This is a portion of the eBook at doi:10.7551/mitpress/14667.001.0001

Notes

Chapter 1

1. I use both "I" and "we" throughout the book. "I" indicates what I see as my individual contribution to the project, which consists primarily in its framing and in drawing on, and extending, analyses I have developed in previous research. "We" signals the inherently collaborative nature of the project, including data collection and analysis. As I said in the acknowledgments, it is impossible to disentangle specific contributions in our analyses. However, when another group member contributed significantly to a specific analysis and interpretation, I will note that they should be credited as co-analyst.

2. I discuss our rationale for investigating bioengineering sciences practices later, but here I note that I am including them under the rubric of "scientific practice" because I am concerned, mainly, with their epistemic practices. These hybrid fields aim at creating both biological knowledge and engineering applications, and so differ from conventional engineering fields in having epistemic goals (see also Boon 2011). One of our objectives has been to lay out the epistemic structure of biological engineering.

3. Woody (2014) locates the "practice turn" in philosophy in a concern with experimentation in the mid-1980s (e.g., Hacking 1983; Franklin 1989); however, there was also a simultaneous emerging concern with theoretical and conceptual practices (e.g., Gavroglu and Goudaroulis 1989; Gooding 1990; Nersessian 1984). The Science and Philosophy book series (Nijhoff/Kluwer) I created and began editing in 1984 was dedicated to the study of theoretical, conceptual, and experimental dimensions of practice.

4. Some sociocultural studies in the late 1990s also moved toward accounts that can be read as taking note of cognition, such as Peter Galison's (1997) concern with the "image" and "logic" traditions in the material culture of particle physicists, Karin Knorr Cetina's (Cetina 1999) analysis of scientific practices as part of "epistemic cultures," and Hans-Jörg Rheinberger's (1997) analysis of experimentation in molecular biology as producing "epistemic things."

Downloaded from http://direct.mit.edu/books/oa-monograph/chapter-pdf/2231480/c007900_9780262372275.pdf by guest on 15 August 2024

This is a portion of the eBook at doi:10.7551/mitpress/14667.001.0001

5. Classic references for these perspectives are cognition as embodied (Barsalou 1999; Johnson 1987; Lakoff 1987); artifact-using (Clark 1998; Hutchins 1995a; Norman 1988); and situated (Greeno 1989a; Lave 1988; Suchman 1987).

6. Classic references for these perspectives are distributed cognition (Hutchins 1995a; Hollan et al. 2000; Kirsh 1995, 2001; Kirsh and Maglio 1994; Norman 1991); distributed intelligence (Pea 1993); activity theory (Cole and Engestrom 1993); situated action (Lave 1988; Greeno 1989a,b); and extended mind theory (Clark and Chalmers 1998; Clark 1998).

7. It is important to underscore that Hutchins's position is that distributed cognition is an analytical framework and not an ontological claim as advanced by Andy Clark with his "extended mind thesis" (Hutchins 2011).

8. For some time learning scientists have been examining science practices from a D-cog perspective; see especially Hall et al. (2002) and Hall et al. (2010). More recently, a handful of studies in the science studies fields and cognitive science have examined problem-solving in scientific research from a D-cog perspective (see, e.g., Alac and Hutchins 2004; Becvar et al. 2008; Charbonneau 2013; Giere 2002; Goodwin 1995), but they have not explicitly addressed the ways the framework itself needs extension.

9. Hutchins has stated in several talks I have heard that "embodied brains" are participants, without specifying what the brains contribute. Although I will not address this issue beyond what I say about the capacity for mental modeling/simulation here, such specification is an important open problem for the distributed cognition framework, since the necessity for there to be a human in the system to make it a *cognitive* system provides an important contrast with actor-network theory with which it is sometimes conflated (see also Giere 2002). Learning sciences research by Rogers Hall (Hall et al. 2002; Hall et al. 2010) and Charles Goodwin (1995), in particular, also seeks to elucidate the mental resources at work in problem-solving within the D-cog framework.

10. The literature on science listed in note 8 does attend to this dimension.

11. Kirsh and Malgio (1994), Kirsh (1995, 2001) Hall et al. (2010), and Chandrasekharan and Stewart (2007) provide important exceptions.

12. Classic references for this research are discourse and situation modeling (Johnson-Laird 1983; Perrig and Kintsch 1985; Zwaan 1999); mental animation (Hegarty 1992; Schwartz 1995; Schwartz and Black 1996); mental spatial simulation (Finke 1989; Kosslyn 1994; Shepard and Cooper 1982); and perceptual simulation in embodied mental representation (Barsalou 1999; Brass et al. 2002; Bryant and Tversky 1999; Glenberg 1997).

13. Cetina suggests that the collective knowledge in the Large Hadron Collider project could be considered as "a sort of distributed cognition" (1999, 173–174) with

Downloaded from http://direct.mit.edu/books/oa-monograph/chapter-pdf/2231480/c007900_9780262372275.pdf by guest on 15 August 2024

This is a portion of the eBook at doi:10.7551/mitpress/14667.001.0001

no further explication. As Ronald Giere (2002) has pointed out, she seems to mean "collective cognition" in the sense of Durkheim, and not in the sense of cognitive science that we have been discussing. Her notion comprises people, but not the artifacts, which perhaps explains why she does not cast the microbiology lab as a D-cog system, which on my account it is.

14. Sociological and anthropological ethnographies of research labs took off in the late 1980s (see, e.g., Latour and Woolgar 1979; Knorr-Cetina 1983; Lynch 1985; Traweek 1988) and continue to the present day (see, e.g., Roosth 2017). Cognitive science investigations began in the 1990s. Kevin Dunbar (1995) pioneered what he called the in vivo/in vitro method of investigating scientific cognition (understood in the traditional sense of individual cognition) in research labs as a source of hypotheses to then be brought into the experimental psychology lab for rigorous investigation. Cognitively oriented anthropologists and learning scientists investigated lab practices more in line with our investigations relating cognition and culture (see, e.g., Goodwin 1995; Hall et al. 2002; Ochs and Jacoby 1997). Only quite recently have philosophers begun to investigate epistemic practices in research labs through observations, as embedded participants in the research, or with interviews (see, e.g., Andersen and Wagenknecht 2013; Bechtel and Abrahamsen 2013; Bursten 2015; Carusi et al. 2012; Green 2013, 2017; Hangel and Schickore 2017; Leonelli 2016; Loettgers 2007; Sheredos et al. 2013; Wagenknecht et al. 2015). We began our ethnographic investigations in 2000.

15. Kevin Dunbar, as noted earlier, has showed the fruitfulness of ethnographic/observational methods for even the traditional cognitive science perspective. He collected data on cognitive practices of scientists in research labs and then tested his hypotheses about cognitive processes (e.g., the role of analogy in problem-solving) in controlled experiments in the psychology laboratory on nonscientist subjects (see, e.g., Dunbar 1997; Dunbar and Blanchette 2001). Dunbar's research, primarily on research labs in industry, showed a considerable amount of concern with issues of priority, as well as conflict among researchers and among researchers and labs they viewed as competitors, as one also often finds in the STS analyses of research labs. Our research has been criticized as painting the atmosphere in the labs as "too nice" or "too harmonious." However, over fifteen years of nearly daily interaction with them, our ethnographers noted little conflict and drama among researchers in these labs, or in their attitudes toward other labs conducting similar research. There were, of course, the normal frustrations and annoyances of human interaction, but these did not appear to spill over into the research arena. In general, they showed respect, good will, and a cooperative spirit when engaging in research and discussion. One possible "explanation" for the difference that comes to mind is that these labs are populated by engineering scientists, and engineers tend to be pragmatic—a conclusion I have arrived at not only from this study but also from having spent most of my career in engineering environments (MIT, Case Western Reserve, and Georgia Tech).

Downloaded from http://direct.mit.edu/books/oa-monograph/chapter-pdf/2231480/c007900_9780262372275.pdf by guest on 15 August 2024

This is a portion of the eBook at doi:10.7551/mitpress/14667.001.0001

16. The quote is from Hutchins's response to the highly favorable review of his 1995 book by Latour (in Keller et al. 1996). In it, Hutchins counters the claim made by Latour that with D-cog, cognition has been eliminated or reduced to sociocultural factors. I concur that Latour fundamentally misunderstands or misrepresents the central point of D-cog: cognitive processes comprise human cognitive capacities, material resources, and sociocultural practices, and so, in no sense has cognition been eliminated. Empirical research in environmental perspectives across the board establishes the inherently cultural nature of human cognition.

17. Although some ethnographers might object to such abstraction, including those in STS, Hutchins's position is in line with that of Geertz, who argued that ethnographic analysis has a "double task": "to uncover the conceptual structures that inform our subjects' acts . . . and to construct a system of analysis in whose terms what is generic to those structures, what belongs to them because they are what they are, will stand out" (Geertz 1983, 57).

18. The continual development of all dimensions of these research labs over many years is the reason why I have sometimes called the method "cognitive-historical ethnography."

19. Additional kinds of analyses, for example, with respect to social positioning and identity, gender, emotion and affect, epistemic identity, and learning can be found in the publications of the researchers in our group across a range of fields—for instance in the book, *Science as Psychology: Sense Making and Identity in Scientific Practice,* cowritten by Lisa Osbeck, Kareen Malone, Wendy Newstetter, and myself (Osbeck et al. 2011). The book addresses psychologists and philosophers and was awarded the William James Book Award by the American Psychological Association.

20. We had additional questions related to the learning practices and challenges in the different kinds of interdisciplinary fields as part of our NSF-funded research.

21. This research was conducted under an IRB protocol in which the participants are to remain anonymous. I have designated the labs by a letter and the researchers in each lab have been given a code, for example, lab A, researcher A10.

22. I discuss the BME labs in the past tense because both are now closed.

23. We are using the term "device" in the way the researchers in the BME labs referred to their in vitro simulation technologies. This notion differs from the notion of "inscription devices" that Latour and Woolgar (1979, 51) introduced. Their notion refers to technologies for creating figures or diagrams of phenomena. The BME devices are sites of in vitro simulation and experimentation. Further processing with instruments is needed to transform the information they provide into visual representations or quantitative measures.

24. Recently, the extent and variety of the current fields adapting ethnography and qualitative methods to their interests, goals, and epistemic norms and values led the

Downloaded from http://direct.mit.edu/books/oa-monograph/chapter-pdf/2231480/c007900_9780262372275.pdf by guest on 15 August 2024

This is a portion of the eBook at doi:10.7551/mitpress/14667.001.0001

Journal of Qualitative Research to set up a task force to develop guidelines sufficient to evaluate methodological integrity in data collection and analysis across fields when reviewing journal submissions and grant proposals. Levitt et al. (2017) provides a useful conceptual analysis of the task force recommendations as they can be used to guide both the design and review of qualitative research.

25. Each of the researchers on this project already had an interdisciplinary background when they joined. As "instruments" of data collection and analysis, our group brought a wide range of perspectives to the project: philosophy of science and history of science (physics, biology, psychology), cognitive science (AI, cognitive psychology, philosophy), linguistic anthropology, physics, mathematics, learning sciences, human-centered computing, theoretical psychology, gender studies, psychoanalysis, architecture, and industrial design. We have analyzed data through many of these lenses and have published several contributions in fields represented by the interests of group members beyond philosophy of science and cognitive science.

Chapter 2

1. In the BME labs, the researchers used the word "device" to refer to their in vitro simulation models. This led us to believe that devices were a specific kind of model. Only later, in the context of a different usage of the word in an ISB lab, did we find out that, in bioengineering, "device," in general, means any engineered artifact that interfaces with biological entities, as discussed in chapter 6.

2. See Vermeulen et al. (2009) for their refinement of Goodman's definition.

3. To be clear, I am not saying researchers in other contexts have to know the historical processes through which a device or model-system has been developed and has attained its credibility in order to use it. By the time of our research, for instance, the flow-loop device that lab A had developed was in use in many other labs and, recall, the dish model-system was developed in a lab other than lab D, and a few other labs were also using it. These researchers do have to know what features it exemplifies (or can be made to) and why this selection of features is warranted (e.g., simulates first-order forces) and to evaluate the relevance of these for their own research goals.

4. I use italics throughout to indicate I am quoting a researcher from transcript material. I use the convention of ellipses (. . .) to indicate text that has been omitted, without changing the meaning, and em dashes (—) to indicate a pause in speaking.

5. That the researchers all use "*over*" instead of "*through*" the lumen (which is tubular in in vivo) is an interesting slip they made all the time. I suspect they made the mistake because they were thinking in terms of the in vitro simulation, in which, as we will see, the tubular constructs are cut open and laid flat in the flow chamber.

Downloaded from http://direct.mit.edu/books/oa-monograph/chapter-pdf/2231480/c007900_9780262372275.pdf by guest on 15 August 2024

This is a portion of the eBook at doi:10.7551/mitpress/14667.001.0001

6. Although cumbersome, it is possible to cast this process using Hutchins's characterization of information flow in a D-cog system: the forces *generated* by the flow loop *represent* shear stresses (to a first-order approximation) as it *manipulates* the endothelial cells, which, *generates* conditioned cells that researchers *manipulate* with instruments that *generate* quantitative and qualitative information in various representational formats that *propagates* through the D-cog system as it performs a problem-solving task. We did not find Hutchins's characterization of a D-cog system generating, propagating, and manipulating representations a useful way to analyze the dimensions of the D-cog systems of the research laboratory we were interested to understand, but it was useful for constructing our diagrams of various model-systems, such as figure 2.6, which I have pared down for use here.

7. No doubt, by now the reader has noticed that researchers frequently speak of their models with anthropomorphic language. We found this to be the case across all of the labs, and not only with living entities and systems but also with computers and other technologies that performed as cognitive-cultural artifacts. And, contrary to what might be expected, novice researchers rarely used such language, but advanced researchers frequently did. We took this fact to indicate that such anthropomorphizing is not careless use of language or a sign of naivete, but rather signals a growing understanding of the artifact as a partner in problem-solving. Such utterances led us to develop the theme of "cognitive partnership," which can be formed with other researchers and with artifacts of particular salience to the research, that is, the cognitive-cultural artifacts. In the case of such artifacts, researchers often *attributed* agency to them. Such attribution by a researcher is different from the artifacts actually *having* agency (though of course they interact), as proposed by actor-network theory. We have developed the theme of cognitive partnering extensively in previous research, and I will not focus on it in this book (see Osbeck and Nersessian 2006; Osbeck and Nersessian 2013; Osbeck et al. 2011). There we also demonstrate the researchers' affective engagement with the entities and objects of their research we witnessed, namely cells and models, does not taint science, but rather helps to make it possible.

8. Anecdotally, I was seated next to a neuroscientist during a presentation by the lab D director to a cognitive science audience. Afterward I asked him what he thought of the research program. He responded along the lines that although the dish was a tremendously simplified model, if they could induce learning in the network without all the other parts of a brain neuroscientists believe are necessary for learning, they would have demonstrated something very important for the field.

9. MEAs currently in use in the field have around 26K electrodes and allow researchers to do more finely grained recording of the activity of individual neurons.

10. When MEArt was exhibited as mechanical art installation, the researchers called it "The Semi-living Artist," and described it as follows in the exhibits: "A geographically detached bio-cybernetic research and development project exploring aspects of creativity and artistry in the age of new biological technologies from both artistic

Downloaded from http://direct.mit.edu/books/oa-monograph/chapter-pdf/2231480/c007900_9780262372275.pdf by guest on 15 August 2024

This is a portion of the eBook at doi:10.7551/mitpress/14667.001.0001

and scientific perspectives. The installation is distributed between two locations in the world. Its brain consists of cultured nerve cells that grow in a neuro-engineering lab in the US. Its body is a robotic drawing arm in Australia (or wherever exhibited) that is capable of producing drawings. They communicate via satellite. The brain and the body communicate in real time with each other for the duration of an exhibition" (https://www.symbiotica.uwa.edu.au/).

11. Hesse called these features "properties," but I use "features" to capture that properties, relations, and relational structures, as well as behaviors can be mapped.

12. Douglas Hofstadter provides an exception. Although his creative representation-building AI programs are quite simple, he does argue that these processes are a significant dimension of analogy in both mundane and scientific analogies (see, e.g., Hofstadter 1995). In my 2008 book, chapters 5 and 6 provide a discussion of the issue of representation-building in analogy with respect to the cognitive science literature.

13. As will be seen in chapters 3, 5, and 6, much of this account can be extended to building computational models of complex biological systems.

14. In my 2008 book I advocated that although the word "abstraction" is commonly used for separate processes alongside "idealization" and other abstractive notions, this is confusing. It is better to reserve "abstraction" for a comprehensive notion comprising various processes, including idealization, approximation, simplification, limiting case, and generic modeling. All of these processes can play a role in model-building as a means to manage the complexity of modeling biological systems.

15. This concentration was equally important to our educational research goals, since building devices for the purpose of model-based simulations is their primary epistemic practice.

Chapter 3

1. I introduced the view of conceptual change in science as a problem-solving process early in my research on the field concept (Nersessian 1984). Only later, in the course of my research into cognitive science, did I discover that the Russian psychologist Lev Vygotsky (1962) held a similar view about mundane concepts. In characterizing concept formation during learning (acquisition of culturally extant concepts), Vygotsky argued that a concept emerges and takes shape in the course of a complex operation aimed at the solution of some problem. He also advanced the notion that concept formation is an ongoing dynamic and sociocultural process in each use or acquisition of a concept. We both hold that concepts are neither completely fixed units of representation nor solely mental representations, but arise, develop, and live in the interactions among people as they create and use them.

2. Christopher Patton was trained as an ethnographer on this lab and had primary responsibility for data collection over the course of the years discussed here. He was the one who alerted us that something important seemed to be taking place with

Downloaded from http://direct.mit.edu/books/oa-monograph/chapter-pdf/2231480/c007900_9780262372275.pdf by guest on 15 August 2024

This is a portion of the eBook at doi:10.7551/mitpress/14667.001.0001

the building of the computational model, which enabled us to capture additional relevant data as the case and our analysis of it unfolded. He and I did the first analysis of the case together. I later did a reanalysis, based on additional interview data, with Sanjay Chandrasekharan, and what I present here is an elaboration, which incorporates more data, on the work of the three of us.

3. See, especially, Chandrasekharan (2009), Chandrasekharan and Nersessian (2015), Chandrasekharan et al. (2012), Nersessian (1991b, 1992a,b, 2002, 2008).

Chapter 4

1. Gerson (2013) rightly cautions against unreflective appropriation of biological metaphors to analyze culture. However, Wimsatt (2013a) argues for the appropriateness of using the analogy of generative entrenchment in a biological ecosystem for cultural evolution because in those processes, too, there are "multiple evolving and interdependent lineages acting on different time and size scale" (564), which fits with my characterization of the research lab as an evolving, distributed cognitive-cultural system with epistemic goals.

2. We have published case studies of the building processes of the mechanical tester (Nersessian et al. 2005) and the compression bioreactor (Harmon and Nersessian 2008).

3. This discovery led us to apply to NSF for a small supplemental grant and also to the Spencer Foundation to investigate issues pertaining to gender and to race in BME epistemic cultures. Our research scientist, Kareen Malone, took the lead on that research, holding focus groups with BME students and faculty, as well as conducting targeted interviews with lab members on the topics. Some of our findings are discussed in Osbeck et al. (2011, chapter 6) and Malone et al. (2005).

4. Chapter 7, "The Learning Person," in *Science as Psychology* (Osbeck et al. 2011) provides an extended case study of her development as a BME researcher within lab A.

5. As a field, biomedical engineering had been in existence since at least the early 1960s, but there were a few established departments circa 2000, most notably at the Johns Hopkins University (est. 1962). Since this is not a historical account of the field, I present the situation as the founders of this new department expressed it to us. The notion that they aspired to become an interdisciplinary discipline, an "interdiscipline," came from us, but they embraced it immediately and started using it in their proposals and publicity. Indeed, in both studies, part of what we did was to provide them ways to conceptualize their educational and research aims and practices. One of the senior faculty called these ways "Nancy-speak" and "Wendy-speak."

6. Although I do not articulate it here, there are significant insights to be gained regarding the explicit formation of interdisciplinary research fields—characteristic of much late twentieth and early twenty-first-century science and engineering—by

Downloaded from http://direct.mit.edu/books/oa-monograph/chapter-pdf/2231480/c007900_9780262372275.pdf by guest on 15 August 2024

This is a portion of the eBook at doi:10.7551/mitpress/14667.001.0001

examining them through the lens of research on "social movements" (Leonelli 2019). In the case at hand, the call of these researchers for a new breed of biomedical engineer suited for the twenty-first century came well in advance of an articulated means to carry out the objective. It was a collective normative vision, the broad outlines of which were announced to the administrations of the schools involved, the wider intellectual community, funding agencies, and prospective donors. Importantly, it made a bid to reshape the epistemic practices of a field, which the leaders felt needed to move beyond collaboration to hybridization in order to meet specified goals for twenty-first-century BME.

7. Interestingly, they did not get that NSF ERC, but decided to proceed with what they dubbed "a cognitively informed" educational program anyway. The gamble paid off in that in approximately five years they went from nonexistent to the number-two-ranked BME department in the *US News and World Report* rankings, eventually taking over the number one spot, over such rivals as the long-established department at Johns Hopkins, as well as departments at MIT and Stanford. Twelve years after the program started, the program received a State Regents' Award for the best university educational program in the state. The program also received the 2019 Bernard M. Gordon Prize for Innovation in Engineering and Technology Education from the US National Academies of Engineering. These prestigious awards provide validation for the "translational approach" pioneered in our research, as well as for the educational program itself, which has conducted longitudinal evaluations of its outcomes.

8. The BME-dedicated building was under construction as we began to plan the implementation of the PDL approach. Because of the envisioned introductory PDL course, they decided to construct five specially designed classrooms with a seating structure and floor-to-ceiling whiteboards surrounding the room to facilitate interaction among the participants. Since we were doing research on the courses, two rooms were equipped with a separate observational window and recording compartment for us. They recognized that the plan for students to work in groups of eight with a facilitator was costly from the outset, but the educational experiment is seen as so successful by the administration that it has continued to support the model despite the significant growth in the student population. In recent years more than 160 undergraduate students are enrolled in a semester, with facilitators needed for twenty teams, plus for the graduate courses.

9. The department has continued to hire its own cognitive and learning scientists to provide support for ongoing curriculum development.

Chapter 5

1. The analysis developed in this chapter draws significantly on research conducted together (individually) with Miles MacLeod and with Sanjay Chandrasekharan, who were postdoctoral researchers on the project.

Downloaded from http://direct.mit.edu/books/oa-monograph/chapter-pdf/2231480/c007900_9780262372275.pdf by guest on 15 August 2024

This is a portion of the eBook at doi:10.7551/mitpress/14667.001.0001

2. Some of these strategies are in widespread use in all kinds of modeling. However, the modelers we studied transferred and adapted them from their use in the engineering fields in which they were trained or had developed familiarity with.

3. See Stuart and Nersessian (2019) for a discussion of a novel attempt by modelers, in a different systems biology lab that Michael Stuart and I investigated, to mitigate the collaboration problem by developing computational visualizations of what is going on inside the black box that would be comprehensible to experimentalists, as well as other modelers not involved in the building process.

4. There are significant parallels between what these modelers are doing in mathematizing causal relational structures in biological networks and what Maxwell did in constructing a mathematical representation of the causal relational structure between electricity and magnetism that produces the dynamical behavior of the electromagnetic field, without specifying the underlying causal mechanisms. This strategy proved especially productive in the Maxwell case, as it was later understood that the field is a nonmechanical dynamical structure. For a detailed analysis, see Nersessian (2008, chapter 2).

5. BST was developed, initially, by Michael Savageau (1969a, 1969b, 1970), who is a pioneer in the application and adaptation of systems engineering concepts and methods to biological systems.

6. Most members of lab G are not native English speakers, and so many of the quotes I use are ungrammatical.

7. Quite recently, Lenhard (2020) has discussed some of these roles in physics-based simulation. The account he develops relies on publications and anecdotal evidence, and some post hoc interviews, and not in situ examination of modeler practices.

8. Here I elaborate on a case study analysis originally developed and written with Sanjay Chandrasekhran, who should be considered its coauthor.

9. See Wendy S. Parker (2013) for a discussion of ensemble models in climate science, which comprise different models rather than the same model fitted with different parameters.

10. There are other fields in which model-building often does not have a theoretical starting point. For example, Parker (2013) has pointed this out with respect to modeling in behavioral and social sciences, and Peck (2008) has also shown this for modeling in ecology. Our characterization of “modeling from the ground up” should be distinguished from Keller’s (2003) notion of “modeling from above.” As Keller describes it, the latter is a strategy that aims to simulate the phenomenon itself, not by trying to map its underlying causal structure or dynamics, but rather by generating the phenomenon from a simple yet artificial system of interactions and relations. What we call modeling from the ground up is what she would call “modeling from below” in that it relies on information about the causal structure

Downloaded from http://direct.mit.edu/books/oa-monograph/chapter-pdf/2231480/c007900_9780262372275.pdf by guest on 15 August 2024

This is a portion of the eBook at doi:10.7551/mitpress/14667.001.0001

and dynamics of a system's compositional elements. Both however can begin from nontheoretical starting points.

11. Concurrent with our research, some philosophers of biology have begun to examine how modeling works differently in systems biology than in physics. This research largely focuses on issues of mechanistic explanation and template development (see, e.g., Bechtel 2011; Brigandt 2013; Levy and Bechtel 2013; Serban and Green 2019). It is clear that systems biology provides a rich domain with which to expand philosophical understanding of computational modeling and simulation. The kind of mesoscopic model-building practices we have studied, especially in lab G, for instance, tend to provide understanding that, while making use of mechanistic information, does not provide mechanistic explanation (MacLeod and Nersessian, 2015).

12. I, by contrast, have been arguing (since Nersessian 1984) that such considerations need to inform philosophical accounts of scientific practices, generally.

13. Scientific thought experimenting is one form of possible-worlds thinking. We have posited that, with the advent of computational simulation, in many scientific fields what are customarily called thought experiments can largely be supplanted, or reduced to a minimal role, by in silico simulation models (Chandrasekharen et al. 2012).

14. See MacLeod and Nersessian (2019) for a detailed analysis of the mesoscopic strategy.

15. Similar observations, however, have been made with respect to agent-based modeling in ecology. This is not surprising, given the comparable complexity of the problems and lack of domain theory that characterize both fields. As with Peck's point that "there are no formal methodological procedures for building these types of models suggesting that constructing an ecological simulation can legitimately be described as an art" (2008, 393), our modelers, too, describe their modeling practices as, in part, "art." Likewise the ISB modeling we have observed is an individual project in which each modeler chooses the methods and strategies he or she thinks best resolve the problem without any formal procedure governing the process, though often, for novices, in discussion with someone with greater expertise. A major benefit of an ethnographic approach is that it exposes the often hidden, creative choices that are "rarely disclosed in formal descriptions of model-building" (Peck 2008, 395). These parallels with ecology suggest that there is a deeper commonality among the methodologies employed across these kinds of simulation-building contexts. Empirical investigations such as ours help to broaden understanding of the range of scientific practices involved in the methodologies of computational modeling and simulation.

16. Although I am not attending to it here, our investigations show that the affective experiences of researchers in relation to one another and to the entities and artifacts that are part of their research are important dimensions of the cognitive-cultural

Downloaded from http://direct.mit.edu/books/oa-monograph/chapter-pdf/2231480/c007900_9780262372275.pdf by guest on 15 August 2024

This is a portion of the eBook at doi:10.7551/mitpress/14667.001.0001

system that create and sustain epistemic practices. Thinking about this issue led us to note the affective dimension of our own language of "coupling" and cognitive "partnering" (Osbeck and Nersessian 2006). We have used "coupling" guided consciously by previous work in cognitive science, but perhaps unconsciously by cultural conceptions of partnering and coupling as the "joining of two persons into one." The "two as one" notion does come close to expressing the kind of relationship of cognitive and cultural domains that enables these to be understood as a single system, each intimately implicated in the other.

Chapter 6

1. It appears to have been a risk worth taking. I have followed the work of the lab and career of the director at a distance for over five years since we ended our investigation, out of curiosity to see how things were turning out. The lab has produced some significant discoveries, has numerous high-level publications, is well-funded, has many more students and postdocs, and now the lab director, who had just received tenure when we left, has been promoted to full professor with an endowed chair.

2. Although the phenomenon of cell signaling was discovered in 1855 by Claude Bernard when he found that certain "secretions" released into the bloodstream had effects on distant cells, the process was not conceptualized as "cell signaling" until the 1970s. According to Nair et al. (2019) "the word 'signal transduction' appeared in biological literature in the 1970s, further elucidation of which was provided by Martin Rodbell in 1980 who postulated that 'individual cells were cybernetic systems made up of three distinct molecular components: discriminators, transducers and amplifiers'" (2). Although biologists use a variety of terms today (reception, transduction, response), this early terminology shows the role of cybernetic and control engineering in the formation of the biological concept.

3. Modelers in both labs complained about the fact that molecular biologists often see computational model representations as too abstract, while they interpret Michaelis-Menten kinetics as providing direct representations of mechanistic reality. In fact, as modelers point out, Michaelis-Menten is itself an abstraction based on various simplifications and assumptions applied to a mass-action representation. It is a mathematical model of the rate at which enzymes catalyze a particular reaction. As C7 expressed the complaint, "*For example, there's a very famous equation that's just called the Michaelis-Menten equation. It's supposed to represent [enzyme] kinetics. But that is an approximation. Most biologists, you know, do not realize that. And they have it in their subconscious that, well, that this is a precise representation of the exact kinetics, and if I were to use a more basic representation of which Michaelis-Menten is an approximation, they would not trust that.*"

4. Our research goals required that we understand the research conducted by the labs we investigated in sufficient detail so as to be able to discuss the science,

Downloaded from http://direct.mit.edu/books/oa-monograph/chapter-pdf/2231480/c007900_9780262372275.pdf by guest on 15 August 2024

This is a portion of the eBook at doi:10.7551/mitpress/14667.001.0001

modeling, and technology in our interviews. Lab C's intensive and complex biological research, complete with what I deemed its "alphabet soup" terminology, provided our group the most significant challenge of all the labs. I had had sufficient background in physics, computer science, and neuroscience to be able to guide our group in learning what was necessary to probe the participants in the other labs on their research. My lack of knowledge of biology enabled me to understand firsthand some of the challenge lab C and lab G researchers faced as they encountered the alphabet soup. We were fortunate to have in our group, at that time, a graduate student, Vrishali Subramanian, with a background in biosciences, who wanted to be trained to do ethnography to use in her research on environmental policy. She helped us understand the biological content of the specific research, as well as make sense of what we were reading in the recommended immunology textbook, during the first two years of our investigations.

5. After hearing a talk by us that mentioned how they used such language to talk about the cells and some technologies with which the cells interfaced (e.g., Bio-Plex), they discussed how they were unaware that they did so. One researcher called it "*creepy*," and said this was treating the cells "*like [she did] the Muppets*," but she also stated that she found herself "*unable to stop doing it*."

6. In an interview following the meeting, C11 explained that she thought she had been able to come up with the solution to the problem that was stumping C10 because biologists think differently from engineers. As she phrased it, "*She's like 'I calculated, I did the calculations correctly!' Whereas I thought 'yes, but the cells going through this pathway, in this path [zigzag], that would make the cells unhappy' because I think like a biology* [sic]."

7. Here I present a case study analysis originally developed and written with Miles MacLeod, who should be considered its coauthor.

8. C9's paper-writing strategy was that she would begin to develop a paper as she was in the process of conducting experiments or building a model.

9. There has been some recognition of the system-like nature of modeling, simulation, and experimentation in computational biology. In particular, Carusi et al. (2012) argue that these should be viewed as forming an "MSE" system, since each interacts with the other in the discovery process. Although they are correct about the system-like nature, theirs is a generic methodological account that could be used to describe either a unimodal or bimodal strategy. They do not provide detailed examination of such interactions in case studies and do not discuss the novel bimodal strategy we examine here.

10. Although there is no cognitive science literature on embodied engagement by scientists in conducting wet-lab experimentation, there is a substantial literature on how embodiment in mundane experience informs conceptual understanding that I think relevant (see, e.g., Barsalou 1999; Glenberg 2010; Johnson 1987; Lakoff 1987;

Downloaded from http://direct.mit.edu/books/oa-monograph/chapter-pdf/2231480/c007900_9780262372275.pdf by guest on 15 August 2024

This is a portion of the eBook at doi:10.7551/mitpress/14667.001.0001

Prinz 2002). See also the research of David Kirsh (2010) on dance movements as a form of thinking and the study by the anthropologist Natasha Meyers (2015) on the bodily enactments by protein crystallographers of the molecules for which they are constructing three-dimensional models, from a cultural theory perspective.

11. Note our use of "localization" here differs from the use of the term by Bechtel and Richardson (2010), Wimsatt (2007), and others. Our emphasis is on localization of errors or inaccuracies in a network rather than localization of function, although the former often serves to help reveal the latter, as it did for C9.

Chapter 7

1. A fictional character created by Patton who interjects stories and insights to lighten the task of wading through more than six hundred pages on qualitative methods. The name is pronounced "How come."

2. There are some notable exceptions in philosophy and cognitive science, most of them recent. These include Andersen (2010), Andersen and Wagenknecht (2013), Brigandt (2013), Christensen and Schunn (2007), Dunbar (1995, 1999), Goodwin (1995), Grüne-Yanoff (2011), Hall et al. (2002), Hall et al. (2010), O'Malley et al. (2007), O'Malley and Soyer (2012). Additionally, philosophers have begun to attend specifically to what interdisciplinary "integration" means in cases of contemporary and historical science; see, for instance, Andersen (2016), Green and Andersen (2019), Griesemer (2013), Leonelli (2013), Love and Lugar (2013), O'Malley and Soyer (2012), O'Rourke et al. (2016), Plutynski (2013).

3. A broad characterization of interdisciplinary research, as proposed by the US National Research Council, is considered standard: "A mode of research by teams or individuals that integrates information, data, techniques, tools, perspectives, concepts, and/or theories from two or more disciplines or bodies of specialized knowledge to advance fundamental understanding or to solve problems whose solutions are beyond the scope of a single discipline or field of research practice" (NAS, NAE, and IM 2005, 26).

4. An invitation by the Council of Graduate Schools to give a presentation at their annual meeting (2012) about what characteristics foster creativity in interdisciplinary research and how these might be cultivated in graduate education provided the initial context for my thinking about what I now call "interdisciplinary epistemic virtues."

5. Kevin Dunbar's (1995) findings about the relations between creative analogy use and innovative outcomes in biology research labs provides a nice example of cognitive flexibility as a feature of a problem-solving system. He found that in labs where researchers had homogeneous backgrounds, they made fewer analogies in collaborative problem-solving sessions than those where researchers had more heterogeneous

Downloaded from http://direct.mit.edu/books/oa-monograph/chapter-pdf/2231480/c007900_9780262372275.pdf by guest on 15 August 2024

This is a portion of the eBook at doi:10.7551/mitpress/14667.001.0001

backgrounds. As he tracked the research, the heterogeneous labs produced more innovative outcomes.

6. How to distinguish the notions of interactional and contributory expertise has been the subject of an extensive debate in the literature that I need not consider for my purposes here (see, e.g., Andersen 2016; Collins and Evans 2015; Collins et al. 2007; Collins et al. 2016; Goddiksen 2014).

7. When we began our research, the faculty talked about interdisciplinary integration but later adopted our language of "interdiscipline" for their aspirations for the field. As they phrased it in a proposal submitted for an award, "Many educational programs in BME might be described as 'engineering with a little biology thrown in.' We maintain that practitioners for the twenty-first century need to be trained in a truly integrative fashion. BME is best understood as an "interdiscipline"—that is, a field that is inherently interdisciplinary. BME is situated at the intersection of three disciplines: biology, engineering, and medicine. All three are essential to the practice of a biomedical engineer."

8. Although I do not elaborate here, in our in-depth analyses of affordances and challenges in interdisciplinary practice in both BME and ISB, we found it useful to draw from *positioning theory* as developed in social psychology to analyze the ways in which participants talk about themselves and one another. "Positioning theory is a contribution to the cognitive psychology of social action. It is concerned with revealing the explicit and implicit patterns of reasoning that are realized in the ways that people act toward others" (Harré et al. 2009, 5). Our analyses have extended the theory to consider positioning in relation to epistemic identity and the epistemic effects of positioning strategies in scientific communities (see, e.g., Osbeck and Nersessian 2010, 2012, 2017; Osbeck et al. 2011).

Downloaded from http://direct.mit.edu/books/oa-monograph/chapter-pdf/2231480/c007900_9780262372275.pdf by guest on 15 August 2024

This is a portion of the eBook at doi:10.7551/mitpress/14667.001.0001

Downloaded from http://direct.mit.edu/books/oa-monograph/chapter-pdf/2231480/c007900_9780262372275.pdf by guest on 15 August 2024

This is a section of doi:10.7551/mitpress/14667.001.0001

Interdisciplinarity in the Making

Models and Methods in Frontier Science

By: Nancy J. Nersessian

Citation:

Interdisciplinarity in the Making: Models and Methods in Frontier Science
By: Nancy J. Nersessian
DOI: 10.7551/mitpress/14667.001.0001
ISBN (electronic): 9780262372275
Publisher: The MIT Press
Published: 2022

The open access edition of this book was made possible by generous funding and support from MIT Press Direct to Open

Downloaded from http://direct.mit.edu/books/oa-monograph/chapter-pdf/2231480/c007900_9780262372275.pdf by guest on 15 August 2024

MIT Press Direct

© 2022 Nancy J. Nersessian

This work is subject to a Creative Commons CC-BY-ND-NC license. Subject to such license, all rights are reserved.

The MIT Press would like to thank the anonymous peer reviewers who provided comments on drafts of this book. The generous work of academic experts is essential for establishing the authority and quality of our publications. We acknowledge with gratitude the contributions of these otherwise uncredited readers.

This book was set in Stone Serif and Stone Sans by Westchester Publishing Services.

Library of Congress Cataloging-in-Publication Data

Names: Nersessian, Nancy J., author.
Title: Interdisciplinarity in the making : models and methods in frontier science / Nancy J. Nersessian.
Description: Cambridge, Massachusetts : The MIT Press, [2022] | Includes bibliographical references and index.
Identifiers: LCCN 2021061880 (print) | LCCN 2021061881 (ebook) | ISBN 9780262544665 | ISBN 9780262372268 (epub) | ISBN 9780262372275 (pdf)
Subjects: LCSH: Biotechnology—Methodology. | Bioengineering—Methodology. | Biotechnology—Research—Case studies. | Bioengineering—Research—Case studies. | Biotechnology laboratories. | Scientific surveys. | Interdisciplinary research.
Classification: LCC TP248.24 .N47 2022 (print) | LCC TP248.24 (ebook) | DDC 660.6—dc23/eng/20220720
LC record available at https://lccn.loc.gov/2021061880
LC ebook record available at https://lccn.loc.gov/2021061881

Downloaded from http://direct.mit.edu/books/oa-monograph/chapter-pdf/2231480/c007900_9780262372275.pdf by guest on 15 August 2024