

Credibility, Evidence, and Discovery: The Case of the Ivory-billed Woodpecker

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Introduction

This paper discusses an effort to document the rediscovery of a North American bird that was widely believed to be extinct. In April 2005 a team of researchers announced publicly that they had identified an ivory-billed woodpecker¹ (*Campephilus principalis*) in the Cache River swamp in Arkansas. This announcement was a major news story, not only for ornithologists and amateur “birders”, but also for the public at large. The present paper uses publicly available documents to examine how the ornithologists sought to demonstrate their discovery of the bird. Although several professional field ornithologists described and sketched the bird, the effort to document the discovery focused intensively on a frame-by-frame analysis of a brief segment of videotape in which the (alleged) ivory-billed woodpecker was depicted in flight.

Referring to documentary materials available online, the paper focuses on the elaborate efforts the researchers made to demonstrate their rediscovery with the video record. These included simulations and reenactments of the video scene and the conditions under which it was recorded. The same documents provided other ornithologists with material for skeptical reanalysis. The skeptical accounts raised a demand for more definitive, mechanically recorded evidence. This demand for definitive evidence provided a context for the rapid dismissal of further (more detailed) observational reports at a different site a year later. Consequently, the public standards for evaluating evidentiary claims were conditionally relevant: sequentially bound to the fate of prior “rediscoveries”².

¹ Most ornithologists appear to use the lower case for the name of the bird, so I have followed that convention.

² A version of this paper was first presented at the “Scientific Practice as Ordinary Action” workshop, at the University of Fribourg, Switzerland (22-23 March 2007), and then reworked for the international conference, “Cognitive Passions: Investigations into the pragmatic and political dimensions of the love of knowing,” Saint-Etienne, France (7-8 January 2010). I’m grateful to Florian Charvolin and others at the conference for questions and comments. I also benefitted from conversations with Trevor Pinch, Park Doing, and Joeri Bruyninckx. Finally, I am grateful to Philippe Sormani and the reviewers of this paper for helpful comments and challenging criticisms.

The Topic of Discovery

Discovery is one of the most persistent and alluring topics in philosophy, history, and social studies of science, and yet there is surprisingly little work that directly addresses discovering *work*. Hans Reichenbach's (1938) distinction between the context of discovery and context of justification consigned the former to a contingent, non-rational status, while reserving logical analysis for the rational reconstruction of (possible) discoveries. The title of Sir Karl Popper's (1959) *Logic of Scientific Discovery* signaled an effort to tackle what Reichenbach consigned to psychology, but Popper also said little about discovery while devoting far more attention to logical analysis of the procedures through which theoretical ideas acquire consensual scientific status. N.R. Hanson (1967) offered useful grammatical distinctions between different types of discovery (stumble-upon discoveries, discoveries that confirm conjectures, discoveries that undermine prior assumptions, etc.), but Hanson's (1961) legacy of "theory-laden observation" encouraged a reversion to a priori theories or concepts as the epistemic basis for discovery.

Part of the difficulty with characterizing discoveries, either as logical sequences or concrete courses of action, is that the fate of a specific course of observational or experimental work often is not apparent at the time. Moreover, as many researchers can testify, there is a crucial distinction between *thinking* that you have made a discovery (or even announcing that you have made one) and actually making and getting credit for one. Even when no hidden source of error or illusion later emerges, a

"discovery" can turn out to have been an inadvertent "replication" of an earlier achievement made by others. Moreover, even in cases of celebrated discoveries, *what counts* as the discovery may differ from what the researchers in question initially set out to achieve (Barber and Fox, 1958), and retrospective judgments of the significance and validity of discoveries change historically.

Augustine Brannigan's (1981) attributional theory of discoveries encourages us to treat the collective response as the crucial determinant of the status and significance of particular discoveries, but rather than treating such attributions as "merely subjective" historical accidents, he outlines a set of general criteria to describe the conditions of intelligibility for any discovery.

With some modifications (discoveries are deemed to identify "products of nature" and not manufactured inventions), these criteria resemble those used by patent examiners in the Anglo-American legal tradition: novelty, significance, unprecedentedness, and non-obviousness. Unlike in patent law, in cases of discovery the relevant judgments are made by what is often called a "scientific community" rather than by designated officials in a formal submission process, although peer-review of journal submissions and grant applications has some resemblance to patent examination.

Brannigan's (1981) perspective can be misread to imply that it would be hopeless to attempt to observe, record, or analyze "discovering work" (the practices and interactions that constitute discoveries). Such a misreading might

seem plausible, because, absent the wisdom of hindsight, there would be no discernible difference between circumscribed sequences of “replication work”, “mistaken-discovery work” and “actual discovery work” (also see Collins, 1983). Moreover, many recognized discoveries do not trace back, even retrospectively, to discrete “moments”. However, it is worth distinguishing between “discovery” as an accredited historical achievement and “discovering work” as a project of action.

A study such as Garfinkel et al.’s (1981) analysis of a tape recording of a segment of “the night’s work” of two astronomers and an observatory night assistant who set out to observe and document a possible astronomical object (an optical pulsar) can reveal some local features of *discovering* work: courses of action and interaction in which the researchers *attempt* (whether from the outset, or after having ‘stumbled upon’ an intriguing possibility) to successfully observe and document a phenomenon that *might* come to stand as a discovery³.

The *possibility* that the course of work may or may not turn out to have been a discovery is notably part of the actions and interactions themselves. Although, the local actions alone do not secure

their own status as historicized “moments of discovery,” their contingent status, as such, is itself thematic *within* discovering work (cf., Koschman and Zemel, 2009). The contingent historical status of the present moment *as a possible discovery* is explicitly featured in the recorded dialogues among the parties in the tape that Garfinkel et al. (1981:154) analyze.

Discovery is thus a prospective-retrospective product of specific courses of action and later accounts of those actions. Specific sequences of action at particular times and places *and* later efforts to document, validate or discredit, and build upon the outcomes of those actions are all part of discovering work.

Discovering work, as Garfinkel et al. (1981) elaborate, includes the real-time practices (the “lived-work”) of handling equipment, recording and analyzing data, and piecing together different forms of evidence in laboratory and field situations. However, given the fact that a course of action that leads to the announcement of a discovery is not by itself sufficient to establish *that* it will turn out to be a discovery, *and* the fact that would-be discoverers are attuned to such contingency, the public documentation and reception of discovery claims is crucial.

With this in mind, the present paper addresses discovering work through an analysis of the documentation and the reception of that documentation, which itself was documented, rather than through an analysis of the lived-work (or a tape recording of such work). In this

³ Although researchers do often set out to make discoveries, an intention to make a discovery is neither a necessary nor a sufficient condition for becoming part of a retrospective-prospective sequence of discovery work. There are circumstances in which a course of action becomes recognized as being significant for a discovery only well after its completion, and other circumstances in which researchers find that a discovery that they intended to make, and even were sure they had made, later comes to nothing.

case, the lived-work is inaccessible⁴, except in the form of publicly documented reports and visual evidence. What *is* accessible, and makes up the topic of this paper, is an unfolding temporal relationship between observational reports, public documentation, skeptical re-analysis of the documentation, and authoritative assessments of the status of the discovery.

The discovering work described in this paper was part of an effort to “rediscover” an object that had held doubtful, and even non-existent, status for a long time. If accepted as a discovery, it would not be a discovery *of* something new, but a discovery *that* an object with doubtful status actually existed⁵. The work in this case consisted

⁴ The lived-work of composing the evidentiary documents was itself inaccessible (unlike in the documentary practices for a neuroscience project described by Lynch, 1985a), and print publications and online images and videotape segments are the materials for this study. Although there is no question that the material basis for this study differs from that of the tape recording used by Garfinkel et al. (1981), or the tapes and field notes used by Lynch (1985a), post-hoc reconstruction of the lived-work was a necessary part of those studies as well.

⁵ The difference between a discovery and rediscovery is not as clear-cut as it might seem. No single discovery, or type of discovery, provides a model for all others (Hanson, 1967), and the “rediscovery” in this case shares some features with some types of discovery. The optical pulsar case (Garfinkel et al., 1981) also could be said to be a rediscovery. Pulsars – rapidly pulsating astrophysical objects, with frequencies of many beats per second – had already been identified and documented with radio telescopes, but theoretical reasons had been given for doubting whether they would emit energy in the optical range. The resolution of radio telescopes is much lower than optical telescopes, and so coordinating a radio pulsar with an optical star was a difficult and

of a series of sightings of an ivory-billed woodpecker. These sightings were presented and disputed in print publications, online documentation, and public presentations. What was at stake was whether a large, spectacular North American bird, which had been widely assumed to be extinct, had persisted during the decades since the last definitive sighting.

Chronologies produced in reports of the sightings credit the initial sighting to Gene Sparling, an outdoorsman and guide on 11 February 2004. Other sightings had been reported elsewhere in recent years, and so Sparling’s was the “initial” sighting in a retrospectively composed sequence of local sightings at the study site documented in the reports. A “second” sighting in the sequence was recorded to have occurred two weeks later. This one was credited to an ornithologist and a member of a college communications department guided by Sparling. In the months that followed, at least seven other sightings were reported by members of a team of researchers affiliated with the Cornell Lab of Ornithology (CLO) and several other research institutions during a year-long expedition in the Cache River swamp in Arkansas (for convenience, I shall call this the CLO team).

The team publicly announced the rediscovery in April 2005, more than a year after the initial sightings. The researchers documented these sightings

potentially fruitless task. The “discovery” in question coordinated a known radio pulsar with a documented frequency with the position of a particular star visible (with a telescope) in the optical range. That star also happened to be the remnant of a supernova (the Crab nebula), and so identifying that star as the pulsar supported one of the extant theories of how pulsars formed.

with notes, sketches, a blurry videotape, and numerous audio-recordings of “knocks” and call sounds that they believed to be characteristic of the species of woodpecker (the high-pitched bird calls are transliterated with the word “kent”).

The announcement was delayed for at least two reasons. One was to get the best documentation possible, given the likelihood of a skeptical reaction, and the other was to secure protection of the sensitive habitat before drawing public attention to the research site and unleashing a stampede of enthusiastic birders.

As anticipated, the announcement was major news, and not just for ornithologists and birders. The researchers published an article in *Science* magazine (Fitzpatrick et al., 2005b) announcing their discovery and documenting it with photographic and other evidence, and the story was immediately picked up by major newspapers. This was an extraordinary announcement, because sightings of the ivory-bill had not been “conclusively documented” since 1944 (Fitzpatrick et al., 2005b:1460).

Credible reports of “fleeting observations” of at least two individuals in what is often described as a subspecies were made in Eastern Cuba in the late 1980s, but had not been confirmed since then (ibid). The ivory-billed woodpecker is far from an obscure bird. Roughly the size of a common crow, with striking markings, it is (or was) the largest North American woodpecker (the Imperial Woodpecker, a larger species of the same genus in Mexico also is considered very likely to

be extinct). Consequently, it seemed implausible that such a bird could elude ornithologists for decades, not to speak of millions of amateur birders.

However, there had been numerous reports by amateurs over the years. These sightings, in some cases accompanied by audio-recordings and blurry photographs, and in one case a feather, were chronicled and reviewed in the publications by the CLO team, and at least some of them took on greater credibility in retrospect. A few sightings in recent years were deemed credible enough to motivate the Cache River expedition as well as an earlier, unsuccessful expedition in Louisiana. It also was imaginable that a small population of this secretive and largely solitary bird could persist unnoticed in the vast swamplands in the Southeastern USA.

Consequently, while any sighting of this bird was likely to be met with skepticism, this was nowhere near the degree of skepticism that would greet sightings, photographs, and videotapes of, say, plesiosaurs in a Scottish loch. Some popular field guides continued to include the ivory-bill, though often with a notation to the effect that the bird was very likely extinct. One exception, though not the only one, was the relatively new, and very popular, Sibley (2000) guide, from which the ivory-billed woodpecker was conspicuously absent.

As we shall see, there was a reciprocal relationship between public conceptions of adequate evidence, and shifting degrees of skepticism and standards of proof. What counted as definitive evidence was relative to informal

conceptions of probability and possibility: *Could* such a thing exist, and how likely was it that *those* particular observers correctly identified it? The case also suggests that more or less stringent demands for public proof also are conditionally relevant⁶ – retrospectively and prospectively bound to the historical sequences in which they occur.

Methodological and Conceptual Issues

Before proceeding further, it is necessary to address how, if at all, the documentary materials examined for this paper bear on the topic of “discovery”.

The status of the reported sightings in 2005 remains in doubt, and the doubts have grown with the passage of years and the lack of definitive confirmation. Moreover, even if we accept the Cornell team’s observations as evidence of a discovery, others such as Sparling (and even some other amateurs who reported sightings in recent years) could also claim priority, or at least a part of the credit.

The issue was not so much *that* sightings were reported, but *how* they were made and documented. During the expedition,

a videotape shot a couple of months later from another canoe captured a few seconds of flight of a bird expedition members believed to be an ivory-billed woodpecker. The question then was whether these sightings were any more or less conclusive than the earlier ones.

The “discovery” thus was qualified in a number of ways.

First, if credited, it would not be a discovery of an unknown object, but instead a discovery of a known object that was believed to be extinct. The research team announced it as a “rediscovery”, implying that the observation was preceded, but this did not reduce it to a mere replication or confirmation of a prior discovery, since (if successful) it would have retrieved the bird from oblivion⁷. Second, given the previous reports of the bird, the issue was whether *conclusive* evidence could be secured. Third, its status as a discovery would depend upon what “conclusive evidence” meant in detail, in this case.

A “sighting” is not a discovery, though the term implies a degree of success in “catching sight of” a particular object as

⁶ Conditional relevance is an established organizational principle in conversation analysis, where it identifies a sequential relationship between an initial utterance and a “next” utterance. The “next” utterance gains its identity and sense (e.g., as an answer-to-a-question) by virtue of its placement after the “first” (Schegloff, 1968). Conversation analysts did not invent the term, as it has long been an established concept in law and logic, among other fields. It also has affinity with ethnomethodology’s empirical specification of the phenomenological theme of retrospective-prospective sense of occurrence (Garfinkel, 1967:41).

⁷ One of the “grammatical” criteria for a discovery that Brannigan (1981: 60) mentions is unprecedentedness – by this he means that a discovery is *by definition* unprecedented. In this case, however, calling the sighting of the bird a “rediscovery” does not diminish its importance in a way that would be analogous to “reinventing the wheel”. Given the presumption that it was extinct, the “rediscovery” was not simply a confirmation of an “original” discovery. Indeed, no mention of the original discovery (or discoverer) of the bird is mentioned in any of the reports. The informal name that was sometimes given to the bird – “Elvis” – suggests that discovering that it still persists would be akin to discovering that Elvis Presley is alive.

opposed to, say, “looking for, but failing to see, it” (Coulter and Parsons, 1990). Whether or not a reported sighting counts as an adequate observation depends upon the circumstances. A reported sighting that feeds into a survey of common species is reviewed with far less stringency than an unverified report of a rare or vagrant species, let alone a report of a species that had never been seen or recorded or, as in this case, of a bird that was widely assumed to be extinct. The CLO researchers were aiming for something more definitive than a “sighting”: they wanted more than a fleeting glimpse with naked eyes or binoculars. What would count as “conclusive documentation” was not specified in advance, but it was clear both from descriptions of the expedition and its reports, and from the reception of those reports, that seeing-and-reporting would not be enough.

Regardless of the increasingly tentative status of the CLO “discovery”, the case can be treated as an apt example of discovering work.

As noted earlier, discovering work can be described even when the ultimate status of the discovery in question remains in doubt. Such a description cannot say whether the “product” of such work should be granted ultimate status as a discovery, but it can delve into how the work, its documentation, and reception relate to that possibility.

The publicity and controversy surrounding the particular case offered advantages as well as disadvantages for the analysis of discovering work. The advantages are that documentary materials and rival interpretations of

them are readily accessible in publications and websites.

The disadvantages are that, for understandable reasons, access to the site of the (alleged) discovery was restricted, and public announcements were not made until more than a year after the initial sightings were made. The principal parties to the research also gave many public accounts of the events on their own, and in addition to being less than receptive to being studied by outsiders, they were unlikely to reveal “inside” information that they had not already chronicled in their own accounts.

Though, on site investigation, and even in-depth interviews with some of the main parties to the research, would undoubtedly have been valuable for examining and reconstructing practices of documentary work, this paper will make use of published writings and interviews, and supplementary online materials that are already on record.

Readers are urged to consult the relevant sites, to view and review the visual and other materials used to document the discovery⁸. These documentary materials will be insufficient for recovering the “lived-work” through which they were assembled, but they should allow for an appreciation of the documentary practices involved (see also Bjelic and Lynch, 1992; Lynch and Law, 1999).

⁸ Because of copyright issues, and the impossibility of embedding video in a paper, the online sites of the documents are referenced here. It is likely that URLs for some of the sites will have changed before publication of the paper (they changed during the drafting of it as well), but with further searching, all of them should be accessible.

Given the limitations of the material, the key analytical issues addressed in this paper have to do with the formal presentation of and responses to evidence in written reports. One might object that this analysis is about the context of justification rather than discovery, but such an objection would be miscast.

The formal announcement and documentation of the discovery was an extension of the discovering work. It did not simply provide justification for a discovery-already-made, because the standing and success of the prior work as an instance of “discovery” hinged on the contingent production and reception of the documentary materials. The publications, supporting documents, and public presentations strongly emphasized visual materials.

The strenuous efforts to make the best of admittedly bad visual evidence provide an instance of the well-known ethnomethodological research interest in *trouble* – either deliberately induced or found in situ – as a methodological opportunity for explicating orders of practical and interactional activity (Garfinkel, 1967).

In this case, the troubles can help attune us to the work that goes into visualizing and classifying a natural phenomenon. As we shall see, the production and analysis of visual documents in this case involved an elaborate set of practices designed to enhance the intelligibility of the visual evidence and the classification based on them. However, such enhancement was far more than a matter of working with images in order to enhance their visibility and analyzability (Lynch, 1985b; Lynch and Edgerton,

1988; Goodwin, 1994), as it involved an elaborate effort to simulate conditions of observation that allowed for systematic comparisons and categorical discriminations. In brief, it was a matter of building a quasi-experimental space around visual evidence in an effort to maximize the intelligibility and analyzability of that evidence.

Initial Reports

In April 2005, the team published a report in *Scienceexpress*, an online publication from *Science* magazine. The article had seventeen authors, with John Fitzpatrick, director of the CLO, listed as lead author (Fitzpatrick et al., 2005a). The article included links to further documentation (Supporting Online Materials).

According to Fitzpatrick (2005), this online publication was hastened into print as a result of a leak to the press that occurred shortly after the acceptance of the draft-article by *Science*. Simultaneous with the online publication, the CLO team also wrote press releases and gave interviews to news sources. The publication in *Science* followed in June (Fitzpatrick et al., 2005b).

The article and press releases described and exhibited the evidence of seven sightings by different members of the expedition between February 2004 and February 2005. The evidence included personal testimonies, field notes and sketches, and a few seconds of intensively analyzed videotape. The personal testimonies were prominent in the press coverage, such as in the following quotation from a *New York Times* article:

“Within two weeks [after Gene Sparling’s reported sighting] Mr. Gallagher [of CLO] and Bobby R. Harrison of Oakwood College in Huntsville, Ala., were in a canoe in the refuge, with Mr. Sparling guiding them. Mr. Gallagher said he had expected to camp out for a week, but after one night out, on Feb. 27, he and Mr. Harrison were paddling up a bayou bounded on both sides by cypress and tupelo when they saw a very large woodpecker fly in front of their canoe. When they wrote down their notes independently and compared them, Mr. Gallagher said, Mr. Harrison was struck by the reality of the discovery and began sobbing, repeating, ‘I saw an ivory bill’. Mr. Gallagher felt the same. ‘I couldn’t speak’, he said” (Gorman, 2005).

Whether or not the events happened exactly as reported, the story is analytically interesting. Harrison and Gallagher – two academics, one of whom is a professional ornithologist – are portrayed as notable characters in the story, whose passionate reactions are portrayed, not as expressions of bias or interest that detract from the credibility of the discovery, but as expressions of conviction: “Mr. Harrison was struck by the reality of the discovery ...”; Mr. Gallagher is quoted as saying “I saw an ivory bill”.

There is none of the mitigation of a tentative observation (“I think I saw an ivory bill”) that defers commitment to the reality of the object (cf., Pinch, 1985). “I couldn’t speak” is a vivid way to express awe and self-effacement; it is an expression of a modest witness (Shapin and Schaffer, 1985), but in a very particular sense. It marks

transcendence, the Greek *ekstasis* that the existentialists use to define a particular awareness of *standing outside oneself*.

This expression is far different from the “view from nowhere”, originally coined by philosopher Thomas Nagel (1986), which critical studies of science have turned into a slogan signaling the modern, rational, objectifying “gaze”. It also is incompatible with the logical empiricist conception of context of discovery, because Gallagher and Harrison are not marking an initial moment of apprehension. The outpouring of passion is simultaneous with a personal realization and conviction that an object they had just seen could have been nothing other than what they saw it *as*⁹.

The videotape, taken in April 2004 from a camera set up by M.D. Luneau (of the University of Arkansas), which was running while laying unattended in the canoe as he and his brother-in-law Robert Henderson paddled within the study area, was treated as the key item of evidence (The short segment of video can be viewed at: http://clomedia.ornith.cornell.edu/IBW/IBW_RealTime_DEI.mov).

It shows several seconds of an out-of-focus bird flying away from the camera through a forested area. Another very brief segment of the Luneau video, recorded just before the bird revealed itself in flight, also was published. This

⁹ The same newspaper account quoted above has the delayed outpouring of passion occur just as the two witnesses are inscribing their notes “independently” – as disciplined observers. I thank Wendy Sherman for pointing out this odd juxtaposition of disciplined and passionate witnessing.

segment showed a tupelo tree trunk on which the bird apparently was perched just before launching its flight, and the researchers analyzed the frames to show a very blurry image of a bird that was mostly obscured by the tree trunk.

The analysis in the *Science* article devoted extraordinary attention to the two segments of Luneau videotape. First, a frame-by-frame series of still pictures was reconstructed from the segment of the flying bird. Second, the image of the partially hidden perched bird was subjected to a series of analyses involving sketches that extrapolated from the visible parts and compared the video image to models placed on the tree and photographed from a similar angle and distance.

Analyses of both segments compared the bird shown in the video with drawings and scale models of the ivory-billed woodpecker and of the pileated woodpecker (*Dryocopus pileatus*), a widely distributed North American species with superficially similar markings and slightly smaller size. The comparisons were designed to rebut an argument that had been used to call into question many of the “anecdotal” reports made in recent decades:

“Such reports are suspect because of the existence and relative abundance throughout this region of the superficially similar pileated woodpecker” (Fitzpatrick et al., 2005b: 1460).

Although the two woodpeckers should not be difficult to distinguish for experienced birders (let alone world-class professional field ornithologists), because of differences in size, behavior,

flight pattern, and especially the distribution of white and black field marks on the birds’ wings (ibid., figure 1), the researchers “considered and rejected the [more challenging] hypothesis that the sightings and video can be explained by a ‘piebald’ or partially leucistic pileated woodpecker with symmetric white patches on wings and back approximately matching the pattern of an ivory-billed woodpecker” (ibid., p. 2).

The published reports and press releases were far from the end of the story, and the story has not ended six years later. The ivory-billed woodpecker remains in suspended animation between extinction and existence. With the passage of time, the conviction of reality that struck Gallagher and Harrison with such emotional force has faded almost entirely away; though not exactly an embarrassment for the Cornell Lab of Ornithology, the bird is no longer a subject of conviction and celebration.

Analysis

The context of discovery/context of justification distinction does not provide an adequate way to analyze this particular case, but something akin to it may be relevant. As mentioned earlier, researchers are well aware that to *think* you have made a discovery (even with great conviction) does not mean that you *have* made a discovery.

A discovery is a social phenomenon, and a product of disciplinary histories. Researchers are far from passive in relation to such histories. Even for a discovery that can be traced to a key revelatory moment, extensive and painstaking efforts are made to

document *that* it is a discovery. This differs from justifying a discovery, as the very identity of the discovery, as such, turns on the documentation and its reception.

The article in *Science* and the many other presentations and accounts given by the CLO team members made strenuous efforts to head-off or rebut skeptical dismissal of the sightings, even while the popular media was celebrating the (re)discovery of the bird they dubbed “Elvis” (also, supposedly, a code name used by the researchers during the year-long period when they maintained secrecy), or “The Lord God Bird”. The article did so:

- (1) by retrospectively analyzing prior “anecdotal” reports of sightings;
- (2) describing a series of “authentic” sightings made just before and during the expedition, some of which included sketches; and, above all,
- (3) devoting extraordinary attention to the analysis of mechanically recorded evidence.

“Anecdotal” evidence and its reanalysis

The article and Supporting Online Material (Fitzpatrick et al., 2005b) located the present report within a history of reports made in the decades after the last “conclusively documented” reports some 60 years earlier. In a section on “reports with disputed evidence”, the article lists several reports in the decades since 1944, as well as some photographic evidence, audiotapes of knocks and “kent” calls, a nest cavity, and a feather found near the nest cavity. With the exception of the reports of the Cuban subspecies, these reports are described as actually or possibly

mistaken identifications, or in the case of the photos a likely hoax.

More detailed attention and credibility was ascribed to a more recent history of sightings that preceded the expedition. These began with reports of a possible sighting in 1999 by a forestry graduate student in the Pearl River Wildlife Management Area in Louisiana. Efforts to confirm the sighting included a 2002 CLO expedition in which the research team set up recording devices in hopes of capturing signature sounds from the bird. The Louisiana expedition was a notable failure.

The research team admitted that acoustic analysis revealed that repeated sounds that they at first attributed to the bird’s “knocks” on a tree might have been caused by repeated gunfire by a hunter (Fitzpatrick, 2002). Not long after that expedition was abandoned, repeated sightings, again by amateurs and this time in Arkansas, motivated a further expedition.

Particularly notable was a sighting by Sparling. Though he was a local guide and outdoorsman, with no professional credentials as an ornithologist, his description of an unusual bird he saw during a recreational kayak trip (which he had posted on his website) attracted the attention of CLO members. The published paper’s narrative gives special attention to Sparling’s description, giving a precise time, date, and location, and specifying some of the key markings on the bird he described. As summarized in the Supporting Online Materials:

“11 February 2004 sighting. Field marks noted by G. Sparling were the bird’s unusually large size compared to

pileated woodpecker, peculiarly pointed red crest with black anterior edge, long neck, and extensive white on lower half of folded wings showing slight yellowish tinge along edges ‘like parchment paper’” (p. 2).

Sparling also mentioned that he had seen many pileated woodpeckers in the area, and that this bird was clearly larger and had distinctive field marks. Sparling provided a key link to the CLO expedition. “The apparent authenticity” of his sighting is credited with having motivated Gallagher and Harrison to travel to Arkansas in hopes of validating it. They arranged to have Sparling guide them in the swamp, and it was while they were with him that they made their sighting two weeks later. The full expedition was initiated soon after they reported their sightings to Fitzpatrick and others. Sparling eventually was included on the list of co-authors in the *Science* publication.

Neither Sparling’s nor Harrison’s and Gallagher’s sightings in February 2004 counted as conclusive documentation, but neither were they consigned to “mere” anecdote. Like Sparling himself, whose recognition in the article elevated him to an intermediate status between amateur and professional, the sightings themselves had gained greater credibility than the prior, inconclusive sightings. Following the 2005 publication announcing the (re)discovery, the prior sightings also received renewed attention, and some of them gained a degree of credibility, as though riding on the coattails of the successful expedition.

In a presentation to the American Ornithologists’ Union in August 2005 (available at

<http://www.youtube.com/watch?v=rqZXPUDTmOc>), Fitzpatrick reviewed several of the prior sightings, and while discussing how they were partial, flawed, and even fraudulent, he also presented them as possible evidence (Fitzpatrick, 2005). Just as the recent re-discovery seemed to elevate the credibility of some of the earlier sightings, treating those earlier sightings with renewed seriousness now supported the credibility of the re-discovery by suggesting that there was evidence-all-along that the ivory-bill persisted. In the absence of such evidence, the recent sightings would likely have faced a burden of skepticism that would have been even more difficult to overcome.

In other words, although the sixty-year gap between “conclusively confirmed” sightings and the recent expedition remained in place, the interim was filled with variably plausible, if not conclusive, evidences of continuity.

Authentic visual encounters

During the year-long expedition, according to the published report, “at least fifteen reported visual encounters” were made by participating researchers (Fitzgerald et al., 2005b: Supporting Online Materials, p. 2). Of these “seven contained sufficient detail for the authors to treat them as authentic”. One of these is summarized as follows:

“11 April 2004 (Melanie Driscoll watched a large woodpecker fly across a 50-m gap in the forest where she was stationed, and through 10-power binoculars at 120 m she saw broad white trailing edge of wings, white line extending from wings up the long

neck, and small flash of red on crest, with head otherwise black)” (ibid.).

This compact account not only describes what Melanie Driscoll may have seen, but it also does so in a particular way that anticipates the context of argument in which the description is situated. The report’s inclusion of measured distance lends specificity to the account, and its mention of the expanse of open space and the 10-power binoculars supports the credibility of describing the observed field marks from that distance. The described marks are not just any markings, as they include the singularly most important mark for distinguishing the ivory-billed from the pileated woodpecker.

In his lecture to the AUO, Fitzpatrick (2005) repeatedly stresses that large white patches extending all the way to the trailing edge of the dorsal wings provide the most important distinction to note (for illustrations of the comparison, see:

<http://www.birds.cornell.edu/ivory/evidence/segments/upperwing>).

The pileated woodpecker’s wings are described and depicted with smaller patches of white, bordered by a black band at their rear edges, which are visible even in a blurry photograph. Fitzpatrick (2005) also notes that sketches made independently by Harrison and Gallagher immediately after their February 2004 sighting show the markings clearly and schematically (For the sketches, see Supporting Online Materials [Fitzpatrick et al., 2005b:Fig. S1], available at: <http://www.sciencemag.org/cgi/content/full/1114103/DC1>).

Though drawn independently of each other, Harrison’s and Gallagher’s sketches can be construed as extensions of, rather than independent evidence for, their descriptions. They depict and highlight what is most significant for making the key comparison, while eliding the rest. Fitzpatrick (2005) jokes that the two ornithologists drew “headless” birds. The descriptions, hand-written notes, and pointers enhance and supplement the sketches: black is not just black, but “brilliant black” or “jet black”. The notes also indicate that Harrison and Gallagher saw or recollected what they saw consistently with their schematic sketches: “My entire focus was at the trailing edge of the wing ...”; “Because my eyes were drawn to the contrast between black and white, I have no recollection of head or tail feathers”. Drawing and seeing worked in concert to document and highlight conventional criteria for making the identification (Lynch, 1985b; Goodwin, 1994).

Other members of the expedition also documented their sightings with drawings, always emphasizing the telltale white trailing edges, along with other features, in both descriptions and sketches. Taken alone, the evidence provided by the eyewitness accounts, field notes, and sketches, might be viewed as powerful, and even conclusive. If this were a court of law, the eyewitness testimony would be considered quite strong, if not dispositive. Many of the observers were highly credentialed.

Aside from Sparling, who was deemed credible if not credentialed, most of the others who made sightings were professional ornithologists with ample

field experience. They knew what to look for and how to distinguish between the ivory-billed and pileated woodpeckers, and detecting the difference between the two large birds would not normally be viewed as especially challenging for persons with their level of expertise. The multiple sightings also corroborated one another. This combination of personal credentials, corroboration, and descriptive detail might seem sufficient to establish the fact in question. Recall, for example John Locke's criteria for assessing the credibility of matters of fact:

"1. The Number. 2. The Integrity. 3. The Skill of the Witnesses. 4. The Design of the author, where it is a testimony out of a book cited. 5. The Consistency of the Parts and Circumstances of the Relations. 6. Contrary testimonies . . . [As] the Relaters are more in number, and of more Credit, and have no Interest to speak contrary to the Truth; so that matter of Fact is like to find more or less belief" (Locke, *Essay Concerning Human Understanding*, quoted in Shapiro [2002:258-59]).

In a field such as ornithology, personal skill, integrity, and trust remain highly relevant, but in this case the expedition members did not, and apparently knew that they could not, rest their case on personal testimony and "moral certainty" (cf. Shapin, 1994; Shapiro, 2002).

The early-modern conception of moral certainty, which is roughly akin to the notion of "beyond a reasonable doubt" in modern jurisprudence, requires more than personal conviction: it involves a conjunction of the considerations listed

by Locke, which establish corroboration and credibility, and not just individual testimony. Although modern science is often distinguished by a special degree of certainty, sometimes called "mathematical" certainty, in practice contemporary scientists, no less than the rest of us, rely upon testimony, credentials, credibility ascriptions, proxies and trust when making judgments about fact (Shapin, 2008).

Given the extraordinary circumstances, however, when preparing their evidence the CLO team evidently anticipated disbelief, and perhaps even mistrust. They also recognized that they were open to accusations that their professional vision was obscured and overridden by wishful thinking. They did not present themselves as disinterested observers. Many of them spent the 2004-05 year searching through a vast snake- and mosquito-infested swamp for the elusive bird. As the account of Harrison's and Gallagher's emotional reaction indicates, they were passionately searching for confirmation that the bird existed. And, as Fitzpatrick (2005) points out, this was not simply a matter of solving a mystery about the singular bird. He makes clear that he and others in the expedition were committed to saving and restoring a vast ecosystem whose destruction through logging in the 19th and early 20th centuries is held responsible for the decimation of the bird's population. The publicity, and the legal protections, that would follow from a conclusive confirmation that the ivory-billed Woodpecker still lived in those forests (forests, moreover, that were gradually recovering) would help preserve far more than the remnant population.

Though the tight coupling of seeing, sketching, and classification exhibited in the eyewitness accounts can, from one point of view, enhance the credibility of the sightings by showing that the observers specifically attended to the field marks that are most critical for *drawing* (literally and figuratively) a categorical distinction between the two most likely candidate species, this very coupling could be (and was) turned into grounds for disbelief. Given the observers' attunement to those very criteria, and their evident hopes and desires, skeptics could (and did) ascribe such seeing and drawing to a projection of the type specimen on to the fleetingly-glimpsed tokens¹⁰.

So, rather than resting their case on the testimony of several credible eyewitnesses, the CLO team sought a more modern form of documentary evidence: mechanically recorded images and sounds. However, they did not, and could not, let these recordings "speak for themselves", and they supplemented and analyzed the recordings they collected with models, photoshopped images and processed sound recordings, and simulations of comparable observations (See Supporting Online Materials, available at: <http://www.sciencemag.org/cgi/content/full/1114103/DC1> ; and Cornell Lab of Ornithology, "The Search for the ivory-billed woodpecker," at: <http://www.birds.cornell.edu/ivory/>).

¹⁰ The theme of projection is implied in the title of a 2009 documentary film by Scott Crocker, *Ghost Bird*, likening the 2004-2005 ivory-bill sightings with seeing an ephemeral object that has no real existence.

Mechanically recorded evidence and its "reenactment"

The CLO team's resort to mechanical and simulated forms of evidence created a curious situation: whereas the personal testimony of several experienced and credentialed observers should have been highly convincing in terms of standards of eyewitness testimony, the mechanical evidence was of very poor quality by any standard. The researchers acknowledged this, noting that the Luneau video fragment to which they devoted so much attention was far from optimal.

"The woodpecker remains in the video frame for a total of 4 s[econds] as it flies rapidly away. Even at its closest point, the woodpecker occupies only a small fraction of the video. Its images are blurred and pixilated owing to rapid motion, slow shutter speed, video interlacing artifacts, and the bird's distance beyond the video camera's focal plane" (Fitzpatrick et al., 2005b: 1460).

This mere four seconds of blurry video received extraordinarily detailed attention. Fitzpatrick (2005) joked that it was "the most famous lousy video since the Zapruder [film]" – referring to the blurry home-movie of the assassination of John F. Kennedy, which was the subject to endless frame-by-frame analyses during the Warren Commission's investigation. The evidence for the *Science* article included frame-by-frame analysis of the Luneau tape (A more elaborate analysis is available on the CLO website, at: <http://www.birds.cornell.edu/ivory/evidence/segments/>).

They also made measurements of the number of wing-beats per second, and the apparent dimensions of the bird shown on the video. These calculations were an attempt to discriminate between the two kinds of woodpecker, because the ivory-billed measures slightly larger on average than the pileated.

The researchers also examined the run-up to the key four-seconds on the tape to find further evidence (unnoticed at the time) of “an indistinct object” – presumed to be the bird before it began its historic flight. Two brief, very poorly resolved, sequences were found. One set of frames occurred just before the bird appeared in flight. According to the researchers, the flight sequence (Fitzpatrick et al., 2005b: Supporting Online Materials, Fig. S3) traces back to a series of frames in which a white wing patch appears from behind a tupelo tree. They interpreted this to be a partial view of the bird just as it launches into flight. The other sequence occurred some 20-26 seconds before the flight sequence (this clip is available at: <http://clomedia.ornith.cornell.edu/IBW/6PixBird.mov>). The sequence shows a blurry form on a tree in the distance as the canoe drifts by. This form was not seen by the two canoeists at the time, and is extremely difficult to see on the video, even with the aid of a directional pointer.

The video evidence was enhanced and extrapolated to highlight and upgrade the “indistinct object” so that it more closely approximated the overall form of a bird with white patches on the dorsal side of the wings. The researchers used digital image processing techniques to “de-interlace” and “resize” video stills, and they inscribed outlines on the stills that

depict the bird and its key features, with particular attention to the white patches on the wings. The outlines were extended beyond the visible portion to complete the outline of the parts of the bird obscured by the tree (see Fitzpatrick et al., 2005b:1461, Fig. 1).

Then, after measuring the diameter of the tree, the researchers estimated the size of the white patches and of the entire bird, and plotted the measurements (showing estimated error range) against average measurements for pileated and ivory-billed woodpeckers (using measurements derived from museum specimens). Such inscriptions and measures superimposed upon blurry images were reminiscent of the exhibits used in another frame-by-frame analysis of a famous videotape: the primary exhibit used during the 1992 trial of the police accused of excessive force when they arrested Rodney King in Los Angeles (Goodwin, 1994).

Particularly interesting was the research team’s efforts to “reenact” the conditions under which the tape was shot in order to compare similar framings of Ivory-billed and pileated type-specimens. Unlike digital image processing techniques used to enhance the visibility and analyzability of the evidence, the reenactments attempted to *reproduce* or *simulate* rather than to overcome the perspectival limitations and out-of-focus quality of the video. The inadvertently “lousy” quality of the video was made analytically comparable with other, deliberately produced, “lousy” videos.

Instead of trying to enhance or “purify” (Latour, 1993) the evidence, the researchers made painstaking efforts to reproduce its “impurity” so as to enable

comparisons and measurements. The documentary materials shown on the CLO website include a photo of researchers in a canoe preparing the reenactment, complete with director's clipboard (see <http://www.birds.cornell.edu/ivory/evidence/segments/segments/methods>).

This parody of a Hollywood production also drew upon the conventions of filmic realism – the deliberate simulation of the spontaneous, non-choreographed, unedited “feel” of a grainy on-the-ground scenario. However, the point of this production was not to produce a convincing nature documentary (cf. Mitman, 1999), but to produce controlled conditions for inspecting the video evidence and conclusively eliminating the alternative hypothesis that “indistinct object” in question was a pileated woodpecker.

The segments of the perched and launching bird were analyzed, not only by closely inspecting and upgrading stills drawn from the videotape, but also by comparing the frames with other out-of-focus videos shot later with the same camera from approximately the same positions. The “indistinct object” found in the Luneau video did not show up in the control videos, thus supporting the idea that it was the bird. Further comparisons were drawn by building life-sized, painted wooden models of pileated and ivory-billed woodpeckers, attaching them to the tupelo tree at the apparent positions shown in the video segments, and then shooting them from similar perspectives, with the same camera, with similar focus and under comparable lighting conditions (Fitzpatrick et al., 2005b: Supporting Online Materials, Fig. S5, available at:

<http://www.sciencemag.org/cgi/content/full/1114103/DC1>).

Another “reenactment” was made for the analysis of the portions of the video showing the flying bird. The published supplement described the reenactment as follows:

“For the re-enactment (performed 15 March 2005), we constructed, painted, and mounted on long poles lifelike wooden models of ivory-billed and pileated woodpeckers with outstretched wings. Operators could flap the wings at a rate of about 1 flap s⁻¹. Models were held at different angles to reveal both upper and lower wings. Slow shutter speeds (1/8 s, 1/15 s) produced blurred images comparable to those on the Luneau video. The re-enactment's location, camera-to-subject distance, light conditions (overcast sky near midday), camera (Canon GL-2), and studio treatment were the same as in the original” (Fitzpatrick et al., 2005b: Supporting Online Materials:1).

Once again, the videos of the two models were compared with each other and the Luneau sequence, frame-by-frame. The researchers concluded from this comparison that the Luneau sequence more closely resembled the sequence with the ivory-billed model. Moreover, they also claimed (and documented with images) that the sequence with the pileated model “clearly shows a broad black trailing edge, despite the obvious blurring” (<http://www.birds.cornell.edu/ivory/evidence/segments/resultsunderwing>).

Skeptical reanalysis

As anticipated by the researchers themselves, the public announcement of the discovery touched off skepticism as well as excitement. Jerome A. Jackson (2006a) initiated a heated exchange with Fitzpatrick et al. (2006a) in *The Auk*, the official quarterly of the American Ornithologists' Union. After stating a medley of doubts and criticisms, Jackson reviewed the series of anecdotal reports made over the years, and asked about the CLO team's sightings, "what makes them different from the almost annual handful of sightings that cannot readily be dismissed as 'almost certainly a pileated'?" Answering his own question, he asserted "it is not necessarily the quality of the evidence, but the attendant publicity and aura of authority associated with the announcement, that has raised the profile of the Arkansas reports" (Jackson, 2006: 5).

In the course of his wide-ranging criticisms of interests and motives as well as evidence, Jackson mentions that Luneau was an "engineering professor" and Harrison "an art professor", and he points to how Harrison's story became embellished with successive re-tellings. Though he does not accuse them of deliberate deception, he suggests that they and their colleagues rushed to "sell" their discovery (p. 6). Quoting someone named M. Lynch (no relation), he asks: "Will [the ivory-billed woodpecker] be the poster child for new fund-raising efforts for Cornell and The Nature Conservancy?" After running through a list of fund-raising efforts, hyped press accounts, and successful efforts to commit funds to preserve the region of Arkansas, Jackson assumes a sobering tone and invokes a need for "sound

science" and "skepticism".

Jackson cites a personal communication from Fitzpatrick, that there was an error in the supplementary material to the 2005 article (Jackson, 2006a, referring to Figure S5A in Fitzpatrick et al., 2005b): the video frame likely was "a branch stub [...] rather than a perched ivory-billed woodpecker [...]" (this error was later acknowledged in Fitzpatrick et al., 2006a). He then rebuts or casts doubt upon the evidence based on the analysis of field marks (white patches against dark backgrounds), size and flight pattern comparisons, and acoustic recordings. Echoing a criticism made earlier by a pair of Brazilian ornithologists, he suggests that a variant of a pileated woodpecker could show the pattern of white underwing the CLO researchers found so definitive.

In a letter published in *The Auk*, Fitzpatrick et al. (2006a) presented rebuttals to a long list of points in Jackson's article, heading almost every paragraph with phrases such as: "Jackson is incorrect in alleging [...]", "Contrary to Jackson's suggestion [...]", "Jackson is incorrect in stating [...]", and "Jackson is incorrect and naïve in suggesting [...]". Jackson (2006b) continued the exchange in another letter, toning down the argument slightly, while reiterating his earlier assertions that the evidence was inconclusive, expressive of wishful thinking, and potentially damaging to the thus-far highly successful effort to promote conservation of the habitat in question.

Though clearly designed to convince skeptics that the sightings were not fleeting impressions gathered by interested observers, the video-analysis

provided ample material for skeptical re-analyses. Jackson treated that analysis as of a piece with the “faith-based science” that he attributed to the expedition, and he saw no essential difference between that video evidence and the long series of unconfirmed sightings that had been reported over the years. He alluded to a detailed re-analysis of the Luneau video that he and three other ornithologists performed. Their draft paper apparently was never published, but David Sibley, author and illustrator of a popular bird book series, who (as noted earlier) did not include the ivory-bill in his field guide, together with three other ornithologists soon afterwards published such a paper in *Science*. In their article, which was accompanied by an online supplement, Sibley et al. (2006) re-analyzed the video frame-by-frame and suggested that the bird in question was actually a pileated woodpecker, perhaps with unusual plumage.

Like Jackson, Sibley and his colleagues dismissed “[t]he recent sight records” for being “all very brief”, and involving “a single observer, matching the pattern of reported observations over the past few decades”. According to these skeptics, such sightings “do not provide independently verifiable evidence”. Quoting from Fitzpatrick et al.’s (2005b) initial assessment of the video, Sibley et al. (2006) argue, “confirmation that ivory-billed woodpeckers remain in the United States rests on demonstrating that the ‘crucial video of a large woodpecker’ cannot possibly be a pileated woodpecker” (p. 1555).

They then systematically dismantle five critical features of the video evidence, and argue that a reinterpretation of the “posture” of the bird shown in the video

reveals that the size and patterns of light and dark patches correspond to those of the pileated and not the ivory-billed woodpecker.

Sibley re-drew diagrammatic versions of a series of video frames, starting with the launch of the partially exposed bird from the tree trunk, and proceeding through the initial wing beats. The diagrams depicted the bird with a different posture than Fitzpatrick and his colleagues had shown in their reconstructive drawings and reenactments. The Sibley renderings were (arguably) consistent with the light and dark patches shown in the blurry video frames, but also with the field marks of a suitably positioned, schematic image of a pileated type-specimen (see the series of figures S2 in Sibley et al., 2006, Supporting Online Material, available at: www.sciencemag.org/cgi/content/full/311/5767/1555a/DC1).

From this re-analysis, the skeptics “conclude that one cannot reject the hypothesis that the bird is a normal pileated woodpecker (i.e., the null expectation); moreover, the evidence firmly supports this hypothesis” (Sibley et al., 2006:1555). Note how this refutation is phrased in a classic hypothetico-deductive way. This framing is congruent with the quasi-experimental setup described in the original article by Fitzpatrick et al. (2005b). Fitzpatrick et al. (2006b) rebutted this article, though in a more technical way than in the exchange with Jackson.

In his speech to the AOU, Fitzpatrick (2005) mentioned that the argument with Sibley was a cordial exchange among gentlemen. He did not mention the

criticisms by Jackson, which included lay-sociological accounts of motives and vested interests, and accusations of “selling” the bird for the sake of publicity and research funds.

Fitzpatrick et al.’s (2006b) rejoinder included another set of supplementary materials, which further elaborated the analysis of the video through the use of diagrammatic extrapolation and comparisons using models. Perhaps this served to clarify and refine the initial analysis, but it also was becoming evident by then that no amount of further elaboration of the materials would provide the kind of “conclusive documentation” that would settle the argument. Though many of the critics themselves expressed hope that such documentation would be forthcoming, it was clear that qualitatively different evidence would be needed, and not a re-analysis of the same evidence or even a multiplication of witnesses. As though to underline this point, another set of sightings a year later by a different group in a different place, attracted less notoriety and was more quickly consigned to inconclusiveness.

Aftermath

In September 2006 Geoffrey Hill of Auburn University and Daniel Mennill of the University of Windsor, Ontario announced a series of sightings of the ivory-billed woodpecker in the Cyprus swamps of the Florida panhandle (Hill et al., 2006).

They documented their published report with photographs of nest cavities and visual renderings of audio analyses. In some respects, their reported evidence was superior to that produced by the

Arkansas group: they reported 14 sightings, in some cases of more than one bird; some of the sightings were for longer periods and from less distance than the Arkansas sightings; the descriptions and sketches accompanying some of the sightings mentioned several characteristic field markings, as well as a “duck-like” flight pattern; the researchers amassed hundreds of recorded “double-knocks” and “kent” calls; and they collected and photographed evidence of nest cavities and “chiseled” bark that matched historical records of ivory-bill cavities and foraging patterns.

The published article extrapolated from the nest holes to estimate the size of the bird that made them. By comparing graphic estimates of pileated and ivory-billed nest holes, they argued that the holes in question were more clearly within the ivory-billed range. However, they produced no photographs of the bird, and their report was greeted with skepticism by many other ornithologists, including the Cornell researchers. The authors themselves muted their claims, and acknowledged that they had yet to produce definitive evidence: “Future research related to the ivory-billed woodpecker in the Choctawhatchee River basin will focus on gathering definitive evidence for the existence of at least one bird, and searching for evidence of nesting ivory-billed woodpeckers” (Hill et al., 2006:14).

In 2007, the Florida Ornithological Society Records Committee summarized and expressed the skeptical context in which this evidence was received, when voting to maintain that the bird remained extinct.

“There have been a few sightings but no photographs, some interesting recordings of ‘kent’ calls and of double rap drums, and photographs taken of cavities and bark scaling. These observations were made on the heels of the much-publicized ‘rediscovery’ of the species in Arkansas (Fitzpatrick et al., 2005). The species had not been documented to occur since 1944. The video documentation of the bird(s) from Arkansas, however, has been debated by many, although the record was accepted by the Arkansas Bird Records Committee. Our Committee felt that given the controversy of the Arkansas evidence, the species is best considered still extinct. Therefore only evidence that undoubtedly showed a living bird would be considered sufficient to accept a report” (FOS Board Report, April 2007. Florida Ornithological Society Records Committee. Available at: <http://www.fosbirds.org/records-committee-reports>).

As signaled by the line “given the controversy of the Arkansas evidence”, the Florida Committee’s demand for evidence was conditionally relevant to the prior expedition’s evidence and the skeptical treatment of that evidence. The contrast the report draws with the earlier decision by the Arkansas Committee to accept the CLO evidence, suggests a degree of stringency retrospectively and prospectively tied to the intervening controversy. The bar was now higher.

After 2006, the CLO team made yearly expeditions to Arkansas, Louisiana, and later Florida, but without getting further evidence that was better, or even nearly

as good as, what they obtained in 2004-05. Following the 2007-08 expedition, the CLO site summarized:

“Flooding, snowstorms, vehicle breakdowns, alligators, snakes, and bugs — another field season spent searching for the ivory-billed woodpecker has come to a close. The search teams covered lots of ground and tried new survey techniques. [...] Searchers documented more possible sightings and possible ivory-bill double knocks heard, but the definitive photograph, like the bird itself, remained elusive” (http://www.birds.cornell.edu/ivory/08_09stories/0708summary).

In 2009, after five years, with funding exhausted and little to show for the most recent expedition, the search was ended. The CLO website maintained that analysis of the vast amounts of acoustic data would continue, but some (but by no means all) ornithologists now resign themselves once again to the prospect that the bird, if not already extinct, is beyond hope of recovery (Dalton, 2010).

The Florida team also made expeditions after their 2005 sightings, and even released a video (that they acknowledged was inconclusive)¹¹, but by 2009, that expedition also was winding down. After listing the various forms of evidence, Hill and Menill (2009) conclude that they had not yet gathered “indisputable evidence” and that no such evidence “has been gathered since photographic images of Ivory-

¹¹ The video is available in various forms and segments at: http://www.auburn.edu/academic/science_math/ivorybill/ibillvideo.html

billed Woodpeckers were made in the Singer Tract of Louisiana in the 1930s". Note that this assessment moves the last verified evidence of the ivory-bill's existence to an even earlier date – it is now in the 1930s. The rising standard of proof thus implicates the assessment of past as well as future evidence.

Conclusion

Given the researchers' resignation to the fact that what they had collected so far was less than definitive, what, then, would count as conclusive, definitive, or indubitable evidence? The answer given by the Florida Ornithological Society Records Committee was that what is needed is higher quality photographs that would show incontrovertible evidence of a living bird. However, this leaves open just what kind of photographic (or other material) evidence would be high quality *enough*.

As the episode unfolded, documentary evidence and skepticism about the evidence were generated in a reciprocal relation to each other. Though all parties acknowledged the limited quality of the existing evidence and testimony, those limits were relative to the skeptical demands, and vice-versa. Would a crystal clear photograph showing the telltale wing pattern be sufficient, or would the specimen be disputed as a possibly mutated pileated? A proverbial bird in the hand (whether alive or recently dead) would, of course, be impressive evidence, though it seems clear that DNA analysis would be demanded, in order to confirm that the bird was not simply an unusually large, oddly marked pileated woodpecker. Moreover, if no further "conclusive" observations of living specimens were

forthcoming, doubts would likely arise about whether a recently deceased specimen was the last surviving member of a now-extinct species.

In the present context, after several years of fruitless effort to confirm the earlier sightings, it is easy to forget that the 2005 publication in *Science* presented data that, at the time, seemed highly convincing if not absolutely certain. There were several sightings. The observers had impressive credentials for making field identifications, and they described the key distinguishing features and did so with overwhelming conviction.

Under ordinary circumstances the CLO researchers would be assumed to have had no difficulty identifying a bird as large and clearly marked as the ivory-billed woodpecker. Perhaps this would have been enough for a latter-day John Locke to assign "moral certainty" to their testimony.

In this case, however, the team came equipped with sketchpads, digital cameras with powerful lenses, and sensitive audio recording equipment, anticipating (as it turned out, for very good reasons) that their eyewitness testimony would not be enough. And, the extensive amount of work they used to upgrade the intelligible and interpretable quality of the video and audio materials they collected also indicated that they anticipated a skeptical reception. Their practices were designed to make the most out of what they had, to compensate and control for their evident limitations and to place them in a quasi-experimental frame.

By doing so, they may have elucidated

and enhanced the evidential value of the mechanically recorded evidence, but they also provided convenient targets for “scientific” criticisms. The concentration of attention on the video analysis was perhaps a wise concession to the contemporary demand for mechanical measurements and instrumental recordings as marks of objectivity (Daston and Galison, 1992; Porter, 1995).

The conversion of field observations into calibrated measures and comparisons accentuated the distance between professional ornithology and mere birding. Though the popular celebration (and, perhaps, even the highly emotive reactions expressed by team members such as Harrison and Gallagher) treated the sightings as a spectacular instance of birding, much of the work in the publications took the form of a dispassionate proof with graphic materials. Viewed with the wisdom of hindsight, they might have saved themselves immense amounts of time and trouble had they simply rested their case on their credibility as expert birders who knew their woodpeckers when they saw them. However, as they no doubt knew, this would not have been likely to satisfy their critics in a modern scientific forum.

The debates (particularly with Jackson) resembled an adversary trial in which the opponent sought to turn the “authority” of the CLO against its credibility, while also impugning the credentials of some of the team members. Jackson and other critics imputed interests to the researchers, and highlighted the extent to which their actions fell short of “scientific” ideals. They took every opportunity to highlight the interpretive

flexibility of the evidence; laying out alternative interpretations point-by-point. Such tactics are, of course, fair game in scientific controversies, but skepticism also is recognized to have no ultimate limit, since (as is so often said) scientific findings are never absolute.

Philosophical critiques of empiricism (Quine, 1980) and arguments about the theory-ladenness of evidence (Hanson, 1961) suggest that there is always the possibility of coming up with more than one theoretical interpretation of empirical data. Accordingly, a steadfast critic can always maintain skepticism in the face of any amount of evidence. Given the hypothetical possibility of endless argument, it has been said that “closure” of actual controversies is based on pragmatic, social grounds, and not the compulsive force of evidence (Collins, 1985). For the many proponents of such a skeptical treatment of observation in the field of science and technology studies, “discovery” therefore is a fragile category, because the status of any “discovery” depends upon communal assent, which can always be withdrawn, at least in principle.

The fate of the ivory-billed woodpecker’s “rediscovery” in Arkansas and/or Florida might seem to illustrate such *general* skepticism about evidence, above and beyond *particular* skeptical arguments about the quality and interpretation of the visual and auditory evidence and the motives and interests of the researchers. By framing the arguments with talk of null hypotheses and demanding “scientific” virtues and standards of proof, both sides seemed to buy into an all-too-familiar set of general skeptical demands. However, while the

Florida reports showed that fresh, arguably better, evidence did not win the day in the face of dug-in skepticism, perhaps the most definitive (lack of) evidence was provided by the passage of several years without significant new reports to fuel further argument. Repeated efforts to work through the 2004-2005 evidence yielded at best a standoff, with tentative hopes expressed on both sides that more and better evidence would be found in the near future.

The demise of the rediscovery, and the fading of controversy about it, was not due to the absence of a single crucial test that would nail down certainty. Instead, it was due to a lack of accountably “fresh” evidence during a period of relative stasis when an outpouring of more sightings and better documentation was both expected and subject to heavy investment. The demise of the expedition’s status as a source of (re)discovery surely was bound up with the evidence, but the temporal relationship of the evidence to the status of the discovering work was not well described with a binary logic of proof or disproof, or confirmation and falsification. Instead, it was more of an “analog” matter of exhaustion in the face of a rising degree of skepticism compounded by the acknowledged lack of further “good” or “better” evidence.

The apparent failure of the repeated expeditions to secure the status of their rediscovery does not mean that their *discovering work* was uninteresting. Although the episode may fade into obscurity, and take its place alongside the series of inconclusive, “anecdotal” sightings, for a number of reasons it continues to hold interest for studies of

discovering work and visualization. One attraction of the case for research and demonstration has to do with the abundance of online documentary material, together with the relatively non-technical discourse and practices involved in this case (as compared with, say, a controversy in mathematics or particle physics).

The acknowledged limits of the visual documentation, and the debates about them, provided a level of detail that would be lacking in less problematic cases. The extraordinary extent to which the researchers worked to enhance the intelligibility and analyzability of the video and audio materials was especially notable.

I devoted little attention to the work with the audio materials, but it was (and continues to be) at least as significant as the work with the visual images, and would be a rich topic for further analysis (see Bruyninckx, 2011). With both sets of materials, the researchers not only worked with “data” in the sense of collecting, calibrating, replicating, coding, separating signal from noise, and so forth, they also built imagined scenarios around the data in which they reenacted their observations with simulated objects, using models as well as pictures, sound recordings, and measurements derived from historical sources to reconstruct the conditions and systematically compare thematic objects.

They did not simply try to overcome the limits of their “lousy” data; instead, they attempted to re-produce the limited perspectival qualities of their recordings in a kind of controlled experiment, in order to elucidate what they could possibly show. This imaginative work

was no less material than the images under scrutiny, and it vividly exhibited what is involved in *seeing* and *interpreting* what is shown “in” the video frame.

Regarding current transdisciplinary interests in scientific images, this instance helps to exhibit the extraordinary degree to which seeing and showing what is “in” an image is not produced entirely through a relationship between the image and the “mind’s eye” of a well-prepared observer. Instead, it involves a more complicated, collective production, reproduction, and simulation of what the “thing” in question could possibly be, and comparing it to what else it might be.

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