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2 Cloud Chambers: The Peculiar Genius of British Physics

2.1 Cloud Chambers and Reality

When E. N. da C. Andrade surveyed atomic physics in 1923, noting that future historians would see the cloud chamber as one of the lasting accomplishments of the age, he was less sanguine about the durability of theoretical models. These he believed would come and go. Each conceptualization captured features of a restricted field of inquiry, but none (Andrade judged) could be pushed too hard. Optics invoked "ellipsoids of optical elasticity," the kinetic theory fielded small elastic spheres, quantum theory and the wave theory advanced entities in manifest contradiction with each other. Under the fruitful regime of imaginative work, he believed, these tensions were not at all a bad thing; it was far better for new theories, in their first youth, not to introspect excessively. Later, in their dotage, there would be ample time for the peaceful reconciliation of respectable consistency and textbook security. "Whatever may be the fate of the theories which have been so inadequately exposed in this book, whatever modifications or mishaps they may meet, the experimental facts which led to their formation, and those others to whose discovery they in their turn gave rise, will remain as definite knowledge, to form a lasting ornament to an age otherwise rich in manifold disaster and variety of evil change."[1] Despite the depths of felt disaster, the crisis of early quantum theory and the crisis of the Great War left the strong foundation of experimental results intact.

To Andrade, "The triumph of the atomic hypothesis is the epitome of modern physics."[2] So it has been for many from the late nineteenth century to the late twentieth. Philosophers of every stripe returned again and again to the

1. Andrade, *Atom* (1923), 295.
2. Andrade, *Atom* (1923), 1.

question of the reality of atoms; indeed, debates over the "real" existence of the atom stood at the origin of the twentieth-century debate over scientific realism. Consider the young Moritz Schlick (later leader of the Vienna Circle), who in 1917 protested: "[T]he strictly positivist picture of the world seems to be unsatisfactory on account of a certain lack of continuity. In narrowing down the conception of reality in the above sense, we tear, as it were, certain holes in the fabric of reality, which are patched up by mere auxiliary conceptions. The pencil in my hand is to be regarded as real, whereas the molecules which compose it are to be pure fictions." Only realism could patch these rents in the tissue of belief; only realism could reconcile the "antithesis, often uncertain and fluctuating," between the real and the working hypothesis.[3] Historically, the molecule (or atom) was the quintessential theoretical entity. Because the cloud chamber purported to display particulate matter piece by piece, realists and fictionalists alike found they had to confront these wispy cloud tracks. Many agreed that the best support for an experimentally grounded realism below the threshold of microscopy lay with this dramatic, if simple device.

The cloud chamber, by making the subvisible world visible (as in figure 2.17, below), entered the struggle over the status of "theoretical entities" during every one of the last seven decades. Those asserting the reality of the microphysical celebrated the visualization of particulate collisions, dissociations, and scatterings. Percy Bridgman, in his *Logic of Modern Physics* (1932), put stock above all in operations, procedures that (he believed) defined the valid concepts vital to any legitimate description of the world. "In searching for such new experimental operations it seems to me that by far the greatest promise for the immediate future is offered by improvements in our powers of dealing with individual atomic and electronic processes, such as we now have to a limited extent in the various spinthariscope methods of counting radioactive disintegrations, or Wilson's β-track experiments. In this self-conscious search for phenomena which increase the number of operationally independent concepts, we may expect to find a powerful systematic method directing the discovery of new and essentially important physical facts."[4] To Bridgman, as to Andrade, the importance of the Wilson chamber lay in its ability to display individual processes, directly, and not through a long, complicated, and indirect chain of inference. Unlike instruments that measured molecular properties in the aggregate, the cloud chamber singled them out, gave them, as Bridgman so often stressed in other contexts, an "operational" meaning—a meaning tied to features of the sensible world.[5] For

3. Schlick, *Philosophical Papers* (1979), 265–66.

4. Bridgman, *Logic* (1927), 224.

5. Elsewhere Bridgman wrote of the new experimental discoveries, in particular of Brownian motion and the Wilson cloud chamber tracks, "the convenience of the atomic picture has become so overwhelming that we have discarded the alternative point of view completely, and speak of the situation in different terms, as when it is often stated that all these facts have proved the *reality* of atoms." In fact, the Wilson chamber photographs, along

somewhat different philosophical reasons, Sir Arthur Eddington in 1939 had similar concerns about the subvisible world. Like Bridgman, he was troubled by the gap between the phenomena we observe and the theoretical entities that are the subject of physical laws and theories. The cloud chamber promised to bridge this gap. As far as Eddington was concerned, we "actually do count electrons in a Wilson chamber, where their tracks are made visible."[6] Tracks and electrons blurred together; the faint cloud trajectories bound the elusive reaches of theoretical objects to the perceptible. This visibility extended our sight, our sensible world, down to the atom itself: "We can almost see protons and electrons in a Wilson chamber; we can almost see mass being conserved. We do not actually see these things; but what we do see has a very close relation to them."[7]

Andrade, Bridgman, and Eddington used what they took to be the similarity of cloud tracks to underlying particle trajectories to dispense with the common view that atoms and molecules were "useful fictions" not to be considered fully real.[8] This "almost seeing" that made the cloud chamber an extension of our sense of sight is what I mean by the homomorphic form of evidence that becomes characteristic of the image tradition. With these palpable tracks, there appears to be no need for the "inference" of statistics characteristic of the logic tradition—one is (nominally) simply extending sight. Such a generalized argument for microphysical reality and against atomistic agnosticism began long before quantum mechanics. However, it and the cloud chamber assumed added significance when such physicists and philosophers as Pascual Jordan, Werner Heisenberg, and Henry Margenau confronted the philosophically problematic foundation of quantum mechanics.[9]

with allied developments, actually define what "the operational meaning of *reality* is" (Bridgman, *Physical Concepts* [1952], 22).

6. Eddington, *Physical Science* (1939), 175.

7. Eddington, *Physical Science* (1939), 134.

8. On debates over the reality of the atom, see Nye, *Question* (1984), xiiiff., and her *Molecular Reality* (1972).

9. Pascual Jordan (who had already made fundamental contributions to the new quantum theory) hoped in the 1930s and 1940s to unify quantum physics and biology through the processes that began at the scale of the microscopic and ended with the macroscopic. Here again, by illustrating the micro-macro connection, the cloud chamber played a crucial explanatory role. As Richard Beyler has noted, Jordan originally used what he called his "amplifier theory" to account for the way in which (macroscopic) living beings were "directed" or "steered" by quantum events within the cell nucleus; see Beyler's "Physics" (1994) and "Targeting the Organism," *Isis* 87 (1996): 248–73. This steering mechanism would, at least in principle, Jordan hoped, avoid both a vitalistic independence of biology from physics *and* any simpleminded mechanistic reductionism. (Occasionally, as Norton Wise has observed, Jordan's cellular leadership principle made substantive contact with his less than microcosmic nazistic proclivities; Wise, "Pascual Jordan" [1994].)

Among the interpretations of quantum mechanics that Jordan rejected was the notion that quantum "decisions" (for an electron to be in one eigenstate rather than another) were in any way tied to our conscious or mental states. Amplification processes in the cloud chamber, in which an electron track was "particle-like" (and not wave-like), illustrated the way in which amplifiers-cum-recording mechanisms fixed a quantum state without reference to anything supraphysical—no mental processes, no psychic phenomena, no consciousness. According to Jordan, this purely physical process of ionization and droplet coalescence is identical with the "quantum decisions" over

The question of the existence of molecules was put into focus earlier by the kinetic theory of gases, in which the observable properties of gases are deduced from the assumption that gases are made of molecules, even though an individual molecule has no observable effect. To the combative philosopher of science Herbert Dingle, the physics of the molecular theory of gases unreasonably abandoned the age-old philosophical ideal of coming to terms with the "nature of a real, external, material world"[10] and in its place offered merely useful relations among observables. It was, he contended in a lecture reprinted in *Nature* (1951), "a betrayal of the true mission of physicists according to the accepted philosophy." Whereas earlier physicists had been "dedicated to the investigation of reality," now the discipline had fallen to probing "mere appearances." In this fallen state, the ultimate constituents of nature, the molecules, had become "counters . . . dummies . . . useful conceptions," and while Dingle considered such fictionalism an inescapable part of modern physics, he wrote of it in tones of saddened resignation.[11] Shocked by Dingle's reluctant instrumentalism, Max Born retorted that the kinetic theory had much to recommend it beyond merely recapitulating a phenomenal description of gases, the kinetic theory's prediction of the specific heat of monatomic gases being just one example. But Born's ultimate argument was the cloud chamber. There, he contended, one hit bedrock. How could one stand face to face with track photographs while maintaining that microphysics had halted at fictionalism? Born: "Here the evidence of the reality

which his overly mystical contemporaries had spilled so much ink; Jordan, "Measurement," *Phil. Sci.* 16 (1949): 269–78, on 271. In a footnote (272), Jordan then elaborated by quoting himself from an unspecified source: "Ein Lichtquant, welches ein Silberkorn in der Photoplatte entwickelbar gemacht, oder welches durch Ionisierung eines Moleküls die Bildung eines Nebeltröpfchens in der Wilsonkammer eingeleitet, oder welches endlich in einem Zellkern eine Mutation zustande gebracht oder ein Bakterium getötet hat—dies Lichtquant hat sich lokalisiert, hat einen bestimmten Ort angenommen, unter Verzicht auf seine komplimentäre erscheinungsmöglichkeit als räumlich ausgedehnter Wellenzug." See also Jordan's *Geheimnis* (1941), 89, and *Erkenntnis* (1972), 156.

Throughout the 1950s, virtually all of the major philosophers and physicists who wrote on the philosophy of science figured the cloud chamber into their discussions of measurement, observability, and realism; see Margenau's *Physical Reality* (1950) and Heisenberg's *Wandlungen* (1945). Heisenberg wrote in *Wandlungen:* "As far as the track of an electron in a Wilson cloud chamber can be investigated, the laws of classical mechanics can be applied to it. Classical mechanics does predict the correct track of the electron. But if, without observation of its track, the electron is reflected at a diffraction grating, [classical mechanics fails]" (translated in *Philosophical Problems* [1952], 42; see also 43, 46). Heisenberg, along with many others, used Wilson's chamber to exemplify the full force of the wave-particle duality. Inquiries like Millikan's oil drop experiment, Margenau insisted, made electrons stand motionless, "which obviously no self-respecting wave would do." That feat alone would seem to exclude the emerging picture of the wavelike nature of the elementary charge. "But certainly the Wilson cloud chamber cannot be dismissed without mention. . . . It is difficult to conceive how a wave, uncollimated and free to spread in space, could make narrow tracks such as those observed in a cloud chamber" (Margenau, *Physical Reality* [1950], 318). For Margenau and Heisenberg, the cloud chamber served, not to refute fictionalism per se, but to discredit the more specific attempts to minimize the physical reality of complementarity. Once again, this time even more elaborately, the cloud chamber made experimentally accessible aspects of nature previously only representable through theory.

10. Dingle, "Philosophy," *Nature* 168 (1951): 630–36, on 634.
11. Dingle, "Philosophy," *Nature* 168 (1951): 630–36, on 633.

of molecules is striking indeed, and to speak of a 'dummy' producing a track in a Wilson chamber or a photographic emulsion seems to me—to say the least—inadequate."[12]

Where, Born demanded, is the difference between this and the everyday reality that we accept as the standard against which microphysics is to be compared? Following the long history of physicists' armed metaphors, Born shot back at Dingle: "You see a gun fired and, a hundred yards away, a man breaking down. How do you know that the bullet sticking in the man's wound has actually flown from the gun to the body?" No one actually sees or could see the bullet fly through the air, unless high-speed photographic apparatus of the type Ernst Mach had invented had been installed. Conceding that he could not defend the bullet story against a determined skeptic (e.g., someone prepared to claim that the bullet in flight is merely posited to preserve the laws of mechanics), Born did insist that the putative divide between medium-sized and very small objects could not serve as the basis for a principled distinction. A defender of the dichotomy would be forced, for example, to exclude the "existential evidence" of a cloud chamber alpha track, which can be seen, while allowing the bullet, which cannot.[13]

Born's remarks make clear that there is something deeply valued about the visual character of evidence: you *see* a gun fired, you *see* a man breaking down, but while no one *sees* the bullet fly through air, you can *see* the cloud chamber alpha track. Just this imprinted preference for visual evidence struck Stephen Toulmin in his *Philosophy of Science* (1953):

> To a working physicist, the question "Do neutrinos exist?" acts as an invitation to "produce a neutrino," preferably by making it *visible.* If one could do this one would indeed have something to show for the term "neutrino," and the difficulty of doing it is what explains the peculiar difficulty of the problem. For the problem arises acutely only when we start asking for the existence of *sub-microscopic* entities, i.e., things which by all normal standards are invisible. In the nature of the case, to produce a neutrino must be a more sophisticated business than producing a dodo or a nine-foot man. . . . [C]ertain things are, however, generally regarded by scientists as acceptable—for instance, cloud chamber pictures of α-ray tracks, electron microscope photographs or, as a second-best, audible clicks from a Geiger counter. They would regard such striking demonstrations as these as sufficiently like being shown a live dodo on the lawn to qualify as evidence of the existence of the entities concerned. And certainly, if we reject these as insufficient, it is hard to see what more we can reasonably ask for: if the term "exists" is to have any application to such things, must not this be it?[14]

12. Born, "Physical Reality," *Phil. Q.* 3 (1953): 139–49, on 142.
13. Born, "Physical Reality," *Phil. Q.* 3 (1953): 139–49, on 142.
14. Toulmin, *Philosophy of Science* (1953), 136. J. J. C. Smart quotes (or rather misquotes) extensively from Toulmin in "Theoretical Entities" ([1956] 1973), 95.

At this moment one has reached the end: not even the forensic evidence of everyday macroscopic life can compare with the evidence of a cloud chamber. As Born put it, if existence is not demonstrated by these photographs, then what conceivable occurrence would count? If not here, where?

When W. V. O. Quine turned to the cloud chamber a few years later, it was seemingly for a different, and rather more subtle, philosophical end. His "Posits and Reality" (1956) turned on the thesis that the directness of the link between sense impression and theoretical inference would vary over time—what was indirect evidence at one moment could be restructured into a direct link as the practices of science shifted. "Many sentences even about common-sense bodies rest wholly on indirect evidence; witness the statement that one of the pennies now in my pocket was in my pocket last week. Conversely, sentences even about electrons are sometimes directly conditioned to sensory stimulation, for example, via the cloud chamber." Quine's point is that statements about any kind of body gain significance only within a "collectively significant containing system," and so the ontological status of a particle might move from indirect to direct. While some people might use this "slack" within science to claim that a theoretical entity such as an electron had a more dubious status than a couch, Quine disagreed: why, he asked, should one ascribe full reality to everyday objects for which there exists no complete system of discourse at all? Positing subvisible entities has brought benefits to us, including the added simplicity of physical theory, the fecundity for further testing offered by the hypothetical entities, and the added scope of physical theory. "Benefits [of this sort]," Quine concluded, "are what count for the molecular doctrine or any, and we can hope for no surer touchstone of reality."[15] On this view, electrons may well turn out to be *better* established than the room-sized objects against which they are usually, so deprecatingly, contrasted. Like Born, Quine looked at cloud chamber tracks and saw the best argument for existence, not a weak cousin to everyday sights and sounds.

Observation sentences could not live on their own, Quine taught. And throughout the 1960s, this lesson was repeated over and again, though increasingly often with a relativist slant that was not Quine's own. The positivist doctrine was inverted: now the cloud chamber became the target of choice for anyone arguing against observation as bedrock. In perhaps the best, most sustained version of the new *anti*positivist doctrine, Russell Hanson used his *Concept of the Positron* (1963) to defend the view that tracks in a cloud chamber that are now "read" as evidence for the positron were earlier "read" out of physical significance altogether. As Hanson concluded: "Whenever seen, such tracks were discounted as 'spurious,' or as 'dirt effects.' Certainly no experimental physicist before late 1932 made any such track his prime object of study. Part of the

15. Quine, "Posits and Reality" ([1955] 1973), 161.

function of [Hanson's *Positron*] will be to understand why this is so, why such tracks were always overlooked, underevaluated, or explained away."[16] Theory gave meaning to pair-creation tracks just as surely as a gestalt image of a duck or rabbit gave meaning to lines and squiggles on a page. Hanson contended that there was literally no raw data before theory made it into evidence for pair creation; the tracks in many instances could hardly be seen.

In 1970, Mary Hesse dubbed the view that observation and theory were inextricably enmeshed, one with the other, the "network model"; she too invoked the cloud chamber. Unlike many of her contemporary antipositivists, however, Hesse scrupulously distinguished scientific practice (which often treated observations or experiments as highly distinct from theory) from a priori claims that there was something about observation sentences that rigidly distinguished them from theoretical statements. Consequently, she was ready, as Quine was, to concede that "examples are thinkable where highly theoretical descriptions would be given directly: 'particle-pair annihilation' in a cloud chamber." Hesse, following Quine and Hanson, rejected the notion that it is possible to withdraw to a pure observation language from theory-laden descriptions. Even if one replaces "particle-pair annihilation" with "two white streaks meeting and terminating at an angle," the second does "not show that [it is] free from lawlike implications of [its] own, nor even that it is possible to execute a series of withdrawals in such a way that each successive description contains fewer implications than the description preceding it."[17] Hesse's point is that there is no distinction between observation and theory in kind—there is no safe haven to which one can retreat that is free of any reference to theoretical laws. True, it may be possible to find temporary shelter from any particular theory (e.g., of pair creation), but that possibility does not imply that the language of cloud chamber track geometry can be freed of *all* theory.

Bas van Fraassen was therefore building on a long and illustrious tradition when, in 1980, he attacked the notion that the cloud chamber made the electron "observable." Van Fraassen's starting point was a challenge to the very notion of "observability." Like "portability," he contended, the term refers to our human capacities (and not to some entirely abstract definition). We could say that there is nothing special about the bounds of what it is within our strength to lift—in other words, that "in principle" the Empire State Building is portable (were we giants)—but this would be to bend the term "portability" out of all recognizable similarity with standard usage. Similarly, "observability" does not allow for our having the eyes of eagles (or electron microscopes): "observability" refers to what we, as humans, can do under normal circumstances. The point is not that instruments are never useful as stepping stones to observability that would hold

16. Hanson, *Positron* (1963), 139.
17. Hesse, "Observation Language" ([1970] 1974), 23–24.

under better conditions. After all, van Fraassen readily acknowledged that "a look through a telescope at the moons of Jupiter seems to me a clear case of observation, since astronauts will no doubt be able to see them as well from close up."[18] Cloud chamber tracks are *not* like this: the electron or alpha particle is not observable by humans under any conditions; therefore, van Fraassen argued, it occupies a very different place in our epistemic hierarchy from that held by objects humans can observe. We carry an everyday realism about things we can see and feel, but our relation to the microworld is one that he concluded is inescapably instrumental: the cloud chamber reveals regularities of phenomena that are observable. That is all. It does not offer us a direct view of the subvisible in any way "like" or "almost like" our seeing a building, dodo, or man.[19] By now, it will come as no surprise that realists like Alan Musgrave responded to van Fraassen by invoking the cloud chamber in other ways, claiming, for example, that to speak of "detecting" a particle by the device was already to believe it to be "true that the object really exists."[20]

There you have it. The image of the cloud chamber has flown like a banner above almost every realist and antirealist crusade. To Bridgman, the American operationalist, the Wilson chamber was just the procedure needed to give meaning to utterances about the subvisible world of the atom—more, it provides an instance of what we mean by the term "exist," full stop. To Toulmin and Born, the cloud chamber was the very symbol of realism; Wilson's tracks are the final station of the argument against a division between ordinary objects and the population of the submicroscopic. As Born put it, "[I]f the term 'exists' is to have any application to such things, must not this be it?" To Jordan, Heisenberg, and others, cloud chamber pictures illustrated the spacetime half of the complementarity relations—how much more like a realistically interpreted classical particle could one get than the detailed footprints of an alpha particle skittering through the clouds? For Quine, the cloud chamber's images exemplified the most direct evidence for theoretical entities that we were likely to get, and in combination with the rest of our physics, our account of the microworld

18. Telescopes are not, however, to be analyzed in the same terms as microscopes since "the purported observation of micro-particles in a cloud chamber seems to me a clearly different case—if our theory about what happens there is right. . . . Suppose I point to [a contrail that looks like a cloud chamber track] and say: 'Look, there is a jet!'; might you not say: 'I see the vapour trail, but where is the jet?' Then I would answer: 'Look just a bit ahead of the trail . . . there! Do you see it?' Now, in the case of the cloud chamber this response is not possible. So while the particle is detected by means of the cloud chamber, and the detection is based on observation, it is clearly not a case of the [p]article's being observed" (van Fraassen, *Scientific* [1980], 16–17).

19. A closely related point was made many years ago by Nagel, *Structure of Science* (1961), where the cloud chamber occurs in the course of a longer argument intended to dismiss the realist-antirealist debate as "a conflict over preferred modes of speech" (152).

20. E.g., Musgrave, "Realism" (1985), 205–6. It should be emphasized once again that there are many kinds of realist and antirealist arguments that do not depend on the cloud chamber. The point here is that more than any other experimental apparatus, the cloud chamber has formed the locus classicus of the debate for many decades and many (contradictory) philosophical positions.

suggested a pragmatic realism. Full-fledged antipositivists, à la Hanson and Hesse, invoked the cloud chamber photograph as the best example of "neutral data" but went on to argue that even statements about these photographs could never truly be independent of theory. Against Born, van Fraassen celebrated theoretical links among tracks without conceding an inch to the reality of any entity ultimately causing the tracks, while his opponents saw realism embedded in the very notion of cloud chamber detection.

A vast spectrum of views among physicists surrounds the cloud chamber, a similar variety among philosophers. Yet all these accounts share the view that the cloud chamber, perhaps better than any other instrument, instantiates the production of direct evidence for the subvisible world of microphysics. The chamber revealed the positron and the muon to Carl Anderson and allowed George Rochester and C. C. Butler to "see" a new class of "strange" particles. John D. Cockcroft and Ernest T. S. Walton used the device to demonstrate the existence of nuclear transmutation. Indeed, for generations of cosmic ray physicists, and then briefly for accelerator physicists, the cloud chamber gave concrete meaning to the wide range of new particles whose discovery inaugurated the field of particle physics. Cloud chambers were the prototypes for the spate of later detectors that we will examine in the following chapters, including the high-pressure chamber, sensitive nuclear emulsions, and most important, the bubble chamber.

Given the physical and philosophical background, it comes as a shock to find that this quintessential particle detector had its origin in a time, place, and subject utterly removed from the scattering, production, and disintegration of particle physics. But by transporting ourselves away from particle physics, out of the postwar laboratory, and back to an era of Victorian meteorology, we can begin to reconstruct the process by which this "cloud room" moved from the storm-drenched hills of Scotland to become, as Lord Rutherford called it, "the most original and wonderful instrument in scientific history."[21] Only through the lens of Victorian experimentation, through the fascination with the wholesale reproduction of the great forces of nature, can one see the origin of the image tradition. My hope is that this genealogy will help us understand what these vivid pictures have come to be for so much of our century, what Samuel Johnson's rock was for his: the final word in evidence.

2.2 The Romance of Re-creation

Despite the uses to which his device was later put, Charles Thomson Rees Wilson, the creator of the cloud chamber, cannot possibly be considered a particle physicist.[22] From his earliest work in 1895 to his last ruminations on

21. "Professor C. T. R. Wilson" [Obituary], *Times* (London) 16 November 1959, 16.

22. There are only a few articles written about C. T. R. Wilson, who lived from 1869 to 1959. The most complete is Blackett, "Charles Thomson Rees Wilson," *Biog. Mem. F.R.S.* 6 (1960): 269–95; Turner has a short

thunderclouds when he had turned ninety, Wilson was riveted by the phenomena of weather. Even J. J. Thomson, one of his greatest admirers, commented that Wilson's experimentation on fogs was not "a very obvious method of approaching transcendental physics."[23] By "transcendental physics" Thomson meant analytical research into the basic structure of matter that shunned complex but mundane problems like fog and rain. Though the eventual use of the cloud chamber has led atomic, nuclear, and particle physicists to appropriate Wilson as one of their own, his life's work is incomprehensible outside the context of weather. One must come to terms with the dust, air, fogs, clouds, rain, thunder, lightning, and optical effects[24] that held the rapt attention of Wilson and his nineteenth-century contemporaries in order for the invention of the cloud chamber to make historical sense.

The historical reconstruction of mundane as well as transcendental physics is necessary to set Wilson's cloud chamber work in its properly Victorian context. That world will emerge as a startling source of modern physics, revealing in Wilson's work a coherence that is entirely lost when it is divided into "meteorology" on the one hand and "physics" on the other. Lying between or perhaps in the intersection of the two domains is an area better called "mimetic

article, "Wilson," *Dict. Sci. Biog.* 14 (1981): 420–23. Wilson himself left two retrospective articles, "Ben Nevis," *Weather* 9 (1954): 309–11, and "Reminiscences," *Not. Rec. Roy. Soc. London* 14 (1960): 163–73. His Nobel Prize Lecture, in 1927, gives a general overview of his work ("Making Visible Ions" [1965]). In addition, Wilson wrote chapter 7, "1899–1902," of *History of the Cavendish* (1910). Crowther discussed Wilson in two of his books, but without much detail: *Scientific Types* (1968), 25–55, and *Cavendish Laboratory* (1974), 126–75, 213–24. Tomas, "Tradition" (1979), has a provocative thesis that uses art-historical techniques to fix the authorship of certain chambers; Tomas does not use Wilson's notebooks. For a modern explanation of the cloud chamber, see J. G. Wilson, *Cloud-Chamber Technique* (1951). Meteorologists recall Wilson as an important figure in their discipline; see, e.g., Halliday, "Some Memories," *Bull. Amer. Met. Soc.* 51 (1970): 1133–35. The most important source for the cloud chamber, however, is Wilson's own laboratory notebooks, which will constitute the core of this study. They are referenced below.

23. Thomson, *Recollections* (1937), 416. This language was not unique to Thomson, as is evident when Airy wrote Stokes in April 1867: [The Mathematical Tripos] "gave no adequate encouragement or assistance to men of the second degree, who might by proper direction of their studies become men highly educated as philosophers though not so transcendent as the first" (quoted in David B. Wilson, "Experimentalists," *Hist. Stud. Phys. Sci.* 12 [1982]: 325–71, on 338).

24. Several different meteorological optical effects will be mentioned in this paper. The terminology gets a bit confusing, so a short description of each phenomenon is given here for future reference. A *corona* is a series of concentric colored rings formed by the diffraction of light from the sun or moon when it hits water droplets in clouds or high fogs. A *halo* is also a series of colored rings, produced, however, by refraction through ice crystals. Although these two terms are explicitly defined by meteorologists, they are used inconsistently by different observers. A *glory* consists of colored rings that are seen around the shadow that an observer casts on a cloud or a fogbank. It is caused by the diffraction of light by water droplets and can occur with a corona. The entire phenomenon of the shadow and rings is sometimes called a *Broken Spectre*. A *Bishop's Ring* is not due to water droplets but instead is due to the interaction of light with solid particles in the atmosphere. Bishop's Rings were often seen after the Krakatoa eruption of 1883 but are very rare in normal circumstances; they depend on there being a large number of particles in the atmosphere and therefore are usually seen after a catastrophic explosion. See Whipple, "Meteorological Optics" (1923), 3:527–29.

experimentation," a term that will designate the attempt to reproduce natural physical phenomena, with all their complexity, in the laboratory.

It is with mimetic experimentation that Wilson's work begins, and the evolution of his thought and his machine must be understood as a continuing dialogue between general theories of matter held at the Cavendish Laboratory and particular demonstrations of remarkable natural phenomena. The Wilson cloud chamber is the material embodiment of this conversation between the analytic and the mimetic and, as such, became the foundation for the hundred-year reign of the tradition of image-making devices. Exploring the origin of the cloud chamber will offer insight into the intersection and subsequent transformation of the material cultures of both meteorology and matter theory. By following the movements of this specific instrument we will be able to study the formation and disintegration of "condensation physics," a transitory subfield of physics. Then, as the device moves away from condensation phenomena per se, an analysis of the subsequent construction and deployment of cloud chambers will expand our picture of the historical roots both of particle physics and of physical meteorology.

For the Victorian imagination, the extremities and rarities of nature held an endless fascination. Explorers ventured to the ends of the empire, to the deserts, jungles, and icecaps. Painters and poets tried to capture the power of storms and the grand scale of forests, cliffs, and waterfalls. And both artists and scientists recognized a tension between the rationalizing, lawlike image of nature proffered by the natural philosophers and the irreducible, often spiritual aspect of nature presented by their contemporaries in the arts.[25] There was a similar split in science itself between an abstract, reductionist approach to the physical world and a natural historical approach that authors from Goethe to Maxwell had dubbed the "morphological" sciences.[26] Of these sciences, Goethe took particular joy in meteorology, for "atmospheric phenomena can never become strange and remote to the poet's or to the painter's eye."[27] Up through the eighteenth century, there had been no systematic classification of clouds. Then in 1802–3, a British chemist, Luke Howard, presented a classification system that he modeled on Linnaean taxonomy.[28] Through Goethe, Howard's system entered the cultural mainstream.

In front of a small philosophical society, Howard sorted clouds according to a "methodical nomenclature": "cirrus," "cumulus," and "stratus." Howard chose Latin for his system because he thought Latin would capture the universal

25. Much of the debate was fueled by reforms in scientific instruction. See Super, "Humanist" (1977).

26. *Encyclopaedia Britannica*, 9th ed., s.v. "physical sciences" (by J. C. Maxwell): "What is commonly called 'physical science' occupies a position intermediate between the abstract sciences of arithmetic, algebra, and geometry and the morphological and biological sciences." Goethe, quoted in Merz, *European Thought* (1965), 212–13.

27. Goethe, quoted in Badt, *Constable's Clouds* (1950), 17.

28. Howard, "Modifications of Clouds," *Phil. Mag.* 16 (1803): 97–107; 344–57; 17 (1803): 5–11.

validity of his scheme. Moreover, in contrast to chemists who used Greek terms to represent invisible chemical entities, Howard wanted to classify clouds "by [their] visible characters, as in natural history."[29] No doubt this affinity with natural history appealed to Goethe. When he discovered Howard in 1815, the poet was deeply impressed with the new way of seeing clouds: "I seized on Howard's terminology with joy," Goethe announced, for it provided "a missing thread."[30] From Goethe the Dresden school of painters learned to view clouds differently; the art historian Kurt Badt surmises that it was Luke Howard's expanded work of 1818–20 that triggered John Constable's astonishing cloud studies of 1821–22 (though this has been disputed; see figures 2.1 and 2.2).[31] Whether or not Constable's studies were inspired by the new meteorology, it is clear that he avidly followed the popular work of Thomas Forster, *Researches about Atmospheric Phaenomena* (1815), mixing theories of the weather with Forster's own observations. Constable challenged some passages and marked others of particular interest to him, including the following bit of Forster: "[O]n the barren mountain's rugged vertex, in the uniform gloom of the desert, or on the trackless surface of the ocean, we may view the interesting electrical operations which are going on above, manifested in the formation and changes of the clouds, which bear water in huge masses from place to place, or throw it down in torrents on the earth and waters; and occasionally creating whirlwinds and water spouts; or producing the brilliant phaenomena of meteors and of lightening; and constantly ornamenting the sky with the picturesque imagery of coloured clouds and golden haze."[32] Meteorology, given cultural weight through this kind of evocative popular science, fostered cloud studies in painting, in poetry, and later in photography. Clouds became a central figure in romantic thought.

For Luke Howard the study of clouds was much more what Goethe would call "morphological" than "abstract." Howard never felt at ease with mathematics or the newer, more mathematized forms of chemistry.[33] At the end of the nineteenth century, the historian Theodore Merz commented on this dual aspect of systematic thought; he stressed that the "abstract sciences" (e.g., optics,

29. Howard, "Modifications of Clouds," *Phil. Mag.* 16 (1803): 97–107, on 98.

30. Goethe, quoted in Badt, *Constable's Clouds* (1950), 18.

31. Louis Hawes has vigorously disputed the central role of Howard's meteorology in the development of Constable's sky studies. Hawes argues instead that "environmental conditions," such as Constable's work in his father's windmill, along with the cloud depictions by earlier painters played a more important role; see Hawes, "Constable's Sky," *J. Warburg Courtauld Inst.* 32 (1969): 344–65. It seems, however, that some of Hawes's claims have themselves run into trouble. For example, he writes that Constable "nowhere mention[s] Howard or his terminology" (346). Three years after Hawes's article appeared, Constable's copy of Forster's *Atmospheric Phaenomena* (1815) came to light, and it includes, among many other notations, references to Howard's meteorology; see Thornes, "Constable's Clouds," *Burlington Mag.* 121 (1979): 697–704.

32. Thornes, "Constable's Clouds," *Burlington Mag.* 121 (1979): 697–704, on 698.

33. At Goethe's request, Luke Howard composed a brief autobiographical statement that is reproduced in Goethe's collected works: see Howard, "Luke Howard an Goethe" ([1822] 1960); on chemistry and mathematics, see 824–25.

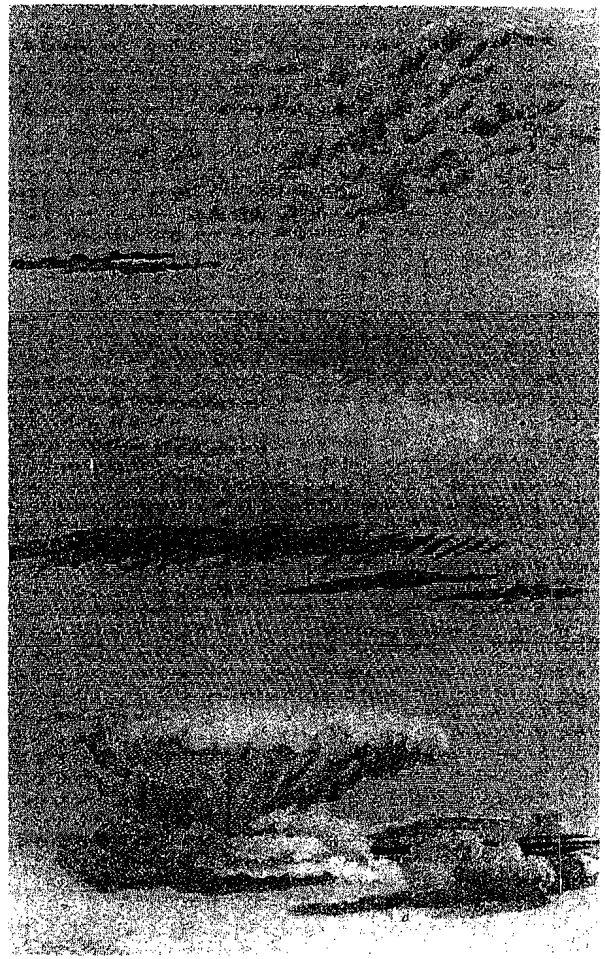

Figure 2.1 Luke Howard's clouds (1802–3). Luke Howard's classification system, widely disseminated by Goethe, launched an aesthetic, popular, and scientific fascination with clouds in Victorian times. Here, *a* exhibits cirrocumulus up close and in the distance; *b* shows a light cirrostratus (just before rain) and a dark cirrostratus (in twilight); and *c* displays "mixed" and "distinct" cumulostratus. Note: Captions in original are misnumbered. Source: Howard, "Modifications of Clouds," *Phil. Mag.* 16 (1803): 97–107, plate on 64.

Figure 2.2 Constable's cloud study (1821–22). John Constable's wildly popular representations of clouds were generally perceived to have broken with previous, formal means of depicting them. His paintings of dramatic sky phenomena brought meteorological explorations front and center for Victorian culture. Reproduced here is Constable's cloud study: "Looking South." 21st Sept. 1822. 3 o'clock afternoon, Brisk wind at East, warm and fresh. Source: Constable, *Study* (1822): No. B1981.25.116, Yale Center for British Art, Paul Mellon Collection.

mechanics, electricity, and magnetism) involved either "literally a process of removal from one place to another, from the great work- and store-house of nature herself, to the small workroom, the laboratory of the experimenter," or a process of removal "carried on merely in the realm of contemplation."[34] The morphological sciences had, almost by definition, a place in nature itself.

But abstraction was the true goal of physics, according to the leading practitioners of matter physics, not classification of the "countenance of the sky" or the earth. William Thomson (Lord Kelvin), for example, allowed natural history a merely preliminary role: "[I]n the study of external nature, the first stage is the description and classification of facts observed with reference to the various kinds of matter. . . . [T]his is the legitimate work of Natural History. The establishment of general laws in any province of the material world, by induction from facts collected in natural history, may with like propriety be called Natural Philosophy." For the "abstract" or "natural philosophical" investigator, the goal of experimentation was to extract the universal law from the particular

34. Merz, *European Thought* (1965), 200.

description—and to thereby achieve a more "transcendent" truth than could be obtained by excessive attention to special phenomena. The success of the natural philosophical approach was present for all to see in Maxwell's theory of electrodynamics and Kelvin's theory of heat. Both took certain known phenomena and gave them a mechanical, dynamical basis.[35]

But despite the manifold benefits of the "chemical and electrical laboratories with the calculating room of the mathematician on the one side, and the workshop and factory on the other,"[36] Victorian scientists realized that there were times when the abstract scientific method was inadequate. Difficulties arose because the natural philosopher who exploited analysis and abstraction exclusively was "forcibly reminded that he [was] in danger of dealing not with natural, but with artificial, things. Instances are plentiful where, through the elaboration of fanciful theories, the connection with the real world has been lost."[37] When the natural philosophers invented a dynamical basis for phenomena, they ran the risk of inferring from the success of a model the existence of possibly spurious entities put forward in the model.

Opposing the "one-sided" working of abstract science lay another ideal of investigation, embodied in the morphological sciences. These sciences were motivated, as Merz somewhat rhapsodically put it, by "the genuine love of nature, the consciousness that we lose all power if, to any great extent, we sever or weaken that connection which ties us to the world as it is—to things real and natural: it finds its expression in the ancient legend of the mighty giant [Antaeus] who derived all his strength from his mother earth and collapsed if severed from her."[38]

The morphological scientist "look[s] upon real things not as examples of the general and universal, but as alone possessed of that mysterious something which distinguishes the real and actual from the possible and artificial."[39] Explaining that he is borrowing and extending Goethe's term, Merz included the large-scale study of landscape—mountains and valleys, glaciers, land and water, stratification of rocks, and formation of clouds—under the rubric "morphological" sciences.[40] Alexander von Humboldt, the great explorer and measurer of natural phenomena like wind and air pressure, was in Merz's eyes the leading advocate of this new line of investigation: Humboldt, Merz told his readers, "may be called the morphologist of nature on the largest scale."[41]

35. Thomson, "Introductory Lecture" (1846), quoted in Smith and Wise, *Energy and Empire* (1989), 121–22; and *Encyclopaedia Britannica*, 9th ed., s.v. "physical sciences" (by J. C. Maxwell).
36. Merz, *European Thought* (1965), 202.
37. Merz, *European Thought* (1965), 201.
38. Merz, *European Thought* (1965), 202.
39. Merz, *European Thought* (1965), 203.
40. Merz, *European Thought* (1965), 219.
41. Merz, *European Thought* (1965), 226.

Susan Faye Cannon, writing in the 1970s, was thus echoing Merz when she identified the "great new thing in professional science in the first half of the 19th century" as "Humboldtian science, the accurate, measured study of widespread but interconnected real phenomena" such as "geographical distribution, terrestrial magnetism, meteorology, hydrology, ocean currents, the structures of mountain-chains and the orientation of strata, [and] solar radiation." By their commitment to work on location, the Humboldtians opposed "the study of nature in the laboratory or the perfection of differential equations."[42] The laboratory dealt with artificially isolated phenomena—not nature—and differential equations disembodied the variegated reality of actual things. Geologists held just such an antilaboratory view; Mott Greene reports that two of the most prominent early-nineteenth-century geologists were "resistant to the idea that laboratory testing could recreate or duplicate natural conditions."[43] According

mountains. Should the program succeed, Reyer wrote, "we will have to assign the long-ignored geological experiment a deep significance."[45] Mimesis preserves the morphological geologist's ideal of capturing nature in toto but does so through imitation, not the hammer.

During this same period, much of meteorology, like geology, welcomed experiment only if it could imitate nature without gross distortion. An advocate of this morphological approach was John Aitken; as we will see, it was on Aitken's miniature cloud building that Wilson most liberally drew. Aitken was born at Falkirk, Scotland, on 18 September 1839, son of the head of an established legal firm. After studying engineering at the University of Glasgow, Aitken served an apprenticeship in Dundee, followed by three further years with the shipbuilders Napier and Sons in Glasgow. Almost immediately, his career as a marine engi-

worked as a businessman. Although William had never received what he considered an adequate education, he was determined that Charles should have one.[49]

At 15 Charles entered Owens College, then part of Victoria University in Manchester, in order to prepare for a medical career. The relatively new institution had equipped itself for science teaching by drawing on middle-class manufacturers to sponsor its scientific and technological facilities.[50] Before he could begin studying medicine, Wilson had to complete a course of study that included lectures in botany, zoology, geology, and chemistry. On graduating with his B.Sc. in 1887, and after spending an additional year studying philosophy, Latin, and Greek, Wilson won a scholarship at Cambridge. William Wilson, obviously proud of his younger brother, wrote from India in January 1888 that "[o]ne of the pleasures of my life was imparted to me on Sunday morning when I heard that you had been successful at Cambridge. I was *very* pleased to hear it, and hope the acquisition of this scholarship may eventuate in much real advantage to you. Now that the impetus of success has set in I expect it won't expend its energy until it finds you seated in the presidential chair of the British Association!"[51]

Charles completed Cambridge's Natural Science Tripos in 1892, keen to pursue science but fearful that he would be unable to support the other members of his family. One route that appeared open was the vocation of a mapper: "I felt I might be of use as an explorer as I had some knowledge of a wide range of sciences and powers of endurance tested on the Scottish hills."[52] It was a career entirely in keeping with the Humboldtian, morphological tradition—exploration in the service of science, the attempt to add precision to knowledge of the variety of nature by examining it in situ and reproducing it to scale. While at Owens, Wilson had spent his school vacations exploring the Scottish countryside, his eye opened by a trip to the North High Corrie in Arran (an island off the West Coast). There he was "strongly impressed with the beauty of the world. . . . [I]n Manchester I spent all my spare time looking for and studying beetles and pond-life which I also learned to love."[53]

Like many Victorians, Wilson and his brothers took up photography. The depiction of nature must have appealed to him, as he specialized in pictures of landscapes and clouds, from which his mother would often paint (see figures 2.3 and 2.4).[54] In Calcutta, William was devoting his "spare evenings . . . to

49. For biographical details on Wilson's childhood and youth, see Blackett, "Charles Thomson Rees Wilson," *Biog. Mem. F.R.S.* 6 (1960): 269–95, on 269–70; and Wilson, "Reminiscences," *Not. Rec. Roy. Soc. London* 14 (1960): 163–73, on 163–64.

50. See Thackray, commentary in "Physical Science," by Sviedrys, *Hist. Stud. Phys. Sci.* 2 (1970): 127–51, on 148.

51. William Wilson to C. T. R. Wilson, 24 January 1888, CWP.

52. Wilson, "Reminiscences," *Not. Rec. Roy. Soc. London* 14 (1960): 163–73, on 165.

53. Wilson, "Reminiscences," *Not. Rec. Roy. Soc. London* 14 (1960): 163–73, on 164.

54. J. Wilson (Wilson's daughter), interview by the author, 10 September 1986.

Figure 2.3 Early Wilson cloud photograph I (ca. 1890). Taken by C. T. R. and George Wilson. Source: Courtesy of Miss Jessie Wilson, CWP, rephotographed from originals by the author.

photography. I have got my enlarging lantern and have been experimenting."[55] About the same time, Wilson and his brother George reported to their elder sibling on their own first steps in the new art. William guided his novices by mail, counseling them even in the details of their selection of a lens: "But I must hasten to give you my opinion of your first pictures. Your exposure has perhaps been full, but your development has been first rate, I think, judging from the pictures. You have printed them very well. I would strongly recommend you to soak your negatives in every case in a saturated solution of alum after they have been fixed. . . . I would advise you to keep a supply of Manchester or Wratten and Wainwrights [instead of Ilfords] beside you for first class pictures."[56]

Thus, long before C. T. R. Wilson turned his camera on the microphysical world, he had used it to recreate the natural world, especially the crags, cliffs,

55. William Wilson to C. T. R. Wilson, 28 November 1887, CWP. We would like to thank Miss J. Wilson for making this and other documents available to us.

56. William Wilson to C. T. R. Wilson, [1887?], CWP.

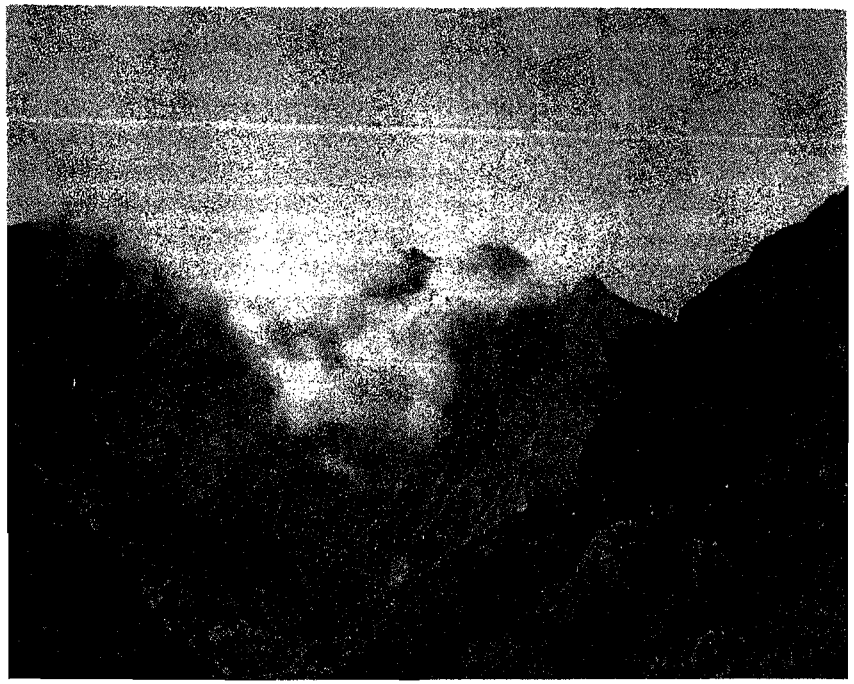

Figure 2.4 Early Wilson cloud photograph II (ca. 1890). Taken by C. T. R. and George Wilson. Source: Courtesy of Miss Jessie Wilson, CWP, rephotographed from originals by the author.

and clouds of Scotland. Later, Wilson took his camera with him on his hikes to Ben Nevis and elsewhere and, alongside scientific notes, inscribed his notebooks with the circumstance of each individual exposure.[57] The immense popularity of such amateur nature photography was a notable feature of Victorian Britain, and in general British photography occupied "a stylistic and conceptual midpoint between French and American photography of the nineteenth century." Where American photographers were for the most part scientists or entrepreneurs and the French tended to come from the ranks of painters, the Victorian amateurs "compromise[d] between these two extremes . . . successfully

57. CWnb A21, e.g., 9–16 April 1907. Wilson's lab books (CWnb) are housed at the Royal Society, London. They have been indexed in Dee and Wormell, "Index," *Not. Rec. Roy. Soc. London* 18 (1963): 54–66. Dee and Wormell have divided Wilson's notebooks into two groups: A, which deals with condensation phenomena and includes the notes on the development of the cloud chamber, and B, which treats the earth's electric field and thunderstorms; as we shall see, the division between the two categories is rather artificial. Hereinafter, references to the Wilson notebooks will simply be by letter, number, and date of entry.

manag[ing] to blend emotional evocation with an objective assertion of sheer physical fact."[58]

The British desire to reproduce "sheer physical fact" had no better object in the 1880s than the effects caused by the violent eruption of Krakatoa on 26 and 27 August 1883. Sounds of the explosion echoed through Rodriguez and Diego Garcia, respectively 3,080 and 2,375 miles from the volcano. Windows burst and walls cracked a hundred miles distant.[59] Filtered by the staggering mass of particles shot into the upper atmosphere, strange optical phenomena appeared around the world. From Honolulu an observer saw a "peculiar lurid glow, as of a distant conflagration, totally unlike our common sunsets";[60] Herr Dr. A. Gerber from Glöckstadt recalled in the first volume of *Meteorologische Zeitschrift* how "[t]he sailors declared, 'Sir, that is the Northern Lights!' and I thought I had never seen Northern Lights in greater splendour. After 5 minutes more the light had faded . . . and the finest purple-red rose up in the S.W.; one could imagine oneself in Fairyland."[61] Reports from Italy, Ohio, Switzerland, Portugal, India, Japan, and Australia cascaded into newspapers; both unknown observers and renowned scientists posted notices to all the major scientific journals. Even Hermann von Helmholtz in Berlin took the time to report on "cloud-glow" and the remarkable illusion of green clouds.[62]

Photographers, artists, and scientists all tried to capture the extraordinary visual events: mere black and white photographs could not do justice to the display. William Ascroft drew the extraordinary Bishop's Ring (corona) he saw on 2 September 1884 (figure 2.5), and at 10-minute intervals he drew quick pastels of the yellow-green sunset over the Thames (figure 2.6).[63] When in 1886, the Royal Society published its comprehensive report on the events of Krakatoa, they included Ascroft's sunset sketches as scientifically useful. Throughout the years following the eruption, debate raged over whether the sun's strength was changing, whether the effects really were correlated with the eruption, and if so what mechanisms could account for them. Even if no explanation was absolutely persuasive, at least scientists could mimic this extraordinary event; Karl Kiessling attempted to reproduce the dramatic coronae of late 1883 with a "diffraction chamber" filled with dust, ordinary air, and filtered vapor.[64]

Aitken was well prepared to contribute to the Krakatoa debate. In 1883 he was continuing the important work he had begun with his article "On Dust,

58. Millard, "Images" (1977), 23–24.
59. Judd, "Volcanic Phenomena" (1888), 27.
60. Rollo Russell and Archibald, "Optical Phenomena" (1888), 153.
61. Rollo Russell and Archibald, "Optical Phenomena" (1888), 157.
62. Rollo Russell and Archibald, "Optical Phenomena" (1888), 171.
63. Zaniello, "English Sunsets" (1981), locates the Krakatoa sunsets as a meeting point for scientific and artistic concerns in Britain during this decade.
64. Kiessling's experiments are described in Rollo Russell and Archibald, "Optical Phenomena" (1888).

Figure 2.5 Ascroft pastel (1884). William Ascroft's pastels of the atmospheric aftereffects of Krakatoa were widely known, not least through their inclusion as the frontispiece to the monumental book *Eruption of Krakatoa,* Symons (1888). This pastel shows the reddish brown corona around the sun known as the Bishop's Ring. Ascroft recorded this scene on 2 September 1884, sunset 7:35 P.M. Source: Ascroft Sketch, Science Museum Library, South Kensington.

Fogs, and Clouds," which demonstrated the role of dust in nucleating fog and cloud droplets;[65] his observations on the extraordinary sunsets followed as a natural sequel. To avoid Britain's cloud cover, Aitken voyaged to the south of France, where he repeatedly witnessed the white glare of the daytime sun, the yellow-orange-red sequence of colors on the western horizon at sunset, and then the brilliant afterglows that emerged some 15 and then 30 minutes after sunset.[66] Even a decade later Aitken was still struggling to understand those cataclysmic events by reproducing the Krakatoa green sunsets in his laboratory using electrified steam: "The colours produced by such simple materials as a little dust and a little vapour are as beautiful as anything seen in nature, and well repay the trouble of reproducing them."[67]

Victorian England thus was fascinated with all kinds of reproductions of the dramatic in nature: through painting, poetry, photography, and even laboratory recreation. In addition, there were immediate, practical issues at stake. Weather affected transportation, fishing, public health, military affairs, agriculture, and communication.[68] Aitken, among others, frequently stressed this practical side of

65. Aitken, "Dust, Fogs, Clouds" ([1880–81] 1923), 34–64.
66. Aitken, "Remarkable Sunsets" ([1883–84] 1923), 123–24.
67. Aitken, "Cloudy Condensation" ([1892] 1923), 283.
68. E.g., an international conference on the study of weather at sea was suggested by Lieut. M. F. Maury of the U.S. Navy in 1853 and the (British) Board of Trade established a Meteorological Department in 1854; see Shaw, "Meteorology," *Nature* 128 (1931): 925–26. The Scottish Meteorological Society had close relations with fisheries organizations, which substantially subsidized the society's research; see, e.g., Scottish Meteorological Society, "Report," *J. Scot. Met. Soc.* 7 (1884): 56–60, on 57. Also, as Sir Ernest Wedderburn mentions, in "Scot-

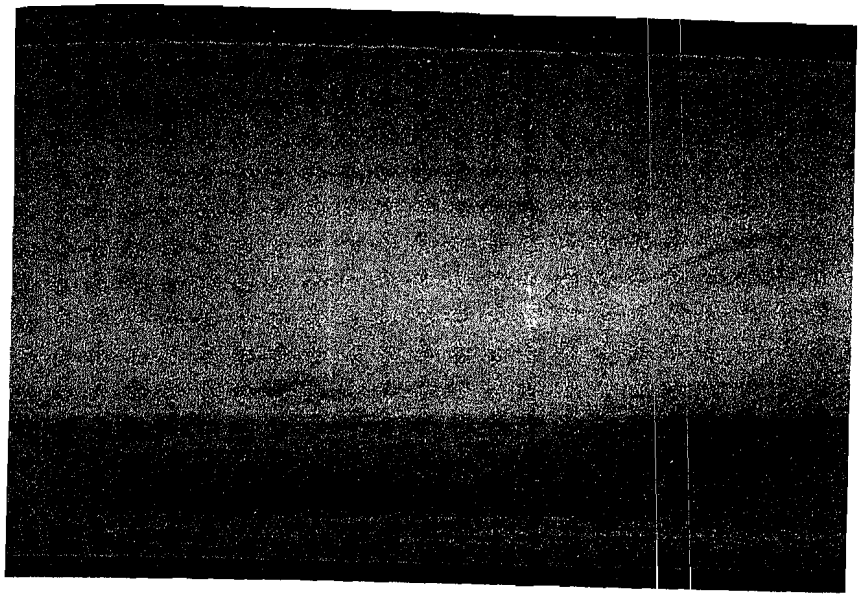

Figure 2.6 Ascroft pastel (1885). Ascroft's sketch of 3 September 1885 illustrates the "Amber Afterglow with Crepuscular Rays," 7:10 P.M.; sunset 6:42. Source: Ascroft Sketch, Science Museum Library, South Kensington (53A5).

meteorology. For example, he knew that the dust-filled industrial output of England produced fogs that could endanger its citizenry: "All our present forms of combustion not only increase the number and density of our town fogs, but add to them evils unknown in the fogs which veil our hills and overhang our rivers."[69] Such evils, Aitken asserted, force our attention to the importance of dust in the origin of clouds: "As our knowledge of these unseen particles increases, our interest deepens, and I might almost say gives place to anxiety, when we realize the vast importance these dust particles have on life, whether it be those inorganic ones so small as to be beyond the powers of the microscope, or those larger organic ones which . . . though invisible, are yet the messengers of sickness and of death to many—messengers far more real and certain than poet or painter has ever conceived."[70]

tish," *Q. J. Roy. Met. Soc.* 74 (1948): 233–42, on 235: "Amongst [the society's] subsidiary objects were . . . the bearing of meteorology on public health, agricultural [*sic*] and horticulture, the alleged periodical recurrence of wind storms, and the general laws regulating atmospheric changes, the discovery of which might lead to a knowledge of the coming weather."
69. Aitken, "Dust, Fogs, Clouds" ([1880–81] 1923), 49–50.
70. Aitken, "Small Clear Spaces" ([1883–84] 1923), 84.

Industrial fogs, Victorian exoticism, and the pragmatic demands of transport combined to bolster a worldwide establishment of weather networks, observatories, and professional meteorological societies.[71] Significantly, Wilson's path would lead him to an observatory located on the peak of Ben Nevis. Ben Nevis, in Northern Scotland, a few miles south of the Caledonian Canal, is the highest mountain in the British Isles. Impressed by the new style of meteorological research pioneered in Germany and the United States, many Britons had enthusiastically supported the establishment of an observatory. With the help of devoted amateurs and private subscriptions, the Meteorological Council publicized their interest in founding an observatory to assist in tracking "vertical meteorological sections of the atmosphere."[72] One Clement L. Wragge volunteered to take weather readings from Ben Nevis by himself to show the value of careful observation. A "king among eccentrics," Wragge assigned Christian names to cyclones, and even edited his own journal, *Wragge—For God, King, Empire and People.*[73] On the heels of the wide publicity given to Mr. Wragge, contributions were made by such diverse benefactors as Her Majesty and the Worshipful Company of Fishmongers, London. These funds allowed the Ben Nevis Observatory to open in 1883. Until it was closed in 1904, resident observers sent daily weather information by telegraph to England. For years Wilson's research was shaped by his experiences at this distant meteorological outpost.

2.3 Mountaintop Glory, Laboratory Ion

Wilson's career as a professional physicist began haltingly.[74] After graduating from Cambridge in 1892, he stayed on, demonstrating at both the Cavendish and Caius Chemical Laboratories. Hoping to obtain a Clerk Maxwell fellowship (he did not succeed), Wilson wrote to J. J. Thomson in November 1893 about his work on the distribution of a substance in solution that was kept hot on the top and cold on the bottom: "Very few experiments appear to have been made on the subject, and it seems of considerable importance in connection with theories of solution and osmotic pressure." The aspiring physicist "would determine the concentration of different parts of the solution optically."[75] In his later work Wilson used theoretical calculations of vapor pressures to try to understand the condensation of water vapor, and he pursued the problem experimentally with an optical method (the cloud chamber). His earlier work with nonequilibrium systems

71. For an excellent bibliography of meteorology, and for further general sources and, in particular, studies of individual national weather systems, see Brush and Landsberg, *Geophysics* (1985).

72. Report of the Council of the Scottish Meteorological Society for 1875, quoted in Paton, "Ben Nevis," *Weather* 9 (1954): 291–308, on 292.

73. Paton, "Ben Nevis," *Weather* 9 (1954): 291–308, on 294.

74. See details in Wilson, "Reminiscences," *Notes Rec. Roy. Soc. London* 14 (1960): 163–73, on 166.

75. Wilson to Thomson, 8 November 1893, CWP.

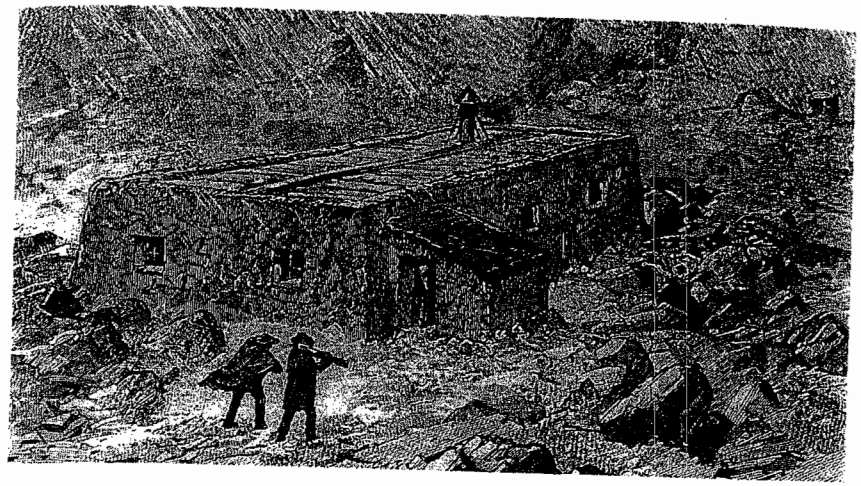

Figure 2.7 Exterior, Ben Nevis Observatory (1885). Wilson worked at Ben Nevis in autumn 1894 and summer 1895; the electrical and condensation phenomena he witnessed became guiding themes for the entirety of his scientific career. Both became natural wonders that he sought again and again to mimic within the confines of the laboratory. Source: *Graphic* 36 (1885): 638, University of Cambridge Library.

contributed to his interest in thermodynamic instability—the fundamental feature of cloud condensation. But the immediate catalyst for Wilson's work came from his stint in the fall of 1894 at the Ben Nevis Observatory.

By the early 1890s, the mountaintop observatory was flourishing and its staff welcomed volunteers to work on Ben Nevis during the easy observing periods of summer and fall (see figures 2.7 and 2.8). After graduating from Cambridge, Wilson's affection for the mountains led him to the small station many times. He made his first trip on 8 September 1894, which began as a cloudless day. Soon a thick haze embraced patches of fog that gradually became continuous. The next evening, at 9 P.M., the observers sighted first one lunar corona, then another at 10 P.M., and at least seven more during the next two weeks. On the fifteenth the logbook records: "Solar fogbow & glories at 16^{H} & Lunar Corona at 23^{H}." Just hours before Wilson descended from the heights on 22 September, light and clouds performed spectacularly—even the dry tone of the logbook rises, in the remark that "some beautiful Triple Lunar Coronae were seen this morning through thin passing fog."[76]

On descending from the station, Wilson, in tune with Aitken and so many contemporaries, wanted to mimic the wonders of nature: "In September 1894 I

76. Ben Nevis Observatory logbook, 15 and 22 September 1894, Meteorological Office, Edinburgh. I would like to thank Marjory Roy for making this material available to me.

Figure 2.8 Interior, Ben Nevis Observatory (1885). Source: *Graphic* 36 (1885): 638, University of Cambridge Library.

spent a few weeks in the Observatory . . . of Ben Nevis. . . . The wonderful optical phenomena shown when the sun shone on the clouds surrounding the hill-top, and especially the coloured rings surrounding the sun (coronas) or surrounding the shadow cast by the hill-top or observer on mist or clouds (glories), greatly excited my interest and made me wish to imitate them in the laboratory."[77]

Returning to the mountain peak just nine months later (June 1895), Wilson recorded that he "[w]alked along Spean Bridge road. Saw lightning lighting up mist in big corrie & heard thunder rolling in that direction." The power of the electrical storm was evident: "Saw the damage done by lightning yesterday. Telegraph instrument fused at various places, including part of one of the steel keys. . . . Boxes on shelf above [lightning] arrester thrown on to floor." After recording the events of a few days mostly spent hiking in the hot, blazing sun, Wilson noted on 26 June an abrupt alteration of weather. "Mist suddenly began to pour down a gully between B[en] Nevis & Carn Dearg, & afterwards hid upper part of cliff. . . . Heard continual muttering of thunder in distance. Walked along ridge to top of Carn Mor Dearg. After a minute or two there, suddenly felt St. Elmo in my hair, & in my hand on holding it up. Ran down into corrie. Bright lightning & loud thunder."[78]

77. Wilson, "Making Visible Ions," (1965), 194.
78. CWnb, A21, 19, 20, and 26 June 1895.

The optical and electrical phenomena that Wilson witnessed during those two trips to Ben Nevis set the outlines of his lifelong scientific goals. Meteorological optics and atmospheric electricity remained central to Wilson's work until his death in 1959.

Between demonstrating physics and tutoring students, young Wilson had earned a living in Cambridge but been unable to find the time to do any research. To improve the situation, he had tried teaching for a few months at the Bradford Grammar School, but this left him no freer than before. Returning to Cambridge, he considered himself lucky to land a job demonstrating physics to medical students at the Cavendish. "With this I had just enough to live on, a connexion with the Cavendish, and at last time to do some work of my own just when I had ideas which I was impatiently waiting to test."[79]

The few months he spent teaching young grammar school students were not directly productive, but his dramatic encounter with meteorological effects at Ben Nevis left him with a burning desire "to reproduce the beautiful optical phenomena of the coronas and glories I had seen on the mountaintop."[80] With this goal in mind, Wilson began a notebook in which he recorded speculations as well as research notes. Immediately preceding the notes on his first "cloud" experiment, we find a page of questions he has addressed to himself. Under a section labeled "Cloud Formations & c[etera]" he pondered:

> 1 Are the rings of corona and glory formed simultaneously in same cloud, equal in radius? Try monochromatic light.
> (2) Conditions of formation. When best formed. (In dusty or tolerably pure air or in presence of soluble substances.[)]
> (3) Are ice particles ever formed instead of water drops. (Halos & c.)
> 4. Are coronae & c. formed when one liquid separates out in milky form from another. (Mixture of ether and water allowed to cool & c.) Also are halos ever formed when crystalline precipitates are formed. Applications.[81]

On what tradition did Wilson draw when he decided to perform these experiments to reproduce clouds, glories, and coronae in his laboratory? Other scientists were working on similar problems; several had even tried to reproduce clouds and fogs. Early in his notebook Wilson reviewed the quite extensive literature on cloud formation. Aitken, Jean Paul Coulier, and Robert von Helmholtz (the son of Hermann) had all performed experiments to see if they could get water vapor to condense—either by using an air pump or an india rubber ball to expand saturated air in a glass vessel, or by watching a steam jet abruptly expand as it escaped from a nozzle.[82] The theoretical justification for this

79. Wilson, "Reminiscences," *Notes Rec. Roy. Soc. London* 14 (1960): 163–73, on 166.
80. Wilson, "Reminiscences," *Notes Rec. Roy. Soc. London* 14 (1960): 163–73, on 166.
81. CWnb A1, preceding entries dated March 1895.
82. Aitken, "Dust, Fogs, Clouds" ([1880–81] 1923); Coulier, "Nouvelle propriété," *J. Pharm. Chim.*, 4th ser., 22 (1875): 165–73, 254–55; and Helmholtz, "Dämpfe und Nebel," *Ann. Phys. Chem.* 27 (1886): 508–43.

method is as follows: an adiabatic expansion of saturated gas lowers the temperature[83] of the gas, causing supersaturation that can lead to condensation. In the term "supersaturated air," supersaturation is usually defined to be the state in which the ratio of actual vapor pressure to the equilibrium vapor pressure above a *flat body of water* is greater than one. The obvious way to study condensation is with supersaturated vapors, but it is not always easy to precipitate the liquid. Under certain circumstances it is possible to have supersaturation without condensation; this occurs, for example, if the vapor is not over a flat body of water.

Among Wilson's predecessors who used some form of expansion of air to investigate condensation Aitken was the most important for Wilson. Also a Scotsman, Aitken initiated research work at Ben Nevis that involved equipment that Wilson would take as a model for his own experiments. The remarkable similarity between Aitken's and Wilson's instruments, and Wilson's reference only to Aitken in his first published paper, suggests looking more closely at the material culture of Aitken's meteorology.[84]

Aitken's first conclusion from his cloud experiments was that dust particles acted as nuclei for water droplets in supersaturated air. Without dust there was no condensation under what Aitken considered "normal" conditions. In 1880 he pointed out that "[d]usty air—that is, ordinary air, gives a dense white cloud of condensed vapour."[85] He decided to use his dust chamber in a purposeful way, by designing an instrument to measure the number of particles in the air. "Powerful as the sun's rays are as a dust revealer, I feel confident we have in the fog-producing power of the air a test far simpler, more powerful and delicate, than the most brilliant beam at our disposal."[86] Aitken's motivations for studying dust were nominally meteorological; he believed in the "possibility of there being some relation between dust and certain questions of climate, rainfall, etc."[87] But dust was clearly much more than a nucleation site for rain. As Aitken repeatedly stressed, he hoped to settle the "great fog question," the problem of town fogs, whose "increased frequency and density . . . [is] becoming so great as to call for immediate action."[88] Dust and fog were both signs and symptoms of power, both industrial and natural, and for the Victorians these motes carried a cultural meaning far beyond their physical content.

By 1888 Aitken had a method of counting dust particles in the air based on his observation that they were a source of condensation in supersaturated air.

83. In practice, the expansion was quick enough to give a good approximation to an adiabatic system (no heat exchange with the outside). Therefore, the gas obeys the equation pV^{γ} = constant, where γ is the ratio of specific heats c_p/c_V. Using the ideal gas equation we obtain a fall in temperature as the air expands, $T_1/T_2 = (V_2/V_1)^{\gamma-1}$.

84. Wilson, "Formation of Cloud," *Proc. Camb. Phil. Soc.* 8 (1895): 306.

85. Aitken, "Dust, Fogs, Clouds" ([1880–81] 1923), 35.

86. Aitken, "Dust, Fogs, Clouds" ([1880–81] 1923), 41.

87. Aitken, "Dust, Fogs, Clouds" ([1880–81] 1923), 41.

88. Aitken, "Dust, Fogs, Clouds" ([1880–81] 1923), 48.

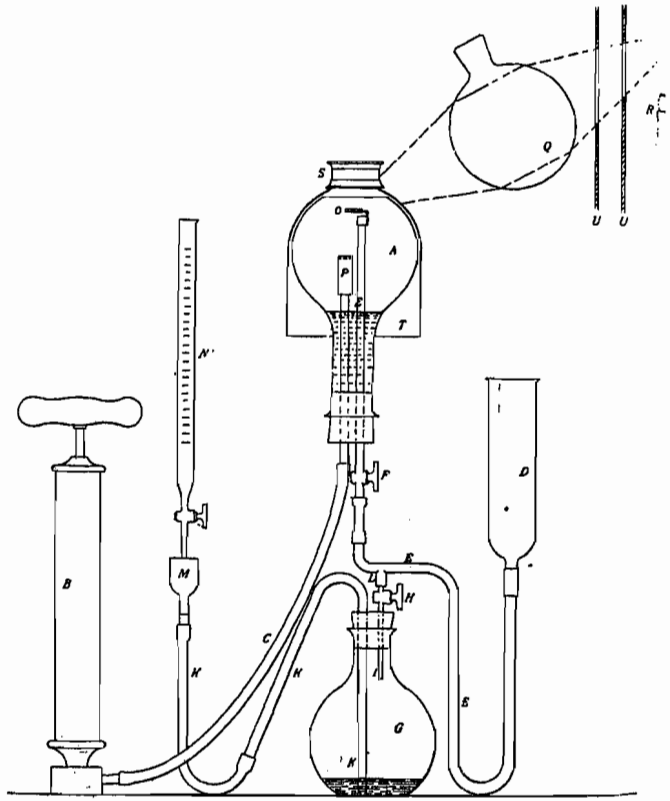

Figure 2.9 Aitken's dust chamber (1888). Aitken's dust chambers were simultaneously a means for the measurement of dust in a given volume of air and a means of imitating the effects of dust on condensation. In both respects, the dust chamber became a model for the cloud chamber—and, in a sense, its antithesis once Wilson began arguing for an electrical rather than a particulate source of condensation. Source: Aitken, "Dust Particles" ([1888] 1923), 190.

His instrument is reproduced in figure 2.9. The condensation occurs in the ordinary glass flask *A*, the receiver, and the water droplets (each surrounding a single dust particle) are counted by means of the compound magnifying glass *S*. The glass flask *G* contains the air to be tested, which is kept saturated by water in the flask. *D* is a cotton wool filter that clears ordinary air of dust particles. Air from *G* and *D* is mixed so that "too much dusty air [is not] sent into the test receiver at one time, or the drops will be too close for counting."[89] Aitken wanted to ensure that the dust particles would be far enough apart that each particle would

89. Aitken, "Dust Particles" ([1888] 1923), 193.

serve as a center of condensation and all would be counted. After this mixture of air is in receiver *A* and stopcock *F* has been closed, the experimenter makes one stroke of the pump (*B*), while watching stage *O* very carefully. As the air in *A* expands, it supersaturates; condensation occurs on the dust particles that then fall onto *O*. Counting is facilitated by a square grid (on *O*) where the rulings are 1 millimeter apart.

Aitken spent five years (1889–94) counting dust particles in British and Continental air with a pocket version of his counter. In a three-part paper published during this period, "On the Number of Dust Particles in the Atmosphere of Certain Places in Great Britain and on the Continent, with Remarks on the Relation Between the Amount of Dust and Meteorological Phenomena," Aitken compared measurements taken on Ben Nevis with those from a low-altitude station at Kingairloch.[90] Mr. Rankin, the observer who took the readings on Ben Nevis, wrote that two dust counters were bought for the observatory in 1890, "one, a portable form of the instrument mounted on a tripod stand, for use in open air; the other, a much larger form, for use in the laboratory. . . . Both instruments were made from plans and specifications prepared by Mr. Aitken."[91] The last measurement recorded on Ben Nevis was in 1893, just a year before Wilson arrived for his first visit of September 1894. Wilson certainly saw the Aitken apparatus on his visit; the observatory was small and crowded and there was not much to do on the mountain after the sun went down.

After his short stay in Scotland, Wilson began to study the optical effects of clouds. By condensing water vapor in an expansion apparatus similar to Aitken's (see figure 2.10), Wilson was able to produce the rings of color that had delighted him on Ben Nevis. Though Wilson obviously derived his method from Aitken's work, their procedures differed crucially. Instead of mixing purified and dusty air, Wilson filtered *all* the air that entered the receiver. Figure 2.10, reproduced from the first page of Wilson's notes on condensation experiments, displays an apparatus that tests only filtered air; there is no valve system, such as Aitken's, for mixing pure and dusty air. Air enters receiver *V* through the cotton wool filter *F* and is kept saturated by water in *V*. A pump (*P*) evacuates *R*, which then is allowed to come into contact with receiver *V* when expansion is desired.[92]

There is a profound puzzle lodged in that cotton wool filter. If Wilson wanted to make fogs, why was he removing the dust that Aitken had so painstakingly demonstrated to be the condensation nuclei? Surely, if Wilson's only motivation was to reproduce the natural phenomena of clouds he would have used ordinary air, not air that was specially prepared for laboratory purposes. This

90. Aitken, "Dust Particles in Certain Places" ([Part I, 1889–90; Part II, 1892; Part III, 1894] 1923).

91. Rankin, "Dust Particles," *J. Scot. Met. Soc.* 9 (1891): 125–32, on 125. The dust counter remained at the observatory until at least 1901. In a deposition of the property at the observatory the dust counter is specifically mentioned; see McLaren et al., "Memorandum," *J. Scot. Met. Soc.* 12 (1903): 161–63, on 162.

92. CWnb A1, 26 March 1895.

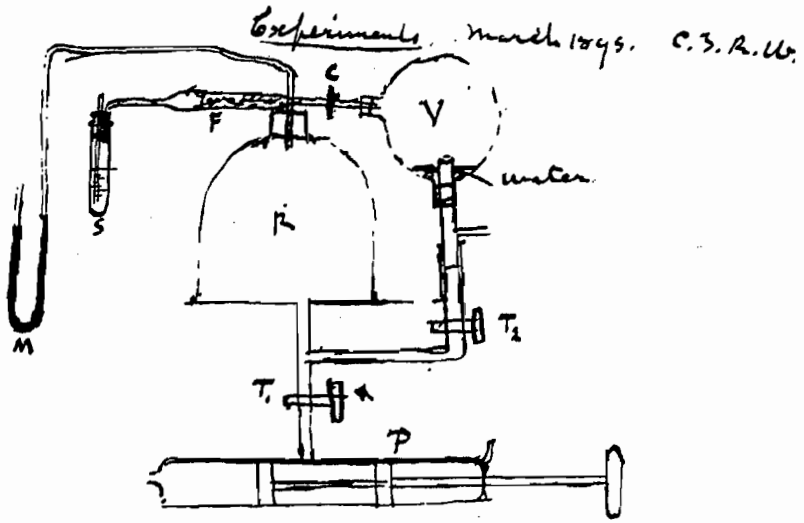

Figure 2.10 Wilson's cloud chamber apparatus (1895). Source: CWnb A1, March 1895.

"artificial" aspect of his experiments signals a significant departure from the mimetic tradition in which his work was initially embedded.

Was there any reason to suppose such filtered air could produce droplets? Several different observers had suggested that electrified air—even without dust—might be capable of nucleating rain. First, it was generally known that as steam hisses out of a nozzle it expands, supersaturates, and condenses. Though they could not explain *why*,[93] R. von Helmholtz, Aitken, Carl Barus, and Kiessling had all found they could increase that condensation by electrifying the jet.[94] Second, Aitken and others had noted (again without knowing why) that a high expansion would produce condensation even in supposedly purified air.[95] After

93. Aitken reasoned that electricity would prevent smaller drops from coalescing into larger drops because of electrostatic repulsion and that therefore the number of the drops would remain large, promoting dense condensation. But, as in the other 600 pages of Aitken's collected papers, he offers no quantitative analysis or formal derivation. Aitken, "Cloudy Condensation" ([1892] 1923), 258–59.

94. In his notes, Wilson referred to Helmholtz, "Dämpfe und Nebel," *Ann. Phys. Chem.* 27 (1886): 508–43 (CWnb A1, preceding entries dated March 1895), and in a later paper, to Barus, Kiessling, and Aitken (Wilson, "Condensation," *Phil. Trans. Roy. Soc. A* 189 [1897]: 265–307).

95. Aitken, "Dust Particles" ([1888] 1923): "We see from this experiment that an expansion of 1/50 is nearly, perhaps quite sufficient to cause condensation to take place on even the smallest particles in the air tested; from which we may conclude that the showers which unexpectedly took place from time to time in the experiments described, where high expansions were used, were not due to the presence of extremely small particles which had become active with the high degree of supersaturation" (201). Aitken got rid of this problem by pumping slowly. He believed that he was eliminating the "shock" of expansion which caused unwanted condensation (202–3). Actually, because his expansion was slow and therefore did not approximate an adiabatic expansion as

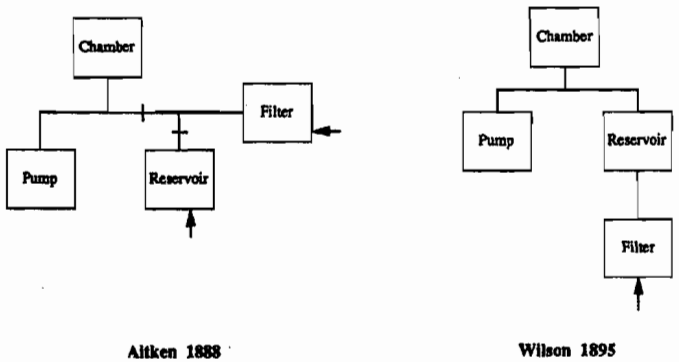

Figure 2.11 Schematic comparison, dust chamber and cloud chamber. Representations of the Aitken and Wilson apparatuses depicted in figures 2.9 and 2.10. In the Aitken apparatus (*left*), air from the outside enters through the reservoir and is mixed with a certain amount of filtered air coming through the filter. The filtered air serves merely to keep the air in the chamber from becoming too dusty. By contrast, in Wilson's apparatus *all* the air must pass through the filter before it can enter the reservoir, the main chamber, or the pump system. Thus, Wilson had to find it plausible that water vapor might condense on dust-free air, a situation only imaginable with the enabling notion of the ion.

satisfying himself that he could eliminate this bothersome effect in his dust counter, Aitken dropped the matter. Wilson was aware of both phenomena,[96] though it would be years before he would be certain of the connection between the two.

Thus, suspecting that there might be several ways for droplets to form, Wilson filtered the air to identify the best conditions in which to make the "wonderful" optical effects, coronae and glories: "When [are the optical effects] best formed[?] (In dusty or tolerably pure air . . . [?])"[97] Soon, the notebooks reveal, he began experimenting *exclusively* with filtered air. For Wilson, dust became just an annoyance to be removed; in contrast, dusty air was Aitken's prize specimen, and condensation in purified air was a background problem to be eliminated and then forgotten. Wilson created artificial laboratory conditions in order to "dissect" nature; Aitken wanted to remove the artificiality from his experiment to imitate nature as it was. It is at first puzzling that Wilson used an apparatus almost identical to Aitken's, yet the two fastened on mutually exclusive phenomena—one on dust, the other on purified air. Wilson's apparently simple move of the filter F (see the schematic figure 2.11) hides a profound shift both in

well as a fast expansion did, the supersaturation was not as large as with the quicker motion, and the unexpected condensation did not take place.

96. CWnb A1, preceding entries dated March 1895.

97. CWnb A1, preceding entries dated March 1895.

material culture and in conceptual structure. The source of this change in traditions was the scientific program of the Cavendish Laboratory.

2.4 Analysis and Mimesis

Through J. J. Thomson and his collaborators, Wilson learned the Cavendish style of analytic physics, a mode of work and tradition of instrumentation altogether different from the mimetic mode to which Wilson was so committed. Thomson, appointed to the Cavendish chair in 1884, was a firm believer in analytic solutions to problems, using explanatory entities far from the visible manifestations of nature: "[T]he principal advances made in the Physical Sciences during the last fifty years . . . [have] intensif[ied] the belief that all physical phenomena can be explained by dynamical principles and [stimulated] the search for such explanations."[98] As a student at Cambridge from 1888 to 1892, and as a researcher at the laboratory starting in 1895, Wilson absorbed much of the matter physicist's style of physics. In addition, the Cavendish was unique in its firm programmatic commitment to the assumption that electric charge came in discrete bits (ions). Wilson borrowed heavily from his Cambridge colleagues; ion physics (or, more specifically, the idea of ions in general) had been essential to his invention of the cloud chamber. In return, Wilson's work became an integral part of the Cavendish investigations into the electrification of gases. Eventually, under Rutherford, his instrument became the laboratory's primary research tool. The cloud chamber, developed under one research program, would itself (in its role as a piece of experimental equipment) establish the boundaries of another, completely different research project. Instruments will not stay put.

Evidence of the effect of the Cavendish research program on its students is easy to find. As a Natural Sciences Tripos student (Wilson took the new laboratory course rather than the more formal Mathematical Tripos), Wilson faced questions such as "Give some account of the phenomena observed in the neighbourhood of the negative electrode when an electric discharge passes through a tube containing gas at low pressure."[99] Interest in this type of discharge was central to the basic thrust of Cavendish research, which sought to understand the structure of matter through the investigation of cathode rays. One of the program's great triumphs followed not many years later when Thomson, in 1897, began arguing that the electron was a subatomic particle.[100]

Since the middle of the nineteenth century when it became possible to produce a moderate vacuum, physicists across Europe had been exploring the

98. Thomson, *Applications of Dynamics* (1888), 1.

99. David B. Wilson. "Experimentalists," *Hist. Stud. Phys. Sci.* 12 (1982): 325–71, on 346–47.

100. Much of the following summary of early atomic research is taken from Heilbron, "Atomic Structure" (1964), especially the first two chapters. Heilbron summarizes: "[T]he 'great discoveries' [of X rays, radioactivity, and the corpuscular electron] developed mainly out of experimental investigations of the phenomena accompanying the discharge of electricity through rarified gases" (59).

effects of electrical discharges. German physicists led the effort to probe the invisible rays emitted from a hot cathode contained in a tube with rarified gas. At first, attention focused on light and dark bands in the tube; as higher and higher vacuums were achieved, scientists saw the glowing in the tube dwindle until, at very low gas pressure, light only appeared on the walls of the tube. Since the rays seemed to originate from the cathode (rather than the anode), they were named cathode rays.[101] In 1879, William Crookes demonstrated that magnets deflected the rays, which led him to identify the rays as streams of charged particles.[102] Crookes thus added support to a view, held intermittently since Faraday's work in the 1830s, that electricity comes in discrete "atoms." Opposing the particulate view were scientists who believed that all electromagnetic effects, including charge itself, were deformations of the ether.[103] By the 1880s English, though not Continental scientists, had decided in favor of the corpuscular nature of electricity. One of the chief proponents of the ion picture was J. J. Thomson at the Cavendish.

Thomson began his study of the discharge of electricity through gases in 1886,[104] and soon most of the laboratory was devoted to this problem. From 1893 to 1895, the year Wilson arrived at the Cavendish, fully half of the papers published by Cavendish scientists were concerned with the discharge of electricity.[105] As a discharge effect possibly explicable in terms of ions, Aitken's electrified steam experiments would have seemed relevant to Thomson. And in 1893, two years before Wilson published his paper on condensation in the absence of dust,[106] Thomson provided a theoretical justification for the growth of a drop in the presence of a nonuniform electric field.[107]

In particular, Thomson first argued that because surface tension puts a drop under pressure, the equilibrium pressure surrounding the drop is very high for small drops. Quantitatively, the presence of a drop of radius r increases the equilibrium vapor pressure by a factor of $1/r$, promoting evaporation. The smaller the drop, the greater this effect. As Thomson put it, in terms readily understandable to his class and culture, "It is evident that this property makes the growth of drops from smaller ones of microscopic dimensions impossible, for these small

101. Wheaton, *Tiger* (1983), 5.

102. Wheaton, *Tiger* (1983), 6.

103. The best source for a discussion of Maxwellian charge concepts and the related electrodynamics is Buchwald, *Maxwell to Microphysics* (1985), e.g., 23, 38ff. Locating these developments within other contexts are two works: Bruce Hunt, *Maxwellians* (1991); and Harman, *Energy* (1982), 89–103.

104. Thomson's first paper explicitly concerned with the discharge of electricity in gases was "Electric Discharge," *Proc. Camb. Phil. Soc.* 5 (1886): 391–409.

105. *History of the Cavendish* (1910), 297–98. I have taken the number of papers published on electrical discharge phenomena from the list (given by year) of all Cavendish papers provided at the end of the volume.

106. Wilson, "Formation of Cloud," *Proc. Camb. Phil. Soc.* 8 (1895): 306.

107. Thomson, "Steam-Jet," *Phil. Mag.*, 5th ser., 36 (1893): 313–27. Much of the theoretical work in this paper was taken from Thomson's earlier book *Applications of Dynamics* (1888), chap. 11, "Evaporation," 158–78.

drops would evaporate and get smaller, and the smaller they get the faster will they evaporate. They are in the position of a man whose expenditure increases as his capital decreases, a state of things which will not last long."[108]

Thomson next argued on thermodynamic grounds that a nonuniform electric field *decreases* the vapor pressure as $1/r^2$. Energy considerations provide a simple way to understand this: Because the dielectric constant of water is about 80 and that of air about one, the energy in the electric field decreases when the charge is surrounded by water. Assuming the air acts as a thermal reservoir, condensation of water is favored since the Gibbs free energy of the system tends to a minimum. Because surface tension increases equilibrium vapor pressure by $1/r$ and the presence of charge decreases it by $1/r^2$, for small drops the electric field wins and the drop can grow.

Thomson's research program, by providing a quantitative model of the physics of ionic condensation in dust-free air, allowed Wilson to consider experiments that would have seemed pointless without Thomson's detailed scheme of ion drop formation. A week after Wilson began his cloud experiments, he explicitly used Thomson's formula to calculate the magnitude of the electric charge he suspected he might be seeing in his chamber. "If nuclei be present in [the] shape of small electrified drops of radius 2 times 10^{-7} [centimeters] each charged with atomic charge, we can calculate magnitude of this charge necessary to neutralize effect of S. T. [surface tension]."[109] As Wilson turns his thoughts to the world of subvisible ions, one sees the impact of Cavendish research on his programmatic goals.

During the remainder of 1895 Wilson used his cloud chamber daily. Each morning he would fill his chamber with air and make several expansions to remove dust, a method he found more satisfactory than using a cotton wool filter. Then for the rest of the day he used this purified air to determine expansion ratios for condensation. (The expansion ratio was defined to be V_2/V_1, where V_2 was the volume after expansion and V_1 the volume before.) On 3 April alone, he made 115 runs.[110] In a one-page paper, the only article he published in 1895, Wilson stated the critical expansion ratio given an initial temperature of 16.7°C: $V_2/V_1 = 1.258$.[111] Such precision would become important to Wilson later while investigating the new "rays" that were soon to be discovered on the Continent.

As he developed the cloud chamber, Wilson never left meteorology, constantly interjecting weather questions into his notebook. During spring 1895 he speculated on the significance of his experiments in a section labeled

108. Thomson, *Recollections* (1937), 416.

109. CWnb A1, 22 April 1895. Wilson is directly citing Thomson's article "Steam-Jet," *Phil. Mag.*, 5th ser., 36 (1893): 313–27.

110. CWnb A1, 3 April 1895.

111. Wilson, "Formation of Cloud," *Proc. Camb. Phil. Soc.* 8 (1895): 306.

"Meteorological." Beginning with optics, Wilson claimed that the "[e]xistence of coronae shows uniformity in size of drops in the clouds showing them." The drops, Wilson speculated, might oscillate in a cloud: as drafts carry them up into regions of high supersaturation they grow bigger until their weight causes them to descend, thus reaching an area of lower saturation where they begin to evaporate until they are light enough to begin the cycle again.[112] In addition, Wilson began to think that weather phenomena might be affected by ions and wondered, "*When drops are suddenly formed do they throw off small electrified drops or free ions?* (Thunderstorms & c.) In either case air should be left electrified."[113]

During the New Year's celebration of 1896 the world was thrilled by news of Röntgen's discovery of X rays. The unique photographic properties of the rays created great excitement among both scientists and nonscientists; Röntgen was even called to Potsdam to give Kaiser Wilhelm a demonstration.[114] At the Cavendish, experiments with X rays began at once. Ernest Rutherford, a new research student at the laboratory, wrote on 25 January 1896, "The Professor [Thomson] of course is trying to find out the real cause and nature of the waves, and the great object is to find the theory of the matter before anyone else, for nearly every Professor in Europe is now on the warpath."[115] Thomson used the new rays in conjunction with a well-known experiment at the Cavendish. He and Rutherford watched the effect that Röntgen rays had on the passage of electricity through gas.[116] Soon they discovered that the rays enhanced conduction and explained this by suggesting that Röntgen rays produced ions in the gas as they passed through it.

When Wilson heard about Röntgen's vision, he was anxious to shine the new rays into his cloud chamber. He borrowed an X-ray tube from Ebeneezer Everett,[117] Thomson's assistant, and upon turning it on his chamber was delighted to find that "no effect is produced by the X-rays unless the expansion is great enough to produce condensation in any case. When it is sufficient to cause condensation without the rays, they produce a very great increase in the number of the drops."[118] Wilson had carefully determined the expansion ratio that caused condensation around nuclei produced by Röntgen rays and found that exactly the same expansion ratio condensed vapor about nuclei naturally present in dust-free air. Therefore, Wilson reasoned, "[i]t seems legitimate to conclude that when the Röntgen rays pass through moist air they produce a supply of nuclei of the same kind as those which are always present in small numbers, or at any rate of

112. CWnb A1, 27 March 1895.
113. CWnb A1, 30 March 1895; Wilson's emphasis.
114. Keller, *Atomic Physics* (1983), 57.
115. Rutherford to Mary Newton (fiancée), 25 January 1896, quoted in Keller, *Atomic Physics* (1983), 57.
116. Thomson and Rutherford, "Passage of Electricity," *Phil. Mag.*, 5th ser., 42 (1896): 392–407.
117. "Wilson of the Cloud Chamber," transcription of radio interview with Wilson on Scottish Home Service, 16 February 1959, 8, item D57, EAP.
118. CWnb A2, 17 February 1896.

exactly equal efficiency in promoting condensation."[119] Quantitative experiment was the tool that allowed Wilson to identify the kinds of nuclei as one and the same because precisely computed expansion ratios were needed to make this comparison. Such a move would have been inconceivable in the qualitative style that marked the work of John Aitken.

Becquerel's discovery of uranium rays in March 1896 added yet another dimension to Wilson's experiments. Wilson found that, like X rays, uranium rays increased condensation at the established expansion ratio of $V_2/V_1 = 1.25$. By the second half of the year 1897, Wilson was willing to go to press with the assertion that "[t]he electrical properties of gases under the action of Röntgen rays and Uranium rays point to the presence of free ions."[120]

Wilson's patience and precision in determining expansion ratios led to a modification in the ion theory of condensation. By 1898 he could show that precipitation in the cloud chamber fell into four regimes bounded by three expansion ratios. Below $V_2/V_1 = 1.25$ there is no condensation in dust-free air, between 1.25 and 1.31 there are distinct "rain" drops, at 1.31 there is a sudden increase in the number of drops, and finally, above 1.37 a dense fog is seen. Wilson supposed that the fog was formed by the statistical aggregation of water molecules because of extremely high supersaturation; no nuclei were needed. He did not, however, understand the reason for the existence of the *two* lower expansion ratios. In March 1898 he speculated, in his notebook, that the difference in condensation in the two domains could be explained by the quantity of charge acting as precipitant, for a larger charge would promote condensation at a lower supersaturation. He thought that two sets of ions might exist, one with a charge twice that of the other: the double charge was strong enough to pull in water at 1.25, while the single charge would only precipitate water at the ratio 1.31. At this stage Wilson drew his ion models from chemistry. His doubly charged carrier (ion) was an oxygen atom. He did not suggest that particles smaller than atoms carried charge.[121]

By 7 July 1898 Wilson was considering another reason for the two different expansion ratios. His notebook entry of that day began with Thomson's idea: "J. J. T. suggests that if the expansion required to catch positive and negative ions is different (say less for the negative than for the positive) gas would be left charged if expansions were only sufficient to catch the negative but not the positive." Wilson saw immediately that "[t]his would have obvious meteorological application if atmospheric air were ionised to even very small extent."[122] What did Wilson mean by this? It was generally known that the earth is negatively charged and that a potential gradient exists between the earth and the ionosphere.

119. Wilson, "Röntgen's Rays," *Proc. Roy. Soc. London* 59 (1896): 338–39, on 339.
120. Wilson, "Condensation," *Proc. Camb. Phil. Soc.* 9 (1897): 333–38, on 337.
121. CWnb A8, 4 March 1898.
122. CWnb A8, 7 July 1898.

(On an ordinary day this gradient is 100 volts per meter, amounting in total to a change in voltage of 400,000 volts.)[123] If there were no external forces acting on this system, such a huge potential difference would discharge the earth in approximately half an hour. In 1898 no mechanism was known that could maintain that voltage gap.

Wilson was quick to realize that if negative ions were more likely to cause condensation, rain would bring down negative charge to the earth, thus maintaining this fair-weather gradient. Immediately, he began to speculate on meteorological consequences of Thomson's suggestion. Under a section heading, "Condensation problems (suggested during previous work) with *meteorological* bearings," Wilson asked himself: "[1] [A]re there any free ions in ordinary moist air in absence of external influences? . . . [2] Are there any agents . . . capable of ionising [air]? . . . [3] Are negative ions . . . more easily caught than positive ones?" He concluded that the "three questions are all connected with the theory of atmospheric electricity suggested on the previous page. That is that the ground is kept negative by the fall of rain, each raindrop containing one negative ion."[124] Another of Wilson's notebooks for this period offers a detailed discussion of balloon experiments that measure the potential gradient accurately and records ideas for laboratory experiments "to find probable nature of the carriers of atmospheric electricity."[125]

Between 7 and 23 January 1899 Wilson carried out the experiments that confirmed Thomson's hunch about the different expansion ratios for negative and positive ions. Wilson used a modified cloud chamber, one with a brass wire down the center that could be held at a constant potential. His conclusions were inescapable: "This showed difference plainly, and to some extent the distribution of the ions in the tube appeared to be made visible; such fogs as were obtained with EMF [electromagnetic field] arranged to drive positive ions outwards, being concentrated around the wire, while the much more marked fogs obtained with the EMF in the other direction extended throughout the tube. The expansions available were scarcely sufficient to catch positive ions."[126] In notebook entries like this one, we see both the Cavendish transcendental, analytic language (ions) and the natural historical, morphological, mimetic language (fogs). Ions, while invoked in both natural philosophical and natural historical projects, were stripped of the detailed physical and chemical attributes that they had in "pure" physical and chemical work.

By this time Wilson's Cavendish colleagues were also using the cloud chamber. Most notably, J. J. Thomson resolved to establish a value for the charge of the electron, e, and thus add support to his growing conviction that the electron

123. Feynman, Leighton, and Sands, *Feynman Lectures* (1963–65), 2:9-1–9-2.
124. CWnb A8, 7 July 1898.
125. CWnb B1, 4 October 1898.
126. CWnb A3, 7–23 January 1899.

was a fundamental particle. Just a year before, in 1897, he had claimed that cathode rays were streams of elemental particles[127] and had shown that the charge-to-mass ratio, e/m, was a constant for these particles. This was no guarantee, however, that e and m were separately constant, and some workers in the field judged it insufficient proof of the existence of an electron.[128] At the Cavendish, however, there were few doubters, and work to determine accurately the value of e started immediately.

Thomson's first attempt was crude, but it established a method that Robert Millikan later exploited in his famous oil drop experiments.[129] Thomson was able to determine ne, the number of ions times the electron charge, from the measurement of current through gas (a procedure he had been using for a long time); he only had to determine n to get a value for the electron charge. Quoting Thomson: "The method I have employed to determine n is founded on the discovery made by Mr. C. T. R. Wilson."[130] Thomson's method was to take the gas for which ne had been determined and subject it to expansion. Assuming that each water droplet caused by the expansion contained a single ion, n would be equal to the number of droplets. By observing how quickly the cloud caused by the expansion of the gas in a cloud chamber fell under the influence of gravity, he was able to estimate the number of drops. Take the mass of water in the air to be fixed. (The greater the number of drops, the smaller each drop must be.) Stokes's law then predicts that the cloud falls more slowly for smaller drops. These early experiments gave the value of 7.3×10^{-10} electrostatic units for the electron charge.[131]

Wilson adopted Thomson's falling cloud method. In a more complicated version of the experiment to test whether negative charge induces condensation before positive, Wilson placed three vertical brass plates in his chamber, creating two regions.[132] The middle plate was grounded, the left plate was kept at a positive potential, and the right at a negative potential. Since oppositely charged ions were drawn into different parts of the chamber, Wilson could determine whether negative charges differed from positive in their ability to condense water vapor. After making an expansion of 1.25, he "counted" the number of charges in the two sides of the chamber using Thomson's method and found that negative charge did precipitate water but positive charge did not.

Soon H. A. Wilson, also at the Cavendish, joined Thomson in his determination of e. Together, they added electrically charged plates (set horizontally,

127. Thomson, "Cathode Rays," *Phil. Mag.*, 5th ser., 44 (1897): 293–316.
128. Holton, "Subelectrons" (1978). This paper provides a detailed discussion of Millikan's different methods of measuring the electron charge, e. Also on Millikan's oil drop experiments, see Franklin, "Oil Drops," *Hist. Stud. Phys. Sci.* 11 (1981): 185–201.
129. Holton, "Subelectrons" (1978).
130. Thomson, "Charge of Electricity," *Phil. Mag.*, 5th ser., 46 (1898): 528–45, on 528.
131. Thomson, "Charge of Electricity," *Phil. Mag.*, 5th ser., 46 (1898): 528–45, on 542.
132. Wilson, "Comparative Efficiency," *Phil. Trans. Roy. Soc. London A* 193 (1899): 289–308.