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Notes

1. I thank Stephen Emmel, Greg Given, and Brent Nongbri for reviewing a draft of this essay and providing helpful comments.
2. I lightheartedly borrow the distinction between the Egypt of history and the "Ægypt" of cultural memory that serves as the anchor of John Crowley's *Ægypt* novels (1987–2007).

Finding Early Christian Books at Nag Hammadi and Beyond

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In two recent articles, Mark Goodacre (Goodacre 2013) and Nicola Denzey Lewis and Justine Ariel Blount (Denzey Lewis and Blount 2014) have raised a number of questions both about the reliability of what has become the "traditional" story of the discovery of the Nag Hammadi codices associated with the work of James M. Robinson and about the circumstances of the burial of the codices in antiquity. These articles constitute an important challenge to established opinions, and their theses are worthy of serious consideration. I should preface my own remarks, however, with an appreciation of Robinson's work. Having undertaken research on the provenance of various papyri on a much more limited scale myself, I have great admiration for Robinson's painstaking accumulation of data, data not only for debates about the discovery of the Nag Hammadi codices, but also for the discovery and marketing

of the Bodmer papyri and the Manichaean codices of Medinet Madi. One can disagree with his conclusions while recognizing that his dedication in these areas stands as a truly remarkable achievement. Yet, no scholar is above critique, and the articles presently under review give compelling reasons to doubt some of the details of Robinson's famous account of the discovery of the Nag Hammadi codices.

In fact, objections to Robinson's account of the Nag Hammadi discovery have been known for some time. Both articles make reference to earlier skepticism expressed by Rodolphe Kasser and Martin Krause in the first footnote in the introductory volume of *The Facsimile Edition of the Nag Hammadi Codices* (Robinson 1984, 3). But these two articles make a much more detailed case undermining various aspects of Robinson's presentation of the facts. Yet, the articles are quite different in scope and execution. Two ques-

tions seem to drive the authors: What can we know about the circumstances of the discovery of the codices, and what can we know about the circumstances of the burial of the codices? Goodacre is concerned with the former question while Denzey Lewis and Blount are concerned with both questions but focus on the latter.

Beginning in the late 1970s, Robinson published the story of the discovery in several different outlets (such as Robinson 1977; Robinson 1979; and Robinson 1981). Ever the synoptic scholar, Goodacre places Robinson's multiple accounts of the events of the discovery side by side and highlights interesting contradictions in the details of the discovery story, including the number of people allegedly present at the discovery. Goodacre then introduces a piece of neglected evidence for the story of the discovery of the codices in the form of a filmed interview with the man who claimed to be the finder of the codices, Muhammad 'Ali al-Samman, preserved in a television documentary *The Gnostics* (1987). This version of events differs again in details from those accounts produced by Robinson, and in fact the televised interview with Muhammad 'Ali differs yet again from another account in the book produced to accompany the television series (Churton 1987, 9-12). Goodacre next points out that the techniques used by Robinson in the interviews he conducted with Muhammad 'Ali and others in 1970s do not measure up to the standards of modern anthropological practice, and finally Goodacre notes that Robinson is on record as himself doubting the reliability of some of Muhammad 'Ali's descriptions (Robinson mentions "instances of not impeccable veracity" on the part of Muhammad 'Ali in Robinson 1979, 213).

The discrepancies that Goodacre highlights certainly raise questions about the reliability of the story. Yet in a footnote, Goodacre notes a possible source of these differences in Robinson's retellings of the story of the discovery, namely Robinson's ongoing interviews in the late 1970s (Goodacre 2013, 307n11). There is a good possibility that the changing details in Robinson's published accounts reflect the shifting stories of Muhammad 'Ali and of Robinson's other informants, as well as Robinson's evaluations and re-evaluations of the interviews and other data he was collecting. As Goodacre correctly implies, Robinson could have clarified matters considerably had he drawn attention to these differences and made clear what version of events he deemed authoritative, or

at the very least conceded some uncertainty in these matters (the corrections offered in Robinson 2014a, 104–19 do not address the sorts of points Goodacre brings up). In a more recent account by Robinson not cited by Goodacre (or by Denzey Lewis and Blount), the numbers of people involved in the discovery are again different from earlier accounts, but the story is expressed with at least a bit more uncertainty than earlier versions (Robinson 2009, 14).

It seems to me that Goodacre's article will make it difficult for any serious scholar of the Nag Hammadi corpus to simply repeat a version of the story derived from Muhammad 'Ali as fact. But, to me, there is an even more powerful reason for doubting the account(s) of Muhammad 'Ali. Although Goodacre alludes to the archaeological evidence (or lack thereof) for the story of the discovery in the form of a citation of C. Wilfred Griggs (Griggs 1990, 217), it is worth examining an early account of the archaeological work at the alleged find site in some detail. The report of the first field season (published in 1976) contains an especially stark presentation of what was found and of the credibility of the story elicited by Robinson from Muhammad 'Ali. I quote from this report at length:

The discoverer of the codices had himself been discovered: Mohammed Ali El-Samman Mohammed Khalifa. Since he had avenged the murder of his father by killing Ahmed Ismail from Nag Hammadi a month after the discovery, he had not visited the site of the discovery for thirty years, for fear of his life. But a clandestine drive past the cliff, [led] by Dr. Hanny M. El-Zeiny, Director General of the Nag Hammadi Sugar Factories and our benevolent and effective host, resulted in the identification by Mohammed Ali of the tomb of Thauti as the cave just inside of which he had discovered the codices in a sealed four-handled jar some 60 cm. high and 30 cm. wide buried beside a corpse or mummy which he reburied [sic] where he found it. By this time the caves had been numbered in black paint 1-158, from south to north; this is T 73. . . . Since the excavation of the floor of Cave 73 to bed rock...had not provided any confirmatory evidence of Mohammed Ali's identification of it as the site of the find, he was asked on 11 December to reconsider his identification. He changed his identification to the northern side of the fallen broken boulder visible in Doresse's photo, where the boulder forms an overhanging slab at some 45°. On 14 – 15 Decem-

ber this and the adjoining areas were cleared to bed rock, without finding evidence of a burial or reburial, or of sherds from the jar. The excavation of places associated with the site of the discovery (Areas A – D, T 1 and T 73) included the sifting of the debris first with a 1 by 1 cm. mesh, then by the finer mesh of a window screen, in view of the report by Mohammed Ali that he broke the jar near the site of the discovery and saw flying into the air what must have been papyrus fragments; none were found. Nor were sherds found that could be readily associated with the jar in which the codices were discovered. Nor was it possible to locate the corpse or mummy Mohammed Ali said he reburied where he found it, beside the jar on a bed of charcoal. According to the memory of Regheb Andrawus 'El-Kes' Abd El-Said, a neighbor of Mohammed Ali who acquired Codex III, Mohammed Ali told him at the time that a staff lay beside the jar, a detail later denied by Mohammed Ali; in any case, it was not found. Thus the excavation produced no archeological confirmation of the precise site of the discovery. (Robinson and van Elderen 1976, 19-21)

Not only did Muhammad 'Ali offer more than one find spot, but excavation at both places he identified failed to confirm any part of his story. While it is highly probable that illicit diggers plundered these areas between the time of discovery and 1975, it seems unlikely that they would have taken the time and trouble to completely sift all the soil in these areas with fine mesh removing every last scrap of evidence. It seems equally (if not more) probable that Muhammad 'Ali al-Samman simply did not at all remember where exactly the codices had been found thirty years earlier. Furthermore, apropos of Goodacre's approach, it is fascinating to see the way this report as a whole and this passage in particular are redacted in Robinson's more recent account (Robinson 2014b, 1114–22); for those who might be interested, a synoptic comparison is most illuminating.

It is fair to say that the differences highlighted by Goodacre cast considerable doubt on some of the details of the story of the discovery as Robinson has narrated it, but I do not think this is so much a condemnation of Robinson as it is an indictment of a certain slackness of the guild of biblical and early Christian scholars for so long ignoring the conflicting data that Robinson has himself provided. As Goodacre notes, most scholars are unaware of the different versions of the story (Goodacre 2013, 308). Both the article

of Goodacre and that of Denzey Lewis and Blount have done us a service by drawing our attention to these kinds of discrepancies and serving as a good reminder of the necessity of returning to sources and checking our facts, even with material with which we feel familiar.

If Goodacre's article suggests we ought to doubt the details of Robinson's retellings of the story of the discovery of the codices, Denzey Lewis and Blount go a step beyond Goodacre and undertake the more ambitious and challenging task of providing an alternative account of the discovery and contextualization of the deposition of the codices in antiquity. The remainder of my response will focus on their article. I understand their case as consisting of basically three parts: They argue that the books were not discovered in the find-spot proposed by Robinson, they seek to raise doubts about an allegedly widespread view that the codices should be associated with monastic activity, and they challenge the idea that the books were buried "for posterity," arguing instead that the books were "grave goods" or possibly even "new Christian Books of the Dead" (Denzey Lewis and Blount 2014, 418).

On the issue of the location of the discovery itself, Denzey Lewis and Blount make two claims. First, the find spot proposed by Robinson was "quite different" from the one proposed in earlier studies by Jean Doresse, and second, the actual find spot was "a burial cave" (Denzey Lewis and Blount 2014, 402 and 413).

Each claim requires some clarification. The clearest description of the area of the find in any of Doresse's works is in a caption to a photograph used as the frontispiece in *The Secret Books of the Egyptian Gnostics* (Doresse 1960), in which the spot of the find is located at "the south-east flank of the Gebel et-Tarif . . . at the foot of the wall of rock," where "overturned earth in front of the cliff" which "marks the site of the ancient cemetery where the jar containing the manuscripts was buried."

This description has no parallel in the French edition of the book, in which a different photograph carries only the brief caption "Le lieu de la découverte" (Doresse 1958, 137). In the text of the book, Doresse seems to indicate that a tomb in the "overturned earth in front of the cliff" was the location of the discovery: "Was it in one of these tombs that the papyri were found? Certainly, one cannot, even if one searches very far around, see any other place—

any ruin or sepulcher—from which they could have come.” He did not provide any more specific locale: “As to the exact location of the find, opinion differed by some few dozen yards; but everyone was sure that it was just about here” (Doresse 1960, 133). The various words Doresse used to describe the find spot (*cavités, les sépultures se dispersent même jusqu’à une centaine de mètres du pied de la montagne, l’antique cimetièrre, vaste mais pauvre nécropole*) leave it unclear whether he was referring to the Pharaonic tombs cut into the cliff itself, the talus (rubble) at the foot of the cliff, or in the plain beyond the talus. Nevertheless, there seems to be no question that Doresse was describing a find spot in a burial context at the southeastern end of the Jabal al-Tarif. And although different, the photographs in the French and English editions of Doresse’s book both depict portions of the cliffs in this area (Robinson 1981, 25-26).

The question of the find spot identified by Robinson is also somewhat complicated because, as we saw above, Muhammad ‘Ali changed his story about the exact place of the discovery. He first identified a cave some seven hundred meters away from the area in Doresse’s photographs and then switched his identification to a boulder in the area within the photograph published in the French edition of Doresse’s book (Doresse 1958). And it was this latter identification that became the focus of Robinson’s preferred story of the find:

On each side of the Nile Valley cliffs rise abruptly to the desert above. The section of the cliff on the right bank marking the limit of the Nile Valley and the arable land between Chenoboskia and Pabau is called the Jabal al-Ṭārif. A protruding boulder shaped somewhat like a stalagmite had broken off in prehistoric times from the face of the cliff and fallen down onto the talus (the inclined plane of fallen rock that over the ages naturally collects like a buttress at the foot of a cliff). Under the northern flank of one of the huge barrel-shaped pieces of this shattered boulder the jar containing the Nag Hammadi library was secreted. (Robinson 1988, 22)

Thus Robinson locates the find within the rather vague area identified by Doresse: the talus at the south eastern edge of the Jabal al-Tarif. The chief difference in the accounts is Doresse’s descriptions of the area as including “sepulchers...scattered about

to as far as a hundred yards from the base of the cliff” and his characterization of the area as an “ancient cemetery” or “necropolis.” The archaeological work in the area carried out in the 1970s (using both test trenches and non-invasive ground-penetrating technologies) yielded no evidence of a “cemetery” anywhere in the area of Doresse’s photograph. Indeed, according to the report, there were only two instances of irregular patterns indicative of underground disturbance in the area, both of which were inspected by means of excavation “with negative results” (Robinson and van Elderen 1976, 19-20). So, to be clear, Robinson’s preferred find spot lies within the area proposed by Doresse, it is just that Doresse’s reports of “sepulchers” and a “cemetery” in the immediate area were not confirmed (unless by these terms Doresse simply meant to describe the Pharaonic necropolis in the cliff itself). Thus, the claim of Denzey Lewis and Blount that the codices were found in a “burial cave” is not unambiguously supported by the conclusions of either Doresse or Robinson. This does not, however, mean that their claim is baseless. On the contrary, the earliest surviving reports of the discovery also located it in variously a “tomb” or “cave” (Mina 1948, 129; Robinson 2014a, 93), although Robinson simply dismisses such early reports (“a cave was the common inaccurate assumption,” Robinson 2014a, 93). In addition, as Denzey Lewis and Blount note, the archaeological explorations in the area in the 1970s did uncover human bones and other artifacts from the caves in the cliff, most notably, Cave 1, not far from the area identified by Doresse and Robinson as the site of the discovery (Robinson and van Elderen 1976, 20). In the final analysis, I think that one of these caves is just as likely a find spot as Robinson’s boulder in the talus, but it is important to be as clear and precise as possible about what the actual evidence for this conclusion is.

On the issue of the supposed prevalence of the monastic hypothesis for the origins of the Nag Hammadi codices, I wonder if Denzey Lewis and Blount have overstated their case, especially in regard to Robinson’s own opinions. This seems evident in their attribution to Robinson of the following quotation: “Since it is hardly conceivable that there would have been more than one orthodox monastic organization simultaneously operating in the same place, we should be justified in concluding, *even without further evidence*, that the Nag Hammadi material came

from a Pachomian monastery" (Denzey Lewis and Blount 2014, 406, their emphasis). To the best of my knowledge, the actual source of these words is not Robinson, but rather John Barns, the original editor of the fragmentary papyri used to stuff the leather covers of the codices (Barns 1975, 13). Barns was indeed convinced of the monastic origin of the codices because of what he read as references to monasticism in general and Pachomian monks in particular in the papyri from the covers.

In the period immediately after Barns' 1975 publication, there was a good deal of excitement about the possibility that the codices had their origins in Pachomian circles, and Robinson's publications from that period do reflect this general mood (Robinson 1977, 16-17). But as Denzey Lewis and Blount note, the final editors of the papyrus fragments from the covers rejected Barns' readings and found that "evidence for monasticism in general in these papers is less frequent than was supposed in that work, and there are no texts in which a specifically Pachomian background comes plainly to the fore" (Shelton 1981, 2).

My impression of the scholarship since 1981 is that there has been a marked ambivalence about the social settings in which the codices may have been produced and used. Indeed, Denzey Lewis and Blount cite more recent studies to that effect (such as Emmel 2008 and Khosroyev 1995; one could add reference works like Emmel 1991, or even general introductions like Layton 1987, xxvii).

And it seems to me that even Robinson's own more recent view of matters is more subtle than Denzey Lewis and Blount imply. For instance, in the introduction to the 1988 edition of the English translation of the Nag Hammadi works, Robinson has written that "the relation of the Nag Hammadi codices to the Pachomian movement remains a tantalizing possibility, more concrete than any other that has been suggested, and yet far from assured" (Robinson 1988, 17; Denzey Lewis and Blount regularly cite from the 1977 edition of this book, which was produced prior to the overturning of Barns' preliminary conclusions in 1981).

While it may be a quirk of my own reading habits, I find that the "monastic hypothesis" is rather less monolithic and dominant than it is portrayed in parts of the article of Denzey Lewis and Blount. And much of the article seems to posit a sharp divide between "monastic" and "non-monastic" such that

their observation that "the lines between coenobitic monks, anchorites, and private citizens were fluid in the fourth century" comes as a surprise and calls out for further development (Denzey Lewis and Blount 2014, 416).

In the place of a monastic setting for the production and deposition of the codices, Denzey Lewis and Blount propose that the codices were "grave goods" or perhaps even "Books of the Dead." They emphatically reject Robinson's suggestion that the person or persons who buried the codices in antiquity did so "for safekeeping, perhaps for posterity" (Robinson 1988, 21) on the grounds that such a practice is "not attested in Egypt" (Denzey Lewis and Blount 2014, 414). Operating on the assumption that the Nag Hammadi codices were found in a burial context, Denzey Lewis and Blount seek to situate these books among other "Gnostic" books: "Egypt has a rich history of books and corpses found together, and indeed all our other so-called Gnostic manuscripts—the Berlin Codex, the Askew Codex, the Codex Tchacos—came from, or most likely came from, burial sites" (Denzey Lewis and Blount 2014, 405).

Their case begins to look quite impressive—until we subject the individual elements of the case to the same kind of scrutiny to which Denzey Lewis and Blount subjected some of Robinson's claims about the Nag Hammadi codices. In fact there is very little reliable evidence for early Christian books being buried with bodies and almost no such evidence for specifically "Gnostic" books. It is true that Carl Schmidt asserted that the Berlin "Gnostic" codex 8502 came from a "cemetery in Akhmim," but this is purely Schmidt's conjecture. All that is actually known is that a dealer from Akhmim said the book was found in a "wall niche" (Schmidt 1903, 2). Of the provenance of the Askew codex, we know nothing prior to its purchase by Anthony Askew in 1772: "Dr. Askew had chanced to buy it in a bookshop" (Buhle 1796, 69).

So also the Bruce codex: only vague descriptions of its provenance are known prior to its purchase by James Bruce in Egypt in about the year 1769. Bruce himself said simply that the codex was "dug up at Thebes" (Bruce 1790, 7. I owe the reference to Créghour 2014, 49-50), while a later writer has elaborated somewhat: "When Mr. Bruce was at Medînat tâboû or Thebes, in Upper Egypt, he purchased a Coptic manuscript which had been found in the ruins near that place, in the former residence of some

Egyptian monks" (Robins 1842, 35). The Manichaean codices that appeared on the antiquities market in the early 1930s are of unknown origin but are said to have been discovered at Medinet Madi in the ruins of a "domestic dwelling" (Schmidt and Polotsky 1933, 9). The exact find spot of the collection that included the Tchacos codex is also not known. Although it has been widely reported that the books were found in a "catacomb" in Qarara, these reports are based on third-hand hearsay from anonymous antiquities traders packaged in an account replete with the same kinds of orientalist tropes and contradictions that Denzey Lewis and Blount flag in Robinson's stories of the provenance of the Nag Hammadi codices (Krosney 2006, 9-27). When Stephen Emmel saw these books in 1983, all he was told by the owner was that they were found somewhere "near the village of Beni Masar, about 8 km. south of Oxyrhynchus (modern Behnasa)," and Emmel seems to have regarded even this earlier report with suspicion, commenting, "It is difficult to know how seriously to take such information" (quoted in Robinson 2007, 56).

For Christian books not generally classified as "Gnostic," there is some evidence for burial with bodies, but it is slim. A recently discovered early report that the Chester Beatty biblical papyri were found in three jars on top of a coffin is open to some suspicion (Horton 2004 on the discovery; Nongbri 2014 for doubts). And even when there are reliable reports about codices being found in the vicinity of "cemeteries," it is not always clear that the codices were actually buried with people. Consider the account of the discovery Qau codex, a Coptic codex of the Gospel of John assigned to the fourth or fifth century:

Near the village of Hamamieh, close to a large wady or ravine, one of these spurs, covered with limestone detritus, has been used as a cemetery in Predynastic, early Dynastic and Roman times. When Mr. Guy Brunton was clearing this in March 1923 for the British School of Archaeology, a broken crock was found, buried 18 inches under the surface, in the neighbourhood of the Roman or early Coptic graves. The pot is of red pottery painted pale buff, with a decoration in black of bands and spots, which cannot unfortunately be closely dated. Mr. Brunton's assistant, Mr. Starkey, in emptying the dust from the pot found that it contained a little package of papyrus wrapped

in rag, and tied with thread. (Petrie 1924, ix)

Although the codex was found in a shallow burial "in the neighbourhood" of graves, there is no indication that the codex was associated with a body. To my knowledge, there are only three (fairly) certain instances of Christian books being buried with bodies in late antique Egypt. A codex containing the *Gospel of Peter* and other texts, now generally thought to date from the sixth or seventh century, was described as having been found in a necropolis in Akhmim in the "tomb" of an individual who may or may not have been a monk (Bouriant 1892, 94; Van Minnen 2003, 17-18). A Coptic papyrus codex of the fourth or fifth century containing Deuteronomy, Jonah, and Acts was reported to have been discovered in a coffin in Hermopolis, wrapped in linen lying between the feet of a corpse (Budge 1920, 2.372-74). More recently, a miniature Coptic codex of the fourth or fifth century (the so-called Mudil Psalms) was said to have been found, during controlled excavations, under the head of the corpse of a young girl (Gabra 1995), although, remarkably, there seems to be no published photographic documentation of the find *in situ*. In any event, there is nothing to indicate these examples are representative of usual practices. As controlled excavations have begun to give us more reliable data on find spots of codices in Egypt, other contexts have emerged, such as domestic spaces like those in which the Kellis Isocrates and Manichaean literature were found (Worp and Rijksbaron 1997; Gardner 2007), although at least some of the Manichaean literature seems to have been secondarily deposited in the houses as trash. Similarly, one can point to the rubbish heap outside a monastic dwelling in Thebes where three Coptic codices were uncovered ten years ago (Górecki 2007).

The non-Christian evidence cited by Denzey Lewis and Blount for late antique burial of books with bodies is equally difficult. In this regard, they mention the discovery of a copy of a roll containing portions of the *Iliad* discovered buried under the head of a woman (the so-called Hawara Homer) and write that the "papyrus dates to the fifth century." It is true that in the first publication of the papyrus in 1889, Herbert Maunde Thompson assigned it to the fifth century (Petrie 1890, 24). But just ten years later Frederic Kenyon made an argument, based among other things, on the presence of cursive writing in the margins of the manuscript, that the "Hawara Hom-

er” was a product of the second century CE, rather than the fifth (Kenyon 1899, 101-3). Thompson himself found Kenyon’s case so compelling that he later used the Hawara Homer as (literally) a textbook example of a second-century Greek formal hand in his introduction to palaeography (Thompson 1912, 141-43). And indeed, the papyrus retains that position in more recent palaeographic handbooks (Turner 1987, 38). Thus, even if we imagine the papyrus was buried a century (or even two centuries) after the time when experts think it was copied, it would still be of questionable relevance for fifth-century funerary practices.

Finally, we have the Theban magical papyri, which Denzey Lewis and Blount claim were “discovered by *fellaheen* under suspicious circumstances, almost certainly tomb robbing” (Denzey Lewis and Blount 2014, 414). Yet, again, we actually know nothing with any certainty of the history of these books prior to their appearance in the collection of Jean d’Anastasy in the early nineteenth century. Even the degree to which they constitute a single find is open to question. That they may have come from a burial context is simply speculation (Dosoo 2014, 28-35 and 149-56). It seems to me that the claim “This manuscript was found in a graveyard with a corpse” might well be a trope used to pique the interest of potential European and American buyers and increase selling prices (it is a bit more interesting and romantic than “This book was found in a wall niche”). This is not to say, of course, that the trope might not occasionally reflect the actual circumstances of the find. We should nevertheless probably be wary of building arguments based on unsubstantiated claims that ancient books were found in tombs or cemeteries or the like. While it is certainly possible that the Nag Hammadi codices were buried with a body, the evidence provided by other discoveries of early Christian books in Egypt is not nearly as strong as Denzey Lewis and Blount suggest.

And this brings me to an irony that I find at the heart of the article of Denzey Lewis and Blount: With their critique of Robinson, they offer us some genuinely exciting new ways to approach the Nag Hammadi material, but they simultaneously slip into some of the same problems that trouble Robinson’s own account: i.e., taking a rather uncritical view of the evidence in favor of their hypothesis and relegating any contrary arguments to the status of simply being wrong *tout court* (evinced by their use of the

word “demolished” three times in reference to theories of a possible monastic context for the production and burial of the codices). What we have in the work of Denzey Lewis and Blount is a useful polemic, a prod to get us thinking in different ways about old problems. But I mentioned earlier the importance of clarity and precision in dealing with the evidence. A more critical review of the evidence would produce more ambiguity in regard to the questions of the production and discovery of these codices. And this ambiguity should encourage a variety of explorations. The idea that the books are burial goods or even that their contents might have significance for their owners’ concerns about the afterlife are theories worth developing, but so also are the possibilities of monastic connections to the codices. We are simply not in a position at present to suggest that any connection to monasticism be definitively ruled out. There are, after all, papyri from the cover of Codex VII that ultimately derive from an unambiguously monastic context; whether they found their way into the cover of Codex VII via “a town rubbish heap” or through some more direct route is an open question (Shelton 1981, 5–11). What are needed are detailed studies exploring the different possible contexts for the production and use of the codices. A fresh examination of monastic connections has recently appeared (Lundhaug and Jenott 2015).

A second irony has to do with concerns about “orientalism” in the story of the discovery of the codices. Both Goodacre and Denzey Lewis and Blount note what they call “orientalizing” features of Robinson’s narrative (Goodacre 2013, 317). Denzey Lewis and Blount are more openly critical of this aspect of Robinson’s work:

To begin, we might do well to recognize [the narrative’s] many colonialist, orientalizing elements as relics of a bygone era in Egyptian archaeology. The narrative is a fine one for classroom telling, but it works less and less effectively as we become more sensitized to our own Western prejudices and assumptions. Egyptian peasants do not fear *jinni* in bottles or rip out each other’s hearts and eat them on the spot—and shame on us for believing, even for a moment, that they do. (Denzey Lewis and Blount 2014, 418)

The irony of a critique of orientalism that includes such confident declarations about what “Egyptian peasants” do and do not think is clear, and such

criticism could easily be turned on its head. Indeed, Robinson has already done so, in effect charging that those who express skepticism about his account of the discovery deny the agency of native Egyptians: "Such scholars [look] down condescendingly on the Egyptians as natives one could never trust" (Robinson 1998, 138). In any event, these sorts of arguments strike me as red herrings in the present discussion. Modern anthropological studies do in fact stress the importance in rural Egyptian societies both of non-obvious beings like *jinn* and of retributive justice involving homicide (Hopkins 2007 and Nielsen 2006). If we are going to doubt the reliability of Muhammad 'Ali's story (and I think we should) let it be because of the inconsistencies in its various retellings rather than because certain elements of his story may appear to pander to "Western" prejudices with regard to "Orientals." For a more nuanced treatment of orientalism in the study of the Nag Hammadi materials, see the contribution of Dylan Burns in this issue.

The articles of Goodacre and Denzey Lewis and Blount are timely and important. With the new information that has appeared since the publication of these articles (Robinson 2014a, 71–104), we are in a good position for a reconsideration of "facts" that we all thought we knew fairly well. The critical comments I have made here come in the hopes that this renewed discussion takes into account as fully as possible the complexity and ambiguity of some of the relevant evidence. Perhaps future studies will come to convincing conclusions about some of these difficult problems. In the meantime, it might be best to adopt a more, shall we say, agnostic view of the circumstances of the deposition and discovery of the Nag Hammadi codices.¹

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Notes

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