

# A Response to Nahyan Fancy's Review Article

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First, I would like to express my appreciation to Nazariyat for providing this space for a discussion of my book *The Medieval Islamic Hospital* (2015).<sup>1</sup> I would also like to thank Nahyan Fancy for taking the time to pen a ten-page review of it, thereby providing the chance for this discussion.<sup>2</sup> In the coming pages, I will offer some remarks on his review, and shed some light on specific points that may have been misunderstood. I will then offer brief and general remarks about certain aspects of my intellectual project that I believe his review may have overlooked. I regret that the tone of Fancy's review may have gotten in the way of a fruitful intellectual discussion about the priorities of our field. My intention here is to look beyond that tone (and the occasional ad hominem comment) in order to focus on the more pertinent substantive questions.

Without further ado, I start with his summary of my book, in which some of the misreadings and mischaracterizations first appear. For example, Fancy claims that I used the Ṭulūnid *bīmāristān* (est. ca. 265/879) as the exemplar of hospitals built in Egypt and the Levant (138). While seemingly insignificant, this mischaracterization is at the heart of his misreading of the historical trajectory discussed in the book. The Ṭulūnid *bīmāristān* was studied in the book because contemporary sources claimed that it was Egypt's first such institution. While I questioned

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1 Ahmed Ragab, *The Medieval Islamic Hospital: Medicine, Religion, and Charity* (New York: Cambridge University Press, 2015).

2 Nahyan Fancy, review of *The Medieval Islamic Hospital: Medicine, Religion, and Charity*, by Ahmed Ragab, *Nazariyat Journal for the History of Islamic Philosophy and Sciences* 3, no. 1 (November 2016): 137-46.

these claims and provided evidence that other *bimāristāns* may have existed before it, I explained that none of these possibly earlier ones appeared to have been of this size or importance. The point of this being one of the earliest *bimāristāns* as opposed to being the exemplar, as Fancy states that I claimed, is important because the book's first part attempts to build a historical arc in which different origins led to somewhat different models and in which exchanges, travels, communications and connections between physicians, bureaucrats and patients eventually led to many commonalities among the *bimāristāns* of Iraq, the Levant, and Egypt. I made this point clear by explaining how al-Bimāristān al-Nūrī (est. ca. 548/1154), the major institution in the Levant for centuries, was connected to al-Bimāristān al-ʿAḍudī (est. ca. 370/981). As for the Baghdad institutions, Fancy states that I focused on al-Bimāristān al-ʿAḍudī, which again misrepresents the book's historical arc (138). In the prologue, where this discussion occurred, I compared al-Bimāristān al-Ṭūlūnī to earlier Iraqi *bimāristāns*, such as those built by the Abbasid caliphs al-Muʿtaḍid (r. 279-89/892-902) and al-Muqtadir (r. 295-320/908-29) as well as Bimāristān al-Sayida, which was patronized by Shaghab Umm al-Muqtadir (d. 321/933). All of these institutions were nearly contemporary to al-Bimāristān al-Ṭūlūnī. I relied upon Peter Pormann's remarkable study of these hospitals, for which he relied upon and analyzed Ibn Abī Uṣaybi'a's (d. 668/1270) text.<sup>3</sup>

Similarly, Fancy asserts that I overstated the difference between the *bimāristāns* in Iraq and those in the Levant and Egypt, citing my statement that the former were "not...intended to immortalize the patron's name" (141). He counters by stating, correctly, that most of them were, in fact, named after their patrons. However, he quoted only part of the relevant sentence. Reading it in its entirety makes the argument clear: "[T]he Bimāristāns of Baghdad were not part of a larger building program intended to immortalize the patron's name and symbolize his greatness and wealth; they were instead integral to the patronage of medicine and physicians."<sup>4</sup> Indeed, the point here concerns the building programs and the institutions' comparative place in such programs. As I mentioned in the book, these *bimāristāns* were named after their patrons. However, this is rather inconsequential when we think about it in relation to the comparative costs of building these institutions, or the comparative importance accorded to them in the hagiographies of their patrons, both of which I analyzed in my book. I also addressed the naming traditions, a fact that the reviewer

3 Peter E. Pormann, "Islamic Hospitals in the Time of al-Muqtadir," in *Abbasid Studies II: Occasional Papers of the School of Abbasid Studies, Leuven, 28 June-1 July 2004*, ed. John A. Nawas (Leuven: Uitgeverij Peeters en Departement Oosterse Studies, 2010), 337-81.

4 Ragab, *The Medieval Islamic Hospital*, 39.

did not mention, to explain how, in Baghdad, those *bīmāristāns* named after exiled courtiers were protected and maintained their original names.<sup>5</sup> During the same period in Egypt, their counterparts were vandalized if not razed outright. A fuller consideration of my argument, as opposed to a partial quotation, would have helped direct the discussion to more fruitful and accurately defined grounds.

In the same manner, Fancy mistakes discussions of indirect influences and historical trajectories for claims of direct causality that I never made. He claims, without evidence, that I argue for the influence of Crusader institutions on al-Bimāristān al-Manṣūrī (est. 683/1285) (138). But this is an inaccurate reading. I did indeed argue, in detail and with evidence from multiple contemporary sources, in favor of important influences and interactions between the Crusader hospital of Jerusalem and Ṣalāḥ al-Dīn al-Ayyūbī's (d. 589/1193) Jerusalem hospital there. I used and assessed sources written by Ṣalāḥ al-Dīn's historians and scribes, some of whom were involved in building his hospital. In relation to Qalāwūn's works, I explained how he was influenced by Ṣalāḥ al-Dīn and Nūr al-Dīn Zankī (d. 569/1174). When discussing Qalāwūn's *bīmāristān* in Hebron, I used a variety of sources to detail how both Jerusalem and Hebron were connected in the local writings, pilgrimage and visitation trips, and imaginaries. I also compared this relationship to that between Makkah and Madinah and used Baybars' (r. 658-76/1260-77) and Qalāwūn's work in these two holy cities to contextualize their works in Jerusalem and Hebron. As such, I presented a more complex picture of the consistent influences that existed between similar and neighboring institutions that continued to exist together in Jerusalem and Hebron.<sup>6</sup> Unfortunately, Fancy does not read this influence in the context in which I located it.

In addition to missing some of these historiographic details, Fancy oversimplifies several of the book's arguments, a fact that underlies many of his criticisms of said arguments. For instance, in relation to al-Manṣūr Qalāwūn's (r. 678-89/1279-90) patronage, he asserts that I maintained that Qalāwūn was not interested in medicine. He criticizes my use of Linda Northrup's work, which I cited and fully concurred (146). In fact, I maintained that Qalāwūn's interest in medicine manifested itself *mainly* in *bīmāristāns*. Aside from building two of them and renovating a third, I explained that Qalāwūn chose not to build medical madrasas, for instance, similar to those built in Damascus only a few decades earlier or to support other

5 Ibid, 38-39.

6 Ibid., 59-75.

medical projects, like those in Baghdad, as described by Peter Pormann.<sup>7</sup> The point of this observation, which Fancy's review fails to mention, is to understand the *bimāristāns* both in relation to Qalāwūn's interest in medicine *as well as* his overall building program. In this respect, I stand on the shoulders of giants in the field, such as Adam Sabra,<sup>8</sup> Linda Northrup,<sup>9</sup> and many others. I also build on Yasser Tabba's inspiring work on hospitals and on al-Bimāristān al-Nūrī.<sup>10</sup>

Another example of the reviewer's problematic representations of my arguments is his claim that I do not accept the influence of medical thought on these hospitals' design and functioning (140). This is quite jarring to read, given the presence of long sections that discuss the relevant *waqf* documents, the architecture of al-Bimāristān al-Nūrī and al-Manṣūrī, as well as others that describe patients and physicians, and explain how both the design of al-Bimāristān al-Manṣūrī, as well as the concerns of its patrons and administrators, were deeply influenced by Galenic medical knowledge.<sup>11</sup> I do, in fact, assert that *bimāristāns* were more than just medical institutions; rather, they were influenced by political, social, cultural, and religious concerns.<sup>12</sup> Here again, Fancy summarizes a long argument in a manner that misrepresents my intended point and, regrettably, then proceeds to criticize this short-hand misrepresentation. I think that both I and the readers would have benefited from an engagement with the actual argument.

Fancy also criticizes my use of sources and my supposed "reliance" upon Ibn Abi Uṣaybi'a even in discussing Ibn Ṭūlūn and his times (141). This is, however, entirely inaccurate; he may have missed my analysis of three different contemporaneous sources penned by Ibn Ṭūlūn's contemporaries and scribes.<sup>13</sup> I neither claimed that my sources' accounts were unbiased nor I took them for granted. Looking into their authors' lives and careers, I highlighted their potential biases and what may have influenced their narratives. An important methodological point here, to which I

7 Peter E. Pormann, "Medical Methodology and Hospital Practice: The Case of Fourth-/Tenth-Century Baghdad," in *In the Age of al-Farabi: Arabic Philosophy in the Fourth-Tenth Century*, ed. Peter Adamson (London: Warburg Institute, 2008), 95-118.

8 Adam Abdelhamid Sabra, *Poverty and Charity in Medieval Islam: Mamluk Egypt, 1250-1517*, Cambridge Studies in Islamic Civilization (Cambridge: Cambridge University Press, 2000).

9 Linda S. Northrup, *From Slave to Sultan: The Career of al-Mansour Qalawun and the Consolidation of the Mamluk Rule in Egypt and Syria (678-689 A.H./1279-1290)* (Stuttgart: F. Steiner, 1998).

10 Yasser Al-Tabba, "The Architectural Patronage of Nūr al-Dīn, 1146-1174" (PhD Dissertation, New York University, 1982); Y. Tabbaa, *Constructions of Power and Piety in Medieval Aleppo* (University Park: Pennsylvania State University Press, 1997).

11 Ragab, *The Medieval Islamic Hospital*, 109-36, 177-87.

12 Ibid., 223-30.

13 Ibid., 33-37.

will return later, is that historical sources may be used not only to construct “accurate narratives” or “truths,” but also to understand what the authors wanted to represent about themselves. For instance, in my analysis of how one author described a dream in which Ibn Ṭūlūn went to heaven, I definitely did not claim that this was true, for such a claim would be hard to substantiate at least in this world, as would the “fact” that the person actually had this specific dream. However, such accounts can help us understand what these authors viewed as the most valuable acts and what they thought solidified Ibn Ṭūlūn’s public claim to heaven. I employed Ibn Abi Uṣaybi‘a as a source about his own practices and readings, as well as those of his masters and colleagues. I analyzed and discussed his biases and, in fact, “used” them in an attempt to reveal how this group of physicians wanted to portray their work and what they saw as pious, virtuous, or valuable. Sadly, I cannot claim credit for this methodological approach, for it has been used by many historians who employed the same or similar methodological tools, including discourse analysis, in writing their own specific narratives.<sup>14</sup>

14 A central example here is Roy Mottahedeh, *Loyalty and Leadership in an Early Islamic Society* (London: I. B. Tauris, 2001). Mottahedeh used a variety of sources and genres to construct larger meanings and discourse on loyalty and leadership. Similarly, William Graham has shown the role of traditions and the authority invested in historical materials (William A Graham, “Traditionalism in Islam: An Essay in Interpretation,” *The Journal of Interdisciplinary History* 23, no. 3 (1993): 495-522). Also, Khaled El-Rouayheb’s work on homosexuality stands as another example in this vein (Khaled El-Rouayheb, “The Love of Boys in Arabic Poetry of the Early Ottoman Period, 1500-1800,” *Middle Eastern Literatures* 8, no. 1 (2005): 3-22; Khaled El-Rouayheb, *Before Homosexuality in the Arab-Islamic world, 1500-1800* (Chicago: University of Chicago Press, 2009). Konrad Hirschler’s work on historiography analyzed historical narratives to show the authors’ views on piety and virtue (Konrad Hirschler, *Medieval Arabic Historiography: Authors As actors* (London: Routledge, 2006). Patricia Crone and Martin Hind’s work on the caliphate and political authority utilized a variety of sources to construct the sociocultural and religious meaning of this institution in early Islam (Patricia Crone and Martin Hinds, *God’s Caliph: Religious Authority in the First Centuries of Islam*, vol. 37 [Cambridge & New York Cambridge University Press, 2003]). In the larger field of medieval studies, the work of Jacques Le Goff (Jacques Le Goff, *Pour un autre moyen age: Temps, travail et culture en occident: 18 essais* (Paris: Gallimard, 1977); Jacques Le Goff, *Héros & merveilles du moyen age* (Paris: Seuil, 2005); Jacques Le Goff, *A la recherche du temps sacré: Jacques de voragine et la légende dorée* (Paris: Perrin, 2011); Gabrielle Spiegel (Gabrielle M. Spiegel, “Genealogy: Form and Function in Medieval Historical Narrative,” *History and Theory* 22, no. 1 (1983): 43-53; Gabrielle M. Spiegel, *The Past as Text: The Theory and Practice of Medieval Historiography* (Baltimore: The Johns Hopkins University Press, 1997); Gabrielle M. Spiegel, *Practicing History: New Directions in Historical Writing after the Linguistic Turn* (New York: Routledge: 2005); and Carlo Ginzburg (Carlo Ginzburg, *Inner Dialogues: The Jew as Devil’s Advocate* (Jerusalem: Israel Academy of Sciences and Humanities, 2014); Carlo Ginzburg, *History, Rhetoric, and Proof* (Hanover, NH: University Press of New England, 1999); Carlo Ginzburg, *The Night Battles: Witchcraft & Agrarian Cults in the Sixteenth & Seventeenth Centuries* (New York: Penguin Books, 1985) are leading examples of such methodological approaches. In the history of science, one can look at Katharine Park’s work (Katharine Park, *Secrets of Women: Gender, Generation, and the Origins of Human Dissection* (New York: Zone Books, 2006) and her co-authored work with Lorraine Daston (Katharine Park and Lorraine Daston, *Wonders and the Order of Nature, 1150-1750* (New York: Zone Books, 1998), among many others.

In the same way, Fancy misses my historiographical argument concerning the casebook attributed to Abū Bakr al-Rāzī (d. 313/925) and wonders why I chose to use one tenth-century text while neglecting another tenth-century text, namely, that of *al-Ḥāwī*. In his focus on and interest in texts qua texts, Fancy does not notice what I actually said in this section. The casebook was attributed to al-Rāzī; however, the oldest surviving manuscript was compiled by a physician who worked in al-Bimāristān al-Nāṣirī (est. 576-7/1181) and died in 656/1258. We therefore cannot claim with certainty that this attribution to al-Rāzī is accurate. But more relevant to our discussion is the fact that this book was compiled or composed during the thirteenth century, which is the focus of my study. And so I analyzed it as the thirteenth-century text that it is. For *al-Ḥāwī*, the manuscript tradition is neither clear nor conclusive enough to determine the exact version that circulated, let alone the one that was produced, at that time and how it differed from earlier versions. Hence, I utilize *al-Ḥāwī*'s cases rather briefly and only to look for the origins of the arguments made in other contemporary texts. In this instance, Fancy's interest in textual and philological analysis obfuscates the importance of the manuscript tradition in constructing a historiographical argument.

Another example of the reviewer's view on how one should deal with texts is his contention that I did not know that Ibn al-Nafis' (d. 687/1288) commentary on the *Aphorisms* had reverted to the Hippocratic text (143). This is an unfortunate assertion, given that I initiated my discussion of this commentary by stating: "The commentary by Ibn al-Nafis on the *Aphorisms* followed the Hippocratic text in its focus on practice."<sup>15</sup> However, again, what Fancy misses is the entire point of this particular analysis. Once more, his focus on texts qua texts causes him to argue that the fact that Ibn al-Nafis' arrangement is similar to Hippocrates' means that the former added nothing (143-44). In the book, however, I compared Ibn al-Nafis' work to the most popular commentary at that time, namely, that of Ibn Abī Ṣādiq (d. after 460/1068).<sup>16</sup> Here, his deviation (or reverting to the Greek original) was significant because it departed from the more common texts being used *at the time*. My book is not, nor does it claim to be, a textual analysis of the *Aphorisms* or its commentary tradition. Rather, I used this commentary, as one example among others, to show the intellectual priorities of a particular group of physicians. Here, also, Fancy does not see the point of discussing what physicians were interested in writing about without engaging in a textual and philological analysis of these writings themselves. While such an analysis is admirable and informs us about various

15 Ragab, *The Medieval Islamic Hospital*, 158.

16 *Ibid.*, 158-63.

texts, texts need to be contextualized within a library or an archive of writings that shows what was regarded as interesting at a particular time and place and why and how these priorities changed thereafter in various ways. I sought to reconstruct this circle of physicians' archive by looking at the full picture of their medical library, which included not only their writings but also what they read, when they read them and why. We are then able to analyze their interests and their concerns. Again, I sadly cannot lay claim to this as a methodological innovation, as numerous scholars have deployed it in the past and in various contexts.

Despite Fancy's reading, my book does not claim that this circle of physicians was uninterested in philosophy (142). In fact, I explained its founder's interest in philosophy and mentioned his philosophical training.<sup>17</sup> I also cited Fancy's work on Ibn al-Nafis' philosophical works.<sup>18</sup> My analysis focused on the changing medical library and how it reflected changing priorities and the growing interest in medical practice. None of this means that philosophy was unimportant or was not part of their interests. His advice that I should have relied more upon the commentaries on *al-Mūjaz* was surprising, for not only did I cite his own doubts as regards this book's authorship, which he expressed in his own book, but I also explained that I had used it sparingly and only to corroborate narratives produced in other contemporaneous treatises.<sup>19</sup> I agree, however, that I could have made both of these choices more explicit and should have discussed my examination of the group's commentaries in more detail. I also accept his suggestion to engage with Fakhr al-Dīn al-Rāzī's (ö. 606/1210) works explicitly, if only to clarify how his own works and those of his students fit within this picture. These are valuable suggestions that I would like to address in upcoming works about the same period.

Ultimately, this discussion is related to our different views about the field's direction and the nature of the work that we do. Fancy's published work has focused almost exclusively on Ibn al-Nafis and has done an admirable job of tracing his works and the commentaries upon them. Such scholarship that analyzes a particular author and his/her commentators is a most welcome addition to the library of Islamic history and Islamic studies. However, in some cases, such scholarship may leave something to be desired in terms of how such historical texts impact the larger context of scientific knowledge in a given society. It also stops short of telling more consequential stories about the intellectual community, patronage

17 Ibid., 144-45

18 Ibid., 153, 157, 163.

19 Ibid., 211-12.

structures, and sociocultural contexts in which these works were produced or how they were received beyond a limited circle of commentators, and it often remains confined to a readership interested in these particular authors without serious engagement with major questions that animate larger fields of study—and not only in the Islamicate context.

For instance, the growing field of science and religion, in which I locate my work, has posed serious questions about intellectual authority, its sources and sociocultural implications, the meaning of scientific and religious authorities in society, among many others. The field of history of science continues to ask important questions about practice, scientific authority, trust and communication, professionalization, and the meaning of nature—as well as about the meanings of objectivity and neutrality. Scholars in religious studies discuss important questions related to piety, rituals, the meaning(s) of sacred texts, exegesis and hermeneutics, multi-religiosity, and communications in theological investigations. These concerns underlie a lot of my thinking and scholarly engagement in this book and beyond, which I hope is evident to the reader through my research questions and argumentation.

In my work, whether on the *Medieval Islamic Hospital* or beyond, I have sought to look at the unfolding cultures of science and religion within their social contexts. I look at epistemic practices both as debates and negotiations of authority, as well as ritualistic practices that construct myths, virtues, and values. I study the role played by history and historical narratives in constructing individual and collective identities not only in the medieval and early modern contexts, but also in the colonial and postcolonial contexts. Such work builds upon the work of scholars such as Emilie Savage-Smith, Peter Pormann, Charles Burnett, and Efraim Lev in the field of Islamic medicine; William Graham, Robert Wisnovsky, Khaled El-Rouayheb, Baber Johansen, Andrew Rippin, and Harald Motzki in Islamic studies; Katharine Park, Joan Cadden, and Monica Green in history of medieval sciences; Steven Shapin, Lorraine Daston, Peter Galison, and Michael Gordin in history of science; and Catherine Bell, Mary Douglas, Michael Taussig, Mark Jordan, and Amy Hollywood in religious studies, to name but a few. In all these works, scholars pay attention to how specific texts speak to their environment. More importantly, they understand that written records show no more than a fraction of a particular society's intellectual life and seek to piece together many sources of evidence in order to reconstruct an image of the practice of scientific knowledge. What Fancy rejects as the use of literary sources to discuss medical practice is, in fact, an attempt to form a more holistic image of how people in a particular period perceived hospitals and

understood medical practice. What he might have noticed, but failed to indicate in his review, is that I never took these sources literally, but rather utilized them to provide a rich textual environment from which we can begin to understand social currents and cultural trends.

My published scholarship, like that of the aforementioned scholars and many others, is interested and invested in a deep engagement with the larger questions addressed in the history of science and medicine, in religious studies, and in science and religion. It goes beyond a textual analysis of the writings of a few scholars and their commentators. Instead, it attempts to engage with the larger questions related to practice, pharmacopeia, teaching, authority and its sources, practices of authorship, subjectivities, social fashioning, rituals, sensoria, and the processes that construct narratives and discourses. This also means that we have a responsibility as scholars to make our work legible and accessible to other scholars not working on our immediate subfield. This is not an invitation to sloppy analysis or superficial engagement. Instead, it poses important editorial responsibilities as to how we can frame our work and communicate the results of our research. These questions become all the more important as scholars working in the humanities and social sciences come under political and intellectual attacks, and our students face the pressures of job markets (in the academy and beyond) that require innovation, engagement, and wide-ranging expertise. The widely cited works of Graham, Savage-Smith, Pormann, and Robert Morrison, among many others working on medieval Islamic societies outside their own home disciplines, show how one can write an engaging and influential work that transcends the limits of period and seeks to invite larger scholarly conversations. Again, this by no means should take away from the depth of a given scholarly work. In fact, engagement across disciplines, regions, and periods allows for deeper investigations and more effective and sharper questions.

As such, Fancy misses some of my book's central arguments about who the *Bīmāristān*'s patients were, who the targets of charity could have been, and how patrons conceived of medicine and of hospitals as part of their charitable work. He also misses key narratives about the hospitals' embeddedness in their local environments and how they expressed and responded to local needs, how local perceptions of hospitals and their roles affected local trajectories, and how patients experienced the architectural details and the sensorium that they inhabited inside the hospital. Undoubtedly my book has its shortcomings, not least because it is the first monographic study on medieval Islamic hospitals. As is the experience of many authors, I became aware of some of these shortcomings immediately after it was too late to fix them. I hope to learn about other shortcomings from my colleagues and students.

Once again, I regret that Fancy's comments were framed in a rather hostile narrative and that his valid points were sometimes placed alongside irrelevant ad hominem attacks. However, I do welcome his engagement and suggestions, as well as this opportunity to engage in a larger discussion about how we write the history of science and about science and religion in the Islamic world. In this response, I have engaged with only some of his more significant mischaracterizations. I chose not to comment on each of his points out of respect for the space limitations and because I consider it more productive to engage with larger and more consequential questions.

Finally, I am grateful for this opportunity to discuss my book at such length. I hope that my response represents the beginning of deeper discussions that will both transcend my book and stimulate our thinking and work. I hope to continue to engage with Fancy and others in such discussions, but I do not, after this, intend to engage in further commentaries on how to read my book. My hope is that readers will be able to consider these questions independently as they read it themselves.