of the equinox, unfolded. Only observations carried out over centuries, and in some cases millennia, could discern and specify cycles with long periods or subtle correlations. But part of their solicitude also derived from a desire to test—not just add to—and improve upon past observations, a process that paradoxically led them first to vaunt their own advances and later to cultivate an ever more scrupulous awareness of possible sources of error. Pride in progress as well as fear of error were both tied to what was, at least in the Latin West,[58] a new practice in astronomical observation, with parallels in other early modern observational sciences: the systematic repetition of the same observation night after night, over years and decades.

The consequences of this new practice of sustained and repetitive observation (rather than at special points like quadrature or conjunction) are thrown into relief by a comparison of Tycho's methods of the 1570s and 1580s with those employed a century later. Although Tycho made his reputation with the observation of singular events such as the nova of 1572, analogous to the contemporary medical *observationes* of unusual cases, once established at his purpose-built observatory Uraniborg in the late 1570s, he began a program of sustained observation of the sun, moon, planets, and fixed stars on every clear night for over twenty-one years. His account of solar observations made clear that he was well aware of the novelty of this program: "First of all we determined the course of the sun by very careful observations during several years. We not only investigated with great care its entrance into the equinoctial points, but we also considered the position lying in between these and the solstitial points, particularly in the northern semicircle of the ecliptic since the sun there is not affected by refraction at noon. Observations were made in both cases and repeatedly confirmed, and from these I calculated mathematically both the apogee and the eccentricity corresponding to these times."[59]

Tycho's arduous, costly, decades-long regimen of observation, involving many new instruments of his own invention and of unprecedented size and accuracy, was intended to make future observations superfluous, at least in those areas to which Tycho had devoted the most time and effort. At least his mature observations, Tycho thought, were "completely valid and absolutely certain"[60] and would never need to be repeated. Yet by the 1670s, leading astronomers considered Tycho's observations insufficiently exact. As Astronomer Royal John Flamsteed wrote to Samuel Pepys in 1697 apropos of Tycho's cherished fixed-star observations, "though what he did, far excelled all that was done before him; yet it was much Short of the exactness requisite in this Business."[61] One reason why Flamsteed could pronounce Tycho's observations outdated was the introduction of telescopic sights and the micrometer, both invented circa 1640 but not put into systematic use until the 1660s. Although the telescope had been responsible for some spectacular discoveries in the hands of Galileo and others during the seventeenth century,[62] it by no means displaced sextants and quadrants; telescopic sights arguably contributed more to astronomical observations during this period than the telescope itself did, refining angular resolution to 15 seconds of arc by 1700.[63]

It was not only improvements in instrumentation that persuaded late seventeenth- and eighteenth-century astronomers that their observations were an advance on Tycho's, just as Tycho had vaunted the quality of his observations over those of all previous astronomers. The very practice of sustained, continuous observation that Tycho had institutionalized sharpened the astronomers' awareness of the possibility, perhaps the inevitability, of error. The more observations are made, the more likely it is that they will diverge from one another. Tycho's Uraniborg had been in operation for only about twenty years, but this was long enough to notice a scatter of data and to redouble vigilance to counteract the possible effects of atmospheric refraction, the sagging and stretching of heavy instruments under their own weight, jumpy clocks, and a myriad of other disturbances. But the problem did not go away, no matter how many precautions were taken. With the establishment of observatories like those in Greenwich and Paris in the late seventeenth century,[64] observations stacked up over decades and even centuries. At the Paris Observatory, Picard worried about whether the smoked glass through which the sun was observed might distort the solar diameter or whether the effects of refraction were greater in the winter than in the summer—and many other sources of minute errors.[65] By the first half of the eighteenth century, a heated debate had begun among astronomers about what to do with discordant observations. In astronomy, these were issues that were moralized, mathematized, and ultimately psychologized.[66] Despite these problems, however, by the mid-eighteenth century, all scientific observation was ideally repeated, continuous observation, in studied contrast to the singular or rare phenomena that had dominated medical and scientific collections of *observationes* a century or so earlier.

NOTE TAKING

Some form of note taking has probably since ancient times been part of taking note, of remarking, describing, and remembering—in short, of observing. But note taking itself has a history, one that was consequential for the practices of observing in the early modern period.[67] Two notebooks, one from the late seventeenth century and the other from the late eighteenth century, illustrate some of these changes.

The first was kept by Locke, from September 1666 to April 1703, and entitled "Adversaria physica," or "memoranda on physic."[68] It is a large-ish (approximately 8″ × 12″) calf-bound volume, written in ink, and continuously paginated. The entries, written in Latin, English, and French, relate mostly to medical but also to some natural philosophical matters, mingling excerpts from reading (with references), recipes for medications (e.g., Lady Chichley's eye ointment), practical tips (e.g., where to get the best French olive oil), and some of Locke's own observations, initialed "JL." At the back of the volume is a weather diary, presenting daily thermometer, barometer, hygrometer, and wind observations for a period of almost thirty-seven years (fig. 3.2) These are the only dated entries; insofar as there is another order, it is spasmodically alphabetical, with an elaborate but incomplete index at the front and back of the volume; most of the entries are flagged with a marginal keyword (e.g., "Reason," "Fulmen," "Palpitatio cordis").[69]

Locke's mélange of reading notes and observations was not exceptional in seventeenth-century commonplace books.[70] Culling facts from experience bore some resemblance to culling information and insight from books, and the commonplace books that held the latter were similar in form and aims to the lists and tables that held the former. Among the personnel in Bacon's House of Salomon there were not only "Mystery-men" who collected experiments in the mechanical arts; there were also "Depredators" who collected experiments from books.[71] The keeping of commonplace books of quotations and moral adages culled from the reading of classical authors was a pillar of early modern education in rhetoric.[72] The engrained humanist habits of excerpting, ordering, and recombining the entries of commonplace books offer a suggestive parallel for at least the recording of facts about nature, as in Locke's case. Bacon himself is alleged to have preferred the keeping of commonplace books to other forms of note taking on reading, "because they have in them a kind of Observacion."[73]

A notebook from about a century later offers a study in contrasts. On 10 July 1774 the Genevan naturalist Horace-Bénédict de Saussure began a little yellow notebook (approximately 5″ × 7″), which he labeled "Voyage autour du Mont Blanc en 1774, 10e Juil. Brouillard en crayon No.1. Extraits de l'Agenda." Each page was headed with the day of the week and the date, followed by a lettered (a, b, c, etc.) sequence of short observations, beside each of which was noted the time, often to the minute. Although Saussure recorded a terse "agenda" of the main topics to be covered by the observations on the notebook's flyleaf, he strayed from "primitive and secondary mountains" when something else caught his eye along the way: a ruined château, the strata of slate that struck him as displaced from their original position, the nickname of his local guide, barometer and thermometer readings, a terrifyingly steep mountain pass traversed in the snow in mid-July. The timed entries and the execrable handwriting suggest that the entries were made in real time, bouncing along on a bumpy mountain road. Some entries are exceptionally in ink and in a far more legible hand: "Sunday, 17 July. (a) This morning was set aside for rest or at least some observations at Cormayer. However, I was not

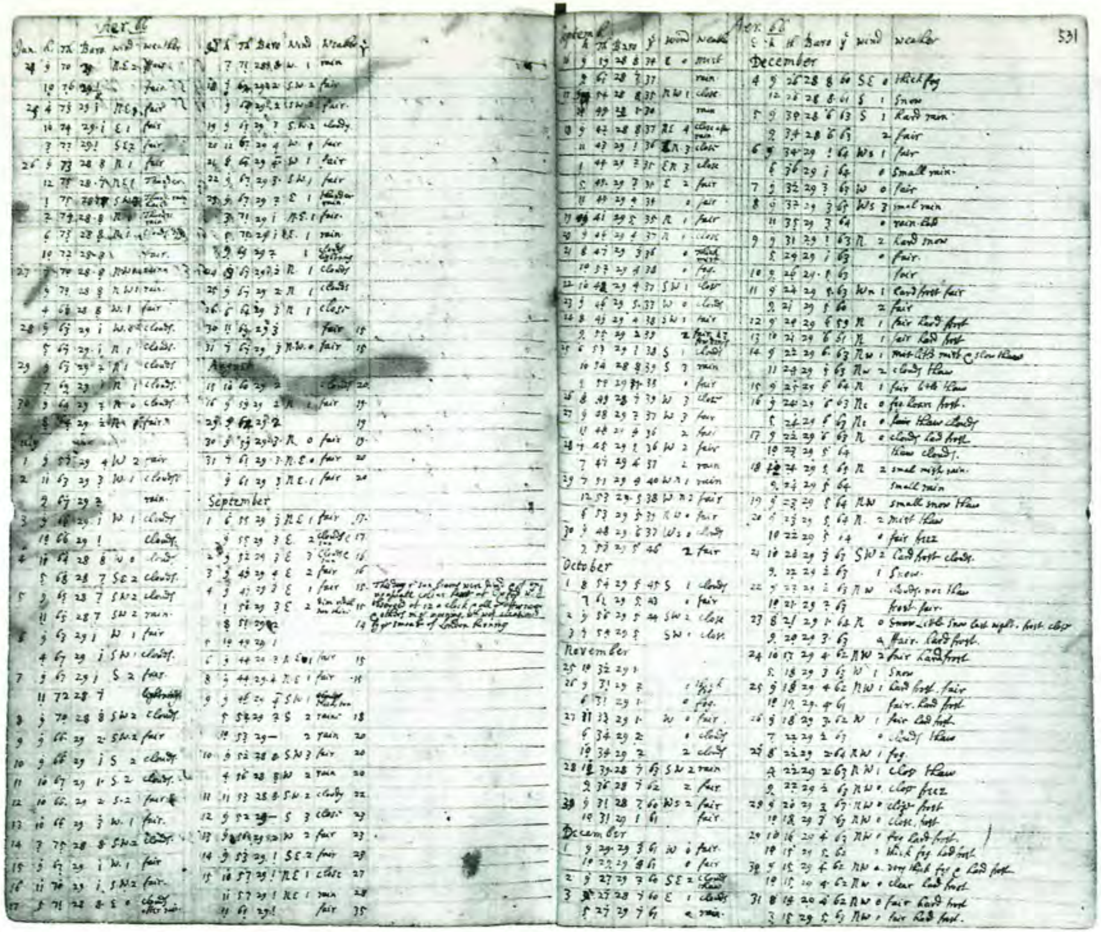

FIGURE 3.2. Weather table. John Locke, "Adversaria physica." Bodleian Library, University of Oxford, Shelfmark MS Locke d.9.

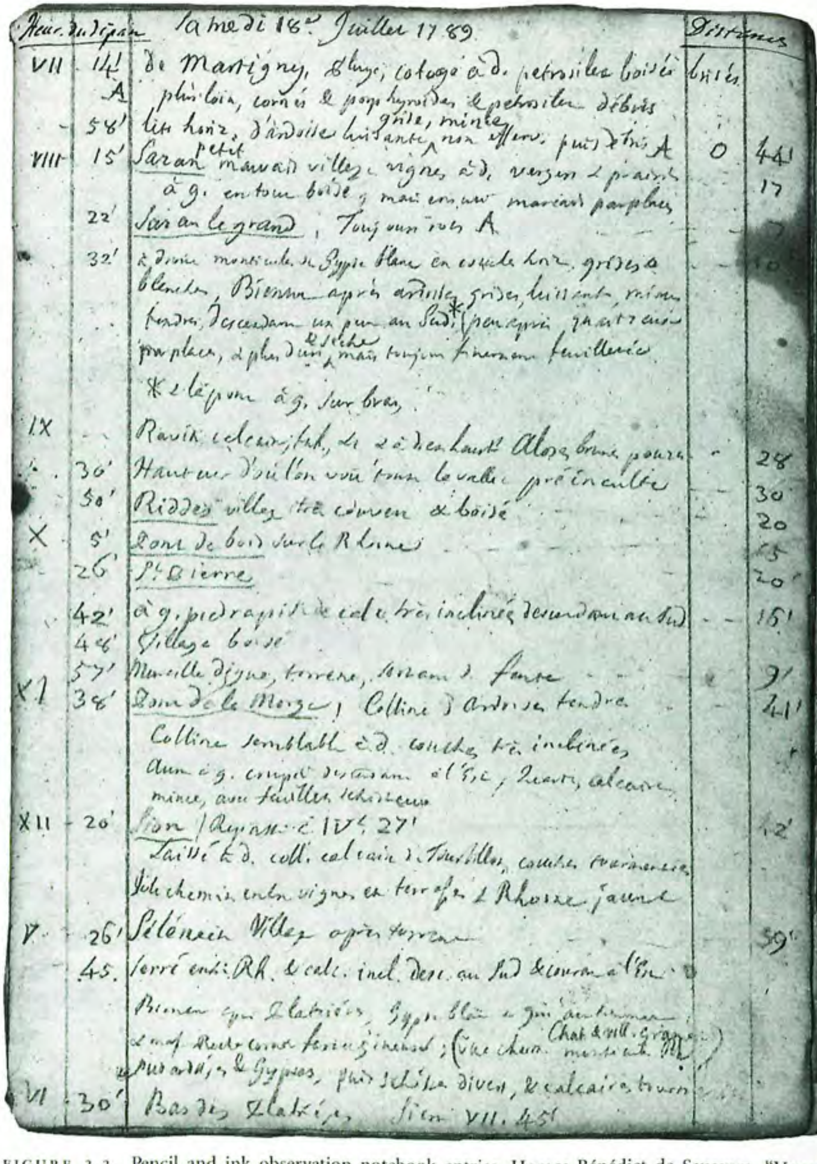

FIGURE 3.3. Pencil and ink observation notebook entries. Horace-Bénédict de Saussure, "Voyage autour du Mont Blanc en 1774, 10e. Juil. Brouillard en crayon No.1. Extraits de l'Agenda," Bibliothèque de Genève, Archives de Saussure 14/1.

at all tired from yesterday, however arduous it had been. (b) I made a trial of several rocks gathered yesterday with eau fort . . ." (fig. 3.3).[74]

This is a typical observation notebook from the latter half of the eighteenth century: pocket format, dated entries further broken down into subentries by a sequence of letters or numbers, real-time entries in pencil and retrospective entries in ink, descriptive observations interspersed with reflections, conjectures, and personal details. There are no thematic indices or reading notes. The model is the journal, more specifically, the travel journal kept en route rather than the commonplace book filled by the desk-bound scholar: Saussure's cardboard-bound notebook was small and light enough to be carried along everywhere; when Locke traveled to the Continent, he left the bulky "Adversaria physica" at home. Above all, the axis of organization has shifted from the topical to the temporal. Locke's notes were assembled with an eye to collation by subject matter; his commonplace book recycled material from old books into the stuff of new books and was itself a proper book, hefty and leather-bound; the entries (with the exception of the weather tables) are as timeless as the pages of a book. Saussure's record is in contrast driven by the calendar and his pocket watch. Time was almost always the vertical dimension of eighteenth-century tables of observation, whether the object of observation was lunar perturbations, the temperature, the incidence of smallpox, or the reproduction of aphids.

PAYING ATTENTION

For Enlightenment naturalists like Saussure and his uncle Bonnet, observing was first and foremost an exercise of attention. As Swiss Protestant minister and naturalist Jean Senebier wrote in his influential 1775 treatise *L'art d'observer*, "[a]ttention alone renders the observer master of the subjects he studies, in uniting all the forces of his soul, in making him carefully discard all that could distract him, and in regarding the object as the only one that exists for it [i.e., attention] at that moment." The peculiar economy of attention cultivated by the Enlightenment naturalists was pointillist, magnifying, and therefore deliberately repetitive. Visually and intellectually, the observer pulverized the object into a mosaic of details, focusing first on one, then another. Senebier directed the fledgling observer to compensate for the "feebleness of his soul and senses in fragmenting [*morcelant*] the subject of his observations and in studying each of its parts separately." Only the narrowness of focus could sufficiently concentrate attention to the level of intensity required for exact observations.

So pencil-thin and intense was the beam of attention that it could hardly be sustained over long periods. Hence the observer must return over and over again to the same object, picking out different details, different aspects each time, and multiply confirming what had already been observed.[75] Still better was the repetition of observations by several observers, not because the veracity of the initial observations was in doubt, but rather to widen the panorama of different perspectives on the same object. In this spirit, Bonnet urged Italian naturalist Lazzaro Spallanzani to repeat the observations of others, including his own: "Nature is so varied that we can hardly vary our attempts too much."[76] The use of microscopes and, especially, the more portable and versatile magnifying glass also tended literally to focus and circumscribe the observer's attention.[77]

SYNTHESIS AND DESCRIPTION

The result of these practices was an avalanche of descriptive detail, both visual and, especially, verbal. It was a byword among the naturalists that it was by the detail with which observations were reported that one could separate the novice from the old hand, the artisan from the savant, the bumbler from the "genius of observation."[78] The most ingenious efforts of observers were directed toward the discernment of the most fleeting details, the finest nuances. Saussure invented an instrument called the cyanometer to measure the shades of blue of the sky, ranging over fifty-three graduations, from milky white to midnight blue.[79]

No study of natural particulars could afford to become permanently mired in particulars. Bacon had feared naturalists might drown in them; Enlightenment observers gladly wallowed in them—but no one deemed them an end in themselves. The practices of taking notes and paying attention as they were cultivated during the mid- and late-eighteenth century tended to fragment the object of inquiry: numbered, dated notebook entries chopped up time into slices; narrowly focused attention dissolved wholes into tiny parts. Whereas collective observation posed the problem of the coordination of many individuals, the challenge of the practices of synthesis confronted the individual observer: how to glue all these fragments back together again into a coherent mosaic—but not in order to reconstitute the actual object of observation. Instead, the result of the synthesis was a general object—variously described by Enlightenment astronomers, anatomists, and naturalists as an archetype, an ideal, an average, or a pure phenomenon—that was more regular, more stable, more universal, more *real* than any actually existing object.[80]

Although observers were sometimes struck by singular phenomena such as an aurora borealis or a monstrous birth, by the mid-eighteenth century they attempted whenever possible to situate individual objects and events in a series. This practice had its antecedents in the longstanding astronomical practice, common since the late sixteenth century, of creating long baselines of multiple observations of the same star or planet. In other sciences of the eye, observers repeated observations of the same or similar objects in order to establish a series. Linnaeus prided himself upon having examined thousands of plant specimens, many supplied by former students dispatched to distant lands.[81] Johann Wolfgang Goethe, reflecting in 1798 on his researches in morphology and optics, described the quest for the "pure phenomenon," which can be discerned only in a sequence of observations, never in an isolated instance.[82] If such a sequence was not readily available to direct observation because of the rarity of the phenomenon, it was compiled from past records: French astronomer Louis Godin began his report to the Académie Royale des Sciences on the October 1726 aurora with a compilation of all previous such sightings, starting with Flavius Josephus in Roman times and concluding with a summary of the features common to all such cases.[83] Ideally, not only the naturalists but also their artists were supposed to be familiar with a broad range of exemplars, so that images as well as descriptions would be the distillation of not one but many individuals carefully observed.[84]

The process of how particulars were forged into generalities is most graphically displayed in the observation notebooks. Under the rubrics of "Reflections," "Results," or "Remarks" (or—in the case of Saussure—simply the shift from pencil scribblings to inky fairhand) were recorded the digestion of first impressions into second (and sometimes third) impressions. These were observations upon observations, the refinement and distillation of raw materials into what Bacon had called "vintages"—or, in his histories, "major observations."[85] Here the older Renaissance practices of humanist note taking were preserved in spirit if not in substance: what sixteenth-century scholars had done for the writings of Cicero and Livy, eighteenth-century naturalists did for oysters and aphids. A first round of observations selected the noteworthy; a second round winnowed these further by comparisons and cross-correlations, seeking patterns and regularities; a third synthesized the features now understood to be the most significant or essential into the general observation.

Observation as a Way of Life

"Never has so much been observed, as in our century."[86] By the mid-eighteenth century, observation was practiced, theorized, and celebrated in almost all sciences. But because observation and observance remained conjoined, the very success of observation, demanding ever more time and dedication from its practitioners, made it controversial as a way of life, an observance too absorbing to be easily compatible with other social, professional, and religious commitments.

Although it never ceased to be arduous, and was recognized to be dangerous at times, early modern scientific observation was seldom described as work (except perhaps by the astronomers: Flamsteed at least complained that it was "labour harder than *thrashing*"[87]). On the contrary, its delights had become so intoxicating they verged on obsession. In a time when travel was fraught with hardship and peril, Clusius could write of the "great pleasure" his wanderings to "observe plants" had provided.[88] Observation obliterated fear and even pain: when in 1770 the Genevan savant André Deluc, armed with thermometer, hygrometer, and barometer, set out to explore the peaks and glaciers of the Alps, he had a foolproof remedy for vertigo: "There is not the slightest danger for those who do not perceive the increase in height, except by a sort of pleasant sensation, which occurs when one is not afraid, and by the pleasure of continually discovering new objects."[89] Leaves dismembered under the microscope, an aurora borealis spotted after many nights' vigil, thermometer readings faithfully registered in the chill dawn, every day for decades on end—these were pleasures of "discovering new objects" evidently so intense that they tempted Enlightenment naturalists to defy parental counsel, neglect civic duties, and deplete family fortunes.

Although moralists were critical of naturalists who sacrificed their families and their health to a demanding regimen of observation,[90] the naturalists themselves could at least count upon the sympathy of their colleagues, with whom they were in constant, copious, and often commiserating correspondence. Although the publication of observations had become increasingly common by the late seventeenth century, the format by which even printed observations were first communicated was the letter. The Sicilian naturalist Paolo Boccone, for example, chose to publish his observations on coral in 1674 as a series of letters to named correspondents scattered across Europe: the Avignon doctor Pierre Guisony, the Pisa professor of mathematics Alexander Marchetti, the London Fellows of the Royal Society Hooke and Nehemiah Grew.[91] Naturalists had been exchanging observations and specimens among themselves since the sixteenth century, a practice that by the early seventeenth century had cemented a strong sense of community among them.[92]

It was in correspondence that fledgling naturalists apprenticed themselves to recognized masters, as the young Bonnet did to the French naturalist René-Antoine Ferchault de Réaumur and Spallanzani in his turn did to Bonnet, presenting their most precious observations for comment and approval. Observations presented in correspondence were also the way in which one naturalist took the measure of distant colleagues: an insufficiently circumstantial or detailed observation report reflected badly on its author—and conversely. Réaumur, for example, accepted La Hague naturalist Pierre Lyonet's corrections to his own observations on the generation of aphids, bowing to the talents of a master observer and draughtsman: "The figures you have sent me are drawn with such a great air of truth that I believe them to be very perfectly conformable to nature." And in their letters naturalists cheered each other on, comrades in a fellowship scorned by outsiders: when Lyonet was too downhearted to continue to observe insects after his proposal of marriage had been thwarted at the last minute by "a most strange caprice" of the lady's mother, Réaumur remonstrated with him not to give up on insects: "[I]t would be a great pity if you became indifferent to them [insects]; they will not fail to repay the attention you have given them with new marvels that they will make you see. I plead for my good friends."[93]

The sociability of specialized correspondence substituted for the more usual sort, since the demands of strict regimens of observations, like those of religious observances, clashed with those of friends and family. The astronomer Picard, for example, rose at 5:30 a.m. and observed with at most a break of an hour or two until midnight, beginning anew at 5:30 the next morning.[94] By the mid-eighteenth century, observant gentlemen all over Europe were interrupting their daily routines to take thermometer and barometer readings to record in diaries and journals.[95] Weather watching, especially if pursued at fixed times of day, could become a way of life, a regimen that set schedules, shooed guests to the door, and fostered clock consciousness. Tycho contemplated a move to Basel because there, close to France, Germany, and Italy, "it would be possible by correspondence to form friendships with distinguished and learned men in different places," whereas on his property in Knudstrup "a continuous stream of noblemen and friends would disturb the scientific work and impede this kind of study."[96] Réaumur moved out of central Paris to have more room for his beehives and fewer visitors—and where, as Mary Terrall shows in her essay in this volume, he could interweave observation regimens with household routines.[97] For the dedicated observer, normal social life became all but impossible. In his *Traité de météorologie* (1774), the Oratorian and corresponding member of the Académie Royale des Sciences Louis Cotte admitted that the perfect weather observer would have to "renounce almost all other business and every pleasure. Not only would he have to live for years on end in the same place; he would have to be home regularly every day for the hours of his observations."[98]

A shared commitment to observation could, however, forge as well as sever social bonds, even surmounting other barriers to friendly contact. When a group of French Jesuits landed at the Cape of Good Hope on their way to Siam in June 1685, they were greeted with suspicion by the Reformed Dutch colonists, who suspected one of the Jesuits' microscopes (draped with an ornamental cover) of being an outlawed Eucharist chalice "because . . . you are the greatest enemies of our religion." Yet their Dutch hosts were pleased to "lead the life of an observer with us" when the Jesuits measured the longitude by following the satellites of Jupiter, and both parties parted on the warmest of terms, the Jesuits presenting the Dutch with a microscope and a small burning mirror in exchange for gifts of tea and wine.[99] Despite the criticisms of moralists and the warnings of physicians, observers were not so much antisocial as highly selective about the company they kept: although they went to considerable lengths to evade conventional social obligations, they craved contact with other observers, if only by letter. Observation was a solitary and obsessive but also communal pursuit.

Conclusion: Observation as a Way of Reasoning

By the late eighteenth century, the relationship between observation and conjecture had taken yet another turn. As we have seen in chapters 1 and 2, medieval natural philosophers associated observation with conjecture because its results were uncertain, confined to particular instances, and mute concerning causes, while early modern physicians had prized observations just because they were allegedly divorced from foolhardy conjecture and system spinning. In this spirit the Roman professor of medicine Giorgio Baglivi recommended observation as an antidote to "the ardent and eager pursuit of new Hypotheses."[100] But in the course of the eighteenth century, observation became a tool of conjecture, a way of excluding some explanatory hypotheses and hatching new ones, which could in turn be submitted to a new round of observation and often experiment as well. In contrast to late seventeenth-century injunctions to segregate observation and conjecture strictly, mid-eighteenth-century manuals of scientific observation insisted that observation was a way of reasoning about, not just collecting experience: while it was deplorable to observe with prejudice for or against a system, it was utter folly to observe without ideas.[101]

The work of the French naturalist Georges Cuvier illustrates how powerful, sophisticated, and deliberate observation had become by the turn of the nineteenth century and serves as a conclusion to the long story of how observation became an essential way of reasoning in the sciences. Cuvier was celebrated among his contemporaries for his anatomical comparisons of extant and fossil organisms, a form of research that depended on the full armamentarium of techniques and resources developed by scientific observers since the sixteenth century.

In his pioneering monograph on the relation of contemporary African and Indian elephants to fossil pachyderm remains, Cuvier mobilized the well-stocked library and museum in order to construct series, in the sense of both long timelines and arrays of gradually differing specimens. From ancient sources on domesticated elephants to the latest fossil discoveries in Russia and the Americas, Cuvier marshaled an exhaustive list of all previous relevant observations in order to establish the geographic distribution of the species. He attributed particular observations to named individuals, with places, times, and stringent evaluation of reliability, discerning progress in the quality of the more recent observations: in 1577 the Swiss savant Felix Platter had mistakenly identified fossil bones as those of a giant, but Cuvier's esteemed Göttingen colleague Johann Friedrich Blumenbach had recently pronounced them as definitely elephant.

While depending heavily on a community of observers dispersed in space and time, Cuvier voiced his preference for firsthand observation wherever possible, dissecting three elephants himself and having a large drawing made "under my eyes, with much care." Drawings and measurements now counted as essential parts of an observation and were also subject to critical scrutiny. Focusing literally with a magnifying glass trained on fossil teeth, Cuvier inspected minute differences of size, shape, and wear as a function of stage of life. Pages of tables displayed the results of his observations of all the elephant molars he had observed, arranged by minutely noted features such as the length, width, and number of lamia. Amid this elephantine mass of information, ancient and modern, first- and secondhand, literary and visual, qualitative and quantitative, descriptive and tabular, Cuvier sought general, constant features that withstood thousands of comparative observations: "However the size alone of the [fossil elephant's] molars suffices in order to recognize them, because it is much more constant."[102]

For Cuvier and his contemporaries, observation had become a tool to think with, a genuine logic of discovery and proof. It was still collective and *longue durée*, but its practitioners were no longer anonymous nor were its results summarized in proverbs and rules of thumb. The work of observation consisted in collating and comparing the observations of others as well as making one's own. The store of observations burgeoned, repeated by individuals and multiplied by communities. The mission to reveal unsuspected correlations among phenomena persisted, but methods of repetition, note taking, establishing series, and inventing synoptic devices such as tables and maps had replaced what Cicero had called "natural divination."

More than ever before, observation was also an observance, regulating waking and sleeping, looking and overlooking, attention and memory, solitude and sociability. When von Haller, perhaps the most celebrated scientific observer of the Enlightenment, fell gravely ill in 1772, he recorded his own symptoms with the same ingrained habits of noting date and time, counting and measuring, and, above all, repeating an observation once, twice, three times:[103] "At five o'clock in the evening the room was a bit too warm, and there being several people there, I felt very ill, with an intermittent pulse after 1–2 or 3 pulsations. I took acid elixir and had the window opened: the air, although very warm, being a sirocco, had a surprising effect: the pulse immediately regularized itself. Three times I made the same experiment."[104] Observation and observance converged in the practices that remade the observer, body and soul.

Notes

1. See, for example, [Jean-Joseph Ménuret de Chambaud], "Observateur (*Gram. Physiq. Méd.*)," in Denis Diderot and Jean Le Rond d'Alembert, eds., *Encyclopédie, ou Dictionnaire raisonné des sciences, des arts et des métiers* [1751–80] (Stuttgart and Bad Cannstatt: Friedrich Fromann Verlag, 1967), 11: 310–13; also the three entries (one general, the other two referring to astronomical and medical observation specifically) on "Observation, *Observatio*," in Johann Heinrich Zedler, *Grosses Vollständiges Universal-Lexicon* [1732–50] (Graz: Akademische Druck- und Verlagsanstalt, 1994), 25: 278–87.

2. Charles Bonnet to Albrecht von Haller, Geneva, 22 July 1757, in Otto Sonntag, ed., *The Correspondence between Albrecht von Haller and Charles Bonnet* (Bern: Hans Huber Publishers, 1983), 107.

3. These included *historia, casus, observatio, experimentum, experientia, phaenomenon, factum.* See Arno Seifert, *Cognitio historica. Die Geschichte als Namengeberin der frühneuzeitlichen Empirie* (Berlin: Duncker & Humboldt, 1976); Peter Dear, *Discipline and Experience: The Mathematical Way in the Scientific Revolution* (Chicago: University of Chicago Press, 1995); "Fatti: storie dell'evidenza empirica," ed. Simona Cerutti and Gianna Pomata, special issue, *Quaderni storici* 108, no. 3 (2001); and Gianna Pomata and Nancy G. Siraisi, eds., *Historia: Empiricism and Erudition in Early Modern Europe* (Cambridge, Mass.: MIT Press, 2005).

4. [M. J. A. N. Condorcet], "Éloge de M. Du Hamel," in *Histoire et mémoires de l'Académie royale des sciences. Année 1783* (Paris: Imprimerie Royale, 1785), 131–55.

5. See, for example, Thomas Shadwell, *The Virtuoso* [1676], ed. Marjorie Hope Nicolson and David Stuart Rodes (London: Edward Arnold, 1966); and Jean de La Bruyère, *Les caractères de Théophraste traduit du grec avec les caractères ou les moeurs de ce siècle* [1688], ed. Robert Pignarre (Paris: Garnier-Flammarion, 1965), 338.

6. See the census of keyword frequencies in Marta Fattori, *Lessico del Novum organum di Francesco Bacone* (Rome: Edizioni dell'Atteneo & Bizzarri, 1980), 106–7, 168–74, 208–9, 342.

7. On the Baconian projects, several of them medical, of the Hartlib circle, see Charles Web-

ster, *The Great Instauration: Science, Medicine and Reform, 1626–1660*, 2nd ed. (New York: Peter Lang, 2002).

8. Uwe Müller, "Die Leopoldina unter den Präsidenten Bausch, Fehr und Volckamer 1652–1693," in *350 Jahre Leopoldina. Anspruch und Wirklichkeit*, ed. Benno Parthier and Dietrich von Engelhardt (Halle: Deutsche Akademie der Naturforscher Leopoldina, 2002), 45–94. Andreas Büchner, *Academia sacri romani imperii Leopoldino-Carolinae naturae curiosorum historia* (Halle and Magdeburg: Johann Gebauer, 1755), 181–97, gives the text of the original bylaws of 1652 (originally published in 1662 under the title *Salve academicum*). Laws 1–8 concern the monographs.

9. Müller, "Die Leopoldina"; Büchner, *Academia*, 194.

10. [Henry Oldenburg], "A Preface to the Third Year of These Tracts," *Philosophical Transactions of the Royal Society of London* 2 (1667): 409–15, on 414; also Mordechai Feingold, "Of Records and Grandeur: The Archive of the Royal Society," in *Archives of the Scientific Revolution: The Formation and Exchange of Ideas in Seventeenth-Century Europe*, ed. Michael Hunter (Woodbridge and Suffolk: Boydell Press, 1998), 171–84.

11. "Nr. 376. Oldenburg to Sachs, 30 May 1665," in A. Rupert Hall and Marie Boas Hall, eds., *The Correspondence of Henry Oldenburg*, 13 vols. (Madison: University of Wisconsin Press, 1965–86), vol. 7 (1670–71), 432–35.

12. Contrast, for example, "Of an Observation, not Long Since Made in England, of Saturn," *Philosophical Transactions* 1 (1665–66): 152–53; or "Observations Concerning Cochineel, Accompanied with Some Suggestions for Finding out and Preparing Such like Substances Out of Other Vegetables," *Philosophical Transactions* 3 (1668): 796–97, with "Some Considerations upon Mr. Reeds Letter . . . in what Sense the Sap May be Said to Descend. . . . ," *Philosophical Transactions* 6 (1671): 2144–49.

13. On the early membership of the Royal Society, see Michael Hunter, "Catalogue of Fellows, 1660–1700," 159–252, in Hunter, *The Royal Society and Its Fellows, 1660–1700: The Morphology of an Early Scientific Institution* (Chalfont St. Giles: British Society for the History of Science, 1982); and for the Académie Royale des Sciences, Roger Hahn, *The Anatomy of a Scientific Institution: The Paris Academy of Sciences, 1666–1803* (Berkeley and London: University of California Press, 1971), 31–49; *Index biographique de l'Académie des sciences*, 1666–1978 (Paris: Gauthier-Villars, 1979).

14. For example, *Relation d'une observation faite à la Bibliothèque du roy, à Paris, le 12 May 1667, sur les neuf heures du matin, d'un halo ou couronne à l'entour du soleil* (Paris: J. Cusson, 1667); or *Extrait d'une lettre écrite à Monsieur de la Chambre, qui contient les observations qui ont esté faites sur un grand poisson dissequé dans la Bibliothèque du roy, le vingt-quatrième Juin 1667* (Paris: F. Léonard, 1667).

15. Anne-Sylvie Guénon, "Les publications de l'Académie des sciences," in *Histoire et mémoire de l'Académie des sciences. Guide de recherches*, ed. Éric Brian and Christiane Demeulenaere-Douyère (Paris: Technique & Documentation, 1996), 107–40.

16. Procès-verbaux, Archives de l'Académie des sciences, Paris. The manuscript registers begin with the meeting of 22 December 1666.

17. Gianna Pomata, "A Word of the Empirics: The Ancient Concept of Observation and Its Recovery in Early Modern Medicine," *Annals of Science*, forthcoming.

18. Francis Bacon, *Novum organum*, II.xxxvi, in Basil Montagu, ed., *The Works of Francis Bacon*, 17 vols. (London: William Pickering, 1825–34), 9: 375–90.

19. Robert Hooke, "An Attempt to Prove the Motion of the Earth by Observations,"

Lectiones Cutlerianae [1679], reprinted in R. T. Gunther, *Early Science in Oxford* (London: Dawsons, 1968), 8: 4.

20. Bacon, "Distributio operis," *Novum organum*, in *Works*, 9: 173.

21. Bacon, *Novum organum*, II.xlv, in *Works*, 9: 416–17; cp. the table of densities, followed by "Modus experimenti" and "Observationes" in *Works*, 11: 184–92.

22. Bacon, "Phaenomena universi," *Works*, 11: 180.

23. Bacon, *Novum organum*, I.lxx, *Works*, 9: 219–21.

24. On the emergent rhetoric of *experientia/experimentum* in both French and English during this period, see Christian Licoppe, *La formation de la pratique scientifique: le discours de l'expérience en France et en Angleterre, 1630–1820* (Paris: Éditions de la Découverte, 1996), 19–87. The accounts of the experiments performed by the Florentine Accademia del Cimento (roughly, "Academy of Trials"; established 1657) were entitled *Saggi di naturale esperienze fatte nell'Academia del cimento* [1666], rendered as *Essayes of Natural Experiments made in the Academie del Cimento* in the 1684 English translation.

25. Quoted in Hans Poser, "Observatio, Beobachtung," in *Historisches Wörterbuch der Philosophie*, ed. Joachim Ritter and Karlfried Gründer (Darmstadt: Wissenschaftliche Buchgesellschaft, 1984), vol. 6, cols. 1072–81, on col. 1073.

26. See, for example, [Dumarsais], "Expérience," in Diderot and d'Alembert, eds., *Encyclopédie*, 6: 297–98; "Erfahrung," in Johann Georg Wald, *Philosophisches Lexicon* [1775] (Hildesheim: Georg Olms, 1968), 1082–84.

27. [Jean Le Rond d'Alembert], "Expérimental," in Diderot and d'Alembert, eds., *Encyclopédie*, 6: 298–301, on 298.

28. Joseph Priestley, *Experiments and Observations Relating to Various Branches of Natural Philosophy*, 3 vols. (London: J. Johnson, 1779–86), 2: ix. Priestley's experiments on the color of marine acid furnish a striking example of the back and forth between observation and experiment (1: 78–80).

29. One exception was vitalist medicine, whose proponents argued vehemently in favor of observation over experiment: see, for example, [Chambaud], "Observateur (*Gram. Physiq. Méd.*)," in Diderot and d'Alembert, eds., *Encyclopédie*, 11: 310–13, on 312–13.

30. Brian W. Ogilvie, *The Art of Describing: Natural History in Renaissance Europe* (Chicago: University of Chicago Press, 2006), 34–36, 46–44 and passim.

31. Charles Bazerman, *Shaping Written Knowledge: The Genre and Activity of the Experimental Article in Science* (Madison: University of Wisconsin Press, 1988).

32. Staffan Müller-Wille, *Botanik und weltweiter Handel: Zur Begründung eines natürlichen Systems der Pflanzen durch Carl von Linné, 1707 78* (Berlin: Verlag für Wissenschaft und Bildung, 1999).

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PART TWO

Observing and Believing: Evidence

All scientific observation ultimately aims to provide evidence: for the bare existence of phenomena, for or against a hypothesis, for the significance of this or that detail in the broader context of inquiry. But in order to achieve any of these evidentiary goals, observation must first be conceptualized as a distinctive way of acquiring knowledge, with its own methods, guarantees of reliability, and functions vis-à-vis other modes of investigation. The essays in this part explore all of these aspects of observation as evidence, from the mid-seventeenth to the early twentieth centuries, in medicine, natural history, and physics. In each of these essays, the evidentiary weight of the observation is intertwined with the personal credibility and skills of the observer: across centuries and disciplines, the persona of the virtuoso observer, open-eyed and open-minded, attentive, and preternaturally patient, persists.

Domenico Bertoloni Meli's essay on the observation of color in seventeenth-century physiology, chemistry, and natural history is situated in the context of early modern medicine. To embrace observation as a source of evidence at first overwhelmed naturalists with an embarrassment of riches: out of the myriad phenomena, what was worth observing, as evidence of what? This was as much a philosophical as a practical problem: new doctrines of primary and secondary qualities espoused by Galileo and others, as well as chemical and medical experiments, sowed doubts in the minds of Pisan physicians Giovanni Borelli, Carlo Fracassati, and Marcello Malpighi as to the value of color as evidence of the essential properties of substance, despite a venerable medical tradition of using color to diagnose disease and ascertain the composition of blood. Observations on caked blood indicated that color could be all too easily altered by exposure to air or water. Yet the very same observations that had discounted the evidentiary import of color for the Pisans were eagerly received by their colleagues in Oxford and London as sterling evidence for the role of air in the lungs. Converted from his earlier skepticism about the evidentiary value of color, Malpighi went on to observe colors with exquisite attention to variety and nuance in his study of silkworms.

Michael D. Gordin raises the question of how the evidence of observation commands belief in starkest form in his essay on the exuberant nineteenth-century Russian naturalist and man of letters Nikolai Petrovich Vagner. Vagner made a career and forged a persona as the indefatigable, attentive observer of highly implausible phenomena. Who would have believed that some insect larvae could contain yet another generation of larvae, without fertilization or even maturation? Certainly not the editor of the German scientific journal to whom Vagner submitted his sensational article, at least not until he had himself replicated the observations and the leading authority on embryology had endorsed them. Flush with victory, Vagner tested his observational prowess on still more improbable effects: the apparitions from the beyond reported in Spiritualist séances. Armed with his scientific reputation as an ace observer and a razor-sharp polemical pen, Vagner was undaunted by the skepticism of colleagues like Mendeleev and the jeers of literati like Tolstoy. This was evidentiary observation simultaneously at its most humdrum and most radical: unbiased patience and perspicacity would, Vagner was convinced, reveal brave new worlds.

Charlotte Bigg's essay also presents a case of virtuoso observation: French physical chemist Jean Perrin's attempts to observe the ultimate unobservable, the atom, in his study of Brownian motion. Although Perrin's apparatus and pencil-and-paper methods were low-tech in comparison to those used by other laboratories to test Einstein's claim that Brownian motion could prove the reality of atoms, they were extraordinarily sophisticated in their use of statistical ideas to minimize observational error. Perrin's observational strategies also highlight the often blurred boundaries between experiment, observation, and theory in the early twentieth century: his preparations of colloid solutions were manipulative enough to qualify as "experiments," but the actual tracking of the paths of the suspended granules counted as "observation." Einstein's 1905 theoretical paper on the relationship between atomism and the measurement of Brownian motion directly inspired Perrin's efforts. All of these elements contributed to the evidentiary weight of Perrin's iconic drawing of Brownian motion.

4

The Color of Blood: Between Sensory Experience and Epistemic Significance

DOMENICO BERTOLONI MELI

Introduction: Between Anatomy and Philosophy

In his monumental *Canon*, the Persian philosopher and physician Ibn Sīnā—known in the West as Avicenna—discussed the nature and composition of blood with regard to its role in nutrition. In an important passage, he argued that blood is a humor consisting of four components, as can be ascertained by pouring the blood drawn from a patient into a vessel and observing its separation into a foamy "colera rubea," a turbid "fex" or "melancolia," a portion resembling egg white, and lastly a watery part. The first three parts are themselves humors, namely, bile, melancholia, and phlegm, whereas the last part is that which expells its excess as urine. Avicenna relied on a range of features, including color, for the identification of the blood's components. This passage attracted the attention of physicians and alchemists alike because of the importance of blood, the compound nature of the humors, and the problem of their separation. In this paper I will discuss some implications of Avicenna's passage for the nature of color and blood in the seventeenth century.[1]

In his 1651 *De generatione*, for example, William Harvey argued that blood is heterogeneous and is composed of different humors, but while the animal is alive "it is a homogeneous animate part, compounded out of soul and body"; this unity disappears in death when the soul fades away and blood decomposes into its constituents and becomes corrupted. Harvey also noticed that blood found in the lungs was especially florid, but he believed that the difference in the color of blood from arteries and veins depended on accidental circumstances, such as the size of the openings: blood squirting from a tight opening, like that in an artery, was brighter, whereas blood from a wider opening, like that in a vein, was darker. He added that between venous and arterial blood there were no physical differences and that arterial blood collected in a bowl would soon look venous.[2]

These observations and reflections on blood and its components call into question the nature of color as a tool of investigation in a number of areas ranging from chemistry to philosophy. Color is one of the most immediate sensory experiences and at the same time one of the most complex philosophical and physiological problems in sense perception. The seventeenth century was a particularly remarkable period in this regard, one that saw the crystallization of the notions of primary and secondary qualities and the publication of a number of celebrated studies and experiments on the nature of light and colors, as well as the investigation of the significance of color change in blood. This essay moves across a varied terrain conceptually and geographically: it starts by providing a brief synopsis of physical-philosophical stances on color in a few decades around midcentury, beginning with Galileo's *Assayer* (Rome, 1623) and Descartes' *Dioptrique* (Leiden, 1637). Moreover, Robert Boyle and Robert Hooke joined a chemical with a mechanistic standpoint in *Experiments and Considerations Touching Colours* (London, 1664), *The Origine of Formes and Qualities* (Oxford, 1666), and *Micrographia* (London, 1665).[3]

Anatomical investigations are particularly relevant because color enters the description of important structures and processes in the body. For this reason I will focus on a key episode, the study of color change in blood between 1659 and 1669, with special emphasis on the group around Giovanni Alfonso Borelli in Pisa, including Marcello Malpighi and Carlo Fracassati, and some scholars moving between Oxford and London, including Thomas Willis, Boyle, and Hooke. Later in his career Malpighi changed his philosophical stance on color in dramatic fashion; therefore a study of his work promises to shed light on a broad range of epistemological positions. Briefly put, at an early stage, relying on his own philosophical views and the experiments of the Cimento Academy, Borelli explained to Malpighi that color was not a useful way to explore the properties of substances. The *Saggi*, or samples of experiments of the Cimento Academy (Florence, 1667), tackled the problem of the nature of color change experimentally, discussing tests with color indicators leading Borelli to believe that colors could easily be changed and were therefore unreliable indicators of the true nature of a substance—a much more radical stance than Boyle's. As a result, in his study on lungs and respiration, Malpighi ignored color change in blood. As reported in print by Fracassati, Malpighi observed that air—among other factors, to be sure—was responsible for color change in blood, but he did not consider this change to be indicative of a corresponding transformation in its substance and therefore as a meaningful feature of respiration.

It is especially useful to contrast the works by the Pisa anatomists with the *Tractatus de corde item de motu & colore sanguinis et chili in eum transitu* [*Treatise on the Heart as well as on the Motion and Color of Blood and on the Transit of Chyle through it*] (London, 1669) by the physician Richard Lower, a treatise examining respiration—among other topics—in which color change in blood is prominently included in the title. Both Boyle and Hooke were engaged not only in philosophical and experimental reflections on color, but in anatomical investigations as well: Hooke offered a decisive contribution to Lower's work, one that Lower chose to acknowledge in print. Lower was a student and follower of Thomas Willis, a physician, anatomist, and chemist whose reflections on the nature of blood and the site where its color changed in the body proved quite influential.

In a concluding section I show that at a later stage, after having broken with Borelli and having become associated with the Royal Society, Malpighi gave increasing attention to color: not only did his description of the silkworm display a stunning sensitivity to color, but he also attributed an epistemic significance to it, since the change of color of the silkworm's eggs indicated whether they had been fertilized. I suspect that Boyle's work joined forces with Malpighi's medical background and artistic sensibility in effecting this remarkable transformation, both in the style of description and in its philosophical underpinning. This episode provides material for reflection on the nature of observation and its epistemic presuppositions and consequences.

The issue of color in philosophy, anatomy, or medicine in the seventeenth century is a huge one that cannot possibly be exhausted within the compass of a short paper, even one confined to the study of blood; therefore my aim here is limited to raising some questions and stimulating further investigations through the lens of a particularly significant episode rather than offering a comprehensive examination of the issues at stake.

Color in the Mid-seventeenth Century

I wish to open this short section by discussing Galileo's celebrated passages from the *Assayer* in which he introduced the distinction between what we can call "objective" and "subjective" qualities, later called primary and secondary. In section 48 Galileo discussed the nature of heat and then went on to argue that some qualities—such as size, motion, spatial relation to other bodies, and number—are inseparable in our mind from corporeal substance. Other qualities, however, such as tastes, odors, and colors reside only in the sensorium of the perceiving animal; if this were removed, they would disappear. Heat, according to Galileo, was one of those qualities; heat would consist in a multitude of tiny particles—the *ignicoli*—moving at great speed, which are the only entities existing independently of the perceiver; consequently there is no such thing in nature as heat independently of those who perceive it. Looking more closely at Galileo's text one notices a significant difference among the purely subjective qualities: in some cases, as with tastes, odors, and sounds, Galileo provided an explanation of their origin, such that tastes and odors are associated with the shape, size, and speed of particles entering the pores of the tongue or the nostrils. Galileo had already discussed sounds in the celebrated and much discussed fable of the cicada; whatever their specific forms of production, however, such as the vibration of a string, they stemmed from the motion of air. By contrast, Galileo left the issue of light and especially color open, arguing first that he understood very little about it and then that it would require a long time to explain the little he knew.[4]

Probably Borelli had Galileo's *Assayer* in mind when in the 1649 *Delle cagioni de le febbri maligne* he applied a similar reasoning to medical matters, pointing out that neither tastes, nor smells, nor colors are reliable or indeed viable ways to distinguish poisons from healthy foods: as we shall see, for Borelli those qualities could be changed without a change in substance and therefore all we can do to find a substance's properties—medical or otherwise—is to study its effects.[5]

I believe that Descartes too was familiar with Galileo's *Assayer*, which was published in Rome just before his arrival in the eternal city; several passages from the 1644 *Principia philosophiae* echo quite closely Galileo's text. Descartes retained Galileo's dichotomy between "objective" and "subjective" qualities, namely, qualities like size, shape, and motion on the one hand, and colors, tastes, and odors on the other, arguing that there is nothing in nature that corresponds to color as such independently of the perceiving subject. Light played a major part in Descartes' natural philosophy, so much so that his treatise *Le monde* was originally conceived as a treatise on light: although he treated the problem in different ways depending on the problem he was addressing, overall he understood light in terms of pressure from particles of a fluid. Already in the *Dioptrique* Descartes moved one step further than Galileo in providing a mechanical account of the corresponding quality of colored particles, namely, their spin. According to his view, the different rotational speed of light particles makes us see color: red for the greatest spin and blue for the smallest. Descartes, too, dealt with the color of blood, framing his study in a neo-Galenic fashion to try to explain how white chyle is transformed into red blood in the liver: his answer was that just as the white juice of black grapes is turned into red wine, so chyle passing through the pores of the liver "takes on the color, and acquires the form, of blood," a comparison borrowed from Galen.[6]

Moving to England, we find writings of Boyle and Hooke especially pertinent to the philosophy of color: both were engaged in anatomical experiments on respiration and the reasons for the change of color of blood. Several documents from that period testify to Boyle's interests in the matter: for example, he wished to investigate the differences between arterial and venous blood, as well as their color, taste, odor, and specific gravity. Boyle's *Experiments Touching Colour* claim that color and color changes are due to the change of the mechanical texture of bodies, especially their surfaces. Boyle's argument that color is related to the roughness of surfaces led him to accept the report by Sir John Finch that John Vermaasen, a blind man in the Netherlands, was able to distinguish colors by touch, a report savagely lampooned by Jonathan Swift in *Gulliver's Travels.* According to Vermaasen, black and white had the roughest surfaces or the "most asperous," while red and blue were the least rough or "asperous," the full range going from black to blue. It is of particular interest that Boyle reported several experiments with color indicators, much like the Cimento Academy, though he did not reach Borelli's radical conclusion that color is ultimately unrelated to the nature of substance. Rather, he showed a typically restrained attitude to formulating a general theory. Boyle, however, did surmise that colored bodies appear opaque but may in fact consist of transparent corpuscles. In *The Origine of Formes and Qualities* he argued that colors are not inherent qualities of a body due to its substantial form; rather, they derive from the mechanical texture of its minute parts and can be easily changed by changing that texture. The very first of ten experiments in his book involves the dissolution of camphor into oil of vitriol, producing a deep yellow-red color in striking contrast to the colorless ingredients; add water to the solution, however, and the solution turns colorless and camphor regains its piercing odor that it had lost in its dissolution. Boyle's *Memoirs for the Natural History of Humane Blood* (London, 1684), published twenty years later, testifies to his lasting interest in blood. The book was dedicated to John Locke, who in the mid-1660s was interested in the color change of blood and believed it was due to the niter in the air.[7]

Robert Hooke, too, indulged in speculations on light and color in several bodies, such as Muscovy glass—a mineral composed of tiny flakes with varying optical properties as they got smaller—and a diamond presented by Mr. Clayton to the Royal Society, which produced light when rubbed, struck, or beaten in the dark, a matter discussed by Boyle too. Hooke concluded from painstaking examination of the behavior of Clayton's diamond that light resulted from a very short vibratory motion. While examining the color of bodies, Hooke argued that even those appearing opaque are composed of tiny transparent elements, hence the importance of his study of Muscovy glass, which thus appeared not as a peculiar exception but as exemplary of the structure of bodies.[8]

Whatever the specific view about the color of bodies, both Boyle and Hooke, unlike Borelli and his followers, did not dismiss the significance of color and color change altogether. Rather, they adopted a more flexible approach whereby color did have some correlations to bodies, if not strictly to their material substance, at least to its arrangement in the texture and especially to the surface of bodies.

The Pisa Scene: Borelli, Malpighi, and Fracassati

Between 1656 and 1667 Borelli held the chair of mathematics at Pisa University. Although traditionally this position was not especially highly remunerated or of very high rank, Borelli's close contacts with the Medici rulers and their academy enabled him to enjoy an unusually high standing at the university, where he was the "philosophical" and "political" leader of a group that included at different times the professors of medicine Malpighi (1656–59) and Fracassati (1663–68). Let us focus on Malpighi first: his position was especially interesting because, besides being an anatomist, he was also a professor of the practice of medicine and a physician, and this adds another dimension to the issue of color. Although the venerated practice of uroscopy—involving the careful inspection of urine, including its color—may have fallen into disuse, color remained a key feature of medical diagnosis as a meaningful indicator of health and disease: jaundice, for example, relied on the observation of a yellow tinge in some solid and fluid parts of patients. Therefore it was natural for Malpighi to pay attention to the color of body parts, as he did in a letter to Borelli in 1660 in which he commented on the changing color of some callous particles—possibly of blood—extracted from a patient and friend afflicted by pain in the articulations; he reported that those particles turned from white into a rotten color, *color di marcia*, or the color of rotten or putrid matter. In a revealing reply, Borelli stated that the change of color of those callous particles was not a matter of great interest, "knowing that the colors of things can be very easily changed."[9]

In a later letter Borelli discussed the issue of color at greater length in a medical and therapeutic context: the topic of discussion was the nature of some fevers afflicting Pisa and the search for the best therapy. Since postmortems revealed an excess of bile in the victims' cadavers, bile played a major role in his and Malpighi's reflections. Malpighi had argued that no fever arises in those cases in which bile is mixed with blood, as the example of jaundice shows. In his reply Borelli questioned whether bile is truly to be found in the arteries and veins of patients; he recalled having tested by means of a piece of paper the urine of a patient with exceedingly yellow face and eyes and found that the paper did not turn yellow. He pointed out that since nature can change colors very easily, it would be conceivable that jaundice could be due to causes other than bile. Thus in this instance Borelli still considered color as a valid symptom in that the yellow face and eyes of the patient indicated jaundice, yet he questioned the traditional causal mechanism linking the appearances to bile. At this point he embarked on a chemical-philosophical excursus on color—and taste too—arguing that colors can be changed without a corresponding change of "substance," by which he meant the constituent matter of the body. He mentioned the experiments performed at the Cimento Academy and later published in the *Saggi.* It is to these experiments that we now turn.[10]

Study of color change occupied a small part of the agenda of the Cimento Academy around 1660. Its activities aimed to promote experimental philosophy without an explicit philosophical agenda for or against novelties in order to present irrefutable experimental results and avoid sterile philosophical disputes. As in many other cases, however, it seems likely that the experiments on color indicators did follow a philosophical agenda in challenging the view that colors were related to substantial forms, in that colors could be easily changed without changing the substance generating them in any meaningful way. This way of proceeding by allusions or coded messages was standard at the Cimento. The *Saggi* of the Cimento states that, the academy truly did not wish to meddle with color changes studied by the chemists, but the members investigated some of those changes in connection with their study of the properties of mineral waters. The third experiment offers an example:[11]

> Tincture of red roses extracted with spirit of vitriol becomes a very beautiful green when mixed with oil of tartar. A few drops of spirit of sulphur make it all bubble up into a bright red foam, and it finally returns to a rose color without ever losing its scent and can no longer be changed by oil of tartar poured into it.

The text specifies that ten or twelve drops of oil of tartar and of spirit of sulphur in half an ounce of tincture of roses are enough to achieve the desired result. Although at first sight this and other similar experiments seem like neutral factual reports, Borelli's correspondence reveals a different side of the story. Borelli drew the conclusion that there was no fixed relation between color and the substance generating it: a few drops of oil of tartar or spirit of sulphur could turn a much larger amount of liquid obtained from red roses and spirit of vitriol from red to green and back to red. The red liquids, however, had such different properties that the first had a pleasant taste and was innocuous, whereas the final product could have proved lethal. Similarly, tastes too could be deceptive, as he had just experienced by noticing the similarity between two fluids with different properties, such as the brine in which olives macerate and that found in the stomach of fishes, or milk and the liquid found in the stomach of hawks: whereas the digestive fluids were very corrosive, the others were innocuous. Hence nature could easily change colors and tastes without changing a body's substance; conversely, it could make very different substances look and taste similar: changes in color or taste were unrelated to substantive transformations.[12] As we are going to see, in this tradition joining subtle philosophical thinking with the latest experimental results, the far less dramatic color change in blood from dark to bright red and back to dark seemed unworthy of serious investigation; the change may be attributed to the rearrangement of blood components in the lungs, but the investigators in Borelli's circle did not test where and in what circumstances it occurred.

These observations about color had an anatomical counterpart in the study of blood carried out by Borelli and Malpighi when they overlapped as professors at Pisa University between 1656 and 1659. At the time Malpighi planned a dialogue in Galilean form dealing with medical and anatomical issues; although in the end that work was not published and is now lost, in 1665 Malpighi incorporated portions of it into a *Risposta* he drafted against some traditionalist Galenist physicians at Messina, which was published only posthumously in 1697. The Galenists argued that even barbers know that blood contains bile, phlegm, and melancholia or black bile, as can be seen in blood let from a healthy person, a reference dating back to Avicenna's *Canon.*[13]

Malpighi disagreed with the Galenists and challenged their interpretation: both taste and odor of the various parts are intermingled and therefore they cannot be easily distinguished, so much so that even the bitterness of bile is overshadowed by the sweetness of blood. Thus color turns out to have a crucial role: the components of blood could allegedly be detected by visual inspection by taking some congealed blood, showing a bright red portion at the top and a darker, heavier portion at the bottom: the former could be identified as rich in bile—Avicenna's "colera rubea"—that is yellow and also lighter in weight and therefore rises to the top; the latter could be identified as melancholy—Avicenna's turbid "fex." It is at this point that Malpighi deployed his philosophy of color derived from Borelli, arguing that being dark or bright are "accidents" (*accidenti*) unrelated to the change of substance or its "mixing" (*temperie*). In fact, they can be repeatedly reversed since they depend on causes that have nothing to do with what the Galenists think. Malpighi went on to report a number of experiments on the caked blood intended to show that color is not a valid indicator of the nature of a substance. He started by arguing that putting some salt on the dark portion of blood will turn it very bright red; yet the earthy nature of salt ought to have turned it dark like melancholia, according to the Galenists. The simplest experiment consisted in turning upside down the caked blood and then observing the inversion of colors, the dark portion at the bottom turns bright red once it is at the top and, vice versa, the bright portion at the top turns dark once it is at the bottom. By putting the cake under water, even the bright red portions turn entirely dark. Malpighi was fully aware of the role of air in the changing color of blood: in another passage dealing with pulmonary disease, he argued that blood spits are bright red because blood is mixed with air, whereas blood in the rest of the body can be quite different in color and texture.[14]

The blood experiments carried out at Pisa between 1656 and 1659 were first reported in print by Malpighi's friend and colleague Carlo Fracassati in his 1665 treatise on the brain, *De cerebro*, with a clear attribution to Malpighi. Fracassati's treatise is a rather disorganized work covering nearly twenty double-column folio pages in the 1699 edition from the *Bibliotheca anatomica*. His report occupies just a few lines and follows closely the style of argument we have seen above, including the challenge to the link between dark blood and melancholia; Fracassati, too, explicitly mentioned the role of air in the changing color of blood from dark to bright red.[15] Yet his acknowledgment did not imply the recognition of the anatomical significance of that transformation: Borelli and his group thought that the substance of blood remained the same whether it was mixed with air or not. Indeed, in a later passage dealing with the changing color of blood mixed with various substances Fracassati explicitly warned readers not to trust colors, "*ne crede colori*," as he put it.[16] Thus it would be erroneous and anachronistic to attach great significance to Fracassati's report, as if it had claimed that Malpighi had discovered that exposure to air turns venous blood into arterial and, conversely, privation of air turns arterial blood into venous. In fact, Borelli's correspondence and the study of the Cimento experiments offer a revealing and entirely different context to interpret Malpighi's and Fracassati's claims: color is not a valid indicator of substance and—one may add—it is therefore legitimate to ignore it in the study of nature and in anatomical investigations in particular. Moreover, mixing with air was only one among several processes that turn the color of blood bright red, besides sprinkling it with salt, for example.

We are now equipped to attempt a fresh reading of Malpighi's celebrated *Epistolae* (1661) on the lungs, in which he announced the discovery of their microscopic structure. By studying the lungs of frogs, whose microstructure is easier to detect, he showed that the lungs were not spongy as was traditionally believed, but rather consisted of a series of smaller and smaller cavities or *alveoli* delimited by membranes covered by a network of blood vessels. Malpighi was able to see the anastomoses or junctions between arteries and veins, and also venous and arterial blood flowing in opposite directions. These findings provided direct visual proof of Harvey's circulation and showed that blood always flows inside blood vessels, thus closing the missing link in Harvey's system. Malpighi, however, did not stop with structure and tried to provide an explanation of the purpose of respiration, one directly influenced by Borelli. Their account has been aptly described as purely mechanical in that they did not attribute any role to chemistry. The role of the lungs was simply to mix blood with chyle so that it could nourish all the parts of the body. The motion of inflation and deflation of the lungs allows them to mix the blood better. This account soon proved grossly inadequate, since it was shown that animals could not breathe the same air but need fresh air to enter their lungs. More significantly from our perspective, in line with Borelli's views as highlighted in the contemporary correspondence and with the experiments of the Cimento, Malpighi paid no attention whatsoever to color change of blood in respiration. The finding that air changes the color of blood from dark to bright red seemed irrelevant, since Malpighi had shown that blood is never in direct contact with air but flows always inside blood vessels. Thus Malpighi did not see a connection between the color of blood in the lungs and the purpose of their structure, or to put it another way, he did not see a connection between the color of blood and respiration.

The English Scene: Boyle, Hooke, and Lower

It may seem peculiar to start a brief account of the English scene from a review of Fracassati's experimental report of Malpighi's observation; however, since there was a fundamental shift in the way the Pisan experiment was interpreted at Oxford and London, one may well argue that the same observation played a radically different role. Even the name differed: despite Fracassati's clear attribution to Malpighi, the experiment became known in England as "Fracassati's." In a brief report in the *Philosophical Transactions*, Henry Oldenburg teased out of Fracassati's disordered work the few lines dealing with color change in blood. Oldenburg reported that when blood has turned cold in a dish, the portion at the bottom is darker than at the top. The standard explanation that this observation would reveal the presence of melancholia in blood, however, was disproved by exposing blood to air, showing that blood becomes a florid red, "An experiment as easie to try, as 'tis curious."[17] As the admirable work by Robert Frank has shown, Fracassati's report reached England in the midst of a flurry of investigations on respiration and exerted a considerable impact. In a letter to Oldenburg of 26 October 1667, Boyle gave guarded approval to the truth of the experiment and Fracassati's interpretation that air plays a role in the color of blood. They differed significantly, however, in their interpretation of the significance of this observation: by now Avicenna's original report had underwent a major reconceptualization, from a proof of the composite nature of blood to evidence of the role of air in respiration.[18]

We now take a step back in time to consider a major figure, the Oxford Sedleian Professor of Natural Philosophy Thomas Willis. Willis combined medical, chemical, and anatomical interests with a sympathetic attitude to Descartes' mechanical understanding of nature, making the notion of fermentation a hallmark of modernity. In *Diatribae duae medico-philosophicae* (London, 1659), an influential treatise dealing with fermentation and the nature of fevers, Willis had provided a chemical reason for the change of color of blood, arguing that this phenomenon resulted from the combination of the sulphurous particles of blood with those of salt and spirit. In line with Descartes and other Continental investigators, he located the site where blood changes color in the heart: thus, unlike Borelli, he attributed a significant role to the change of color of blood. Willis too referred to the stratification of blood components once blood cools in a bowl, much like milk and wine; blood separates into a purer sulphureous part at the top, which in healthy individuals is bright red, and a thicker darker part at the bottom. We encounter here exactly the classical observation Malpighi and Fracassati had reported.[19]

Following his teacher Thomas Willis, Lower attached great importance to blood, its fermentation, and color change. In a letter to Boyle of June 1664, Lower discussed the reason for the difference of color between arterial and venous blood, arguing that arterial blood is bright red, whereas blood that has circulated through the muscles and thereby lost many particles before reaching the veins is darker. Unlike Harvey, Lower could confirm that blood let from the artery of a dog and kept in a "porrenger" or a small bowl remained bright red for one or two days, whereas blood let from a vein of the same dog remained dark, except for a thin layer at the top.[20]

In October and November 1664 the Royal Society debated whether air enters the body through the lungs. The fact that during the vivisection of a dog it was possible to revive the heartbeat by blowing air into the *receptaculum* *chyli*, whence it reached the heart through the thoracic duct, suggested a role for air in heart pulsation. On November 7, Hooke, together with Oldenburg and Jonathan Goddard, a former student of Francis Glisson at Cambridge, inserted a pair of bellows into the trachea of a dog and inflated its lungs. Opening the thorax and cutting the diaphragm, Hooke observed the heart beating regularly for over one hour as long as air was in the lungs. Hooke could not determine whether air entered the lungs, but he could establish that the motion of the heart was related to the inflation of the lungs, even though the two were not synchronous.[21]

The English anatomists soon elaborated on this experiment and went beyond this initial result relying on Lower's skill with vivisection. Emphasis on experimentation was a hallmark of both the Royal Society and the Italian Cimento Academy, but in this case the English investigators asked questions about issues the Italians had deemed of no significance, such as the color of blood. Initially, in *De febribus vindicatio* (London, 1665), a defense of Willis's *Diatribae* against the attack by the Bristol physician Edmund Meara, Lower had claimed that blood changes color in the heart as a result of a ferment in the left ventricle and also that blood in the lungs was venous, probably also because in his early trials the animal's lungs had collapsed and were empty of air; but regardless of where the color of blood changed, the very fact that it changed was deemed significant. Lower described his experiment in the same year in which Fracassati reported Malpighi's observations on the role of air in changing the color of blood—a finding still unknown to Lower.[22] But additional experiments refuted his initial view.

On 10 October 1667 Hooke and Lower performed an experiment at the Royal Society analogous to that of 1664, but this time they relied on two pairs of bellows instead of one, producing a continuous airflow. An incision in the pleura allowed air to exit the lungs, which thus remained inflated. In this way the animal was kept alive without motion in the lungs, thus showing that their motion was not required to keep the animal alive. By cutting a portion of the lungs, they could observe the blood moving through the lungs whether they were inflated or not.[23] This experiment refuted the purely mechanical view of respiration put forward by Malpighi and Borelli and later adopted by others in England.

Lastly, Hooke and Lower performed yet another two-part experiment on a dog. First, in the initial vivisection, they closed the trachea and showed that the blood coming from the cervical artery, after the blood had gone through the left ventricle of the heart, was venous. Thus the change of color of blood did not occur in the heart. Then the animal died, and they performed the insufflation experiment with the two pairs of bellows mentioned above, managing to obtain arterial blood from the pulmonary vein. Thus it was not the motion of the lungs, or a ferment in the heart, or the animal's heat that was responsible for the change of color of blood in the lungs, but only air, in line with Fracassati's report but against the view of Malpighi, Borelli, and Fracassati himself. This experiment strikes me as being especially significant in showing that the change of color of blood was not due to the soul or one of its faculties, because the animal was dead; although Hooke and Lower followed Harvey with respect to their acceptance of the circulation and emphasis on vivisection experiments, in this respect one wonders what they would have made of Harvey's belief in the soul and its location and role in blood.[24]

Thus in Borelli's group the experimental evidence with color indicators and the anatomical evidence that blood flows always inside blood vessels joined forces in denying a meaningful role to the change of color of blood, even after the realization that air was one of the factors responsible for this change. By contrast, in the group around Willis and Boyle the medical and chemical traditions joined forces in attributing a significant role to color and the change of color of blood, a phenomenon that anatomical experiments located in the lungs.

Finale: Malpighi and the Colorful Silkworm

Matters did not end there for Malpighi. Based on a range of sources, he revised his views on respiration, eventually accepting that a portion of air enters the blood through the lungs and plays a chemical—as opposed to a purely mechanical—role in respiration. In *De polypo cordis*, for example, first published in 1668, Malpighi studied the composition of blood starting from pathology, notably the polyps found during postmortems in the heart of deceased patients. In this work he dealt with the color of blood from a different perspective: observing blood through the microscope, Malpighi noticed that the red coloration was due to a large number of "red atoms," while the rest consisted of a network of whitish fibers. Malpighi put his finger on the color dichotomy between the macroscopic and the microscopic world, whereby what appeared on unaided visual inspection as a homogeneous red humor was shown by the microscope to be quite different; he then commented on the stratification of coagulated blood. Thus he relied on microscopic observations to reassess his own 1659 experiment and observation based on Avicenna and reported by Fracassati in 1665. He attributed the black color at the bottom not to melancholia—as some had believed—but to the great abundance of those particles he had called "red atoms," which he claimed returned to purple by a mere change of position. In a later passage of *De polypo* *cordis*, Malpighi spelled out that the lungs filter from the air a "salt of life" that awakens—"*suscito*"—the red potion of blood: thus the changing color of blood emerged as a significant feature of respiration.[25]

At this point, rather than following Malpighi's attempts to salvage what he could from his earlier views, I wish to shift to another topic. In 1669, the same year in which Lower published *De corde*, the Royal Society published Malpighi's treatise on the silkworm, *De bombyce*, and elected its author a fellow. By that point Malpighi had broken off his friendship and correspondence with Borelli and had departed in key respects from his former mentor's philosophical stance, notably with respect to color. His publications show a growing interest in color, but it is with *De bombyce* that Malpighi let his sensitivity to color burst forward in dramatic fashion. The reader of *De bombyce* is struck by a different Malpighi from that of the *Epistolae* on the lungs: now color—including many shades of gray—takes on a significant epistemic role in the description of silkworms, revealing an author with a striking sensitivity to nuanced shades and a remarkable ability to describe color in words. Color had become an integral part of Malpighi's descriptions, not only as a source of pleasure but also as a philosophical feature of the object under investigation. The fact that Malpighi was an art collector and enthusiast is related to his ability to observe and describe nature.[26] From the first pages of *De bombyce* we read of eggs turning from *violacea* to *caerulea* or light blue, then *sulphurea* and thereafter *cinerea* or ash-colored. Nor are Malpighi's identifications of color approximate: on one single page we find him distinguishing between *cinereus* or ash-colored and *fuliginosus* or soot-colored in describing the color of the just born silkworm, a color that soon turns into *perlatus* or pearl; the head is *coracinus* or raven black; the hairs and legs are *ziziphini* or jujube-colored. Elsewhere Malpighi describes the color of the silkworm as *achatis* or agate in those parts free of folds, and *argenteum* or silvery elsewhere. The silk thread is *luteus* or *auratus*, yellow or golden, or also *subalbus* or whitish with *sulphuris tinctura* or sulphur shade. Malpighi's sheer delight in describing and his remarkable sense of color are striking. Only in this way can we explain his extraordinarily nuanced descriptions. We are also reminded of his artistic interests, in which color played a major role. In an exactly contemporary letter of 24 November 1668 to the noted Sicilian collector Antonio Ruffo, who owned paintings by Rembrandt including *Aristotle with a Bust of Homer*, now at the Metropolitan Museum in New York, and *Homer*, now at the Mauritshuis in The Hague, Malpighi provided a rich account of artistic news about recent acquisitions and prices. He regretted that a fever—probably the same that in his *Vita* he attributed to excessive work on the silkworm—had prevented him from going to Parma and Correggio to see works by "Correggio e Parmigianino"; he did go to Mirandola, however, where he saw a nude Venus by Titian with "mezze tinte di Paradiso," or heavenly halftones. These observations on Malpighi's language and artistic interests, especially about color, go hand in hand with Matthew Cobb's attribution of the watercolor of the silkworm now at the Royal Society to *De bombyce*, since the drawing and especially the color range correspond remarkably to a development stage described in the text: Cobb included the color reproduction of the watercolor in his article. It is reasonable to surmise either that Malpighi himself was responsible for the watercolor, or that the artist who executed it worked directly under his supervision. Here too the letter to Ruffo proves useful, since Malpighi states that in the summer of 1668 he had employed a young painter "per dessegnarmi alcune cosette," to draw for me a few little things, and also to make copies of paintings by members of the Carracci family—Ludovico, Agostino, and Annibale. The young painter executed a few little things for Malpighi exactly at the time of his most intense work on the silkworm; thus it seems plausible that Malpighi used the same painter to help him draw and color the silkworm and make copies of paintings by the Carraccis.[27]

Color was not just a pleasurable appendage to the treatise: Malpighi identified the significance of color differences of eggs from *violacea* or purple to *sulphurea* or pale yellow as an indication of whether fertilization has occurred. He also made an attempt at artificial insemination by sprinkling male semen on the eggs, but his experiment failed and the eggs remained sterile, as testified by the lack of color change.[28]

This brief excursus has uncovered profound links among views about color and rival philosophical, anatomical, medical, and chemical perspectives. Sense perceptions and observations were mediated by deep-rooted and radically different philosophical positions in the process of observation: Borelli and his group—notably Fracassati and, for a while, Malpighi—downplayed the role of color, while Fracassati went so far as to warn readers not to trust color, "*ne crede colori.*" By contrast, Willis, Boyle, Lower, and Hooke adopted an approach according to which color appeared related to at least some properties of a substance and was therefore worthy of attention.

Even such an apparently straightforward and simple observation as the change of color in blood has required unraveling a complex web of philosophical opinions and chemical experiments. Malpighi's stance is especially revealing because he crossed boundaries in dramatic fashion: his initial tendency—probably stemming from his medical training—was to consider color as a significant diagnostic sign; following Borelli's prodding, color was then ignored in his investigations of the structure of the lungs and respiration, only to reemerge following his break with Borelli around 1667–1668. Malpighi's attention to color burst forth in all its esthetic nuances and philosophical significance in the study of the silkworm and the fertilization of its eggs, published by the Royal Society in 1669, and remained a feature of his views on nature until the end of his life as pontifical archiater.

Notes

1. Avicenna, *Canon* (Venice: In edibus Luce Antonij Junta, 1527), 7r; William R. Newman, "An Overview of Roger Bacon's Alchemy," in *Roger Bacon and the Sciences*, ed. Jeremiah Hackett (Leiden: Brill, 1997), 317–36.

2. William Harvey, *Exercitationes de generatione animalium* (London: Typis Du Gardianis, impensis Octaviani Pulleyn, 1651), trans. with an intro. and notes by Gweneth Whitteridge as *Disputations Touching the Generation of Animals* (Oxford: Blackwell, 1981), 254–55; Robert G. Frank, *Harvey and the Oxford Physiologists* (Berkeley: University of California Press, 1980), 40–41, 205–6.

3. Alistair C. Crombie, "Le proprietá primarie e le qualitá secondarie nella filosofia naturale di Galileo," in *Galileo*, ed. Adriano Carugo and Paul Tannery (Milan: ISEDI, 1978), 207–37. Surprisingly, William F. Bynum and Roy Porter, eds., *Medicine and the Five Senses* (Cambridge: Cambridge University Press, 1993), ignores color.

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5. Giovanni Alfonso Borelli, *Delle cagioni delle febbri maligne della Sicilia. Negli anni 1647 e 1648* (Cosenza: Gio. Battista Rosso, 1649), 143.

6. René Descartes, *Treatise on Man* (Cambridge, Mass.: Harvard University Press, 1972), 9, see also n.19; it is noteworthy that Descartes uses the Aristotelian notion of "form" here. Galen, *On the Usefulness of the Parts of the Body*, 2 vols., trans. and intro. by Margaret Tallmadge May (Ithaca: Cornell University Press, 1968), 1: 205–6; A. I. Sabra, *Theories of Light from Descartes to Newton*, 2nd ed. (Cambridge: Cambridge University Press, 1981), 65–68; John Cottingham, "Descartes on Color," *Proceedings of the Aristotelian Society* 90 (1989–90): 231–46; Stephen Gaukroger, *Descartes: An Intellectual Biography* (Oxford: Oxford University Press, 1995), 158–64, 262–69, 345–46.

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8. Robert Hooke, *Micrographia* (London: John Martyn and James Allestry, 1665), 47–79. Sabra, *Theories of Light from Descartes to Newton*, 187–95; Shapiro, *Fits, Passions, and Paroxisms*, 99–105.

9. Borelli to Malpighi, Pisa, 5 March 1659 [*more pisano* = 1660], cited in Domenico Bertoloni Meli, "Additions to the Correspondence of Marcello Malpighi," in Bertoloni Meli, *Marcello Malpighi, Anatomist and Physician*, 275–308, on 281–82. On the role of the senses in medical diagnosis, see Bynum and Porter, eds., *Medicine and the Five Senses*; Tobias Heinrich Duncker, "'Wie nämlish könnten diese einander gleich sein . . . ?' Zur Hermeneutik farblicher codierung in der antiken Medizin," *Farbe, Erkenntnis, Wissenschaft. Zur epistemischen Bedeutung von Farbe in der Medizin*, ed. Dominik Gross and Tobias Heinrich Duncker (Berlin: Lit Verlag, 2006), 29–38.

10. Borelli to Malpighi, Pisa, 20 Dec. 1661, in Marcello Malpighi, *Correspondence*, ed. Howard B. Adelmann, 5 vols. (Ithaca: Cornell University Press, 1975), 1: 105–9, on 107–8; in reply to Malpighi's undated letter, 1: 104–5. See also Nicolaus Steno, *Observationes anatomicae* (Leiden: Apud Jacobum Chouët, 1662), paragraphs 33–34.

11. W. E. Knowles Middleton, *The Experimenters* (Baltimore: Johns Hopkins University Press, 1971), 234–37, on 234–35; see also 362, 364. As to Aristotle's views on color, see *Categories*, 9b10–32; *Metaphysics*, 1007a31–3; Boyle, *Works*, 4: 150, 152; Domenico Bertoloni Meli, "Authorship and Teamwork around the Cimento Academy," *Early Science and Medicine* 6 (2001): 65–95; William Eamon, "Robert Boyle and the Discovery of Chemical Indicators," *Ambix* 27 (1980): 204–9.

12. Borelli to Malpighi, Pisa, 20 Dec. 1661, in *Correspondence*, 1: 105–9, on 107–8.

13. The author of the tract by the Galenists, *Galenistarum triumphus* (Cosenza: Apud Io. Baptistam Russo, 1665), is Michele Lipari. The text is reproduced from a manuscript by Corrado Dollo, *Modelli scientifici e filosoofici nella Sicilia spagnola* (Naples: Guida, 1984), 290–304, on 296; the unique copy of the printed version is described in Rosario Moscheo, "The *Galenistarum triumphus* by Michele Lipari (1665): A Real Edition, Not Merely a Bibliographical Illusion," in Bertoloni Meli, *Marcello Malpighi, Anatomist and Physician*, 331–35.

14. Marcello Malpighi, *Risposta*, in *Opera posthuma* (London: A. & J. Churchill, 1697), 32, 40–41.

15. Carlo Fracassati, *De cerebro*, in Daniel Le Clerc and Jean-Jacques Manget, eds, *Bibliotheca anatomica*, 2 vols., 2nd ed. (Geneva: Johan. Anthon. Chouët & David Ritter, 1699), 2: 76b: "sed tenes etiam quam male ad oculorum fidem provocent, nam inter alia color saturatus, & nigricans in sanguine, quia fundum scyphi occupavit, & ideo sanguis melancholicus habetur statim ac in lancem projicitur, & aëri inde miscetur, mutatur; scis, quomodo confertim melancholia abeat, & debeant spectatores huic insulsae operationi, quam superciliose aggrediuntur, ludibrium, si parum morentur; etenim color non idem manet, clarior, ac nitentior redditur; nonnes Achilles hic in Thersitem degenerat? Sed de his alias, dum (ne me plagii arguas) fateor tuum hoc esse inventum, & te praeunte hoc didicisse; de natura tamen sanguinis, si aliquid ab obsitis jam situ suo sententiis expectamus, decipimur."

16. Fracassati, *De cerebro*, 2: 79a, emphasis in the original: "Si arbitrarer ex colore nos posse in indicia naturae sanguinis mutuari, non omitterem, quomodo purpureus color ex digestione salium volatilium cum oleis emicet sola etiam coctione, ut in succis symphyti, pyrorum etc. quomodo ab acido hoc in sanguine contingat; sed hic recte admoneor, *ne crede colori*."

17. [Henry Oldenburg], "An Experiment of Signior Fracassati upon Bloud Grown Cold," *Philosophical Transactions* 2 (1667): 492; Frank, *Harvey and the Oxford Physiologists*, 205–6.

18. Boyle to Oldenburg, 26 October 1667, in Robert Boyle, *Correspondence*, ed. Michael Hunter, Antonio Clericuzio, and Lawrence M. Principe, 6 vols. (London and Brookfield, Vt.: Pickering & Chatto, 2001), 3: 357–59, on 357.

19. Thomas Willis, *Diatribae duae medico-philosophicae* (London: Tho. Roycroft, impensis Jo. Martin, Ja. Allestry, & Tho. Dicas, 1659), separate pagination: *De fermentatione*, 1, 10; *De febribus*, 13–15, 20; Frank, *Harvey and the Oxford Physiologists*, 165–68.

20. Lower to Boyle, 24 June 1664, in Boyle, *Correspondence*, 2: 282–91, on 288–89.

21. Frank, *Harvey and the Oxford Physiologists*, 157–59. The insufflation experiment whereby air is blown into the heart was already known to Galen and is mentioned by Harvey and Malpighi.

22. Richard Lower, *De febribus vindicatio* (London: Jo. Martyn & Ja. Allestry, 1665), 117–18; Frank, *Harvey and the Oxford Physiologists*, 188–92 (at 190), and 206.

23. Robert Hooke, "An Account of an Experiment of Preserving Animals Alive by Blowing through Their Lungs with Bellows," *Philosophical Transactions* 2 (1667): 509–16; Frank, *Harvey and the Oxford Physiologists*, 330–31.

24. Richard Lower, *Tractatus de corde* (London: Typis Jo. Redmayne impensis Jacobi Allestry, 1669), 165–67; Frank, *Harvey and the Oxford Physiologists*, 189, on Willis and Lower on Harvey and the soul, 214–15.

25. Marcello Malpighi, *Vita*, in *Opera posthuma*, 16, idem, *Opere scelte* (Turin: UTET, 1967), 193, 200–1, 212–14, 533; Domenico Bertoloni Meli, "Blood, Monsters, and Necessity in Malpighi's *De polypo cordis*," *Medical History* 45 (2001): 511–22; John M. Forrester, "Marcello Malpighi's *De polypo cordis*: An Annotated Translation," *Medical History* 39 (1995): 477–92, on 483–84; Howard B. Adelmann, *Marcello Malpighi and the Evolution of Embryology*, 5 vols. (Ithaca: Cornell University Press, 1966), 1: 196–97.

26. On physicians and art collecting in this period, see Pamela H. Smith, *The Body of the Artisan* (Chicago: University of Chicago Press, 2004), chap. 6 on Sylvius de le Boë, and "Science and Taste: Painting, the Passions, and the New Philosophy in Seventeenth-Century Leiden," *Isis* 90 (1999): 420–61.

27. Marcello Malpighi, *De bombyce*, in *Opera omnia*, 2 vols. (London: Thomas Sawbridge and others, 1686–87), vol. 2, 2 (wrongly numbered 66), 7, 20; Matthew Cobb, "Malpighi, Swammerdam, and the Colorful Silkworm: Replication and Visual Representation in Early Modern Science," *Annals of Science* 59 (2002): 111–47, on 119–21; Malpighi to Antonio Ruffo, 24 Nov. 1668, in Malpighi, *Correspondence*, 1: 388–89; Martin Kemp, *The Science of Art* (New Haven: Yale University Press, 1990), part 3; Trevor Lamb and Janine Bourriau, eds., *Colour: Art and Science* (Cambridge: Cambridge University Press, 1995); John Gage, *Color and Meaning: Art, Science, and Symbolism* (Berkeley: University of California Press, 1999); and idem, *Color and Culture: Practice and Meaning from Antiquity to Abstraction* (Berkeley: University of California Press, 1993).

28. Marcello Malpighi, *De bombyce*, in *Opera omnia*, 2 vols. (London: Thomas Sawbridge and others, 1686–87), 2: 37, 42–43; idem, *Dissertazione epistolare sul baco da seta*, in *Il bacofilo italiano*, vol. 2 (1860), 1–90, on 77–78, 87–88; Howard B. Adelmann, *Marcello Malpighi and the Evolution of Embryology*, 5 vols. (Ithaca: Cornell University Press, 1966), 2: 856–58.

5

Seeing Is Believing: Professor Vagner's Wonderful World

MICHAEL D. GORDIN

> In a word, he was a very original Cat, although he didn't like any kind of originality and persecuted it: first of all, because he couldn't differentiate at all the original from the fashionable, and chiefly because everything original, in his opinion, shielded from us everything ordinary, simple, that we should study and that demands our help.
>
> NIKOLAI VAGNER, "Who Was Kot-Murlyka?"[1]

Russian zoologist Nikolai Petrovich Vagner (1829–1909) enjoyed going for walks in the countryside armed with a clear head, a magnifying glass, and an avid sense of curiosity. You never knew what you might observe. For example, as he wrote in a German scientific article in 1863, describing one of his walks:

> In the environs of Kazan' I found on 12 August 1861 under the bark of a dead elm a group of little white worms that didn't move. Under the microscope these little worms revealed themselves to be larvae of arthropods with antennae and tracheas, in a word, insect larvae. Each of them was filled with other larvae.
>
> I believed at first that what I was dealing with was a case of parasitism, which is so common among insects. The similarity of the enclosed larvae with the enclosing ones, a similarity that extended to the chief external identifying marks, however, soon led me to the thought that I was dealing with a normal formation, not with a pathological occurrence. On the other hand this was something too unusual, that in an insect larva a second generation of larvae could develop, and only after much to-ing and fro-ing and after many investigations did I come to the conviction, supported by evidence, that I had finally found the truth.[2]

Vagner wielded his prose with agility: he moved from observation, to description, to investigation, to conviction—all in the span of a few sentences. His finding, later dubbed pædogenesis, sparked intense disbelief and some sustained controversy. In the end, Vagner was vindicated, merely by sticking to his observations, invoking distinguished authorities among eminent scientists, and publishing early and often. This was a lesson of perseverance that he would long remember, much to the detriment of his reputation in later years.

He had made a high-stakes discovery against the objections of critics, and he would continue to stalk ever more elusive and unbelievable prey.

Vagner spent his life committed to the observation of animals in all their varieties, but especially two: invertebrates and humans. There are three major ways to consider the observation of people and bugs together. The first is to observe the bugs closely and then convince people of your observations (and the implicit interpretation that goes along with them). I shall call this "observation as persuasion." The second claims that one can observe humans as part of the same natural processes as their spineless counterparts. This approach flattens distinctions in the natural world in favor of comprehensive and general laws of biology—which the naturalist of course *also* claims to "observe": "observation as generalization." The third tactic capitalizes on the "observational authority" gained from observing insects—a tricky and painstaking business—to build credit for controversial observations of human phenomena, such as paranormal occurrences at séances. Vagner never did anything by halves. He would employ all three.

This essay follows several strands in Vagner's highly idiosyncratic career: professor of zoology and comparative anatomy first at Kazan' and then at St. Petersburg University, and also an author of classic children's stories published under the persona of Kot-Murlyka (lit. "Cat-Purr"). Although the narrative is biographical in structure, its purpose is to excavate the category of "observation" for naturalists in Imperial Russia and thus add to the taxonomy introduced by Katharine Park, Gianna Pomata, and Lorraine Daston in part 1 of this volume. Among the several dominant Russian terms to describe the investigation of nature—*opyt* ("attempt," "experiment," and "experience"), *issledovanie* ("research," with the definite connotation of literally following), and *ispytanie* ("probe," bearing whiffs of putting something to trial)—only "observation" (*nabliudenie*) carries the specific sense of the *visual*, a sense of being passive, of letting nature wash over one. That is as far as the etymology takes us, and it is not very far at all.

The reason to focus on Vagner, to explore how he connected both his practices of observation and his observational narratives of the insect and the human world for his intended audiences, is that he repeatedly made explicit many of the cultural assumptions that lay behind "observation," and that at times put the category in tension with contemporary interpretations of "science." Vagner did not perceive the three observational strategies mentioned earlier as distinct; each was simply part of what it meant to "observe" in a hostile universe. For him, what linked both strategies and observations together was the ubiquity of "struggle" (*bor'ba*) in the inorganic, organic, and social worlds. It is commonplace now to emphasize the tropes of *control* in understanding experimental science, which prides itself on manipulation of an environment to heighten a single effect. Vagner, however, saw struggle as the defining feature of *observational* science as well: not only did one have to struggle to maintain clarity and focus on surprising phenomena, struggle to free oneself of preconceptions and biases, and then struggle to persuade people of the controversial reality; one also literally *observed* struggle. It was both the content and the form of what the naturalist-observer did. *Nabliudenie* was about as active as you could get.

Observation as Persuasion: Weird Sex

Even specialists on the history of the life sciences in Russia may not recall Nikolai Vagner. Although his name lightly veils his family's German ancestry, Vagner was Russian through and through. He was born in 1829 at Bogoslovskii Zavod in the region of Perm, but he moved as a child to Kazan', where his father, Pëtr Ivanovich Vagner, had obtained the chair in zoology at the local university. Nikolai received his secondary education at the second Kazan' *gymnasium* and entered the university in 1845. In 1849 he received his candidate degree and one gold medal for his thesis, "On the Best Characteristic Signs for the Classification of Insects." He then took a post as teacher of natural history and agriculture at the Nobleman's Institute in Nizhnii Novgorod, where he stayed until 1851, returning to Kazan' University for a master's degree.

From a career that began in the provinces, Vagner began to push to the metropoles. In 1855 he defended his doctoral dissertation, "A General View on Arachnids and a Particular Description of One of the Forms (*Androctoceus occitans*) Belonging to Them," in Moscow (it was later translated into Dutch).[3] In 1858 he went abroad for postdoctoral work in Giessen, and then returned to Moscow to edit the *Journal of the Moscow Agricultural Society*. He returned to Kazan' in 1861 as an adjunct in comparative anatomy and physiology and became an extraordinary professor the same year. On 9 June 1862 he became ordinary professor of zoology. He edited the *Scholarly Notes*, the official academic journal of the university, from 1862 to 1864, and from 12 May 1869 was the first president of the Society of Naturalists at Kazan' University. Vagner traveled to Europe for another academic trip in 1870–1871 and returned as professor of zoology and comparative anatomy at St. Petersburg University.[4]

To hear Vagner tell it, this training had only limited impact on his abilities as an observer. As he imagined his own student training from the perspective of the late 1880s, he and his schoolmates were not adept at observing from nature:

> We were all patriots and unconditional monarchists and were not troubled by any doubts or questions. This absence of ideal interests was expressed also in the interests of science and life. We related to science completely superficially, not from the philosophical side. We studied lectures from their formal and factual sides. We wrote everything down accurately and constantly and, it stands to reason, considered it a sin to skip a lecture of the main subject. These written lectures were almost the only sources of our knowledge.[5]

Not that we should necessarily take all this too seriously. Vagner was prone to exaggeration in all his writings, and he was very fond of dramatic revelation.

We know, for example, something more about his practices of entomological observation—practices that informed all of his graduate work at Kazan'—from the publications themselves. Consider, for example, that walk in the environs of Kazan' that yielded those observations of insect larvae. Observation was a simple affair. In the context of the vastly understudied Russian steppe and Volga regions, there were plenty of publishable zoological finds to be obtained simply by walking around with a magnifying glass. This was fortunate, because at this stage in his career, Vagner did not have access to substantial state support for expeditions or even the less-elaborate makeshift sets arranged by René-Antoine Ferchault de Réaumur in the essay by Mary Terrall in this volume. For Vagner, one needed only to be alert and to take one's eyes to where they would do the observing for you. And on that particular walk, he found something worth publishing.

He translated his original Russian article into German and sent it to the *Zeitschrift für wissenschaftliche Zoologie*, published in Munich by Karl Theodor Ernst von Siebold. Von Siebold chose to sit on the manuscript, considering the findings too implausible to publish. St. Petersburg naturalist Karl Ernst von Baer (1792–1876) later articulated this feeling of disbelief:

> One could expect that this discovery of Vagner's would create a great sensation, but also many doubts, before it was to achieve complete confirmation or refutation. That in a formed insect larva a brood of new larvae of the same sort could develop had until then never been observed; likewise, many deviations from the usual methods of reproduction in the higher animals also had never been observed in the lower orders.[6]

Only after von Siebold had found analogous phenomena (replicating Vagner's observations) outside Munich *and* received living samples from Vagner did he agree to publish the piece in 1863—with an introductory footnote explaining his delay.[7]

The findings remained controversial even as additional observations of the phenomenon poured in.[8] The dispute over the existence of these unusual *Diptera* larvae was resolved not through Vagner's persistence, or additional eyewitness evidence, but mainly through the support of the grand old man of Russian biology, Academician Karl Ernst von Baer. Von Baer published two articles supporting the discovery in the *Bulletin* of the St. Petersburg Academy of Sciences. Not only did he add his own observations of Vagner's specimens, his tremendous authority in European zoology as the world's greatest living embryologist led the Petersburg Academy to award Vagner the coveted Demidov Prize in 1863.

On the one hand, von Baer sealed the credit for the discovery for Vagner, and on the other, he completely eclipsed it. Von Baer endowed the phenomenon of sexually immature larvae reproducing parthenogenetically with its still-current moniker of pædogenesis: "Provisionally only one difference from *parthenogenesis* has revealed itself, that the newly emergent individual, from an immature and for that matter unfertilized egg, emerges as a sexually mature individual."[9] Although nationalist Soviet textbooks highlighted Vagner's role, most later accounts of the history of the phenomenon elided the Kazan' entomologist completely. Stephen Jay Gould, in the standard reference on the history of theories of reproduction, devotes much attention to pædogenesis, but never mentions Vagner. Instead, he attributes the discovery to its namer, von Baer, even though the title of the source Gould cites references Vagner prominently.[10]

Vagner, of course, had no sense of his impending marginality, and in 1865 was ready to gloat, retelling his discovery as a victory of observation:

> If each day one watches the larvae carefully with naked [*unbewaffneten*] eyes, one sees clearly that new larvae grow from these, and after 7–10 days again bring forth new larvae just as the former did.
>
> Such observations must surely be proof enough for any skeptic, as long as there arises no suspicion that the observer himself has intentionally meddled with the facts.[11]

We should note Vagner's stress on the *absence* of intervening instruments, on the importance of the trained *naked* eye as the conduit of (passive?) observations. Indeed, by the early twentieth century, it was considered so easy to find pædogenesis in the wild in various species of *Diptera* that it was touted as an excellent pedagogical tool to introduce students to fieldwork.[12] Pædogenesis had become entirely domesticated *qua* observation.

In his later entomological studies, such as his attempts to explain the coloring of butterfly wings by subjecting larvae to electric currents, Vagner again returned to the tropes of observation explicitly—even though in this instance he was dealing with a more obviously *experimental* setup, a baby step toward Perrin's devices described in the essay by Charlotte Bigg in this volume.[13] Vagner would later relate that in his sole meeting with famed French physiologist Claude Bernard, the latter expressed admiration for precisely this kind of experiment *cum* observation developed by the Russian with phrases like "zoology will only stand on real scientific ground when physiology becomes its leader."[14] (Bernard's emphatic writings on the inferiority of observation to experiment lead one to doubt the true extent of agreement between the two.)[15]

By this time, however, Vagner was less interested in being a narrow observer of insects. Instead, he shifted to grander schemes, hoping to observe even the abstract, the extremely general. As he commented in his St. Petersburg University lectures in 1879: "In the world there exists one phenomenon, which comprises in itself all of the rest. This world phenomenon, in which are assimilated all the facts observed in nature, is the gradual complexification (*oslozhnenie*) or development of everything that exists."[16] He wanted here to convince humans that they fit into the same patterns as insects. And this was not going to be just a struggle. It was to be a struggle *about* struggle.

Observation as Generalization: Where's Wallace?

Consider these three quotations by Vagner, the first drawn from a textbook for children, the second from one for adults, and the third from an editorial from his interdisciplinary magazine, *Svet*:

> Thus, you see, in nature there is a constant battle. Here one can't be weak, clumsy, unaware, clueless and lazy. . . . Couldn't one truly call this battle a *battle for life* or *for existence?* And from this battle, in the very end, there constantly emerge more perfect, more solid, and stronger animals.[17]
>
> [Nature's] goal is achieved by the battle for existence, the battle among the elements of organs, the battle among the organs themselves, the battle among organisms, finally, the battle among entire groups of organisms. Everything battles so that it can destroy everything weak, ugly, which doesn't harmonize with the environment and in general with surrounding conditions. In the strata of our planet, and maybe on other planets, there are buried many unharmonious, ugly forms, which were at their time and place made so that development would further new, more complex, harmonious forms via impossible anachronisms.[18]
>
> In the life of the world, struggle continues as an endless, central theme, diversifying itself in billions of different forms. It began from the first steps,

> from the first germs of the planetary system and, developing, moved wider, further—into the endless distance of the future.[19]

Struggle metaphors in the life sciences in the second half of the nineteenth century evoke natural selection: Darwinian evolution through Malthusian conflict. But there was a bit of a twist here: Vagner was the single prominent advocate in late imperial Russia of natural selection . . . in the form expressed by Alfred Russel Wallace, *not* Charles Darwin. This was a peculiar position, and one first needs to understand Vagner's motivations for advocating evolution, and then Russians' resistance to the same, in order to make sense of it.

After the acceptance of pædogenesis, Vagner set his sights higher. He had a dim opinion of "those scientists who do not want to see in natural history anything besides naked data and facts."[20] He exhorted an interdisciplinary meeting of Russian scientists to seek the whole picture:

> Open a book of any scientific journal and you will see that fruitful scientific works, those which really push science forward, those which expand our worldview or give results directly applicable to life, stand out like bright oases.—Meanwhile, there follows a plethora of works that comprise the material for the future growth of science. Finally, there appears a large contingent of scientific workers who, with true pleasure, gather various petty facts with the firm faith that even these will one day prove useful. One should admit that this faith is sometimes justified, but how much labor in this blind, impassioned work falls in vain![21]

In the small hive of Russian naturalists, Vagner did not want to be a worker bee. He wanted to throw his weight behind a monumental general law, recognizing that "[s]uch general laws are explicated slowly."[22] These laws were not the fruits of speculation; they were *observed realities* just like the multiplying larvae in the decayed elm stump (or like the economies observed in the essays by Harro Maas, Theodore M. Porter, and Mary S. Morgan in this volume). You just needed to know *how* to observe.

This is where natural selection comes in. The tortured history of the reception of Darwin's theory in Russia has been recounted many times. Russian naturalists were generally supportive of the idea of common descent. This can be easily observed in the publication history of the classic works: *The Origin of Species* was translated into Russian in 1864 by S. Rachinskii, with a second edition in 1865; noted physiologist I. M. Sechenov translated *The Descent of Man* in 1871, the same year as the English edition, with a second edition in 1874; *Variation of Animals and Plants*, *The Expression of Emotions*, and *The Voyage of the Beagle* all appeared in the 1870s; and between 1907 and

1909 botanist K. A. Timiriazev ("Darwin's Russian bulldog") oversaw an edition of eight volumes of Darwiniana in Russian.

Among the general enthusiasm, criticism came from two fronts. Conservative intellectuals, such as Nikolai Strakhov and Nikolai Danilevskii, attacked the theory for being irreligious, materialist, and corrosive of morals.[23] The main source of scientific objections, as detailed by Daniel Todes, was the perception among Russians that Darwin's reliance on the thinking of Thomas Robert Malthus was incorrect.[24] Alfred Russel Wallace, Darwin's rival for priority in discovering natural selection, is oddly absent in all of this scholarship.[25] And yet, if one wanted to test the resistance to Malthusianism in Russian culture, Wallace was the more Malthusian and human-directed of the two evolutionists.[26]

There were not one but three Russian editions of Wallace's *Contributions to the Theory of Natural Selection* (1870). The first appeared in 1876, edited by a man named Lindeman, who mistranslated, arbitrarily reordered chapters, and cut out the theistic conclusions.[27] The second edition came out from a certain G. B. Our Vagner did not care for this version at all, mostly because G. B. "directed toward me his astonishing grumbles that I had the insolence to demand respect for the views of scientists such as Wallace."[28] Vagner had had enough; he would put out his own translation, complete and unexpurgated, with updated footnotes, new illustrations, and an appendix that included his own views about what Wallace had gotten right and wrong.[29] In 1879 a man named Gusev published an entire book devoted to these translations and interpretations of Wallace, and he found that "only in the translation under N. P. Vagner's editorship does Wallace appear before the Russian public with his present and full form of thought on the question of the origin of man."[30]

Why so much attention to Wallace? The first reason was his strict adherence to natural selection, rejecting all vestiges of Lamarckian adaptationism, which Darwin's "pangenesis" theory of heredity in part preserved. Vagner saw pædogenesis as a consequence of a Malthusian pressure: there were not enough males to go around fertilizing eggs, and asexual reproduction by adult insects (parthenogenesis) would require resources to bring the females through gestation, so there was a selective pressure in favor of immature asexual reproduction. The second reason Vagner backed Wallace over Darwin concerns the most famous conflict between the two British naturalists: the adequacy of natural selection to explain human consciousness. Here is Wallace's judicious account of the difference:

> My view, on the other hand, was, and is, that there is a difference in kind, intellectually and morally, between man and other animals; and that while

> his body was undoubtedly developed by the continuous modification of some ancestral animal form, some different agency, analogous to that which first produced organic *life*, and then originated *consciousness*, came into play in order to develop the higher intellectual and spiritual nature of man.[31]

Wallace and Vagner both thought there were limits to natural selection, and they both found evidence for it in the same place: Spiritualism. Vagner's adherence to the doctrines of modern Spiritualism—table turning, table rapping, spirit materialization, automatic writing, and so on—will be addressed in the next section. Vagner's conversion was independent of Wallace's views on the subject, but it surely only enhanced Vagner's belief in natural selection as the best candidate for a universal law of development that the most prominent British scientific advocate of both natural selection and Spiritualism was Wallace.

For Wallace, belief in Spiritualism was crucially about *observation*:

> Each fresh observation, confirming previous evidence, is treated [by critics] as though it were now put forth for the *first* time; and fresh confirmation is asked of it. And when this fresh and independent confirmation comes, yet more confirmation is asked for, and so on without end. This is a very clever way to ignore and stifle a new truth; but the facts of Spiritualism are ubiquitous in their occurrence and of so indisputable a nature, as to compel conviction in every earnest inquirer.[32]

Wallace's observations of Spiritualism that led to his own conversion took place at séances in 1865–1866. Malcolm Jay Kottler and others have argued convincingly that this experience of supposed communication beyond the borders of death persuaded Wallace that more must be involved in the evolution of man than natural selection alone.[33] The supposed contradiction between Wallace the rigorous naturalist and Wallace the devoted mystical spiritualist has spawned the plethora of recent biographies of Wallace.[34] The same thing sparked some of Vagner's interest, and he threw himself into Spiritualism *à la* Wallace: first piggybacking on his observational authority from his entomology to argue for the validity of his findings, and then invoking Wallace (in counterpoint to von Baer's earlier intervention on his behalf) as an even *better* observer with a stronger reputation to justify his own claims.

Observational Authority: Psychic Polemic

Nikolai Vagner was an avid polemicist, but not an unfair one; he recognized that Spiritualism was particularly difficult for the arch-rationalist (and here the scientist was the prime exemplar) to swallow. In 1902, Vagner published an appropriately titled monograph, *Observations on Spiritualism*, which attempted to explain how difficult these phenomena were to observe:

> Here the conditions of observation are so capricious (i.e., varied and elusive) that it is almost never possible to predict that such and such a phenomena will appear and such and such an experiment will work. . . . The main reason for this is 1) the unusual complexity of these phenomena and 2) the impossibility of knowing and studying them as a consequence of the inadequacy and limitations of our own organism.[35]

Anyone interested in Spiritualism required special instruction in how to conduct inquiries into the relevant phenomena: "In all these cases [of dark or dim rooms] I advise people who want to be convinced of the reality of these phenomena to maintain composure, patience, and not to arrive at a definitive conclusion after the first séances in which they have participated. Only after long and careful pondering can the observer arrive at the true conclusion."[36] Fundamentally, the observer *was supposed to resist* the existence of these phenomena at first: "I willingly concede that these facts are in the highest degree improbable, that they sharply contradict all contemporary psychological and natural-historical data. They unexpectedly open before us that quasi-fantastic world, in the existence of which we are unaccustomed to believe to the extent our consciousness has developed, developed apparently quite firmly, thanks to exact, experimental research. But nevertheless these are facts."[37]

From his first public writings on Spiritualism in 1875, Vagner always stressed that Spiritualism should not be understood as a mystical belief system, but as a collection of "mediumistic phenomena"—raw observations—that needed to be investigated using the standard methods of science. (In this, his position foreshadows that of the stroboscopists in the essay by Jimena Canales in this volume.) The crucial ingredient for a Russian séance, exactly parallel to contemporary British standards, was a "medium," someone purported to channel phenomena between the two worlds. To the extent that Vagner thought a séance was an "experiment," it was an experiment to test whether the medium's claims were accurate. The way this was done was through "observation": one eliminated all interference that might disturb the medium (bright lights, intrusive experimental devices), and then simply observed. This is similar to Vagner's entomological research in its emphasis on mere presence and attention, for those were his proven mechanisms. One needed to be skeptical, to be sure, but if one were *too* skeptical, then this might distort the phenomena, scare the medium, and ruin the observational setup.

This form of open inquiry to establish the phenomena was where Vagner had started in the 1870s, although soon he began to consider Spiritualism a quasi-religious system.[38] The stakes were quite high: "For me, spiritualist phenomena explain the life of the entire visible and invisible world. They connect the physical world and the transcendental, science with religion."[39] But one should not assume Vagner to be antirational or dogmatic from the beginning. By his own lights, this *profession de foi* was no different from his advocacy of the universal laws of biology. Wallace was not only an interlocutor and a source; he was an *exemplar* that Vagner observed and hoped to emulate.

When he returned from Europe in 1871 and began to teach at St. Petersburg University, Vagner had at first scoffed at his schoolmate and now colleague, chemist Aleksandr Butlerov, for advocating scientific investigation of mediumistic phenomena. But Butlerov insisted that Vagner at least attend a few séances with the noted European medium Camille Bredif, organized by himself and his cousin Aleksandr N. Aksakov. Vagner would later describe his conversion experience in terms analogous to his account of the discovery of pædogenesis in the early 1860s:

> In the fall of 1874 in Petersburg Bredif arrived, and Butlerov invited me, together with A. Ia. Danilevskii and A. I. Iakobi, to participate in his séances. The latter, however, could only participate in two séances. The whole array of strong visible phenomena convinced me finally of the existence of mediumistic facts and pushed me to print a letter in the *Messenger of Europe*. Butlerov and A. N. Aksakov took a lively part in this publication. They in no way believed that my letter would convince anyone of the existence of mediumistic facts. But in the end it turned out differently. My scientific authority and my firm conviction awakened the entire intelligentsia. From all sides Butlerov and I began to receive letters, with a request for admission to séances with Bredif. Meanwhile, the party of a priori skeptics did not dither and began to print articles in refutation of those facts that they had not seen.[40]

The reference to pædogenesis is not tendentious on my part. Consider the April 1875 article referred to above, the one that sparked the Spiritualist controversy of 1875–1876 by triggering chemist Dmitrii Mendeleev's campaign against Spiritualism.[41] Vagner began this article with the oft-repeated quotation from *Hamlet* (I.v) when the doomed prince tells his friend, "There are more things in heaven and earth, Horatio, / Than are dreamt of in your philosophy." This quotation was very much in vogue among British Spiritualists, but Vagner was not interested in invoking them (yet). Instead, he recalled that the permanent secretary of the Academy of Sciences, Konstantin S. Veselovskii, had intoned these words in reference to Vagner's entomological discoveries.[42] Vagner tried to bootstrap from his acknowledged reputation as a skillful observer in the realm of the small and wriggly to the dead and immaterial. In a footnote to a German article on Spiritualism by Vagner, A. N. Aksakov cemented the connection:

> Among the main scientific works of Prof. Vagner that are well known to specialists an entirely special sensation was aroused by his remarkable discovery of a particular form of asexual reproduction (pædogenesis). . . . This discovery of Prof. Vagner's was first met generally with distrust, the reported facts were seen as something unbelievable and impossible; new observations, however, soon set them firmly as facts.[43]

Vagner's counterparts in the polemic understood what he was doing and called him on it.[44] S. Rachinskii (also the translator of Charles Darwin's *Origin of Species*) conceded that *something* must have occurred in the room if Vagner and Butlerov said it had; he just thought the medium produced these phenomena himself (or herself).[45] The hypothesis of spirit action from the beyond was simply unnecessary.

Perhaps no one agreed with Vagner more on the link between observational authority and Spiritualism than Mendeleev, who targeted Vagner (among others) with his supposedly impartial investigative commission.[46] Vagner maintained a constant back channel of correspondence with Mendeleev during the commission's activity (fall 1875–spring 1876), and he repeatedly insisted that the chemist was observing irresponsibly. First, observation was fundamentally an *individual* affair and did not require (or might even be harmed by) the presence of too many persons: "If you sincerely strove to convince yourself that mediumistic phenomena exist, then the form of your action would have been entirely different.—For this you didn't need to gather a commission of scholarly physicists and mechanics. You yourself are the authority and judge for yourself."[47] The second problem was the prejudice induced by excessive skepticism on Mendeleev's part: "The black worm [of suspicion] drove you further. He showed you things that any reasonable observer would have associated with the sphere of subjective sensations and hallucinations. And you?! You came forth with these sensations as with proofs against the medium, accusing him of charlatanry."[48] (Here also was an echo of entomology; the skepticism of von Siebert and others had hindered the acceptance of Vagner's findings.)

Mendeleev, for his part, emphasized two major problems with Spiritualists as observers of nature. The first was that they were convinced in advance that the phenomena existed, and hence were likely to conflate the very subjective and objective events that Vagner wanted to distinguish. Characteristically for a physical scientist, who demanded *experimental* confirmation of controversial claims, Mendeleev believed that predisposition could seriously distort *observations* conducted in a purely natural-historical vein of nonintervention. The desire to observe would in turn yield the observation of what one desired.[49] The second problem was the refusal of many Spiritualists to recognize fraud. In this manner, Vagner was very similar to Wallace, who claimed that no medium had ever committed fraud in his presence. (Even fraudulent mediums produced mediumistic phenomena *despite themselves* when he was present.)[50]

None of the criticism prevented Vagner from defending the faith. Long after Mendeleev had ceased to care about the propagation of Spiritualism by scientists, Vagner continued to explore new methods of observing the phenomena, such as the difficult craft of spirit photography, a significant concession on his part to the potential of instruments to aid naked-eye observations (and with resonances in the essay by Kelley Wilder on Henri Becquerel in this volume).[51] Much like William James and Oliver Lodge in parallel contexts, Vagner continued to push a program of psychical research that would help uncover (via observation) the general laws of these phenomena. In fact, Vagner considered observation via automatic writing a superlative exemplar of observation for any scientist: "This phenomenon, by its objectivity, especially affords facility for observation, and deserves full attention and investigation from competent persons and institutions."[52] He also never stopped engaging in "real science" during this period. He conducted a series of expeditions to the White Sea on Russia's Arctic coast in 1876, 1877, 1880, 1882, and 1887, and in 1881 helped to establish the first Russian marine biological station, which he directed until it closed down. His research on Arctic marine invertebrates, published in the 1880s, was a major collective achievement.[53]

Vagner's visibility in the popular press sagged after Mendeleev's assaults, intensified censorship against mediumism in general, and direct attacks on his character by rationalist writers—and even some by pronounced antirationalists, such as Fedor Dostoevskii.[54] And then, after a decade, Vagner's polemic reemerged in a rather curious episode pitting him against the most famous of his contemporaries, writer Lev Nikolaevich Tolstoi. The conflict between Vagner and Tolstoi broke out in response to Tolstoi's four-act play, *The Fruits of Enlightenment*, first drafted in 1889 and finished in 1890. It had begun as a quick sketch in November 1886 but was then shelved until, as Tolstoi would have it, "my daughters asked to be able to play it, I started to correct it, in no way thinking that it would ever go further than our home, and it ended with its being distributed."[55] *The Fruits of Enlightenment* is essentially a classic farce, in which a smart peasant girl (Tania) manages to win her hapless lover (Semën) and persuade her noble master, Leonid Fedorovich Zvezdintsev, to sell land to Semën's village family at reasonable rates. The rub is how

Tania pulls this off. Zvezdintsev was an avid Spiritualist, and Tania rigs a séance by persuading him that Semën, while napping, has mediumistic powers, and then "materializing" the deed of sale during the séance.

Vagner was not amused, and he wrote an irate letter to Tolstoi after attending a reading of the play in Petersburg, horrified at what he saw as a "satire on professors, on scientists!"[56] In particular, he was upset with the learned professor, Aleksei Vladimirovich Krugosvetlov (meaning "surrounded by light," which may have been a barb at Vagner's crypto-Spiritualist magazine venture from the late 1870s, *Svet* [*Light*]). Tolstoi described Krugosvetlov thus: "Scientist, about 50 years old, with calm, nicely self-confident manners and a similar slow, singing speech. Speaks carefully. To those who don't agree with him he relates curtly, contemptuously. Smokes a great deal. A thin, mobile person."[57] This is a pretty good description of Vagner. The tone of the professor as he explains away other theories and defends his doctrines is also strongly Vagnerian. To pick one Krugosvetlov monologue: "The same thing here also. The phenomenon is repeated, and we subject it to research. And that's just the start, we subject the researched phenomena to laws general to other phenomena. The phenomena, after all, appear supernatural only because the reasons for the phenomena are attributed to the medium himself."[58] Perhaps most Vagnerian of all, Krugosvetlov refuses to admit the fraud after it is exposed by Semën's rival for Tania's affections, the valet Grigorii.[59]

Whatever Vagner's personal wounded feelings, his objective ire was sparked by Tolstoi's refusal to accept mediumistic facts as they stood. Criticizing scientists for hubris is one thing—and Tolstoi had repeatedly attacked physicians (and Darwinists)—but refusing to accept the sincerity of Spiritualists was quite another. It was not simply insulting, it was uncharitable—and hence un-Christian, a criticism he felt sure would prick Tolstoi. Here, Vagner's Spiritualism was bound together with his corporate identification with scientists, his insistence on observation, his orientation toward struggle, and finally, his literary persona. It is time, at last, to turn to Kot-Murlyka.

Observers Observed: Endless Childhood

As early critics of Spiritualism noted, one of the reasons why Vagner needed to be stopped from propagating his dangerous creed was that he was a gifted literary stylist: "The uncontrived sincerity of tone, the energy and literary achievements of exposition, finally, the adoration of scientific authority—all of these are very important tools, found in the hands of Mr. Vagner, and which he uses to the fullest."[60] Perhaps the best comparison is with the writer who gave him the name for his literary persona: E. T. A. Hoffmann, whose

"Kater Murr" exhibited just the range of nonchalance, mockery, and sincerity that characterized Vagner's writings. One of the foremost critics of classic Russian literature, D. S. Mirsky, lauded Vagner as the only author of his age to write well outside of the canons of the "natural school."[61] His anti-Spiritualist critics did not shy away from approaching Vagner as primarily a writer about nature; neither, I contend, should we.

I conclude with Vagner's position in Russian literature for several reasons. First, separating off his scientific work from his literary activity implies an artificial separation denied by the subject himself and absent from the sources. More importantly, belles lettres in late Imperial Russia held a position of authority most closely reflected by science in contemporary Western culture. My goal is to show that the practices and theories of observation Vagner at first developed in the field and séance room became tools to develop a prominent literary status, and thus the history of Russian culture is incomplete without attention to "observation" as a category, scientific and otherwise. For Vagner treated his literary efforts like his Spiritualist publications and scientific articles: as tools to train Russian children to *observe*—not investigate, not experiment on—the world around them. Literature, and especially literature for children, was thus the highest stakes game of all.

According to an 1892 autobiographical piece, Vagner's interest in literature emerged from folktales told to him by his family's nanny, Natalia S. Aksënova. A precocious youth, he memorized extended tracts of various stories that pleased him with their rhythm and cadence, and performed impromptu one-boy shows for his relatives, but he did not endeavor to write fiction until he reached adulthood. The trigger for his own ventures was the 1868 publication of Hans Christian Andersen's tales:

> Reading the praiseworthy and even rapturous reviews of these tales in our magazines and newspapers, I bought them and read them through. Many of them I enjoyed as well, but I was also dissatisfied with many of them; I found them weak and asked myself the question: couldn't I perhaps write something like this or even better? Thus the task was posed, and in three years I had written about a dozen stories, which comprised the first edition of the "tales" of "Kot-Murlyka."[62]

Andersen may have been the immediate trigger, but Kot-Murlyka took on further tasks. His writing for children assumed two forms: nonfictional guides to induce children to observe outdoor nature, and fantastical tales to induce children to observe their inner natures.[63]

Although Vagner's literary reputation hangs entirely on the second group, the first was more important to him. He produced several popular texts of natural history for children of various ages, including two editions of a translation of Paul Bert's French textbook. As he indicated in his preface to the first edition, the purpose of having such a book available in Russian was to encourage children: "For them [children] it [nature] is an open book, in which they can, unwittingly, learn a great deal if only they had a guide near them. It would show them how one can observe, look into things, and chiefly, think about subjects and phenomena."[64] Children, more than adults, were natural observers, and must observe *Russia*: "Let's stop, although not for long, on still another country, which lies both in Europe and in Asia, but which for every Russian lies closer than any other, because it lies in his own heart. You, of course, have guessed, that I want to speak to you about the population of your mother country, with which you are connected through your birth, language, your faith, customs, habits, finally, your character."[65] Vagner was once again pushing for a repeat of his pædogenetic discovery. Accomplished with a magnifying glass, a pair of walking shoes, and some natural curiosity, it had been a victory for Russian natural history. If children simply indulged the childlike in themselves, they would produce wonderful science.

Then one had the tales of Kot-Murlyka, which appeared in nine editions between 1872 and 1913, and in 1923 were issued in their first Soviet edition. This was also the last edition until 1990, right before the collapse of the Soviet Union.[66] The corpus ranges widely among morality tales, children's heroic narratives, just-so stories, and prose poems about nature.[67] Interestingly, some of his peers considered these stories deeply unsuitable for children:

> They [the stories] are undesirable, first, because they are almost all unbearably heavy tales, full of woe, suffering, unilluminated human gloom. All of this acts too strongly on the reader, acts oppressively, even frighteningly in places. Second, these stories for children are not desirable even for those who are older because the philosophy of many of them can lead an impressionable, sensitive child to despair, to a total lack of desire to live.
>
> One of the main themes of the tales of Kot-Murlyka is the inevitability of woe and sufferings, the naturalness of woe and suffering, the battle of knowledge and blind naïve faith in the wonderful [*chudesnoe*], in which the sympathies of the author are always on the side of the victor—knowledge. Next comes a description of the muddle of human society, in which poverty is completely unavoidable, and in which even the battle with poverty and human unhappiness is completely fruitless. Finally—the heavy battle for existence, the extinction of everything weak, incomplete, or not suitable for life.[68]

Even here, in the realm of fictional tales for children, the same themes emerged: his opponents' resistance to careful observation, and Vagner's imperviousness to opposition. The man kept writing until the end of his life (although his later works abandoned some of the fantastic features and took a regrettable slide into anti-Semitism),[69] just as he held on to his Malthusian Darwinism (or should we say Wallaceism?), as he had persisted with pædogenesis, and as he continued to defend Spiritualism. All of these were marked with struggle, by struggle, and through struggle: in the discovery of pædogenesis, it was the struggle of observation as persuasion, convincing recalcitrant entomologists; in his attachment to Wallace's natural selection, observation as generalization found struggle woven in the fabric of nature; and that struggle needed itself to be struggled against in order to achieve the expectant calm of the Spiritualist observer. It was not so much that Vagner believed his observational authority sufficed to bear him along against the slings and arrows of outrageous fortune; rather, it was the very nature of observation, properly done, to exhibit and elicit these kinds of attacks. The fact that his critics continued to attack him suited him just fine. He knew he would eventually be vindicated. All his observations supported it.

Notes

1. N. P. Vagner, "Kto byl Kot-Murlyka?" in *Skazki Kota-Murlyki* (Rostov-on-Don: Prof-Press, 2001), 5–10, on 5–6. Russian dates are given according to the old-style Julian calendar, European dates in the new-style Gregorian; transliterations follow the modified Library of Congress standard. All unattributed translations are my own.

2. Nicolas Wagner, "Beitrag zur Lehre von der Fortpflanzung der Insectenlarven," *Zeitschrift für wissenschaftliche Zoologie* 13 (1863): 513–27, on 514.

3. N. P. Vagner, *Obshchii vzgliad na klass zhivotnykh paukoobraznykh (*Arachnidae) *i chastnoe opisanie odnoi iz form, k nemu prinadlezhashchikh* (Kazan': Universitetskaia tip., 1854).

4. Biographical facts are drawn from G. A. Kliuge, "Vagner, Nikolai Petrovich," in N. P. Zagoskin, ed., *Biograficheskii slovar' professorov i prepodavatelei Imperatorskago kazanskago universiteta (1804–1904)* 2 vols. (Kazan': Tip. Imperatorskago Universiteta, 1904), 1: 286–89; N. Knipovich, "Vagner (Nikolai Petrovich)," *Entsiklopedicheskii slovar' Brokgauza i Efrona*, v. 5 (St. Petersburg: I. A. Efron, 1891), 343–44; and V. I. Mil'don, "Vagner, Nikolai Petrovich," in P. A. Nikolaev, ed., *Russkie pisateli, 1800–1917* (Moscow: Sovetskaia entsiklopediia, 1989), 1: 385–86.

5. N. P. Vagner, "Vospominan'e ob Aleksandre Mikhailoviche Butlerove," in A. M. Butlerov, *Stat'i po mediumizmu* (St. Petersburg: A. N. Aksakov, 1889), iii–lxvii, on vi.

6. K. E. von Baer, "Über Prof. Nic. Wagner's Entdeckung von Larven, die sich fortpflanzen, Herrn Ganin's verwandte und ergänzende Beobachtungen und über die Paedogenesis überhaupt," *Mélanges biologiques tirés du Bulletin de l'Académie impériale des sciences de St. Pétersbourg* 9 (1865): 64–137, on 66.

7. Wagner, "Beitrag zur Lehre von der Fortpflanzung der Insectenlarven," 513n.

8. M. Ganin, "Neue Beobachtungen über die Fortpflanzung der viviparen Dipterenlarven," *Zeitschrift für wissenschaftliche Zoologie* 15 (1865): 375–90; Fr. Meinert, "Weitere Erläuterungen über die von Prof. Nic. Wagner beschriebene Insectenlarve, welche sich durch Sprossenbildung vermehrt," trans. and ed. C. Th. V. Siebold, *Zeitschrift für wissenschaftliche Zoologie* 14 (1864):

394–99, on 397; N. Wagner, Meinert, Pagenstecher, and Ganin, "La reproduction parthénogénésique chez quelques larves d'insects diptères," *Annales des sciences naturelles, Série zoologique* 5, no. 4 (1865): 259–91; and K. E. von Baer, "Bericht über eine neue von Prof. Wagner in Kasan an Dipteren beobachtete abweichende Propagationsform," *Mélanges biologiques tirés du Bulletin de l'Académie impériale des sciences de St. Pétersbourg* 6 (1863): 239–41, on 240.

9. Von Baer, "Über Prof. Nic. Wagner's Entdeckung von Larven," 97. Emphasis in original.

10. Stephen Jay Gould, *Ontogeny and Phylogeny* (Cambridge, Mass.: Belknap Press, 1977); B. N. Shvanvich, *Kurs obshchei entomologii: Vvedenie i izuchenie stroeniia i funktsii tela nasekomykh* (Moscow: Sovetskaia Nauka, 1949), 698–99. This elision had already begun in the nineteenth century: Otto Taschenberg, "Historische Entwicklung der Lehre von Parthenogenesis," *Abhandlungen der Naturforschenden Gesellschaft zu Halle* 17 (1892): 366–453, on 398–400.

11. Nicolas Wagner, "Ueber die viviparen Gallmückenlarven," ed. C. Th. v. Siebold, *Zeitschrift für wissenschaftliche Zoologie* 15 (1865): 105–17, on 107. Emphasis in the original.

12. E. P. Felt, "Miastor and Embryology," *Science*, n.s. 33, no. 843 (24 Feb. 1911): 302–3, on 302.

13. Nicolas Wagner, "Influence de l'électricité sur la formation des pigments et sur la forme des ailes chez les Papillons," *Comptes rendus* 61 (1865): 170–72, on 172.

14. N. P. Vagner, "Vospominanie o Klod-Bernar," *Svet* 2, no. 2 (Feb. 1878): 64–65, on 65.

15. Claude Bernard, *An Introduction to the Study of Experimental Medicine*, trans. Henry Copley Greene (New York: Henry Schuman, Inc., 1949), 196–226.

16. N. P. Vagner, "Zoologiia," stenograph from lectures [1879], Rossiiskaia Natsional'naia Biblioteka (hereafter RNB), St. Petersburg, Russia, 18.167.6.46.

17. Paul Bert and Nikolai Vagner, *Ocherk zoologii* (St. Petersburg: N. P. Vagner, 1883), 166. Emphasis in the original. Vagner became enmeshed in a publication dispute over this translation, since publisher F. Pavlenkov offered a rival translation at the same time. Vagner framed his case even in this domain in terms of battle metaphors: "In all of these conditions, I hope that the public will understand and value that *here there isn't a simple competition, but a battle of scientific work with publishing capital and knowledge with its all too shameless exploitation.*" "Dokumenty po delu N. Vagnera s F. Pavlenkovym," [1883], RNB, p. 4. Emphasis in the original.

18. N. P. Vagner, *Istoriia razvitiia tsarstva zhivotnykh: Kurs filogeneticheskoi zoologii* (St. Petersburg: N. A. Lebedev, 1887), 33.

19. N. P. Vagner, "Bor'ba," *Svet* 1, no. 11 (Nov. 1877): 241–47, on 241. This was the first in a series of editorials on struggle: idem, "Prisposobliaemost' i tselesoobraznost'," *Svet* 2, no. 3 (March 1878): 69–72; idem, "Bor'ba za sushchestvovanie," *Svet* 2, no. 4 (April 1878): 105–10; and idem, "Organizm i obshchestvo," *Svet* 2, no. 7 (July 1878): 105–10.

20. Vagner, *Istoriia razvitiia tsarstva zhivotnykh*, 4.

21. N. P. Vagner, "O sredstvakh dlia resheniia slozhnykh nauchnykh voprosov," in *Rechi i protokoly VI-go s"ezda Russkikh Estestvoispytatelei i Vrachei v S.-Peterburge s 20-go po 30-e dekabria 1879 g.* (St. Petersburg: Tip. Imp. Akademii nauk, 1880), otd. 1: 39–47, on 41.

22. Vagner, *Istoriia razvitiia tsarstva zhivotnykh*, i. See also N. P. Vagner, *Zhorzh Kiuv'e i Et'en Zhoffrua-Sent-Iler (Fiziologicheskii ocherk)* (Kazan': Universitetskaia tip., 1860), esp. 55.

23. N. Ia. Danilevskii, *Darvinizm: Kriticheskoe izsledovanie*, vol. 1, pt. 1 (St. Petersburg: Merkurii Eleazarovich Komarov, 1885); and N. N. Strakhov, "Durnye priznaki," in *Kriticheskiia stat'i (1861–1894)*, vol. 2 (Kiev: I. P. Matchenko, 1902), 379–97.

24. Daniel P. Todes, *Darwin without Malthus: The Struggle for Existence in Russian Evolutionary Thought* (New York: Oxford University Press, 1989).

25. Alexander Vucinich's ostensibly comprehensive history of Darwinism in Russia—

Alexander Vucinich, *Darwin in Russian Thought* (Berkeley: University of California Press, 1988)—ignores Wallace completely and only mentions Vagner twice in passing. Even Todes minimizes our hero: he only receives three mentions, and two of them in the context of the work of Vagner's student, Modest Bogdanov.

26. H. Lewis McKinney, *Wallace and Natural Selection* (New Haven: Yale University Press, 1972), 81.

27. See A. Gusev, *Naturalist Uolles, ego russkie perevodchiki i kritki (K voprosu o proiskhozhdenii cheloveka): Po povodu perevodov knigi Uollesa: Estestvennyi podbor* (Moscow: M. Katkov, 1879), 4–5.

28. A. R. Wallace, *Estestvennyi podbor*, trans. G. Glazenap, ed. N. P. Vagner (St. Petersburg: F. Shushchinskii, 1878), ii.

29. He was in contact with Wallace during the production of this translation, since Vagner mentioned that Wallace had sent him some illustrations for inclusion (Wallace, *Estestvennyi podbor*, iv). I can find no mention of Vagner by Wallace, although the latter recalled having met Aleksandr Butlerov, a "biologist and also a spiritualist." Alfred Russel Wallace, *My Life: A Record of Events and Opinions*, 2 vols. (London: Champman & Hall, 1905), 2: 93. This misidentification of the *chemist* Butlerov might mean that Wallace confused him with Vagner.

30. Gusev, *Naturalist Uolles*, 8. Vagner himself reviewed Gusev: N. P. Vagner, "Otvet g. Gusevu na ego knigu: Naturalist Uolles i ego russkie perevodchiki," *Svet* 3, no. 4 (April 1879): 200.

31. Wallace, *My Life*, 2: 17. Emphasis in the original.

32. Alfred Russel Wallace, *A Defense of Modern Spiritualism* (Boston: Colby and Rich, 1874), 33.

33. Malcolm Jay Kottler, "Alfred Russel Wallace, the Origin of Man, and Spiritualism," *Isis* 65 (1974): 144–92.

34. Especially Ross A. Slotten, *The Heretic in Darwin's Court: The Life of Alfred Russel Wallace* (New York: Columbia University Press, 2004); and Martin Fichman, *An Elusive Victorian: The Evolution of Alfred Russel Wallace* (Chicago: University of Chicago Press, 2004).

35. N. P. Vagner, *Nabliudeniia nad mediumizmom* (St. Petersburg: F. Vaisberg and P. Gershunin, 1902), iv, ellipses added.

36. Ibid., xviii. See also N. P. Vagner to A. D. Butovskii, 2 Dec. (n.s.) 1883, Naples zoological station, Pushkinskii Dom, Institute of the History of Russian Literature (hereafter PD), f. 1, op. 2, d. 174, ll. 3–40b., on 11. 3–30b.

37. N. P. Vagner, "Mediumizm," *Russkii Vestnik* 119 (Oct. 1875): 866–951, on 878, ellipses added.

38. Vagner, *Nabliudeniia nad mediumizmom*, 121.

39. N. P. Vagner to L. N. Tolstoi, 10 April 1890, PD f. 231, d. 279, ll. 4–90b., on 1. 8–80b.

40. Vagner, "Vospominan'e ob Aleksandre Mikhailoviche Butlerove," xxxix–xl. (This Danilevskii is *not* the anti-Darwinist referred to above.)

41. See Michael D. Gordin, *A Well-Ordered Thing: Dmitrii Mendeleev and the Shadow of the Periodic Table* (New York: Basic Books, 2004), chap. 4; and also the suggestive interpretation of Vagner in Ilya Vinitsky, *Ghostly Paradoxes: Modern Spiritualism and Russian Culture in the Age of Realism* (Toronto: University of Toronto Press, 2009), chap. 4.

42. N. Vagner, "Pis'mo k redaktoru: Po povodu spiritizma," *Vestnik Evropy* (April 1875): 855–75, on 855.

43. Nicolas Wagner [Vagner], "Ueber die psychodynamischen Erscheinungen," *Psychische Studien* 2 (1875): 97–106, on 97n. Ellipses added.

44. A. Shkliarevskii, "Chto dumat' o spiritizme?: Po povodu pis'ma prof. Vagnera," *Vestnik Evropy* (June–July 1875): 906–18, 409–18, on 906.

45. S. Rachinskii, "Po povodu spiriticheskikh soobshchenii g. Vagnera," *Russkii Vestnik* (May 1875): 380–99, on 380.

46. The chief target was Akaskov, not Vagner, as can be seen in Mendeleev's detailed footnotes to the protocols of his commission, published as D. I. Mendeleev, *Materialy dlia suzhdeniia o spiritizme* (St. Petersburg: D. Mendeleev, 1876).

47. N. P. Vagner to D. I. Mendeleev, 1 January 1875 [*sic* 1876], Archive-Museum of D. I. Mendeleev (hereafter ADIM), St. Petersburg, Russia, Alb. 4/52.

48. N. P. Vagner to D. I. Mendeleev, 19 Feb. 1876, ADIM Alb. 4/56.

49. This argument is quite similar to Fedor Dostoevskii's writings on Spiritualism in his 1876 *Writer's Diary*. This dispute of tone between Mendeleev and Dostoevskii is addressed in Michael D. Gordin, "Loose and Baggy Spirits: Reading Dostoevskii and Mendeleev," *Slavic Review* 60 (2001): 756–80.

50. Fichman, *An Elusive Victorian*, 183. The Vagnerian equivalent was the Russian's attack against the self-proclaimed fraud (and Mendeleev ally) I. Livchak. See the account in N. Lerner, "Tainstvennye uzelki: Sluchai s Dostoevskim," *Literaturno-khudozhestvennyi sbornik "Krasnoi panoramy"* (Oct. 1928): 36–42.

51. V. Pribytkov, "O fotografiiakh, predstavlennykh professorom N. P. Vagnerom v Tekhnicheskoe Obshchestvo," *Rebus* no. 13 (1894): 131; and N. P. Vagner, "Sine ira et studio. (Po povodu mediumicheskikh fotografii)," *Rebus* nos. 15–16 (1894): 151–52, 164–66; and A. N. Aksakov, *Animizm i spiritizm: Kriticheskoe issledovanie* (1900; Moscow: Agraf, 2001), 80–85.

52. Nicholas Wagner, A. Boutlerof, and A. Dobroslavin, "Seance for Autographic Writing with Mr. Eglinton," *Journal of Society for Psychical Research* (June 1886): 329–31, on 331.

53. The results of the White Sea research were published preliminarily as N. P. Vagner, *Predvaritel'noe soobshchenie o meduzakh i gidroidakh Belago moria* (St. Petersburg: V. Demakov, 1881); and then in the gorgeous volume of idem, *Bezpovochnyia Belago moria: Zoologicheskiia izsledovaniia proizvedennyia, na beregakh Solovetskago zaliva, v letnie mesiatsy 1876, 1877, 1879 i 1882 goda* (St. Petersburg: M. M. Stasiulevich, 1885).

54. Dostoevskii had brutally mocked Vagner's Spiritualism (and in print, too!) in his very funny 1878 feuilleton "From the Dacha Strolls of Kuz'ma Prutkov and His Friends, I: Triton" (F. M. Dostoevskii, *Polnoe sobranie sochinenii* [Leningrad: Nauka, 1972–90], 21: 250–51).

55. L. N. Tolstoi to N. P. Vagner, April 1890, PD f. 231, d. 287, ll. 1–4, on 1. 1ob.

56. N. P. Vagner to L. N. Tolstoi, 22 March 1890, PD f. 231, d. 279, ll. 1–3, on 1. 1

57. L. N. Tolstoi, "Plody prosveshcheniia: Komediia v chetyrekh deistviiakh," in Tolstoi, *Sobranie sochinenii* (Moscow: Khudozhestvennaia literatura, 1978–85), ll: 101–94, on 101.

58. Ibid., ll: 158.

59. Ibid., ll: 192.

60. A. Shkliarevskii, "Kritiki togo berega," *Russkii Vestnik* 121 (Jan. 1876): 470–95, on 470.

61. D. S. Mirsky, *A History of Russian Literature: From Its Beginnings to 1900* [1926], ed. Francis J. Whitfield (Evanston, IL: Northwestern University Press, 1999), 296–97.

62. N. P. Vagner, "Kak ia sdelalsia pistalem? (Nechto v rode ispovedi)," *Russkaia shkola* 1 (Jan. 1892): 26–38, on 37.

63. I am uncertain how to classify Vagner's retelling of the Synoptic Gospels for young adults: N. P. Vagner, *Razskaz o zemnoi zhizni Iisusa Khrista po Sv. Evangeliiam, narodnym predaniiam i ucheniiam Sv. Tserkvi* (St. Petersburg: M. M. Stasiulevich, 1908).

64. Bert and Vagner, *Ocherk zoologii*, i. The same is true of the wonderful N. P. Vagner, *Kartiny iz zhizni zhivotnykh: Ocherki i razskazy* (St. Petersburg: A. F. Marks, 1901).

65. Bert and Vagner, *Ocherk zoologii*, 157. Chapter 9 was added to cover Russian nature.

66. Viktor Shirokov, "Russkii Andersen," in N. P. Vagner, *Skazki Kota-Murlyki* (Moscow: Pravda, 1991), 5–14, on 5.

67. For the complete collection, see N. P. Vagner, *Romany, povesti i razskazy Kota-Murlyki*, 4th ed., 7 vols. (St. Petersburg: Obshchestvennaia Pol'za, 1905–14).

68. Evgenii Elachich, *Sbornik statei po voprosam detskogo chteniia* (St. Petersburg: Khudozhestvennaia pechat', 1914), 132–33.

69. The relevant text here is Vagner's novel of the Crimean War, *Temnyi put'*, published as a book in 1890, although partially serialized from 1881 to 1884 in the Spiritualist journal *Rebus*. For an extensive discussion of its anti-Semitic tropes, see Savelii Dudakov, *Istoriia odnogo mifa: Ocherki russkoi literatury XIX–XX vv.* (Moscow: Nauka, 1993), 242–60.

6

A Visual History of Jean Perrin's Brownian Motion Curves

CHARLOTTE BIGG

> In sum the science of drawing consists in instituting relations between curves and straight lines. A painting containing only curves or straight lines would not express existence. (En some la science du dessin consiste à instituer des rapports entre les courbes et les droites. Un tableau qui ne contiendrait que des droites ou des courbes n'exprimerait pas l'existence.)
>
> ALBERT GLEIZES AND JEAN METZINGER, *Du "Cubisme"* (1912)

A sheet of squared paper on which three broken lines have been drawn. A connect-the-dots game gone slightly awry, with no pattern obviously recognizable. No scale is inscribed that might provide clues about the size and nature of the object or phenomenon represented here. No indications on the procedure involved in the production of this two-dimensional abstraction. No numbers, letters, or symbols to tell the viewer how to hold the figure, or in what direction the lines run; indeed, its author (or perhaps was it the publisher's initiative?) occasionally published it sideways (fig. 6.1).[1]

Yet show this image to a physicist or a mathematician and the response will be immediate: *this is Brownian motion.*

This image, published for the first time in September 1909 by French physical chemist Jean Perrin (1870–1942),[2] has acquired iconic status in the physical sciences. It was and is still perceived as an experimental confirmation and a visual equivalent of Albert Einstein's theoretical demonstration, in a paper of 1905, of "the reality of atoms and molecules, of the kinetic theory of heat, and of the fundamental part of probability in the natural laws."[3]

Yet Einstein's publications on Brownian motion do not feature any images, nor did he suggest that the phenomenon should be represented in this way. In fact, until proven wrong, Einstein even doubted that the methods he suggested for measuring Brownian motion could be realized experimentally: "I would have thought such a precise study of Brownian motion impossible to realize," he wrote in admiration to Perrin in November 1909.[4]

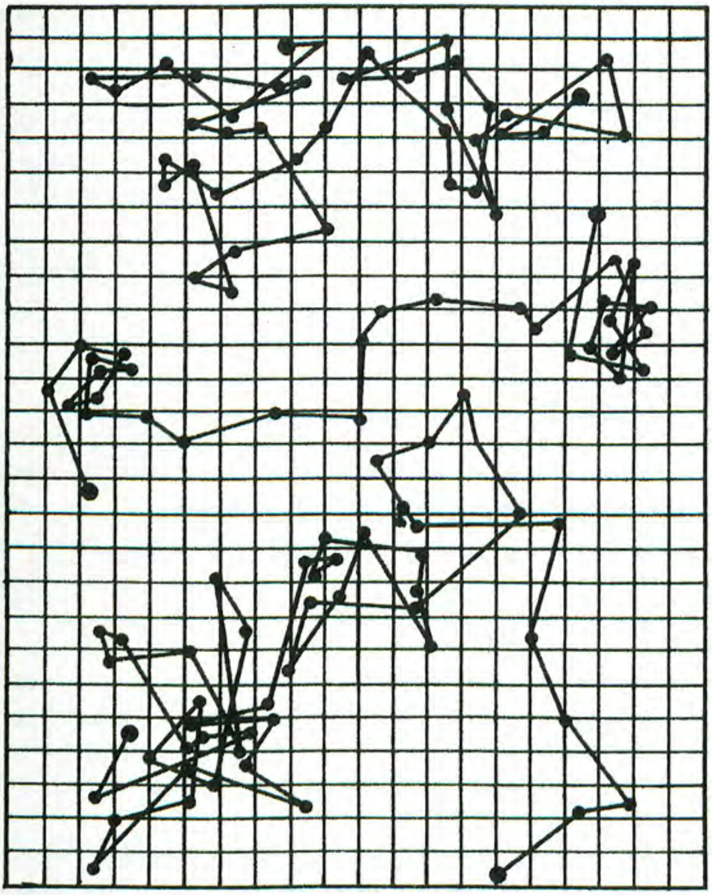

FIGURE 6.1. Jean Perrin, "Mouvement brownien et realité moléculaire," *Annales de chimie et de physique* ser. 8, 18 (1909): 81.

Einstein's and Perrin's Brownian motion work is justly famous for raising a number of issues central to the epistemology and historiography of the physical sciences, in particular, related to the nature of evidence, the relationship between theory and experiment, and realism.[5]

Rather than investigating the detailed ways in which a perfect fit between Perrin's experiments and Einstein's theory was realized, this paper explores the gap between Einstein's formulas and Perrin's image, a gap that stretched across four years and different scientific cultures but was subsequently erased when the image collapsed onto the formula it represents. What happens when we pry apart the formula from its representation? How did this image come both to encapsulate and help permanently establish a new way of seeing and understanding Brownian motion?

Certainly, Perrin's work took place within an epistemological economy structured by the twin categories of theory and experiment. When he wrote that his Brownian motion project was explicitly devised to serve as a crucial experiment to test the validity of the kinetic theory and the atomic hypothesis, Perrin adhered to the ruling epistemology of the laboratory sciences since the second half of the nineteenth century, in which observation played an epistemologically subordinate role.

Nonetheless, Perrin's work constitutes a milestone in the history of observation in the physical sciences, having established the existence of that most famous of all unobservables, the atom—though in fact neither atoms nor molecules were ever actually observed. A close look at his work reveals that observation, as a practice, as a skill, and as a product occupy a central place in Perrin's project. By focusing the attention on the techniques, skills, and resources involved in Perrin's practical laboratory work, this chapter not only shows how much takes place between the development of a theory and its experimental verification, and in particular, the role played by visual representations in the production of evidence; it also serves as a good reminder of the continuing importance of creative skill and technical ingenuity in the experimental sciences, broaching several of the issues developed in part 3 below, "Techniques." A parallel reading of this chapter together with Mary Terrall's chapter on Réaumur's observation of frogs in this volume brings out especially vividly, beyond the obvious differences, surprisingly similar concerns with ways of presenting the interaction of observers and their objects and the social organization and bodily disciplines of virtuoso observation. Finally, by uncovering the interdisciplinary dialogue that was determinant in the elaboration and subsequent appropriations of this image, this chapter shows how Perrin's work was embedded in the cultural and scientific fabric of his times. Tracing the history of this image, it shows how a virtual community was created through the making of Perrin as an observer and the making and the reception of his observations of Brownian motion.

"Mise en Observation"

Figure 6.1 appeared for the first time in the September 1909 issue of the *Annales de chimie et de physique.* As Perrin's laboratory notebooks of the time testify (fig. 6.2), this image was a composite picture of three drawings made earlier that year by Perrin during a series of experiments carried out together

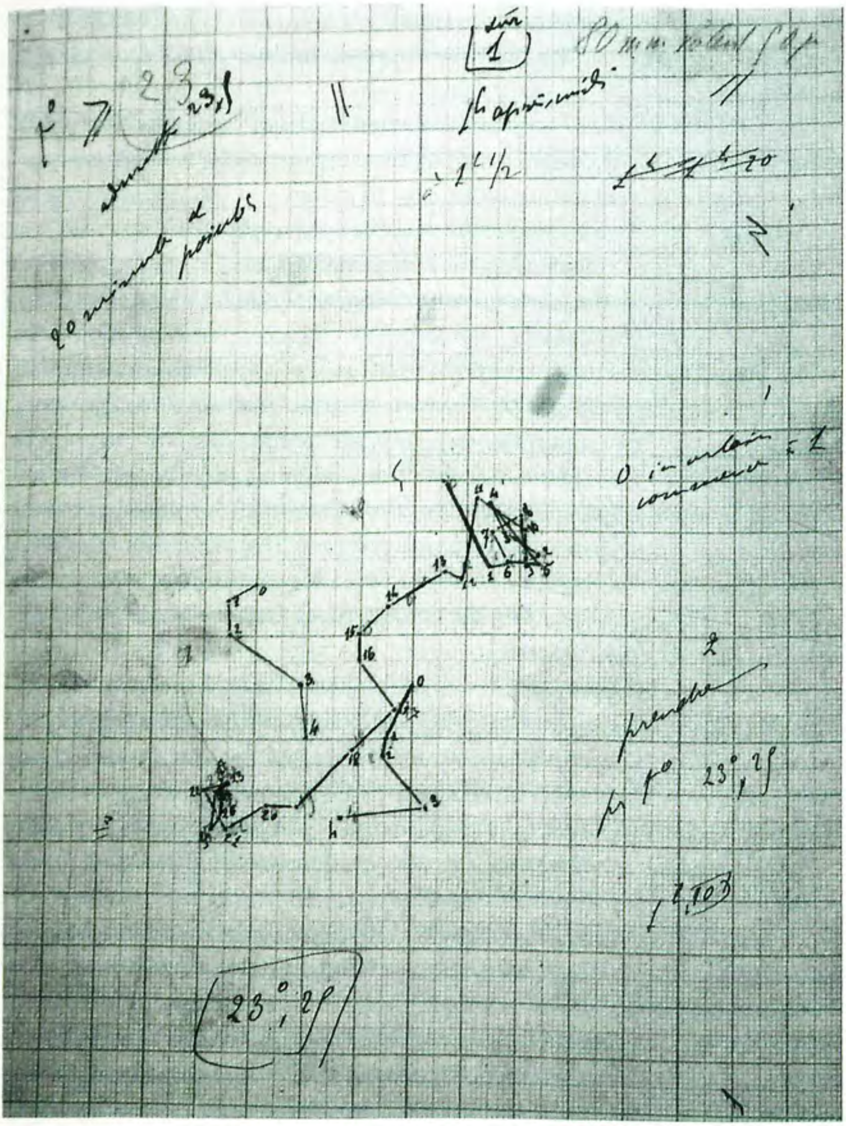

FIGURE 6.2. Perrin notebook, "Calcul de *N*," undated, c. 1909. This trajectory is reproduced in figure 6.1 (*middle*), with slight modifications in the segment angles and omitting the numbers. Dossier Jean Perrin, Archives of the Académie des Sciences, Paris, with permission.

with his student Dabrowski in the small laboratory for physical chemistry Perrin had set up at the turn of the century in an attic of one of the Sorbonne buildings.

In 1898, shortly after completing studies in physics and chemistry at the École Normale Supérieure with a Ph.D. showing that cathode rays were negatively charged (and therefore consisting of streams of particles, later to be named electrons), Perrin had begun teaching physical chemistry at the Sorbonne. He had been entrusted by the recruiting commission, in the person of Henri Poincaré, with the task of "naturalizing" on French soil the work of the mainly German pioneers and of bringing Gallic precision to the dynamic new field.[6] Taking up the challenge, Perrin recast in his own words contemporary discussions on the relationship between thermodynamics and mechanics, propagating his views in a textbook, *Traité de chimie physique. Les principes* that appeared in 1903.[7] There, but also in his teaching, popular lectures, and articles,[8] he stood up as a staunch advocate of atomism, a minority position in the French scientific community—though one that was well represented at the École Normale Supérieure and in the circle of scientists he interacted with on a daily basis, including his teacher Aimé Cotton, Marie and Pierre Curie, and Paul Langevin.

In 1903, once his laboratory had been fitted out, Perrin launched a series of experiments on the electrical properties of colloid solutions, suspensions of submicroscopic particles, that increasingly attracted the interest of chemists and biologists in these years, and on which Cotton was working with a colleague at the Institut Pasteur, Henri Mouton.[9] From there, Perrin moved on in early 1908 to the study of the Brownian motion of colloid particles.

Figure 6.1 was a product of one of the very last experiments within this new project. Perrin and Dabrowski had prepared what they referred to as their emulsion by bringing mastic, extracted from the bark of *Pistacia lentiscus* from the island of Chios and commonly used in the production of varnishes, into contact with methyl alcohol, obtaining a solution floating above a sticky insoluble residue. When diluted extensively, the solution became white as milk, in fact a suspension of spherical granules of varying sizes. Perrin and Dabrowski then subjected this emulsion to a series of "fractioned centrifugations" to obtain a suspension of grains of identical sizes. For this experiment they selected grains of a radius of 0.52 micrometers.[10]

In order to observe his emulsions, Perrin usually placed a drop of the suspension in a cavity about one-tenth of a millimeter deep, created by gluing a glass plate in which a wide hole had been bored onto an object slide. The cavity was then covered with another glass plate and sealed. This preparation could be used for several days or weeks.[11] For this particular experiment, the stability of the liquid's viscosity (and hence its temperature) was essential, so Perrin and Dabrowski immersed both the cell containing the drop and the microscope objective in a water-filled tank. The temperature was regularly measured by dipping a thermometer close to the microscope objective. On the notebook page shown in figure 6.2 the temperature is indicated, "23°,25" (23,25° C).

These procedures—the selection of the grains, enclosing the emulsion, and setting up the microscope for optimal conditions of visibility—constituted, with slight variations involving different types of microscopes and different sizes and types of grains, the starting point for all of Perrin's Brownian motion experiments. They were described by him as the "mise en observation" of his emulsions, the setting up of the conditions under which the Brownian motion of colloid particles was to be observed and produce fruitful insights.[12] Perrin relied mostly on techniques learned or developed in previous years. As he later wrote, "the study of colloids had familiarized me with the observation of Brownian motion."[13] It is noteworthy that many of these techniques were borrowed or adapted from the biologist's arsenal, from centrifugation, "as one does to separate the red cells from blood serum," to the use of dissection needles and plates engraved with a grid to help count cells in solutions, not to mention the microscope and camera lucida, standard microbiologist's devices.[14] The cheapness and ready availability of these instruments were certainly an advantage in a modestly endowed laboratory such as Perrin's. Moreover, they testify to Perrin's close acquaintance, perhaps through Mouton, of biological techniques and their creative adaptation for physical-chemical investigation.

The majority of Perrin's observations concerned the behavior of large numbers of particles, for instance, when they reached equilibrium in the emulsion, their concentration decreasing with altitude. In the particular experiment discussed here, Perrin and Dabrowski were interested instead in measuring the motion of *individual* particles.

The broken line at the center of figure 6.2 represents the motion of a single particle over a period of 20 minutes ("20 minutes de pointés"). In the published version (fig. 6.1), 16 divisions of the grid correspond to 50 micrometers. This is a simplified representation of the more complex trajectory followed by the particle, obtained by marking its position at regular intervals of time, here every 30 seconds. The trajectory is also simplified in the sense that it is a projection on a two-dimensional plane of the three-dimensional motion of the particles in the liquid. For this, Perrin attached a camera lucida to his microscope, enabling the simultaneous visualization of the particles swimming in the liquid and of his sheet of paper. While one man stationed in front of a chronometer called out the signals every 30 seconds, the other stood at the microscope eyepiece, following the motion of a particle and noting its position on paper when the signal was called. Perrin and Dabrowski regularly swapped positions, presumably to compensate for personal idiosyncrasies in each scientist's observing technique. The dots on the sheet were then numbered successively and joined by straight lines to produce trajectories as shown in figures 6.1 and 6.2. The notebook page featured in figure 6.2 indicates that this particular set of observations took place one undated afternoon between 1:00 p.m. and 1:30 p.m. Altogether, 950 observations were made using this particular emulsion, with each observation corresponding to one curve segment.[15]

Theory and Experiment

Brownian motion, the irregular and perpetual motion of small particles suspended in a liquid or a gas, varies in intensity as a function of the viscosity and temperature of the medium, but also of the size of the particles: the smaller the particle, the greater the motion. It accordingly affects microscopic and submicroscopic particles particularly strongly (as such it is well known to microscopists for interfering with microorganisms' proper motions). Brownian motion presented an opening for Perrin as a phenomenon that might be enrolled in his advocacy of atomism. In previous years several scientists, including Aimé Cotton and Pierre Curie's close friend the physicist Georges Gouy, had suggested that the Brownian motion of microscopic particles was a perceptible consequence of molecular agitation in fluids, and therefore that the phenomenon could be interpreted as empirical evidence in favor of atomism and the kinetic theory (which supposed liquids and gases to be made up of very small, hard spheres, or atoms).[16] However, as even the supporters of atomism recognized, existing measurements of the Brownian motion of particles did not correspond, by far, to the values predicted by the kinetic theory, in turn casting doubt on the atomic hypothesis.

Perrin's project explicitly aimed at producing experimental evidence in favor of the molecular-kinetic interpretation of Brownian motion. For this, he relied on new methods for measuring this motion that offered hope to bring observation in line with the values predicted by the kinetic theory. The temporal development of Perrin's Brownian motion work, as recorded in his laboratory notebooks, makes clear that Einstein's theory was a primary resource for developing his own project. The first entry in his first notebook, begun around March–April 1908, begins: "Langevin-Einstein hypothesis: each granule is assimilable to molecule (same $\sqrt{mv^2}$)." Further, Perrin consigned "requires experimental verification," outlining first ideas for producing emulsions of same-sized grains by centrifugation and measuring experimentally Avogadro's number, *N* the number of molecules in a mole. Then Perrin noted: "Thereafter, nothing comes in the way of verifying Einstein's formula

$$\sqrt{\Delta_x^2} = \sqrt{t}\sqrt{\frac{RT}{N}\frac{1}{3\pi\mu a}}$$

(*a* radius of the granule μ viscosity), after which the application of this formula enables *a* to be obtained for *any* arbitrary granule, followed from moment to moment."[17]

The reference to his close friend the physicist Paul Langevin on the first page of the notebook is indicative of the latter's role in attracting Perrin's attention to Einstein's work. On 9 March 1908, Langevin had presented a paper to the Académie des Sciences entitled "Sur la théorie du mouvement brownien" that gave Einstein's formula in a form very close to that appearing in Perrin's notebook. Langevin also discussed Maryan von Smoluchowski's publications, and he assessed critically the first attempt at an experimental verification of Einstein's methods by Swedish physicist The Svedberg.[18] Perrin himself claimed in 1911 that Langevin had first brought Einstein's investigations to his attention.[19]

In his Brownian motion paper of 1905, Einstein had proposed new quantitative methods of measuring Brownian motion and of determining the dimensions of the particles, thus offering novel tools for testing the validity of the kinetic theory. He argued in particular that it was meaningless to measure the instantaneous velocity of individual particles, as previous researchers had done. Instead he proposed to measure their *mean displacement*, suggesting that the mean displacement of a particle on the *x* axis during an interval of time *t* should be proportional to the square root of *t*.[20] In Perrin's formulation of Einstein's formula above, the mean displacement over a given interval of time $\sqrt{\Delta_x^2}$ can be calculated when *R* (gas constant), *N* (Avogadro's number), *T* (absolute temperature), μ (the viscosity of the fluid), and *a* (the radius of the particle) are known; conversely, *N* or *a* can be obtained when mean displacement and the other factors are known.[21] Perrin proposed first to provide an experimental confirmation of Einstein's formula by calculating the value of *N* based on the experimental determination of the other factors in the equation. If this *N* corresponded to the values of *N* obtained by other methods, the formula could be considered reliable and could in turn be used for determining the size of the suspended grains.

Perrin's broken lines, then, aimed at determining experimentally the mean displacement of particles of known radius in a liquid of known viscosity. The squared paper enabled a quick measurement of the length of each segment of his trajectories;[22] from there the mean displacement of a particle during successive time intervals could be calculated. Factoring in the constants *R* and *T*, Perrin could obtain, for each series of measurements, a value for Avogadro's number *N*.

Perrin found that the values of *N* obtained on the basis of 3,000 measured displacements agreed well with other determinations of *N* he and other scientists, from Lord Rayleigh and J. J. Thomson to the Curies, had made on the basis of different emulsions or different phenomena entirely. This "miracle of concordances" constituted for Perrin decisive evidence of the validity of Einstein's formula, of his method of measuring Brownian motion, and beyond, of the kinetic theory and of molecular reality.[23]

It is worth remarking that Perrin's argumentation followed the conventional epistemology of the laboratory sciences of his times in that he put forward his experiments as testing hypotheses derived from theories. As he wrote in 1912: "To this end I searched for a crucial experiment that, by approaching the molecular scale, might give a solid experimental basis to attack or defend the kinetic theories."[24] His 1909 *Annales* paper begins with a theoretical discussion before his experimental setup and results are brought up. One should be wary, however, of concluding from these claims that this case illustrates the progressive division of labor between theoreticians and experimentalists that emerged in the early twentieth century, most evidently among the German-speaking physicists. In the French context and in particular in the scientific circles in which Perrin lived and worked, other disciplinary faultlines prevailed. Perrin saw himself primarily as a physical chemist and by no means as an experimental physicist who left theory to more competent colleagues.

Of Photography versus Drawing and the Uses of Statistics in Perfecting Observation

A comparison with contemporary representations of Brownian motion shows that figure 6.1 was by no means the only way in which Brownian motion could be observed and represented, and in fact was quite unusual. Figures 6.3 and 6.4 were, for instance, published in the same year. Maurice de Broglie and Henry Siedentopf, respectively, captured the trajectories of Brownian particles on photographic plates using long exposures. Their images superficially resemble many of the photographs taken by physicists investigating subatomic entities (ions, electrons, α particles) circa 1900, such as photographs of particle tracks in a cloud chamber.

FIGURE 6.3. Long-exposure photographic recording of the Brownian motion of ultramicroscopic tobacco smoke particles. Maurice de Broglie, "Enregistrement photographique des trajectoires browniennes dans les gaz," *Comptes rendus hebdomadaires des séances de l'Académie des sciences* 148 (1909): 1164.

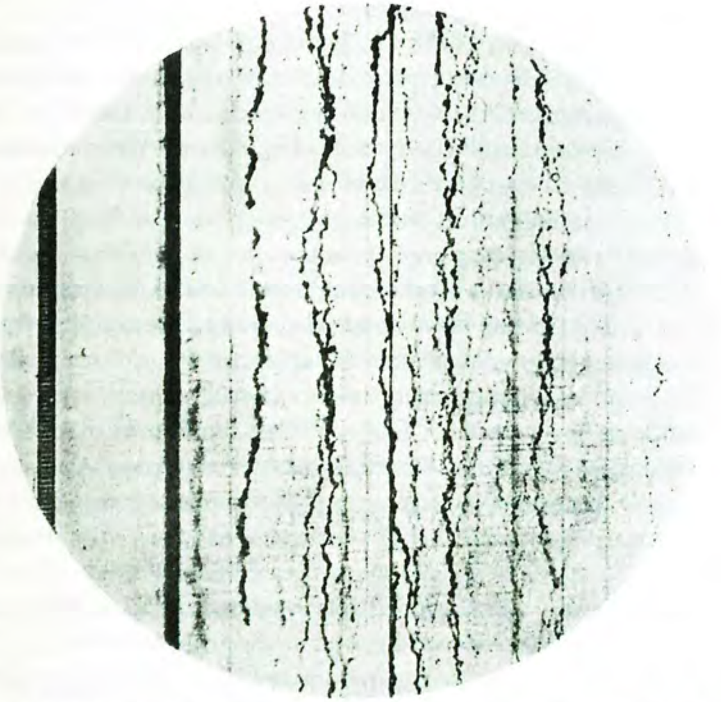

FIGURE 6.4. "Brownsche Molekularbewegung. Momentaufnahme auf fallender Platte mit aplanatischem Dunkelfeld-kondensor von Zeiss." Henry Siedentopf, "Über ultramikroskopische Abbildung," *Zeitschrift für Wissenschaftliche Mikroskopie* 26 (1909): plate.

The use of photography for observing and representing Brownian motion brought with it a whole new set of challenges. Perrin occasionally used the technique but found that though photography was less time-consuming and tiring to the eye than a camera lucida, "the eye is more sensitive than the photographic plate with regard to the visibility of very small, pale grains on a background that is nearly as pale."[25] And even when photographic observations could be made, the images obtained did not always give satisfactory results in print, as was especially the case in the observation of the smallest particles, visible only in a particular type of darkfield instrument, the ultramicroscope.[26] Maurice de Broglie apologized for the poor quality of figure 6.3: "The imperfection of the typographical reproduction accompanying this note scarcely gives an exact idea of the photographs obtained."[27] Worse still, photographs of Brownian motion were not so easily legible as experimental evidence. In figures 6.3 and 6.4, the particles' displacements could only be measured by magnifying the photographs, and the Brownian motion had to be distinguished from the overall motion of the gas or solution. In figure 6.4, the Brownian motion of falling particles can be identified as the small deviations on either side of the vertical.

Perrin's image looks very different. Figure 6.1 is a drawing, not a photograph. It looks more abstract. Even though it depicts the specific trajectories of three specific particles measured at a specific time and place, Perrin stripped from his drawing all elements that might have pointed to a specific experiment. Only the bare essentials remain: three trajectories and a grid. The absence of any indications on the figure suggests that these three lines are simply examples, perhaps even chosen at random, of Perrin's measurements. They stand for all his other measurements and all the measurements that can be made following his method. The strong presence of the grid in figure 6.1 helps emphasize the quantitative character of the observation. The reproduction of three exemplary trajectories in the publication thus served both to illustrate Perrin's own technique of measuring Brownian motion and his experimental verification of Einstein's theory. The unusual appearance of the trajectories as broken lines, underscored by the abstraction of the rendering, was a strong visual marker of the novelty of the method of measuring motion.

An interesting counterpoint to Perrin's approach was provided by the Marburg-based physicist Max Seddig, who developed around the same time a complex chronophotographic apparatus to test Einstein's theory of Brownian motion, capturing the position of ultramicroscopic particles at regular intervals of time. To avoid heating the liquid and thereby changing its viscosity during the experiment, Seddig used a stroboscopic technique to illuminate the solution intermittently, timing his photographic camera to open the shutters at exactly the same time. Seddig explicitly put forward his results as being superior to drawings because they were "objectively obtained": "Following the uncertain results of the subjective methods to date, an objective one should be attempted. As such, only the photographic process could come into question." Seddig referred here to Felix Exner's camera lucida drawings on a smoked glass plate of the Brownian motion of particles observed through a microscope (Perrin had not yet published at this point). Such drawings were necessarily unreliable for Seddig because the drawing hand was too slow to follow the particles' exceedingly rapid motions. His own photographic method, by contrast, supplied a "direct, experimental confirmation" of the kinetic hypothesis.[28] Yet Seddig, for reasons unspecified, did not publish any of his photographs, giving only the numerical values he had obtained in the form of a table. Paradoxically, his objective method did not yield images that could be shared with readers, leaving them no other choice than to trust in Seddig's skills and apparatus.

Perrin used a similar rhetoric, putting forward his own observations of Brownian motion as supplying direct evidence of the existence of atoms. But he diverged from Seddig in arguing that drawing was a perfectly legitimate technique. He admitted freely that marking the positions of the particles by hand introduced a measure of uncertainty: "each time that a grain's position is marked, a small error is made, analogous to that made when shooting at a target, which itself obeys the law of chance and which has the same effect on the readings as if one overlaid a second Brownian motion over the one under observation."[29] However, this inconvenience was largely compensated by the large number of observations made by Perrin and his collaborators. The striking agreement in the determinations of N made with a range of solutions and colloid particles and in different circumstances showed that small observing errors did not compromise the overall result.

There was also a more fundamental reason for Perrin to trust that the accumulation of measurements compensated for any approximation due to the lack of high-precision recording technology, hinted at in his mention above that observational error could be assimilated to a secondary form of Brownian motion: the fact that Brownian motion could only be investigated using a statistical approach because it was essentially stochastic in character:

> Thus appears a profound, eternal property of what we call a *liquid in a state of equilibrium*. This equilibrium only exists in an average manner and for great masses: it is a statistical equilibrium. In reality, the whole fluid is indefinitely and *spontaneously* agitated in movements that are all the more violent the

> smaller the portions they concern; the static notion of equilibrium is completely illusory.[30]

After investigations of radioactive decay (Exner, Curie) and the emission of α particles, Perrin's Brownian motion work constituted one of the early experimental attempts to apply statistics and probability theory to physical systems, and here Perrin followed closely the lead of James Clerk Maxwell and Ludwig Boltzmann.[31] Brownian motion, considered macroscopically, at the level of the solution, consisted in the minute and random fluctuations around the average state of the fluid. And like the radioactivity experimentalists, Perrin adopted a pragmatic attitude to fluctuations, using knowledge of their existence to develop better methods of measurement.[32] His whole strategy aimed at developing experimental and theoretical tools for smoothing out these fluctuations and obtaining average numbers that correspond to the equilibrium state of his emulsions. This statistical approach offered Perrin a means of achieving almost unlimited precision in his observations:

> Once this point is well established, one finds in this very equation, to determine the constant *N* and the constants depending upon it, a method that seems *capable of achieving an unlimited precision.* The preparation of a uniform emulsion and the determination of the values other than *N* that figure in the equation can indeed be pushed to the desired degree of precision. It is a simple question of patience and of time: nothing limits a priori the exactness of the results, and we can obtain, if we wish, the mass of an atom with the same precision as the mass of the Earth.[33]

Once experimental errors were excluded, the greater the number of observations, the closer the average of these measurements would be to the statistical equilibrium and the true value of *N*. In an experiment that involved measuring the concentration of grains at different levels of his solutions, Perrin made six series of measurements using different solutions and grain sizes. He noted a series of numbers whose average value reached a limit that corresponded to the average frequency of the grains at the level under investigation, remarking that "several thousand readings are necessary if one wants a little precision."[34] Just the sixth series of measurements involved counting no less than 11,000 grains using one method, and 13,000 grains using another.

For his Brownian motion work, Perrin was awarded the La Caze Prize in 1914. In their laudatio, the commissioners picked up on this aspect, noting that "Mr. Perrin's method is capable of achieving an infinite precision. It is only a question of patience; it comes down to a numbering of grains and a statistical calculation of averages whose accuracy increases proportionally with the square root of the total number of observations."[35] It was