Discovery and discovering work in the sciences

Michael Lynch

Professor Emeritus, Department of Science & Technology Studies, Cornell University

Jeff Coulter (1948–2025)

Professor Emeritus, Department of Sociology, Boston University

Abstract

Following the publication of Thomas Kuhn's *Structure of Scientific Revolutions*, there was a brief period in which sociologists took an interest in the phenomenon of *discovery* in the sciences. The constructivist orientation that flourished in the sociology of scientific knowledge in the 1980s and early 90's encouraged initial interest in discovery, but trends in the newly coalesced field of Science & Technology Studies (STS), along with changes in the natural sciences themselves, eventuated in widespread disregard of the topic. The present paper begins with a discussion of ordinary uses of the word "discovery" and more specialized conceptions of scientific discoveries. Drawing upon ordinary language philosophy, the paper suggests that "discovery" is used as an achievement verb, marking a "terminus" rather than an extended process or procedure. After discussing social-historical treatments of discovery as a communal *construction*, the paper observes that the topic of discovery, as a substantive event or process, largely vanished from research in the field of Science & Technology Studies. Finally, the paper summarizes Garfinkel's focus on the *work* of a *discovering science* and considers if such a focus can offer a renewed interest in discovering work for studies of scientific practices.

INTRODUCTION

N. R. Hanson once wrote that "[p]hilosophers of science have [...] left all scholarly, analytical concern with discovery, creativity, and innovation to the academic psychologists, sociologists, and historians; such as these seem to be the inheritors of the entire subject" (1967, 321). That inheritance did not prove to be long-lasting in Sociology or Science & Technology Studies (STS),[1] though psychologists and historians have retained some interest in the

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1 The STS acronym does double-duty as an abbreviation for Science & Technology Studies and Science, Technology & Society. We prefer the former for its broader reach, and not-incidentally because it happens to be the name for the department in which one of us has worked during the past 25 years.

subject. As we discuss in this paper, some important sociological and social-historical work on scientific discovery was done in the 1970s through the early 1990s, but, as Sormani, Gonzalez-Martinez, and Bovet (2011) observe, STS has grown quiet on the subject in recent decades.[2]

Hanson attributed the lack of philosophical interest in discovery in the mid-20th century to the entrenched distinction between context of discovery and context of justification.[3] According to that distinction, the context of *justification* was deemed more tractable to logical analysis, while the context of discovery was consigned to individual psychology and accidental circumstance. Despite its title, Popper's (1959) *The Logic of Scientific Discovery* had little to say about the immediate context of discovery and much more about the logical relations between theories, hypotheses, and empirical evidence. Hanson and other philosophers and historians of his generation (most famously, Thomas Kuhn) rejected that dichotomy and the associated assumptions about the individual origins of discovery and encouraged investigations of the communal production and conceptual *grammar* of discovery.

At the height of its popularity, Kuhn's ([1962] 1970) *Structure of Scientific Revolutions* inspired social historians and sociologists to critically explore concepts that had been of interest to philosophers of science.[4] Prior to these developments, sociologists had occasionally taken interest in simultaneous discoveries (Merton 1961) and serendipitous discoveries (Barber and Fox 1958), as well as the distinctive forms of reward and credit for discovery and the social sources of "resistance" to discovery (Barber 1961). For the most part, however, they deferred to philosophers for analyzing the logic of discovery and practicing scientists and awards committees for identifying discoveries and assigning them to authors.

Kuhn's and related treatments of scientific practice suggested that observations, experiments, and descriptions not only were "theory-laden," but also "laden" with historically specific communal paradigms that sociologists (despite Kuhn's own hostility towards their interpretations) eagerly linked to cultural ideologies, social interests, and frames of meaning. This emphasis on the social and cultural ladenness of scientific practice provided a major impetus for the avowedly relativist treatments of the natural sciences and mathematics in sociology of scientific knowledge (Bloor 1976; Collins 1985; Latour and Woolgar 1979; Knorr-Cetina 1981).[5] A proliferation of social and cultural studies coalesced to form the field of

2 Woolgar (1976), Brannigan (1981), Schaffer (1986, 1994) developed studies of discovery that focused on narrative accounts and communal attributions, rather than a general logic, process, or objective reference point that such accounts and attributions posited.

3 Hanson attributes the distinction to Frege and Reichenbach, but variants of it have become (and still are) entrenched in vernacular references to scientific discoveries.

4 To put it mildly, Kuhn did not personally encourage the widespread appropriation of his ideas (especially "paradigm shift") by social scientists. See Shapin (2023).

5 By the time the second edition of *Laboratory Life* was published and re-titled (Latour and Woolgar 1986), both authors had disaffiliated from the social constructivism that was prominent in the sociology of scientific knowledge, but for the most part the first and second editions were strongly affiliated with that perspective.

STS, and it became commonplace for such studies to take up basic themes in the philosophy of science, such as observation, representation, replication, measurement, and demarcations between science and non-science, and to subject them to social-historical, sociological, and cultural interpretations (Collins 1985; Shapin and Schaffer 1985; Gieryn 1983). The concept of *scientific discovery* was among the themes that were subjected to such revision, particularly in Brannigan's (1981) elucidation of an intrinsic social logic of scientific discovery. However, before long, the topic of discovery not only underwent "socialization," it also largely vanished from the STS literature. This paper provides some suggestions about why *discovery* has faded from relevance to STS and, following Garfinkel's (2002, 2022; Garfinkel, Lynch, and Livingston 1981) ethnomethodological treatment of the *work* of a discovering science, it suggests that studies of *discovering work* can avoid the *ex-post facto* reification of discoveries and deification of discoverers.

The paper begins with a brief examination of some of the uses of the words "discovery" and "discovering" in ordinary language and in more specialized treatments in the sciences. It then focuses on Brannigan's innovative treatment of the grammar of scientific discovery, which emphasizes "common sense" criteria scientists use to decide *what counts as* a discovery. Following that discussion, the paper takes up a conundrum that arises from *constructivist* treatments of discoveries as achievements that are inseparable from the reception and adjudication of discovery *claims*. The conundrum has to do with the apparent inability of any "internal" description of scientists' practices to explain scientific discoveries, even though discoveries continue to be attributed to persons and collectives. The paper also suggests that the *phenomenon* of discovery has changed in many fields, both as a matter of practice and of discursive claims and formal attributions. It concludes by recommending investigations in line with an ethnomethodological conception of *discovering work* in science, as an alternative to treating discovery as a substantive process or retrospective communal construct.

CONCEPTIONS OF DISCOVERING AND DISCOVERY

Before delving into the specialized sense and status of discovery in the sciences, it is worth reminding ourselves that "discovering" and "discovery" are ordinary words. In some contexts of use, *discovering* something is akin to *seeing* something. As Coulter and Parsons (1990, 255) observe, paraphrasing Ryle (1949, 233), unlike "looking for" something, "'seeing' is akin to an *achievement* and is not any sort of activity, process, or 'undertaking.'" "Discovering" can be likened to what Ryle says about "seeing": that it is not "something that I go through, am engaged in. It does not signify a substretch of my life-story" (Ryle 1954, 88). For example, if someone were to say, "I finally discovered where I had left the battery charger for my camera—the damn thing was hidden in the back of the desk drawer in my study," they would be using the verb "discovered" to mark a terminus or upshot. Similarly, if they had "discovered" the battery charger while reaching into the back of the drawer for a pencil, the verb would denote an inadvertent success, rather than an intentional *process* or *project*. When "discover" is regarded as an "achievement word," it casts into relief the conceptual confusion inherent in statements such as, "I shall construe discovery rather narrowly as concerned with 'the *eureka* moment', i.e., the time when a new idea or conception first dawns" (Laudan 1980, 174).[6] The problem is that the dawning of a new idea or conception only marks the *achievement* of a discovery after it pans out, is ratified, and credited.

An analysis of ordinary usage provides insight into, but does not fully address, the distinctive use of "discovery" that occurs in (and in stories about) major scientific achievements. When a person says, "I just discovered a new Thai restaurant in town," or "we discovered after returning from a winter trip abroad that our furnace had gone out while we were away," the discovery is a circumscribed "achievement" with limited interest beyond the telling. Discoveries of a lost item, a new restaurant or a broken furnace tend to be significant only in localized temporal and personal contexts, and stories about such discoveries are likely have very limited distribution.[7] Even in scientific fields where discoveries are notably valued, more mundane senses of discovering and of what is discovered remain in play. Routine laboratory work often involves minor discoveries of methodological tweaks and novel steps in routine procedures that only in rare cases travel beyond a particular laboratory. Even many cases of reported scientific discovery are likely to have no more than limited distribution within small specialties, such as when a heretofore unknown species of beetle is characterized and named. Such instances rarely count as "Big D" Discoveries.

When philosophers and historians of science speak of discoveries and moments of discovery, they have major novelties and breakthroughs in mind. As Hanson, Kuhn and others recognized, however, discoveries not only are of interest to professional historians and philosophers, but also are communally recognized achievements. To paraphrase Austin (1970, 185), while historians may have the last word (or, at least, a later word), they do not have the

6 Laudan (1980) does make a helpful suggestion, however, which is that the context of *pursuit* of discovery is intermediate between the two classic "contexts" of discovery and justification. Ryle (1954, 89) likens expressions using perceptual verbs (e.g., "Now I see it!") to "terminus-verbs" such as "to win" a race or a temporal marker like "noon." Running is a substantive action with duration, but winning a race requires other runners in a competition, and is relative to what the other runners do and how victory is judged. "Noon does not begin, go on and finish. It is itself the end of the morning, the beginning of the afternoon and the halfway point of the day. It cannot itself go on for a time, however short. It is not a process or a state" (*ibid*., 87). Schaffer (1994, 13) also mentions Ryle's (1949, 151) inclusion of discovery in a list of achievement words, and cites Ryle's example of "checkmate" as a comparable expression to "eureka" for marking a terminus. However, Schaffer seems to link Ryle's grammatical investigation with conventional philosophers and popularizers who conflate "achievement words" with substantive historical achievements. However, Ryle is focused on the *grammar* of ordinary usage. While it may be contradictory to say that a scientist made a discovery "unsuccessfully," this does not mean that a *declared* discovery cannot turn out to be unsuccessful (Ryle 1949, 238). One thing Ryle does not consider in the case of discovery, which Schaffer does, is that a declared discovery can turn out to be *precedented*, and thus deemed unsuccessful *as a discovery* even though the procedure was correct.

7 For an illuminating account of how stories told in ordinary conversations about witnessed events have limited distribution, see Sacks (1992, 242–248).

first word. Prior to being chronicled as part of the history of science, the certification of discoveries, allocation of credit for them, and assessment of their importance are part and parcel of scientists' communal activity. Accordingly, members of scientific fields themselves act as practical historians when orienting to the possibility of making scientific discoveries as well as when evaluating discovery claims made by others.[8]

Scientific discoveries are, by definition, unordinary, and they are subject to both formal and informal announcement and adjudication, but "discovery" also is an ordinary word in the vernacular discourse of historians, philosophers, and scientists themselves. Hanson (1967: 324) proposed to explore "the informal logic of discovery talk" with a characterization of questions raised in disputes about what counts as a discovery and who should be credited with it.[9] He developed a taxonomy that differentiated between different activities and circumstances in which discoveries arise:

- *Trip-over* discoveries which lack theoretical anticipation (for example, the Coelacanth; Galileo's observation of the moon's craters);[10]
- *Back-into* discoveries that arise despite expectations against them (for example, Michelson's discovery of the nonexistence of the ether);
- *Puzzle out* discoveries that begin with a theoretical expectation that is confirmed by persistent and innovative effort. An example is Clyde Tombaugh discovery of Pluto, which was predicted, but not realized until corrections were made to the technological means of observation;
- *Concluding that*, for example, a prior "discovery" or established theorem is incorrect;
- *Subsuming-and-reticulating*, such as when Newton subsumed planetary motion to earthly laws of motion and gravity.

8 Leudar and Nekvapil (2011) draw upon ethnomethodological treatments of local historicity and its practical relevance (Garfinkel, Lynch, and Livingston 1981; Lynch and Bogen 1996). Leudar and Nekvapil examine narratives of the surprise attacks by Al-Qaeda members on the US World Trade Center and Pentagon on September 11, 2001 and compare how rival political and media agents pragmatically acted *as* historians of 9–11 when they highlighted notable actions and placed them in historical contexts to explain or justify them.

9 As a reviewer of an earlier draft of the present paper astutely pointed out, Hanson does not analyze "ordinary talk of discovery," but instead develops a taxonomy informed by capsule summaries drawn from history of science. The "talk" that Hanson alludes to and the distinctions he makes are roughly akin to the reflective analysis of ordinary language that was extant in his day. It was not anything like the "shop talk" and practical discovering work that ethnomethodologists and conversation analysts took up, starting in the 1970s.

10 It is often said that a "prepared mind" is necessary for recognizing the significance of what is "stumbled over." In Galileo's case, according to Edgerton (1984), prior practice with the artistic technique of chiaroscuro prepared the eye of the Renaissance man to see and draw three-dimensional figures rather than two-dimensional spots on the moon.

Hanson's taxonomy is presented as a description of the achievements that historical scientists pursue, and which other scientists and members of prize committees publicly recognize *as* discoveries. Professional historians and philosophers also take part in such recognition, though usually at considerable distance from the constitutive actions and judgments. For example, one of the cases that features prominently in Kuhn's ([1962] 1970) *Structure* is the late 18th century discovery of what eventually was characterized as oxygen. Kuhn argued that "[a]t least three different men have a legitimate claim to it, and several other chemists must, in the early 1770's, have had enriched air in a laboratory vessel without knowing it" (1970, 53). He quickly dismissed one of them, C. W. Scheele, by saying, "We may [...] ignore his work" because it was published after the discovery had repeatedly been announced elsewhere (*ibid*.). Kuhn also dismissed another claimant, Joseph Priestley by saying, "If holding impure oxygen in one's hands is to discover it, that had been done by everyone who had ever bottled atmospheric air [...] in 1775 he saw the gas as de-phlogisticated air" (*ibid*., 54). Kuhn did not give Lavoisier full credit, because at the time Lavoisier identified the gas as "air itself entire," rather than the element oxygen. Kuhn (*ibid*., 55) credited Lavoisier for identifying the gas in 1777 as "oxygen," even though, for the remainder of his life, he characterized it as "an atomic principle of acidity" associated with caloric. Kuhn concludes from his review of the case, that assigning a specific date and naming the discoverer is a pointless endeavor: "Discovery is not the sort of process about which the question [of priority] is asked" (*ibid*., 554). However, in light of Ryle's insight that an achievement word marks a "terminus" and not a duration or even a moment of time, the word "discovery" is not a name for a substantive "process" at all.

Simon Schaffer (1986) agrees that there is no single moment at which historians can date the discovery of oxygen, but he criticizes Kuhn for continuing to search for "some means of guaranteeing the labelling of a discoverer in such a way that does not depend on the local practices of the relevant community" (*ibid*., 392). "In view of this central role of linguistic usage in the very discovery stories being investigated, it is damaging to gloss eighteenth-century language in a way we would need if we were to seek identifiable discoveries" (*ibid*., 396).

Schaffer argues that Kuhn's virtual adjudication of a two-and-a-half-century old achievement raises conceptual matters that Augustine Brannigan (1981) refined into an "attribution model" of discovery, where such attributions are made by scientists—or, prior to the 19th century, natural philosophers—at the time or soon afterwards. One may be tempted to construct an anachronistic pun on "natural philosophers" by construing them to be "natural analysts": commonsense reasoners whose "analyses" of one another's situated actions constitute their collective organization and import. Indeed, Brannigan comes close to doing so.

Brannigan lists four "common sense grounds or criteria of intelligibility" in his analysis of discovery: "that the events of the research were [1] *possible*, [2] *motivated achievements*,[11]

11 The criterion of "motivated achievement" might seem inapplicable to serendipitous discoveries (Barber & Fox, 1958), but the relevant issue is not that researchers must be motivated from the start to discover what they end up with; instead, it is that they must give a credible account of what they have, regardless of how

which were [3] substantively *true* or *valid* and whose announcements were [4] *unprecedented*" (1981, 71; emphasis in original). A key point Brannigan makes is that individualistic and procedural accounts of discovery cannot fulfil all those criteria. Considerations of individual genius, eureka moments, serendipity, and other individual or group qualities, experiences, or methods become determinate only in retrospect, after a discovery and discoverer have been communally recognized as such.

Brannigan (1981: 66) observes that making a discovery is not like following a recipe for making beer. Although many routine procedures may be involved in a discovery, the result is credited with being novel and unprecedented. This applies to procedural descriptions of discovery, since the procedure followed by a group of scientists that announces a discovery without knowing that another group had announced it earlier, will consequently be deemed a procedure for a *replication* or *confirmation* that is logically and historically distinguished from the original *discovery*. Even Hanson's taxonomy characterizes courses of action that eventuate in *failed* as well as successful discovery claims. By implication, a computer program that reproduces the results of an established discovery can only count as a *replication* (Brannigan 1989). Even a novel "discovery" attributed to a machine learning program awaits its ratification in a human community.[12] Given the criterion of *unprecedentedness*, the context of justification—perhaps better thought of as the context of adjudication—becomes an open-ended communal matter which is part of the social production of discovery, and is not limited to "rational reconstruction" of an already achieved discovery. Rarely are professional historians and philosophers members of the relevant community, but Kuhn's retroflection on Scheele, Priestly, and Lavoisier enacts, if only in imagination, the collective adjudication that Brannigan summarizes.

The list of common-sense criteria of intelligibility that Brannigan puts forward has puzzling connotations, given the contingent status of what counts as a discovery. However, as Austin (1962, 109–121) points out, these connotations apply more broadly: one cannot specify in advance what it is about a particular action that may or may not turn out to succeed in "persuading," "alarming," or "humiliating" others. The "achievement" or consequence that eventuates from what the parties to the action succeed in doing (or are credited with or blamed for doing) are *not* themselves actions, even when they intend such outcomes. Discovering X, or discovering *that* X is or is not the case, is an *achievement* marked by its consequence, but not an intentional or unintentional *activity* or *process* that can be described separately from the consequence.

Brannigan's account was made to order for sociologists of science who had been pressing the case for "constructivism" (sometimes dubbed "constructionism") in the sociology of scientific knowledge. His account lent support for the idea that scientific discovery is a retrospectively conferred achievement in a community of scientists, so that the most that can be said about what an individual or group *originally did* is that they put forward a discovery claim about a *possible* discovery whose fate was contingent upon what key members of the

they came upon it.

12 See MacKenzie (1999) on the dilemma for mathematicians created by a partially mechanized proof.

relevant community or network retrospectively made of it. Further, Brannigan's account supported the emphasis in the sociology of scientific knowledge on *social* agency and contingency by asserting that no cognitive or procedural account could distinguish between a claimed discovery and a ratified discovery.

CASE-BY-CASE COMPLICATIONS

Two well-known cases illustrate complications and controversies that arise in accounts of discovery: the discovery of the double-helical structure of the DNA molecule in the early 1950s, and the discovery of pulsars a decade later. In both cases, accounts of these discoveries told and re-told by participants and historical analysts (including participants *as* historical analysts) used criteria of intelligibility to narrate key events and allocate and re-allocate credit for discovery. These examples show how criteria of intelligibility intrinsic to the grammar of discovery themselves become contingent polemical tools.

The discovery of the double-helical structure of the DNA molecule is a well-known case, not only because of its significance for molecular biology, but also because J. D. Watson's ([1968] 1980) personal account of his and Frances Crick's pursuit of the structure of that molecule at Cavendish Laboratory in Cambridge in the early 1950s successfully promulgated the story of that discovery to a large readership. Watson's story, and many of the public statements he made over the ensuing half-century, also generated public controversy in and well beyond the scientific community about *how* he told the story and how he and Crick conducted themselves during some of the episodes *in* the story. Hanson's taxonomy can be used both to characterize the discovery attributed to Watson and Crick, as well as the unsuccessful or partially successful pursuit that Watson attributes to others (particularly Rosalind Franklin, James Wilkins, and Linus Pauling). As Watson tells it, he and Crick *puzzled out* a solution by assuming that the molecule would have a helical structure and attempting to build a three-dimensional model of that structure that was consistent with evidence of its chemical composition, especially Rosalind Franklin's x-ray diffraction images of two crystalline forms of the molecule. According to Watson, interpersonal distrust and interactional difficulties between Wilkins and Franklin at Kings College, London prevented them from putting together theory and evidence, and Pauling—who had the theoretical wherewithal—was blocked from first-hand inspection of the evidence in London by the U.S. State Department.[13] Watson attributed the conflict between Wilkins and Franklin to their clashing views of Franklin's place in Wilkins' laboratory group; where she viewed herself as an independent expert in x-ray-crystallography and he considered her to be a subordinate whose job was to supply data for his theoretical reconstructions of the structure of the molecule. Watson describes his own difficulties with getting Franklin's cooperation and portrays her as a hostile and narrow-minded "feminist" whose unique technical skill was not matched with the ne-

13 During the early years of the Cold War, Pauling's anti-war activities were flagged by the State Department, which refused his application for a passport to travel to London for a conference.

cessary insight to see that the beautiful images she produced provided clear evidence of a helical structure.

As Watson tells the story, and as others correct and criticize his telling,[14] the path to the discovery was fraught with social-interactional as well as practical and theoretical challenges. Possible connections between theory, evidence, and model were enabled, distorted, and blocked by various contingencies and all-too-human tensions between friendship and rivalry, trust and distrust, competition and cooperation, and secrecy and open communication.

In one of the key episodes in Watson's story, he and Crick constructed a triple-helical model of the DNA molecule that they were convinced was correct, but which quickly unraveled (almost literally) after they invited Wilkins and Franklin to inspect it. As Watson recounts, Franklin immediately found gross and irreparable mistakes in the model, which were based on Watson's incomplete and mistaken recollections of crystal radiographic evidence Franklin had shown during a presentation he attended. The episode not only illustrated how the eureka moment, which Watson says he and Crick experienced when they assembled their later-to-be-dismantled-model, had no intrinsic connection to discovery. It also illustrated Wittgenstein's line (1969, Sec. 12) that, "'I know' seems to describe a state of affairs which guarantees what is known, guarantees it as a fact. One always forgets the expression 'I thought I knew.'" Paraphrased for this case: "To say 'Eureka!' seems to characterize a state of affairs that guarantees having made a discovery. One always forgets the expression, 'We thought we made a discovery.'"

Watson described a later eureka moment he experienced when discovering the way cardboard mock-ups of the four nucleotide bases paired together neatly, thus demonstrating how a "backbone" of complementary nucleotides could fit together to form a double-helical structure with phosphate and sugar molecules arranged on the outside. That moment was canonized in his book, and decades later in a docudrama in which he was portrayed by a rapturous Jeff Goldblum as inspiring music played in the background.[15]

After Watson and Crick assembled their model, using metallic parts prepared in the machine shop, rather than cardboard cut-outs, they again summoned Franklin and Wilkins to witness their achievement, and this time they were confirmed. They quickly published their announcement in *Nature*, along with articles by Wilkins, Franklin, and their collaborators (Watson and Crick 1953; Wilkins, Stokes, and Wilson 1953; Franklin and Gosling 1953). In 1962 Watson, Crick, and Wilkins received the Nobel Prize in Chemistry. Franklin's untimely death in 1958 from ovarian cancer excluded her from consideration for the prize (only living scientists were eligible), though it is uncertain that she would have received it had she lived.

Even if one supposes, as we do, that the double-helical model was correct, while the triple-helical one was not, to count as a discovery the correct model had to be established as

14 For alternative views, see the commentaries in the expanded Norton edition (Watson 1980), and books favorable to Franklin by Sayre (1975) and Maddox (2003).

15 The 1987 docudrama was directed by Mick Jackson for the BBC Horizon Series *Life Story: The Race for the Double Helix*.

an unprecedented accomplishment rather than a replication of what others had done before, or as an incremental step that was parasitic on earlier accomplishments by others.

A decade-and-a-half after Watson and Crick announced their discovery, another notable discovery—also attributed to researchers at Cavendish Laboratory, though in the field of radio astronomy—was announced in *Nature* (Hewish et al. 1968). The narrative of the discovery also gave rise to lasting controversies over matters of authorship, sequence, and credit.

In an article based on his dissertation research, Steve Woolgar (1976) examined the "intellectual history" of the pulsar discovery, highlighting discrepancies among what he characterized as first-order announcements, publications, second-order review articles, personal recollections, and third-hand accounts by scholars and journalists (*ibid*., 396). Woolgar interviewed some of the principal parties to the discovery story. Especially interesting was an account of the discovery given by Jocelyn Bell, who Woolgar interviewed at length. He characterizes her as "a research student centrally involved in the discovery" (*ibid*., 401), and quotes her recollection of the initial observations: "As far as I remember, when I was analyzing that bit of paper [a quarter-inch sector of a 400 foot chart recording from a day's sky survey] I sort of wrote down 'curious bit of scruff at 19.19'—that's the time—and likewise the subsequent times it cropped up" (*ibid*.).

According to Bell in the interview, she showed the "curious bit of scruff" to Hewish, and while it did not seem related to the sort of scintillation they were surveying at the time, the research team investigated it further. They tried different ways of recording data from the apparent source and attempted to screen out possible sources of artefact. The signals (if "signals" from an astrophysical "source" is what they were) went away and came back, but some recordings showed "pulses" spaced at intervals of one and a third seconds. Bell told Woolgar that the research team initially concluded that it must have been human- or alien-made (Woolgar 1976, 403) and added that it exhibited "a suspiciously round figure" (*ibid*., 404). The researchers performed various instrumental checks to eliminate possible sources of artefact, and meanwhile other, similar data were detected when Bell inspected records from the continuing sky survey.

Following months of investigation, Hewish, Bell, and three other members of the research team announced their finding of "pulsing radio sources" in *Nature* (Hewish et al. 1968).[16] The article focused on the properties of the signal, as well as the equipment and procedures for recording it and screening out sources of noise. It also presented tentative suggestions about possible sources; neutron stars and white dwarfs being the main candidates.

In his comparison of discovery accounts by different parties, Woolgar contrasts Bell's account in the interview to a "gloss" that Hewish (1969) presented in a published article. In response to a "probing question" of Woolgar's, Bell recounted that, rather than making a dis-

16 See Pinch (1985) on the conditions under which researchers attempting to detect neutrinos would publicly announce "proximal" characterizations of data or "distal" accounts of substantive source objects. Latour and Woolgar (1979) also provide a scheme of "modalities" to qualify their characterizations of findings as more or less definite.

crete observation, she noticed a "cumulative effect" from reviewing the records after numerous rounds of survey. Hewish (1969) gave a more compact account of an initial moment: "Soon after we started in August 1967, we notice a rather peculiar looking signal." Woolgar observes that Hewish's gloss collapses what Bell in the interview describes as a gradual, retrospective realization from repeated reviews of the chart recordings, and the mention of "signal" also simplifies what also was described as a possible effect of interference.[17]

At the close of his article, Woolgar mentions criticisms of Hewish that focused on the delay of more than half a year between "the discovery" and its public announcement a few days prior to publication. The criticisms had Mertonian tones (violating the "norms" of disinterestedness and communalism that Merton (1942) codified), accusing Hewish's team of maintaining secrecy as they investigated the phenomenon in an effort to maximize the credit they would get if and when they announced it. Hewish responded to "the Americans" to whom he attributed the criticism by saying that "the first pulse was recorded on November 28th" 1967, not in September as one critic claimed.

Woolgar suggests that the debate about the timing of the announcement arose, at least in part, from a notion of discovery as an instantaneous moment, rather than an extended process. The discrepant accounts of the date of discovery may seem "academic" in this case, but the criticism involved the very idea of *what* was discovered and *when* it was discovered. It also involved, as we shall see, the issue of *who* deserved credit for it.

For years after the announcement of the discovery, and continuing after the awarding of the Nobel Prize in Physics to Hewish in 1974, a controversy lingered about the fact that Bell did not receive equal recognition for her role in the discovery. Superficially understood, along the lines of Kuhn's account of the discovery of oxygen, Bell's initial noticing of "scruff" was analogous to Priestly and Scheele's non-recognition or misrecognition of what was in the vial they held in their hands. Accordingly, the initial noticing of "scruff" was not a discovery until it was further investigated to eliminate alternative possibilities that the participants glossed as LGM for "little green men," and not until after it was announced, confirmed, and consensually established in the research community. *If* it were assumed that Hewish had the theoretical insight, analytical skill, and mathematical training to eliminate alternative explanations and explore possibilities consistent with current astrophysical theory, then by Kuhn's lights the Nobel Prize was appropriately awarded to him.

Similarly, Watson's account of Rosalind Franklin, while praising her technical skill, demeaned her for lacking the theoretical insight necessary to *see* her radiographic image of crystallized DNA *as* evidence of a helical molecule. Following Kuhn, Franklin also would not count as a discoverer if all she did was develop an image without being able to recognize its theoretical significance. However, this restricted account of *what* she did and what she knew about the images she had "in her hands" was contested. The contestation over just who

17 Hewish et al. (1968, 709) give a more expansive account in the first paragraph of the paper: "Soon after the instrument was brought into operation it was noticed that signals which appeared at first to be weak sporadic interference were repeatedly observed at a fixed declination and right ascension; this result showed that the source could not be terrestrial in origin."

made the discovery, what they discovered, and how they did so, continues to the present, and the adjudication of the terms of discovery has broken out from specialized fields and awards committees to include historians and journalists (Cobb and Comfort 2023; Anthes 2023).

In both cases, a convergence of history of science with history as told *in* science and *by* scientists gradually established a revision of the prior historical accounts of what Franklin and Bell (later Dame Jocelyn Bell Burnell) achieved, and both received more recognition than they were initially given. In Franklin's case, former colleagues, scholars, and science journalists emphasized that she had far more insight into the structure of the molecule than Watson gave her credit for, and they further cast aspersions on Watson's character for using her data without giving her due credit. In Bell's case, her work on the pulsar project was later described as far more extensive than simply making an initial noticing of anomalous data. Among other things, she is credited with setting up and maintaining the equipment, painstakingly analyzing the sky survey data, and discovering several other comparable sources. Independently of the pulsar research, she later developed a distinguished career of her own, and later obtained many honors.

Although Franklin did not live to see the revision/correction of her place in history, Bell was able to accept the accolades and recognition accorded to her in the decades that followed. One small example of the current view of the co-discovery is in an abstract of an obituary of Hewish in the *Bulletin of the American Astronomical Society* (Longair 2021). According to the abstract of the obituary: "Hewish was a pioneering radio astronomer. His research student Jocelyn Bell detected a strange scintillating radio source that they subsequently showed was the first identified pulsar. Hewish was awarded the 1974 Nobel Prize in Physics for his 'decisive role' in the discovery." The mention of Bell, what she "detected," the account of what "they" (both she and Hewish) showed, and the quotation marks around Hewish's "decisive role" qualify the credit Hewish received, even while commemorating him.[18]

THE OPTICALLY DISCOVERED PULSAR AND DISCOVERING *WORK*

In their article in *Nature*, Hewish et al. mention that the "positional accuracy so far obtained does not permit any serious attempt at optical identification" and that in "the absence of further data, only the most tentative suggestion to account for these remarkable sources can be made" (1968, 712). Hewish et al. then say that their analysis thus far suggests "an origin in terms of the pulsation of an entire star, rather than some more localized disturbance in a stellar atmosphere," and they add that "it is interesting to note that it has been suggested [citing sources in footnotes] that the radial pulsation of neutron stars may play an important part in the history of supernovae and supernova remnants" (*ibid*.).

Almost exactly a year later, another publication in *Nature* announced a successful attempt at optical identification (Cocke, Disney, and Taylor 1969): "Discovery of Optical Sig-

18 While easily read as "scare quotes" injecting a note of irony, in light of Bell's having been slighted, the quotation marks around "decisive role" in Longacre's obituary are from the Nobel Prize press release announcing the prize given jointly to Hewish and Martin Ryle for their contributions to Astrophysics.

nals from Pulsar *NP* 0532." By that time, the name "pulsar" was established and a growing list of them had been identified, including a very recently identified one designated as *NP* 0532, detected in the central region of the Crab Nebula, a previously established supernova remnant. Given the suggestion by Hewish et al. (1968, 712) that neutron stars were possible sources of the pulsating radio signals, and a further suggestion that a highly dense neutron star would result from the collapse of a red-giant star, Cocke, Disney, and Taylor pursued the possibility of locating the source of the radio pulsar by focusing an optical telescope on Baade's star, the "South Proceeding Star" in a binary star system at the center of the Crab Nebula. They did not look directly through an eyepiece of the telescope, but instead ran the optical signal collected by the telescope through a photomultiplier connected to a Computer of Average Transients (CAT), which they set at the measured period of the pulsar (approximately 33 times a second). What they viewed in the observatory was a cathode ray screen connected to the CAT from which they viewed blurry dots rising on the screen to cumulatively form a jagged pattern. If the signal was "pulsed" at a regular interval and the accumulator was set at the correct frequency, a peak should build up on the screen with the accumulation of photons.

By their own account, Cocke and Disney were not experienced observers, but they were able to secure several days of observation with a 36-inch optical telescope[19] at Steward Observatory on Kitt Peak, Arizona. (Taylor, the third collaborator on the project, had constructed the CAT apparatus, but was not present at the observatory at the time.) With technical help from the night assistant Robert McCallister, they set the telescope on the South Proceeding Star in the central region of the Crab Nebula, which had previously been suggested to be a neutron star resulting from the implosion that ejected the gas forming the nebula. They ran the output from the optical telescope through the CAT, which was set at the measured radio frequency of the pulsar. During the initial runs over a two-day period, they failed to get a positive result, but after making some corrections of the frequency on the third day, Cocke and Disney observed the buildup of a peak on the oscilloscope screen of the CAT. After further checks made by setting the telescope away from the possible source star, and then setting it back on the star, but with the CAT set at a different frequency, the researchers tentatively confirmed that the peak accumulated only when the telescope was pointed at the candidate source and the electronic apparatus was set at the reported radio frequency of the pulsar.

The optical pulsation was confirmed at other observatories soon after Cocke and his colleagues sent out the coordinates and frequency on a telegram relayed to an international network of observatories. The *Nature* publication followed within weeks. In grammatical terms, it was a discovery *that* a radio pulsar frequency could be correlated with an optical signal from the coordinates of a visible star. Further, this observation was consistent with the possibility *that* the source was an extraordinarily dense neutron star at the center of a supernova remnant, whose rapid rotation produced the signal detected on earth. The observation

19 This was a small telescope by contemporary professional standards, which would have reduced the competition for getting consecutive nights on it.

that this pulsar (and a few others of the many radio pulsars that were identified in the decades that followed) was visible in the optical range held implications for analysis of the energetic properties of pulsars. In terms of its historical significance, it appears to have taken a subsidiary place to the discovery attributed to Hewish (and, of course, Bell).

This is not the end of the story, however. When Cocke and Disney were performing their observations, the night assistant McCallister was recording the data on a multi-track audiotape. According to Cocke,[20] McCallister would announce each observation and then record the incoming photon data outputted by the electronics on audiotape, and he inadvertently failed to disconnect the jack of the recorder from the start of Observation Number 18 through a consecutive sequence of numbered observations until the tape ran out during Observation Number 23. Consequently, the tape not only recorded McCallister's announcement of a series of observations, it also recorded Cocke's and Disney's voices as they observed the "pulse" build up, expressed excitement and voiced doubts, proposed and enacted subsequent modifications of the observation to check on the result and, after doing so, celebrated the possibility that they had made a discovery. A copy of the recording was given to the Center for History and Philosophy of Physics at the American Institute of Physics (AIP) and was later used as documentation for a radio broadcast titled, "Moments of Discovery."[21]

More than a decade after it was recorded, Harold Garfinkel was told about the tape and received a copy from the AIP. Together with Eric Livingston, who was conducting dissertation research on the practical achievement of mathematical proofs (see Livingston 1986), and Michael Lynch, who was a postdoctoral fellow at UCLA after completing a dissertation on a neuroscience laboratory (see Lynch 1985), Garfinkel prepared a plenary address and a journal article on "the work of a discovering science," in which the recording was featured (Garfinkel, Lynch, and Livingston 1981). Regardless of how significant the discovery turned out to be for astronomy, the fact that the astronomers' voices were recorded during a key stretch of what Garfinkel and his colleagues called their "night's work," made it of interest for ethnomethodological research on discovering work. The interest had to do with the fact that the tape provided a detailed "real-time" record of the voices of Cocke, Disney, and McCallister while they watched the shape of the pulse build up on the screen, without yet knowing whether they were witnessing what they had hoped to see or would soon have their hopes dashed when it turned out to be an artefact.

Garfinkel, Lynch, and Livingston (1981) developed an analysis of the "work of a discovering science" in which Cocke and Disney addressed the "Optically Discovered Pulsar" (ODP): the as-yet unresolved object of the night's work; the *possible* pulsar (and possible artefact) developing on the screen; a phenomenal subject of hopes and doubts; a recalcitrant subject of calculations and manipulations of the equipment. Garfinkel, Lynch, and Livingston distinguished the ODP from the "Independent Galilean Pulsar" (IGP)[22] that was announced at the end of a series of observations and checks on the instrumentation, and was

20 John Cocke, interviewed by Michael Lynch in 1980.

21 An illustrated transcript of the AIP radio broadcast, "Moments of Discovery: A Pulsar Discovery," is available at: https://history.aip.org/exhibits/mod/pulsar/pulsar1/pulsar.pdf.

identified with coordinates assigned to the location of Baade's Star, various electromagnetic properties, and a period of fluctuation. The reference to the "Galilean" object was a way of alluding to a mathematically characterized, objective, astrophysical source independent of the conditions of observation. What the tape documented was a "first time through" encounter with the ODP from which Cocke and his colleagues analytically and mechanically derived the abstract properties of the IGP they announced in their report. Instead of consigning the ODP to the status of a temporary precondition for the discovery of a real astrophysical object (the IGP), whose further properties and origins awaited investigations by competent astronomers, Garfinkel, Lynch, and Livingston were interested in characterizing the local history of the ODP as *their* ethnomethodological phenomenon of interest.

Further elaboration on the pulsar study is given in Garfinkel, Lynch, and Livingston (1981), Garfinkel (2022, part II), and Lynch, Livingston, and Garfinkel (1983). Koschmann and Zemel (2009), and Hoeppe (2023) provide comments on and cogent criticisms of the pulsar study, and Bovet, Carlin, and Sormani (2011) use an analysis of the "academic influence" of the pulsar paper as an occasion for examining the peculiarities of bibliometric assessment. Of interest for the present paper, however, is a criticism that was briefly stated in an appendix of an article by H.M. Collins (1983, 104–105). After praising aspects of the pulsar paper, Collins points to what he considers a fundamentally misleading feature of the way Garfinkel, Lynch, and Livingston (1981) present their preliminary analysis of the Steward Observatory recording.[23] In a commentary reminiscent of Brannigan's (1981) account, Collins uses a counterfactual formulation to suggest that Garfinkel, Lynch, and Livingston (GLL) had an insufficient basis for recovering the work of a *discovery*:

> [T]he contents of the tape recording and *GLL's analysis of it*—indeed the whole of GLL's paper—would be precisely the same even if what Cocke and Disney had "discovered" through their evening of shop work was something that was not an optical pulsar. Suppose, for example, that it later turned out that Cocke and Disney had been looking at an artefact—the result of a fault in their oscilloscope—and that this was the scientific consensus. Under these circumstances everything that GLL wrote about Cocke and Disney's work would remain unchanged and would be equally valid! (Collins 1983, 105)

22 Garfinkel, Lynch, and Livingston (1981) treat the IGP as a Galilean object, in line with Husserl's (1970) inversion of the Galilean primacy accorded to a mathematically analyzed physical system that gives rise to sensory qualities. Vernacular accounts of the IGP used by astronomers include the term "source" as a reference to astrophysical origin of manifest signals.

23 Garfinkel, Lynch, and Livingston (1981) refer to ongoing research that aimed to expand upon their brief and compressed account, but no further work was published from the project. Further research had been initiated but was not continued after 1981. Garfinkel conducted phone interviews with Taylor, and entertained hopes of reconstructing the equipment he had built in order to create a mock-up of the praxeological conditions of the original observation, but he was informed that the equipment had long ago been dismantled, and that rebuilding it would be an expensive undertaking. Other contingencies also arose which ended the collaboration between Garfinkel and Lynch.

Collins concludes his discussion of this problem by saying about Cocke and Disney's work: "[T]o know what it is about their work that makes them scientists who *are making* a great discovery,[24] one needs to look elsewhere. What it is that made it that they were making a great discovery is to be found outside their night's work" (*ibid*.).

Collins (1983, 110, n. 31) adds that the recording that GLL used as a basis for their article was a fragmentary and incomplete document of Cocke and Disney's "night's work" (not to speak of their several nights of work and their preparations for those nights). There is no disagreement about this point. However, on the matter of *discovery*, if we take it that the topic of the pulsar paper is, as Garfinkel, Lynch, and Livingston (1981) put it, "the work of a discovering science," then Collins' counterfactual question—"What if it turned out to be an artefact?"—is beside the point.[25] The point was not to *explain* how the discovery was made by examining an audio recording of the astronomers' voices during the "moment of discovery." As Collins puts it, "[w]e would not, after all, want to make too much of a tape-recording of the word 'Eureka'" (1983, 110). However, would we even want to speak of a "process" of discovery documented by the recorded voices? It is not as though there is more to the process than an audiotape can "capture"; instead, as we have argued, "discovery" is *not a process*.

To underline this point, we need to return to the grammar of the word "discover" as a verb implicated by the *achievement* of a "discovery." The difference between what Garfinkel, Lynch, and Livingston (1981) described and what Collins (1983) argued that GLL could not have described with the materials they had can be presented as a matter of *accent*. Whereas Collins argued that GLL purported to, but could not, account for the work of a scientific *discovery*, GLL presented an account of the *work* of a discovering science. In other words, GLL provided an account of discovering *work* (a course of work oriented to the *possibility* of making a discovery), which they exhibited with the materials they examined, without regard to the *ultimate* validity or significance of what that work achieved. Cocke, Disney and Taylor's (1969) discovery announcement was quickly confirmed by other observatories, and it has not been overturned in the subsequent half-century. However, this does not entirely negate all possibility that it could be overturned: Kuhn, among others, provides numerous historical examples of consensual facts and theories associated with paradigms

24 In a review of Garfinkel (2022), Hoeppe (2023) points out that the pulsar discovery was less than a "great discovery" which Garfinkel's account of it seems to suggest. Cocke, Disney, and Taylor's research may have helped to settle questions raised in the wake of the Hewish et al. (1968) announcement about the properties of the signal and the possible astrophysical source, but aside from the documentation provided by the recording, it was not at the time, and certainly not now, granted the significance of the initial characterization of pulsars.

25 Naively understood, mention of a "counterfactual" argument implies that it lacks empirical grounding, but this would ignore that counterfactuals are routinely used for exploring *possibilities* in both ordinary and scientific efforts to investigate empirical phenomena. The present characterization of Collins' argument as counterfactual is not meant as a criticism. Indeed, the possibility of artefact was explicitly addressed by the voices recorded in the pulsar tape.

that were later overthrown, such as geocentric theories of what we now call the solar system. To base a description of discovering work on the *ultimate* validity of a claimed discovery would be akin to an infinite regress, except that it would be more of an infinite "pregress" awaiting ultimate validation (or an absence of falsification) in an indefinite future.

On the question of whether discovery is a "process," Coulter and Parsons quote a passage from Hanson's imagined and anachronistic example of Tycho and Kepler viewing a sunrise:

> The physical processes involved when Kepler and Tycho watch the dawn are worth noting. Identical photons are emitted from the sun; these traverse solar space, and our atmosphere. The two astronomers have normal vision; hence these photons pass through the cornea, aqueous humor, iris, lens and vitreous body of their eyes in the same way. Finally, their retinas are affected. Similar electro-chemical changes occur in their selenium cells. The same configuration is etched on Kepler's retina as on Tycho's. (Hanson 1961, 6; quoted in Coulter and Parsons 1990: 254)

According to Hanson's story, despite the (imagined) equivalence in physical and physiological conditions of observation, Kepler *saw* situated visual effects of the earth's rotation on its axis relative to a fixed sun, whereas Tycho *saw* the sun rising as it revolved around a fixed earth. Collins' criticism can be compared with Hanson's example, with the complication that Cocke and Disney endeavored to "see" a pulsar by means of the mediating optical and electronic instruments through which photons passed, were collected and analyzed, and then visualized graphically as possible evidence of a pulsar. Moreover, the route taken by the photons would materially differ if other astronomers, using different complexes of equipment, concluded that an artefact was responsible for what Cocke and Disney *thought* they had observed. Cocke and Disney did conduct checks against possible artefacts before announcing that they had discovered evidence of a source whose location and optical light curve correlated with pulsar *NP* 0532. Their checks could have overlooked a crucial contingency, but Collins' argument serves to nullify *whatever* Cocke and Disney did in pursuing and claiming the discovery: "What it is that made it that they were making a great discovery is to be found outside their night's work" (Collins 1983, 105). However, the lesson from Coulter and Parsons is that *no* causal process "inside" *or* "outside" the night's work "made it that they were making" a discovery.

THE DISAPPEARANCE OF DISCOVERY AS A THEME IN SCIENCE & TECHNOLOGY STUDIES

Coulter and Parsons (1990, 255) argue against what they call "cognitive constructivism," which they attribute to Hanson's two-step account of (1) photons from the sun, filtered through a visual apparatus and impressing an image on the retina, and (2) an interpretation of the incoming information in accordance with the perceiver's theoretical presuppositions.[26] This two-step conception of observation became entrenched in the sociology of scientific knowledge, of which Collins was a major proponent (also see the account of perception in Barnes, Bloor, and Henry 1996, chapter 1). It also was prevalent in related social constructivist conceptions which proliferated within and well beyond STS (see Hacking's (1999) survey and criticism of the proliferating use of "the construction of x" in the humanities and human sciences). In a *social* constructivist account, the agency that translates incoming sense data into theory-laden inferences is not internal to the mind of the individual, but it is bound up with an expansive network in a scientific community.

Following the height of popularity of social constructivism in the 1980s and 1990s, interest in the topic of discovery began to wane. One reason for this was the demise of the "heroic model" of discovery that emphasizes extraordinary individual feats and their "eureka" moments (see Schaffer 1994, 13). Constructionists tended to be critical of individualist conceptions of innovation that assigned credit to leading (almost always male) figures rather than their anonymous collaborators. To the extent that they employed ethnographic methods, some constructivists also emphasized more mundane, day-to-day, and contingent features of scientific practice, rather than spectacular achievements. The constructivist orientation also downplayed the very idea of "discovery" as a disclosure of novel natural objects and/or principles, and increasingly focused "beyond" or "outside" the work of laboratories.

Various other reasons for the declining interest in discovery in STS, and in the sciences themselves, also can be entertained. STS expanded in recent decades, both in numbers of members in the major professional societies such as the Society for Social Studies of Science (4S), and in the subject matter covered, which now includes technology, biomedicine, science policy, industrial science, and the human sciences. At the same time, as STS has become more autonomous as a field and present-centered in its historical scope, there has been a decline in its connections with history and philosophy of science, both institutionally and intellectually. Research in STS on the "discovering sciences"[27] of physics and chemistry has grown scarce, as a glance through paper titles in recent programs of the annual 4S meetings can confirm. Not only is there less interest in so-called "basic" science, the very idea of "pure" research is widely regarded as dubious (see Lynch 2014).

While the topic of scientific discovery became less salient in STS, it also has become less salient in the sciences themselves. The organization of research increasingly involves collective projects, sometimes assigned to research teams numbering in the hundreds. The sponsorship and management of research, as well as the credit for innovation, is tied in with corpor-

26 Coulter and Parsons (1990) point out that, when Hanson invokes Wittgenstein's (1958) distinction between the logical grammar of "seeing" and "seeing-as," he collapses the distinction by arguing that "the logic of 'seeing as' seems to illuminate the general perceptual case" (Hanson 1961, 19). The conception of observation as a physical process overlayed by a cognitive process guided by theory turns "seeing" into an interpretive process, contrary to Wittgenstein's treatment of logical grammar.

27 "Discovering sciences" is a category Garfinkel (2022) uses for fields in which making discoveries is both possible and a strong part of the agenda. He does not draw a hard boundary between which fields he places in that category, though he explicitly excludes the social sciences.

ate and government bureaucracies, rather than single individuals and small-scale collaborations. Given the increased emphasis on intellectual property, "innovation" has become the order of the day, and what otherwise might have been dubbed discovery is presented and litigated as invention. In the terminology of US Patent Law, a patent can be awarded for a "composition of matter," but not a "product of nature."[28] This distinction approximates the one between, respectively, invention and discovery. Accordingly, while a "product of nature" can, of course, be possessed, a person or corporate entity cannot claim control over all commercial transactions involving a *type* of naturally occurring mineral, species of plant, or "native" genetic sequence. In the 20th century, decisions by patent examiners, and courts of law in cases of litigation, were trending toward treating an ever-broader range of living organisms, genetic sequences, and organic chemicals as products of "manufacture."[29] This coincided with a trend toward the privatization of research, and efforts to nominalize the products of such research as proprietary "compositions of matter."

The criteria for awarding patents are roughly similar to Brannigan's common sense criteria of intelligibility for the attribution of discoveries. According to the US Patent and Trademark Office, to qualify for a patent, a claimed invention must be clearly described, and must be *novel* (not precedented by a prior claim), *non-obvious*, and potentially capable of *use* (to have specifiable *possibility* of being realized, implemented, and useful). Litigation on such matters not only is important for ongoing efforts to expand and resist the commercialization of scientific research and its material "products," it also provides material for *describing* problematic instances in which "common sense" criteria of attribution are themselves formally contested under the rubrics of "common law." The very idea that scientific "discovery" can be reformulated as "construction" is at the heart of such contestation. Given the many ways the prospect of patentability has penetrated the organization and orientation of research in many fields, it can be said that the discovery-construction distinction is *internal* to science as well as law. This conception of "internal" differs from Collins' use of an internal-external distinction: it does not mean "inside" the walls of a laboratory; instead, it means reflexively part of conceptual usage on occasions of disputation—i.e., *the work*—of law and science.

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28 The distinction not only separates nature from culture; more importantly, it treats nature's *categorical* "products" as part of a commons that is protected from private monopoly. While a private party can own an expanse of forest, it cannot claim a patent on wood derived from a species of tree; at least it could not until techniques of genetic modification complicated the issue.

29 In the US Supreme Court case of *Association for Molecular Pathology v. Myriad Genetics, Inc.*, 569 U.S. 576 (2013), the Court unanimously ruled against Myriad's patents on research and clinical applications using genetic sequences extracted from two genomic sites associated with a proportion of breast and cervical cancers. The implications of this ruling for the trend towards the patentability of what previously had been categorized as "products of nature" have yet to be determined. The litigation in the case at the District Court level included "declarations" by an STS scholar, Shobita Parthasarathy and an historian of science, Myles Jackson, who had conducted research the subject of patenting and patentability in molecular genetics (Parthasarathy, 2007; Jackson, 2015).

In the current intellectual property regime, it is imaginable that the late 18th century case of the purification and characterization of oxygen that Kuhn describes and virtually adjudicates would today be adjudicated as a patent dispute litigated in a court of law. If this seems far-fetched, consider a case in the US that was resolved after 12 years of litigation, in which Bell Labs was awarded patent rights to yttrium barium copper oxide (YBCO), described as "the first superconductor with a transition temperature exceeding the boiling point of liquid nitrogen" (Feder 2000). Described as both a "discovery" and an "invention," this composition of matter involved several corporate and public sector claimants, and the claims covered different properties of an unknown or not-completely-known substance, demonstration of its superconducting properties, analysis with x-ray diffraction technology, and characterization of the molecular constituents. A famous precedent from the early 20th century concerned the purification of adrenaline, a naturally occurring hormone extracted from adrenal glands from slaughtered animals and used for human medical treatments.[30]

DISCUSSION AND CONCLUSION

"Discovery" is an ordinary word that takes on specialized significance in connection with scientific research where it marks an original achievement in two senses: (1) an unprecedented creative achievement attributed to a person or collaborating group, and (2) a novel thing discovered (a previously unknown natural law or principle, a substance or species that heretofore had not been characterized). An aura of mystery has been associated with discovery in both popular and philosophical accounts, as it is recognized that discoveries do not simply follow rules or recipes and sometimes appear to come from nowhere. When a past discovery is formulated as a step-by-step procedure and other researchers follow the procedure and get the same result, the result is a replication and not a discovery. It might seem as though "discovery" is an aura attached to an original scientific achievement, as with an original work of art (Benjamin [1935] 1969), so that no copy, however exact, can retain that aura. The suggestion in this paper, however, is less mysterious: if it is the case that a scientific discovery is a (possible but contingent) *consequence* of laboratory work, it cannot be decomposed into any set of constitutive procedural steps, routines or performances.

This is not to say that researchers are indifferent to the making of discoveries. In the case of Watson and Crick attempting to build a model of DNA, or Cocke, Disney and Taylor setting up a series of observations with equipment designed to discover if a radio pulsar also emits optical radiation, it is clear that their activities, with more or less definite aims and prospects, were *oriented to* making a discovery. The retrospective accounts of the Hewish/Bell discovery were more complicated, as the sky survey project was not designed as a pursuit of a possible source of what Bell called "a bit of scruff," according to her account of the initial noticing that touched off a lengthy pursuit.

30 *Parke-Davis & Co. v. H. K. Mulford Co. Circuit Court*, S.D. NY, 189 F. 95: 1911 U.S. App. LEXUS 5244 (1911).

Although it is fair to say that Cocke, Disney, and Taylor set out to make the discovery they succeeded in making, their attempt to do so did not "make it that they made" a discovery. However, the *possibility* of making a discovery animated their work, and that *work* can be described, even if one concludes that Garfinkel, Lynch, and Livingston failed to describe it adequately. Garfinkel (2022) articulates an ambition for research on "the work of a discovering science" that would require a competency with the practices examined (which he and his co-authors did not claim in the case of the pulsar study), such that the analysis of the discovering work would exhibit constituents of the work that remain unmentioned in published reports and oral histories of the discovery, and that such exhibition would contribute to the science studied (see also Sormani 2014). Garfinkel did not realize this ambition, despite living to an advanced age, but he left an agenda for others to pursue.

Aside from Garfinkel's (2022) dissatisfactions with the pulsar project, as well as with "analytic ethnographies" of laboratory science (e.g., Lynch 1985), his general conception of discovering work can itself be a source of dissatisfaction for the way it leaves unclear just what counts as a "discovering science," what distinguishes such a science from others, and just what would count as an adequate ethnomethodological description of the constitutive "discovering work."

Much of the work of the sciences is *not* explicitly oriented to making discoveries. Shapin (2007, 183) uses the expression "sciences of the particular" to describe the vast amount of research that pursues agendas set by government, corporate, and other administrations of research. When pursuing such agendas, researchers endeavor to work out standards, develop and refine measures, inform regulatory agencies on levels of toxicity for specific substances, and so on. One can suppose that the sequences of methodic action, the equipment, and the shoptalk performed in such circumstances would be much like that of a project that explicitly pursues notable discoveries. Contrary to Collins' (1983) suggestion, however, "making it that" a discovery is made is not done by a discrete "outside" agency that is added to mundane practical work; instead, a description of such work in a case where a discovery *is* credited, for all practical, temporal, and communal purposes, can provide an exquisitely detailed understanding, albeit not an explanation, of the praxiological conditions of possibility for the discovery.

Gathering from the brief remarks in this paper about intellectual property litigation, another avenue for ethnomethodological research remains open, which Garfinkel (2002, 181–182) alludes to under the rubric of "perspicuous settings." This involves a respecification of what otherwise might be called theoretical or methodological concepts and distinctions by describing the use of such "concepts" in real-worldly settings. In effect, the investigation is receptive to a tutorial on the lived work performed in instances of what the distinction or concept glosses. Garfinkel (2024, 28ff.) ascribed the inspiration for this procedure to Harvey Sacks, who proposed an unusual way to explicate a legal distinction between "possessables" (things that are publicly recognized as what "anyone" can take possession of) and "possessitives" (things that evidently belong to somebody). As Garfinkel tells it, Sacks proposed that, instead of explicating the distinction through research in a law library, he would aim to learn of its use through ethnographic work with members of a police department whose day's work involved identifying abandoned (as opposed to legally owned and possibly stolen) vehicles.[31]

The distinction in patent law between "product of nature" and "composition of matter," is akin to the possessable-possessitive distinction. In terms of intellectual property law, a "product of nature" is part of the commons: it pre-exists the sorts of proprietary extraction, modification, and combination with other materials that make up a "composition of matter" eligible for patenting. Investigating cases of litigation in which this distinction comes into play can add potentially interesting and complicating socio-logical detail to general ontological arguments over the natural or constructed origins of specific material phenomena. The advantage of patent litigation as a subject matter for ethnomethodological description is that it publicly articulates the terms and practical consequences of what counts as a discovery and/or invention.

Acknowledgements

Jeff Coulter died prior to the publication of this paper. An unpublished and unfinished paper of his, "Constructionism and the problem of discovery in the sociology of science," provided an argument about the grammar of discovery that initially inspired the present paper and was incorporated into it. Although Michael Lynch developed the examples and much of the discussion, Coulter's insightful, critical treatment of the grammar of "discovery" remained at the core of the argument. Versions of this paper were presented at Linköping University in 2019 and Ruhr University in Bochum in 2023, and the paper benefitted from discussions with participants in the colloquia. Steve Woolgar, Augustine Brannigan and Steven Shapin read and criticized earlier drafts of the paper and made very helpful suggestions about ideas and sources.

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31 Sacks discusses the distinction in his lectures and mentions the police unit that searched for abandoned vehicles, but does not elaborate upon the idea for an ethnography that Garfinkel attributes to a personal communication with him. See "Possessables and Possessitives," Lecture 16, Spring, 1967 (Sacks 1992, 605–609).

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