How Did Kettlewell's Experiment End? *

DAVID WŸSS RUDGE

Biological Sciences & The Mallinson Institute for Science Education
Western Michigan University, Kalamazoo, MI 49008-5410
david.rudge@wmich.edu

ABSTRACT: The past quarter century has seen an enormous growth of interest among scholars of science and technology in both particular experimental episodes and the process of experimentation. Among the most influential accounts have been those developed by Allan Franklin (1986, 1990), Deborah Mayo (1996) and Peter Galison (1987), each of which was developed primarily with reference to examples drawn from the history of physics. One useful way to access the generality of an account of experiment is to see how it fares with reference to examples drawn from disciplines far removed from the context within which it was developed. In previous essays I examined and compared the adequacy of Franklin and Mayo's views on experiment with reference to an episode drawn from the history of evolutionary biology, H.B.D. Kettlewell's classic studies of the phenomenon of industrial melanism (Rudge 1998, 2001). The present essay reanalyzes Kettlewell's work once more, this time as a test of Peter Galison's provocative account of experimentation in the sciences. Kettlewell's investigations can indeed be interpreted within Galison's perspective, but this appears to reflect the vagueness of many key distinctions Galison makes more than any special insights his views provide on the nature of experimentation in evolutionary biology.

1. Introduction

The past twenty-five years since the publication of Ian Hacking's watershed *Representing and Intervening* (1983) has seen an enormous growth of interest among scholars of science and technology in both particular experimental episodes and the process of experimentation. This trend is particularly evident in the proliferation of studies of historical episodes of experimentation conducted by philosophers, historians, and sociologists of physics (e.g. Achinstein and Hannaway 1985, Batens and Bendegem 1988, Gooding et. al. 1989, Le Grand 1990). Their analyses, partly in reaction to

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traditional accounts focused exclusively on the results of experiment in relation to theory, draw attention to the nature of experimental practice as a social activity and instrumentation as part of its material culture. This trend is part of a broader movement in contemporary science studies that emphasizes sociological and historical aspects of scientific practice. Beyond stressing how external influences such as a scientist's religious or ideological views affect decisions regarding theory choice (the purview of traditional external histories of science), contemporary analyses increasingly stress sociological and political aspects of the research process (e.g. a scientist's need to gain research funding and prestige within the community). These analyses have also demonstrated how, using the tools of literary criticism and citation analysis, reports of experiment can be examined as literary texts used to win agreement and prestige from other members of the scientist's research community.

Accounts of scientific practice that emphasize the role of extra-scientific elements seem counter-intuitive to readers versed in more traditional internalist accounts of science that portray the practice of science as a paradigmatic example of rational and objective inquiry based on evidence. They are also often read as undercutting traditional historical and philosophical questions concerning the relationship of evidence to theory, because they implicitly suggest that studies of experimental episodes divorced from their sociological context are not simply incomplete, but nonsensical.

It would, of course, be incorrect to suggest that scientific texts lack rhetorical elements or that they are written without the expressed aim of convincing the intended reader of the correctness of some particular claim(s). Nor would any contemporary scholar of science deny that the process of science is a social activity with attendant rewards and punishments associated with conformity to culturally and socially-determined standards. The locus of serious debate between scholars on these topics concerns the extent to which such elements affect the process of science.

Some, particularly those with connections to the so called "strong programme" in sociology, have pursued a radical underdetermination thesis. In essence they argue that since the data themselves cannot provide a basis for a unique interpretation, a scientist's choice of how to interpret data fundamentally involves reference to extra-scientific considerations, such as his/her intellectual framework and how receptive the community will be to the interpretation. On this view, the process of science is construed as a fundamentally subjective and sociologically-driven enterprise (e.g. MacKenzie 1989).

Others interpret the stress on sociological aspects of science as nothing short of an attack on the rationality of science. Alan Franklin in a series of influential books and articles argues that the history of physics reveals that scientists do behave reasonably in choosing between theories on the basis of experiments by drawing attention to a host of strategies scientists use to determine whether their results are “valid”, each of which can be justified with reference to Bayes theorem (Franklin 1986, 1989, 1990, 2007). Peter Galison (1987), drawing from historical examples in the context of high energy physics, similarly aims to defend the rationality of scientific reasoning from the excesses of the strong programme in sociology. Galison’s analysis is intended to reveal the debates and assumptions that lie behind decisions that the effect “will not go away”, one that emphasizes the dynamics of social interaction between theoretical and experimental cultures, yet nevertheless resists the conclusion that the decision of when to end an experiment is made on the basis of sociological or theoretical presuppositions alone.

One way to gain insight into the merits of these two contrasting approaches to understanding experimental reasoning in the sciences is by seeing how they fare with reference to examples drawn from contexts other than which they were developed. Rudge (1998) demonstrated Franklin's Bayesian perspective can accommodate H.B.D. Kettlewell's classic investigations of the phenomenon of industrial melanism. The present essay analyzes Kettlewell's work once more, this time from Peter Galison's perspective. The essay begins by reviewing the phenomenon of industrial melanism and Kettlewell's work on the subject. It then establishes that Galison's perspective can accommodate this example before comparing and contrasting Franklin and Galison's responses to the challenge posed by the strong programme in sociology with reference to this episode.

2. The Phenomenon of Industrial Melanism

Large scale manufacturing associated with the Industrial Revolution (which started in Britain and Continental Europe during the mid 1800s) led to a dramatic increase air pollution. This had profound effects on the surrounding environment. In manufacturing centers smog darkened the skies and buildings became visibly darker owing to the accumulation of grime and soot. Waste gases such as nitrogen and sulfur oxides adversely affected inhabitants, leading to widespread respiratory problems. Exposure to these contaminants likewise affected the flora and fauna of rural areas downwind of industry, initially killing off the lichen cover and gradually over time darkening the surface of tree trunks. Naturalists throughout Britain and Continental Europe noticed, coincident with these changes, that rare dark forms of many moth species were becoming more common in the vicinity of manufacturing centers. The *phenomenon of industrial melanism* refers to this rapid rise in the frequency of dark (melanic) forms in many moth species that appeared to have occurred as a consequence of industrial air pollution.

The most famous and best studied example of the phenomenon of industrial melanism is the peppered moth, *Biston betularia*, a common moth found throughout Britain and Continental Europe. Like other moth species, the adult form was believed to be nocturnal (active at night), spending most of the day motionless on trees, rocks and other resting sites. The dark form in this species was first discovered in 1848 near Manchester (already a major manufacturing center) by Robert Smith Edelston (Edelston 1864), and over the course of next 50 years, numerous additional sightings by naturalists and amateur lepidopterists led to widespread recognition that the range of the heretofore rare dark form was spreading throughout Britain and Continental Europe. These anecdotal reports also documented a dramatic increase in the frequency of the dark form in the vicinity of polluted areas.

This widespread interest and curiosity surrounding a phenomenon occurring right before their eyes ultimately led the Evolution Committee of the Royal Society to institute a collective inquiry in which they sent circulars to moth collectors throughout Britain inviting them to survey local moth populations and reflect on any changes witnessed during their own lifetimes. Although the sporadic nature of responses to this inquiry prevented the committee from drawing any strong conclusions, it did allow them to document in a more systematic fashion that the change was indeed occurring (Doncaster 1906).

Since it was first discovered, many explanations have been offered to account for why dark forms in many species of moth were becoming more common near manufacturing centers. Among the first were what might be referred to as a "Lamarckian" explanations. Nicholas Cooke (1877), for instance, drew attention to the fact that the dark form was becoming more common not only in industrial areas, but also areas where increased humidity likewise darkened tree trunks. He suggested that in industrial melanic species, like the peppered moth, the change to a darker form represented a physiological response to a changing environment, owing to changes in climate.

J.W. Heslop Harrison alternatively suggested that the increasing frequency of melanic forms of moths in the vicinity of manufacturing areas was a direct consequence of industrial pollution. Harrison claimed lead and manganese salts contained in soot that covered food plants of moths had mutagenic properties that caused mutation of genes for melanin-production. In support of these claims, Harrison cited the results of breeding experiments he had conducted on caterpillars of *Selenia bilunaria* and *Tephrosia bistortata* fed on polluted foliage (Harrison and Garrett 1926, Harrison 1928). These results were later challenged by Kettlewell's mentor, E.B. Ford, who questioned the legitimacy of using species that did not exhibit a trend towards industrial melanism. Ford pointed out that in the vast majority of industrial melanic species, the melanic form was believed to be dominant in contrast to the two species Harrison used (Ford 1955, p. 197). Ford also drew attention to the fact that independent investigators were unable to repeat Harrison's experimental results (Hughes 1932, Thomsen and Lemeche 1933).

The first detailed published account of industrial melanism in terms of natural selection is generally attributed to James Tutt (1890), who, building off the work of Buchanan White (1876-77) and others, drew attention to the role of selective elimination by birds. A brief comparison of the two forms of the moth when they rest against tree trunks in unpolluted and polluted settings reveals the intuition behind Tutt's theory (see Figure 1). In unpolluted environments of the sort one finds in the rural countryside, trees are covered with lichen, which makes them a pale background against which the "typical" form of the moth is very difficult to spot, but the presence of the dark form is obvious. This contrasts with the situation one finds in forests near manufacturing centers where years of exposure to air pollutants has led to the removal of lichen and a visible build up of soot, effectively darkening the tree trunks. In these environments, it is the dark form that is difficult to spot and the pale form that is easily seen. Thus, Tutt concluded, the spread of the dark form could be accounted for entirely in terms of selective predation by birds in the two environments. In unpolluted environments, whenever the dark form arose by mutation (or was introduced by migration) it would be quickly eliminated by birds, as such, in these environments the pale form is common. In polluted environments, in contrast, the pale form is the form most vulnerable to bird predation, as such, in these environs, it is the dark one that has increased over time.

Figure 1. *Biston betularia*: one typical and one *carbonaria* resting on a lichen-covered tree in unpolluted country (Dorset); and, one typical and one *carbonaria* resting on blackened and lichen-free bark in an industrial area (the Birmingham district). These photos originally appeared separately as Plates 14 and 15 in Ford ([1964] 1975).

About the turn of the century, geneticists began to take an interest in the phenomenon of industrial melanism. Researchers in this emerging field were just beginning to recognize the numerous advantages of lepidoptera as model organisms for the study of heredity and population genetics, such as their relatively short life spans and the fact that their easily observed wing patterns had a genetic basis (Ford [1964] 1975). Pioneering work by Col. W. Bowater established that the dark form in the peppered moth, *carbonaria*, was the result of a single dominant gene (Bowater 1914). Bowater also drew attention to the existence of intermediate melanic forms, later referred to as *insularia*, which range in appearance from individuals nearly as pale as the typical form to those that are nearly as dark as *carbonaria*. Additional breeding experiments by E.B. Ford and others strongly suggested that the gene responsible for dark coloration might also have a physiological effect on the constitution of the moth, making it "hardier" than the pale form. Precisely what these investigators meant by "hardier" seems to have varied with the investigator, some identified it with a tendency to emerge earlier in the year and at a lower temperature, others identified it in terms of higher than expected frequencies in backcross broods. In view of these results, Ford offered an alternative theory for industrial melanism in terms of natural selection that invoked two selective forces. Ford argued that the rapid spread of the melanic gene was primarily due to the physiological advantage it conferred. Ford explained why the spread was limited to industrial areas by drawing attention to the obvious handicap of dark coloration in unpolluted environments against visual predators such as birds (Ford 1937).

During this same time period the phenomenon of industrial melanism became of increasing theoretical interest for evolutionary biologists as well. Darwin's original presentation of his theory depicted natural selection as a slow process that led to the gradual accumulation of numerous slight variations over geological time. Indeed, Darwin himself went so far as to publicly doubt that natural selection could be directly observed during the brief span of a human lifetime. This consideration, coupled with numerous other apparent difficulties for his theory of natural selection, such as Lord Kelvin's 1868 theoretical estimate that the earth was about a hundred million years old (far below the amount of time required on Darwin's theory), briefly led to a period in the history of biology known as the "eclipse of Darwinism" in which Darwin's theory of natural selection was publicly doubted by many scientists during the turn of the twentieth century. Within this context, the phenomenon of industrial melanism provided a particularly striking example of natural selection for proponents of Darwin's theory, an example that J.B.S. Haldane in a very influential paper used to emphasize, in contrast to

Darwin's portrayal, how very powerful the force of selection in nature could actually be (see Figure 2). Haldane pointed out that if *carbonaria* was a simple Mendelian dominant and represented only 1% of the population in Manchester when it was first spotted in 1848 and completely ousted the typical form by 1901, the minimum selective advantage of the dominant gene would have to be roughly 50% greater than the recessive (Haldane 1924).

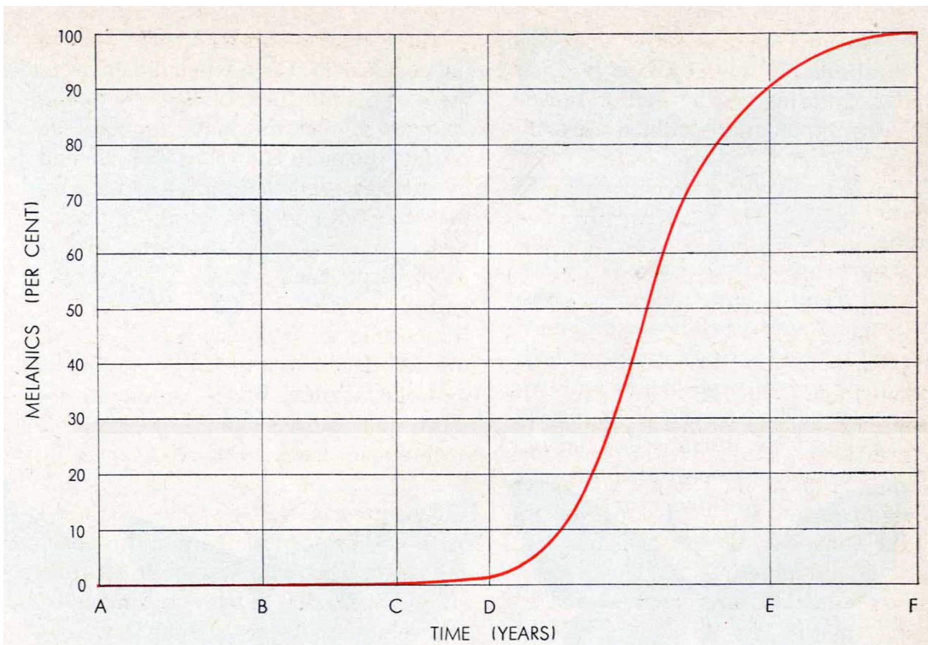

Figure 2. SPREAD OF MUTATION from the light form to the dark (melanic) is expressed by this curve. The mutation occurs in the period AB, spreads slowly during BD and spreads rapidly during DE. During EF the light form is either gradually eliminated, as indicated by the curve, or remains at a level of about 5 per cent of the population. (Reproduced from Kettlewell (1959)).

This estimate was particularly important in that it was far in excess of what theoreticians had previously regarded as a realistic value for the force of selection in nature. Industrial melanism also figured prominently in theoretical debates between Haldane and Sir Ronald Fisher, two extremely influential figures in the development of mathematical theories of population genetics that provided an important basis for the "evolutionary synthesis", a dramatic period in the history of biology during the 1920s and

1930s during which a broad consensus on numerous fundamental issues in evolutionary biology was forged.

3. Kettlewell's 1953 and 1955 Investigations

With hindsight, it is easy to see how the foregoing developments mentioned above set the stage for someone to systematically study the phenomenon of industrial melanism. That someone was H.B.D. "Bernard" Kettlewell (1907-1979), a life-long amateur naturalist and entomologist, who at the age of 45 left medical practice to pursue his hobby full time as a research worker under the supervision of E.B. Ford in 1952. Edmund Briscoe "Henry" Ford was a pioneering researcher in the new field of "Ecological Genetics" he and others, including A.J. Cain and Philip Sheppard, were founding at Oxford University during the 1950s. Ecological genetics, broadly speaking, is the study of adjustments and adaptations of natural populations to changes in their environments by a combination of field and laboratory techniques. Cain and Sheppard originally worked on snail banding patters in the Grove Snail, *Cepaea nemoralis*; Ford and others devoted themselves to the study of population fluctuations in several species of lepidoptera, including the Scarlet Tiger Moth (*Panaxia dominula*) and the Meadow Brown Butterfly, *Maniola jurtina*. As noted above, Ford was keenly interested in the phenomenon of industrial melanism, which he had independently studied from the standpoint of genetics and his own pilot field study (Ford 1937, 1953). While it is fair to say that Kettlewell inherited the project of work on industrial melanism from Ford, the extent to which Ford actually mentored him is less clear (Rudge 2006).

Kettlewell initially pursued industrial melanism as one project among several others, including some pioneering work on the use of radioactive tracers to track locust populations. Over time, however, his growing interest in all aspects of the phenomenon of industrial melanism led him to devote his entire career to the study of the subject. Kettlewell studied the direct effect of air pollution on local vegetation by means of monthly leaf washings that allowed him to quantify the amount of soot accumulating on the surfaces of leaves in the vicinity of manufacturing centers. Comparative studies of foliage and tree trunks in the two settings led him to recognize that aphids and lichen often die off in the presence of low levels of contaminants, presaging their use as bioindicator organisms. Kettlewell also orchestrated a comprehensive survey of industrial melanics throughout the whole of the British Isles (see Figure 2). The latter ultimately led to the amassing of more that 100,000 records of melanic and typical frequencies in over 50 species of Macrolepidoptera by 100 part time lepidopterists (Kettlewell 1973). With regard to the peppered moth, *Biston betularia*, these records amply documented the spread and rise in frequency of the *carbonaria* gene responsible for dark coloration in the vicinity of manufacturing centers. This work also documented a striking correlation between areas where the dark form was becoming more common and air pollution.

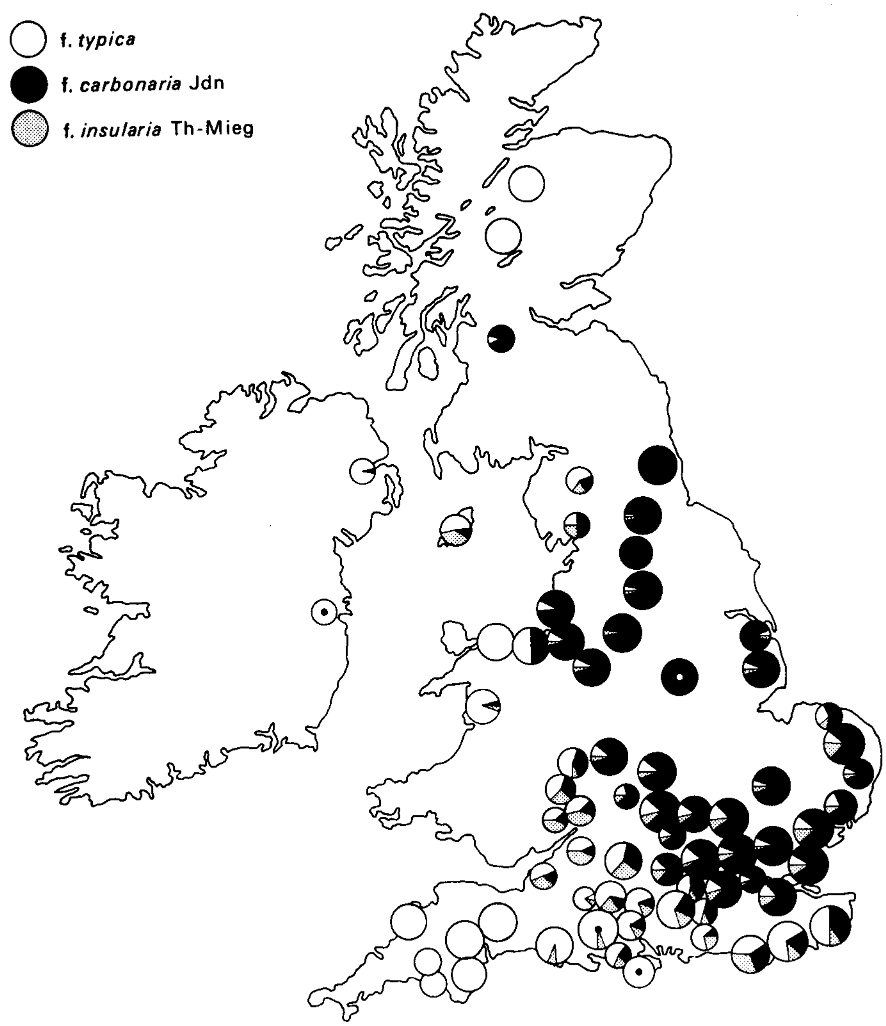

Figure 3. A frequency map of *Biston betularia* and its two melanics, f. *carbonaria* and f. *insularia* comprising more than 30, 000 records from 83 centres in Britain. (Reproduced from Kettlewell (1973), *The Evolution of Melanism*, from Figure 9.1, p. 135, by permission of Oxford University Press.)

The studies for which Kettlewell is most famous, however, are a series of field investigations he conducted in the early 1950s. His initial investigation, conducted in the summer of 1953 involved three steps. First, he conducted what he referred to as a scoring experiment, in which he developed a method of objectively determining how conspicuous or inconspicuous pale and dark moths were when they rested against pale lichen-covered or soot-darkened pieces of bark. His specific technique involved placing moths representing the three forms (typical, *insularia* and *carbonaria*) on representative pieces of bark from the two settings and then determining how far away one could walk from the bark and still spot the moth. As a result of these trials, Kettlewell and his associates determined that the typical (pale) form was regarded as inconspicuous when it rested on lichen-covered bark, but quite conspicuous when it rested on soot-darkened bark. The reverse was true for the *carbonaria* (dark) form, which was easily spotted when it rested against lichen-covered bark but much more difficult to spot when it rested on a soot-darkened piece of bark. Kettlewell laid great stress on the fact that multiple observers independently reached the same conclusions as establishing the reliability of this scoring procedure.

The second step was to document that birds have the same difficulty humans do when it comes to spotting moths when they rest on matching backgrounds. Kettlewell had to consider the possibility birds would have keener powers of detection than humans; moreover, at the time of his studies, many naturalists publicly doubted that birds prey upon moths at all. To address this question, Kettlewell built a large cage within an aviary, which he subsequently divided by a large sheet into two sections, one of which contained two nesting Great Tits, *Parus major*. In the second section Kettlewell and his associates introduced numerous pieces of pale lichen-covered and soot-darkened pieces of bark, upon which 16 moths representing the three forms were released. Kettlewell then removed the sheet, exposing the moths to predation by the birds, and monitored the experiment from a distance using binoculars and also by periodically checking to see which moths remained. Kettlewell found to his dismay that the birds ignored the moths for the first two hours, after which they ate all of the moths on their incorrect backgrounds as well as two on their correct backgrounds in the space of an hour. The high predation rate on both conspicuous and inconspicuous moths led Kettlewell to suspect the birds were becoming specialists on peppered moths. In a subsequent trial he introduced a broad spectrum of endemic insects. This alteration “proved successful”, in that from this time onward, Kettlewell was able to document that the birds preyed primarily on the most conspicuous form when presented with a choice.

The third step was to assess whether Kettlewell could document these same results in nature. These are the classic field experiments he conducted in a heavily polluted wood in Birmingham (and later in an unpolluted wood near Dorset) using a technique known as mark-release-recapture previously developed by Fisher and Ford (1947). As the name suggests, the experiment involved three steps. First, Kettlewell raised a total of 630 male moths representing the three forms (137 typical, 46 *insularia* and 447 *carbonaria*) marked with a dab of quick drying cellulose paint on the undersurface of the wings. Second, Kettlewell released moths onto tree trunks, one per trunk, in a well-circumscribed forest with several natural boundaries to minimize migration from the test sight. The third and final step was an attempt to recapture as many of the released moths as possible, which he did using a combination of a mercury vapour light trap in the center of the wood and multiple assembling traps containing virgin females around the periphery of the wood. Kettlewell reasoned that, all things being equal, the recapture rates of the three forms should be the same. If, however, one form was better able to survive than another, for example the *carbonaria* form was better able to avoid avian predators than the typical form, more of the favored form should be recaptured owing to the fact that more of them would presumably survive during the interval between release and recapture. And this is exactly what Kettlewell found (see

Table 1). Expressed as percentages, the recapture rate for *carbonaria* was 27.5% (123/447) over twice the recapture rate for typical, which was only 13% (18/137). Kettlewell clarified his interpretation of the results of this experiment by ruling out potential alternative explanations, such as the possibility that pale moths were more likely to migrate from the test site or that the dark form simply lived longer. Kettlewell also made numerous direct observations of bird predation when endemic birds were presented with a choice of moths representing the different forms. In each case he found they were much more likely to take the conspicuous moth first. In 1955, Kettlewell conducted a repeat smaller scale mark-release-recapture experiment in Birmingham and a slightly larger companion experiment in an unpolluted wood near Dean End, Dorset. The repeat experiment yielded similar results. In the unpolluted wood, he was able to document the reverse was true: in the Dorset wood it was typical that appeared to be at an advantage compared to *carbonaria*. The mark-release-recapture experiment in this setting resulted in a recapture rate for typical (13.7% (54/393)) that was nearly three times that found for *carbonaria* (4.7% (19/406)). Direct observations of bird predation in the unpolluted setting likewise suggested that birds were more likely to take the conspicuous form of the moth first, in this case, *carbonaria*. Kettlewell is widely regarded as clinching the argument by having his friend and colleague, the well known ethologist Dr. Niko Tinbergen, film the order of bird predation in the two settings. This latter piece of evidence was widely regarded as incontrovertible evidence that birds prey upon moths, and further that they do so selectively with reference to how conspicuous the moth is against its resting site.

	Birmingham (1953)	Birmingham (1955)	Dorset (1955)
Marked Individuals released	447 f. *carbonaria* 137 f. *typica* 46 f. *insularia*	154 f. *carbonaria* 64 f. *typica* 9 f. *insularia*	406 f. *carbonaria* 393 f. *typica* 0 f. *insularia*
Marked Individuals recaptured	123 f. *carbonaria* 18 f. *typica* 8 f. *insularia*	82 f. *carbonaria* 16 f. *typica* 2 f. *insularia*	19 f. *carbonaria* 54 f. *typica* 0 f. *insularia*

Table 1. The results of Kettlewell's mark-release-recapture experiments. (Developed from figures reported in Kettlewell 1955, 1956).

The results of Kettlewell's 1953 field experiments, conducted in the polluted wood near Birmingham, were initially published in E.B. Ford's (1955) *Moths,* part of the New Naturalist series (a popular series of scholarly books written for amateur entomologists). Kettlewell's results, as recounted in Ford's book, met with initial skepticism. Reviewers of the book publicly doubted that Kettlewell had actually observed large numbers of moths being preyed upon by birds as reported by Ford. It was this reaction, by amateur entomologists (clearly an important audience for Kettlewell) that led directly to his decision to make a film record of bird predation during the follow up 1955 experiments. Kettlewell later published the results of the 1953 and 1955 in the prestigious journal, *Heredity* (Kettlewell 1955, 1956). The second paper included two plates featuring eight photographs, five of which illustrated different species of birds caught in the act of preying on the moths. These publications, as well as professional presentations and exhibits featuring a short silent movie made from excerpts of the film record collected during his investigations, effectively ended public doubt that birds prey on moths and do so selectively (Rudge 2003).

4. Kettlewell's Study from Galison's Perspective[1]

Peter Galison's (1987) *How Experiments End* examines three episodes from the history of microphysics (Albert Einstein's studies of the gyromagnetic effect, Robert A. Millikan's studies of cosmic radiation, and the search for the neutral current by a team of workers at CERN). His is an explicit attempt to understand the "transformation" of evidence from subtle hint to persuasive demonstration that marks the end of the experiment. While openly acknowledging that what constitutes an experimental demonstration has changed over the history of physics, Galison claims experiments as essentially procedures aimed at distinguishing "signal from noise" by establishing background or mimicking effects are negligible.

Crucial for Galison's analysis is a distinction he draws between *theoretical* and *experimental cultures*, which function far more independently from one another than is often recognized. Galison defends this distinction on more than sociological grounds or a division of labor. Theorists often disagree over starting principles, assumptions or approximations, but not the "rules of legitimate inference from them" (p. 244). Experimentalists, in contrast, often agree with regard to the specific goals of their enterprise but disagree over specific details concerning the execution and interpretation of experiments. Galison points out this asymmetry between theoretical and experimental work is rarely captured in the published record because claims made of experimentalists

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[1] Unless otherwise noted, all page references in this section and the next refer to Galison (1987). The analysis in this section and the next are based on a more lengthy treatment provided in Rudge (1996), pp. 261-303.

are often stated as if they were knowable independent of the researcher’s judgement and expertise.[2]

Galison portrays the process of experimentation as a process by which investigators attempt to distinguish signal (the effect of interest) from background noise (disturbing effects) through the imposition of long- middle- and short-term *theoretical* and *experimental constraints*. The ultimate aim of experiments is the development of arguments that convince both experimentalists and their colleagues that the effect in question is "solid," i.e. not an artifact, using procedures and techniques that increase both the *directness* and *stability* of results. Experiments end, in short, when these arguments lead investigators to conclude the effect “will not go away”.

As will be shown below, Kettlewell’s classic investigations can easily be interpreted from Galison’s perspective.

Theoretical and Experimental Cultures

Galison's distinction between theoretical and experimental cultures captures a similar, albeit less distinct, division of labor present in Kettlewell’s investigations. Section 2 above drew attention to the perceived theoretical importance of the

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[2] “This asymmetry between experiment and theory is often hidden because experimentalists express their public claims in a language that suggests experimental results are independent of the researcher's judgments, experience, or skills. Reading an article, one could conclude that an effect would follow from an experimental setup with the inexorability of logical implication. But lurking behind the confidence of the experimental paper lies a body of work that relies on a kind of subtle judgment that is notoriously ill-suited for the prose of hypothesis and deduction. Experiments can only artificially be reduced to a protocol–and with the enormous growth of experimental facilities the difficulty of execution has only increased. Only the experimentalist knows the real strengths and weaknesses of any particular orchestration of machines, materials, collaborators, interpretations, and judgments.” (p. 244)

phenomenon of industrial melanism as an example of how rapidly selection can act in nature. Section 3 further pointed out that Kettlewell initiated his investigations as a test of E.B. Ford's specific theory for why melanic forms were becoming more common in areas near manufacturing centers. As such, it certainly makes sense to discuss this episode as having an associated theoretical culture.

At first glance, Galison's use of the term experimental culture might seem altogether inappropriate for describing Kettlewell's investigation. This is because Galison identifies experimental cultures with mechanical apparatus and associated craft techniques, machines, instruments and procedures that have no clear analogues Kettlewell's investigations.[3] Galison describes experimental culture as follows:

> Experimental culture is grounded in expertise–the ability to eliminate kinds of backgrounds and an instinctive familiarity with the valid limits of an apparatus. Judgment of this kind often only comes with the repeated use of certain classes of instruments. By the time they came to E1A, Mann and Rubbia were experts at spark chambers and electronic apparatus in general. Similarly, many of their counterparts in Europe had long standing knowledge of the techniques used in bubble chambers: photography, optics, scanning, acquired in earlier bubble-chamber work or in emulsion and cloud-chamber studies. (p. 248)

In brief, an experimental culture is identified closely with the use of a particular instrument or machine, items that play a much less prominent role in Kettlewell's work. This apparent disanalogy is particularly problematic when one considers that much of the decision to end an experiment on Galison's analysis relies heavily on the skills and judgments of individuals who have gained a familiarity with a particular apparatus, its strengths and limitations, when it is working correctly, etc.

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[3]Clearly Kettlewell made use of tools--e.g. Kettlewell used Mercury Vapour and Assembling traps to recapture moths. But, as discussed and argued for below, the "apparatus" of interest most analogous to Galison's use of this term in defining experimental cultures are the test populations of Kettlewell's studies.

The absence of machinery and other apparatus in biological examples, however, does not undercut application of Galison's notion of an experimental culture to Kettlewell's investigations. Galison also identifies experimental cultures in terms of the use of specific procedures, procedures that do not have to be defined strictly with reference to particular detectors or other instruments. More to the point, test organisms can be meaningfully understood as "technology" (c.f. Kohler 1994, esp. pp. 6-8). In this light, Kettlewell's use of artificially assembled populations of moths to detect the effect of selection in nature is wholly analogous to the use of particle detectors in high energy physics.[4] Kettlewell introduced his artificially assembled population of moths into a soot darkened environment precisely to detect and quantify the extent to which dark moths were at a selective advantage relative to pale moths in such a setting. It also emphasizes how Kettlewell's experiments must be understood not simply with reference to particular theoretical problems (mentioned above) but also with regard to ongoing traditions of experimentalists devoted to particular species. Kettlewell's investigations took place in culture composed of lepidopterists, butterfly collectors, breeders and other naturalists interested in the natural history of the peppered moth (and other affected moth species).

To truly understand the associated experimental culture of interest in this episode, however, one must also appreciate the fact that the phenomenon of industrial melanism is not merely a phenomenon that occurs in moths. It is at one and the same time a phenomenon having to do with air pollution, the ecological effects of air pollution on flora and fauna, and bird behavior. Kettlewell's surviving correspondence draws attention to the important role experts in each of these areas played in his investigations. To this

[4]Griesemer and Wade (1988, pp. 81-2) advance a similar notion of laboratory systems as "cause detectors."

list one might additionally include Philip Sheppard, who appears to have assisted Kettlewell with the experimental design of his investigations, and mathematicians, such as R.A. Fisher and J.B.S. Haldane, who assisted Kettlewell with the statistical analysis of his results (Rudge 2006). Indeed, on Galison's analysis, the associated experimental culture of this episode must also include individuals who assisted Kettlewell with numerous technological problems he encountered in the creation and use of a mercury vapour light trap (e.g. Hugh Robinson), which at the time was still a relatively new technology.

Theoretical and Experimental Constraints

In order to make sense of the guiding principles, gut intuitions and other aspects of experimental inquiry, Galison broadly distinguishes between theoretical and experimental constraints. He favors this division primarily for historiographical reasons--each of his episodes exemplify a relative independence of theoretical and experimental training, skills, and judgments, an independence which has increased during the twentieth century as the result of the scale and complexity of contemporary experiments in high energy physics. In addition to other virtues, he points out that this division also "avoid[s] the traditional image of science as an inescapable web descending from high theory to observational regularities" (p. 255) by recognizing the relative autonomy of lower level commitments from more abstract claims (*sensu* Hacking 1983). This is a widely accepted view on the relationship of theory and experiment among contemporary philosophers and historians of science, one which Brandon (1994) specifically defends in the context of evolutionary biology.

Within each broad type, Galison distinguishes between long- middle- and short-term constraints, modeling his schema after Fernand Braudel's (1972) similar division of history into geographical, social and individual events (pp. 246-255).[5] *Long-term theoretical constraints* are "metaphysical commitments to methods and goals that transcend beliefs about the nature of matter," such as a belief in conservation of energy or a desire to produce a unified theory. *Middle-term theoretical constraints* include more programmatic goals that transcend specific projects, as for instance, Einstein's goal of explaining or at least testing the possibility of a zero point energy. *Short-term theoretical constraints* are beliefs associated with particular projects, theories or models, e.g. Einstein's use of a physical model (the navigational gyrocompass) to address a more programmatic goal of explaining the existence of a zero point energy. Scientists also hold long, middle and short term experimental and instrumental beliefs, which likewise function as the starting assumptions of experimental inquiry and the principles by which moves are made to eliminate disturbing backgrounds in the development of arguments that the effect is stable. *Long-term experimental constraints* include confidence in particular instrument types and beliefs and attitudes regarding what constitutes a sufficient demonstration; *middle-term experimental constraints* involve beliefs that again go beyond the span of a particular investigation (e.g. beliefs concerning the reliability of a particular apparatus); *short-term experimental constraints* (e.g. beliefs concerning the validity of particular runs using the apparatus). All of these commitments are subject to

[5] These categories are not fixed-- Galison recognizes that some may disagree over whether a particular belief is best described as experimental or theoretical, or the degree to which a scientist is committed to that belief (p. 250).

revision or change; long-term constraints are characteristically longer enduring, short-term constraints, less so.

Examples of Galison's theoretical and experimental constraints present in Kettlewell's investigations are provided in Table 2.

THEORETICAL		
Long-term	Middle-term	Short-term
e.g. Kettlewell's belief that natural selection was an important evolutionary force	e.g. Kettlewell's commitment to a selectionist interpretation of the phenomenon of industrial melanism	e.g. Kettlewell's view that avian predation was responsible for differential survival among different forms of moth
EXPERIMENTAL		
Long-term	Middle-term	Short-term
e.g. Kettlewell's experience and knowledge of *Biston betularia*	e.g. Kettlewell's use of the mark-release-recapture technique	e.g. Kettlewell's confidence in the proper functioning of the assembly and light traps he used

Table 2. Examples of theoretical and experimental constraints in Kettlewell's investigations.

One aspect deserves special note. As alluded to above presuppositions about the ecology and life history of the study organism constitute long term experimental constraints. These are formally analogous to Galison's repeated references to cases in which laboratory judgment about the proper functioning of apparatus in physics rests on experience and may at times appear more an art than science.[6] Precisely the same sorts of

[6]"Precisely this lack of routine [in deciding whether a run is acceptable or not] led one historian to label Albert Michelson's interferometer work 'less a science than an art' and to cite an anecdote that lauded Michelson's device as a 'wonderful instrument if operated by Michelson.' Michelson captured a single brushstroke of his art when he recorded that '[i]t frequently occurred that from some slight cause (among others the springing of the tin lantern by heating) the fringes would suddenly change their position, in which case the series of observations was rejected and a new series begun.' The lesson to be drawn is not that experimenters are merely capricious or that experimenters are 'biased.' Rather, we must come to see laboratory judgment as a subtle but essential part of the experimental process from beginning to end. It took Michelson's trained eye and hand to assess when the momentary jitter in the image, barely noticeable even to other optical experts, was grounds for dismissing the run." (p. 254)

issues arise in the context of animal studies, where familiarity with the habits of the organism, its range of behaviors, etc. help the investigator to develop a feel for when an experiment is running properly. Galison also points out that the experimental constraints at issue in the episodes he has examined are most evident in debates between experimental groups associated with different techniques, e.g. his examination of investigations associated with the search for a neutral current highlights how investigators trained in the use of visual detectors, such as cloud chambers, were suspicious of evidence provided by investigators trained in another tradition emphasizing electronic detectors, and vice-versus. An analogous debate occurs between investigators of selection phenomena who work on different animal systems, and for precisely the same reasons--investigators trust evidence garnered from organisms they themselves have studied, if only because they are more familiar with the strengths and limitations of a particular animal model with regard to specific research questions.

The process of conducting an experiment, according to Galison, may be thought of in terms of the successive imposition of theoretical and experimental constraints:

> Each of these broad classes of constraints helps to restrict the laboratory moves and verbal conclusions that appear reasonable to the working experimentalist. Each helps to isolate phenomena and to divide them into classes. It is the progressive imposition or acceptance of these constraints that constitutes the separation of signal from background. (p. 255)

Galison stresses that imposition of experimental and theoretical constraints may at times take the form of attempts to minimize or remove the background, and at other times attempts to isolate the foreground. Yet it is important to recognize that "the two tasks are one and the same," and in particular, how debates about background effects are often the arenas within which the case for a demonstration of an effect is either lost or won. These moves take many forms, from designing an apparatus to exclude background to what considerations enter in during the interpretation of the results of particular experimental runs:

> In physics the… process of 'liberation' of an effect from the background is linked to theory on… many levels… Each of the different levels of theory, by articulating assumptions about what kind of things exist and what things are grouped together, can encourage–or preclude–an investigation. An experimentalist often will design an apparatus precisely to exclude a background and, just as in the choice of where to look, may exclude phenomena later considered vital. During the 'runs' of the apparatus, a further selection takes place, in modern experiments often electronically, before phenomena are ever recorded. Once recorded, data selection again cuts between the foreground and background as 'good' events are split from 'bad.' The 'bad' can be discarded on the basis of quite general principles–as when energy is apparently not conserved, or on the grounds of the details of phenomenological models describing the process or the apparatus. Sometimes an event can be thrown out simply because it does not look right. (p. 256)

Galison's suggestion that experiments should be viewed "in a certain sense… [as] elaborate filters set up in the space of phenomena" seems particularly appropriate for understanding Kettlewell's investigations. His experiments are readily interpreted as a series of strategies aimed at isolating an effect of interest (i.e. the effect of selection) from background effects. The analysis of the Kettlewell study in Section 3 above points out how theoretical and empirical considerations framed the problem of explaining the phenomenon of industrial melanism, how considerations of the ecology of the birds and moths were used to constrain the design of the experiment to minimize the presence of background variation, and how considerations of the behavior of the birds (e.g. observations of their predation on moths) limited possible alternative explanations. It is likewise striking, and in accordance with Galison's views, that much of the controversy surrounding Kettlewell's interpretation of his field investigations centered on questions regarding whether the design of the experiments or their actual execution had succeeded in eliminating background effects, such as the possibility his results were simply an artifact of how he released the moths into the test site (Grant 1999).

The latter problem is an instance of a more general one referred to in the literature as the *problem of experimental artifact,* namely the possibility that the observed effects represent an unintended effect of the experimental procedures rather than the phenomenon of interest. Galison points out how controversies surrounding the possibility of experimental artifact reveal aspects of the experimental process that defy a simplistic analysis of experimental results as independent of theory:

> Procedures, designs, interpretations, and data acceptance all fashion the end of an experiment. Each step effects a partial identification and isolation of the artifactual, and any account of science that glosses over the difficulty of the process misses the real content of laboratory life. Constraints can also occasionally function too well–at least as seen by competing scientists or physicists working after the experiment. At such times other physicists may judge an experimental procedure or a theoretical consideration to have plunged a signal into the background or plucked a mere artifact from the sea of noise. It is therefore absurd to treat experiments as if fixed procedures lead unambiguously to results, independent of prior theory and experiment. Counterexamples fill this book. But it emphatically does not follow that expectations are always met. (p. 257)

Similar considerations apply with regard to Kettlewell's investigations. The above analysis of Kettlewell's work reveals a dynamic interplay between theory and experiment, within which the possibility of experimental artifact led to specific features in the design of the experiment and/or ancillary observations and experiments to rule out alternative explanations.

How Did Kettlewell's Experiment End?

As noted above, Galison argues experiments end when investigators reach consensus that the effect in question is "solid", i.e. it is not an artifact, using procedures and techniques aimed at increasing both the *directness* and *stability* of results. The preceding analysis of Kettlewell's investigations in Section 3 demonstrates how Kettlewell's investigations progressed from tests involving humans, to birds in captivity to ultimately birds in the wild, a progression that can be easily interpreted as successive attempts at increasing the directness by which the phenomenon of interest could be measured. Other more specific examples are provided in Table 3 below.

	Examples of procedures used to increase directness of measurement	**Examples of procedures used to increase stability of results**
Kettlewell	e.g. Kettlewell's use of ancillary observations of the order of bird predation; e.g. Kettlewell's choice of a test site that would minimize migration e.g. Kettlewell's use of two different types of traps to recapture the moths	e.g. Kettlewell's release of marked moths into the test sites of his studies

Table 3. Procedures used in Kettlewell's investigations.

Of course, the question of interest is whether *all* of the procedures used in Kettlewell's investigations neatly fit Galison's schema. Does, for example, Kettlewell's use of survey data constitute a procedure that increases the directness of measurement or the stability of results? They were clearly important in measuring the extent of the phenomenon of industrial melanism.[7] Other ancillary studies, such as the breeding experiments that identified that the *carbonaria* phenotype was the result of a single dominant gene to those which established that the three moth phenotypes had similar life-

[7]There is a sense in which Kettlewell's survey experiments can also be interpreted as providing evidence for stability of results by demonstrating a correlation between *carbonaria* frequencies and pollution. This was a fundamental presupposition of Kettlewell's studies and the basis for his claims that the results of his studies in the Birmingham wood were representative to other forests downwind of industrial centers. But this is not how Galison uses the term stability, which focuses on "procedures that vary some feature of the experimental conditions" (p. 260).

spans, also can be understood as attempts to increase the directness of measurement by virtue of their role in ruling out alternative explanations.

So when did Kettlewell's experiments end? and Did Kettlewell's ultimate decision to end his investigations reflect the operation of processes identified on Galison's account? In some ways, the decision to end a particular experimental run reflects features of the design of Kettlewell's experiments. The mark-release-recapture experiments, for instance, were designed to take place over a specified time period of a few days and no longer. These considerations do not suggest Galison's analysis has overlooked how features of the experimental design lead to a logical terminus of a particular experimental run; rather, they emphasize that Galison's interest is not so much on when particular experimental runs end as on when *investigations* end.

Examined in this light, the questions become more subtle. How did Kettlewell reach the conclusion that his trials during the scoring experiments had demonstrated the applicability of his procedures to birds? How did he reach the conclusion that his aviary experiments were sufficient to establish captive birds do prey upon the moths and in an order of conspicuousness similar to that gauged by the human eye? How did he reach the conclusion his mark-release-recapture experiments had demonstrated the presence of selective differences due to relative crypsis in different environments? And in general, how does an investigator (or team of investigators) make the decision that no further trials are needed, that they have sufficient evidence upon which to stake their claims? The analysis of Kettlewell's investigations provided in Section 3 suggests that much of an investigator's confidence that an effect has been demonstrated relies on whether the results of the experiments conform to prior theoretical expectations. Kettlewell ended the scoring experiment when he and other observers reached a consensus on the fit between the quantitative results of trials and earlier qualitative claims regarding the relative conspicuousness of different forms. During the aviary experiments, Kettlewell ran multiple trials varying different aspects of the design, such as the availability of other types of prey, until he achieved a run he judged "successful," which is to say, in accord with his previous expectations. Likewise, he concluded that the mark-release-recapture experiments had established that differences in relative survival existed between different forms of the moth in different environments largely because the differences he observed were in accordance with what he believed should be the case.

The above perspective on Kettlewell's studies appears to support claims of proponents of the strong programme regarding the problem of experimenter's regress (e.g. Collins 1985, but see Culp 1995), which is the supposition that since an investigator's intuitions that an experimental trial is functioning correctly is based (at least in part) on prior commitments, there is no independent or objective means of assessing whether an experiment has been successful.[8] This view suggests that since the process of experimentation is inherently self-referential, it does not provide an independent means of testing theory. Some stronger versions of this argument, such as Collins', go so far as to suggest that this indicates the socially constructed nature of experimental reality. It should be noted that such a cynical attitude toward Kettlewell's work would be supported only if it could be shown that he only accepted confirming instances and ignored clearly negative evidence.

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[8]Franklin (1994) argues against Collins' specific position, but see Collins (1994) for a reply.

The analysis of Kettlewell's work provided above does not support such claims. Consider Kettlewell's aviary trials, which (as described in detail above) were characterized by a series of false starts and apparently ended only when Kettlewell's finagling of different aspects of the design of the experiment (e.g. prey availability) got the birds to behave as expected. A cynic could easily claim that this part of Kettlewell's investigations illustrates the self-referential character of experiments, in this case, Kettlewell's identification of a run as "successful" just in case it met with his prior expectations. Yet such a position on Kettlewell's experiments (and science in general) ignores three aspects of his work. First, Kettlewell had never conducted such an experiment before--it is only understandable that his first initial runs would encounter some problems, particularly in the absence of craft knowledge regarding how to run such an experiment, information about feeding behaviors of captive birds, etc. Second, Kettlewell did not alter aspects of the design of the experiment only with the aim of obtaining a desired result--for each of the changes he introduced, he provided a rationale for why this or that aspect of his procedures had to be altered. Third, and more generally, Kettlewell's attempts to make the experiment work can just as easily be seen as attempts to demonstrate a phenomena by developing the most persuasive case possible.[9] It is disingenuous to fault Kettlewell for his attempts to improve the design of his experiments after running into problems. This is apparent when one considers that Kettlewell had reasons independent of his expectations of what the results of the trial should be for

[9]The metaphorical characterization of experiments as attempts to build up a case that will "stand up in court" is adopted by both Franklin (1986, 1990) and Galison (1987). See Sargent (1989) for a study of the historical connections between experimental argumentation and common law reasoning.

believing the initial runs might reflect some unanticipated artifact of the procedures (e.g. his understanding that captive birds might behave abnormally).

In summary then, the Kettlewell episode provides a clear example of how investigators make the decision to end their experiments. Obviously a host of factors influenced these decisions, such as the availability of funds and his need to publish. There is also no doubt that these decisions were made in consultation with colleagues, and thus socially mediated. The question to consider is whether the decision to end an investigation in Kettlewell's case fit the general model Galison presents. Clearly it does, although as indicated above, this may in part reflect how vaguely several features of Galison's model are stated (e.g. theoretical culture is never defined).[10]

5. Discussion

As noted above, Franklin and Galison portray their respective accounts as defenses against the strong programme in sociology's perceived attack on the rationality of scientific reasoning.

Franklin's numerous accounts defend the reasonableness of scientific reasoning by drawing attention to numerous reasoning strategies used by past scientists in the episodes he has examined and pointing out that an independent Bayesian justification can be provided for each. While Franklin does not harp on the distinction between theoretical and experimental cultures identified as central to Galison's account, the reasoning strategies Franklin identifies certainly map on to the logical moves scientists make in the

[10] See Rudge (1996) for similar analyses of two other well known selection experiments (Theodosious Dobzhansky's *Genetics of Natural Populations IX and XII*, and Michael Wade's early experiments on group selection) from Galison's perspective, both of which support the conclusions to be drawn below.

successive imposition of theoretical and experimental constraints (e.g. compare Section 4 above with the analysis of Kettlewell's investigations provided in Rudge (1998)).[11]

The chief difference between them seems to be Franklin's perceived over reliance on published accounts, which, understood as rational reconstructions of what happens in the laboratory, can be seen to trivialize Franklin's analysis (e.g. Pinch 1988). Franklin's position appears to be that the experimental reasoning one finds in published scientific reports is sufficient to account for why a reader would find it compelling, and as such, recourse to extra-scientific considerations is unnecessary. And indeed, if one conceives of the writing of a scientific paper as part of the process of experimental reasoning, rather than an artificial reconstruction done after the fact for rhetorical purposes (c.f. Nickles 1988), this seems plausible.

Throughout his book Galison seems to imply that there is a type of reasoning not captured in published accounts that is central to understanding the process of scientific reasoning. Indeed, one might say that whereas Franklin's interest is on whether the reasoning found in a paper is compelling to fellow scientists; Galison's attention instead is focused on how researchers (usually conceived of as a team of several and even dozens of scientists) finally reach the conclusion that the effect is real, i.e. prior to writing it up for publication. I suspect these authors are not that far apart, in that Franklin could respond by pointing out that published accounts are reviewed by peers, peers who read

[11] Galison criticizes Franklin (1986) for his non-committal stance on how to interpret the prior probabilities of his analysis, a standard criticism of Bayesian approaches in general. He also questions the status of Franklin's strategies: "It is not completely clear what the status of Franklin's observations is, since he does not claim that they are sufficient, necessary, exhaustive, or even independent of one another. Nonetheless, the many strategies he adduces contribute to the cause of showing the partial autonomy of experiment from theory" (Galison 1988, 469).

with a host of insights about the process by which data is collected and interpreted that may not be explicitly stated in the paper.

Whereas Franklin's answer to the challenge posed by the strong programme in sociology appears to amount to an outright denial that such extra-scientific considerations can or do play any such overriding role, Galison's answer is more nuanced. Galison lauds the work of Barry Barnes (1977) and Andrew Pickering (1984) in drawing attention to the actual conduct of science and the social dimensions of scientific practice, yet faults them for denigrating the role of nature in the process of science:

> This more radical stance [identified with the work of Barnes and Pickering] claims not only that presuppositions can affect the kind of investigation undertaken at a given time–*that* much would be accepted by even the most conservative positivist philosopher of science. In its strong form, the interest-theory account denigrates the role of nature and supposes that scientists' presuppositions–bolstered by their interests–condition the admissible phenomena in such a way as to render a particular theory and its associated experiments closed and self-referential. In this view, a theoretical outlook, with the experiments that its advocates determine to be relevant, will be entirely divorced from the combination of theory and experiment that succeeds it.
>
> In perfect contrast to the positivists, some interest theorists adopt the view that experimental tests have no power to adjudicate between theories. Andrew Pickering… takes a particularly clear stand on this issue: 'scientific communities tend to reject data that conflict with group commitments and, obversely, to adjust their experimental techniques and methods to 'tune in' on phenomena consistent with those commitments.' Such statements apparently constitute the opposite pole from the positivists: where Carnap grants observational procedures full autonomy, Pickering grants them none. (p. 10)

Galison disagrees with Pickering's approach and others like it for three reasons. First, it fallaciously assumes that because experiments do not provide logically compelling conclusions, an experimentalist's beliefs must be ascribed entirely to 'interests' (p. 11). Second, it exaggerates the flexibility of theory, ignoring the presence of mathematical and physical constraints (p. 11). And third, it does not appreciate the constraints imposed by the skills and techniques of experimentalists on their work, constraints that are independent of whatever 'interests' or theories the experimentalist entertains (pp. 12-13). As Hacking (1983) has emphasized, experiments and the experimental life have developed in many ways independently of theory. Thus, in contrast to Pickering among others associated with the strong programme in sociology, Galison does not believe that recognizing the social dimensions of science or the underdetermination of theories by evidence necessarily leads to a radical skepticism about the rationality of science often associated with Kuhn. Nor does he believe that the development of separate theoretical and experimental cultures in physics during the twentieth century precludes their ability to work and communicate with one another, even during significant changes in theory.[12]

Galison argues for his position by pointing out that his specific historical examples do not support the claims of interest theorists. For instance, in his examination of the search for neutral currents, he points out how David Cline, who had much of his research and reputation riding on the claim that there were no neutral currents, nevertheless in an internal memorandum admitted that he saw no means of making the alleged effect go away. "'Interest' had to bow to the linked assemblage of ideas and empirical results that rendered the old beliefs untenable, even if they were 'logically possible'" (p. 258). In general, Galison accuses interest theory accounts of doing poor history. For instance, Pickering (1984) minimizes the significance of the Aachen single-electron event in the decision by particle physicists to accept the neutral current, "because a single event cannot prove the existence of new phenomena." Yet, as Galison points out,

[12]Examples of productive collaborations between theoreticians and experimentalists in the context of evolutionary biology are also forthcoming - e.g. Wright and Dobzhansky, Fisher and Ford, etc.

there are many processes in particle physics which have been accepted by experimentalists after the discovery of only one or two events. Different teams of investigators may find certain types of evidence more persuasive, but this often reflects differences in training, familiarity with apparatus etc. rather than the operation of "interests" having nothing to do with the phenomena. Galison also criticizes interest theorists for exaggerating the flexibility of theory in the face of "interests":

> Microscopic phenomena, like the gyromagnetic effect, are not simply observed; they are mediated by layers of experience, theory and causal stories that link background effects to their tests. But the mediated quality of effects and entities does not necessarily make them pliable; experimental conclusions have a stubbornness not easily canceled by theory change. And it is this solidity in the face of altering conditions that impresses the experimenters themselves–even when theorists dissent. (p. 259)

In short, Galison argues that although scientific claims do not arise simply through the inspection of evidence, the fact that the process is more complicated and socially mediated does not necessarily mean that it is inherently irrational or dominated by bias.

With regard to the case study of the present paper, it is striking to note that a recent popularization of Kettlewell's work written by Judith Hooper (2002) attempts to account for Kettlewell's results in terms of extrascientific factors, such as Kettlewell's need to establish his *bone fides* as a researcher despite his training as a medical practitioner and the Ford group's need for a spectacular example of natural selection to further their pan-selectionist agenda. As with Galison's critique of Pickering (1984) mentioned above, Hooper's argument can be shown to rest entirely upon shoddy historical reasoning and numerous misunderstandings about field work and issues associated with the nature of science (Rudge 2005).

6. Summary and Conclusions

The preceding essay establishes that Kettlewell's classic investigations on the phenomenon of industrial melanism can be made sense of on Galison's account. It is historically accurate to portray Kettlewell's work as starting from a "subtle hint" (gut intuition) that birds might have the same difficulty humans do in finding moths when they rest on matching backgrounds. Kettlewell's attempts to first quantify how conspicuous moths are against different backgrounds, establish that captive birds attend to this difference, and finally establish that birds in the wild have the same difficulty can be interpreted as the successive imposition of theoretical and experimental constraints. Some of these moves can be seen as attempts to minimize or remove the background (e.g. Kettlewell's procedures to prevent or control for alternative explanations of his recapture figures). Others can be seen as attempts to isolate the foreground (e.g. the creation of a film record of the order of bird predation). And, as noted above, it does make sense to discuss Kettlewell's decision to his investigation when on balance the evidence available suggested the effect was "solid", which by and large consisted in the conformity of his results with previous expectations. This consideration alone does not support the extravagant claims of individuals associated with the strong programme in sociology that the process of experimentation is so inherently self-referential as to involve an infinite regress. As emphasized by Galison (and illustrated in the above analysis of Kettlewell's investigation), theoretical and experimental constraints that guide the process of experimentation are often highly independent of one another.

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References

Achinstein, P. and Hannaway, O. eds. 1985. *Observation, Experiment and Hypothesis in Modern Physical Science*. Cambridge, MA: MIT Press.

Barnes, B. 1977. *Interests and the Growth of Knowledge*. London: Routledge and Kegan Paul.

Batens, D. and van Bendegem, J. P. eds. 1988. *Theory and Experiment: Recent Insights and New Perspectives on Their Relation*. Dordrecht: D. Reidel Publishing Company.

Bowater, W. 1914. “Heredity of Melanism in the Lepidoptera.” *Journal of Genetics* 3:299-315.

Brandon, R.N. 1994. "Theory and Experiment in Evolutionary Biology." *Synthese* 99:59-73.

Braudel, F. 1972. *The Mediterranean and the Mediterranean World in the Age of Philip II*. New York: Harper & Row.

Collins, H.M. 1985. *Changing Order*. London: Sage Publications.

-----. 1994. "A Strong Confirmation of the Experimenter's Regress." *Studies in the History and Philosophy of Science* 25:493-503.

Cooke, N. 1877. “On Melanism in Lepidoptera.” *The Entomologist's Monthly Magazine* 10:92-6, 151-3.

Culp, S. 1995. "Objectivity in Experimental Inquiry: Breaking Data Technique Circles." *Philosophy of Science* 62:438-458.

Doncaster, L. 1906. “Collective Inquiry as to Progressive Melanism in Lepidoptera: Summary of Evidence.” *Entomologist's Record and Journal of Variation* 18:165-170; 206-208; 222-226; 248-264.

Edelston, R.S. 1864. “*Amphydasis betularia*.” *The Entomologist* 2:150.

Fisher, R.A. and Ford, E.B. 1947. "The Spread of a Gene in Natural Conditions in a Colony of the Moth *Panaxia Dominula* L." *Heredity* 1:143-74.

Ford, E.B. 1937. “Problems of Heredity in the Lepidoptera.” *Biological Reviews* 12:461-503.

----. 1953. “The Experimental Study of Evolution.” *Australian and New Zealand Association for the Advancement of Science* 28:143-54.

----.1955. *Moths*. London: Collins Press.

----. [1964] 1975. *Ecological Genetics* (4th ed.).New York: Chapman and Hall.

Franklin, A. 1986. *The Neglect of Experiment*. Cambridge MA: Cambridge University Press.

-----. 1989. "The Epistemology of Experiment." Pp. 437-60 in *The Uses of Experiment*. Edited by D. Gooding, T. Pinch, and S. Schaffer. New York: Cambridge University Press.

-----. 1990. *Experiment, Right or Wrong*. Cambridge, MA: Cambridge University Press.

-----. 1994. "How to Avoid the Experimenter's Regress." *Studies in the History and Philosophy of Science 25*:463-491.

-----. 2007. "The Role of Experiments in the Natural Sciences: Examples from Physics and Biology." Pp. 219-274 In *Handbook of the Philosophy of Science: General Philosophy of Science– Focal Issues*. Edited by Kuipers, T. Amsterdam, The Netherlands: Elsevier Press.

Galison, P. 1987. *How Experiments End*. Chicago IL: University of Chicago Press.

-----. 1988 "Review of Franklin's The Neglect of Experiment." *ISIS* 79:467-70.

Gooding, D. 1990. *Experiment and the Making of Meaning*. Dordrecht: Kluwer Academic Publishers.

Grant, B.S. 1999. "Fine Tuning the Peppered Moth Paradigm." *Evolution* 53:980-4.

Griesemer, J. R. and Wade, M. J. 1988. "Laboratory Models, Causal Explanation and Group Selection." *Biology and Philosophy* 3:67-96.

Hacking, I. 1983. *Representing and Intervening*. New York: Cambridge University Press.

Haldane, J.B.S. 1924. "A Mathematical Theory of Natural and Artificial Selection." *Transactions of the Cambridge Philosophical Society* 23:19-41.

Harrison, J.W.H. 1927-28. "A Further Induction of Melanism in the Lepidopterist Insect, *Selenia bilunaria* Esp. and its Inheritance." *Proceedings Royal Society B* 102:338-347.

----. and F.C. Garrett. 1925-26. "The Induction of Melanism in the Lepidoptera and its Subsequent Inheritance." *Proceedings Royal Society B* 99:241-263.

Hooper, J. 2002. *Of Moths and Men: An Evolutionary Tale*. New York: W.W. Norton & Company.

Hughes, A.W. 1932. "Induced Melanism in the Lepidoptera." *Proceedings Royal Society B* 110:378-402.

Kettlewell, H.B.D. 1955 "Selection Experiments on Industrial Melanism in the Lepidoptera." *Heredity* 9:323-42.

-----. 1956. "Further Selection Experiments on Industrial Melanism in the Lepidoptera." *Heredity* 10:287-301.

-----.1959. "Darwin's Missing Evidence." *Scientific American* 200:48-53.

----.1973. *The Evolution of Melanism: The Study of a Recurring Necessity*. Oxford: Clarendon Press.

Kohler, S. 1994. *Lords of the Fly:* Drosophila *Genetics and the Experimental Life*. Chicago: University of Chicago Press.

Le Grand, H.E. ed. 1990. *Experimental Inquires: Historical, Philosophical and Social Studies of Experimentation in Science*. Dordrecht: Kluwer Academic Publishers.

MacKenzie, D. 1989. "From Kwajalein to Armageddon? Testing and the Social Construction of Missile Accuracy." Pp. 409-35 in *The Uses of Experiment*. Edited by D. Gooding, T. Pinch, and S. Schaffer. New York: Cambridge University Press.

Mayo, D. 1996. *Error Statistics and the Growth of Knowledge*. Chicago: University of Chicago Press.

Nickles, T. 1988. "Reconstructing Science: Discovery and Experiment." Pp. 33-53 in *Theory and Experiment: Recent Insights and New Perspectives on Their Relation*. Edited by D. Batens, and J. P. van Bendegem. Dordrecht: D. Reidel Publishing Company.

Pickering, A. 1984. *Constructing Quarks: A Sociological History of Particle Physics*. Chicago: University of Chicago Press.

Pinch, T. 1988. "Review of Allan Franklin's Neglect of Experiment." *British Journal for the History of Science* 21:122-3.

Rudge, D. 1996. *A Philosophical Analysis of the Role of Selection Experiments in Evolutionary Biology*. Ph.D. Dissertation. University of Pittsburgh.

-----.1998. “A Bayesian Analysis of Strategies in Evolutionary Biology.” *Perspectives on Science* 6:341-360.

-----. 2001. “Kettlewell From an Error Statistician's Point of View.” *Perspectives on Science* 9:59-77.

-----. 2003. “The Role of Photographs and Films in Kettlewell's Popularizations of the Phenomenon of Industrial Melanism.” *Science & Education* 12:261-287.

-----. 2005. “Did Kettlewell Commit Fraud? Re-examining the Evidence.” *Public Understanding of Science* 14:249-268.

-----. 2006. “H.B.D. Kettlewell’s Research 1937-1953: The Influence of E.B. Ford, E.A. Cockayne and P.M. Sheppard.” *History and Philosophy of the Life Sciences* 28:359-388.

Sargent, R. 1989. "Scientific Experiment and Legal Enterprise: The Way of Experience in Seventeenth-Century England." *Studies in the History and Philosophy of Science* 20:19-45.

Thomsen, M., and H. Lemeche. 1933. “Experimente zur Erzielung eines Erblichen Melanismus bei dem Spanner *Selenia bilunaria* Esp. *Biologisches Zentralblatt.*” 53:541-560.

Tutt J.W. 1890. "Melanism and Melanochroism in British Lepidoptera." *The Entomologist's Record and Journal of Variation* 1:5-7, 49-56, 84-90, 121-5, 169-72, 228-34, 293-300, 317-325.

White, F.B. 1876-77. “On Melanochroism and Leuochroism.” *The Entomologist's Monthly Magazine* 13:145-149.