Chapter 9
From the Determination of the Ohm to the Discovery of Argon: Lord Rayleigh's Strategies of Experimental Control

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9.1 Introduction

Lord Rayleigh (1842–1919) was undoubtedly one of the most eminent nineteenth-century British physicists. During a career that spanned more than 50 years, he published an astonishing number of papers—446 in total. He studied in Trinity College at Cambridge University and graduated as Senior Wrangler of the Mathematical Tripos in 1865. He was renowned both for his outstanding mathematical skill and his facility with experiments, and theory and experiment went hand in hand in the vast majority of his work.

According to Arthur Schuster, an eminent physicist himself and author of Rayleigh's obituary in the *Obituary Notices* of Fellows of the Royal Society:

> Rayleigh's scientific activity may for convenience be divided into five periods. … The first period extends up to the time when he took up the Cavendish Professorship [in 1879, after Maxwell's death] …. The second period is dominated by his work on electrical standards. The third period … bridges over the interval between his departure from Cambridge and the experiments which led to the discovery of Argon …. The fourth, or "Argon," period was followed by one of great fertility, but no further distinction can be drawn, and it must be taken to extend to the time of death. (Schuster 1921, iii)

Although this periodization does not reflect the breadth of Rayleigh's research interests, it indicates both the significance and the duration of the two projects that are our primary focus here: determining the ohm and discovering argon. These were two of Rayleigh's foremost contributions, and they greatly reinforced his reputation as an exact experimenter.

The quest for rigor was a ubiquitous theme in Rayleigh's physics. In his experimental practice, that pursuit involved the application of control strategies, which

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pervaded his work at various levels. They included the multiple determination of experimental results,[1] the variation of experimental tools and conditions, and "guided manipulation,"[2] as in the case of magnifying a disturbance or discrepancy. The control strategies also had a social character, because they involved other members of the scientific community. Moreover, experimental control had various aims, such as standardizing measurement units in determining the ohm and validating experimental results in the discovery of argon. With the ohm, Rayleigh and his team varied their apparatus design to control experimental conditions. Those control efforts lay at the heart of their methodology and aimed at dealing with errors. With argon, control permeated every step of the discovery process. This paper aims to investigate and contrast the strategies of control employed in those two cases, and to clarify their various purposes.

9.2 Rayleigh on Different Standards of Rigor

During the first half of the nineteenth century, physicists in Britain were not trained to be physicists. They often graduated as mathematicians, and the most prominent ones came from the Mathematical Tripos at Cambridge. Rayleigh, although a mathematically trained physicist, distinguished between the viewpoints of the pure mathematician and that of the physicist. In his mind, physicists proceeded without pursuing absolute rigor, and they used arguments that mathematicians would deem lacking rigor. As he wrote in the preface to *The Theory of Sound*:

> In the mathematical investigations I have usually employed such methods as present themselves naturally to a physicist. The pure mathematician will complain, and (it must be confessed) sometimes with justice, of deficient rigour. But to this question there are two sides. For however important it may be to maintain a uniformly high standard in pure mathematics, the physicist may occasionally do well to rest content with arguments which are fairly satisfactory and conclusive from his point of view. To his mind, exercised in a different order of ideas, the more severe procedure of the pure mathematician may appear not more but less demonstrative. (Rayleigh 1945, xxxiv–xxxv)

As John Howard argued in his foreword to Rayleigh's biography, which was written by his son, Rayleigh "practiced what is sometimes called the method of modest rigor" (Strutt 1968, xiii). Indeed, he used approximations, often successive ones, and expanded functions into series of terms, keeping only the lowest orders after reaching the desired accuracy. In particular, Howard noted that if the approximations were insufficient to fit the observed data, Rayleigh would use the term of

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[1] The epistemic significance of multiple determination has been debated extensively in the history and philosophy of science (see Schickore and Coko 2013). Those debates have often focused on Jean Perrin's determination of Avogadro's number by several methods (see Coko 2020b).

[2] Schickore (2019) uses this term to refer to a targeted, precise intervention.

next-higher order. He also said that, although this iterative technique underlies processes in modern computing, “in Rayleigh’s day any lack of rigor was considered distressing” (Strutt 1968, xiii).

Further examples may illustrate Rayleigh’s stance toward those different standards of rigor, a stance he expressed in research papers, in reviews of other scientists’ works, and in public pronouncements on science. For instance, in a research paper titled “On the manufacture and theory of diffraction-gratings,” Rayleigh said that “In the present state of our knowledge with respect to the nature of light and its relations to ponderable matter, *vagueness in the fundamental hypotheses is rather an advantage than otherwise; a precise theory is almost sure to be wrong*” (Rayleigh 1874b, 218, our emphasis). Furthermore, in a review of Isaac Todhunter’s *A History of the Mathematical Theories of Attraction and the Figure of the Earth from the Time of Newton to that of Laplace*, Rayleigh questioned Todhunter’s tendency “to prefer rigour of treatment to originality of conception.” He also suggested that “the strictest proof is not always the most instructive or even the most convincing,” and added that “To deserve the name of demonstration an argument should make its subject-matter plain and not merely force an almost unwilling assent” (Rayleigh 1874a, 198).

Rayleigh did not specify exactly what rigor (or “absolute” rigor) meant for mathematicians. As we gather from these examples, he didn’t always favor clearly-defined fundamental hypotheses or strict proofs, which are often thought to be indispensable features of a mathematical treatment. And when those features conflicted with physical considerations, Rayleigh preferred the latter.

Rayleigh’s article on “Clerk-Maxwell’s Papers” illuminates this preference for the physical. There he maintains that a physicist may sometimes depart from the dictates of “strict method”:

> A characteristic of much of Maxwell’s writing is his dissatisfaction with purely analytical processes, and the endeavour to find physical interpretations for his formulae. Sometimes the use of physical ideas is pushed further than strict logic can approve … *the limitation of human faculties often imposes upon us, as a condition of advance, temporary departure from the standard of strict method*. The work of the discoverer may thus precede that of the systematizer; and the division of labour will have its advantage here as well as in other fields. (Rayleigh 1890b, 428, our emphasis)

Thus, according to Rayleigh, a physicist could escape the strict rules of mathematics when seeking a phenomenon’s physical explanation. He held this view and argued for it throughout his scientific life.

In his Presidential Address at the anniversary meeting of the Royal Society in 1906, Rayleigh again explained his position on rigor for mathematicians and physicists. More than 30 years after publishing the *Theory of Sound*, he reiterated the same point:

> Much of the activity now displayed [in Mathematics] has, indeed, taken a channel somewhat remote from the special interests of a physicist, being rather philosophical in its character than scientific in the ordinary sense. […] Closely connected is the demand for greater rigour of demonstration. Here I touch upon a rather delicate question, as to which pure mathematicians and physicists are likely to differ. *However desirable it may be in itself, the*

> *pursuit of rigour appears sometimes to the physicist to lead us away from the high road of progress*. He is apt to be impatient of criticism, whose object seems to be rather to pick holes than to illuminate. *Is there really any standard of rigour independent of the innate faculties and habitudes of the particular mind?* May not an argument be rigorous enough to convince legitimately one thoroughly imbued with certain images clearly formed, and yet appear hazardous or even irrelevant to another exercised in a different order of ideas? (Rayleigh 1906, 89, our emphasis)

Rayleigh noted further that "what is rather surprising is that the analytical argument should so often take forms which seem to have little relation to the intuition of the physicist" (Rayleigh 1906, 89). He believed that, until reconciling the two approaches, "we must be content to allow the two methods to stand side by side, and it will be well if each party can admit that there is something of value to be learned from the point of view of the other" (Rayleigh 1906, 89).

Rayleigh then commented on experimenters and their occasional neglect of the mathematicians' view, stating

> As more impartially situated than some, I may, perhaps, venture to say that in my opinion many who work entirely upon the experimental side of science underrate their obligations to the theorist and the mathematician. *Without the critical and co-ordinating labours of the latter we should probably be floundering in a bog of imperfectly formulated and often contradictory opinions*. Even as it is, some branches can hardly escape reproaches of the kind suggested. I shall not be supposed, I hope, to undervalue the labours of the experimenter. The courage and perseverance demanded by much work of this nature is beyond all praise. *And success often depends upon what seems like a natural instinct for the truth—one of the rarest of gifts*. (Rayleigh 1906, 89, our emphasis)

In any case, although he advocated for differing standards of rigor in mathematics and physics, the quest for it was omnipresent in his scientific practice. In experimentation in particular he associated rigor with control strategies, which he followed to secure experimental outcomes. This association manifests both in determining the ohm and in discovering argon.

The search for rigor also appeared as he explored spiritual phenomena, a topic he was interested in throughout his life. He even served as President of the Society for Psychical Research in the last year of his life (1919). Rayleigh believed that the problematic nature of such phenomena arose "from their sporadic character," as they could not be "*reproduced at pleasure and submitted to systematic experimental control*" (Rayleigh 1919, 648, our emphasis).[3] He maintained that, in general, "we are ill equipped for the investigation of phenomena which cannot be reproduced at pleasure under good conditions" (Rayleigh 1919, 650). For that reason, controlled experimentation was essential.

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[3] Rayleigh's approach to spiritual phenomena is discussed in Noakes (2019). The role of control practices in investigations of spiritual phenomena is discussed in detail in Cristalli (Chap. 6, this volume). For the problems that arise from "singular experiments," see Baker (Chap. 2, this volume) and Schürch (Chap. 3, this volume).

9.3 The Determination of the Ohm

In the second half of the nineteenth century, there was persistent debate over determining and constructing electrical standards, including for resistance. Determining the ohm became an issue of great international importance for reasons both scientific and commercial. The process was intertwined with and significantly directed by the needs of electrical telegraphy (Lagerstrom 1992; Schaffer 1992, 1994, 1995; Hunt 1994; Olesko 1996; Gooday 2004; Kershaw 2007; Mitchell 2017).

In Great Britain, at its 1861 annual meeting, the British Association for the Advancement of Science (BAAS) formed a committee to determine the resistance unit and construct a corresponding standard. In 1863, noted physicist James Clerk Maxwell, engineer and electrician Fleeming Jenkin, and Balfour Stewart, a physicist and meteorologist who had been appointed director of Kew Observatory in 1859, began their experiments in King's College. They meant to determine a wire's resistance in absolute units in order "to construct the material representative of the absolute unit."[4]

In the following year, Maxwell, Jenkin, and Charles Hockin, another Cambridge Tripos graduate who assisted Maxwell and later Rayleigh in their resistance-unit experiments, repeated earlier experiments and reported their results. Their efforts resulted in defining the B.A. unit and in constructing a standard. This determination was soon questioned, however, and the matter was still unsettled when Rayleigh became Director of the Cavendish Laboratory in 1879.[5]

Before Rayleigh undertook the project, others raised objections to previous experiments and argued they were not in "reasonable agreement."[6] More specifically, during the 1860s and 1870s, eminent physicists performed resistance experiments but their results differed both from those of the Committee and among themselves. The most characteristic case involved the famous German physicist and experimentalist Friedrich Kohlrausch and the Danish physicist Ludvig Lorenz. Their results differed by 4%, with the B.A. unit falling in the middle.

Henry Augustus Rowland (1848–1901) has been described as the "father" of the American physics discipline.[7] In 1876, he was appointed the first professor of physics at Johns Hopkins University, a post he held until his death in 1901. In 1878, and amid the disagreement over the resistance standard, Rowland proceeded with his own experiments and a new method. To secure his results from unsuspected

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[4] Report of the Committee appointed by the British Association on Standards of Electrical Resistance 1864, 116.

[5] For the unification of the lab through a research project, see Schuster (1911, 30), as well as Schaffer (1992, 1994).

[6] We owe this phrase to Kuhn, who argued that scientists do not seek "agreement" in numerical tables but "reasonable agreement". See Kuhn (1961, 161–162).

[7] For this characterization, see Kargon (1986, 132) and Wise's introduction in Sweetnam (2000).

constant errors,[8] he attempted to eliminate them in advance by means of the experimental design. As he noted:

> Such a great difference in experiments which are capable of considerable exactness, seems so strange that I decided to make a new determination *by a method different from any yet used, and which seemed capable of the greatest exactness; and to guard against all error, it was decided to determine all the important factors in at least two different ways, and to eliminate most of the corrections by the method of experiment, rather than by calculation.* (Rowland 1878, 145, our emphasis)

For Rowland, different methods lay at the heart of his approach against errors. He thought that his method was "capable of greater exactness than any other, and it certainly possessed the greatest simplicity in theory and facility in experiment" (Rowland 1878, 145). Using a new method, however, was also key for checking existing measurements and for detecting possible errors. Thus, he used "at least two different ways" to determine the experiment's important factors and for securing its result.[9] In addition, Rowland considered constant errors the ultimate threat to the experiment's success, and he sought to avoid them by designing the experiment in a suitable way. Rowland based his method on Kirchhoff's but made modifications. In Kirchhoff's approach "the magnitude of a continuous battery-current in a primary coil is compared with that of a transient current induced in a secondary coil when the primary circuit is removed." Rowland reversed the current's direction in the primary circuit in order to avoid the motion of the primary coil.[10]

Gabriel Lippmann (1845–1921), the notable French physicist whose work spanned many branches of physics,[11] also participated in determining the resistance unit. In 1882 Lippmann proposed a method based on earth induction,[12] consisting in balancing the maximum electromotive force of a continuously rotating earth inductor[13] against the fall in potential in a resistance produced by a measured current.[14] That is, a copper-wire frame revolved around a vertical axis with its circuit open. An electromotive force was thus produced by induction, which reached its peak when the plane of the frame aligned with the magnetic meridian. At that moment, the ends of the moving armature were connected to two wires and through them to a potential

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[8] Here, we follow Rayleigh's terminology. Actually, Rayleigh used "constant error" and "systematic error" interchangeably, but the term "constant error" appeared more often in his writings.

[9] Of course, the idea of determining an experimental result in multiple ways did not originate with Rowland. For the history of this idea in the nineteenth century, see Coko (2015).

[10] For a description of Kirchhoff's method and Rowland's alteration of it, see Rayleigh (1882c, 135–37).

[11] Lippmann's name is principally associated with color photography by interference, an achievement for which he was awarded the Nobel Prize in 1908.

[12] This method in its original plan is attributed to Carey Foster, who proposed it in 1874. Lippmann maintained he had not heard of it when he designed his method for determining the Ohm. See Lippmann (1882a, 316).

[13] An earth inductor is a coil revolving in the earth's magnetic field, generating an induced current.

[14] For more on Lippmann's method, see Lippmann (1882b) and Mitchell (2012).

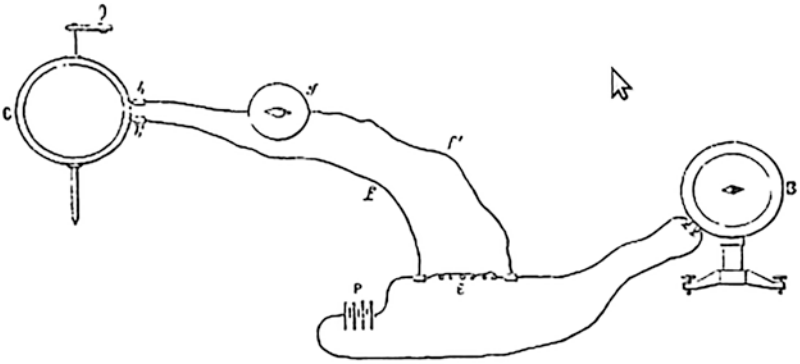

Fig. 9.1 A schematic representation of Lippmann's method (Lippmann 1882a, 315)

difference. If the electromotive force was balanced by the potential difference, no current occurred and the resistance could be estimated by deviations in a tangent galvanometer. The following schematic representation of the experiment was given by Lippmann himself (see Fig. 9.1).

Lippmann never gave the results produced by his method. Nevertheless, he believed that its strength was its directness,[15] meaning that the experimental design avoided errors and therefore avoided corrections. The result was direct control over the method. As he wrote: "Note that this method is most direct: it does not require any calculation of reduction or correction. … As a result, *the control of the method is also direct*."[16] In Lippmann's case, as in Rowland's, we see a distinction between ways of eliminating errors: by calculating corrections to measurements, and by designing experimental controls. From that distinction Lippmann advocated his own experimental method for determining the ohm.

It was characteristic of the determination process that different scientists used different methods. Éleuthère Mascart (1837–1908), the renowned French physicist also involved in the project, coauthored a famous book with Jules Joubert (1834–1910), titled *Leçons Sur L'Électricité Et Le Magnétisme*. There they listed the methods and results they had produced until 1885 (Mascart and Joubert 1897, 619–20). At least seven were based on physical processes—six on induction phenomena, and one on the mechanical equivalent of heat and calorimetry. The number of scientists was equally impressive: more than twenty individuals or teams.[17]

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[15] Mitchell (2012) argues that Lippmann followed the tradition of Regnault in his preference for the direct method.

[16] "On remarquera que cette méthode est des plus directes: elle n'exige aucun calcul de réduction ou de correction. … Il en résulte que *le contrôle de la méthode est également direct*" (Lippmann 1882b, 1349, our emphasis).

[17] A complete enumeration of the methods for determining the resistance unit is not an easy task, and there are different accounts. Harvey L. Curtis (1875–1956), a member of the National Bureau of Standards who was widely recognized for his work on absolute electrical measurements, classified the methods up until the 1930s. See Curtis (1942, 41, 51–52).

The primary purpose in using these different methods was not to choose the best but to reinforce the trustworthiness of the results. Rayleigh did review those methods and attempt to compare them, but this effort was of secondary importance.

In this international debate, multiple determination as a control strategy was a common theme. Gustav Heinrich Wiedemann, the German physicist known for editing *Annalen der Physik und Chemie*, had himself an active role in determining the ohm. In 1882, he described the requirement for multiple determination:

> Hence at any rate it is indicated that the final determination of the ohm must not rest alone on experiments made only according to one method and carried out at one place. Further, the results of each separate method (as I have already mentioned) offer security against possible constant errors only if they are obtained from entirely independent series of experiments, made with apparatus varied in all possible ways. Since investigations are already in progress in different places, with excellent apparatus and according to different methods, we may shortly expect to be in a position to compare together the data which they yield, and so to attain as reliable a final result as possible. (Wiedemann 1882, 275)

The methods and observers should be multiple, and the apparatus should vary in all possible ways. These checks would guard against constant errors. As Wiedemann stated, "the apparatus itself must be frequently altered in various ways. *Only so can we obtain results independent of each other, which can be used for mutual control*" (Wiedemann 1882, 265, our emphasis). Thus, multiple determination of experimental results functioned as a control strategy.

Rayleigh was elected Cavendish Professor of Physics in 1879. Partly because the original apparatus was at the Cavendish laboratory, he tried to unify his laboratory in a common cause by taking up the redetermination of the unit of electrical resistance. He decided "to repeat the measurement by the method of the Committee, which has been employed by no subsequent experimenter" (Rayleigh and Schuster 1881, 1), making alterations he considered necessary. In performing their experiments, he and his team followed this method in two phases, where the composition of the team taking measurements and the apparatus they employed were different. Changing the apparatus aimed at better controlling the experiment conditions.

In the first phase they made experiments with the original apparatus, altered in certain respects to secure uniformity and more accurate measurements. Here Rayleigh's team consisted of Mrs. Sidgwick, Horace Darwin, and Arthur Schuster. Mrs. Sidgwick was Rayleigh's sister-in law, a graduate of Newnham College and an activist for women's rights in education. She assisted Rayleigh in some of his research on electrical standards. Horace Darwin (1851–1928), son of Charles, was an engineer who designed and built instruments and was a co-founder of the Cambridge Scientific Instrument Company. Others took part too, although not in the measuring process; Professor James Stuart (1843–1913) was one, the "first true professor"[18] in engineering in Cambridge. He secured the insulation of the apparatus.[19]

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[18] This is how Stuart is described on the official site of the Cambridge Engineering Department. See http://www-g.eng.cam.ac.uk/125/1875-1900/stuart.html#:~:text=The%20first%20true%20professor%20of,and%20for%20the%20working%20classes

[19] Some critics of the Committee's apparatus had pointed out that its insulation was defective.

According to the preliminary conclusions in the experiments' first phase, it was necessary to enlarge the apparatus to improve the results' accuracy. Thus, the experiments of 1881 were repeated with a new apparatus—with linear dimensions in a ratio of about 3:2. This time, the team recording the measurements also changed as they obtained them. Its members included Rayleigh, Shuster, Mrs. Sidgwick, Lady Rayleigh, Arnulph Mallock, the experimental assistant, and J. J. Thomson, who had just received his B.A. and become Fellow of Trinity College.

Figure 9.2 provides a schematic representation of the method Rayleigh and his team used to determine the ohm. It also shows the enlarged apparatus from the second phase of their experiments. The method was to cause a coil to revolve around a vertical axis and then to observe a magnet's deflection from the magnetic meridian as it hung suspended from the center. The amount of deflection was independent of the earth's magnetic field and varied inversely as the resistance of the circuit. Throughout their work Rayleigh and his team used control strategies, which extended from their initial plan and experimental design to the measuring process and validation of the experimental results. The strategies' principal aim was to standardize the measurement unit.

Control came in different forms in each experimental phase. Controlling the experimental conditions was one form. Rayleigh and his team tried to avoid disturbances (e.g., they performed experiments during the night) and tried to eliminate the effects of things such as short circuits, ground tremors, and observer eye fatigue. Even the experimenters were targets of control.

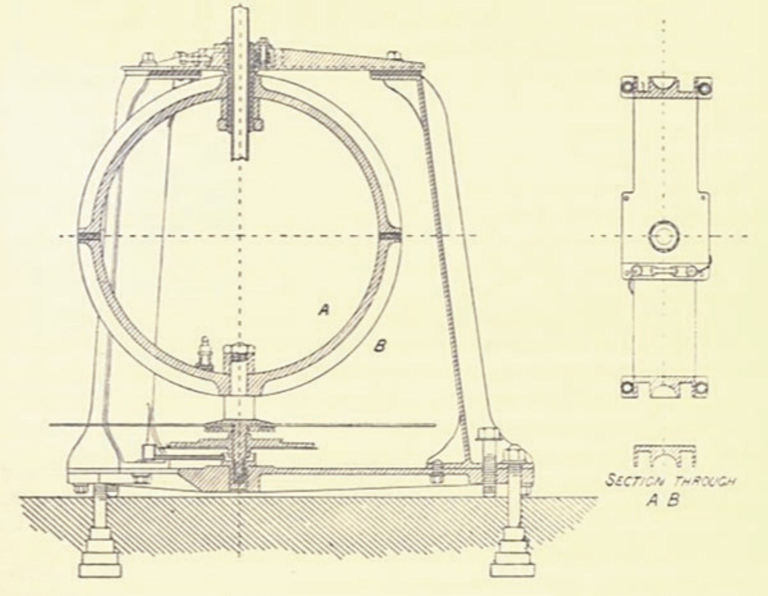

Fig. 9.2 A representation of the apparatus for determining the ohm (Rayleigh 1882a, 39)

Moreover, the direction of the earth's magnetic action varied constantly, and so it was necessary to correct for that variation during the experiment. In this case, the source of interference was itself variable and could not be eliminated; for that reason, it had to be controlled via a measuring process. Rayleigh and his team used a second magnetometer to make direct comparisons between the two devices, whereas the Committee had compared their magnetometer with photographic records of the earth's magnetism obtained by the Kew Observatory at the time of their experiment.[20] Rayleigh and his team were therefore attempting to calibrate their instrument to avoid errors from potential variations in the magnetic field.[21] As Rayleigh explicitly stated:

> It is perhaps worth remarking that owing to the absence of any *controlling instrument* equivalent to our auxiliary magnetometer, the Committee of the British Association had no opportunity of discovering the presence of air currents, as any changes in the zero position would naturally have been ascribed by them to a causal change in the direction of the earth's magnetic force. (Rayleigh and Schuster 1881, 30, our emphasis)

Rayleigh also tried to better control the apparatus' prime mover. The Committee had used a Huygens' gearing,[22] driven by hand in conjunction with a governor. Rayleigh thought that an engine acting by a jet of water upon revolving cups would be an improvement.[23] To achieve a constant head of water,[24] with Darwin's help he connected the engine to a cistern at the top of the building. Although he intended to use a governor of his own invention, he found it unnecessary in the end, as the observer "could easily control the speed" by having the water power a little in excess and using the stroboscopic method (Rayleigh and Schuster 1881, 8–9).

One other general principle Rayleigh followed was to "magnify the disturbances," in order to more closely view any possible causes. Maxwell had advocated this approach as well. In 1876, in a paper titled "General considerations concerning Scientific Apparatus," Maxwell explained that the disturbing agents in an experiment may become the subject of other experiments. In his words:

> We may afterwards change the field of our investigation and include within it phenomena which in our former investigation we regarded as disturbances. The experiments must now be designed so as to bring into prominence the phenomena which we formerly tried to get rid of. (Maxwell 2010, 505)

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[20] See Jenkin and Thomson (1873, 104–105) and Rayleigh and Schuster (1881, 9).

[21] Calibration as a means to secure stability in Rayleigh's experimental practice is also discussed in Schaffer (1994).

[22] A few years later, Rayleigh explained what a "Huygens' gearing" was, and how it could be used in research in electromagnetism. See Rayleigh (1890a).

[23] He wrote in the report "This, it appeared to me, might advantageously be replaced by a water-motor." See Rayleigh and Schuster (1881, 5).

[24] The head of water is a measure for the power of a pump, that is, the highest height at which a pump can convey the water against the action of gravity.

Rayleigh knew Maxwell's work and was probably aware of this guiding principle. It is clear, in any case, that the point is at the core of Rayleigh's experimental practice, as we shall see below in analyzing the discovery of argon.

It is also evident that Rayleigh's team used multiple determination of self-induction, a principal factor for their result's accuracy. Maxwell had done the same.[25] However, Rayleigh thought that, in the Committee's experiments, the value of the coefficient for self-induction had been underestimated. He and his team determined it by different means, including calculating it directly from the dimensions of the coil, basing it on measurements with an electric balance, and deriving it from the principal observations themselves.[26]

Furthermore, at the end of the first part of the experiments' first reports,[27] Rayleigh suggested that most existing determinations introduced time by a swing of the galvanometer needle. Although he did not question the reliability of those determinations, he pointed out that "it is, to say the least, satisfactory to have them confirmed by a method in which the element of time enters in a wholly different manner" (Rayleigh and Schuster 1881, 20). In the second report, Rayleigh included a brief comparison of their own result with values obtained previously by Kohlrausch[28] and Rowland and Joule,[29] and he commented on their (dis)agreement and their expected accuracy.[30]

Thus, Rayleigh's control strategies included using multiple experimental methods and comparing their results. For him, this aspect of control was crucial, as it secured an experiment's outcome. In 1882 he devoted an article to the subject, entitling it "Comparison of Methods for the Determination of Resistances in Absolute Measure." There he reviewed the six available methods for determining the resistance unit, pointing out their relative merits and demerits. Those methods were based on different experimental apparatuses and used different formulas for the value of the electrical resistance according to which procedure was followed. Rayleigh focused on methods involving an induced electromotive force, not considering Joule's calorimetric method. The others included three we have already mentioned (Kirchhoff's, Lippmann's, and that of the BAAS Committee), along with three others: two by Weber (employing transient currents and damping, respectively)[31] and Lorenz's method. Rayleigh was convinced that "it is only by the coincidence of results obtained by various methods that the question can be satisfactory settled" (Rayleigh 1882c, 139).

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[25] Note, though, that they did not include that determination in their report. It was to be found in Maxwell's paper on the "Electromagnetic Field." See Rayleigh and Schuster (1881, 11).

[26] Maxwell had also determined it by different means.

[27] The first report of 1881 was written in two parts, one by Rayleigh and the other by Schuster.

[28] Kohlrausch had followed "Weber's *Method by Damping*." See Rayleigh (1882c, 145).

[29] Here, Rayleigh referred to the experiments on the mechanical equivalent of heat involving measurements of absolute resistance.

[30] See Rayleigh (1882a, 47–51).

[31] Weber had actually proposed four methods. See Wiedemann (1882).

It is worth noting here that Rayleigh also cared about evaluating each method's accuracy. At the end of the article, he suggested that Lorenz's method offered the best chance of success. In that method, "A circular disk of metal, maintained in rotation about an axis passing through its centre at a uniform and known rate, is placed in the magnetic field due to a battery-current which circulates through a coaxal coil of many turns" (Rayleigh 1882c, 145–46). Rayleigh believed that, with this way of performing the experiment, the errors of the principal quantities to be measured did not affect the final result as much as they did with other methods. He reached his conclusion regarding the propagation of errors by applying differential calculus. Before pursuing that method (Rayleigh and Sidgwick 1883), however, he thought that "the value now three times obtained in the Cavendish Laboratory by distinct methods should be approximately verified (or disproved) by other physicists" (Rayleigh 1882c, 150).[32]

Collective knowledge and experience were indispensable elements of the control process for validating Rayleigh's experimental results. Indeed, standardization demanded consensus among the members of the scientific community—and not among them only, but also among the "practical men," the practitioners working in electrical telegraphy.[33] As several scholars have argued, consensus in determining the ohm was a complex matter, involving national rivalries and personal agendas. Agreement was not established solely on scientific grounds or on the accuracy of the determinations as such.[34] At any rate, multiple determination was a guiding principle for Rayleigh, and stemmed from his beliefs about sound experimental methodology.

Schuster mentioned that Rayleigh "never felt satisfied until he had confirmed his results by different methods, and had mastered the subject from all possible points of view" (Schuster 1921, xxvi). Rayleigh thought that all experimenters should follow this principle in physics, and in 1882 he discussed it in his Address to the Mathematical and Physical Science section of the British Association meeting. In his words:

> The history of science teaches only too plainly the lesson that no single method is absolutely to be relied upon, that sources of error lurk where they are least expected, and that they may escape the notice of the most experienced and conscientious worker. *It is only by the concurrence of evidence of various kinds and from various sources that practical certainty may at last be attained, and complete confidence justified*. Perhaps I may be allowed to illustrate my meaning by reference to a subject which has engaged a good deal of my attention for the last two years—the absolute measurement of electrical resistance. (Rayleigh 1882b, 119–20, our emphasis)

It is noteworthy that Rayleigh drew his example from the project of determining the ohm. He did not search for a concurrence of evidence solely with his own experiments; he also appealed to the scientific community, and this appeal served as a basis for controlling his own results. At the very end of his 1882 Address, he stated that:

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[32] Rayleigh here referred also to the experiments made by Glazebrook following Rowland's method. Cf. Schaffer (1994, 282).

[33] See Hunt (1994).

[34] See Schaffer (1994) on 'fiat' agreement, and Gooday (2004).

> If there is any truth in the views that I have been endeavouring to impress, our meetings in this section are amply justified. If *the progress of science demands the comparison of evidence drawn from different sources, and fully appreciated only by minds of different order*, what may we not gain from the opportunities here given for public discussion, and, perhaps, more valuable still, private interchange of opinion? Let us endeavour, one and all, to turn them to the best account. (Rayleigh 1882b, 124, our emphasis)

Rayleigh's expression "minds of different order" referred to different kinds of physicists. In particular he distinguished between two kinds: the experimenters and the mathematicians. He claimed that each values different sorts of evidence and argumentation. The experimenters, according to Rayleigh, "disregard arguments which they stigmatise as theoretical," while the mathematicians "overrate the solidity of the theoretical structures and forget the narrowness of the experimental foundation upon which many of them rest" (Rayleigh 1882b, 122). For Rayleigh, however, each approach mattered: using different experimental methods and multiple observers, finding agreement among experimental results, and appealing to different sorts of arguments (theoretical and experimental) all had their place in securing an outcome's validity.

Rayleigh's involvement in determining electrical standards was not limited only to experiments with the Committee's method, or to reviews of other methods. In 1882 he and Mrs. Sidgwick also began experiments by Lorenz's method, reporting their results the following year. They worked on related topics as well, such as the electro-chemical equivalent of silver and the absolute electromotive force of Clark cells. Regarding silver's equivalent, in 1897 Rayleigh recollected that, when they undertook the task, the previous results' uncertainty was at least 1%. He also restated his conviction about the necessity of using different methods and different observers for securing experimental results:

> Security is only to be obtained by the coincidence of numbers derived by different methods and by different individuals. It was, therefore, a great satisfaction to find our number (*Phil. Trans*. 1884) (0.011179) confirmed by that of Kohlrausch (0.11183), resulting from experiments made at about the same time. (Rayleigh 1897a, 332)

As we have already mentioned, this was a guiding principle in his experimental practice. And it is also a principle at work in the discovery of argon.

9.4 The Discovery of Argon

Determining the ohm was the project that gave Rayleigh a reputation as an exact experimenter. He is perhaps best remembered, however, for the discovery of argon, a new element and hitherto unknown constituent of the atmosphere.[35] He won the 1904 Nobel Prize in Physics for "his investigation on the densities of the most important gases, and for his discovery of Argon, one of the results of those

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[35] The discovery of argon has been the topic of many studies in the history and philosophy of science. For full references to this literature, see Arabatzis and Gavroglu (2016).

investigations."[36] The same year, William Ramsay won the Prize in Chemistry for his "discovery of the inert gaseous elements in air, and his determination of their place in the periodic system,"[37] with argon being the first. Rayleigh and Ramsay, working at first independently and then in concert, took on the task of isolating the gas and studying its properties.

As Arabatzis and Gavroglu have argued,[38] the discovery of argon was not an event but an extended process. As such it comprised not only detecting but also identifying and assimilating argon into the conceptual framework of nineteenth-century chemistry. Throughout the process Rayleigh used various control strategies: from detecting discrepancies between the densities of "atmospheric" and "chemical" nitrogen, to isolating and identifying a new constituent of the atmosphere, and subsequently to exploring its properties. Here as elsewhere, the main aim of experimental control was to validate the experimental results.

The starting point for the discovery process was Prout's law, which says that the atomic weights of the elements were whole multiples of the atomic weight of hydrogen. As early as 1882, Rayleigh had expressed his willingness to redetermine the densities of the "principal gases"[39] to test that law. He started by determining the relative densities of hydrogen and oxygen and then proceeded to the density of nitrogen. Given that he originally aimed to test Prout's hypothesis, Rayleigh determined the ratio of atomic weights of oxygen to hydrogen via the densities of those gases. But he also tried an independent and novel determination, one based on the composition of water.[40] Rayleigh was not the only one who used more than one method to determine atomic weights. For instance, the American chemist Theodore William Richards (1868–1928) used five to determine copper's atomic weight.[41]

In experimenting with the density of nitrogen, Rayleigh used two principal methods to prepare the gas.[42] In the first, atmospheric air was "freed from CO_2 by potash" and then the oxygen was removed by "copper heated in hard glass over a large Bunsen" burner. It was then passed over "red-hot copper in a furnace" before being treated with "sulphuric acid, potash and phosphoric anhydride" (Rayleigh 1892, 512). Regnault had followed this method in experimenting with the densities of the principal gases.

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[36] Award ceremony speech. Available at https://www.nobelprize.org/prizes/chemistry/1904/ceremony-speech/

[37] Award ceremony speech. Available at https://www.nobelprize.org/prizes/chemistry/1904/ceremony-speech/

[38] Arabatzis and Gavroglu (2016).

[39] That is, hydrogen, oxygen, and nitrogen. See Rayleigh (1882b, 1904).

[40] See Rayleigh (1888) and Clarke ([1882] 1897).

[41] See Ihde (1969).

[42] Different methods of preparation were not only applied to nitrogen. As mentioned above, in his earlier experiments on the relative densities of hydrogen and oxygen Rayleigh also used different methods for the preparation. See Rayleigh (1887). As he pointed out in another paper concerning the densities of carbon oxide, carbonic anhydride, and nitrous oxide, "agreement … is some guarantee against the presence of impurity." See Rayleigh (1897b, 348).

The main difference between the first and second method is the use of ammonia. In the second the oxygen was combined with the hydrogen of ammonia, through which the air passed before the furnace with the red-hot copper. Rayleigh used the method on Ramsay's suggestion. In his reports, Rayleigh referred to nitrogen of different origins with different names. He called the gas obtained by the first method "atmospheric nitrogen," whereas that prepared with ammonia was "chemical nitrogen."

Although the results of the second method[43] were in close agreement, Rayleigh still used the other. As he observed in his Nobel Lecture, multiple methods were always desirable:

> Turning my attention to *nitrogen,* I made a series of determinations using a method of preparation devised originally by Harcourt and recommended to me by Ramsay […] Having obtained a series of concordant observations on gas thus prepared I was at first disposed to consider the work on nitrogen as finished. Afterwards, however, I reflected that the method which I had used was not that of Regnault *and that in any case it was desirable to multiply methods*, so that I fell back upon the more orthodox procedure according to which, ammonia being dispensed with, air passes directly over red hot copper. (Rayleigh 1904, 212–13, our emphasis)

To his surprise, he found a discrepancy of 1/1000 in nitrogen's density as given by those two methods. He could not attribute the discrepancy to experimental error because the measurements for each method did not present deviations greater than 1/10000.[44] With this order-of-magnitude difference, Rayleigh claimed that experimental error could not explain the discrepancy. Thus, his claim stems from his confidence that the experimental conditions were stable and well-controlled. As it turned out, the discrepancy between "atmospheric" and "chemical" nitrogen was the initial step that later led to discovering a new constituent of air: the inert gas argon.

Faced with the discrepancy, Rayleigh published a letter in *Nature*[45] inviting criticism from chemists and asking for their help. At the time he regarded the situation "only with disgust and impatience," although his call for help may seem striking in itself.[46] At any rate, his rush to publish the letter may be due to a lack of confidence in his chemical knowledge.[47] As with determining the ohm, however, the call reveals a communal aspect to his control processes. Rayleigh expected that chemists would make suggestions, and then he could examine them. It was not only an invitation for

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[43] The numbering of the methods here follows the one that Rayleigh gave in his reports. He considered the first method more established, something apparent in the following quotation and the fact that he called it "the more orthodox procedure."

[44] In earlier work on the densities of hydrogen and oxygen, Rayleigh had also aimed at an accuracy of 1/10000. Thus, the magnitude of experimental error was established both from previous research and from experiments on nitrogen. On this point see also Spanos (2010, 362).

[45] Rayleigh (1892). See also Rayleigh (1895).

[46] Note, though, that this was actually rather common in the eighteenth and early nineteenth century. See Schickore (2023) and Schürch (Chap. 3, this volume).

[47] As Rayleigh's son suggested in his account of the discovery of argon. See Strutt (1968, 189).

public discourse—he appreciated private communication as well. Thus, he placed control in the hands of the community and did so early in his research. He hoped others would help him explain the unequal measurements. He only obtained, however, "useful suggestions, but none going to the root of the matter" (Rayleigh 1895, 189).

His next step was to magnify the discrepancy. In the preparation of "chemical" nitrogen by ammonia, only one-seventh of the final quantity was "derived from the ammonia," with the rest from atmospheric air (Rayleigh 1895, 189).[48] Thus, the most obvious way to achieve such a magnification was to get all the nitrogen from ammonia. Here is how Rayleigh explained that process:

> One's instinct at first is to try to get rid of a discrepancy, but I believe that experience shows such an endeavour to be a mistake. What one ought to do is to magnify a small discrepancy with a view to finding out the explanation; and, as it appeared in the present case that the root of the discrepancy lay in the fact that part of the nitrogen prepared by ammonia method was nitrogen out of ammonia, although the greater part remained of common origin in both cases, the application of the *principle* suggested a trial of the weight of nitrogen obtained wholly from ammonia. (Rayleigh 1895, 189, our emphasis)

In his Nobel Lecture he repeated the same point: "It is *a good rule in experimental work to seek to magnify a discrepancy* when it first presents itself, rather than to follow the natural instinct of trying to get quit of it" (Rayleigh 1904, 213, our emphasis). Whether a rule or principle, "magnifying the discrepancies" was indispensable to Rayleigh's experimental practice. This form of control amounted to "guided manipulation," which aimed at finding an appropriate explanation for the discrepancy. In this case the discrepancy was magnified about five times, firmly establishing the initial experimental outcome and indicating the need for further research.[49]

The next stage in the discovery process was to explain the discrepancy. Was the "atmospheric" nitrogen heavier than the "chemical" because of impurities? If so, in which nitrogen were the impurities to be found? Were there lighter impurities in the "chemical" nitrogen, or heavier impurities in the "atmospheric" nitrogen? Was there some other form of nitrogen, like N_3 or nitrogen in a partially "dissociated state"?

Rayleigh altered the preparation of nitrogen to confirm his initial result and clarify the discrepancy's cause. In the next 2 years he produced "atmospheric nitrogen" by replacing hot copper with hot iron or ferrous hydrate, and "chemical nitrogen" by using nitric oxide, nitrous oxide, and ammonium nitrite, along with substituting hot copper with hot iron. The result did not change.

Regarding the possibility of lighter impurities, the possibility of hydrogen as their source struck Rayleigh as the most worth investigating. If that was the case, however, and hydrogen was present, the copper oxide should consume it. Rayleigh approached the matter experimentally. To exclude the possibility of a lighter hydrogen-based impurity in the "chemical nitrogen," a certain amount was

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[48] Note, however, that in his Nobel Lecture some years later, Rayleigh claimed that a larger part of the nitrogen (one-fifth) was obtained from ammonia.

[49] Here, we follow Rayleigh's account in his Nobel Lecture.

introduced into the heavier "atmospheric nitrogen." It made no difference to the result and the hypothesis was rejected.

At first at least, Rayleigh leaned toward the possibility that nitrogen was being produced in a "dissociated state." But he changed his mind because of skeptical suggestions from his "chemical friends." There was chemical evidence that if nitrogen was dissociated, it was likely the atoms would not continue to exist for long. Rayleigh also checked the hypothesis of dissociated nitrogen by subjecting both gases to the action of silent electric discharge. Their weights remained unchanged, indicating the hypothesis was probably wrong. Finally, he made another experiment to secure the conclusion. He stored a sample of "chemical nitrogen" for 8 months to check its density. He found no sign of increase.[50]

As is evident, every step in the detection process was cross-checked, either with different experimental methods or with a combination of theory and experiment. The methodology of multiple preparations motivated Rayleigh and Ramsay to the conclusion that "chemical" nitrogen was a uniform substance. The properties of the samples produced different showed it had to be one and the same substance. On this point they stated: "That chemical nitrogen is a uniform substance is proved by the identity of properties of samples prepared by several different processes and from several different compounds" (Rayleigh and Ramsay 1895, 180). Rayleigh and Ramsay also maintained it was difficult to see how a gas of chemical origin could be a mixture. If that was the case, there should have been two kinds of nitric acid (when that acid was used in the preparation). They argued further that the claim that nitrogen is a mixture could not be reconciled with the work of Belgian chemist Jean Stas and others on the atomic weight of nitrogen.[51] Thus, control via multiple preparations went hand in hand with control via consistent agreement with the works of other chemists.[52]

In addition, the question of whether "atmospheric" nitrogen was a mixture of nitrogen and another substance was also investigated in detail, along with its isolation. They first tried to isolate it using two methods and then used atmolysis to ascertain its nature.

On the one hand, Rayleigh approached the question as Cavendish had done in 1785, more than a century earlier. He turned his attention to Cavendish after Dewar made a suggestion in 1894.[53] Nitrogen was removed with the aid of oxygen, subjecting the mixture to an electric spark (see Fig. 9.3). The process always left residue, which could be isolated.

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[50] See Rayleigh (1894, 104–8).

[51] See Rayleigh and Ramsay (1895, 135–36).

[52] See Rayleigh and Ramsay (1895, 180).

[53] On this point there was a difference of opinion. According to Ramsay's recollection, he was the one who had first drawn Rayleigh's attention to Cavendish. However, Rayleigh himself, together with his son, claimed that Dewar was the first to say it. Rayleigh's son also supplied testimonies from other scientists, such as Dewar and Boy, confirming that claim. See Rayleigh (1895, 191) and Strutt (1968, 194–95).

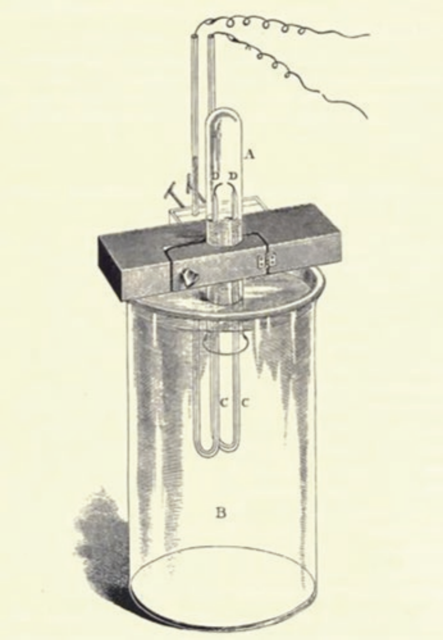

Fig. 9.3 Isolation of argon with the aid of oxygen, subjecting the mixture to an electric spark (Rayleigh and Ramsay 1895, 142)

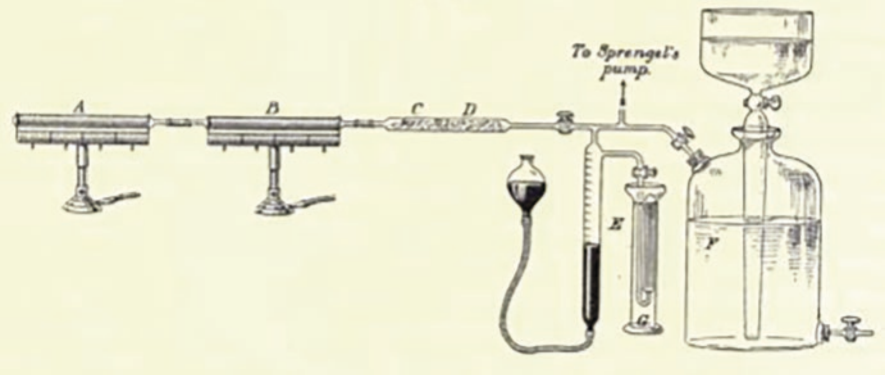

Fig. 9.4 Isolation of argon by means of red-hot magnesium (Rayleigh and Ramsay 1895, 144)

On the other hand, Ramsay also followed another method: absorbing the nitrogen by means of magnesium at full heat (see Fig. 9.4).

Rayleigh and Ramsay gave seven reasons to justify their conclusion that atmospheric nitrogen was a mixture of nitrogen and argon. One was based on the double isolation just mentioned, along with their belief that "It is in the highest degree improbable that two processes [Cavendish's and Ramsay's], so different from each other, should each manufacture the same product" (Rayleigh and Ramsay 1895, 180).[54] This philosophical commitment was key to their method.

Rayleigh and Ramsay examined every alternative hypothesis that they or others thought of, and eliminated all but one: they concluded that the origin of the discrepancy must be a new constituent of the atmosphere. Considering alternative hypotheses was another way to control their explanation's validity. They tested the alternatives with auxiliary experiments and/or theoretical considerations. The latter method was a way to control theory, since it rested on reasons to exclude possible explanations and not on any material manipulation.

Control practices were also present in exploring argon's properties. To determine its density, for example, Rayleigh and Ramsay used different methods and directed a number of experiments toward that end. The gas(es) obtained from Cavendish's method, along with those from Ramsay's, were examined to determine their densities.

A first estimation of the gas's density from Cavendish's method used the initial measurements of the densities of nitrogen from different origins. They were able to calculate its density as long as they assumed the densities differed because of argon. Because it was difficult to directly determine the density of argon owing to the small quantities collected, they filled a large globe with an oxygen–argon mix of known proportions and determined its density. In every measurement, experimental objects and conditions were standardized and then correction applied for certain constant errors. The amount of the residual nitrogen was estimated through spectrum analysis.[55]

Rayleigh and Ramsay also determined argon's density using Ramsay's magnesium method. Using another gas as reference, they had three auxiliary experiments to test the accuracy with which the density of the unknown gas could be determined. They chose the density of dried air as their reference value and compared the mean of their measurements with that obtained by "several [other] observers" (Rayleigh and Ramsay 1895, 149). The control process again rested on multiple determination and knowledge established by other scientists. Rayleigh and then Ramsay proceeded to directly determine argon's density, and concluded it was "at least 19 times as heavy as hydrogen" (Rayleigh and Ramsay 1895, 150).

Spectroscopy was another means used to identify the atmosphere's new constituent. After isolating it on a larger scale by the magnesium method, two other scientists—William Crookes and Arthur Schuster, working independently—subjected the gas to spectrum analysis. They meant to identify it and determine whether it was a mixture or not.[56] To achieve the best results they used electrodes of different materials. Both sources of argon gave identical spectra.

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[54] A similar "argument from coincidence" has been more recently employed by Ian Hacking to support "truth in microscopy." See Hacking (1983, 200–2).

[55] See Rayleigh and Ramsay (1895, 166; footnote added in April 1895).

[56] At the time, spectroscopy was a controversial technique among chemists. See Arabatzis and Gavroglu (2016).

Crucial to the new element was its ratio of specific heats, as it was directly related to its number of atoms. To determine the ratio, Rayleigh and Ramsay performed experiments on the velocity of sound in argon. They used a familiar apparatus, but in a way that “differed somewhat from the ordinary pattern.” To test “the accuracy of this instrument,” “fresh experiments were made with air, carbon dioxide and hydrogen,” and their results were compared to those of other observers. By this control process they “established the trustworthiness of the method,” which then led to a ratio of specific heats that was “practically” identical with “the theoretical ratio for a monatomic gas” (Rayleigh and Ramsay 1895, 174–76).

Rayleigh and Ramsay therefore employed several control strategies in discovering argon, and these strategies were integral to the discovery. They also played different roles in different research stages. A primary form of experimental control was the multiple methods. This was not just because Rayleigh and Ramsay participated in that project independently. Rather, as we explained, multiple determinations were essential throughout the discovery process—from Rayleigh working by himself to detect the initial discrepancy, to Rayleigh and Ramsay working together to identify argon and explore its properties.

9.5 Concluding Remarks

Other historians and philosophers of science have discussed each episode we have treated here. The reason to bring them together in this paper is to explore the control strategies used by Rayleigh across his research. We have thereby revealed a pervasive pattern in how he conducted experiments. Both stories began as a project of redetermination: of the B.A. unit of resistance, and of the densities of the principal gases. But the stories developed differently. In determining the ohm, the project never changed direction and terminated with a measured value; with argon, the research agenda shifted radically. After the first experiments revealed the initial discrepancy, the aim was no longer to determine the density of nitrogen. It was to establish the discrepancy beyond doubt and identify its cause. This explanatory quest ended with identifying a new constituent of the atmosphere, after all other explanations had been rejected. Thereafter their research focused on determining the new gas’s properties.

Experimental control itself had different aims in those two cases. With the ohm, control served to standardize a unit, while with argon control strategies were used to validate initial experimental results, identify argon’s sources, and investigate its properties. Nevertheless, in both cases the control strategies shared some common features, such as varying the experimental conditions and obtaining agreement among results produced in different ways. Those features stemmed from Rayleigh’s general methodological approach and from his attitude about experimental practice. As we saw, Rayleigh advocated the use of multiple determination whenever feasible. This focus was evident at various levels in determining the ohm, and at nearly every step in discovering argon.

Crucial for Rayleigh also was a check or comparison via multiple determination, along with the evaluation of each method. Although agreement between items of evidence was essential to secure experimental results in both cases, it was especially in the case of argon that the agreement became the ground on which both the validity of the result and the identification of the new element rested.

Furthermore, multiple determination had several epistemic aims. First, it was a means to secure experimental results against undetected systematic errors. If results from independent methods and/or observers agreed within certain limits of accuracy, researchers could assume that no important sources of error had been left out of consideration. Rayleigh used this argument in both of the cases that we have examined here. Moreover, the significance of multiple determination as a security factor against error was widely recognized among the other scientists involved in those cases.

The epistemic aims continued, though. Agreement of experimental results was also the ground on which a fact could be distinguished from an artefact. On that basis, for instance, Rayleigh and Ramsay argued that argon was a new element in the atmosphere and was not a "manufactured" product of the experimental process. Here, an existence claim was based on the epistemic strategy of multiple determination.

Another key feature of Rayleigh's experimental methodology also evident in both cases was magnifying a discrepancy or disturbance. The gist of this procedure was to control the experimental conditions via a "guided manipulation," so as to magnify the initial discrepancy and distinguish it from experimental artefacts or background noise. In this way it facilitated the search for an explanation.[57]

Finally, in both cases, Rayleigh favored involving different experimenters. In his view, the communal aspect of control was essential. Previous knowledge of the scientific community, along with accumulated experimental results, were points of reference for his checks or comparisons.[58]

All in all, various control practices were key features of Rayleigh's experimental research. Among them we may underscore the following: controlling the experimental conditions, varying experimental parameters so as to find out the underlying causes and determine their contribution to the final result, and multiple determination at every step. These practices were accompanied by an open attitude toward the scientific community, which could offer supplementary control for any results. At any rate, one thing is certain: control strategies and multiple determinations were not idle philosophical constructs, but rather indispensable elements of Rayleigh's experimental practice.[59] As he insisted many times, a multiply determined outcome was more secure than one derived from a single method. His systematic use of

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[57] This rule of "magnifying the discrepancy" or the disturbance was applied by Rayleigh both to distinguish a fact from an artefact, and to identify an experimental error and its contribution to the final result. However, artefacts and systematic experimental errors are not the same thing.

[58] For the communal aspect of control, see also Schürch (Chap. 3, this volume).

[59] Cf. Coko (2020a, 508), who points out that multiple determination "is not a philosopher's invention, but a strategy employed by scientific practitioners themselves."

multiple determination indicates that researchers, well before the early twentieth century, recognized the epistemic force of agreement among independently produced experimental results. Jean Perrin's use of multiple determination for demonstrating the existence of atoms, which has become a canonical case in the history and philosophy of science literature, had a worthy precedent in Rayleigh.

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