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Replicating the Practices of Discovery: Michael Faraday and the Interaction of Gold and Light

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The late historian of science Frederic Lawrence Holmes began a recent book on the early researches of Antoine Lavoisier (Holmes, 1998) by noting the extreme difference between the public face of the great 18th-century scientist and the image suggested by his laboratory notebooks: "We ... see a public Lavoisier announcing for the first time an 'epochal' theory with a grand gesture and an aura of self-assurance that contrasts strikingly with the trepidations of the private investigator" (p. 4). For Holmes, this discrepancy was important, and his search, as a historian, for the roots of scientific creativity led him to the intensive examination of notebooks and laboratory diaries, not just of Lavoisier but of Hans Krebs and of Claude Bernard as well.

Understanding the roots of creativity in science and technology might seem—obviously—to demand close attention to such sources, but it is also true that notebooks and diaries cannot be studied in isolation. Scientific discovery depends on a context far larger than that of the single investigator alone in a laboratory, and even the fullest of laboratory records must acknowledge that discovery always begins with a context external to the discoverer and ends (with text or demonstration or built artifact) in an external—public—context. It is thus no surprise that, complementing the work of historians such as Holmes, whose focus has been on "private" science, there have been richly revealing accounts of the "public" side of science. For example, Golinski (1992) analyzed the debate between Lavoisier and Priestley, and Rudwick (1985) examined the emergence of the Devonian concept in geology by tracking the enterprise of a network of amateur and professional scientists.

Part of the context of scientific discovery resides in its material entities; thus, historians and philosophers of science are paying more attention to the artifacts of science (instruments, graphs, mathematical representations, etc.), just as cognitive scientists are beginning to analyze cognitive artifacts, those external objects and representations that serve as computational substitutes for "in the head" cognition (Hutchins, 1995; Zhang & Norman, 1994). It has long been recognized that the analysis of cognitive behavior is incomplete unless the complexity of the environments of cognition is accounted for (e.g., Simon, 1967/1996). What seems new, however, is the growing attention given to the need for an interactive play among both internal and external aspects of cognition (e.g., Gooding, 1992). Such blurring of the boundaries of cognition is most essential in the attempt to understand the formation of representations in the first place.

In this chapter we describe aspects of our investigation of the surviving experimental artifacts used by Michael Faraday in his 1856 research on the properties of gold films and his resulting discovery of metallic colloids and the associated optical phenomenon now known as the *Faraday–Tyndall effect.*[1] These are important findings, although, as we shall see, Faraday felt frustrated with the research program, which aimed at larger questions. Even so, our study can add to knowledge of the personal side of Faraday's endeavors, especially because we focus on what can be learned from the material objects that accompany Faraday's very complete diary records. Previous readings of Faraday's 1856 diary records have sometimes argued that the record was merely a confusing hodgepodge of unconnected pieces (e.g., Williams, 1965, and perhaps also Zsigmondy, 1909, who referred to Faraday's

[1]A *colloid* is formed when finely divided particles of a substance are held in suspension in a fluid. Colloids differ from *solutions* in that solutions represent ionized particles of atomic size, carrying an electrical charge. Ions, the particles that form a solution, are much smaller than colloidal particles. Faraday's discovery that metals could form colloids was important, as was his demonstration that the metallic colloids were particulate and possessed optical properties—the Faraday–Tyndall effect —that are very different from solutions (as we explain later).

methods as "crude," p. 75). We instead make the case that the apparent gaps between Faraday's experiments and his finished (and unfinished) mental models must be filled with additional information to understand his work. The discovery of Faraday's slides allowed a new and crucial visual domain to aid in interpretation of the diary, and our replications, as we show, extend this even further. Far from a muddle, the combination of diary *plus* specimens, *plus* replications, allows a deep understanding of the progression from confusion to knowledge.

By replicating some of Faraday's experiments, we show that the understanding of his scientific practices requires more than what a purely textual analysis could achieve and that Simon's (1967/1996) point about the analysis of the environments of cognition constitutes a necessary imperative for the understanding of discovery. The experiments carried out in our laboratory were formulated with the intent of understanding how Faraday combined his beliefs about light, matter, electricity, and magnetism; wove these beliefs into a creative and productive series of laboratory experiments; and used his experiences in the laboratory to formulate new ideas and explanations. Our experiments resemble his in one sense: Like Faraday, we began with only a series of broad beliefs about the nature of scientific cognition, beliefs that have been productive in other domains (including other aspects of Faraday's research). Like Faraday, we sought to use our experiences to refine and sharpen our own beliefs about scientific discovery.

Our results, including the early phases in which we "stumbled around," trying to get it right, allowed us to account for an important conceptual change that occurred in Faraday's thinking early in the project he conducted in 1856. Thus, later in the chapter we argue for a constructive role of such "confusion" in the process by which Faraday reconceptualized his ideas. Furthermore, our replications led us to a new description of the differences between the discovery processes early in a research project versus those that occur later, when the research is more developed.[2] As we argue, the value of programmatic replication is most evident in this psychological domain in which the cognitive processes of an investigator are under examination; such processes otherwise leave no direct evidentiary trace, and only in their reconstruction can their dynamics be made visible.

To begin our account, we first describe an unexpected opportunity (a historical "discovery") that inspired the replications. We then describe two series of our replication experiments, one on colloids and precipitates of gold

[2]Others have used replication as an aid in understanding specific experiments in the history of science. For example, Heering (1994) reconstructed the electrostatic balance by which Coulomb established the inverse square law of electrostatic attraction and repulsion. The results suggested that Coulomb's study could not have been done exactly as he described it in his formal report.

and the other on "deflagrations," that is, on gold wires that are exploded with rapid bursts of electric current. After each replication series, we place our results in context with Faraday's results, as reported in his diary and as manifest in the surviving specimens of his work. Finally, we consider the implications of the project for the understanding of scientific thinking.

FARADAY AND THE PROBLEM OF GOLD

In 1856, Michael Faraday (1791–1867) was nearing the end of a very productive scientific career. He was, of course, best known for his discoveries in the domains of electricity and magnetism, but his activities as a chemist were no less consequential, particularly his 1832 demonstration of the electrochemical equivalent of electric current, a discovery that quantified the close relation between electricity and the compositional forces of matter. Throughout his career, a constant theme was the attempt to unify the seemingly separate domains of force, and this had led to the precise articulation of his comprehensive (if not yet complete) field theory (Gooding, 1981; Nersessian, 1985; Tweney, 1992a; Williams, 1966). By 1850, Faraday had shown that the electric and magnetic fields could be regarded as the manifestation of physically real (but immaterial) lines of force that extended throughout space, and his conviction that light must be related to these lines of force was partly responsible for his discovery (in 1846) of the rotation of a plane polarized beam of light passing through a dense optical medium to which a magnetic field was applied. If not yet a complete "unification," the phenomenon was an important clue that light was somehow implicated in the nature of electromagnetic fields (James, 1985; Tweney, 2002).

Thus, when Faraday carried out nearly a year's worth of research in 1856 on the optical properties of thin gold films (our focus in this chapter), he was seeking more than just the explanation of certain optically pretty effects. The program was also intended to explore some of the deepest questions of physics: Were particles of matter best considered as "force centers"? Was there an ether that served as the medium of light transmission? Could this speculation about force centers be integrated with the demonstration that field forces (electricity and magnetism) consisted of nonmaterial lines of force? James (1985; see also Chen, 2000) has argued that Faraday was committed in many ways to an "optical mode of investigation" and that he frequently used optical criteria, for example, in his development of the distinction between diamagnetic and paramagnetic substances, and his long-standing concern with whether there was an ether. In the gold research, Faraday sought to extend his optical research to an account of light itself.

Gold has richly varied optical properties and was thus a prime suspect in the search for deeper understanding of the interaction of light and matter. Gold is so malleable that it can be hammered out into extremely thin transparent films, and it was known by 1856 that such films were much thinner than the wavelength of light. Furthermore, these films interested Faraday because thin transparent gold films are a different color by transmitted light than by reflected light. When a beam of light is passed through such a film, it manifests a green, blue, or purple color rather than the familiar shiny yellow–gold seen by reflected light. For Faraday, the initial question was simple: How could such very thin (and apparently continuous) films alter light so dramatically? In the end, Faraday failed to answer this question, but the research had important consequences—not only the discovery of metallic colloids and the Faraday–Tyndall effect, as we have noted, but also the first clear indication that all of the color effects of gold were the product of the interaction of *particles* of gold and light. That is, contrary to his initial expectation, Faraday had to confess at the end that he had no evidence for the existence of *continuous* matter, not even for the shiny gold films!

In this chapter we argue that replications of some of Faraday's earliest experiments leads to a perspective on how the research was launched, how Faraday resolved some of his initial "confusions," and how these were important to his final conclusions.

There have been many studies of Faraday's research conducted by historians, philosophers of science, and psychologists, most of them based on his surviving laboratory diary. This extraordinary set of documents, a large part of which has been published (e.g., Martin, 1936), covers nearly his entire career, and the completeness of the record has caused it to be compared to a "think aloud" protocol (Tweney, 1991). Some have used it as the basis for a reconstruction of his research practices (e.g., Steinle, 1996). In some cases, however, as in the case of his research on gold, much of the diary is hard to interpret by itself: He makes reference to specimens and results that are only partially described, and the text, by itself, leaves many questions unanswered.

Faraday wrote 1,160 numbered entries on his gold research, roughly 250 printed pages in the transcribed version (Martin, 1936). These are dated from February 2, 1856, to December 20, 1856. In an earlier account (Tweney, Mears, Gibby, Spitzmüller, & Sun, 2002), we noted that the distribution of entries was roughly bimodal, with the greatest density of entries occurring at the beginning of the series and toward the end. The very first entries appear to be summaries of previous notes. These first entries also include several dozen entries in which Faraday outlined possible experiments, much as he had earlier kept an "idea book" in which to record possible studies (Tweney & Gooding, 1991). Faraday's (1857) published article on gold was submitted to the Royal Society on November 15, 1856, and read before the Society on February 15, 1857, just before it appeared in *Philosophical Transactions,* the society's official journal and one of the premier scientific journals in the world at that time. Not surprisingly, the character of the entries in the second peak suggests that Faraday was "mopping up" prior to ending the research: conducting some necessary control experiments, trying again to resolve some inconsistencies, replicating key preparations, and so on.

As noted at the beginning of the chapter, our program of research was initiated by an unexpected discovery: more than 600 surviving microscope slides and other specimens made by Faraday as part of the 1856 research. The specimens, mostly gold films mounted directly on ordinary 1" × 3" glass microscope slides, were "hidden in plain sight," on display in the museum area at the Royal Institution in London (Tweney, 2002). Examination of the slides revealed that each slide was numbered by Faraday, and each was indexed and referenced in Faraday's laboratory diary covering this work (Martin, 1936). Thus, we have nearly the complete set of metallic film specimens used by Faraday in 1856, as well as a few of his colloidal specimens.

Faraday's surviving colloidal specimens have long been noted, and there are at least five bottles of these on display in the museum area of the Royal Institution. One especially notable one has still (a century and a half later!) the characteristic pink color described by Faraday. The others are pale and nearly colorless. Four of the five bottles show a clear Faraday–Tyndall effect; that is, when a narrow beam of light is passed through the colloid, the light is scattered sideways, rather like a sunbeam through smoky air. This characteristic property marks each as a true colloid, and it is this property that suggested to Faraday their particulate nature.[3]

The surviving specimens fill in the missing dimension of Faraday's diary; those specimens and results that are not adequately described in the text itself can now be examined. Furthermore, by replicating his preparations and comparing them to the originals, we can gain even more insight into the processes Faraday used to create the specimens, and we can reproduce Faraday's often-destructive manipulations (heating the slides, burnishing them, treating them with corrosive substances, etc.). As one of our first efforts, we prepared our own colloids (using modern methods, but obtaining results identical to Faraday's). In this chapter we focus on our efforts to re-create two kinds of experiments carried out near the beginning of Faraday's work on gold: (a) the precipitation of gold from solution and, (b) the "deflagration" of gold wire, that is, exploding a gold wire using sudden surges of electric current. In a later publication (Tweney, in preparation), we plan to discuss our efforts to produce gold colloids and thin metallic films of gold using Faraday's favored technique, reduction by phosphorous.

REPLICATING PRECIPITATES

Faraday's first diary entry on gold (February 2, 1856) describes a visit the week prior to Warren De la Rue's home, to examine gold leaf through his

[3]It is interesting that an examination using a parallel beam of light revealed that one of the "colloids" is actually a solution of an unknown substance. Although it has a color nearly that of a gold colloid, it does not manifest a Faraday–Tyndall effect, unlike the true colloids.

friend's better microscope. By the following week, De la Rue had made some especially thin films using phosphorous reduction, and on February 2, Faraday examined these in his laboratory. Three days later, he began his first active work on gold in his own laboratory. It is surprising that, at first sight, he began by making some precipitated gold. Because the precipitation reaction of gold was long familiar by 1856, Faraday could learn nothing new here, and the text of the diary alone does not indicate why he initiated his gold research with such a common procedure. In fact, the experiments with precipitates were far from trivial, as we learned when we replicated his procedure. We were thereby able to detect a "confusion" that served a heuristic role in the important step of arguing that the colors of gold are due to particles interacting with light.[4]

When a reducing agent is added to a solution of a gold salt, metallic gold (Au) is precipitated as a solid; in modern terms, the positively charged gold ions combine with electrons from the reducing agent, forming uncharged particles of elemental gold. These aggregate together, forming larger particles, which then are prone to settle out of the fluid medium. Such simple chemical reactions are familiar to every beginning chemistry student, and we accordingly thought that replicating these first experiments of Faraday would be a simple exercise for our laboratory group, if not a particularly revealing one. However, the chemistry of gold in solution is more complex than we had anticipated, and we thereby experienced our first "confusion"!

Faraday's diary entry stated only that he "prepared a standard weak solution of Gold" and a "standard solution of proto sulphate of Iron ... consist-[ing] of 1 vol. saturated solution at 54° F. plus 2 vols. Water, and a little sulphuric acid to keep all in solution during the changes" (Entries 14291 and 14292, February 15, 1856). In modern terms, "proto sulphate of iron" is *ferrous sulfate,* and the fact that it was saturated allowed us to reproduce the exact substance used by Faraday. However, gold salts are complex,[5] and Faraday stated only that he used a "standard weak solution of Gold." What was this? In the end, we used pure gold wire (0.025 in. [0.06 cm] diameter,

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[4]Williams (1965) suggested that Faraday's work on gold in 1856 was an indication of his "declining powers," perhaps due to aging or to the many toxic exposures he experienced over the years. At first, the seeming aimlessness of the precipitation experiments appears to support the claim, but our analysis suggests that the experiments were not at all aimless. Our results have failed to reveal anything deficient about the mental powers displayed by Faraday during this research.

[5]Gold chlorides exist in solution as $[AuCl_4]^-$ ions and various hydrolyzed ions as well. These more complex species and reactions were not known to Faraday. As we discovered, however, and as Faraday must have known, the complexity of the reactions is reflected in a very complex phenomenology: Gold salts are unstable, dissolve in water to varying degrees, leave varying undissolved residues, and manifest a variety of colors—all of which was extremely confusing in our first efforts. See Puddephatt (1978) for further details on the chemistry, and Tweney et al. (2002) for a more detailed account of our procedures.

99.99%), which we dissolved completely in Aqua Regia, a 3:1 combination of hydrochloric acid and nitric acid. A saturated solution of the reducing agent was then prepared by dissolving crystalline ferrous sulfate ($FeSO_4$) in heated water. When cooled, three drops of ferrous sulfate solution were added to 5 ml of the dissolved gold solution. At first, no reaction was apparent, but on the following day a yellow–orange residue of metallic gold had settled at the bottom of the experiment tube. This could be redispersed by shaking, although it would gradually settle again over the course of half an hour or so. After shaking, the fluid had a muddy, brownish-yellow appearance, in which individual particles could be seen moving about, some glinting with the familiar bright metallic color of gold.

With the advantage of modern knowledge, we of course knew that the precipitate was physically similar to the colloid, except that the particles in the precipitate were much larger than those in the colloid. Yet, when placed side by side, the precipitate looked very different than the colloid we had prepared earlier; the colloid was a clear fluid, red in color but transparent, and very unlike the nearly opaque precipitate. In fact, except for the fact that the overall color was red, rather than the yellow–gold of the dissolved gold wire solution, the colloid more nearly resembled the solution than it did the precipitate.

The relative appearance of these three changed, however, when directional lighting was passed through the fluids, as shown in Fig. 7.1. In the figure, a parallel beam of light produced by a fiberoptic illuminator (entering from the left) is being directed through our prepared gold colloid, a solution of gold chloride, and the precipitated gold preparation, respectively. (Color images of these three can be accessed at http://personal.bgsu.edu/~tweney.) The precipitate was shaken just before the photograph was taken. Note that the colloid scatters light to the side, illuminating the path of the beam. This is the Faraday–Tyndall effect, the effect that first suggested to Faraday that colloids were particulate. By contrast, the solution does not scatter light, except for some small reflections from the sides of the glass test tube being visible in the photograph, and the precipitate scatters light like the colloid.

The important point here is that the colloid and the precipitate resemble each other most closely under *transmitted* light conditions, whereas the colloid and the solution most resemble each other under *reflected* (ambient) light conditions. There is no record in the diary of Faraday placing these three in one context (as we did in Fig. 7.1), but we now believe that he was attending these differences very carefully; they later constituted part of the basis for his conclusion that the colloids were in fact metallic particles of gold. In the light of our own experiments, we believe that the sequence of Faraday's entries in his diary can be reconstructed in the following way.

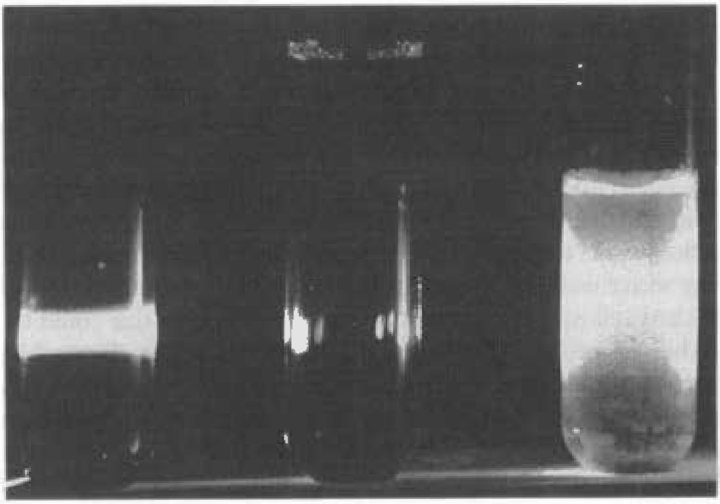

FIG. 7.1. Colloid, solution and precipitate.

Recall that Faraday had visited his friend Warren De la Rue the week prior to February 2 (the date of his first diary entry), to examine some gold leaf through the microscope. Faraday recorded this (Entry #14243, February 2, 1856), indicating that, also on February 2, he received the thin gold films prepared by De la Rue, who had used phosphorous to reduce the gold (Faraday later used this technique himself). On February 6 (1 day after preparing the precipitates), Faraday used a careful optical method to examine the precipitates and recorded that, in the evening, he went to De la Rue's again and observed how the thin gold films were made. In his description of De la Rue's method, Faraday recorded an apparently incidental observation, made during the cleaning up of the glassware used to make the films; "A very fine red fluid is obtained [from] the mere washing" (Entry #14321). With hindsight, we know that this was a colloid, but it is significant that Faraday noticed it in this context.[6] In fact, Faraday saved the fluid, returning to it 2 weeks later, on February 18 (Entry #14437), after his experiments with precipitates and his first examinations of thin films. At that point, he was able to ask of this red fluid: "The question is, is it [i.e., the gold] in the same state as whilst apparently *dissolved* in the fluid" (Entry #14437). During the intervening period, Faraday had referred to the red fluid using two terms interchangeably: *fluid* and *solution*. Only later was he sure that the red fluid was not a solution. It is clear, however, that the possibility had suggested itself very early.

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[6]Parkes (1822, p. 500) described a similar experiment that may have resulted in a gold colloid. He dissolved gold in Aqua Regia, evaporated it to dryness, and then dissolved the crystals in water. By adding a salt, he observed a faint violet hue that changed to a deep purple. This may have been colloidal gold, although he did not provide an account of the nature of the reaction or the resulting fluid.

The sequence of Faraday's ideas must then have been the following. He first compared thin films (which he suspected to be gold in a *continuous* state) to the precipitates, which he knew to be made of *discrete particles*. Because gold film (i.e., gold in a continuous state) changes appearance in transmitted light and reflected light, he developed an "optical method" for examining precipitates under the same two conditions, namely, "reflected" light (ambient) and "transmitted" light (passing a beam through the substance). We enclose these terms in quotation marks to suggest that each is slightly different from the analogous procedure with thin films. To prepare the precipitates, he must have had before him the clear yellow–gold solution of gold chloride, and this could have suggested, while he was at De la Rue's, a question about why the washing fluids were clear. The substances used to produce that clear red solution (phosphorous, carbon disulfide, and a gold chloride solution) could only have produced metallic gold. But why did it look like a solution? Resolving this confusion is then the reason why he examined the red fluids more closely—using both "transmitted" and "reflected" light, just as he had done with the precipitates.

According to Hanne Andersen (2002), taxonomic change in scientific concepts is best construed as model-based change, because existing "family resemblance" and structural accounts are too limited when seen in the context of change in actual scientific concepts. In the present case, Faraday eventually saw the "red fluid" as gold in a "divisible state," like the precipitates. Note that stating the reorganization in this manner is, in fact, model based, but it describes the end-product of his thinking and experimentation. At the earliest stages, the ones we are concerned with, Faraday was not in a position to make a model-based claim, because the "model" was still too vague; it was really based only on a set of unusual appearances. The text of the diary alone does not, of course, reveal these, because they were obvious, visually, to Faraday—and they became obvious to us only when present as the result of our own "makings." At the phenomenological level, the term *confusions* is thus a better description of what he was faced with (see Cavicchi, 1997). Reorganization of the appearances, not reorganization of the taxonomy of model-based classification, is the issue.

The red fluids provided Faraday with a first important clue that his inquiry into the color of gold in its various states was going to have to focus on the influence of particulate gold on light. Earlier (Entry 14279, February 2, 1856), he had speculated that the color of metallic gold could perhaps be a manifestation of particulate effects, but the discovery of the colloidal state of gold was a strong clue that perhaps size of particle was an important variable. Because the colloids could be seen to be particulate only under certain lighting conditions, were there other conditions that would suggest that even apparently continuous gold (e.g., as in mounted gold leaf) was also particulate? This suggests why Faraday took up the determination of the optical properties of gold in a state in which it was clearly particulate: "Would a metallic surface made up of particles, like de la Rue's films, reflect light so as to give colours of thin plates? Perhaps may find here a test for continuity" (Entry 14407, February 12, 1856).

REPLICATING DEFLAGRATIONS

Faraday did not immediately take up the question of continuity that he had posed on February 12. Instead, his work with the thin films produced chemically using De la Rue's method occupied him for almost 2 months. Then, quite without warning in the ongoing text, he suddenly recorded using a Grove's battery (Grove, 1839) as a source of current to explode gold wire.

Like all metals, gold has a melting point (1,064°C) and a boiling point (2,856°C). However, the liquid state that stands between these two values can be bypassed: If gold is heated quickly enough, it can be vaporized directly, a process known as *deflagration*. One method of deflagration involves vaporizing material with heat energy generated by a rapid current of electricity. The experiment is seemingly simple; in Faraday's words:

> Provided some gold terminals and a voltaic battery of Grove's plates, then brought the terminals suddenly together and separate, so as to have a momentary deflagration. Did this upon and between glass plates, so that the deflagrated gold should be deposited on the glass in films more or less graduated. (Entry 14664, April 9, 1856)

Some of the results (Slides 236, 238, and 239, noted in the same diary entry) are shown in Fig. 7.2. Faraday exposed each slide to multiple deflagrations, each of which has produced a spot or arc of metallic gold deposit.

Similar means of deflagrating gold were well known by 1856; Charles Wilkinson (1804), for example, described a series of experiments in which Leyden jars were charged by a frictional machine and then discharged across a strand of metallic wire (see Fig. 7.3).

Others had conducted similar experiments (e.g., Bostock, 1818), and there was a consensus that the films of metal deposited on a card or a glass plate held near the deflagration represented particulate matter. Wilkinson (1804) believed that the particles consisted of oxides of the metal, but Faraday was suspicious of this claim in the case of gold. Because gold will not burn in air or oxygen, it is likely that only pure gold particles are deposited when gold wire is deflagrated. Later, Faraday was able to verify this belief; when gold is deflagrated, only gold is deposited on a nearby slide. In any case, he did accept that the deposits were particles of gold.

At first, it was not clear what motivated Faraday's choice of a battery of Grove's cells. In fact, this appears to be a rather odd choice; a Grove's cell is made using *platinum* plates in a zinc cell. The special virtue of such a cell, which would have been extremely expensive in Faraday's time, as in ours, is

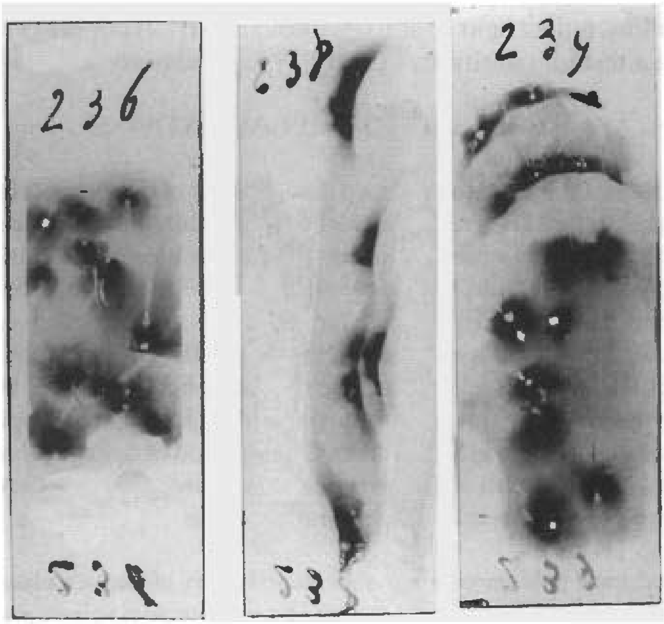

FIG. 7.2. Three deflagrated slides (exploded gold wire) made by Faraday.

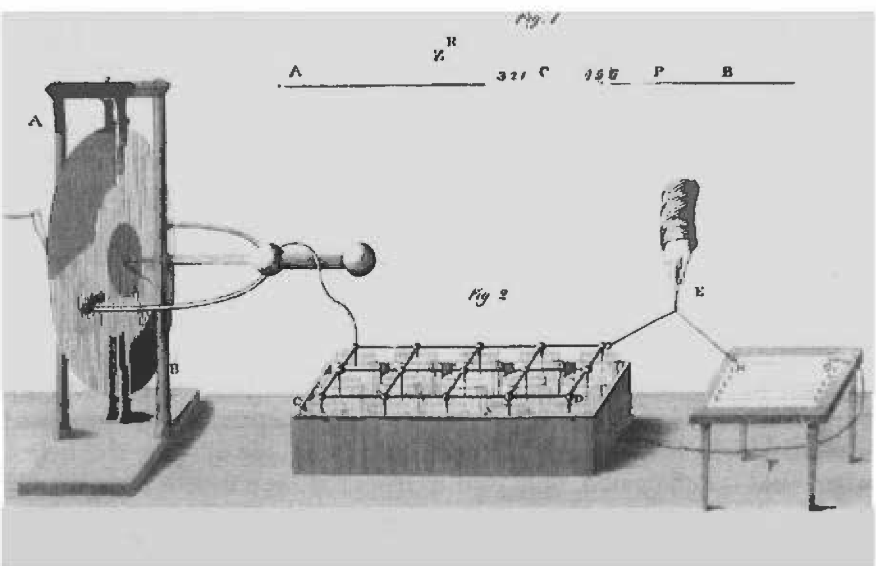

FIG. 7.3. An early apparatus for exploding gold wires (Wilkinson, 1804, Vol. 2, Plate 9).

its very low internal resistance (Bottone, 1902). Thus, the simplicity of Faraday's first deflagrations may be only apparent. Perhaps he had also tried earlier and failed, until he could provide a suitable current source. Furthermore, he was not entirely happy with the results obtained with the Grove's cells. Even Wilkinson (1804) had indicated that wires could be exploded along their entire length. Perhaps the more elaborate setup shown in Fig. 7.3 was necessary?

How can a current of electricity be generated swiftly enough to bring the temperature of the metal to the point that a portion or the entirety of the wire explosively vaporizes? Because current is a function of both time and voltage, one must have a circuit that not only can be rapidly closed but also has very low resistance. When we set out to replicate this process, our initial attempts used various combinations of automotive storage batteries as a current source and a knife switch that could be rapidly closed. We were able to melt wires with this setup, but not deflagrate them, probably because the internal resistance of such batteries is relatively high (which meant, in turn, that the heating effects on the wire were too slow). We eventually had to use a bank of capacitors as a source of low-resistance current. Two heavy brass mounting brackets were therefore attached to a parallel bank of seven capacitors. A direct-current generator with a maximum output of 250 volts charged the capacitors, and a heavy utility knife switch was used to open and close the circuit. All the connections had to be made with heavy gauge copper wire to minimize the resistance of the circuit. After test trials with copper wire, pure gold wire (0.025 in. [0.64 cm] diameter, 99.99%) was mounted in the 2.5-cm gap between the brackets, and a glass microscope slide was placed beneath the wire. We calculated that when the circuit was closed, an amount of energy equivalent to that used by a 100-watt light bulb in 0.3 seconds would pass nearly instantly between the brackets containing the gold wire.

When the circuit was closed, the specimen wires exploded with a flash of light and a sharp cracking noise. Enough energy was generated by the deflagration to propel bits of unvaporized metal to distances of several feet. Most important, there was a small cloud of vaporized gold, manifested as a deposit on the glass slide. We repeated this procedure several times with gold wire, obtaining similar results each time. In each case, one example of which is shown in Fig. 7.4, the wire was vaporized only at certain points along the wire before the circuit was broken and current was no longer able to pass through the wire. This pattern was similar to that found on some of the first deflagrated slides produced by Faraday (note that Fig. 7.4 shows only one deflagrated spot, at greater magnification than the image of Faraday's slides in Fig. 7.2).

In Faraday's first deflagrations, as in ours, only portions of the wire were vaporized, probably because of the lower voltage potentials pro-

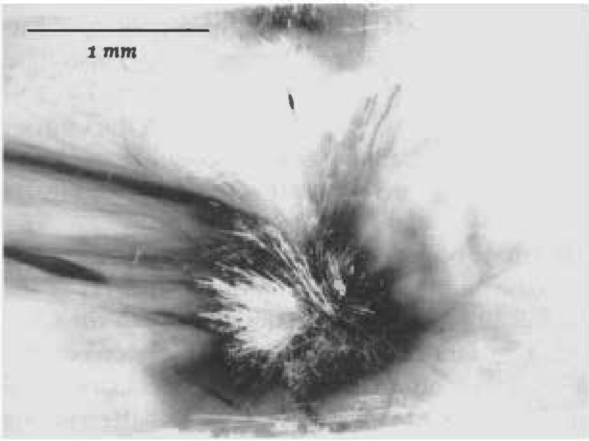

FIG. 7.4. Blue and red deposit of exploded gold (our replication).

duced by the Grove's batteries; one cell produces 1.94 volts, so a battery of 10 such cells could not have produced more than 20 volts. This accounts for the fact that Faraday later used a setup like that shown in Fig. 7.3, a battery of Leyden jars, charged by a frictional electricity machine. Such a battery could store thousands of volts of potential. Using such an apparatus, Faraday, like Wilkinson (1804), was able to vaporize wires along their entire length (Entry 14699, April 14, 1856); see the example slide on the right in Fig. 7.2. Also, the result was immediate and convincing: "Here we obtain the general tints obtained by other processes [i.e., the manipulations of chemically produced gold films]—supporting the conclusion that these other process[es] yield divided gold" (Entry 14699, April 14, 1856). The next day, a careful optical examination in daylight confirmed his belief: "Every part of the [deflagrated] gold film reflected yellow light .... The transmitted light was very various, from a rose on the outside by a green or green violet to ruby at the place of the wire and heat" (Entry 14708, April 15, 1856).

Color images of some of these slides, numbered by Faraday from 319 to 340, can be seen at the following Web site: http://personal.bgsu.edu/~tweney, along with the corresponding images of our own slides. We were, of course, pleased by the resemblance between his slides and ours, but the more important comparison is between those slides Faraday prepared by deflagration and those he prepared by chemical means. Both are metallic and shiny, yellow by reflected light and green or blue or ruby by transmitted light. Because Faraday was convinced the deflagrations are particulate, what evidence could he still invoke to argue that the chemical films are continuous? At this point, we can see that his quest for continuity is breaking apart. His "confusions" have passed into the realm of true anomalies!

DISCUSSION

Many cognitive accounts have emphasized search through a problem space as the fundamental feature of scientific thinking (Klahr & Simon, 1999). For example, Kulkarni and Simon (1988) successfully modeled the problem-solving activities of Hans Krebs using such an approach, basing their model on the historical analysis of Krebs's diaries provided by Holmes (1980), whereas Tweney and Hoffner (1987) examined the problem space of Faraday's 1831 diary account of his discovery of electromagnetic induction. Later work—for example, that by David Klahr and his students (e.g., Klahr, 2000)—suggested separating problem spaces into those specific to experimentation design and those centered on hypothesis search, and there have been proposals that have extended this idea beyond two spaces to as many as four (e.g., Schunn & Klahr, 1995).

Other analyses have concentrated on the heuristics used in scientific research. Thus, Tweney and Hoffner's (1987) analysis of Faraday's 1831 research demonstrated how Faraday's experimental strategy involved a two-step process. First, he used a narrow search to find evidence confirming newly developed ideas, without paying particular attention to potentially disconfirming evidence. Second, in later steps, the disconfirming evidence received more of Faraday's attention; at this stage, he explicitly attempted to disconfirm ideas generated and supported by his initial searches for confirmatory evidence. Earlier work in our laboratory had found similar patterns of heuristic use among some student participants and working scientists attempting to discover the rules that governed an "artificial universe" (Mynatt, Doherty, & Tweney, 1978; Tweney & Doherty, 1983). A similar "confirm early–disconfirm late" strategy was observed in studies of individuals attempting to discover the function of an obscure control key on a robot toy (Klahr, 2000) and by Dunbar (1995) in an *in vivo* study of laboratory molecular biologists.

Despite the success of the account of scientific thinking as problem space search, and of the investigation of the heuristics of scientific discovery, other aspects of scientific thinking seem resistant to such description (see, e.g., Kurz & Tweney, 1998). David Gooding (1990), for instance, explored in great detail the process of Faraday's 1821 work on the principles of electromagnetic rotations. For Gooding, the discovery of the electromagnetic rotations, and their consolidation within a simple theoretical scheme, was part of a dynamic "eye–hand–brain interaction." According to Gooding, Faraday had to construct the meaning underlying experimental results that would have otherwise appeared only chaotic. Similarly, Elizabeth Cavicchi (1997) replicated some of Faraday's experimental work on diamagnetism and found that Faraday's discovery process depended on attention to both anomalies and ambiguities, which then led to successively more refined exploration. In other words, Faraday's developing understandings were part of a critical pattern-finding stage of the discovery process. Cavicchi argued that Faraday's experimentation proceeded "not by progressively refining explanations, but by exposing previously unnoticed ambiguities in the phenomena, and uncertainties in interpretation. This exposing deepens the space of [his] confusions" (p. 876). For Cavicchi, these "confusions," like Gooding's "construals," are a crucial aspect of the pattern-finding involved in discovery; both Gooding and Cavicchi were attempting to describe the construction of a phenomenology, to sort out a mass of experience and sensation into more or less relevant domains, and to formulate what is strange or puzzling in the domain. Cavicchi was thus able to show that Faraday's "confusions" resembled those of a student exploring the relationships between bar magnets and iron needles. One implication of her account is that the cognitive analysis of anomalous results has so far been incomplete, because most such analyses focus on later stages of inquiry in which already-sophisticated expectations are violated by phenomena, a process that contrasts markedly with the wholly unexpected "confusions" that characterize the pattern-finding stages of inquiry.

The use of anomalous data has received much attention in the psychology of science.[7] For example, Trickett, Trafton, Schunn, and Harrison (2001) showed that astrophysicists presented with optical and radio data of ring galaxies paid attention to anomalies in the data and used such anomalies to search for analogies with other features of the data. Dunbar (1995) paid special attention to the use of analogies to explore anomalous findings, in the analysis of protocols gathered during his *in vivo* study of molecular biologists. He found that the relative "nearness" or "farness" of the analogies was related to the relative success of a given laboratory. Our account of Faraday's gold research is fully consistent with all of these other accounts. Thus, uses of analogy are present, imagery is extensive, and much of the record could be interpreted as search through a problem space (see, e.g., Dunbar, 2001; Gentner et al., 2001; Gorman, 1992; Langley & Jones, 1988; Nersessian, 1999; Tweney, 2001). Yet none of these processes taken singly can fully capture the way in which Faraday interacted with the materials and objects of his laboratory, and thus none of these, taken singly, can fully account for his creative discovery processes. His "confusions" must be part of a complete account.

An earlier examination of two of Faraday's papers—one on acoustic vibrations and one on optical illusions of motion—explored the development of a series of representations in Faraday's work and suggested

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[7]See also the accounts of anomaly finding and anomaly resolution provided by Darden (1992). By contrast, Nersessian's (1999) account of the role of generic abstraction in anomaly resolution is closer to the level we are emphasizing here.

that his constructive perceptual processes imply a continuum of developing representative explicitness (Ippolito & Tweney, 1995; Tweney, 1992b). Beginning with what appeared to be little more than the perceptual rehearsal of remembered events, Faraday used these and his first experimental efforts to construct "inceptual" representations, that is, representations that abstracted away potentially irrelevant features, with an effort to "see" what the results would look like. Only toward the end could he be said to have developed a mental model of the phenomena. Faraday clearly appeared to be using an eye–hand–mind dynamic in constructing new spaces for both thought and action, much as he had done earlier in his discovery of electromagnetic rotations in 1821 (Gooding, 1990). Similarly, Nersessian (1999) argued that Maxwell used analogies and imagery in a process of generic abstraction, a process by which intangible and vague "hunches" became explicit mental models.

Accepting such a view of the nature of scientific discovery requires that one focus on the process by which meaning is *made* (Gooding, 1990). It is just here that the "situatedness" of cognition is most manifest, precisely because it is here that a representational system is under construction. In the case of experimental research in particular, as in the case of technological development, a full understanding of the process requires close attention to the cognitive and epistemic artifacts in the arena of interest. Thus, whether in the mind of one investigator or of a team of investigators, there is a need to understand what Nersessian, Newstetter, Kurz-Milcke, and Davies (2002) referred to as the "biography of the object." The artifacts of scientific cognition are themselves contingent historical objects with both agency and a developmental past and, just as Faraday's slides shape the representation of the properties of gold, so too do our replications of Faraday's slides shape the representation of his discoveries.

In seeking to understand Faraday's achievements at the remove of a century and a half, it should be clear from our presentation that his epistemic artifacts and the practices of his experimentation are required elements in a full account. The term *epistemic artifact* is deliberately chosen here; we mean to imply that the artifacts (slides, colloids, precipitates) were made by Faraday precisely because they can answer a question—or, for that matter, ask a question (Tweney, 2002). Like the related concept of a *cognitive* artifact, an artifact that must be regarded as conducting an externalized computation (Zhang & Norman, 1994), an *epistemic* artifact externalizes cognition, but it also serves as a source of new knowledge. In a real sense, although made by Faraday, the specimens become agents in his inquiry (see also Rheinberger, 1997).

Our replications of Faraday's work contribute to understanding the processes by which the vague becomes concrete—by which, to use

Gooding's (1990) terms, *construals* become *concepts*, which become, eventually, public demonstrations. "Phenomena are made visible and public through the *invention* [italics added] of observational practices." (Gooding, 1990, p. 214). The phenomena by themselves are without meaning; the experimenter sets the stage, first to enable his or her understanding and then so that others may participate. In all of Faraday's research one can see a determination to produce phenomena of such clarity that the explanations of the phenomena would be transparent to his audiences: "Seeing was believing" in a deep sense for him (see also Fisher, 2001) and was the ultimate criterion for the authority of a claim.

What Faraday "saw" in looking at his slides, precipitates, or colloids is private and varies in permanence and in the ease with which it can become public. The permanence of the slides, and the relative permanence of some of the colloids was, in fact, the initiating cause of our studies, whereas the impermanence of his precipitates in effect forced us to replicate them in order to see what he saw. Of course, "seeing" in the context of scientific discovery is not simple; without the context created by all three kinds of preparations—slides, colloids, and precipitates—and the experience of our own experimental practices, we could not have "seen" the artifacts in the proper fashion (even granting, of course, that we can never see them exactly as Faraday saw them). It thus makes sense to speak of Faraday as *negotiating* what he sees with the artifacts and of our seeing as being also a negotiation—with Faraday's text, his artifacts, and our own replicated specimens and activities. The term *negotiation* implies more than one active agent, of course, and that makes the metaphor even more apt. Faraday negotiated with "nature," in a sense, but his eventual audience is part of the negotiation as well, just as our potential audience is part of the negotiation we undertook in this project. The distinction between the private and the public is a blurred distinction indeed![8]

Just as Holmes (1998) suggested that the public side of science and the private side of science, as revealed in laboratory notebooks, were complementary, so also do we believe that the material side of science complements the private (mental) side. It is not right, however, to specify these three domains—the public, the private, and the material—as if they were separate and independent. Each depends on the others, and the classification is truly a nominal one. In practice, science is a system of activity, and the unit of

[8]Gooding (1990) noted an apt comparison between such an account of scientific discovery and a series of shadow box experiments conducted by Howard Gruber (1990). By placing participants into an epistemic context in which two individuals had to share descriptions of the same object producing two very different shadows, Gruber was able to study the negotiation process directly in a social context. There is a striking parallel between Gruber's account and our account of Faraday making sense out of his colloids as particulate in nature.

study ought to be not the individual scientist alone, not the social network alone, and not the laboratory (instruments and objects) alone but the individual scientist *in* the laboratory and *in* the social context.

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