Enlightenment: Error & Experiment
Henry Cavendish's Electrical Researches

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(ABSTRACT)

I have attempted two major tasks in this thesis. First, I argue that Deborah Mayo's Error Statistical epistemology makes an excellent tool for historical research into experimental episodes. This is because it focuses the historian's eye on the nitty gritty details of experimental arguments, particularly on the generation and manipulation of data. Moreover, her hierarchy of models provides an excellent organizing tool for disentangling complex experimental narratives. I illustrate the fruitfulness of this method by contrasting John Dorling's and Ronald Laymon's summaries of Cavendish's Great Globe experiment with my own account. Second, and perhaps less successfully, I have used her concept of "arguing from error" along with her attendant hierarchy of models and severity criterion to make claims for the procedural objectivity of Cavendish's experimental tests of an inverse square force law for describing electrical attraction and repulsion. Simultaneously, I confronted Harry Collins' experimenters' regresses and Pickering's view of experimenters' "tinkering" (in his mangle of practice) and show that neither is either a necessary part of experimental practice nor holds for Cavendish's experiments.

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This thesis is dedicated to Lisa LeMacks and Betty Speedy--two friends who had faith in me when I had none and convinced me to go to graduate school.

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Introduction

In this introduction, I explain my reasons for perusing my thesis topic. I also discuss concepts of "objectivity" and how I will use them. Lastly, I cover my research methods.

In an introductory physics class I took a few years ago, I was fascinated to learn that though an entire scientific sub-field called electromagnetism exists, scientists still do not know exactly what electricity is! Yet every home appliance I owned, especially my beloved TV set, not only used electricity, but were designed based on scientific knowledge of electrical principles. This puzzle aroused my curiosity. A year later, I rushed off to Virginia Tech's STS program to find some answers. I found myself increasingly bewildered until I became familiar with the work of Deborah Mayo.

Mayo argues, in *Error and the Growth of Experimental Knowledge,* that an adequate epistemology of experiment can be built upon the notion that scientists not only learn from error but the success of experimental practices resides in scientists' abilities to develop strategies and arguments revolving around error albeit in a piecemeal fashion. It is experimenters, ability to manipulate, model, and use a barrage of low-level or local techniques to discover, amplify, distinguish and eradicate errors that gives a robustness to their results. That is, they can argue that were an error present, it would have been discovered. Moreover, these "error methods" are or can be made independent of the larger theory or hypothesis being tested. Of course, her entire epistemology is far more subtle and sophisticated but that is the gist of it. Furthermore, her view of objectivity--grounded in and by low-level or local testing procedures--offered a solution to my puzzlement about electricity.

In this thesis, I focus on the objectivity of experimental procedures and results, a focus determined in part by my own approach to objectivity. Allan Megill, in *Rethinking Objectivity*, outlines four different approaches to objectivity. The first is the absolute sense of objectivity. This concept equates objectivity with reality. "It [absolute objectivity] aspires to a knowledge so faithful to reality as to suffer no distortion, and toward which all inquirers of good will are destined to converge."[1] This view fits into what philosophers call a correspondence theory of truth. I am not championing this sort of objectivity for reasons that will become clearer later.

Nor do I champion the next two types of objectivity that he discusses. Disciplinary objectivity "no longer assumes a wholesale convergence and instead takes consensus among the members of particular research communities as its standard of objectivity."[2] I reject this sense of objectivity, for I do not feel a sense of objectivity deserving the name can be achieved by committee vote. The third sense of objectivity that MeGill defines is dialectical or interactional "which holds that objects are constituted *as* objects in the course of an interplay between subject and object; thus, unlike the absolute and disciplinary senses, the dialectical sense leaves room for the subjectivity of the knower."[3] This sense of objectivity comes from anthropology and is based on the fact that both ethnographers and the groups they study bias anthropological results with subjective expectations of one another. Pickering supports this view of objectivity as I discuss in Chapter 1.

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[1]MeGill, p. 1.
[2]ibid.
[3]ibid.

MeGill's fourth and final sense of objectivity reflects my views more closely than the preceding three concepts. Procedural objectivity "aims at the practice of an impersonal method of investigation or administration. Here, the exclusion of subjectivity prominent in both absolute and disciplinary objectivity is pursued in abstraction from the belief that truth or justice will actually be attained thereby."[4] I will argue, in agreement with Mayo, that objectivity resides in experimental testing procedures. I discuss my basis for belief in this sense of objectivity in Chapter 1 when I discuss Mayo's epistemology in more detail. To indicate what I mean, without giving away the entire plot of my thesis, I will return to my initial confusion about the inscrutable ontological status of electricity in science and its compelling reality in my living room.

Scientists have considered electricity to be a fluid, a charge carried by a particle, and even a field. No one seems to know exactly what electricity is in an absolute sense. What is amazing to me, besides my TV set, is through all these different theories and theory change, the law of electric force has remained basically unchanged. Electricity, like gravity, follows an inverse force law: q_1q_2/r^2 where q_1 and q_2 stand for charge and r represents distance. To compute the force of electrical attraction or repulsion between two charged objects, one plugs in the respective charges and divides by the distance between them squared. All three types of large-scale or "global" electrical theories predicted this simple law. Moreover, this law was experimentally proven under each of the three different theories. Jackson, the modern graduate student's bible on electricity, even includes a brief passage, with picture, of Henry Cavendish's eighteenth-century

[4]ibid.

experimental proof of this law![5] Global theories get discarded but some of their predictions, those that have survived experimental testing, have extraordinary tenacity.[6] This suggests the objectivity of at least some experimental knowledge. (Certainly, I would never claim "all" scientific knowledge is objective.)

Objectivity for me, taking off from Mayo's work, is grounded in testing procedures. A hypothesis is warranted to the extent that it has passed at least one, if not more, severe tests. Mayo's definition of severity reflects our common intuition of what "passing" a test means. First, in order to pass a severe test, a hypothesis must accord with or fit the evidence or results of an experiment. Second, and most importantly, the test must be so designed that if the hypothesis is false, then the test will, with high probability, discover this and reject the hypothesis. It should not be "easy" for the hypothesis to pass the test, if it is false or it really would not be much of a test. Severe tests are used to probe for ways one could be wrong, for errors that could cause one to pass an incorrect hypothesis or fail to pass a correct one. Because I am tying this idea of severe testing to objectivity, my notion of objectivity may be referred to as "procedural objectivity." I will develop and extend this view of objectivity throughout the rest of this thesis as I explore Henry Cavendish's electrical researches.

I have used Mayo's error statistical epistemology, especially her hierarchy of models and severity criteria, as my main research tool. It was no accident that I chose

[5]Jackson (1975) pp. 5-7.

[6]See Mayo, 1996; Hacking, 1983; Franklin, 1994; Pickering, 1995.

Cavendish for my research. He earned quite a reputation for his obsession with error.[7] I used Mayo's epistemology to provide structure to his narrative and am quite explicit where I am imposing this modern terminology upon him. However, the procedures and methods I describe are his as are the reasons for their implementation. I tried to let Cavendish make my case for me. My presentism becomes obvious when I use Mayo's epistemology to show how his methods helped him gain objectivity for his evidence.[8] I have, in general, merely made explicit that which is implicit in Cavendish's writings. I quote generously from him to back up my claims.

Mayo's epistemology allowed me to utilize what Michel Foucault would call an archeological approach to history.[9] In general my narrative is vertical, digging into Cavendish's experiments, rather than horizontally trying to tie them into a broad sweeping historical context. Of course, I am not totally ignorant of the larger historical background. Cavendish did not work in a total vacuum and I do not pretend that he did so. However, his own peculiarities make him an ideal case study for my narrow concentration.

As I have mentioned, Cavendish was eccentric. He devoted his entire personal life to his work, which in general he conducted in relative seclusion, and to the British Royal Society. Moreover, the only interest Cavendish took in the Society was scientific.

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[7]Here is a paradigmatic story of this obsession. Cavendish was pathologically shy, especially around women. He once ran into a female servant on the staircase in his house. To make sure this mistake never recurred, he had a separate staircase built for his use only. The moral of the story: find an error then take the steps to eradicate it.

[8]While Cavendish claimed his experiments showed the "truth" of his hypotheses, I am not so bold. Moreover, I believe his term "truth" and my term "objective" are not at odds with each other within the context of his work. I will discuss this in Chapter 4.

[9]However, this "deep trench" approach is all I've borrowed from him.

If the topic of conversation changed, Cavendish fled.[10] He was supported by his father and later became wealthy in his own right through inheritance. Thus, he did not need outside employment nor patrons to support his research. One of the most difficult problems faced by researchers in science and technology studies rests in sorting out the causal factors that convinced an experimenter or group of experimenters to champion a particular experimental inference. Were social factors like tenure, publishing opportunities, or financial gain at work? Was the inference warranted by the experimental evidence offered? That is, was the experimental evidence *itself* warranted or manipulated to support some larger personal or social gain? Cavendish's aloof shyness and lack of concern about publishing suggest the possibility that factors such as those listed above influenced his work or colored his belief in the rightness or objectivity of his own methodology were minimal. He insulated himself from worldly cares, leaving his methods bare.

But doesn't that make his own work an exception, something outside of normal scientific procedures? I don't believe so, since later experimenters, with little to gain, have praised his methods. Philosophers of science have looked to his famous "Great Globe" experiment not only to justify such scientific praise but to offer their own various interpretations about the strength or weakness of this experiment, depending on their philosophical orientation. I am no different. However, I do not add any assumptions into his work whether they are implicit continuity assumptions, like Ronald Laymon's, or subjective degrees of belief, like Jon Dorling's. I do not assign assumptions to

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[10]Crowther (1962, pp. 308-317). All the biographies in my bibliography support this view of Cavendish's personality.

Cavendish, he made enough of his own to keep us both busy. Which brings me full circle back to my research strategy. Mayo's epistemology is such that where Cavendish doesn't seem to fit, he simply doesn't. And in her view that can be just as instructive and important a bit of information as where he does fit in.[11]

[11]This attitude of Mayo's is the foundation of my objectivity. Whether Cavendish exemplifies her epistemology or not, I'll still get a thesis out of it.

Chapter 1: Objectivity: Problems and Solutions

In this chapter, I provide a brief overview of problems raised and solutions offered regarding the objectivity of experimental evidence in science. I also suggest that eighteenth-century experimenters confronted and articulated problems with objectivity and that this led experimenters to develop a sophisticated experimental epistemology which can be seen in the work of Henry Cavendish and others.

Many people who challenge the objectivity of science, whether of scientific observations, methods or hypotheses, base their challenges on the fact that there are erroneous observations, methods and hypotheses. Their arguments, in various guises, all share a common theme: Scientists knowingly or unknowingly make errors; therefore, not only can science not be objective, but objectivity as a goal cannot even be seriously entertained. This sort of argument is fundamentally wrong. It is wrong because to claim to pursue objectivity as a standard *assumes* the very possibility that one can be wrong, that one can and will make mistakes. Without the threat of error, there would be no need for a standard of objectivity.

Israel Scheffler argues that objectivity is a scientific standard. This standard holds scientists responsible for the claims they make: "In assertion he is not simply expressing himself but making a claim; he is trying to meet independent standards, to satisfy factual requirements whose fulfillment cannot be guaranteed in advance."[12] Moreover, "[a]ssertions that purport to be scientific are, in sum, held subject to control by reference to independent checks."[13] Scientific assertions are not "guaranteed in advance," that is, science is not governed by an algorithm. However, the bearing of experimental evidence

[12]Scheffler (1982, p. 1).
[13]ibid. p. 2.

on scientific claims is subject to scrunity based on control and independent checks. In sum, the *minimum* requirements for objectivity are twofold: acknowledging that one can be wrong and systematically searching for methods to control the errors that can make one wrong. This systematic search for experimental control identifies what Kuhn calls normal science and Mayo calls normal testing.[14] Both implicit and explicit in science is the view that a hypothesis is most strongly warranted when a strong experimental argument in its support has been given.

That an experiment or test of a hypothesis is truly a test of it in the true spirit of what is commonly implied by the term 'test' revolves around showing that a particular test of a hypothesis is robust against mistakes, errors, biases and similiar criticisms. A good test is one that will have a high probability of detecting the falsity of the hypothesis being tested, if it is false; and, will with low probability, fail to pass a true hypothesis, if it is true. Thus, the objectivity of science is grounded in procedural objectivity.[15] Moreover, the onus is on the experimenter to demonstrate that a particular test is a good one (e.g., hard, difficult, severe) and for his or her peers to judge whether or not that is the case. Thus, error and the search for errors play a large role in giving experimental arguments and making claims for objectivity in science. I will devote the first part of this chapter to exploring a representative sample of both the attacks on and the defenses of experimental objectivity from the past twenty years. In the last part of this chapter, I will argue that problems and solutions with the objectivity of experimental

[14]Kuhn (1962); Mayo (1996).

[15]See the beginning of my Introduction for a brief discussion of the four types of objectivity as outlined by MeGill (1994).

evidence have been discussed, of course, for much longer in scientific circles. First, however, I will briefly set-up the conceptual background from which the present debate evolved and identify three specific challenges to objectivity around which much of it continues to revolve.

(1.1) Contexts & Challenges: The Three Foes of Objectivity

Hans Reichenbach's distinction between the context of discovery and the context of justification in science, articulated in *Experience and Prediction*, set the general tone of the objectivity debate in the philosophy of science for decades.[16] Reichenbach's two contexts putatively represented two very different sides of science. The creative and subjective side of inventing theories fell under the rubric of the context of discovery. The context of justification was occupied by the more sober and disciplined experimental side of science with its presumed objective methods and procedures for testing inferences. While no one has ever considered experimental methods and results to be totally un-problematic or "given," experiments were considered to form a real link between scientific theories and an external objective reality. One job for proponents of Reichenbach's distinction was to build bridges between the two contexts. Therefore, philosophers[17] trying to justify science and scientific knowledge as rational and/or objective typically pursued this goal by developing a method or logic for relating experimental evidence to scientific hypotheses or theories.[18]

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[16]Reichenbach (1938). In their introduction to *The Philosophy of Science*, Boyd, Gasper and Trout maintain that Reichenbach's distinction was assumed by the Positivists.

[17]These philosophers include A.J. Ayer, Rudolf Carnap, Carl Hempel, and Karl Popper.

[18]Mayo calls these accounts Evidential Relationship (ER) methods as they are more concerned with forming a relationship between evidence, once it is in hand, and hypotheses rather than concentrating on the gathering and modeling of the evidence itself. Accounts which concentrate on testing procedures or

The many and varied reasons for the failures of these individual philosophies are covered extensively in the literature[19] and so I leave these attempts behind with one generalization. When Thomas Kuhn proposed that experimental justification could not be distinguished from the context of the theory being justified another element entered into objectivity debates. The objectivity of the evidence itself and its production needed to be critically examined and addressed.[20]

Kuhn first articulated the assumption that scientific theories determine the "world" scientists find in his seminal book, *The Structure of Scientific Revolutions*. Kuhn argued that the history of science was not a continuous accumulation and refinement of knowledge but instead underwent radical discontinuities. Science was punctuated by revolutions in which one global theory or paradigm (e.g., Einstein's) replaced another (e.g., Newton's). Moreover, he asserted that any two competing paradigms are incommensurable, which by definition means non-comparable:

> ...the third and most fundamental aspect of incommensurability of competing paradigms...the proponents of competing paradigms practice their trades in different worlds...see different things when they look from the same point in the same direction.[21]

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methods of gathering and modeling data she calls testing accounts to differentiate them from ER accounts. (Mayo, 1996 p. 72.)

[19]Ian Hacking (1983) gives a nice non-technical overview of these early attempts at justifying science. Wesley Salmon (1967) gives a detailed and somewhat technical discussion of problems with justifying scientific inference using induction, probability, and statistics.

[20]Kuhn was not the only one to suggest that these two contexts did not exist nor was he the only one to cast doubt on the objectivity of justificatory procedures in science. He is, however, the best known and his work is considered pivotal in the field.

[21]Kuhn (1970, p. 150).

Nor can one look to Kuhnian normal science (experimental practice) to provide objectivity for in Kuhn's framework normal science is part and parcel of a paradigm.[22] Kuhn's theory of paradigm determinism--that experimental methods of appraisal are hopelessly enmeshed within the larger global theory within which an experimenter works-denies, *a priori*, objective hypothesis assessments based upon experimental results.[23]

However, Kuhn has only asserted that scientists perforce wear the "theoretical blinders" of the paradigm they work in. While few would deny the existence of scientific revolutions, the conclusion Kuhn drew from their existence, that scientists must embrace a new paradigm through a religious conversion or a one-way gestalt shift, has yet to be convincingly argued. Moreover, reading the published debates suggests experimental evidence plays an important part in discussing the merits of different theories.[24] Nonetheless, Kuhn has raised interesting questions and problems for scientific objectivity and his work forms the basis of much of the modern debate. How does Kuhn suggest a paradigm or large scale theory can infiltrate or pollute experimental evidence within its

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[22]"I shall hence forth refer to as 'paradigm' a term that relates closely to 'normal science'. By choosing it, [I] mean to suggest that some accepted examples of actual scientific practice--examples which include law, theory, application, and instrumentation together--provide models from which spring particular coherent traditions of scientific research (Kuhn, 1970, 10)." Normal science cannot on his view be distinguished from the paradigm in which it is practiced.

[23]Critics of relativism have pointed to the drastic consequences that would arise by accepting this view of theory domination in experimental practice. Without any outside constraints imposed by reality, the assumption is "anything will go" in science. I have two objections to this argument. First, not "anything will go," for most relativists argue that social factors will constrain theorizing. Second, I think looking at the implications of relativism and then denying the adequacy of that epistemology because I don't *like* the results is in fact a rather relativist attitude! Relativism is better addressed by offering an alternative account of experiment that can answer some of the more pertinent objections relativists have raised and by highlighting assumptions underlying *specific* concepts of relativism and not the very idea of relativism itself. That relativism is inherently a self-contradicting view is often agreed upon by both parties and so adds little to the debate. My views on relativism owe a deep debt to Scheffler's discussion (1982, p. 22).

[24]Kuhn maintains that such debates are illusory for the proponents of different paradigms necessarily, by *his definition* of paradigm, talk through each other.

own normal science tradition? The gist of Kuhnian incommensurability as applied to normal science and for denying objectivity to experimental evidence lies in what Mayo calls his "circularity thesis." This is Kuhn's idea that scientists do not use experimental evidence to judge a hypothesis in a paradigm, they use the paradigm to interpret and judge the evidence. But how does this circularity actually play itself out in an experimental context?

The fundamental problems facing objectivity in experimental evidence coming out of Kuhn's work can be grouped into three general categories for ease of discussion. Of course, in real practice, the difficulties often overlap. These three fundamental problems all weaken the assumption that experimental evidence is objective.

(1) The reliability of observations: Are observations theory-laden, and if so to what degree? That is, do scientists see what is really there or what they expect to see? This problem has been exacerbated by the introduction and use of new and complicated technologies. Observations are no longer (if they ever were) "direct" visual experiences but are mediated by or created in complex machinery like scanning electron microscopes and particle accelerators.[25]

(2) The interpretative nature of results: Observations, especially those mediated by machines, are open to interpretation. Are these interpretations constructed socially, either by an individual or group, to fit pet theories and advance particular subjective interests? A strong statement of this problem suggests that there are no objective

[25]Of course, even the idea of a direct visual experience can be problematic. Scheffler (1982, Chapter 2) has, in my opinion, successfully addressed this problem at length. Other interesting discussions of this problem appear in Kuhn (1964); Hesse (1962, 1970), and Grover Maxwell (1962).

constraints whatsoever upon interpretation, only the cleverness of the scientists limits the possible interpretations.[26]

(3) The role of auxiliary hypotheses and ceteris paribus conditions: Scientists do not test a hypothesis, even a relatively low-level one, in isolation. In any experiment, a certain amount of background knowledge is assumed. Implicitly or explicitly, auxiliary hypotheses or background theories are part of the experimental context. The claim that the craters on the moon I see through my telescope are really there and are not an artifact of the telescope relies on certain hypotheses or theories about telescopes, vision, geology, and topology. *Ceteris paribus* conditions are all the things that should not effect the results; the assumption that "all other things being equal" of an experiment. They arise from the fact that experiments are conducted in the world by, and often on, human beings. The day of the week, the amount of traffic on the road outside the lab, or the gender of the experimenter *should not* affect the experiment; however, sometimes such considerations have been found to affect experimental results.[27] This problem, though faced daily by scientists, clinched the case against the view that an algorithm could be formulated to decide which theory a body of evidence supported. Technically, scientists could credit any number of lesser or supporting hypotheses or *ceteris paribus* conditions for the failure or success of a theory under test.[28]

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[26]Shapin (1988) suggests that the social status of the observer affected the warrant for belief in his results in the seventeenth century. Harry Collins (1985) sees social negotiation as the dominant constraint on interpreting results.

[27]Such factors are especially relevant in psychological studies where factors such as gender may bias results. The weather was one such *ceteris paribus* clause that affected Henry Cavendish's work.

[28]This problem is commonly known as Duhem's problem named for the French scientist/philosopher who first articulated it.

These then are the specific objections to and problems with objectivity in experimental evidence and thus to science itself. If evidence is to be "objective," scientists must successfully confront these problems. To understand how scientists solve these problems, epistemologists need to look at how scientists actually attempt to solve them and *see whether* those methods *are* justified. Fortunately, over the past twenty or so years, members of the Science and Technology Studies (STS) community and its constituent fields have been doing just that--looking at actual scientific practice to gain insight into problems regarding the objectivity of experimental evidence.

(1.2) The Current Debate Over Objectivity: Regresses & Egresses

One aspect of the current debate over the objectivity of experimental evidence can be viewed primarily as a dialogue between sociologists of science and the "New Experimentalists."[29] In *Changing Order*, Harry Collins claims: "The acceptance of replicability can and should act as a demarcation criterion for objective knowledge."[30] Based on this criterion, he constructs two paradoxes which he labels "experimenters' regresses" to assert that experimental objectivity is impossible. Collins' main premise behind his experimenters' regresses is that if there can be no algorithmic solution for getting machines to run (paradox 1) or replicating experiments (paradox 2), then social factors must provide an explanation for scientific decisions:

[29]A term coined by Mayo from Ackermann. New Experimentalists would include Allan Franklin, Peter Galison and Ian Hacking. These philosophers all look to local strategies within experimental practice to justify scientific rationality (Franklin and Galison) or entity realism (Hacking). Harry Collins and Andrew Pickering are my chosen representatives for sociologists of experimental practice. Both deal with problems and local practices similar to the ones looked at by the New Experimentalists, though from a radically different perspective.

[30]Collins (1985, p. 19).

> Some "non-scientific" tactics *must* be employed because the resources of experiment alone are insufficient [to break the experimenter's regress]. In the absence of an algorithmic recipe for proper replication of an experiment, these tactics are a way for trying to establish what is to count as "going on in the same way" in the future.[31]

However, one needs to keep in mind that it is Collins who demands that for experimental practice to be "non-socially determined," it must be algorithmic or, as he states elsewhere, follow "murine" rules to distinguish it from non-scientific practices.[32] Still, Collins has raised two questions with his paradoxes that are worth investigating. How do scientists decide whether a detection machine is working? And, is the inability *to exactly duplicate* an experiment irreconcilable with objectivity? While Mayo does not discuss Collins, her epistemology provides the tools for successfully combating his regresses and I will use these in my discussion of him.

Collins' first paradox revolves around the idea that in order to find a theoretical object, a scientist needs a particular detection machine. However, to know whether the machine is working, a scientist has to find the object it was designed to detect. This circular relation forms an infinite regress he alleges. Collins' underlying assumption here is that the *only* criterion for knowing whether a detection machine works is its success in finding objects. This is false. One may be able to determine, by the very properties of the instrument and/or analytic tool, the type of object or effect it would detect were it to exist. We can also develop reliable checks in order to determine whether an alleged

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[31]Collins (1985, p. 143). By "non-scientific" tactics, Collins means social tactics.

[32]Collins (1985, pp. 38-46). The view that scientists have ever considered the role of experimental testing as an *algorithmic* activity or a *formality* as Collins proposition 6 suggests (p. 76) is entirely unsupported by either the history of science or Collins' own case studies. Nor is an algorithm or "magic recipe" necessarily required for a sense of objectivity.

"detection" or positive reading is really an artifact of the machine or spurious. The very idea or process of "de-bugging" a machine lies in finding and knowing how it can err and how it should function when working properly. Furthermore, much of this knowledge is independent of a specific machine and, instead, is based on knowledge of how machines in general or particular types of machines can malfunction.[33]

Galison showed in his neutral currents discussion that experimenters have the ability to model suspected errors using statistical techniques to see what a result would look like if a specific proposed error were in operation.[34] In Galison's example, researchers wanted to see if back-scattering muons were the source of their results rather than neutral currents. Using standard statistical significance methods and a computer simulation (Monte Carlo) which showed what the results would be if they were due to back-scattering muons not making it to the detector, this source of error could be disregarded as incredibly improbable. In my case study of Henry Cavendish, I will illustrate how sources of technical error can be isolated and independently tested to check that a machine is working properly before an unknown relationship or object is detected.[35] Collins' assumption that success is the only criteria for assessing the proper functioning of machines does not stand up to scrunity.

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[33]My chapter two illustrates these points. Cavendish combined his mathematical knowledge and his technical knowledge of electrical instruments to both design and de-bug a machine (his Great Globe apparatus) to detect if an inverse square force law described electrical attraction and repulsion. By using a combination of analytical and technical knowledge, Cavendish not only knew how his machine should work if the relationship he was looking for existed but how his machine would behave if his hypothesis of an inverse square law were incorrect.

[34]Galison, (1987, 219-223).

[35]While Collins concentrates on entities, his regresses, if viable, should also apply to relationships or laws such as those describing electrical attraction and repulsion.

More generally, Collins' experimenter's regress takes the form of another paradox. No experiment can ever be an exact replication of another. Another test will be needed to test the first test, and yet another to test that test, and so on. This experimenter's regress finds its home in the three problems with objectivity previously discussed. For example, *ceteris paribus* conditions are constantly changing--experiments run the morning of a warm and sunny day are different from experiments run later that evening when it is cold and cloudy.[36] Collins' point is that no experiment is ever an *exact replica* of another and thus scientists cannot use the ability to duplicate experiments as warranting experimental results. Thus, Collins concludes the problem of duplication is resolved socially through negotiation among experimenters.

But to resolve this paradox, we need to know what it is that experimenters need for replicability to count as a justification for a result. Mayo argues, and I concur, that what experimenters want from a replication is the appropriate scrutiny to rule out with reliability that they have committed a mistake. For example, they need replication in order to deal with the threat that an effect was a fluke. Experimenters need to check that they have a real effect and they know that candidates for real effects often go away when put under further scrutiny. The key idea behind replication is to rule out mistakes: that it is just a fluke, that it was really due to some artifact of the apparatus, that it only works in a given type of case, and so on.

[36]Of course, Collins' argument is more sophisticated and focuses on the use of different technologies and detection machines. Nonetheless, his underlying argument is the same as in my example. Moreover, this example is not given to trivialize Collins' point, but is drawn from my own case study where weather was found to affect experimental duplication.

Often, experimenters obtain this information by deliberately varying the case rather than repeating the application. Ian Hacking, one of the first philosophers to discuss the local nature of experimental methods and their independence from the more theoretical aspects of science, gives an excellent example of this sort of non-duplication repetition.[37] His primary example of "sorting out" an artifact from a real effect consists of a description of dense bodies (structures within blood cells) under two different microscopes--an electron microscope and a light microscope--each of which operates on different physical principles. If a scientist "sees" the same "dots" under each instrument, the probability that the "dots" were an artifact of either instrument would be very small.

Hacking's example underscores the idea that the motivation behind replication is to get *reliability* through deliberately varied applications. An exact duplication would provide no new information beyond the first test and thus would not serve as a check. However, detecting the presence of an entity using different physical operations supports the view that the entity is not an artifact of a particular physical process. Another example of this type of experimental reasoning is asking someone else to make a measurement as a check on one's own measurement. Collins assumes that without an algorithm experimental assumptions are at best socially agreed upon and ignores the role of experimental design to delineate assumptions, as well as the many non-algorthimic methods for independently testing assumptions.

Experimenters are not in search of an algorithm that will guarantee them an error free experiment. What experimenters do want and often get is sufficient reliability

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[37]Hacking (1983, Chapter 11)

through the use of robust experimental methods.[38] For example, as Mayo shows, noisy data often give way to far more accurate (modeled) data using statistical techniques such as averaging. Raw data can be seen as (and often is) a list of discrete measurements from several runs or trials or "repetitions" of an experiment. To get more accurate data, one can average such measurements and that statistical operation (averaging) eliminates or at the very least reduces random variations that come in from, as Collins' has pointed out, the inability to ever exactly repeat a run of an experiment. Moreover, scientists can use experimental knowledge to indicate the amount of variability to be expected if changing experimental conditions exceed the margin of error in their experimental design. I will illustrate this point in my case study of Cavendish.

Allan Franklin, in his response to Collins' regresses, makes similar observations.[39] Gooding, Pinch and Schaffer in their introduction to *The Uses of Experiment: Studies in the Natural Sciences* concisely sum up Franklin's defense for the reliability of experimental knowledge:

> Franklin outlines a number of epistemological strategies' or arguments designed to establish the validity of an experimental result or observation. These include: looking at the same phenomena with different pieces of apparatus; prediction of what will be observed under specified circumstances; regularities and properties of the phenomena themselves which suggest they are not artifacts; explanation of observations with an existing accepted theory of the phenomena; the elimination of all sources of error and alternative explanations; calibration and experimental checks; predictions of a lack of phenomenon; and statistical validation. These strategies are shown to have been important in a variety of cases drawn

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[38]A robust method may be understood here as one with the ability to function validly despite violations of assumptions.

[39]Franklin (1994), "How to Avoid the Experimenter's Regress."

> from both contemporary physics and from the history of physics. Franklin's chapter is littered with examples.[40]

Using the verb "litter" to describe Franklin's use of examples, Gooding *et al* are suggesting that he doesn't give anything like a full-blown epistemology of experiment.[41] This is precisely what Mayo regards as a fundamental weakness of the New Experimentalists who look to experiment to answer challenges to objectivity.

Mayo builds her Error Statistical epistemology around three themes she identified in the work of the New Experimentalists:

> 1. Understanding the role of experiment is the key to circumventing doubts about the objectivity of observation.
> 2. Experiment has a life of its own apart from high level theorizing (pointing to [a] local yet crucially important type of progress).
> 3. The cornerstone of experimental knowledge is the ability to discriminate backgrounds: signal from noise, real effect from artifact, and so on.[42]

Her account revolves around error -- detecting, simulating and eradicating error. Scientists gain objectivity in experimental evidence by confronting Kuhn's three challenges to objectivity head-on by ruling out errors, sources of subjectivity, noise, confusion, et cetera. Like other New Experimentalists, Mayo focuses on the local tools and strategies used in experimental practice.

Mayo goes beyond them by scrutinising what the various elements in experimental design -- gathering data, modelling data, justifying assumptions, testing assumptions,

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[40]Gooding, Pinch, and Schaffer (1989, pp. 21-22).

[41]That is, his arguments are neither epistemologically related or justified as a whole. This has led Collins (1994) to charge that Franklin has merely pushed social factors one step back in the process of experimental inference and that choosing which of these local tasks to use or accept as providing good evidence is the result of social negotiation. My response based on Mayo's work is not subject to this charge, as I will show.

[42]Mayo (1996, p. 63).

robustness arguments, arguments from coincidence, etc. -- all have in common. She argues that underlying all the various methods of experimental design is the central idea of giving an argument from error.

In order to make an argument from error, experimenters first must be able to detect errors. Mayo lists seven general tactics scientists have at their disposal for learning about errors[43]: Briefly, experimentalists use (1) before trial planning and (2) after-trail checking to avoid known sources of error or to see if known errors have been committed. Scientists build up (3) an "error repertoire" of mistakes and (4) study the effects of mistakes. They also (5) simulate errors and (6) amplify and listen to error patterns. That is, they magnify effects of errors to study them.[44] Utilizing all of the above tactics allows experimenters to argue for (7) the robustness of their experimental arguments even to the extent of showing when violating background assumptions is un-problematic.[45] All of the above seven points allow experimenters to learn from and detect errors. Using some or all of these tactics then allows experimenters to design experiments that (8) severely probe for error. To severely probe for error simply means using the above

[43]Mayo (1996) pp. 4-7.

[44]A sub-experiment of Henry Cavendish will provide an example of "magnifying" an error. Cavendish, in order to see if electricity could travel along his insulating glass stands, charged the stands themselves. By magnifying a possible source of error--electrifying the stands directly--he discovered that they were not providing adequate insulation. He then coated them with sealing wax to resolve this problem. I discuss this example in detail in chapter two.

[45]Mayo's example of this concerns testing a new teaching method by using two groups of students--a control group (one taught the old way) and another group taught using the new method. At the end of the year, each group is tested on the material covered. If the group using the new method does better, then the new method is inferred to be more effective. One major background assumption is that two groups of students have equal capabilities and backgrounds. If it turns out that the group using the new method were actually less capable or had less adequate preparation before the test began, then their doing better on the test, even though the background assumption was violated, would in fact strengthen the case for the success of the new method. (Mayo 1996, p.6.)

tactics, experimenters have good grounds for arguing when and where errors are present or absent and whether a specific test procedure can give reliable information about the correctness of a particular hypothesis. (Moreover, using the same criteria, a test can be criticized as forming an inadequate test of a hypothesis.) Mayo calls this approach "*arguing from error.*"

Mayo provides the following generalized pattern for recognizing and constructing an argument from error:

> It is learned that an error is absent when (and only to the extent that) a procedure of inquiry (which may include several tests) having a high probability of detecting the error if (and only if) it exists nevertheless fails to do so, but instead produces results that accord well with the *absence* of error. (An analogous argument is used to infer the presence of an error.)[46]

Hacking's example[47] of seeing dense bodies under two different types of microscopes provides a severe test whose results accord well with the absence of the artifact error, i.e., that dense bodies are an artifact of either microscope. This is because were the dense bodies an artifact of one microscope it is almost impossible that they would be seen using an different type of microscope.

Mayo also provides another version of the argument from error:

> In terms of a hypothesis *H*, the argument from error may be construed as follows: Data in accordance with hypothesis *H* indicates the correctness of *H* to the extent that the data result from a procedure that with high probability would have produced a result more discordant with *H*, were *H* incorrect.[48]

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[46] ibid. p. 64.
[47] See my p. 19 for details and references.
[48] Mayo (1996) p. 445, fn 1.

Henry Cavendish designed a complicated and intertwining series of experiments to severely test his hypothesis that electrical attraction and repulsion could be described by an inverse square law. Were his hypothesis incorrect, then there was a very high probability that his results these from eight different experiments would have given very discordant results compared to his theoretical calculations. In fact however, his experimental results were very close to his calculated predictions. Thus, Cavendish argued: "I think that the difference being so small is a strong sign that the theory is true."[49] Before I discuss the underpinnings of her epistemology, I want to look at one more sociological account of experimental practice for it claims a modified sense of objectivity grounded by testing. This tactic will allow me through contrast with Mayo to distinguish what is required for a testing account to ground objectivity claims.

(1.3) The Current Debate Continued: Mangled Objectivity

Some sociologists of experiment have tried another tack which they say offers room for a modified sense of objectivity. Andrew Pickering describes experimental practices as forming a "mangle." He defines his mangle as a "dialectic of resistance and accommodation...a genuinely emergent process that gives structure to the extension of scientific culture in the actual process of scientific research."[50] Pickering sees experimental practice as an attempt to form a stable "association of three disparate cultural elements: a material procedure (assembling and running a piece of apparatus); an instrumental model (a theoretical understanding of how the apparatus functions); and

[49]Cavendish (Article 278). He uses this same type of reasoning or argument from error throughout his electrical researches.

[50]Pickering (1994, p. 112). He is championing the sort of dialectic objectivity which I briefly discussed in my introduction.

a phenomenal model (a theoretical understanding of the phenomenon under investigation)."[51] In simpler words, his three cultural elements are (1) the machines, (2) the theories underlying the design and operation of the machines and (3) the theory which initiated experimenters' research in the first place and will be used to assess the results of their experiments. It is his third cultural category, which is reminiscent of Kuhn's circularity thesis, that seems to hold the greatest threat to the objectivity of experimental evidence. But let me explore his "mangle" further to see what consequences and questions come out of his view of experiment.

According to Pickering, an experimenter's driving aim is to form a stable association between elements in the three cultural categories he outlines. Pickering defines resistances as objects or circumstances from any of his three cultural categories which block the formation of a desired association. These resistances force an experimenter to change some elements in one or more of the three categories, most often the technology, in order to achieve accommodation. Once an experimenter has overcome any source of resistance, technical, conceptual or social, and stably joined all three elements together, the accommodation is complete and closure is achieved--a new scientific fact is established. Moreover, Pickering's narrative emphasizes that it is an individual's ability to tinker with his or her experimental apparatus and results which brings about successful accommodation.[52]

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[51]ibid. p. 113.

[52]Actually on Pickering's account no association is ever permanently stable, but rather should be thought of as "a temporary and potentially unstable limit to practice." (ibid. p. 116).

Pickering's view that experimenters will manipulate their experiment and their technology until they get a result which agrees with the theory under investigation, fails even the most minimal requirements for objectivity. In fact, it is this ability to manipulate experiments that raised questions about the objectivity of experimental evidence in the first place. Moreover, given his third cultural category, his account does not rule out social negotiation to uphold the "status quo," in order to make a "happy" accommodation. But why would he hold this view?

This view is based on Pickering's assumption that resistance is never an objective constraint of nature: "Resistance is truly emergent in time...one can speak of the *contingency* of resistance. I can think of no principled way for explaining why particular resistances surface at particular times in particular projects. Resistance "just happens."[53] Scientific knowledge seems quite a haphazard affair on this view. However, just because Pickering has defined resistance this way does not make it so. I would argue that should experimenters' experiments reach an "accommodation" that did not reflect any aspect of nature objectively, external reality would sooner or later become a resistance either in future experiments exploring other predictions of a theory or in the results of similar experiments. Pickering, to be consistent in his mangle theory, must reject this view in that resistance for him is always historically contingent rather and never transcendent to an experiment. Resistance is a local technological/theoretical/social constraint and can

[53]ibid. p. 115. This view is coherent with his ideal that "scientific knowledge is relative to chance" (ibid. p. 117).

not be an inherent property or aspect of exploring experimentally an outside reality.[54] I will illustrate this idea in my case study of Cavendish.

I have suggested that Pickering's account of experimental tinkering seems quite damning to any quest for objectivity. Furthermore, given his claim that experimental practice is also relative and non-objective,[55] how does Pickering feel he can claim even a modified sense of objectivity? He suggests objectivity arises in his account because an experimenter has successfully accommodated his results or experiment to external resistance from three different worlds:

> The entire history of his [Morpurgo's] program of experiments points to a continual struggle with resistant otherness in the material world...and in the conceptual and social worlds....To put the point most sharply, I can imagine no more stringent test, no tougher requirement, to place on knowledge claims affecting objectivity than that they should have passed through the mangle.[56]

Pickering claims that it is this struggle or test (surviving his "mangle") that turns an accommodation into a successful association which in turn is an objective fact. But this sense of testing is far different from the one I have been championing. Based on Mayo's work, I have suggested that good tests are designed on a pattern of arguing from error. Experimenters design their tests to find and understand errors that would lead them to incorrectly assess the hypothesis under test. Good tests are designed to further the responsible and objective evaluation of claims and to pass a claim only to the extent it

[54]The quotes from Pickering at the beginning of this paragraph support this point. Also, if one reads his footnotes (Pickering 1994) in a row, this local and contingent view of resistant 'reality' rather than reality as a constraint emerges quite clearly.

[55]Pickering (1994), Subjectivity is, however, not relative to any enduring social principle but to chance and the serendipity of time and history.

[56]Pickering (1994, p. 118).

is correct. In contrast Pickering's "mangled" test is the ability to accommodate an amorphous mash of technology (material world) and social and conceptual expectations. Pickering's ambiguous definition of his mangle is one of the major obstacles to understanding his explanation of experimental practice.[57]

However, more important than un-mangling his mangle, I think what needs to be unpacked and seriously addressed is his contention that experimenters tinker until they get the answer they want. The underlying assumption of this charge of "tinkering" is one shared by Harry Collins and is the cornerstone of many relativist interpretations of science. "It is not the regularity of the world that imposes itself on our senses but the regularity of our institutionalized beliefs that imposes itself on the world. We adjust our minds [or tinker with our machines] until we perceive no fault in normality."[58] However, this is not an argument but the fundamental assumption of their models which they owe to Thomas Kuhn and his circularity thesis.

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[57]Michael Bishop (1996) states the problem nicely:

> [Pickering's] position promises to embrace relativism, since it understands scientific knowledge to be essentially culturally situated, and objectivism, since passing through the mangle of practice is an extremely stringent test on knowledge claims. One worry about Pickering's view as described here is the vagueness of its principle components. Could the association-resistance-accommodation triplet that defines the mangle of scientific practice just as well define the "mangle" of novel-writing, football or philosophy? Another worry is that, *pace* Pickering, this view does not seem to exhibit an objectivism deserving of the name. In absence of any restrictions on what counts as a *successful* scientific theory (or successful "accommodation"), the fact that a theory survives the mangle of practice can provide no objective warrant for believing it.

Bishop's remarks appear in a review of an article written before Pickering had fully worked out his theory. Still, Pickering's chapter on objectivity in his finished work is very close to his views in the article Bishop reviewed.

[58]Collins, 1985, p. 148. The words in brackets suggest how I see Collins' statement as reflecting my interpretation of Pickering's discussion of "tinkering."

(1.4) To Err Is Human--To Learn From Error Is Science

The New Experimentalists inability to formulate an overall epistemological rationale to explain why experimenters use the wonderful yet inchoate local tools sprinkling their narratives have led sociologists like Collins to suggest that such strategies do not eliminate social or cultural factors but merely push them a step back. Mayo's epistemology both synthesizes and generalizes the work of the New Experimentalists. All the local tools they discuss in their often detailed experimental narratives can be epistemologically grounded in and warranted by her view that experimenters design and judge experiments based on their ability to construct an argument from error. Thus, adopting her view allows for a scientific standard, arguing from error, to replace the social negotiations or theoretical biases which Collins, Pickering, and Kuhn assert decide the outcome of experimental episodes. But is her "argument from error" open to the same sociological criticism that what constitutes an argument from error is also socially determined? Not in any non-trivial sense because Mayo's concept of severity forms the bedrock of her epistemology to gauge arguments from error and ground claims for procedural objectivity.

By severity, Mayo simply captures common intuitions about what a good test entails. For a test to tell experimenters anything meaningful it must be severe. Severe tests must rigorously assess the hypothesis under investigation by probing for possible errors. Mayo describes the role she sees for severity in experimental practice in the following passage:

> The cornerstone of an experiment is to do something to *make* the data say something beyond what they would say if one passively came across them. The goal of this active intervention is to ensure that, with high probability, erroneous attributions of experimental results are avoided. The error of concern in passing [hypothesis] *H* is that one will do so while *H* is not true. Passing a severe test, in the sense I have been advocating, counts for hypothesis *H* because it corresponds to having good reasons for ruling out specific versions and degrees of this mistake. Stated simply, *a passing result is a severe test of hypothesis H just to the extent that it is very improbable for such a passing result to occur, were H false.* Were *H* false, then the probability is high that a more discordant result would have occurred.[59]

A hypothesis is warranted if it has passed at least one, or more, severe tests. Mayo's concept of severity was derived from standard Neyman-Pearson statistics.[60] Experimenters use severe tests to probe for ways they could be wrong, for errors that could cause them to pass an incorrect hypothesis, or fail to pass a correct one.

Mayo formulates her concept of severity formally via the combination of her severity requirement and severity criteria[61]:

> *Severity Requirement*: Passing a test *T* (with evidence *e*) counts as a good test of or good evidence for hypothesis *H* just to the extent that *T* is a severe test of *H*.
>
> *Severity Criterion 1a*: There is a very high probability that test procedure *T* would *not* yield such a passing result, if [hypothesis] *H* is false.

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[59]Mayo, p. 178.

[60]However, her concept of severity is more generalized and goes beyond Neyman-Pearson (NP) statistics. "To really get down to the bare bones, the NP testing theory can be seen to define mathematical functions on random variables." (Mayo p. 365) Mayo's concept of severity encompasses both informal and formal testing procedures. There are other differences, including her rejection of Neyman's behavioral model, which the scope of this thesis does not allow me to address, but which Mayo covers in Chapter 11 of her book. Moreover, her epistemology was designed to subsume the multi-variate practices which the other New Experimentalists describe, many of which are non-statistical.

[61]An important point to keep in mind here is: While the severity of a test or which test to use to probe a hypothesis is determined by what one wants to learn, that is the hypothesis under investigation, the actual design and underlying theory of a test is most often, if not always, independent of the hypothesis in question.

> *Severity Criterion 1b*: There is a very low probability that test procedure *T* would yield such a passing result, if [hypothesis] *H* is false.[62]

It is clear from the above definitions that Mayo is not assigning certainty as an outcome or byproduct of her concept of severity. However, the probability of making an erroneous inference based on a severe test is very low on her account. Nor is severity an algorithm. Different tests have different degrees of severity depending on the experimental context in which they are used.

Furthermore, the same test can also have different degrees of severity depending on the hypothesis alleged to pass:

> It is important to stress that my notion of severity always attaches to a particular hypothesis passed or a particular inference reached. To ask, How severe is this test? is not a fully specified question until it is made to ask, How severe would a test procedure be, if it passed such and such a hypothesis on the basis of such and such data? A procedure may be highly severe for arriving at one type of hypothesis and not another.[63]

An example may be helpful here. Reading and summarizing Dr. Suess's *The Grinch Who Stole Christmas* may be a good (severe) test of a third grader's reading ability, but it would not be a good test of a graduate student's ability. The test is too easy, or not severe, because it is highly probable that someone below a graduate reading level could pass this test. But it *is* a good test of a third grader's ability to read. Thus, the severity of the test is attached to or judged in the context of the hypothesis being tested. Conversely, reading and summarizing Spinoza's *Ethics* may be a very severe test of a graduate student's reading ability but would not be a severe test for a third grader because

[62]Mayo. p. 180.
[63]Mayo (1996, p. 184).

it is too hard. By too hard, I mean there is a high probability that a third grader with excellent reading skills would fail this test. Again, the severity of the test attaches to the hypothesis in the view that it is to be judged a severe test if and only if it is a good test of a graduate students reading ability. A text as difficult as Spinoza's *Ethics* would be a good test of a graduate student's reading ability. The important point which I have tried to illustrate is that while severity is a universal criterion or standard, it is always and only locally determined for each individual test and each individual hypothesis passed.

Mayo has offered an interesting new possibility for responding to Collins' claim that any non-algorithmic solution to his experimenters' regresses must be social. (Remember, Collins said the strategies Franklin had outlined were themselves ultimately chosen based on "non-scientific" social considerations.) All the diverse experimental strategies the other New Experimentalists have uncovered in their narratives share an underlying thread or epistemological strategy of offering an argument from error. These strategies are chosen, individually or together, to form severe tests of hypotheses. The choice of strategies to use and judgement of strategies used are based on achieving severity for the argument (i.e., experiment) offered. Thus, Mayo's concept of arguing from error holds that scientists use (or can use) an objective standard -- severity -- to both design and judge experimental arguments. The choice of which test to use or which test is best is based on severity requirement for making an argument from error not on social negotiations.

This brings out one of the underlying consequences of her philosophy. While social factors may influence scientists both individually and corporately, the success of science is *grounded* in severe testing procedures.[64] Her epistemology is an evolving one in the sense that, as the tools and techniques for designing and implementing test procedures improve and new technologies arrive on the scene, they can be incorporated into her account. In this sense her epistemology is not at odds with science being a practice situated historically in time and space. However, this view of historical contingency is not damaging to a sense of procedural objectivity.

Mayo's concept of severity provides the epistemological grounds for the objectivity of testing procedures in experimental practice. First, experimenters use and design severe tests to detect specific errors. The discovery that a particular error is present or absent is objective, if not ground shaking, knowledge.[65] Second, searching for and eradicating errors forms the basis of objective experimental procedures. Remember, the *minimum* requirements for objectivity are twofold: acknowledging that one can be wrong and searching for methods to control the errors that can make one wrong.[66] Third, when experimenters reduce errors -- subjective biases towards a particular hypothesis, artifacts of a piece of equipment, or just poorly designed experiments -- the experimental knowledge gained (and refined) becomes more and more robust and objective as it becomes less error-laden. While Mayo gets the objective assessment from statistics, this assessment can be generalized to non-statistical cases. I

[64]Moreover, failures, temporary or long term, can be explained as due to experimenters having failed to achieve adequate severity in their testing procedures, thus allowing errors to enter and/or bias experimental results.

[65]I do not mean to denigrate this sort of low-level knowledge by any means. I feel it is, in fact, very important as my case study will illustrate.

[66]This statement is a summary of Scheffler's argument about the minimum requirements for objectivity, see the first two pages of this chapter. Again, it is the possibility of error that makes experimental evidence non-objective. Personal, social and theoretical biases are errors if they unduly affect experimental results.

will illustrate this in Chapter 4. So far in this chapter, I have described the various views of some STS practioneers about the objectivity of experimental evidence. Moreover, I argued that Mayo's epistemology provides good grounds for the objectivity of testing procedures in experimental practice. However, few of the New Experimentalists, Mayo included, have looked at case studies earlier than the mid-nineteenth century. While Mayo is championing an epistemology which grows out of standard Neyman-Pearson statistics, the general pattern of reasoning from error and her concept of severity is compatible with non-statistical experimental practice.[67] Several accounts accentuating social factors and/or theory-ladeness are available describing scientific practice prior to the nineteenth century.[68] Have experimenters historically worried about the objectivity of their results? Have particular experimental methods assured scientists that their inferences were warranted? The history of both science and natural philosophy suggests that the insights of the New Experimentalists, and Mayo's epistemology in particular, can be quite useful for studying historical episodes in experimental practice.[69]

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[67]Mayo (1996, p. 451) has also noted this compatibility: "I have largely dealt with questions that may be articulated within statistical models, and yet experimental inquiry relies on numerous informal exemplars and models from outside statistics."

[68]Shapin (1988) and Kuhn (1970), to mention two.

[69]Please note, I am suggesting Mayo's epistemology is useful for explaining historical episodes in experiment, *not* that historical episodes should be used to support her epistemology. I believe her epistemology is the best one available at present for studying experimental episodes. (I have already given my reasons for this belief in my introduction.) I want to use it to understand Cavendish's researches which have not been adequately explained on other accounts in my opinion and to explore questions of objectivity in his work as well.

(1.5) The Eighteenth-Century Crisis in Certainty

Several currents of thought in the eighteenth-century joined together to produce a crisis in certainty on the part of many natural philosophers. In the British Isles, the skepticism of Hume, Newtonian instrumentalism, and the growing interest in and application of probability theory all combined to shake the foundations of seventeenth-century rationalism.[70] Especially damaging to any naive sense of objectivity was the instrumentalism that arose from Newton's theory of universal gravitation.[71] By postulating gravity, an invisible force and using his theory of fluxions (the calculus), Newton devised a theory with precise predictions open to empirical testing. Newton's theory of gravity proved to be quite a powerful and successful instrumentalist theory. However, forces like gravity were seen by many in this period as occult qualities and thus a step backward in scientific reasoning.

The eighteenth-century also saw an explosion of interest in probability theory. Many mathematicians including Thomas Bayes, Daniel and Jacob Bernoulli, Abraham DeMoivre, and Pierre Simon Laplace turned probability applications into the probability calculus. Mathematicians and natural philosophers were deeply concerned and interested in the uneasy mesh between quantification and observation, the complex relations in a method both analytic and synthetic.[72] To further complicate this picture of

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[70]There has been considerable historical research which supports this view. See for example: Daston (1988), Heilbron (1979), Hahn (1990), and Cassier (1951). Rationalists claimed that from a few self-evident *a priori* truths, certain knowledge could be gained using the "rational" methods of mathematics and logic. The zenith of rationalism was reached in the works of Rene Descartes.

[71]By naive objectivity, I am referring to a view which would equate objectivity with scientific realism.

[72]That is, a method which combines both *a priori* mathematical principles and experimental observations. See Cassier (1951, Preface, Chapters 1 & 2) for a good discussion of this problem. The long and often bitter clashes between Cartesians and Newtonians illustrate the depth of concern and interest in these problems. Heilbron (1979) is the best general reference I know of which discusses this history. Nor

Enlightenment science, the machine age -- air pumps, Leyden jars and other complex technologies -- had been exploding upon the experimental scene, especially in electrical studies. Nature was not only being tamed but being, in a very real sense, created in the laboratory. Moreover, in electrical experiments, natural philosophers did not observe electricity nor its strength directly but needed special electrometers (instruments for measuring the presence and strength of electricity) to observe the presence of this invisible phenomena. Observations were thus indirect or second order. That is the quality and precision of experimental data depended upon on the quality and accuracy of one's measuring devices. In sum, the inherent unreliability of observations, instrumentalist theories, and the introduction of complex mediating technologies introduced several new sources of error into experimental results and raised disturbing questions for experimental objectivity which Enlightenment experimenters were forced to confront. Eradicating error became an urgent concern for experimenters and other natural philosophers.

In the eighteenth-century, error, experiment, probability and statistics really began to come together:

> As a technical tool, statistical inference was prized for helping natural philosophers to distinguish between likely and spurious causes, thus preventing them from lapsing into unfounded speculation. Systematic errors of observation attributable to instruments could thus be distinguished from those dependent upon human failings.[73]

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was this interest restricted to natural philosophers. Moral or social scientists and governments were equally interested in the uncertainty of knowledge and the application of probability and statistics to human affairs. Condorcet's attempts to develop a "social mathematics" is an extreme exemplar of this prevalent Enlightenment concern and attitude (Baker 1975).

[73]Hahn (1990), p. 379.

The probability calculus held promise as a bridge between certainty and uncertainty in knowledge. Uncertainty itself would be quantified. However, probability theory applied to experimental observations is not all abstract mathematics. It is a calculus born of empirical observations. Many of its canonical models are drawn from common experience. A canonical model is a generalized model or exemplar which can be applied in various situations by experimenters to determine errors in their experimental set-ups and results.[74] The easiest way to explain canonical models is through an example.

Daniel Bernoulli, in 1778, derived a canonical model based on archery. It was used for averaging observations. A *skilled* archer shoots arrows at a target with a vertical line drawn down the middle for the goal.[75] The target is further subdivided into vertical zones equally distributed from the center target line. More than likely, arrows will be equally distributed to the left and the right of the goal. The majority of shots would fall in the first vertical band on either side of the mark, fewer in the second bands off the mark and so on. By averaging all the arrows, the "real" center mark could be determined more precisely than by one observation alone.

This particular canonical model is based on probability theory. It is an example of using statistical tools to gain more precise information -- information that goes beyond the actual data in hand. Bernoulli explains the idea that motivates his example:

> Now is it not self-evident that the hits must be assumed to be thicker and more numerous on any given band the nearer this is to the mark? If all the places on the vertical plane, whatever their distance from the mark,

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[74]However, the distinction between systematic biases versus randomness was not well worked out until later.

[75]Note, Bernoulli has emphasized that the archer is a skilled archer and thus only randomness of individual shots should affect the experimental distribution of arrows.

> were equally liable to be hit, the most skillful shot would have no advantage over a blind man....In this way, therefore, the degree of probability of any given deviation could be determined to some extent *a posteriori*, since there is no doubt that, for a large number of shots, the probability is proportional to the number of shots which hit a band situated at a given distance from the mark.[76]

Canonical models such as Bernoulli's skilled archer are drawn from common experience. Other canonical models can be derived from more rigorous mathematical models. They are one of the many tools experimenters since the eighteenth-century have had at hand to learn about and control error. The uncertainty of knowledge which empirical methods introduced can be manipulated, controlled and subtracted out to a certain extent. This type of test reasoning or arguing from error was explicitly used by Henry Cavendish in his electrical researches which I cover in chapters 2 and 3. Thus, objectivity can be regained, but in a more restricted and local sense, rather than in the absolute and universal sense as demanded by seventeenth century rationalists.

I have tried to suggest that eighteenth-century natural philosophers had already uncovered and were grappling with objectivity questions. Further, I have suggested that they had also recognized the role technology played in problematizing observational situations. This period thus seems a fruitful time to explore how objectivity problems and the role of error were initially articulated and dealt with. But rather than assert generalizations about objectivity in experimental evidence, such assertions need to be examined locally in particular experimental contexts. That is the lesson that comes out

[76]Bernoulli, p. 158.

of the work in recent philosophy and sociology of science and which underscores Mayo's error statistical approach to experimental epistemology.

(1.6) Henry Cavendish

Henry Cavendish is a fascinating figure in his own right--a wealthy nobleman who was pathologically shy, an exacting experimenter who rarely published, a man whose entire life revolved around his lab and the Royal Society.[77] He seems a figure more to inspire pity rather than the resounding acclaim accorded him both by his own generation and succeeding ones. He remains an enigma to this day. However, my fascination with him results from a view that sees him as the experimenter nonpareil of the eighteenth century.

In his work on electricity, Cavendish was confronted by the problems that new and relatively complicated technology posed. The electricity Cavendish studied in his lab, like the particles physicists study in particle accelerators today, was a technological effect. That is, it was created and controlled in the lab rather than passively (if such a thing is possible) observed in nature.[78] Furthermore, the subjective and objective elements in probability theory had not been fully disassociated nor really recognized yet.[79] Experiment was coming into its own as an important and legitimizing aspect of science. Amidst all these changes, and despite his silence about many of his experiments,

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[77]There are several biographies of Henry Cavendish available. I have listed some in my Bibliography. A new biography, (Jungnickel C. & McCormach R (1996), *Cavendish* in *Memoirs of the American Philosophical Society* Vol. 20, American Philosophical Society, Philadelphia, has just come out. It is not yet available to me.

[78]Of course, the obvious exception, today, would be lightening. However, back then, the status of lightening as an electrical phenomenon was questionable. Franklin's famous kite experiment was a test to determine whether or not lightening was an electrical phenomenon.

[79]Daston (1988) covers this nicely in Chapters 4 & 5.

Cavendish has garnered a reputation as a great experimenter praised for his methods and attention to error. What is it about his methods that have inspired such admiration? What can students of science and technology learn from him?

In this thesis, I will explore how at least one experimenter, Henry Cavendish, dealt with the problems to objectivity in experimental evidence. In his electrical researches, he simultaneously straddled three frontiers in science. First, electricity was still in its infancy and full of new and little understood technologies. Second, experimental practice itself was still in its infancy. Third, he was one of the first experimenters to include error analysis as an integral part of his experiments. Cavendish was truly an experimenter at the frontiers of science. His work should be fruitful for finding exemplars on how to and how not to search for objectivity in new fields mediated by new technologies using new experimental methodologies.

Chapter 2: The Great Globe Experiment

In this chapter, I briefly discuss Cavendish's global theory of electricity within the context of 18th century instrumentalism. Next, as a brief introduction to his Great Globe experiment, I review Jon Dorling's and Ronald Laymon's different summaries of the experiment. I devote the rest of the chapter to exploring Cavendish's Great Globe experiment using Mayo's Error Statistical epistemology. I contend that applying Mayo's epistemology not only brings out important details of the experiment but provides a framework to discuss the objectivity of Cavendish's methods and results.

(2.1) Models & Instrumentalism: Henry Cavendish as Theoretician

By 1750, many natural philosophers had embraced instrumentalism -- the view that theories are instruments rather than ontological truths -- to explain, manipulate, and predicate phenomenon.[80] For natural philosophers, one strength of an instrumentalist attitude was its flexibility. Natural philosophers could postulate "explanatory" forces or descriptive *models* of nature, often based on analogies[81] and/or mathematics, without concern about the ontological status of their theories. (This form of instrumentalism is quite compatible with Hacking's view that experimenters are more concerned with manipulating rather than representing phenomena.) Hackmann points out that the new generation of Mathematical physicists, including Cavendish, arose during this same period.[82]

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[80]Heilbron, pp. 70-73. Natural philosophers were the scientists of the eighteenth century. Science was not yet established as a separate branch of knowledge distinct from philosophy. The term "scientist" was not used until the nineteenth century (Cunningham & Williams 1993). However, I will use the term "experimenters" throughout since experimenters formed a distinct category at this time and the term is not an anachronism.

[81]Several writers (Achinstein, Hackmann, Hesse) discuss the power of analogy for scientific theory making.

[82]Hackmann, p. 57.

Cavendish's global theory of electricity[83] was one such instrumental model. He united Newtonian mathematics and known experimental effects into a predictive and explanatory global theory. For Cavendish, the experimenter, his theory's strength probably lay in its ability to be experimentally approximated or realized. This ability for experimental realization and repetition allowed for objective results even if the overall global theory was wrong. How?

Experimenters use instrumental theories to set up predictions for "real" experimental events or results. These smaller or more local predications can then be severely probed for error. Even if the theory is later discarded, the "real" effects or local predications, which were experimentally determined, remain.[84] Nature, or rather nature as expressed and controlled in the laboratory, constrains instrumentalism.

Before continuing, I will digress for a moment about my choice of the term "instrumentalism." I have chosen to use the term 'instrumentalism' to discuss Cavendish's theorizing for several reasons. First, I am using the term in accordance with Heilbron's meaning of the term to describe the attitude of late eighteenth-century physicists towards their theories and "theoretical entities", like the electrical fluid:

> Physics ended the century richer in essences than it had begun, and more conscious of their *hypothetical* character....This agnosticism and its implied instrumentalism--by all means invoke imponderables, feign hypotheses, multiply forces, if it is necessary to save the phenomena conveniently--are characteristic of the Newtonianizing physicists of the second half of the eighteenth-century. This was the science of men who grew up familiar

[83]By this I mean, Cavendish's over-arching theory of electricity, his version of a one fluid theory of electricity of which his hypothesis--that an inverse square law described electrical attraction and repulsion--was a prediction or part of his theory.

[84]This point has been raised by several other people including Mayo (1996); Galison (1987); Hacking (1983) and even Kuhn (1962) to name a few.

> with attractions and repulsions and the mathematics needed to treat them; who disposed of more and better data than their predecessors, and lacked their epistemological sensibilities.[85]

While my use of the term 'instrumentalism' may seem on the surface similar to 'operationalism' as articulated by P. W. Bridgman in physics,[86] the two terms are not interchangeable. In classic operationalism, the idea is to reduce all key concepts in a theory into observables or phenomenalist language. Instrumentalism, as I am using it, emphasizes using theories as instruments to find and manipulate observables--to gain experimental knowledge--but not necessarily to translate the theory itself into observables.

Let me quickly review Cavendish's global theory before proceeding to his experimental work. According to Cavendish, two types of matter existed -- regular matter and electrical matter. Cavendish postulated the existence of a single electrical fluid that was unevenly distributed throughout the "regular" matter composing the universe. Cavendish qualified his definition of the electrical fluid as matter by stating "for the future, I would be understood never to comprehend the electrical fluid under the word matter, but only some other sort of matter."[87] Cavendish was quite vague about the electrical fluid's ontological status outside of its peculiar attractive and repulsive powers.

Cavendish's instrumentalism (postulating some "other sort" of matter) allowed him to directly translate Newton's work on gravity into the field of electricity. Unlike most experimenters of his time, Cavendish had the advantage of a Cambridge education where

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[85]Heilbron (1979) pp. 70-71 (emphasis in original).

[86]Moyer (1991, Parts I and II).

[87]Article 4. In referencing Cavendish's work, I will only state article numbers. All references are from *The Electrical Researches of the Honourable Henry Cavendish*, ed. J. Clerk Maxwell, Frank Cass & Co. Ltd., London: 1967, original publication 1879. Cavendish wrote these papers between 1771-1781.

he probably first encountered Newton's ideas.[88] Newton's theory of gravity, with its occult forces but exemplary predictive powers as outlined in the *Principia*, served as an exemplar for Cavendish. Cavendish acknowledged the *Principia's* powerful influence on the theoretical aspects of his researches.[89] Newton's calculus -- his theory of fluxions -- as applied to matter and gravity was directly appropriated by Cavendish in a straightforward analogy to electrical phenomena.[90]

According to Cavendish, regular bodies or matter contained a certain proportion of the electrical fluid inside them. A body so composed could be in one of three states. If a body was saturated, the two types of matter (regular and electrical) were in equilibrium and carried no charge. A body was overcharged if it had an excess of electrical fluid, and undercharged if it had a deficiency of electrical fluid. These excesses or deficiencies of the electrical fluid caused attraction and repulsion between material objects. Cavendish's global theory predicated that the force law describing electrical attraction and repulsion between bodies was inversely square to the distance separating

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[88]Heilbron p. 477.

[89]Articles 18, 19, and 97 for a few examples.

[90]In Articles 18 & 19 he accepted Newton's demonstration that only an inverse square law will have a resultant net zero force in a sphere. Also, if the force was higher than the square, a particle in the sphere would be impelled to the center; if lower than the square, the same particle will be impelled outward from the center to the surface. Cavendish translated Newton's work on the resultant net force effects described by various inverse laws on a sphere as follows: an inverse square would have a net zero force or null charge on the sphere, an inverse cube law would result in the sphere being overcharged and an inverse linear law in the sphere being undercharged. Moreover, given an inverse square law, all excesses and deficiencies of electrical fluid would be located entirely on the surface of material bodies.

The analogical appropriation of Newtonian gravity has also been noted by Achinstein (1971, pp. 153-55) for Priestly's experimental work. Achinstein feels such analogies increase experimenters beliefs in their results. However, in Cavendish's case, I argue his experimental methods convinced him of the appropriateness of his analogy *not* that his analogy convinced him that his results were correct.

them. The flow of electrical fluid from one object to another was accomplished as if the two objects were connected by an infinitely small canal of incompressible fluid.

Cavendish's theory became unique when he introduced his canals of incompressible fluid:

> By a canal, I mean a slender thread of matter, of such kind that the electric fluid shall be able to move readily along it, but shall not be able to escape from it, except at the ends, where it communicates with other bodies. Thus, when I say that two bodies communicate with each other by a canal, I mean that the fluid shall be able to pass readily from one body to the other by that canal.[91]

These canals were infinitely small, frictionless pathways through which the electrical fluid flowed from one body to another. While Cavendish did not use the term "potential" in relation to his work, nonetheless, his description of the canals and their action clearly suggested such an idea to later electricians.[92] His global theory became an almost purely instrumental model at this point.

Cavendish was himself ambivalent towards the "reality" of his canals or at least his idealized description of them: "This supposition of the fluid in the canal being incompressible, is not mentioned as a thing which can ever take place in nature, but is merely imaginary; the reason for making of which will be given hereafter."[93] The reason given was that an idealized model such as this simplified his calculations by eliminating the effects of the canals themselves on the fluid which they transported. As Heilbron aptly stated:

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[91]Article 40.

[92]See J. Maxwell's footnote to article 199 and his Note 5. See also Heilbron, pp. 480-482.

[93]Article 69.

> The canals represent wires. The assumed incompressibility of the fluid within them is an artifice introduced to preclude accumulations or deficiencies Cavendish could not calculate; in the case of long, thin wires, as he expected, the idealization creates unimportant errors.[94]

Cavendish's theoretical canals were an idealization of known experimental effects and, thus, lent themselves readily to a physical interpretation. For an experimenter like Cavendish, that may have been their primary attraction or perhaps even his inspiration for devising them.

I have suggested Cavendish's method of theorizing closely followed Newton's *Principia*. Moreover, his experience with the ability of wires to conduct electricity may have been the inspiration for his canals. Now I want to suggest that there were other experimental constraints on his theorizing. These constraints I call "experimental puzzles" for while they may not be clearly understood at the time, any successful experimental theory should take them into account. These puzzles are known technological or laboratory effects.[95] The one I suggest constrained Cavendish was the Leyden jar.[96] I am certainly not the first to suggest this as a technological constraint on theorizing.

Roderick W. Home has argued in great detail and quite convincingly that the death of effluvial theories of electricity was not due to previous explanatory or predicative failures, nor due to an inability to provide a fruitful paradigm to shape future work.[97] Effluvial theories failed, according to Home, because they could not explain one

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[94] Heilbron, p. 481.

[95] I call them puzzles for while they form a subset of Kuhnian anomalies, I want to emphasize the experimental rather than theoretical context in which they arise. See Mayo (1996, p. 51) for a similar discussion on the constructive role anomalies play for revising and devising theories.

[96] The Leyden jar in modern terms was a capacitor, a vessel which could store electrical charge. It was basically a glass vessel lined two thirds up both inside and out with tinfoil.

[97] Home (1981, Chapter 5)

commonly used technological phenomenon, the Leyden jar. Glass played an ambiguous role in effluvial theories but was supposed to be transparent to electricity and thus the Leyden jar was an inexplicable phenomena.[98] In general, Cavendish rejected effluvial theories because they did not agree with experimental knowledge.[99] Moreover, he devoted eleven articles of his own theory to explaining the Leyden jar.[100] Considering the dominant place the Leyden jar occupied in electrical experiments, it should be no surprise that an experimenter, such as Cavendish, would feel compelled to accept or devise only theories that could incorporate this electrical wonder.

Cavendish's electrical theory was not novel. Cavendish admits "that this way [his theory] of accounting for the phenomena of electricity is not new. AEpinus, in his *Tentament Theorioe Electricitatis et Magnetismi*, has made use of the same, or nearly the same hypothesis that I have; and the conclusions he draws from it agree nearly with mine, as far as he goes."[101] He still thought his own theory worthy of the Royal Society's attention, however, since "I have carried the theory much further ... and have considered the subject in a different, and, I flatter myself, in a more accurate Manner."[102] Whether Cavendish devised his theory independently of AEpinus' theory or with prior knowledge of it, I will leave to others to debate.[103] As an active member of the Royal Society,

[98]Heilbron also covers this failure, pp. 287, 309, 315, 316.
[99]Articles 196-199.
[100]Articles 128-139.
[101]Article 1.
[102]ibid.
[103]I am more concerned with his role as an experimenter rather than theoretician. Heilbron (1979, p. 478) suggests Cavendish was familiar with AEpinus' work before he articulated his own theory.

Cavendish, though working in isolation, was quite familiar with current electrical knowledge.[104] I will now proceed to Cavendish's experimental work.

(2.2) The Great Globe Experiment: Two Overviews

In the next section, I give an error/severity interpretation of the Great Globe experiment. First though, I will review Jon Dorling's and Ronald Laymon's accounts of "the experiment" to give a quick overview before my much longer error account. I believe this will highlight the importance of differing philosophical approaches for understanding experimental episodes and also show how an error account makes room for and focuses on questions of objectivity that the others do not really address.[105]

Jon Dorling, in his article, "Henry Cavendish's Deduction of the Electrostatic Inverse Square Law from the Result of A Single Experiment" states that he will "analyze the argument of Cavendish by which, in 1773, he deduced the electrostatic inverse-square law from the *result of a single experiment.*"[106] Later, in my error account, I will dispute that the inverse square law was the result of a "single" experiment, but for now I will accept Dorling's claim that there was but one experiment:

> Cavendish's experiment was the following: He set up two concentric conducting globes. He charged the outer globe, made electrical contact and then removed the outer globe. Finally he tested with his electrometer for the presence of charge on the inner globe, having previously ascertained how small an amount of charge his electrometer could detect. He was unable to detect any charge on the inner globe and on the basis of

[104]Though painfully shy, he worked with Franklin and others on designing lightening rods for the Purfleet arsenal (Berry 1960, pp. 22, 23; Maxwell, Introduction pp. xxxii-xxxiv). He was also an active member of the Royal Society provided his fellow members stuck to scientific discussions. If the topic drifted away from science, he withdrew into silence or bolted. (Crowthers, pp. 312-315.)

[105]Neither Dorling nor Laymon claim to address objectivity. Dorling focuses on the cognitive rationality of individual scientists and Laymon on the use of deduction in making experimental inferences.

[106]Dorling, p. 327, emphasis mine.

> this result and the known amount of charge given to the outer globe Cavendish deduced, by an argument which can be made perfectly rigorous, that if the electrical force falls off as the *n*th power of the distance than *n* must lie between 1.98 and 2.02.[107]

For Dorling, this summary constitutes the experiment in its entirety. Dorling accepts Cavendish's results without questioning how they were gathered. Instead, Dorling concentrates on giving a Bayesian analysis of this experiment.

A Bayesian analysis revolves around Bayes' formula, an equation in probability theory that can be used to compute the probability of an event happening conditional on another event occurring.[108] For most Bayesians, probability is interpreted as subjective degrees of belief. Agents update their prior degrees of belief in a hypothesis to obtain posterior degrees of belief after the evidence or experimental result is in. To be rational, by this limited criterion, Cavendish (or an historian studying him) must compute:

> the prior probability ascribed to the particular experimental result; the prior probability ascribed to the conjecture in question; and the estimated probability of the experimental result on the assumption of the truth of the conjecture. *In so far as these data refer only to approximate qualitative subjective assessments so too will any conclusions that these data license.*[109]

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[107]Dorling, p. 328. While admittedly this is only an overview, there is a surprising lack of important experimental details. How or with what was electrical contact made?; Were either of the globes grounded? Several of these details will be filled in shortly when I review Laymon's summary of the experiment. My error statistical narrative focuses on these types of experimental details.

[108]Bayes' Theorem: $P(H_1/e)=P(e/H_1 1)xP(H_1)$ / $P(e/H_1)xP(H_1)$ + $P(e/H_2)xP(H_2)$ +...+$P(e/H_n)xP(H_n)$. In this formula, Bayesians attach probability assignments based on subjective degrees of belief to the hypothesis being tested (H_1), the experimental evidence (e), as well as all other possible hypotheses (the so-called "catch-all factor"). Once all the numbers are plugged in, if, once the evidence is in, a person's degree of belief is greater than their prior degree of belief, then it is rational to accept the hypothesis that was tested.

[109]Dorling, p. 337 (ital in original).

These probabilities are then plugged into Bayes theorem and if the probability of his belief increased after his experiment, it was rational for him to believe in his hypothesis.[110] In order to assess these probabilities, Cavendish would also have to assign a probability to an "exhaustive classification of all possible alternative theories...[and]...some estimate of the probability of his experimental result finding an alternative explanation of a kind he has simply not considered" otherwise known as the Bayesian catch-all factor.[111]

There are specific problems with Dorling's analysis even within his chosen Bayesian framework. Dorling contradicts himself when trying to assign degrees of belief to Cavendish. First he states that an inverse square law would be "implausible" for Cavendish and his contemporaries to believe in.[112] However, four pages later he states belief in inverse laws was plausible at the time.[113] More to the point, Dorling overlooks or ignores the fact that, regardless of Cavendish's belief, he did assume an inverse square law in the last six experiments, of which the Great Globe is the first experiment, in this series. Dorling also states that Cavendish needed to consider possible errors in his other

[110]Dorling, pp. 334, 337. See his footnote 17 for an explanation (and the problems in it) of his Bayesian analytical framework. See Mayo (1996) for a full-scale critique of Bayesianism as an inadequate epistemology of experiment.

[111]Dorling, p. 338.

[112]Dorling, p. 334. "...as soon as the *n*th-power law assumption is examined critically, it becomes clear that it must be rejected as unacceptable for Cavendish himself and for his contemporaries." Why does Dorling argue that Cavendish and his contemporaries would not accept an inverse power law, especially as that type of law is the focus of Cavendish's experiment? Dorling continues: "I would emphasize that this objection to Cavendish's major premise is not a philosophical or logical objection but a technical historical objection." That is, the intellectual climate was not ripe for n^{th} power law assumptions. Such assumptions, according to Dorling, would be unusual or even unacceptable in Cavendish's particular historical context.

[113]Dorling, p. 339. "But it is difficult to see how he [a hypothetical skeptic] could convincingly argue that an approximate inverse-square law was anything less than *quite probable* prior to the experimental result" as seen in the work of Bernoulli (1760), Priestley (1767) and Robison (1769).

theoretical and experimental assumptions as well as the possibility that his "experimental result is simply a freak result."[114] However, as I will show in my analysis of this experiment, Cavendish did not assign probable degrees of belief to these problems but explicitly addressed and experimentally grappled with them.

Ultimately, the use of a Bayesian analysis to look at the rationality, much less the objectivity, of experimental results, is a futile exercise at best, especially for an historical analysis. Researchers can dispute each others assignment of subjective degrees of belief to Cavendish's personal degrees of belief in his hypothesis before and after his results were in. The argument would now revolve around second order or twice removed degrees of belief about results, not about the experiment itself. To successfully address questions of rationality and objectivity, researchers need to investigate whether an inference was warranted based on the experimental evidence and how it was gathered, not on the changing subjective beliefs of experimenters.

Ronald Laymon gives a more in-depth and accurate summary of the Great Globe experiment and in so doing introduces the very problems I am concerned with:

> Cavendish's basic apparatus consisted of a spherical air condenser: two concentric spherical conductors connected by a removable thin wire. The experiment begins with the placement of a positive charge on the outer sphere or shell....[H]e knew, [from] mathematics,...the inverse square is the only inverse power consistent with there being no net force exerted (by charged particles on the outer shell) on particles within the outer sphere. Hence, he concluded that positive or negative charge would accumulate on the inner sphere only if the inverse square law were incorrect. So, experimentally, the idea was to measure the charge, if any, that accumulated on the inner sphere. In order to measure this charge, Cavendish first removed the wire connecting the sphere, and then

[114]Dorling, p. 338.

> separated, removed, and grounded the two hemispheres that made up the outer sphere. The inner sphere was then tested for accumulated charge using an electroscope of known sensitivity....*By using concentric shells in the way indicated, Cavendish had in essence reduced the experimental complications to those associated with adequately insulating the shells and avoiding induced charges due to nearby conductors.* Cavendish found that *within the sensitivity of his measuring instrument* there was no charge on the inner sphere. Hence, "the Law of Electrical Force" had to be inverse square.[115]

Laymon's version of the Great Globe experiment, which includes some of the experimental problems faced by Cavendish in designing his experiment and gathering his data, paints a more complicated picture than Dorling's account. In the passages I have emphasized, Laymon suggests three obstacles to objectivity faced by Cavendish. These three experimental obstacles were inadequate insulation, induction, and the sensitivity of his measurements. Laymon also points out in his footnote 8 that "Cavendish reports having conducted several versions of the experiment during 1772 and 1773."[116] Laymon devotes much of his article to discussing Cavendish's sensitivity calculation and (so Laymon claims) the apparent circularity it introduces into this experiment. This circularity arises because Cavendish's test plate was calibrated assuming an inverse square force law or so Laymon alleges. Laymon further postulates that Cavendish and other scientists have used "continuity assumptions" to legitimize what he sees as circularity flaws in several experimental arguments. I will return to Laymon's charge of circularity and his continuity assumptions in chapter four after I have analyzed Cavendish's series of electrical experiments. Outside of his summary of the experiment, Laymon does not

[115]Laymon, p. 28, emphasis mine.
[116]Laymon, p. 52.

discuss the other experimental complications he raised nor Cavendish's attempts at their resolution.

What I want to do now is complicate these pictures of the experiment even further using Mayo's error epistemology.[117] Cavendish's fame rests upon his error considerations.[118] His own writing stressed the importance of identifying, controlling, and eradicating sources of error in his experiments. Mayo's epistemology provides a conceptual framework for organizing a close examination of the ways in which Cavendish designed his experiment and gathered his data, the very areas where objectivity is most disputed. I see my job not to focus him artificially upon error, but to rely on his actual methods by looking at the details of his experiment which are sometimes mentioned but rarely stressed in the literature.[119]

(2.3) An Error Epistemology Interpretation: A Hierarchical Approach to Understanding Cavendish's Experiment

In *Error and The Growth of Experimental Knowledge*, Mayo argues that experimenters do and should use a piece-meal approach to experiment in various forms similar to the one she articulates. She offers a four tier hierarchy as a general model to explain the process of designing, running and critiquing experiments.[120] The following chart is a compilation of Mayo's (1996) Table 5.1 and 5.2.[121]

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[117]Chapters 5 & 7 in her *Error and the Growth Experimental Knowledge* are particularly useful for anyone wishing to apply her epistemology in analyzing historical episodes in experiment.

[118]Hankins (1985), Maxwell (1879), and, Heilbron (1979) to name but a few writers who have pointed to this as the source of his fame.

[119]Dorling's and Laymon's summaries provide stereotypic illustrations of this neglect; Laymon's less than Dorling's.

[120]She modeled her hierarchy on Suppes' (1969) hierarchy.

[121]Mayo (1996) p.130 for first three levels and p. 140 for fourth level. Mayo (1996), Table 5.2 on p. 140 provides a detailed table for all four levels.

Brief Overview of Mayo's Hierarchy of Models

PRIMARY MODELS:
How to break down a substantive inquiry into one or more local questions that can be probed reliably.

EXPERIMENTAL MODELS:
How to relate primary questions to (canonical) questions about the particular type of experiment at hand.

How to relate the data to these experimental questions.

DATA MODELS:
How to generate and model raw data so as to put them in canonical form.

How to check if the actual data generation satisfies various assumptions of experimental models.

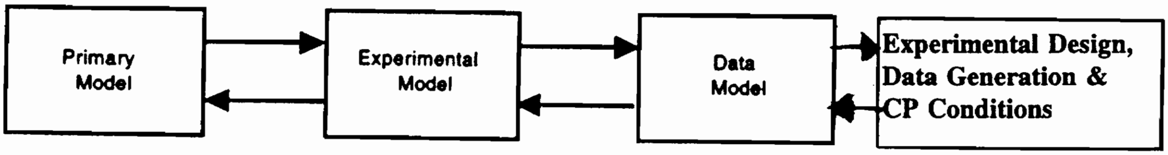
EXPERIMENTAL DESIGN, DATA GENERATION, AND CETERIS PARIBUS CONDITIONS:
Planning and executing data generation procedures:

Introduce statistical considerations via simulations and manipulations on paper or on computer.

Apply systematic procedures for producing data satisfying the assumptions of the experimental data model, e.g., randomized treatment-control studies.

Insure the adequate control of extraneous factors or estimate their influence to subtract them out in the analysis.

Information in her hierarchy flows both up and down from level to level interconnecting all the different stages in an experimental inquiry. Schematically this can be represented:[122]



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[122]See Mayo (1996) p. 128. I have added the fourth level into her original diagram.

Briefly, Mayo's piece-meal approach begins with experimenters splitting off and modeling into an experimental context a small piece of a larger global theory to test. Determining how data are to be gathered, modeled, and interpreted, as well as developing means for adequately controlling experiments in order to get meaningful results, forms the rest of her hierarchy and provides the crux of her approach. This process will become clearer as I briefly explore her hierarchy in the next few pages before using it to explore Cavendish's work.

One or more primary hypotheses or questions occupy the top of Mayo's hierarchy of models for experimental inquiry. The primary hypothesis is a small piece of a larger global theory broken off for investigation. Mayo gives a fairly detailed description of the function and characteristics of these primary questions:

> A substantive scientific inquiry is to be broken down into one or more local or "topical" hypotheses, each of which corresponds to a distinct *primary question* or *primary problem.* In a comprehensive research situation, several different piece-meal tests of hypotheses are likely to correspond to a single primary question. Typically, primary problems take the form of estimating quantities of a model or theory, or of testing hypothesized values of these quantities. They may also concern the form or equation linking theoretical variables and quantities.[123]

Once a primary question has been formulated, experimenters need to model or translate their question into a laboratory or testing setting. Mayo places this act of modeling under the heading of experimental models which forms the second level in her hierarchy:

> The experimental models serve as the key linkage models connecting the primary model to the data, and conversely. Two general questions are how to relate primary questions to (canonical) questions about the particular type of

[123]Mayo (1996, p. 129).

> experiment at hand and how to relate the data to these experimental questions.[124]

Experimenters devise their experimental models to accomplish two major tasks. First, at this level of an experiment, scientists formulate their primary hypothesis or question of interest into an experimental format. That is, they translate it into a testable setting. Often, this involves building apparati to "model" their question. Second, experimenters use their experimental models to set out how the data can (or cannot) answer their experimental questions. Thus, experimental models are used by scientists as an intermediary between their primary questions and possible experimental results. Conversely, they use their experimental models to relate their experimental results into answers about their primary hypothesis. An important point to keep in mind here is that this linkage is between modeled, not raw, data and a primary question. The modeling of raw data occurs at the next level down in her hierarchy.

Many philosophers and sociologists of science agree that the principal task of the experimenter is acquiring the data and manipulating it, "making the data talk."[125] Since data *can* be manipulated, the process of manipulation is also where objectivity can be most strongly challenged and defended. Mayo formulates her epistemology specifically to address the problems inherent in acquiring and modeling data:

> Modeled data, not raw data, are linked to the experimental models. Accordingly, two questions arise, one before the data are collected--"before-trial"--and one after the data are in hand--"after-trial." The Before-trial question is how to generate and model raw data so as to put them in the

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[124]ibid.

[125]I am referring to those philosophers and sociologists that I discussed in chapter 1. (Collins, Franklin, Hacking, Galison, Mayo, and Pickering)

> canonical form needed to address the questions in the experimental model. The after-trial question is how to check whether actual data satisfy various assumptions of experimental models.[126]

Data models serve both pre-test and post-test functions for experimenters. Experimenters employ before-trial data models to generate and manipulate data so that it can be used to answer their experimental questions. They employ after-trial models to check that the experiment was not compromised during the gathering and manipulation of the data, that their various experimental assumptions were met. It is at the level of data models that experimenters turn raw data not so magically into "givens".[127]

Mayo identifies a distinct fourth level below the data models for experimental design, data generation and *ceteris paribus clauses*:

> *Experimental Design, Data Generation, Ceteris Paribus Clauses*: Experimental design and *ceteris paribus* both concern possible flaws in the *control* of the experiment -- auxiliary factors that might be confused with those being studied. An adequate account of an experimental inquiry requires explicating all the considerations, formal and informal, that bear on the assumptions of the data from the experiment.[128]

In this chapter, I combine this fourth level in her hierarchy with her third level, data models. I have done this because there is no clear distinction between the two levels in his Globe experiment. (In chapter three, I re-instate Mayo's distinction between these two levels because formal statistical ideas play an important part in his experimental argument.) I believe my ability to do this supports the flexibility of Mayo's model; a

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[126]Mayo (1996, p. 129).
[127]Dr. Richard Burian first brought it to my attention that the term "data" literally means "given".
[128]Mayo (1996, pp. 139-140).

characteristic that makes it a useful tool for historical research. I will now use her hierarchical framework to investigate Cavendish's Great Globe experiment.

(2.4) Experiment 1: The Great Globe on its Stand

Cavendish clearly stated the primary question he designed his Great Globe experiment, Experiment I, to answer:

> EXPERIMENT I. The intention of the following experiment was to find out whether, when a hollow globe is electrified, a smaller globe inclosed within it and communicating with the outer one by some conducting substance is rendered at all over or undercharged; and thereby *to discover the law of the electric attraction and repulsion.*[129]

That the electrical fluid's attractive properties could be described by an inverse square force law was one estimated quantity in Cavendish's large-scale theory he felt he could measure. As I discussed in chapter one, if any sense of objectivity is to be captured in scientific knowledge, it will be found in experimental evidence. Thus, Cavendish needed to model this piece of his larger theory into an experimental context as a prerequisite for gaining objectivity for his scientific inferences. How did Cavendish relate his primary question to a laboratory environment? To answer that question I must move down the hierarchy to what Mayo calls experimental models.

One reason Cavendish's "Great Globe," or spherical condenser, experiment provided him with such a good experimental model is that it was an actual model, a physical replica, of his theory. Cavendish constructed a working model of both his

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[129]Article 218, emphasis mine.

borrowed Newtonian mathematics and his own incompressible canals of fluid when he built his Great Globe apparatus.[130]

18] LEMMA IV. Let BDE, bde, and $\beta\delta\epsilon$ (Fig. 2) be concentric spherical surfaces, whose center is C: if the space* Bb is filled with uniform matter, whose particles repel with a force inversely as the square of the distance, a particle placed anywhere within the space Cb, as at P, will be repelled with as much force in one direction as another, or it will not be impelled in any direction. This is demonstrated in Newton, *Princip.* Lib. I. Prop. 70. It follows also from his demonstration, that if the repulsion is inversely as some higher power of the distance than the square, the particle P will be impelled towards the center; and if the repulsion is inversely as some lower power than the square, it will be impelled from the center.

Fig. 2.

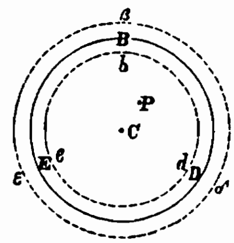

Figure 1. *Cavendish's mathematical diagram of his Newtonian mathematical treatment of electrical force. (See my Figure 3 (p. 64) to compare this diagram with the "Globe" apparatus he built for his experiment.)*[131]

The two globes represented a concrete realization of his (and Newton's) geometrical proof for the actions of inverse force laws in a sphere. Once the outside of the outer globe was charged, if Cavendish could successfully isolate his two globe system from any other influence, only an inherent electrical force should move charges along the connecting wire between them. Thus, the state of the inner globe (over, under, or un-charged) should reflect which inverse force law described electrical attraction and repulsion.

[130]Hackmann (1989) has called attention to the popularity of this experimental tactic among experimenters during this period. Cavendish used this tactic of physical simulation in another famous experiment wherein he built a model of the torpedo, an electrical fish, to show that the shocks produced by those fish were in fact electrical. (See Articles 194-215.)

[131]Article 18.

One advantage of his experimental model lay in the fact that the inner globe could only be in one of three physical states: overcharged, undercharged or un-charged. Cavendish had proven mathematically that only the operation of an inverse square law should result in the globe displaying no net charge, the null result. If the force of electrical attraction varied inversely between the second and third power, the globe should be overcharged. If the force law varied inversely as less than the square, the globe should be undercharged.[132] Thus, I suggest that Cavendish's experimental model, the Great Globe, connected possible data readings (over, under, or no charge) to his primary question about the possible nature of electrical forces. It is important to note that while the inner globe could only be in one of the three distinct states discussed, Cavendish did not assign three distinct integer values to the action of possible force laws. That is, Cavendish did not rule out non-integer values for the strength of electrical forces.[133]

One major assumption of Cavendish's experimental model was that the *only* factor acting between the two globes was an inherent attractive property of the electrical fluid. The question of paramount concern now is: did his experimental model keep its promise? That is, could Cavendish acquire objective data from his experimental model that he could use to evaluate his primary hypothesis? Another way to conceive Mayo's hierarchy of models is to consider them to be a series of checks and balances. To see if the apparatus Cavendish built to represent his experimental model permitted him to conduct a severe

[132]Cavendish, Art. 232 & 233. Mathematical reasoning found in his theoretical papers, Arts. 18-27.

[133]Depending on the sensitivity of his experimental apparatus and his error calculations, his experimental model was open to measuring a spectrum of inverse force laws between 1 and 3.

test of his prediction, I must continue down Mayo's hierarchy and deeper into the heart of Cavendish's experiment.

The part of Laymon's quotation that I previously emphasized bears repeating here because it is a succinct summary of the before-trial data questions Cavendish had to deal with: "By using concentric shells in the way indicated, Cavendish had in essence reduced the experimental complications to those associated with adequately insulating the shells and avoiding induced charges due to nearby conductors."[134] The major source of error and the one Cavendish was most concerned about was "the running off of electricity"[135] or induction in modern terminology. He took several measures to eradicate and minimize this primary source of error in all his electrical experiments.[136]

Cavendish made four major modifications to his experimental apparatus specifically to control possible sources of error and improve the accuracy of his data. These changes included three basic design modifications, improved insulation by coating his glass stands with sealing wax, making and using a standard Leyden jar to charge his apparatus, and pre-charging his pith ball electrometer for greater sensitivity. I will discuss each in turn. The point that I want to illustrate is that none of these modifications were "automatic." Rather each resulted from Cavendish's attention to error as he interacted with and "got to know" his new equipment. Furthermore, he conducted separate

[134]Laymon, p. 28.

[135]Art. 264. He also termed such worries as "the electricity could spread itself" (art. 225) or "escape" (art. 227).

[136]This same concern runs throughout the other experiments comprising his series which I discuss in chapter 3.

experiments or sub-experiments to test for and resolve errors in his apparatus.[137] As Cavendish himself admitted regarding his experimental set-up, "It is more complicated, indeed, than was necessary, but as the experiment was of great importance to my purpose, I was willing to try it in the most accurate manner."[138] Collins, Franklin, and Pickering all discuss the challenges faced by experimenters using new equipment.[139] The question which I need to answer is "Did Cavendish's experimental "tinkering," to use Pickering's term, pose a threat to the objectivity of his experimental results or did his changes reflect the objectivity of his procedures?" To answer this question, I must descend to the bottom two levels of Mayo's hierarchy which, as I discussed earlier, I have combined into one.

Cavendish designed his "complicated" experimental apparatus to facilitate the rapid execution of his tasks and, thus, reduce the effect induction might have on his results. The experimental routine he needed to accomplish quickly consisted of: electrifying the hemispheres, separating and discharging them, and applying a pith ball electrometer to the inner globe to measure its charge.[140]

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[137]Specific examples are his tests on the conductivity of glass and the effect of charge strength on the rapidity with which the electrical fluid spread.

[138]Article 221.

[139]Collins (1994); Franklin (1994); Pickering (1994).

[140]He described this electrometer in Art. 244: "The balls were made of pith of elder, turned round in a lathe, about one-fifth of an inch in diameter, and were suspended by the finest linen threads that could be procured, about 9 inches long." The presence of electricity was detected when the balls separated. Note: his electrometer was not dependent upon (or theory laden by) an inverse square law.

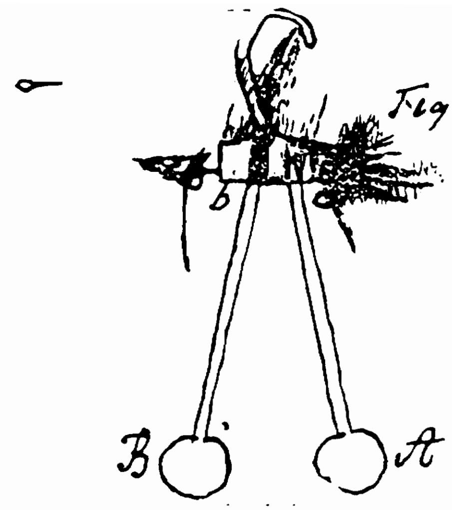

Figure 2. *Cavendish's drawing of his pith ball electrometer.*[141]

His apparatus evolved through three distinct stages. The apparatus he used during his first recorded attempt at this experiment was designed to allow "the two hemispheres [to] slide on two sticks of glass by means of two tin hooks and a stick of glass fixed to the back of the hemisphere."[142] Even at this rudimentary stage of his experiment, Cavendish used a clever arrangement of silk strings that allowed him with one motion of his hand to complete four separate tasks: (1) lift up the wire charging the outer globe, (2) lift up the wire connecting the two globes, (3) separate and remove the hemispheres, and (4) place his electrometer on the inner globe. This was a pretty amazing machine and it was only the first of three models, a mere rough draft.

Cavendish's second apparatus took the following form: "The experiment was tried in a different manner, the hemispheres being fastened by sticks of glass covered with sealing wax within wooden frames turning on hinges."[143] Cavendish replaced the tin

[141]Article 249; Cavendish's Figure 30. The pith balls separated in the presence of electricity. the stronger the electricity, the farther apart the balls would separate.
[142]Article 512.
[143]Article 513.

hooks of his first model with "wooden frames turning on hinges." He did not discuss his reasons for this modification other than to state that he was trying the experiment in a different manner. However, this second model was clearly a precursor to his third and final one. He explained that he designed and built his third apparatus in order to both increase speed of operation and accuracy of measurement in his experiment.[144] I assume these same considerations drove him to develop his intermediate apparatus as well.

Cavendish's third, and final, version of his experimental apparatus was a wooden frame to which the hemispheres were fastened by waxed glass sticks.

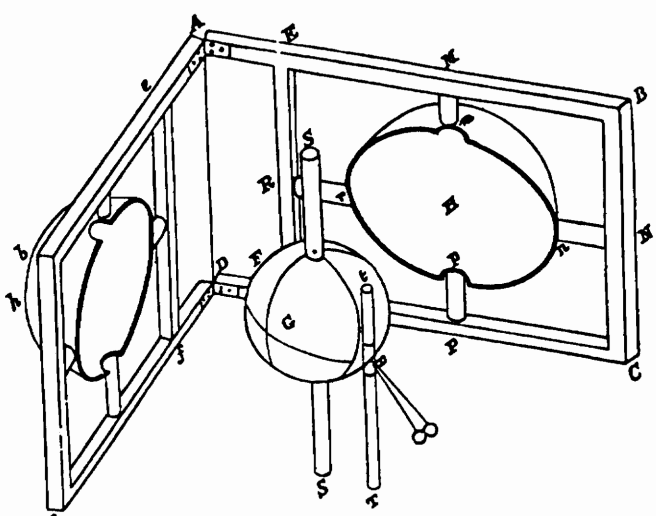

Figure 3. *Cavendish's Great Globe apparatus.*[145]

[144]Throughout his Great Globe experiment, Cavendish emphasized the need for speed in order to reduce errors in his data from "the electrical fluid running off," which justified his tinkering.

[145]Article 221; Figure 12. "[T]he frame of wood by which *Ss* and the hinges at *A* and *D* are supported being not represented in the figure to avoid confusion." Cavendish's hand drawn sketch is in Article 217; also labeled Figure 12.

This wooden frame was itself supported by another frame of wood (not shown in Cavendish's illustration to avoid confusion). Some of the further refinements Cavendish incorporated in his third model included:

> It was also contrived so that the electricity of the hemispheres and of the wire by which they were electrified was discharged as soon as they were separated from each other, as otherwise their repulsion might have made the pith balls to separate, though the inner globe was not at all over charged. The Inner globe and hemispheres were also both coated with tinfoil to make them the more perfect conductors of electricity.[146]

Cavendish was able, with his new apparatus, to immediately discharge (ground) both the hemispheres and charging wire to reduce their inductive effects on his results.[147] Also, the inner globe and hemispheres were coated with tinfoil. Cavendish knew tinfoil was a good conductor.[148] He took further advantage of tinfoil's conductivity when he attached a piece of tinfoil to the point on the glass stick where his pith ball electrometer hung. The tinfoil readily conducted any charge from the inner sphere to his electrometer for measurement.

Cavendish stated that he was careful to insure that the hemispheres closed tightly together, did not touch the inner sphere, and were separated from the inner sphere in all parts equally.[149] This was important for one of his experimental assumptions was the inner and outer globes were connected only after the outer globe had been charged and then only by a wire which represented one of his incompressible canals.

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[146]Article 222.

[147]Art. 222. Grounding ("destroying" or discharging electricity) was a well-known effect and used practically in lightening rods.

[148]Article 221. Cavendish and others knew that certain materials, like metals, conducted electricity more readily than others, like glass and silk. As glass and silk did not readily conduct electricity, they were used as insulators in electrical apparati.

[149]Article 221.

Cavendish frequently conducted separate sub-experiments to reproduce or simulate sources of error that he suspected were in operation.[150] This tactic of stimulating errors is itself a canonical model for learning about and controlling errors.[151] Cavendish argued that if an error was occurring, he could detect it by devising a sub-experiment to amplify the suspected error. His sub-experiment, if it found the error (which if it was occurring, the experiment almost would surely find out), would then allow him to take steps to minimize or eradicate the error. For example, he argued that if his glass insulating stands were allowing the electricity to flow along them, then he could amplify this suspected error by directly charging the glass itself, and so detect glass's inadequacy as an insulator. He *did* detect that the glass held charge. He corrected the problem by coating his glass stands with sealing wax.

Likewise, he checked the effect of different charge strengths on the running off of electricity (induction) and the effect of the air being made over or under-charged by the electricity in the wires of his apparatus. As this tactic was important in Cavendish's work, and is moreover a canonical example of an "argument from error,"[152] I want to explore some of the implications of its use for the objectivity of experimental practice by looking at his glass sub-experiment in some detail.

Cavendish found a major technical problem with his apparatus during his first attempt, the "tin hook" model, of his Globe experiment. Cavendish suspected that the glass sticks, nonconductors, in his equipment were not providing the amount of insulation

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[150]Mayo emphasizes the importance of such tactics (Mayo 1996, p. 6).
[151]See my chapter 1, pp. 21-23 on learning from error and pp. 37-38 on canonical models.
[152]See my chapter 1, pp. 23-24 for a discussion on arguing from error.

either expected or required. He tested his hypothesis that the glass sticks were conducting electricity by running a separate experiment in which he simulated this error. He electrified the sticks and found that, in fact, the glass was allowing electricity to flow along it "pretty readily."[153] Next, he resolved this problem by making these glass sticks "the more perfect nonconductor[s] of electricity" by coating them with sealing wax.[154] He kept this modification in all future versions of his Great Globe apparatus. This sub-experiment formed an integral part of his evolving technological apparatus. While his solution sounds simple -- coat the sticks in sealing wax -- it was not "automatic" but the result of a separate experiment to locate, isolate, and resolve a specific problem. This sub-experiment also illustrates experimental knowledge being produced. In this case, the knowledge gained concerned the inadequacy of glass as an insulator. The glass support rods conducted sufficient electricity to cause noticeable error in his experiment and the addition of sealing wax resolved this problem. Why did Cavendish make such a fuss about re-insulating his glass rods? Silk strings and glass rods comprised the insulating stands used in the new electrical equipment. Cavendish's addition of a coat of sealing wax to his glass insulators was part of his coming to terms with common, but still somewhat unknown, electrical apparatus and its fallibility. This was a bit of low-level experimentally determined knowledge he discovered independently of his larger global theory. The broader lesson this reading of Cavendish's work yields is that laboratory

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[153]"If the inner globe was electrified after the hemispheres were separated, it was a great while before the pith balls closed. This was...owing to the sticks of glass on which the hemispheres slid being electrified thereby *as the same phenomena were produced by electrifying those sticks when the hemispheres were taken off.*

[154]Article 512.

instruments are not open books dispensing pure experimental knowledge but can be riddled with error. Moreover, contra Collins' regress, Cavendish's use of sub-experiments to amplify possible errors shows how experimenters check and fix their apparatus independent of detecting what they are searching for.

Cavendish emphasized the importance of re-insulating his glass support rods because the main assumption of his experimental model was that *only* the inherent attractive properties of the electrical fluid would be acting upon the inner and outer globes during their communication by the connecting wire. If this was not the case, if in fact there was electricity running upon the glass supports and acting on the inner globe while the outer one was being charged, then his results could not indicate which inverse force law described electrical attraction. His experimental set-up would not give an objective measure of force, because other factors would be clouding Cavendish's measurements. The apparatus used to determine the law would itself be influencing the outcome. Thus, Cavendish's sort of "tinkering" helped convince him his results were an objective measurement of the law of electrical force only to the extent that he had eliminated other causal factors.[155]

Cavendish's third major experimental refinement to reduce error was to use a charged coated jar, a mini Leyden bottle, to electrify the hemispheres. This innovation was again inspired by possible sources of error which he used the jar to overcome:

> My reason for using the glass jar was that without it it would have been difficult either to have known to what degree the hemispheres were

[155]Of course, Cavendish did not use the term "objective." Cavendish preferred to claim "truth" for his findings. However, I think no one will find my using the term objective at odds either with his practices or intents.

> electrified or to have kept the electricity of the same strength for a second or two together, and if the wire had been suffered to have rested on the hemispheres while the jar was charging, I was afraid that the electricity might have spread itself gradually on the sticks of glass which supported the globe and hemispheres, which might have made some error in the experiment.[156]

Cavendish used the coated jar for several reasons. First, it allowed him to control and know the degree to which the hemispheres were electrified. Second, it kept the electricity at the same strength for the length of the operation. Third, it provided a means of charging the globes neatly with less chance of inductive interference from nearby conductors.

But how did he know to what degree the jar and thus the hemispheres were electrified? Cavendish made a calibration scale to standardize his procedure for charging the jar. He attached an electrometer to the prime conductor with which he charged the jar and placed a "piece of pasteboard, with two black lines drawn upon it...six inches behind the electrometer on a level with the balls, in order to judge of the distance to which the balls separated."[157] He made a guide for the eye thirty inches away because:

> it is evident that the nearer the eye is placed the further the balls will appear to separate. But as the distance of the balls from the eye is so much greater than their distance from the pasteboard, a small alteration in the distance of the balls either from the eye or the pasteboard will make no sensible alteration in the distance to which the balls appear to separate."[158]

Had he not made such a guide, his readings would have been less accurate and have greater variability from run to run. By using an eye guide, Cavendish could not

[156]Article 225.
[157]Article 249.
[158]Article 249.

subconsciously bias his results by moving closer to or further away from his standard separation chart depending on the trial being run (i.e., by incremental changes in his viewing distance, he could induce the reading or result he preferred). This attention to detail was one reason Cavendish was such an exemplary experimenter. Details of this type were the focus of Cavendish's experimental argument, though until very recently unmentioned in meta-studies (philosophy, sociology, etc.) of experiment.

Cavendish ran another sub-experiment to check what effect different strengths of charge would have on inducing error into his experiment. Cavendish devoted two entire articles to justifying his preference for weakly charging the Leyden vials he used in his experiments. Here is the crux of his reasoning:

> My reason was this, --that the electricity seems to escape remarkably faster from any body, both by running into the air and by running along the surface of the non-conductor on which it is supported, when the body is electrified strongly than when it is weak, which made me afraid that if I had charged the vials much stronger the experiment might have been too much disturbed.[159]

Cavendish discovered experimentally that weakly charged objects did not allow the electrical fluid to "run-off" as quickly as strongly charged objects. Cavendish designed this sub-experiment, like his glass insulator sub-experiment, to test and gain experimental knowledge. And again, this knowledge was the result of his probing for sources of error in his new experimental equipment. Moreover, it is just this sort of low level attention to detail that is the hallmark of procedural objectivity. Already at this early stage in his

[159]Article 264. Cavendish also ran checks on the conductivity of air (Article 256).

experiment, his "one" experiment is beginning to resemble a Russian doll with experiment inside of experiment.[160]

Cavendish's fourth and last major modification to his experimental apparatus centered on increasing the sensitivity of his pith ball electrometer. He pre-charged the pith balls until they were separated approximately one inch before applying them to the inner globe as "a much smaller degree of electricity may be perceived in the globe."[161] Cavendish knew from experience that a larger amount of electricity was required to overcome the initial resistance of the pith balls than that required to make them separate further.[162] Thus far I have covered Cavendish's before-trial data checks. These consisted of adjusting his apparatus to reduce the effects of induction or the spreading of the electric fluid. Let me quickly review them before moving on.

Cavendish devised, built, and modified an elaborate framework to carry out his experiment as quickly as possible. He doubly insulated his glass stands with the addition of a coat of sealing wax. These modifications curbed the threat of error from the electrician's nemesis--induction. He devised a scale to standardize electrical strength and used a glass jar to electrify the hemispheres in order to know the degree and maintain the same degree of electrification for all runs of his experiment. Finally, he pre-charged his pith ball electrometer to increase its sensitivity. Cavendish instigated all these checks in

[160]It is also important to note that designing and preparing experimental apparatus and instruments are an integral part of "doing" an experiment.

[161]Article 229.

[162]A similar example of this sort of initial resistance versus further sensitivity will be familiar to anyone who has ever had to push a car off the highway. It takes a lot of energy or push to get the car rolling; however, once the car is moving it takes much less push to keep it moving and even building up speed. Once the pith balls overcame their initial resistance, it took much less push or electrical force to keep them separating farther apart. They were pre-sensitized to the presence to electricity. Cavendish explained this increased sensitivity in Art. 228 & 229.

order to mitigate experimental and technological complications which could skew his experimental results. He attempted to reduce his experimental complications down to the point where only electrical attraction or force should transfer charges between the outer and inner globes. Furthermore, Cavendish's evolving apparatus and emphasis on reducing errors in his experiment can be viewed as an attempt to construct a severe test of his predicted inverse square force law for describing the effects of electrical attraction and repulsion. Severe in that he devised a stringent test of his hypothesis. This required him to adjust his apparatus so that he could argue his electrometer's readings (his data) were not influenced by his apparatus, the surrounding environment or himself.

I will briefly sketch Cavendish's overall experimental argument and one paradigmatic example of his usual sub-arguments to show how they fit together into a coherent whole. Analytically, using Newton's calculus, Cavendish knew in a spherical set-up like he had built, only an inverse square law would account for the inner globe remaining uncharged. Moreover, if the inner globe was overcharged, an inverse cube law would describe electrical force and an inverse linear law would be the case if the inner globe was undercharged. This analysis would hold only if he could show that no other outside factors were influencing the transfer of charge from the outer to the inner globe. Thus, his main concern and over-arching premise in this experiment was that no outside factors influenced his experimental set-up. He insubstantiated this premise through his several independent sub-experiments. His glass sub-experiment shows the standard argument from error he gave. The null hypothesis was that electricity did not flow along glass. The alternative was that glass would conduct some electricity. To amplify this suspected phenomena (error), Cavendish directly electrified the glass and then using his electrometer measured for charge. The glass did have a charge. Therefore, the null hypothesis was false. He next insulated his glass with sealing wax so that the glass stands now were much better insulators. He gave similar arguments for his use of low power to charge his globes, et cetera. Using these sub-experiments (error probes) he argued his premise of no outside influences was met. This allowed him to conclude that his electrometer's reading indicated well which force law described electrical attraction and repulsion *and* that it was highly unlikely that the inner globe would remain uncharged unless his hypothesis of an inverse square force law was false.

After the third and final version of his Great Globe experiment, Cavendish claimed the force law governing electricity was an inverse square based on the null result he had measured. Cavendish's argument for the objectivity of his experimental result relied on his elimination of the two possible sources of error that he was aware of-- induction and inadequate insulation[163] But how sensitive was his experiment? It would not be a severe test of his hypothesized inverse square law if the experiment was not sensitive enough to discriminate between the various inverse laws under test.[164] That is, Cavendish needed to know if his null result was due to the ability of an inverse square force law to describe electrical attraction or due to his electrometer simply not being sensitive enough to detect whether or not the inner globe was under or over-charged. Qualitatively, the pre-charged sensitivity of his electrometer suggested that his experiment

[163]Again, while Cavendish did not use the term "objective," and instead claimed "truth" for his findings, I think no one will find my using the term objective at odds either with his practices or intents.

[164]Remember, his experiment was designed to show whether a linear, square or cube inverse force law would describe the effect of electrical attraction and repulsion.

was fairly sensitive. But could Cavendish give a more precise, quantitative measure of the sensitivity of his globe experiment? That question brings me, along with Cavendish, to his after-trial data models.

Cavendish decided to "find how small a quantity of electricity in the inner globe might have been discovered by this experiment."[165] He removed the hemispheres so that the inner globe and pith balls stood alone. Next, Cavendish:

> took a piece of glass, coated as a Leyden vial, which I *knew by experiment* contained not more than 1/59 of the quantity of redolent fluid on its positive side that the jar by which the hemispheres were electrified did, when both were charged from the same conductor.[166]

He electrified this mini-Leyden jar or plate condenser to the same degree as the jar had been in his globe experiment. Next, Cavendish grounded his original jar to "empty" it of any electrical fluid and re-charged it with his Leyden plate. The jar now "contained only 1/60th part of the redundant fluid in this experiment that it did in the former [globe experiment]."[167] Finally, he directly electrified the inner globe with this jar and using his pre-charged pith ball electrometer measured the amount of charge thus given to the globe.

Cavendish found that:

> The result was that by previously electrifying the balls, ... the electricity of the globe was very manifest, as the balls separated more when they were previously electrified positively than when negatively, but the electricity of the globe was not sufficient to make the balls separate, unless they were previously electrified.[168]

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[165]Article 230.

[166]Article 230, emphasis mine. The italicized part forms the basis of Laymon's circularity argument.

[167]Article 230. Laymon (1994, fn 29) gives a clear exposition of Cavendish's mathematical calculations for his sensitivity check.

[168]Article 230.

Here, he has repeated his qualitative assessment that the amount of electricity was too small to measure unless the pith balls were pre-charged. Moreover, Cavendish argued, based on this after-trial check, that the amount of electrical fluid communicated to the inner globe by the outer hemispheres must have been less than "1/60th part of that communicated to the hemispheres."[169] Otherwise, he would have gotten a reading on his pre-charged electrometer in his original Great Globe experiment.

Using this knowledge about the sensitivity of his experiment, Cavendish calculated "how much the law of the electric attraction and repulsion may differ from that of the inverse duplicate ratio of the distances without its having been perceived."[170] That is, Cavendish did not assume an inverse-square law then see how far off his experimental result was, he wanted to see how far off the "real" law could be from his experimental proof of an inverse square.[171] His experiment was designed to detect a range of inverse force laws between one and three. By quantitatively calculating the sensitivity of his experiment, he could then determine the range of variation an actual force law could take from his experimental result. His calculations revealed: "that the electric attraction and repulsion must be inversely as some power of the distance between that of 2+1/50th and that of 2-1/50th, and there is no reason to think that it differs at all from the inverse duplicate ratio."[172]

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[169]Article 230.

[170]Article 234.

[171]This shift in emphasis, I believe supports my contention that Cavendish's experiments warranted his belief in an inverse square law rather than the reverse. See my footnote 89, 2nd paragraph.

[172]Article 234. [See Laymon (1994, fn 29) for calculations.] Cavendish did not feel that he could ever get rid of all and any error in his experiment. The closeness of his results, however, to his prediction indicates well that "the inverse duplicate ratio" to describe electrical force held. I discuss problems with his view in chapter four.

Dorling has raised the issue that Cavendish assumed an inverse force law would describe electrical attraction and repulsion and "as soon as the *n*th-power law assumption is examined critically, it becomes clear that it must be rejected as unacceptable for Cavendish himself and for his contemporaries."[173] Moreover, Dorling continues: "I would emphasize that his objection to Cavendish's major premise is not a philosophical or logical objection but a technical historical objection."[174] Dorling then suggests that *given* the implausibility of this assumption had "Cavendish repeated his experiment with all possible radii of inner globes and all possible radii of outer globes (including of course arbitrarily large ones) then he could have used Laplace's theorem instead of his own inverse-power law assumption in order to infer the inverse square law result."[175] First, I again mention the fact that Dorling himself is inconsistent about the historical plausibility of inverse power laws during this period for three pages later he asserts they were quite plausible.[176]

Was Cavendish's null result merely happenstance due to the particular size of spheres he used? His experiment II formed an after-trial check that his results in Experiment I were not a spurious artifact of his Globe apparatus. Rather than varying the size of his globes, he ran a similar experiment using an entirely different shaped enclosure, a box. Cavendish did not, of course, calculate or make a mathematical

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[173]Dorling, p. 334.

[174]ibid. Fortunately, Dorling was not around to tell Cavendish that he should have objections to *n*th power laws if he wanted to fit into his historical period!

[175]ibid. pp. 335-336.

[176]"But it is difficult to see how he [a hypothetical skeptic] could convincingly argue that an approximate inverse-square law was anything less than *quite probable* prior to the experimental result (Dorling, p. 339 (emphasis mine)." Dorling argues the work of Bernoulli (1760), Priestly (767) and Robison (1769) supports this claim.

prediction for a box enclosure. He simply built a symmetrical physical system mirroring his globe apparatus.[177] Cavendish conducted this second experiment to check that the force law he determined in his first experiment was not contingent on the shape of his test objects.

Cavendish summed up his conclusions from his after-trial box check in article 235:

> The experiment was tried in just the same manner as the former. I could not perceive the inner box to be at all over or undercharged, which is a confirmation of what was supposed at the end of Prop. IX [Art. 41]--that when a body of any shape is overcharged, the redundant fluid is lodged entirely on the surface, supposing the electric attraction and repulsion to be inversely as the square of the distance.

This final data check assured Cavendish that the result of his globe experiment was not due to any fortuitous circumstances of his apparatus. The force law he determined in his Globe experiment also held for other geometrically shaped systems. His argument follows the pattern of an argument from error. It would be an error to infer an inverse square law if it only worked for or was an artifact of his Globe apparatus. This seems a good time to summarize and see how an error interpretation has given a facelift to Cavendish's Great Globe experiment so far.

(2.5) Preliminary Conclusions & Remarks

In a nutshell, what Cavendish did was appropriate Newton's mathematical results and experimental good fortune with gravity for his own electrical researches (with the exception of his canals of incompressible fluid). However, Cavendish did not "believe" electricity must conform to his Newtonian analogy. Cavendish's Newtonian analogy

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[177]That is, he simply replaced his spheres with a similar arrangement of boxes.

opened an experimental door for him, but his inference that an inverse square force law described electrical attraction and repulsion came from the experiments he ran. Furthermore this inference seems warranted because of his ability to give an argument from error.

Contra Dorling,[178] Cavendish did not perform one simple experiment from which he deduced an inverse square law described the action of electrical forces. Using Mayo's epistemology to look at Cavendish's Great Globe experiment emphasizes that his final decisive experiment was based on a minimum of four separate experiments or sub-experiments. Especially important to his Globe experiment were the two sub-experiments he ran to check that his experimental assumptions were being kept. He used a weak strength of electricity because he had experimentally determined that stronger strengths induced charge more rapidly into the surrounding air and nearby conductors which would affect his results. His experiment on the conductivity of glass led him to double insulate his glass stands with sealing-wax, an important modification to stop his experimental apparatus from affecting his results. Cavendish's Experiment II, the box, was a separate yet integral part of his Great Globe experiment. This allowed him to argue that his result from Experiment I was not a chance effect of his spherical design. Finally, Cavendish's calibration of his mini-Leyden plate for his sensitivity analysis made this an extremely sensitive experiment. This combination of tactics allowed him to argue that were his hypothesis false, his experiment would have discovered this fact as his result would have

[178]See my section 2.2.

been further away (certainly more than 1/50th) than it was.[179] Thus, Cavendish's experiment clearly shows that the robustness of a particular inference is inextricably anchored by arguments about the procedural objectivity in a specific testing context.

I have shown Cavendish's evolving and complicated apparatus was the result of his concern with error and to insure his experimental assumptions were met. Unlike Pickering's view of experimenters "tinkering" with their equipment until they get the result they want, I have suggested, based on Mayo's epistemology, a different view of experimental evolution driven by error considerations and a search for objectivity. Cavendish's modifications were developed to exclude interference from induction in his results and increase the sensitivity of his apparatus. The point I want to emphasize is Cavendish's technical modifications were made to ensure the severity of his argument or test. It was these low level experimental tactics that warranted his inference that the force law is an inverse square not the reverse as Pickering suggests.[180]

Cavendish's work also illustrates how experimenters get around Collins' regresses. The persuasive power of Cavendish's experiment and his detection machine, the Great Globe, rested not on his having discovered an inverse square law, but rather that he could show, using the localized experimental tactics which I have covered, that other factors

[179]Another way to state his overall argument is: First, he had using the tactics outline in this chapter, successfully isolated the outer and inner globes from outside electrical interference so that the transfer of charge between them could only be due to an inherent electrical force. Second, given the above and his sensitivity analysis, the closeness of his result to his hypothesis of an inverse square law to describe electrical attraction and repulsion indicated well the correctness of his hypothesis.

[180]Pickering does not discuss Cavendish, his suggestion is based on his own quark case study which he feels can be generalized. In his study of Morpurgo, he suggests that the quark (1/3 charge) theory Morpurgo held caused him to tinker with his apparatus until he got a result which agreed with his theory. It was because of Murpurgo's insistence on getting this result, according to Pickering, that Morpurgo rejected his 1/10 charge result--based on theoretical considerations rather than error considerations such as I suggest caused Cavendish to "tinker" with his apparatus. Pickering (1994, pp 113-117).

(known or merely suspected) were not influencing his results. This allowed him to distinguish or isolate what he was looking for, the movement of charge based on an inherent attractive property of the electrical fluid, from sources of known and suspected interference due to his procedures which I have covered throughout this chapter.[181] Cavendish's emphasis was on trimming away possible errors to show his experimental apparatus or detection machine was working rather than finding the result to prove his equipment was working.[182]

Did Cavendish successfully devise and carry out a severe test? My answer so far is a qualified yes. His test was informally severe in that he had gotten rid of or minimized every known possible source of physical interference, leaving only inherent electrical forces operating as demanded by his experimental assumptions. His Experiment II, the box, showed his results were not an artifact of his Globe apparatus. Most importantly for meeting a criterion of severe testing and, thus, objectivity, he ran after-trial checks on the sensitivity of his experiment and found it to be quite sensitive. The biggest problem for judging his test severe is the circularity problem that Laymon brought up regarding Cavendish's sensitivity measurement. I believe that this problem and other experimental assumptions made by Cavendish, which I have not yet discussed, were

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[181]Moreover, because he was testing for a mathematical description of an effect, the mode of transportation of charge (e.g., fluid, particle, etc.) was in many ways irrelevant to his results provided he could isolate and control the movement of charge. Cavendish *was not* testing whether his larger theory of infinite canals was accurate, but only if one piece of that theory was correct, (i.e., that an inverse square law would describe the effect of electrical attraction and repulsion.)

[182]Collins could argue that the experiment was designed so that at least one of the inverse force laws had to be "proven." I would not disagree at this stage. However, I suggest looking at the rest of Cavendish's experiments in this series, which I cover in the next chapter, will help overcome this hypothetical objection that I have ascribed to Collins and furthermore shows his replication regress will not hold either.

addressed by him in his experiments III-VIII. Using Mayo's framework with its emphasis on gathering and modeling data clearly indicates that Cavendish's experiment does not stand alone. Rather this "crucial" experiment is dependent on or enmeshed in a series of electrical experiments and sub-experiments. This view brings up another important point of contrast between my account and Dorling's for historians. Using Mayo's epistemology to understand Cavendish's Globe experiment forces one to look into his entire series of experiments (much as Cavendish's own exposition seems to do) to understand and discuss it in context. Dorling's Bayesian explanation of the rationality of Cavendish's belief in his results ignores not only all the experimental complications he faced and his resolution of them, but also the rest of Cavendish's series of experiments of which his Globe experiment was only the first of eight. I will now look at these subsidiary experiments for they form an integral part of his Great Globe experiment which cannot, therefore, be adequately analyzed without considering them.

Chapter 3: Satellite Experiments of the Great Globe

In this chapter, I explore the rest of Cavendish's series of electrical experiments of which his Great Globe was the first of eight. I cover his use of statistical ideas in both his experimental design and modeling of his "raw" data. I also address Laymon's contention that Cavendish's sensitivity analysis in his Great Globe experiment was circular but I argue fairly un-problematic when viewed within the context of this series as a whole. Finally, I argue both Collins' experimenter's regress about experimental repetition and Pickering's view of experimental "tinkering" will not stand up to scrutiny in this case.

(3.1) Overview

Laymon points out and Cavendish noted that Cavendish's final error calculation for his Great Globe experiment depended on results acquired from another experiment.[183] Experiment V, according to Laymon, was the crucial experiment that allowed Cavendish to calibrate his test plate. I believe Laymon's picture of experimental dependency is even more complicated.

Cavendish wrote 58 articles describing experiments III-VIII.[184] He discussed the primary hypothesis under investigation in his first two articles. In the next 26 articles, Cavendish described the apparatus he used, the specific errors arising from it, and his methods for overcoming these errors and obtaining accurate data. Next, Cavendish wrote 25 articles describing his individual experiments and their results. The last four articles contained Cavendish's general conclusions not only about the preceding six experiments

[183]Article 230: "I then took a piece of glass, coated as a Leyden vial, *which I know by experiment* contained not more than 1/59th of the quantity of redundant fluid on its positive side that the jar by which the hemispheres were electrified did, when both were charged from the same conductor." (emphasis mine)

[184]There are considerably more articles referenced regarding data, sub-experiments, etc.

but also on experiments I and II. I will argue all eight experiments in Cavendish's series, through mutual re-enforcement, formed one large experiment.[185]

(3.2) Primary Hypothesis & Experimental Model for Experiments III-VIII

The first thing Cavendish did in his introduction to this series of experiments was lay out his primary hypothesis: "to examine how far that proportion [of electrical fluid] agrees with what it should be by theory if the bodies were connected by canals of incompressible fluid."[186] He wanted to experimentally determine the amount of electrical fluid different bodies could contain and compare his results to his theoretical predictions based on an inverse square force law and his canals. He tailored his experiments by varying his test bodies and experimental conditions to test this primary hypothesis. His general argument behind this series of experiments once again can be seen to follow the pattern of an argument from error. If the law describing electrical attraction and repulsion was an inverse square (as shown in Experiment I) and his canals of incompressible fluid were well modeled by thin wires, then his experimental results in the next six experiments should not be far from those calculated using his theory.[187] Moreover, given the variations introduced by each experiment, it was extremely likely that were these assumptions false, then his theoretical calculations based upon them would be far from his experimental results. In short, Cavendish could assume his inverse square law and canals because given the multitude of experimental variations he was about to

[185]Or as Cavendish's editor, James C. Maxwell put it: "We have next to consider the steps by which he established the accuracy of his theory...Cavendish himself, in his description of his experiments, has shown us the order in which he wishes us to consider them. Maxwell, p. iii."

[186]Article 236.

[187]That is, his experimental data on charges should closely approximate his calculated charges from his theory.

undertake, if his theory was incorrect, it was highly unlikely this would go undetected.[188]

In the same article that contained his primary hypothesis, Cavendish laid out his experimental model. Once again, Cavendish decided to build a physical representation or replica of his primary hypothesis:

> The intention of the remaining experiments was to find out the proportion which the quantity of redundant fluid in bodies of several different shapes and sizes would bear to each other if placed at a considerable distance from each other and connected together by a slender wire.[189]

He modified this first idealized experimental model into a model that would generate data:

> ...or, which comes to the same thing, to find the proportion which the quantity of redundant fluid in them would bear to each other if they were successively connected by a slender wire to a third body placed at a great distance from them, supposing the quantity of redundant fluid in the third body to be the same each time.[190]

This second model was more practical for obtaining the measurements necessary to answer his primary question (on the amount of fluid or charge different bodies contained) as the third body he spoke of was an extensible trial plate which allowed for comparisons to be made:

> The method I took in making these experiments was by comparing each of the two bodies I wanted to examine, or *B* and *b* as I shall call them, one after another with a third body, which I shall call the trial plate.[191]

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[188]See Article 294, where he first defends both "assumptions" before going on to detail all his experimental variations.

[189]ibid.

[190]ibid.

[191]Article 238. The three preceding quotes illustrate the steps Cavendish went through to model his primary hypothesis into a concrete viable experimental model. As in his Great Globe experiment, Cavendish started with a purely theoretical model, then devised an experimental model by incorporating some apparatus into his first model, and finally completed his "translation" of his theoretical model into a piece of laboratory apparatus.

His method was to compare two bodies (B and b) by first comparing each with an adjustable trial plate (T).

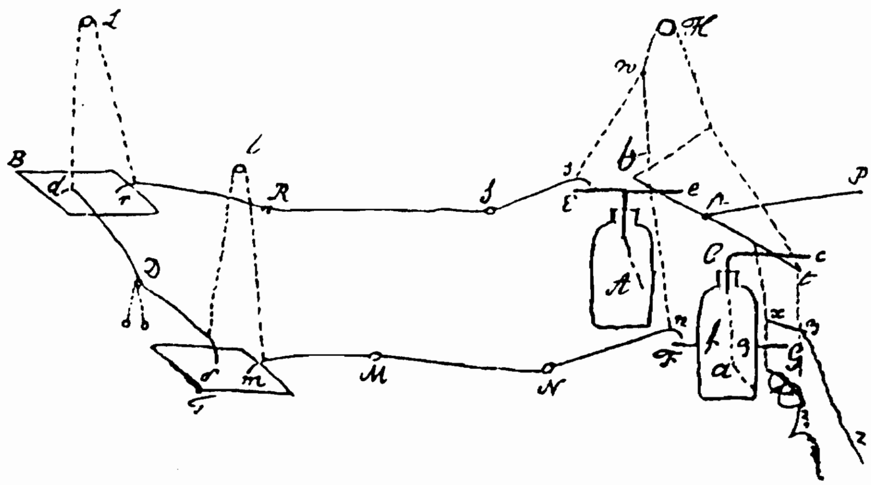


Figure 4. *Cavendish's apparatus for comparing charges.*[192]

The trial plate in each case was electrified to a specific degree negatively and the body being tried to the same degree positively. The wires connecting the trial plate and test body to two sperate Leyden jars (his source of electricity) were then lifted up and a wire to connect the test body to the trial plate lowered down.[193]

Cavendish wanted to discover what size he needed to make his trial plate to bring it into equilibrium with a test body. When the two bodies were in equilibrium, that is contained the same amount of electrical fluid, the pith balls would not sperate as the opposite but equal charges of the two bodies would cancel out. If the test body (B) had

[192] Art 241; Figure 14.
[193] Articles 240-244.

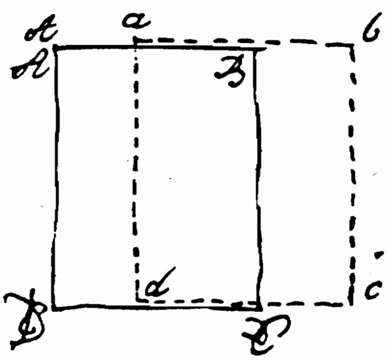
more positive charge than the trial plate, the pith balls would sperate positively and if it has less charge, they would sperate negatively. Next, he took the other body b which he wished to compare to the first body B and ran the same test on it with the trial plate. If both bodies (B and b) achieved equilibrium with the same size trial plate, then Cavendish concluded they had the same capacity to carry charge. In Cavendish's terminology, they contained the same amount of redundant fluid. If the law describing electrical attraction and repulsion was an inverse square (as shown in experiment I) and his canals of incompressible fluid were adequately modeled by long thin wires, then his experimental results in the next six experiments should not be far from what was expected as calculated by theory. Moreover, given the variations introduced in each experiment, it was very likely that if his hypothesis (or assumptions) were false, then this would be discovered in this series of experiments.

Before he discussed his individual experiments, Cavendish devoted many pages, almost half of his manuscript, to his experimental design, apparatus, error considerations, and data manipulation techniques for turning "raw" data into "modeled" and useful data. The apparatus Cavendish designed for these experiments was as complicated as the one he used in his Great Globe experiment. Cavendish also described and employed statistical models in this series of experiments. As will be seen in the pages which follow, Cavendish attempted to build a severe argument from error to justify his results and inferences. He did this in two ways. First, as in his first experiment, he attempted to control or subtract out factors that would bias his results whether from his apparatus or his surroundings or himself. Second, his entire series of experiments taken together provide variation on his first experiment and thus make it even more unlikely that were his hypothesis false, that that would go undetected.[194] As both his apparatus and data models are apropos to all the experiments, I will follow his lead and discuss these first before delving into the specific variations of his individual experiments.

(3.3) Experimental Design and Ceteris Paribus Clauses

In this section, I will describe in detail what Cavendish referred to as his method. A great part of Cavendish's method concentrated on "getting to know" his equipment and learning how to generate and model the data from his experimental model.

Cavendish drew a diagram of and described his trial plates:



> The trial plates I made use of consisted of two flat tin plates ABCD and abcd (Fig. 15), made to slide one upon the other, so that by making the side bc of one plate extend more or less beyond the side BC of the other it formed a plate of a great or less size, and which consequently contained more or less electricity.[195]

Figure 5. *Cavendish's trial plate and his description of its operation.*

[194] I give the full argument from error in my conclusions, chapter 4. See my diagram 1 in Chapter 4 for a flow chart showing how all the experiments and sub-experiments in this entire series of experiments are connected.

[195] Article 239.

The question, Laymon's question, still remains: How did Cavendish know that increasing and decreasing the size of the trial plate would change the amount of electricity it held? Cavendish gave his answer in Article 247: "it will be shewn hereafter (Arts. 284, 479, 682) that the quantity of deficient fluid in the trial plate is in proportion to the square root of its surface." Once again, I am getting ahead of my story; however, I think this sort of "hopping around" illustrates the intimate interconnections between the experiments in this series. (It also strongly illustrates the usefulness of Mayo's hierarchy for giving structure to very complicated nonlinear experimental narratives.) For the moment, I will just give credit to Cavendish for his invention of a rather ingenious device to compare different bodies and continue following his own exposition of this series of experiments.

Cavendish used several different tactics to control induction and improve insulation. In chapter two, I described many of these measures in my look at his Great Globe experiment. The design of his apparatus once again involved a complicated set-up of silk strings and pulleys for quickly performing several actions at once. Moreover, Cavendish also hired an assistant to help him. Again speed was necessary to stop the electrical fluid from spreading, a time dependent effect.[196] To insure the best insulation possible, the glass stands holding both the trial plate and test objects were covered with sealing wax to increase their insulation.[197] (I showed in Chapter two this modification was the result of a separate experiment.) Cavendish invented a clever way to ground both

[196]Articles 241-243.
[197]Article 255.

the wires and the vials more effectively. He attached a broad sheet of metal to the wall which he then used as a discharge plate.[198]

In chapter one, I discussed *ceteris paribus* conditions as posing problems to the objectivity of experimental results. I also used weather in my "trivial" example of one of Collins' experimenter's regresses. The question of concern for objectivity was how damaging can *ceteris paribus* factors, like weather, be to conducting and repeating experiments? In Cavendish's experimental narrative, they could be quite damaging but not irrevocably.

Cavendish was familiar with the confounding affect of the weather and the electrical properties of air on his electrical experiments. This is seen in the several measures he took to overcome such *ceteris paribus* factors. While the weather would not at first seem to affect the experiment, it in fact did:

> In dry weather the linen threads by which the pith balls are suspended are very imperfect conductors, so that the balls are apt not to separate or close immediately on giving or taking away the electricity. To remedy this inconvenience I moistened the threads with a solution of sea-salt, which I found answered the end perfectly well, for the threads after having been once moistened conveyed the electricity ever after very well, though the air was ever so dry.[199]

Cavendish mitigated what he concluded was the effect of dry weather by wetting the linen threads of his pith ball electrometer. His adjustment or modification of his electrometer remedied the effect of the dry weather on its sensitivity. Experimenters were very careful

[198]"If I made he wires to communicate only with the floor of the room instead of the wall of the house, I found it took up some time before the vial was discharged. It must be observed that in this case, where you want to carry off the electricity very fast by an imperfect conductor, such as the wall, the best way is to apply a pretty broad piece of metal to the wall, so as to touch it in a considerable surface, and to fasten the wire to that (article 258)." Cavendish's writings formed a regular "tips from Heloise" for experimenters.

[199]Article 259.

to record factors, like weather, which could affect their experiments and subsequent repetitions.[200] This concern with weather suggests that while not perfectly understood, the conductive properties of air was not unknown even at this early time.

Cavendish clearly recognized that the ability of air to become charged was problematic for his experiments:

> It is well known that the air of a room is easily rendered over-or undercharged, in particular if a wire such as rRSs [Fig. 14] is positively electrified, though even in no greater degree than in these experiments, and kept so for a second or two, and its electricity then destroyed, the air near it will be sensibly overcharged, as may be thus shewn.[201]

He proceeded to devise and run an experiment to test this empirical observation. He took his pith ball electrometer and suspended it above a charged wire. The balls separated. Next, he grounded the wire to remove the electricity from it. However, he found "they [the pith balls] will also continue to separate, though in a less degree, after the electricity of the wire is destroyed, which can be owing only to the air being rendered overcharged by it."[202] As I suggested in chapter two, one of Cavendish's most powerful techniques for handling error was to devise a separate experiment to replicate and amplify a suspected source of error. Having demonstrated that the conductivity of air was a source of error for his results, how did Cavendish take it into account?

In Article 257, Cavendish explained how he designed his apparatus to nullify the effects of the surrounding air being charged. He arranged his apparatus so that the pith balls "are placed about equally distant from both, so that the undercharged air near one

[200]Hackmann (1989) pp. 41,42.
[201]Article 256. See my Figure 4.
[202]Article 256.

wire will nearly balance the effect of the overcharged air near the other." By placing his electrometer in between the two oppositely charged wires, the effect of charged air was canceled out. Moreover, Cavendish reasoned: "Besides that, if it had any effect upon the separation of the balls, it would have much the same effect in trying B as in trying b, and therefore could hardly cause any error in the result of the experiment."[203] This was another strength of his experimental model. As Cavendish was comparing two bodies to each other, using the same piece of equipment, any systematic error in the measurement on the first body should also be present during the measurement on the second and thus would cancel out. His experimental apparatus embodied a statistical idea, averaging (the two opposite but equal charges canceling out) while his experimental design subtracted out systematic error.

However, Cavendish still was not satisfied that he had adequately resolved his problems with the conductivity of air. He continued: "However, still further to obviate any error from that cause, I had a contrivance by which the electricity of the wires rRSs and mMNn, as well as that of the vials, was destroyed as soon as the wires rR and mM were lifted up from B and T."[204] Again, by immediately grounding the wires, he destroyed the charge before it could travel into the surrounding air. Finally, as in his first two experiments, he used a very weak charge to electrify the objects which reduced the amount and speed of electricity gobbled up by the air.[205]

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[203]Article 257.

[204]Article 257. (See my Figure 4)

[205]I also showed in Chapter two that this tactic was the result of a separate experiment (Articles 263 and 264).

Let me briefly sum up Cavendish's experimental tactics for ruling out the "charged air" *ceteris paribus* factor. He suspected the conductive properties of the air in his lab could affect his results. He ran an experiment that proved air conducted enough electricity to disturb his results. To mitigate this problem, he positioned his electrometer halfway between the under and over-charged wires so that this known effect should cancel out. Furthermore, he designed his experiment to be a comparison between two bodies using the same apparatus so that any lingering systematic error from this effect affected both bodies equally and thus canceled out. Finally, "still further to obviate any error from that cause", he re-designed his apparatus to make instant grounding possible which reduced the strength of any lingering effect he may have missed with his other precautions.

Why did Cavendish make such a fuss about his design overcoming the conductivity of air? For his results to have any meaning, he had to insure that his electrometer was measuring the quantity of fluid on the test bodies and trial plate and not the quantity of fluid in the air or on the wires of his own apparatus. Through modification of and the symmetry in the design of his apparatus, he showed this error could be first reduced and any left over affects canceled out.

(3.4.) Data Models

Cavendish opened and closed his trial plate, increasing and decreasing its surface area, to measure the amount of fluid in a test body. If the pith ball electrometer suspended between the oppositely charged trial plate and test body did not separate, this would indicate the two had reached equilibrium and therefore each contained an equal amount of electrical fluid. Cavendish *did not* adjust the trial plate until he got a null reading (i.e., no separation) for that measurement was too imprecise. Instead:

> I [Cavendish] first made the surface of the trial plate such that the deficient fluid therein should exceed the redundant in B, and that the pith balls should separate negatively, just enough for me to be sure they separated; I then diminished the surface of the trial plate till I found, on repeating the experiment, that the pith balls separated positively as much as they before separated negatively, and the mean between these I concluded to be the required surface of the trial plate.[206]

Why did he consider this method for finding the required surface more accurate?

Cavendish explained the necessity for this precaution was due to the large amount of error inherent in merely measuring a null result:

> This way of making the experiment I found much more accurate than the other, for supposing the required surface of the trial plate to be expressed by the number 16, I found that its surface must be increased to about 20 before I could be certain that the pith balls would separate negatively, and that it must be diminished to about 12 before they would separate positively; whereas I found that increasing its surface from 20 to 21 would make the balls separate sensibly further, and that diminishing its surface from 12 to 11 would have the same effect; so that I could determine the required surface of the trial plate at least four times more exactly by the latter method than by the former.[207]

Had Cavendish merely recorded the size of the trial plate as soon as he got a null result, his data would have been quite inaccurate. Therefore, he *modeled* his observations, condensing them by taking the mean between his first positive reading and first negative reading on his electrometer. A simple null reading or no separation covered a range of values or trial plate sizes, by taking the first negative and positive readings and averaging, Cavendish could get a more precise reading. This statistical method also held subjective

[206]Article 245.
[207]Article 246.

biases and readings at bay. There was less leeway for personal interpretation in reading the first positive and negative openings of his electrometer. It was possible that he could, within the range a null reading covered, alter his plates several inches to make a match more fitting with his expectations.[208]

Cavendish used the arithmetic average of his two measurements to get the required surface area of the trial plate. Maxwell, an admirer of Cavendish's work, criticized this procedure. While the arithmetic mean was better than simply using a null result, Cavendish should have used the geometric mean between the two measurements for his test statistic as he was measuring surface area. Notice, Maxwell's criticism had nothing to do with nor refers in any way to any larger global theory about electricity. Rather, it was the sort of low-level local criticism a scientist today could make about this two century old experiment. It is just this type of experimental knowledge and criticism that carry through Kuhnian revolutions.

Cavendish was concerned about error arising from his Leyden jars being unequally charged between the two measurements he needed for his average. To solve this problem, he first added a design modification to his apparatus. He attached an electrometer to his Leyden jars and "in charging the vials took care always to turn the globe [Nairne's electrical machine] till these cylinders just began to separate."[209] His use of averages demanded this experimental complication as the vials had to be recharged to get the

[208]This example also illustrates an experimenter "tinkering" with his results. However, this data manipulation can be justified in that it resulted in precise accurate information. I think this point is easier to see in a simple example like this; however, it is the same sort of thinking, the same sort of argument from error, that is used today by scientists dealing with even more complex, technologically mediated observations. (Remember, Cavendish's observations were also technologically mediated.)

[209]Article 248.

second measurement. Cavendish replaced this first electrometer (a paper cylinder) with a more "exact kind of electrometer"[210] made of gilded straws. His new improved electrometer:

> was suspended by the piece of brass C from the prime conductor, and a piece of pasteboard, with two black lines drawn upon it, was placed six inches behind the electrometer on a level with the balls, in order to judge of the distance to which the balls separated, the eye being placed before the electrometer at thirty inches distance from them a guide for the eye being placed for that purpose*), and the electrical machine was turned till the balls appeared even with those lines. By these means I could judge of the strength of the electricity to a considerable degree of exactness.[211]

Cavendish invented both a scale for charging his Leyden jars as well as a guide for his eye to further insure the equal charging of his jars between runs. Using these two innovations, he could charge the jars equally for all runs of his experiment.

Cavendish was an exacting experimenter and these physical precautions were not reassuring enough for him. Thus, he made an estimation of the amount of error that could at most arise in such a situation. In Mayo's terminology, he devised an after-trial data model. His procedure illustrates Mayo's "manipulations on paper."[212] Cavendish already knew by experiment that his second reading would be off by four from a null result.[213] He set the unknown error in quantity of redundant fluid from unequal charging (x) equal to one. Then Cavendish calculated that the total error he was liable to from unequal charging equaled $2(x\text{-}1)/x$. Furthermore, he stated that the most x could

[210]Article 249.

[211]Article 249. The letter C refers to a picture Cavendish drew of this electrometer which is included in Article 249. The asterisk in this quote sends the reader to one of Cavendish's own footnotes which explained that the eye needed to be at the same distance each time so as not to cause error in reading the separation.

[212]Mayo (1996, pp. 67, 96).

[213]Article 246.

be was 5/4 which would result in his second measurement being off by 1/32 of the whole surface of the trial plate.[214] In his analysis, Cavendish first re-iterated the increased accuracy of using the average of his measurements rather than simply recording a null result. Then, he indicated the greatest possible error (worst case scenario) to be expected in his results was 1/32 of the entire surface of the trial plate if the vials were unevenly charged.

Despite all his efforts to eradicate error through technological innovation, statistical considerations and computations, Cavendish discovered his experiment was still liable to error from a source he admitted he did not know. From his calculation above, Cavendish expected the maximum error in measuring the redolent fluid would not exceed 1/32 part.

However, he found that:

> on repeating the experiment a little after, the balls would separate differently from what they did before, and that I was obliged to alter the surface of the trial plate by 1/12 and sometimes even 1/8 of the required surface in order to make the balls separate in the same degree as before....so that I am liable to make an error of 1/24 or 1/16 part in judging of the proportion of the quantity of redundant fluid in the two bodies.[215]

Cavendish's worst case scenario postulated an error of 1/32 part, but in reality he was liable to errors of 1/24 or even 1/16 part. Moreover, he admitted he could not blame all of this difference on the unequal charging of the vials though some error may have occurred from that source.[216] To compensate for this unknown source of error which caused some irregularity in his measurements, Cavendish "always compared each body

[214]I have only given a brief summary of his results of this error analysis. He calculations are found in Article 250.

[215]Article 261.

[216]Article 262.

with the trial plate 6 or 7 times running."[217] He does not say whether he then averaged these several trials, took the mode (the measurement which appeared the most), or used some other criteria. I believe he took the mode simply because had he averaged the trials or used some other criteria, he probably would have mentioned it as a tip for other experimenters.[218] Despite his worries, Cavendish felt that by conducting several trials he remained within his original error estimate of 1/32 part and that was what was needed for him to justify that his data was accurate.

Thus far, I have only discussed Cavendish's introduction to his experiments.[219] I have, in general, followed his own exposition. I have also shown that his concern with error occupied a central place in his rendition of his experiments so far. I will now discuss his individual experiments III-VII.

(3.5) Experiment III

Cavendish's third experiment was a check on an assumption of his experimental model which underlay this entire series of experiments. He designed it to discover any errors that could arise if the communicating wire contacted the test bodies and trial plate in different ways. He assumed in his experimental model that it should not make any difference. Cavendish used a square tin plate twelve inches on each side as a test body. He attached the charging wire in six different positions to this test plate to see if these positions affected the quantity of fluid the plate could hold (i.e., changed the proportions

[217]ibid. Cavendish started this quote with the phrase: "For greater security."

[218]As I mentioned before, his writings formed a regular "tips from Heloise" for experimenters.

[219]Articles 236-265. Exactly half of the articles in Cavendish's journal were devoted to the elaborating the considerations I have so far mentioned in this chapter.

between the test plate and the trial plate). Below is a diagram showing the various wire positions Cavendish tested:

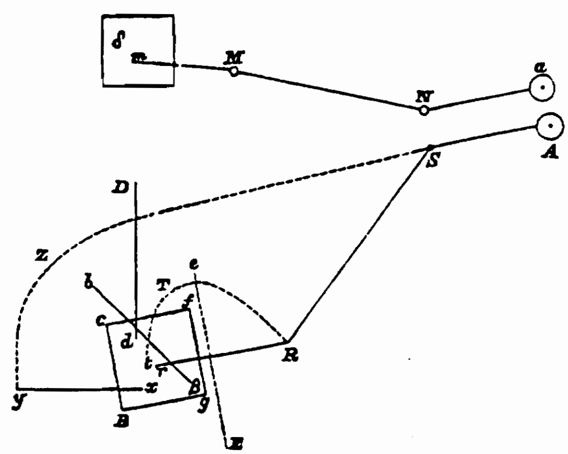

Figure 5. *Arrangement for testing the effect of wire placement for this series of experiments.*

He found:

> It should seem from these experiments that the charge of the tin plate is not exactly the same in all the ways of trying it, as the extremes seem to differ from each other by above 1/12 part, which is more than could arise form the error of the experiments; but, excepting the 4th and 6th ways, the others seem to differ by less than 1/24. This I think we may be well assured of, that no sensible error can arise in the following experiments from any small difference in the manner in which the bodies are touched by the wire.[220]

[220]Article 268. He listed his results in article 267. His results were: first way=11.7, 2nd=11.7, 3rd=12.0, 4th=10.8, 5th=11.5 and 6th=10.8. His fourth way included a cross wire parallel to the front of the body, and his sixth way the connecting wire had a vertical arch in it before touching the center of the plate. The wires in the other four ways were in a horizontal plane to the test object. He repeated the experiment another night for four of his ways and got similar results. In his repetition, the charges were even closer than before and the "off" way continued to be off by almost the same amount. 2nd way=11.9, 3rd=12.0, 5th=11.8 and 6th=11.0.

Cavendish's last sentence in the above quote makes it abundantly clear that the purpose of this experiment was to check an assumption of his experimental model. He gained two bits of practical knowledge from this experiment. First, he needed to keep the charging wires in a horizontal plane when charging both the trial plate and test objects. Second, the wires in all trials should be placed in approximately the same spot at every trial to reduce error from this source. Thus, he modified his assumption of no difference into no difference providing the wires were in a horizontal plane to and similarly placed on the bodies being compared.

(3.6) Experiment IV

Experiment IV was designed to find out whether bodies of the same shape and size but made of different substances carried the same proportion of charge. It was also designed to see "how far the charge of a flat plate depended on its thickness."[221] To test precious metals like gold, expensive then as now, and more liquid substances, he coated panes of crown-glass with them to test. Cavendish tested a wide variety of substances: tin, pasteboard like that used to cover books, gold, thin tin-foil, Gum Arabic, Gum Arabic with salt and then charcoal, water thickened with gum arabic, stone, and different thicknesses of hollow plates made by soldering tin plates together.

But many of these plates were made of non-conductors, so how did Cavendish get any charge to stay on their surface?

> As many of the substances used were but imperfect conductors of electricity, I fastened bits of tin-foil about an inch square on the places on which the wires Rr and Dd touched the plate in order to make the electric

[221]Article 269.

> fluid spread more readily over it, and I satisfied myself beforehand that with this precaution they conducted readily enough for my purpose, as I found by discharging a Leyden vial, and making these substances part of the circuit.[222]

His sub-experiment as described above was based on the idea that the electric fluid had to flow over the surface of the non-conducting plates in order to complete the circuit. None-the-less such results, that non-conducting materials can contain the same amount as conducting materials, do seem suspicious. Was Cavendish really measuring the ability of these non-conducting bodies to carry charge or the ability of charge to flow over them connected by his tinfoil bits? While these results seem quite questionable and throw doubt on what exactly Cavendish was measuring, this is the only experiment in the series where non-conductors were used. I will discuss problems with this experiment at the end of this section.

More interesting and of importance for his experimental series as a whole in this experiment was his work on the affect of plate thickness on the ability of plates to contain charge. Cavendish concluded that "the charge of a thick plate is greater than that of a thin one of the same base, as might be guessed from the theory, and it seems to be equal to that of a very thin one whose side exceeds that of the thick one by about $1^{1/3}$ of its thickness."[223] That is a thick square plate will hold more charge than a thin one of the same dimensions. However, if the length of each side of the thin plate equals 4/3 the length of the thick one, then they will carry equal charge which supported his theory.

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[222]Article 271.
[223]Article 272.

From this experimental result, Cavendish decided to calculate what the charge would be if each of the plates were stretched out in proportion to their breadth:

> Let us therefore increase the mean side of each of these plates by $1^{1/3}$ of its thickness, where that quantity is worth regarding, and alter the charge found by experiment in the ratio of 12 inches to the side thus increased, which will give us the charge of a plate of the same materials and shape whose increased side is 12 inches...These numbers do not differ from each other by more than what may fairly be supposed owing to the error of the experiment.[224]

This is an example of "experiment by other means" as Mayo puts it.[225] Rather than repeating the experiment using larger plates, having experimentally determined the relationship between thickness and breadth, Cavendish used the same data and calculated "what the results would have been" had he tried the experiment on larger plates and compared that with his purely theoretical predictions. The results of the original experiment and his manipulations on paper allowed Cavendish to conclude:

> that the charge of any thick plate is very nearly the same whatever its thickness may be, provided its thickness is very small in respect of its breadth or smallest diameter....and...if the plate is square and its thickness is several times less than its side, though not small enough to be disregarded, its charge is equal to that of a very thin square plate whose side exceeds that of the former by about $1^{1/3}$ of its thickness.[226]

Cavendish argued that the first part of this experiment proved charge or the ability to contain the electrical fluid was dependent only on the dimensions of a body and independent of the substance being electrified. More to the point, and again in support of the view that charge is carried on the surface of a body, he discovered the effect the thickness of a plate had on charge capacity depended on the relation between the

[224]Article 272.
[225]Mayo(1996, p. 6).
[226]Article 272.

thickness and the size of the plate in accordance with his theory. This experiment can be criticized on two points. First, using tinfoil bits to help the electricity spread on non-conductors does not ensure their ability to contain the electrical fluid. Cavendish did not use non-conductors in any other experiment. Second his extrapolation from one set of data on the relationship between of length and breadth for effecting charge capacity could be problematic. He should have tried the experiment with larger plates, rather than just calculating what would have happened, to check that this empirically determined relationship held. This was the most problematic experiment that he ran for those reasons.[227]

(3.7) Experiment V

Cavendish designed his fifth experiment:

> with a view to find what proportion the charges of similar bodies of different sizes bear to each other, and whether it is the same that it ought to be by the theory on a supposition that the electric attraction and repulsion is inversely as the square of the distance, and that the bodies are connected to the jar by which they are electrified by canals of incompressible fluid.[228]

He used his same experimental set-up as in the other experiments in this series replacing the test bodies to be compared with two circles of 9.3" diameter and a circle of 18.5" diameter.[229]

[227]This experiment, however, is interesting in that no-one else to my knowledge has raised any questions about his using non-conductors in this manner which underscores the need for and usefulness of an error statistical approach for looking at experimental histories as it highlights just these sorts of problems.

[228]Article 273.

[229]In discussing his (idealized) experimental model (Art. 273) he suggests he will use two 9" and one 18" diameter circles. However, in article 452 wherein he gives the results of the trial, he corrects this measurement to 9.3" and 18.5" respectively.

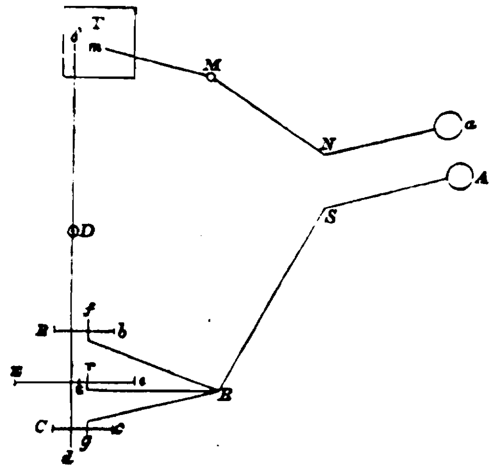

Figure 6. *Layout of apparatus for Experiment 5.*[230]

Cavendish removed the large circle in the diagram when testing the small ones and vice-versa. He "observed that the charge of the two small circles together will not be as much as double the charge of one circle, unless the distance of the two circles from each other is extremely great."[231] Therefore, he tried the two circles separated at three distances: 36, 24, and 18 inches.

Cavendish repeated this experiment with two modifications. First, he increased the distance between the trial plate and its charging vial from 87 inches to 98 inches and decreased the distance between the large circle and its vial from 106 to 63 inches. The distance between the trial plate and the large plate and between the centers of the two small circles remained the same. Second, he built a wooden frame or floor 14 inches

[230]Article 273, Figure 18.
[231]Article 274.

from the ground under the circles to block any effect the under-charged floor may play in his experiment.[232] The proportion of charges from the two experiments were:[233]

		1^{st} way	2^{nd} way
Large circle		1.000	1.000
2 small ones	*@ 36"*	.899	.894
" "	*@ 24"*	.859	.840
" "	*@ 18"*	.811	.798

Cavendish calculated these proportions by taking the size of the trial plate at the first negative separation and added it to the size of the plate at the first positive separation. He then divided by two to get the mean plate size. Next he set the large circle to one and adjusted his other measured proportions for comparison. The greater the separation between the two small plates, the closer their proportion came to that of the large plate.

Next Cavendish calculated, based on his theory, what the charge proportion should be. He admitted: "This cannot be done exactly without knowing the manner in which the redundant fluid is disposed in the circles, which I am not acquainted with."[234] Instead, he calculated three different possible proportions based on three different possibilities for the distribution of the electrical fluid within the circles. The three manners of disposition of the electrical fluid were: spread uniformly throughout the disc, contained entirely within the circumference, and 11/24 in the circumference and 13/24 spread uniformly:[235]

[232]Articles 277, 339, 474. Cavendish explains his theoretical error considerations behind these modifications in Articles 188-194.

[233]Articles 274 and 275.

[234]Article 276.

[235]Article 276.

		Fluid Disposition		
		uniformly	**circumference**	**11/24 & 13/24**
Large circle		1.000	1.000	1.000
2 small ones	*@ 36"*	.933	.890	.920
" "	*@ 24"*	.911	.844	.890
" "	*@ 18"*	.890	.805	.863

Cavendish felt that the figures in the last column were "much the most likely to agree with the truth."[236] His conclusion was based on another experiment, Experiment VII, where "the charge of a circular plate bears the same proportion to that of a globe that it would do if the fluid was disposed in that manner."[237] However, Cavendish qualified his experimental determination of the disposition of the electrical fluid thus:

> I would not be understood by this to suppose that the fluid is actually disposed in this manner in a circular plate, but only that the charges will bear the same proportion to each other that they ought to do on this supposition.[238]

Thus for Cavendish his operationally or instrumentally defined dispersion of the electrical fluid in a circular plate was a true model not an ontological truth.

This was a very important experiment for Cavendish. Laymon points out that in Cavendish's previous experiments "the capacity of the test object was correlated with the area of the trial plate"; however, "such correlation is not a scale."[239] Cavendish needed a scale for his sensitivity analysis in his Great Globe experiment. Laymon gets to the heart of the importance of Experiment V: if two small plates carried the same charge as one large plate of double diameter, then "the capacity of circular plates in isolation varies

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[236]Article 277.
[237]ibid.
[238]Article 277, footnote *.
[239]Laymon, p. 44.

as their radii."[240] Furthermore, Laymon points out that by "generalizing on this result (in part substantiated by additional experimentation), Cavendish obtained the result needed in order to justify his choice of a capacity standard, that is, that the capacity of his trial plate varied as the square root of its area."[241] After explaining Cavendish's experimental argument, Laymon still claims that Cavendish's calibration of his test plate for his sensitivity analysis of his Great Globe experiment was circular, i.e., determined by assuming the very inverse square force law he was testing. What does Laymon base his contention on?

Cavendish explained that while "the proportion of the charges in these two ways of trying the experiment was not greater than what might well be owing to the error of the experiment...I am more inclined to think that the difference is real.[242]" Cavendish's results were not as close to his theoretical square law prediction as he expected, even after trying the experiment in two ways. Moreover, this discrepancy could not be blamed solely on experimental errors but seemed to Cavendish to represent a real difference. Nonetheless, ignoring this real difference, Cavendish used the square root of the surface of his trial plate to calibrate his test plate for his sensitivity analysis and therein lies Laymon's circularity claim. When Cavendish used his trial plate to calibrate his test plates, Laymon concludes that Cavendish had to assume an inverse square force law given the discrepancy due to a "real" difference in his Experiment V.

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[240]Laymon, p. 47.
[241]ibid.
[242]Article 277 and 278.

However, Cavendish felt even such a "real" difference had little effect on the truth of his theory:

> This [real difference], however, can by no means be looked upon as a sign of any error in the theory, but, on the contrary, I think that the difference being so small is a strong sign that the theory is true. For it cannot be expected that the charges of bodies connected together by wires should bear exactly the same proportion to each other that they should do if they were connected by canals of incompressible fluid; and indeed, the third experiment shews that they do not.[243]

His reasoning was basically this: Cavendish expected a certain amount of error, though small, to occur as thin wires were only an approximation of his canals. If the theory was an entirely inaccurate model, then he expected a much larger disagreement between his theoretical calculations and his experimental readings. That the difference was so small, no more than was to be expected from using wires to approximate his incompressible canals, seemed to be a good argument for its aptness.

Cavendish's expectation of increasing discrepancies the farther off his model or theory from reality is not a "continuity assumption" as Laymon[244] asserts, but is epistemologically grounded on arguing from error. In particular that error ramifies.[245] First, Cavendish expected a certain, though small, amount of error to begin with due to his use of wires as approximations of his canals; however, the greater the inaccuracy of the approximation, then the larger the error that should occur. Second, in the context of his numerous experimental variations, there should have been little or no agreement between experimental results, if his theory (based on an inverse square law of attraction

[243]Article 278. His third experiment showed the affect of different wire positions on charging a plate.
[244]Laymon (1994).
[245]David Hull's term.

and repulsion and his instrumentalist conception of canals of incompressible fluid) was a totally incorrect model of reality. In sum, the farther off his model from what was really happening, the greater the disagreement of his results, both from one another and from his theoretical predications, should have been.

(3.8) Experiment VI

Cavendish's primary hypothesis in Experiment V was to compare the charges of similar bodies of different sizes together. Cavendish designed Experiment VI as a modification of Experiment V. However, rather than comparing the charge of two small circles to one large one, he compared two short (3') wires of 1/10" diameter to one long (6') wire of 1/5" diameter. Again, he placed all three wires horizontal and parallel to each other and tested the two smaller ones at the same three different separation lengths of 36, 24, and 18 inches. The one difference in this set-up was Cavendish connected his electrifying wires to the ends of his test wires (cylinders) rather than in the middle.

The following chart summarizes Cavendish's experimental determination of the charge proportion compared to the charges as calculated by his theory of incompressible canals.[246]

	Exper. Charge	**Calculated by theory to lie between**	
Long wire	1.000	1.000	1.000
Short wires @ 36"	.903	.923	.893
" " @ 24"	.860	.905	.860
" " @ 18"	.850	.883	.835

[246]Articles 279 and 280.

Again, the greater the separation between the two smaller objects, the closer their charge capacity came to the capacity of the object double their dimensions. Cavendish stated that based upon his next experiment (Experiment VII), the observed charges should be closer to the second column of proportions calculated by theory. He concluded "[t]he observed charges were actually between the two proportions, but approached much nearer to the latter, so that they agreed as nearly with the computation as could be expected."[247] Cavendish considered the next experiment to be integral to this one, so I shall proceed to it, as Cavendish did, before continuing my discussion of experiment VI.

(3.9) Experiment VII

In this experiment, Cavendish attempted "a comparison of the proportional charges of several bodies of different shapes"[248] compared to the globe he used in Experiment I. Cavendish recorded his results:[249]

A globe 12.1 inch in diameter	1.000
A tin circle 18.5 inch in diameter	.992
A tin plate 15.5 inches square	.957
An oblong tin plate 17.9 inches by 13.4 inches	.965
A brass wire 72 inches long and .185 thick	.937
A tin cylinder 54.2 inches long and .73 in diameter	.951
A tin cylinder 35.9 inches long and 2.53 in diameter	.999

This experiment was important for Cavendish in that it provided him another standard unit of comparison for the amount of electricity bodies contained. He set his globe (the same one used in Experiment I) at one and then computed the proportional amount of fluid the other bodies contained to this standard. This allowed him to discuss or quantify

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[247]Article 280. Also see Articles 453, 477, 683 and 683.
[248]Article 281.
[249]ibid.

the amount of electricity using his experimentally determined standard of "globular inches."[250]

Cavendish summed up the information gained by this experiment in Articles 282-287. He stated:

> We may conclude also from this experiment that the charge of a circular plate is to that of a globe of the same diameter as 12 to 18 1/2, which by the above-mentioned proposition [Prop. XXIX][251] is the proportion which ought to obtain if 13/24 of the whole quantity of redundant fluid in the plate was spread uniformly [overly the surface], and the remainder, or 11/24, was spread uniformly [round the circumference].[252]

Thus, this experiment legitimized his hypothetical dispersion of the electrical fluid in his Experiment V by experimentally substantiating his dispersion claim. Cavendish does not claim reality for his hypothetical dispersion definition even though it has been experimentally substantiated. He insisted it was an operational definition which provides a true model of fluid dispersion, not an ontological truth.[253]

When Cavendish discussed his comparison between the cylinders and the globe and compared it to his results in Experiment VI (two short to one long cylinder), he remarked that discrepancies from his theory of incompressible canals may be due to the fluid being spread less uniformly in a short cylinder "whose length is not very great in proportion to its diameter...that there is a greater proportion of the redundant fluid lodged near the extremities, which seems by no means an improbable supposition."[254] Given

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[250]Article 654: Cavendish defines globular inches: "*The charge of globe 1 inc. diam. placed at great dist. from any other body is called 1 glob. inc.* The circ. 18.5 =13.5 glob. inc. (ital in original)." Thus this experiment also provided a standard unit of electricity for Cavendish.

[251]Article 140.

[252]Article 282.

[253]Article 277, footnote *.

[254]Article 286.

his experiment IV regarding the affect of plate thickness compared to length, this assumption did not seem improbable. Furthermore, on this supposition, the observed charges would then agree with theory more than they did. But does Cavendish only bring up possible difference in fluid dispersion and other sources of error to support his theory in the face of experimental differences, even slight ones?

Cavendish also pointed out a source of error that would make the experiment agree more closely with his theory than it should have. In article 287, he discussed possible errors in his wire experiments based on induction from the surrounding floor and trial plate and the wires--the effects of mutual induction of his apparatus with its surroundings. Interestingly enough he stated: "It must be observed that the first of these two causes tends to make the charge of the wire appear greater than it really was, and consequently to make the observed charges appear to agree nearer with the theory than they really did. Which way the second cause should operate I cannot say."[255] Thus, Cavendish frankly admits that if the first cause is in operation, it would make the observed charges agree *more closely than in fact they did.* The remarkable aspect of Cavendish's exposition of his experiments was his candidness in reporting errors that would both detract from and increase support for his theory.

(3.10) Experiment VIII

Cavendish's last experiment in this series was designed to test a novel or unexpected prediction of his theory. His hypothesis, based on an inverse square force law, was that if he positively electrified three thin parallel plates equally, "the quantity

[255]Article 287.

of redundant fluid in the middle plate will be many times less than that in either of the outer plates, or than that which it would receive by the same degree of electrification if placed by itself."[256] Given all three plates were equally electrified, it seemed odd to expect the middle one to contain less charge than the other two plates without the assumption of an inverse square law.

In article 542, Cavendish described the specifics of his experimental set-up:

> It was tried whether when three tin plates 1 foot square were placed near to and parallel to each other, the line joining their centers being perpendicular to their planes, the middle plate would receive much electricity on electrifying the plates. The experiment was tried with the same apparatus and nearly in the same manner as the experiment with the globe [Art. 218], except that the two outer plates were suspended by two sticks of waxed glass turning on hinges. The wire too by which the plates were electrified was made so as to touch all three plates at the same time. Four bits of sealing wax were stuck to the middle plate, two on each side, to prevent the outer plates coming too near.

Cavendish first electrified this set-up, then detached the electrifying wire and removed the outer plates and applied a cork ball electrometer to the middle plate. Cavendish used his jar and plate condenser from his globe experiment to determine the smallest measurable difference he could detect between electrifying the middle plate alone versus when it was electrified in tandem with the outer plates. He found that the middle plate contained 7 to 8 times less electricity when enclosed compared to being electrified by itself to the same degree. Were his inverse square law hypothesis incorrect or far off from a true description of the force of electrical attraction and repulsion then this result would be highly improbable.

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[256]Article 288.

(3.11) Conclusions & Remarks

Cavendish's experiments III-VIII show how Collins' second experimenters' regress which revolves around the inability to ever *exactly* repeat an experiment is not only unproblematic but undesirable as well for Cavendish's electrical experiments. Cavendish did repeat all of his experiments several times, but had Cavendish merely attempted to repeat his Great Globe experiment (Experiment I) over and over again, he would have learned nothing new. More important for *severely* testing his theory was his ingenious modifications and variations of his experimental environment, apparatus, and test objects to really put his theory through the wringer. Variability replaced repeatability for arguing his claims were warranted. Thus he varied his experiment by varying the substances tested (Experiment IV), the shapes tested and compared (Experiment VII), and by testing his various experimental assumptions such as the effect of different contact positions between his connecting wires and bodies (Experiment III). He also compared two smaller objects (Experiment V, circles) and (Experiment VI, cylinders) with a similar shaped object double their dimensions. In this series of experiments, Cavendish was checking that an inverse square law acted not only on his globes and boxes but on all sorts of objects of varying substances, dimensions, arrangements, and distances. The combination of all these different experiments made it highly improbable for an inverse square force law description of electrical attraction and repulsion to be an artifact of any one experimental arrangement. Moreover, quantifying his results made it even more unlikely that experimental agreement with his theory was spurious as a quantitative assessment is more controlled and likely to highlight errors in his hypothesis than a qualitative one

Laymon's contention that Cavendish's calibration of his test plate for his final sensitivity analysis in his Great Globe experiment was based on a circular argument assumes there was no other experimental support beyond experiment V for the calibration. While Cavendish's Experiment V was quite important, like his Great Globe experiment, it too must be seen functioning as a part of the larger series and supported by his other experimental results. Only after these further tests -- as is quite clear from Cavendish's own narrative and guiding notes -- was Cavendish able to give his final sensitivity analysis. Thus, rather than a circular analysis, his series of experiments formed a feedback loop which constantly checked the accuracy of his theory. He moved from qualitative to quantitative experimental assessments of his inverse square law hypothesis. Further, it was tested by Cavendish's varying experimental circumstances which made it highly likely that he would have uncovered whether an inverse square law was spurious or an artifact of one of his pieces of apparatus. In the next chapter, I will tie all these experiments together and return to this point once I have "drawn the big picture" for my readers.

When I said Cavendish adjusted his apparatus and varied his experiments purposely to put his theory through the wringer, I do not mean to insinuate Pickering's idea of "mangle" which is the British term for wringer. As in the last chapter, in this chapter Cavendish's "tinkering" (Pickering's term) was justified on the grounds of eradicating sources of error. His use of statistical tools, averaging and using comparative proportions rather than direct comparisons, was also justified on the grounds that this type of data modeling increased the objectivity of his results by making them more accurate than his "raw" observations. Pickering's use of the term "tinkering" seems at the very least quite a happen-chance affair and at worst pejorative to any sense of objectivity. Cavendish's modifications to both his equipment and results were painstakingly designed and motivated by a search for objectivity based on error considerations. Once again, as in his first two experiments (the Great Globe and Box), his adjustments were not made to manipulate or acquire results to support his theory but occurred during the process of trying to eradicate errors and increase the accuracy of his results.

One of the more interesting aspects of Cavendish's work in this series of experiments revolves around his ignorance of just how the electrical fluid was dispersed in material bodies and his ingenious solutions to his own lack of knowledge as seen in the interplay between his experimental results and theoretical musings. When Cavendish was unsure of the dispersion of the fluid in his circles, he calculated three possibilities, one of which, 13/24 and 11/24, was later experimentally justified by his Experiment VII. Thus, he instrumentally or operationally defined fluid dispersion within circles. He suggested the discrepancies in his cylinder results were based on the supposition that a disproportionate amount of the charge would accumulate at the extremities of the wires. Again, this was not improbable given his Experiment IV which showed the relation between breadth and thickness for charge capacity. He used a similar tactic again when he treated the possible dispersion of fluid in his square plates as if it was dispersed in the same manner as in his discs. Cavendish's experimental definitions allowed him to work and gather results even when he had to deal with otherwise unknown ontological factors. Such operational definitions allowed him, when tested or experimentally supported, to use objective models and thus gain objective results even if such results were not based on the "true" or "real" nature of fluid dispersal. I will discuss this point in depth in the next chapter.

Finally, this chapter again illustrates the fruitfulness of Mayo's error epistemology not only for addressing objectivity questions but for sorting out enormously complex and non-linear experimental narratives. In the next chapter, my conclusions, I will argue this point more strenuously as well as the other points I have raised in this section.

Chapter 4: Conclusions

In this chapter, I pull together and summarize my arguments from the preceding chapters to show that all eight experiments in Cavendish's series re-enforce each other and combined form a severe test of an inverse square force law for describing electrical attraction and repulsion. I also continue my argument that neither Dorling's nor Laymon's explanations of Cavendish's Great Globe experiment are adequate and that Mayo's epistemology forms a much better tool for historical analysis of experimental episodes. I re-iterate my argument that neither Collins' experimenter's regresses nor Pickering's "mangle" of practice apply to Cavendish's electrical studies. Finally, I will discuss the objectivity of Cavendish's experimental series and how it solves my original puzzlement about the status of electrical knowledge in science.

(4.1) The Great Globe in Context

While Cavendish's globe experiment is considered a crucial experiment to prove that an inverse square force law described electrical attraction and repulsion, the strength of his experimental argument cannot be understood except in the context of his entire series of eight experiments. This is because all eight experiments combine to both check his experimental assumptions and mutually re-enforce one another. How do these experiments combine to form a severe test of Cavendish's inverse square force law hypothesis in his final version of his great globe experiment?

I have already covered the designs of his individual experiments in some detail. The following table is a brief summary of the highlights of each.

I. *Qualitative Experiments:*

Exp. I: (The famous Great Globe experiment) Non-circular qualitative determination of inverse square force law to describe electrical attraction & repulsion.

Exp. II: (The Box) Checked that results of Exp. I were not an artifact of his globe apparatus. (Force law holds for other geometrically shaped objects.)

II. Quantitative Experiments:

Exp. III: Checked assumption that thin wires approximated his infinite canals of incompressible fluid.

Exp. IV: *(A)* Checked if substance a body was composed of mattered for its capacity to hold charge.
(B) Checked to see if the relation between thickness and breadth of a plate on charge matched theory. (Also ran "experiment on paper" once the relation was determined for hypothetically larger plates.)

Exp. V: Checked agreement between theory & experiment about the proportion of charge between two small circles to one circle double their diameter. (This experiment was also designed to show that charge capacity was related to surface area and could be measured thus.)

Exp. VI: Same as Exp. V using cylinders.

Exp. VII: Setting proportional charge of globe to one, compared the charge of various shaped bodies to theoretical predictions. (This also experimentally substantiated his hypothetical fluid dispersion for circles in Exp. V.)

Exp. VIII: Tested novel prediction that a plate in between two other plates and electrified in tandem would contain much less fluid than if electrified alone.

Final version of Exp. I: Repeated Great Globe experiment with final quantitative sensitivity analysis. (Test plate calibrated based on experiment V and supported by the culmination of the experiments above.)

I argued in chapter three that Laymon's circularity claim is un-problematic in the context of the series as a whole. That is because Cavendish's calibration of his test plate was not merely based on his Experiment V, but also on his previous non-circular experimental proofs (Experiments I & II) and severely tested by a barrage of different tests (Experiments III-VIII). Cavendish's experiments III-VIII can best be viewed as follow-ups to his qualitative proof of an inverse square force law designed to assess this law quantitatively.

Experiment I, without his final sensitivity analysis, was a qualitatively severe test. Cavendish's design had narrowed down the experimental complications to problems of induction and inadequate insulation which he successfully resolved. Thus, Cavendish was able to qualitatively establish, and fairly rigorously using his pre-sensitized electrometer, his hypothesis of an inverse square force law. Furthermore, his Experiment II, the box, supported his argument that his results from the Great Globe were not an artifact of his particular globe apparatus.[257]

Each of the follow-up experiments in this series was designed to test either an assumption of his experimental set-up or a prediction of his theory. Cavendish designed Experiment III to check his underlaying experimental assumption that the thin wires used in all these experiments adequately represented his canals of incompressible fluid. He found that the finite wires provided a close approximation if the wires were kept in a horizontal plane to the body being electrified and contacted the surface of the bodies being compared in approximately the same spot.

His most problematic experimental result from a modern view point was his determination that substance did not affect the capacity of a body to carry charge. Using tinfoil bits to make non-conducting bodies part of a circuit, Cavendish determined that the material a body was composed of made no difference to its ability to hold charge. However, only in Experiment IV did he use non-conductors. In Experiment IV, he also determined the effect of thickness in relation to breadth had on the charge of plates and found his experimental results supported his theory.

In experiments V (circles) and VI (cylinders), Cavendish tested two objects of the same shape and material to a similar body double their size and found again the

[257]Remember, however, that he did not make mathematical calculations for this experiment. Instead, he substituted boxes for his spheres. Thus, this check, though ingenious, has a limited severity.

proportions matched his expectations, though he suspected some real difference in Experiment V. However, he concluded it was close enough to confirm his inverse square force law hypothesis, given the problems inherent in his finite set-up, as in his theory the discs or circles should be infinitely separated.[258] This agreement with theory was closest assuming the electrical fluid was dispersed as he hypothetically postulated and then experimentally substantiated in Experiment VII. Cavendish, in Experiment VII, compared several different shaped bodies whose proportions not only closely agreed with his theoretically determined predications (based on an inverse square law and his incompressible canals), but allowed him to establish another standard unit of electricity--globular inches. Finally, he tested a "novel" or unexpected predication of his theory. He electrified three parallel plates and found that the middle plate contained considerably less charge than when electrified by itself.

Because Cavendish experimented with and used a variety of shapes, experimental designs and tests, if Cavendish's inverse square law assumption were incorrect, the overall agreement between all eight experiments would not have accorded so closely with his hypothesis. Moreover, as these were quantified experiments, discrepancies between one another, as well as from his hypothesis, were easier to assess and more likely to appear. Thus, if his assumption were incorrect, these many different and quantified experiments would very probably have disclosed this error. This series of experiments taken together formed an (informally) severe test of his hypothesis. Moreover, it is this severity of the entire series taken together that renders Laymon's circularity charges un-problematic.

[258]Article 277. Also see Articles 188-194 for his theory of the set-up in Experiment V.

Laymon's charge was based on only one experiment, even though an important one for Cavendish, taken out of the context of this series. Cavendish's *final* version of Experiment I incorporated all that was learned in experiments I-VIII. Acknowledging that the series formed one large test of Cavendish's hypothesis renders Laymon's claim harmless as the ability of so many different experiments to give close numerical agreement with an inverse square force law assumption provided robustness to Cavendish's experimental argument.

(4.2) Philosophy as an Historical Tool

Dorling accepted without question Cavendish's final results from his Great Globe experiment ignoring all the experimental complications and the series of experiments which formed the context of this first experiment. Instead, Dorling argued that it was rational for Cavendish to believe in an inverse square force law by ascribing changing subjective degrees of belief that Cavendish and his contemporaries would have (or should have?) held before and after his evidence was gathered about various global electrical theories, the existence of forces and inverse force laws, and the probability of the evidence because plugging these imaginary (or second degree subjective beliefs) into Bayes' theorem shows such a belief would be rational for Cavendish to hold! However, quantifying beliefs about beliefs, especially two hundred year old beliefs, does not in itself seem a particularly rational method for studying an experimental episode. Thus, a Bayesian analysis of historical experimental episodes is untenable.[259]

[259]While Mayo (1996) demolishes Bayesianism as an epistemology, I feel the example above and my discussion in Chapter 2 shows it is not a good tool for historical research.

Laymon mentions many of the experimental complications Cavendish faced but not how he resolved them. Laymon's strength is in explaining Cavendish's mathematical arguments and in showing, albeit in his footnotes, that the experiments were interrelated and interdependent. However, ignoring this experimental dependency and searching for a deductive experimental epistemology, Laymon contends that Cavendish's sensitivity analysis was circular because it was based on Experiment V, which Cavendish thought showed a small but "real" departure from an inverse square law.

Laymon claims the above departure made Cavendish's sensitivity analysis circular. He then proposes to render this circularity harmless by ascribing continuity assumptions to Cavendish's argument. Continuity assumptions for this case state that "if the variation from inverse square were large, then by continuity, the variation in calculated experimental sensitivity would also be large."[260] This is the assumption he thinks Cavendish needed. Moreover, Laymon does not seem to think this is anything but an assumption, one without (non-circular) support. I argue that Cavendish's experimental series formed a severe test of this force law and Laymon's circularity contention is false. Cavendish's "continuity assumptions" were not assumptions but grounded in severe testing procedures and arguing from error. That is, given the variety of experiments performed and the controls Cavendish instituted to isolate the movement of charge under investigation, his sensitivity calculation was justified.

Laymon's attempt to explain Cavendish's inference as being deductively justified based on continuity assumptions seems wasteful given the wealth of experimental

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[260]Laymon, p. 39-40.

arguments Cavendish actually offered. Trying to turn science into a deductive enterprise, whether dressed in the guise of Dorling's Bayesian version of demonstrative induction or Laymon's combination of demonstrative induction and hypothetical deduction, like all the other deductive attempts before, are doomed to failure. Experimental practice is simply too complex and messy, as my analysis has shown, to fit into a deductive schemata.

Only a very naive person views historians as mere compilers of facts. Historians, implicitly or explicitly, are guided by their research philosophy. I have tried to illustrate in this thesis that Mayo's epistemology makes an excellent historical tool for appraising and gaining an understanding of experimental episodes. Her hierarchy of models provides an excellent organizing tool for disentangling extremely complicated experimental narratives, such as Cavendish's series of electrical experiments. Moreover, her hierarchy, while providing structure, is also flexible enough to handle pre-nineteenth century case studies as I have illustrated in the last two chapters.

In contrast with Dorling's and Laymon's epistemologies, Mayo's epistemology does not encourage researchers to ascribe assumptions to historical characters. She argues that researchers need to look at the evidence experimenters claimed convinced them that their inference was correct. Like the annales school, her philosophy concentrates on examining the details of an episode--experimental design, the gathering and modeling of data, etc.--to understand experiments and experimental inferences. Some readers may ask whether her philosophy does not implicitly ascribe a "concern with severity" assumption to experimenters. This is not the case. Severity is not an assumption ascribed to historical characters but a criterion that can be used to justify or critique experimental arguments and data.[261] Nor does this focus on data mean there are never "non-scientific" reasons motivating experimenters' inferences. The whole point of scientific objectivity is that *despite* social and other factors, claims to knowledge are held to inter-subjective constraints within the scientific community. Analyzing the data can help bring to light non-epistemological factors influencing decisions, especially when the data does not support an inference. For all these reasons, I believe Mayo's philosophy makes a good research tool for historians of science, whether used alone to study a particular episode as I have or used in tandem with other methods for understanding an episode within a larger historical or social context.

(4.3) Mangled Regresses

Collins' experimenters' regresses state that experimenters often find themselves locked into an infinite regress both in getting new machines to work and in repeating experiments and thus they must use social negotiation or other non-scientific means to break the regress. Neither of Collins' regresses stand up to scrutiny in Cavendish's case. Moreover, analyzing Cavendish's researches has illustrated how experimenters' get around these supposed regresses. I have shown in chapter two that Cavendish got his detection machine to work by focusing on the ways it could go wrong, simulating and amplifying suspected errors, and then taking steps to eradicate, minimize or otherwise take them into account. He used low level knowledge about machines in general and known laboratory effects (e.g., induction), and he designed several sub-experiments to get his apparatus

[261]See my section 1.4 In a nutshell, severity links naturally to procedural objectivity. Severity is objectively assessed by requiring a hypothesis to pass a severe test before accepting it as warranted, and by always specifying further ways to check it, we proceed by objective methods.

working properly. These tactics, along with his mathematical tools, gave Cavendish insight into how his machine would function when working and how it could malfunction. Thus, Cavendish's electrical researches illustrate how experimenters get around Collins' purported detection machine regress. Cavendish knew his apparatus worked based on the above criteria, not because he detected an inverse square force law was in operation.

In both chapters two and three, I have also argued that, rather than trying to exactly duplicate his first experiment in order to learn anything of interest and to truly test his theory, Cavendish varied his experiments purposefully. These variations allowed him to test his experimental assumptions as well as move from a qualitative assessment of an inverse square law to a quantitative one. Taken together, these experiments provided a severe test of his inverse square law hypothesis *because* of their differences.[262] Had Cavendish's inverse square hypothesis been false, then it was highly unlikely given *all* eight different experiments that this would go undetected. Thus, while Cavendish did repeat all of his experiments several times, the strength of his argument lay in the close agreement of these several different experimental results with his theoretical calculations.

Pickering's strongest challenge to objectivity in his somewhat obscure mangle is his claim that experimenters not only assess their experimental results in light of their theories, but that they also manipulate their experiments and apparati until they achieve the desired or predicted results. Within the realm of allowable manipulations, in Pickering's view, socio-cultural factors (e.g., subjective interests and biases) stand on

[262]Differences such as the size and shape of the objects being compared as well as tying together different predictions based on an inverse square law. See both my table at the beginning of this chapter and my figure 7 in this chapter for a summary of these differences.

equal footing with epistemological factors (e.g., searching for objective procedures). Moreover, and Pickering agrees, manipulations based on subjective interests leave experimental results open-ended and unjustifiable on epistemological grounds. Again, in Cavendish's case, his manipulations of both his equipment and data can be and were justified on the grounds that he was trying to detect and fix sources of error, if present, and improve the precision of his results. His sub-experiments clearly demonstrate this point for his physical manipulations of his equipment. His statistical manipulations of his data were equally supported. If Cavendish's manipulations were guided by bias they would, with high probability, not have passed such a variety of tests. Thus, Pickering's most damaging allegation, that experimenters manipulate their experiment until they get the result they want, is untenable in this series of experiments.

(4.4) Objectivity Revisited

What claims about procedural objectivity can one make for Cavendish's electrical researches? Just as Mayo takes a piecemeal approach to testing, I will take a piecemeal approach to this question. In chapter one, I discussed Scheffler's argument that objectivity is a scientific standard that holds scientists responsible for the claims they make. The requirements for objectivity on his view are acknowledging that one can be wrong and making progress in finding methods to control the errors that can make one wrong. Cavendish clearly meets these requirements. He devoted most of his experimental narrative to detailing errors in his experiment and the methods he used to control them. He also acknowledged discrepancies between his results and his theory and communicated his reasons for why they were either problematic or not.

Cavendish cultivated quite a lot of specific low-level knowledge about how his apparatus could go wrong and other experimental errors. He gained this knowledge by running separate sub-experiments to simulate and amplify suspected sources of error. For example, he discovered his glass insulating rods conducted electricity, enough to disturb his results, and also learned that this problem could be minimized by coating them in sealing wax. Cavendish built up a repertoire of technical errors--from the conductivity of his glass rods and the surrounding air to the variation parameters of null readings on his trial plate--and learned to control them. This knowledge of specific errors was independent of any particular electrical theory. This was objective, if not ground shaking, knowledge. Moreover, this sort of low-level, detailed knowledge was extremely important in order to argue that his experiments provided a severe test of his inverse square force law hypothesis.

I have argued in the last two chapters that because Cavendish's hypothesis survived a severe test, composed of his entire experimental series, he was justified in holding his hypothesis that an inverse square force law correctly describes electrical attraction and repulsion. This is because the range and complexity of this series of experiments and sub-experiments taken together would have, with high probability, detected the falsity of his hypothesis, if it was false. To put my point another way, this complex matrix of experiments made it very improbable that he would get such close agreement between his experimental results and theoretical calculations were his hypothesis false. It is much more likely, looking at the series as a whole, that he would have observed larger discrepancies between his experimental results and predications in several of the experiments, if his hypothesis was incorrect. Cavendish remarked throughout his narrative that "I think the difference being so small is a strong sign that the theory is true."[263]

But what about his other hypothesis, his canals of incompressible fluid? This hypothesis was not severely tested. Actually, it was never really tested at all. Instead, Cavendish's canals were an instrumental idealization for approximating wires to aid his theorizing![264] Instead of testing his canals, Cavendish's premise in these experiments was that, for purposes of theoretical calculation, long thin wires could be approximated by canals of incompressible fluid.[265]

However, as we have seen, Cavendish did successfully construct a severe test of his hypothesis about the attractive and repulsive behavior of electricity through this complex series of mutually re-enforcing and cross-checking experiments and sub-experiments. Cavendish, as the result of severely testing his hypothesis, gained very important *experimental knowledge*. Mayo defines experimental knowledge as:

> learn[ing] about the (actual or hypothetical) future performance of experimental processes -- that is, about outcomes that would occur with specified probability if certain experiments were carried out. ...I mean to identify knowledge of experimental effects (that which would be reliably produced by carrying out an appropriate experiment) --whether or not they are part of any scientific theory.[266]

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[263]Article 278. Similar observations are found in 234, 277, and 294.

[264]Articles 40 and 69.

[265]Cavendish was extremely ambiguous about the reality of his canals as I have previously discussed in Chapter 2, section 2.1. As already noted, Heilbron suggests these canals were an instrumentalist device to facilitate Cavendish's calculations: "The canals represent wires. The assumed incompressibility of the fluid within them is an artifice introduced to preclude accumulations or deficiencies Cavendish could not calculate; in the case of long, think wires, as he expected, the idealization creates unimportant errors (Heilbron, p. 481)."

[266]Mayo (1996) pp.11-12.

Cavendish's inverse square force law hypothesis described a laboratory effect that could be reliably harnessed, along with other experimental knowledge, to manipulate and control electrical charge.[267] By understanding Cavendish's hypothesis as dealing only with how electricity does or would operate in experiments, no conflict arises over how his inference has survived global theory changes which make ontological claims about what electricity is. And thus my puzzlement over the success and weird status of electrical science is solved even though my puzzlement about the ontological status of electricity and electrical charge is not.

[267]While experimental knowledge may be independent of larger global theories which purport to explain just what electricity is, such theories must take into account this experimental knowledge about an experimentally determined property of electrical attraction and repulsion. Similarly, once Leyden jars became part and parcel of the electrical laboratory, electricians were forced to devise and hold only theories which could account them.

BIBLIOGRAPHY

Achinstein, P., (1971), Law and Explanation. Oxford: Clarendon Press.

Aykroyd, W. R., (1935), Three Philosophers, (pp. 71-87). Westport: Greenwood Press, Publishers.

Baker, K. M., (1975), Condorcet: From Natural Philosophy to Social Mathematics. Chicago: Chicago Press.

Bernoulli, D., (1777), "The most probable choice between several discrepant Observations and the formation therefrom of the most likely Induction," pp. 155-167. Studies in the History of Statistics and Probability. (eds.) E. S. Pearson, J. G. Kendall. London: Charles Griffin & Company Limited.

Berry, A. J., (1960), Henry Cavendish: His Life and Scientific Work. London: Hutchinson of London.

Bishop, M., (1996), "Book Reviews," pp. 145-146. PSA, V.

Boyd, R., Gasper, P., Trout, J. D., (1991). "Introduction," pp. xi-xiv. The Philosophy of Science. (eds.) Boyd, Gasper, Trout. Cambridge, Massachusetts: MIT Press.

Boynton, H., (ed) (1948), The Beginnings of Modern Science. Roslyn, New York: Walter J. Black, Inc. Selections by Cavendish, H., (pp. 104-110, 258-262), Chapter IV: "Electricity" and Chapter IX: Scientists Think About Science".

Cassier, E., (1951), The Philosophy of the Enlightenment. Princeton: Princeton University Press.

Cavendish, H., [1879 (written between 1771-1781)], The Electrical Researches of Henry Cavendish. Ed. James Clerk Maxwell. Cambridge: Cambridge University Press.

Cavendish, H., The Scientific Papers of the Honorable Henry Cavendish. 2 Volumes. Cambridge: Cambridge University Press.

Collins, H., (1985), Changing Order. London: SAGE Publications.

Collins, H., (1994), "A Strong Confirmation of the Experimenters' Regress," pp. 493-503. Studies in History and Philosophy of Science V. 25, No. 3.

Crowther, J. G., (1962), Scientists of the Industrial Revolution, London: The Cresset Press.

Cunningham, A. and P. Williams, (1993), "De-centering the 'big picture': The Origins of Modern Science and the modern origins of science," pp. 407-432. British Journal for the History of Science, V. 26.

Daston, L., (1988), Classical Probability in the Enlightenment. Princeton: Princeton University Press.

Dorling, J., (1974), "Henry Cavendish's Deduction of the Electrostatic Inverse Square Law From the Result of a Single Experiment," pp. 327-348. Studies in History and Philosophy of Science, V. 4, no. 4.

Franklin, A., (1994), "How to Avoid the Experimenters' Regress," pp. 463-491 Studies in the History and Philosophy of Science, V. 25, No. 3, 1994.

Galison, P., (1987), How Experiments End. Chicago: University of Chicago Press.

Gooding D., Pinch, T., and Schaffer, S. (1989), "Preface & Introduction," pp. xiii-30. The Uses of Experiment: Studies in the Natural Sciences. Cambridge: Cambridge University Press.

Hacking, I., (1983), Representing and Intervening: Introductory Topics in the Philosophy of Natural Science. Cambridge: Cambridge University Press.

Hackmann, W. D., (1989), "Scientific Instruments: Models of Brass and Aids to Discovery," pp. 31-65. The Uses of Experiment: Studies in the Natural Sciences. (eds.) Gooding D., Pinch, T. and Schaffer, S. Cambridge: Cambridge University Press.

Hahn, R., (1990), "The Laplacean View of Calculation," in The Quantifying Spirit in the 18th Century. (eds.) Frangsmyr, T., Heilbron, J. L., and Rider, R. E.,. Berkeley: University of California Press.

Hankins, T. L., (1985), Science and the Enlightenment. Cambridge: Cambridge University Press.

Heilbron, J. L. (1979), Electricity in the 17th and 18th Centuries. Berkeley: University of California Press.

Hesse, M. (1970), Models and Analogies In Science. Notre Dame: University of Notre Dame Press.

Hesse, M. (1962), "Is There an Independent Observation Language," pp. 35-77. Minnesota Studies in the Philosophy of Science, V. III.

Home, R. W., (1981), The Effluvial Theory of Electricity. New York: Arno Press. (Originally his dissertation at Indiana University.)

Jackson, J. D. (1975), Classical Electrodynamics, 2^{n}d Edition, pp. 5-7. New York: John Wiley & Sons.

Kryzhanovsky, L., (1992), "Why Cavendish Kept 'Coulomb's" Law a Secret," pp. 847-848. Electronics World and Wireless World: V. 98, no. 1679.

Kuhn, T., (1962), The Structure of Scientific Revolutions. Chicago: University of Chicago Press.

Laymon, R., (1994), "Demonstrative Induction, Old and New Evidence and the Accuracy of the Electrostatic Inverse Square Law," pp. 23-58. Synthese Vol. 99.

Maxwell, G., (1962), "The Ontological Status of Theoretical Entities," pp. 3-27. Minnesota Studies in the Philosophy of Science, V. III.

Mayo, D. G., (1996), Error and the Growth of Experimental Knowledge. Chicago: Chicago University Press, (in Press)

Megill, A., (ed.) (1994), Rethinking Objectivity. Durham & London: Duke University Press.

Moyer, A. E., (1991), "P. W. Bridgman's Operational Perspective on Physics: Part I: Origins and Development," pp. 237-258. Studies in History and Philosophy of Science. Vol 22, No. 3.

Moyer, A. E., (1991), "P. W. Bridgman's Operational Perspective on Physics: Part II: Refinements, Publication, and Reception," pp. 373-397. Studies in History and Philosophy of Science. Vol. 22, No. 3.

Pearson, E. S. (ed.), (1970), Studies in the History of Statistics and Probability. London: Charles Griffin & Company Limited.

Pickering, A., (1994), "Objectivity and the Mangle of Practice," pp.109-125. Rethinking Objectivity, (ed.) Megill, A. Durham & London: Duke University Press.

Reichenbach, H., (1938), Experience and Prediction. Chicago: The University of Chicago Press.

Salmon, W. C., (1966), The Foundations of Scientific Inference, Pittsburgh: University of Pittsburgh Press.

Scheffler, I., (1982), Science and Subjectivity. Indianapolis, Indiana: Hackett Publishing Company.

Shapin, S., (1988), "The House of Experiment in Seventeenth-Century England," pp. 373-404. Isis, V. 79.

Westergaard, H., (1969), Contributions to the History of Statistics, New York: Augustus M. Kelley, Publishers.

Wilson, G., (1851), The Life of the Honorable Henry Cavendish, London: Printed for the Cavendish Society. [Reprint (1975), History, Philosophy, and Sociology of Science. New York: Arno Press.