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Tools for the Study of Scientific Practice

Scientific knowledge production aims to make sense of a chaotic, unruly world. Fundamentally, it is a cultural elaboration of a process that cognitive scientists and anthropologists alike casually refer to as "meaning construction," or "meaning-making." Communities engaged in experimental science are situated within complex environments that support a myriad of tasks and goals. Inquiry about how meaning and insight arise from these interactive systems should prove fruitful for what Bourdieu once called "the science of science" (2004: 5). This study is both an ethnographic and theoretical contribution to such a project. It is the product of a two-year-long ethnographic engagement, starting August 2013, with a group of life scientists at the Sea Lice Research Centre (SLRC) in Bergen, Western Norway. The main associates in my story were instrumental in developing a novel experimental system for researching a fish parasite with the scientific name *Lepeoptheirus salmonis*, commonly known as "salmon louse" or "sea louse." Using a wide range of techniques from the molecular life sciences, my interlocutors hope to harness new biological knowledge about the organism's roughly 13,000 genes, to bring this resilient parasite under control before it causes more problems for salmonid mariculture. While the community is working

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toward such applications, they are also producing fundamental insights about the molecular parasitology of this remarkable organism (Fig. 1.1).

On one level, this case study of experimental science can be read as a contribution to the comparative anthropology of knowledge (Barth, 1992, 2002; Cohen, 2010; Crick, 1982). This is an anthropological project in the broad sense, seeking to understand humans as a knowledge-making species, a product of an "indissoluble" relation between minds, bodies, and environment (Marchand, 2010). According to Fredrik Barth, the task of an anthropologist of knowledge is to analyze "the content of an aggregate tradition of knowledge: the variety of ideas it contains, and how they are expressed; the pattern of their distribution, within communities and between communities; the processes of (re)production in this tradition of knowledge, and how they may explain its content and pattern of distribution; thus the processes of creativity, transmission and change" (1990: 1).

Knowledge, as Michael Lambek once remarked, is a productive focal point for anthropologists because the concept bridges a chasm between the ideal and material, subjective, and objective (1993). Knowledge has material effects in the world, is embodied in artifacts and actions, and distributed unequally in groups. The topic also intersects with that ill-defined complex known as the "problem of meaning," how meanings arise, develop, its transmission and reconstruction. Shore noted that the problem of meaning arises because meanings are "twice-born": they are publicly instituted as the meaning *of* something, but also have a parallel life as idiosyncratic meanings *for* particular individuals in specific contexts (Shore, 1995). Solving the problem of meaning through a naturalistic account of culture thus requires a story about the *interactive* nature of public and private representations.

Barth made the observation that knowledge always comes in three modalities: "a substantive corpus of assertions, a range of media of representation, and a social organization" (2002: 1). But while the Barthian approach to knowledge was productively wedded to a naturalistic attitude toward culture and society (1992), it did not cross-fertilize much with developments in psychological and cognitive anthropology, which takes the acquisition and use of implicit and explicit knowledges as its main subject matter. More recently, anthropologists have

Fig. 1.1 Gravid female adult lice, 8–18 mm long, with egg-strings clustering on salmon specimens (photos courtesy of Lars Hamre)

offered programmatic statements arguing for a closer engagement with neighboring disciplines that share the subject of human knowledge, by rethinking psychological and cognitive anthropology (Astuti & Bloch, 2012; Beller et al., 2012; Bender et al., 2010; Maurice Bloch, 2012). This book imagines itself as belonging to this venerable lineage of ethnographic research on human lifeworlds, a vast terrain of scholarship that has cast light on the interplay between institutional structures, enculturated minds, and embodied action by defiantly crossing disciplinary boundaries wherever necessary to answer analytical questions (Quinn, 2018). As such, this work on the anthropology of knowledge should be read as a contribution toward interdisciplinary rapprochement.

Throughout this ethnography, I approach knowledge production in experimental parasitology as fundamentally *cognitive* practices, involving the transformation and propagation of different kinds of representations. At the same time, I want to avoid a prematurely "internalist," or "mentalist" account of knowledge, that omits social and material dimensions which are central for understanding the growth of science. This challenge has been articulated by Alač and Hutchins (2004: 630). They consider cognitive processes playing out between human agents and their social and material environment to be an underappreciated domain of phenomena, ripe for exploration through a new kind of cognitive anthropology. Observing that such epistemic actions are always embedded in culturally constructed environments of practice, they are both fundamentally cognitive and amenable to ethnographic analyzes, such that "*careful examination of these interactions reveals action as cognition*" (ibid., emphasis by authors).

A primary objective is to show how analytical strands within this new kind of cognitive anthropology, specifically the framework of "distributed cognition" and its companion method "cognitive ethnography," can be deployed to make sense of how systems of experimentation become the real working units of the contemporary life sciences. This connects real time, ethnographic snapshots from the lab with work on the history, philosophy, and social aspects of experimental practice. My analysis builds on two working assumptions. The first being that scientific knowledge is a historical product of communities of interacting people and various material artifacts. Secondly, I assume that knowledge production involve cognitive processes such as memory, decision-making, learning, problem-solving, communication, and language. These are culturally constituted activities where production, transformation, and distribution of representations are central.

Reconciling cognitive and social accounts of science has been controversial and is a risky project that is likely to come under critique from at least three sides (Heintz, 2004: 392). From the perspective of rationalists, who want to imbue science with a special ontological status as a truth-seeking enterprise, isolated from other spheres of influence, reconciliating the social and cognitive should raise strong objections, simply because what constitutes scientific thinking and sensemaking is likely too complex to productively analyze. Representatives from certain schools of thought within science studies are also likely to object. The gist of this objection can be identified in the work of Latour and Woolgar, who famously issued a ten-year moratorium on cognitive explanations in their 1986 postscript to *Laboratory Life*: "If our French epistemologist colleagues are sufficiently confident in the paramount importance of cognitive phenomena for understanding science, they will accept the challenge. We hereby promise that if anything remains to be explained at the end of this period, we too will turn to the mind!" (1986: 286). The fact that Latour "lifted" the moratorium a decade later (see J. D. Keller et al., 1996), might ease some skeptics.

Integrating cognitive and social studies should also raise objections from scholars who disdain talk about cognition as a relic of positivist epistemology, one magically transposing normative rationalist and positivist models into the heads of scientists a priori. But it is fallacious to equate cognition with rationality. Rather than presume rationality it is, as Heintz has spelled out, possible to restate the question of scientific cognition anew by analyzing it as the mechanisms and properties that underpin and sustain diverse scientific cultures, and not as patterns of thought that automatically results in true beliefs (2004: 394). The aim, then, is not to discover "the essence of science," but to investigate how the cognitive and social apparatus of science are together situated in various contexts and produce those cultural phenomena that appear throughout the history of science (ibid.). The untapped potential that lies in combining the explanatory powers of cognitive and social approaches to scientific knowledge production, and thus helps navigate the pitfalls of cognitive and sociocultural reductionism (Nersessian, 2005), is simply too promising to ignore.

Unpacking this compound lens for analyzing scientific practice occupies the remainder of this chapter. Here, I show how a new kind of cognitive anthropology emerged, and how this body of work help account for the intricate dynamics of epistemic actions by connecting cognition and culture. Still, recent debates about the role of anthropology in interdisciplinary cognitive science have underscored how cognitive and psychological approaches have alienated many anthropologists (Beller et al., 2012; Bender et al., 2010; Maurice Bloch, 2012). Some propose that this alienation of anthropologists from the cognitive enterprise is due to its overreliance on experimental and quantitative approaches, at the expense of naturalistic, long-term ethnographic participant observation in everyday settings (Astuti & Bloch, 2012; Gatewood, 2012). Others suggest that cognitive approaches neglected the constitutive role of material culture, social relations, politics, and power structures in the making of human communities (Strauss & Quinn, 1997; Vike, 2011). Whatever merits or misconceptions that inform these concerns, there are still valuable opportunities for rapprochement between the social and cognitive (Quinn, 2018). Importantly, ethnographers can contribute to a larger scientific conversation on the nature of cognition and knowledge, around the theoretically central concept of "cultural transmission." Emerging from psychologically informed anthropology, this field is preoccupied with "the emergence, acquisition, storage, and communication of ideas and practices" (Cohen, 2010: 194). While disciplines differ in emphasis on their respective contributions to cultural transmission, "researchers across the human and social sciences are recognizing that the bodily, cognitive, neural, and social mechanisms that permit and constrain knowledge transmission are conjointly operative and mutually contingent" (ibid.). As a naturalistic project, these studies specify relationships between cognitive processes and cultural practice by integrating studies on localized actions, events, and contexts with explanatory models that account for the large-scale evolutionary trajectories of cultural productions (Ellen & Fischer, 2013). In the following chapters, my task is to explore, ethnographically, how such dynamics unfold in the laboratories of biologists who strive toward new knowledge about a pesky parasite that is troublesome for salmon mariculture.

Approaching the Field

My analysis is based on ethnographic fieldwork from August 2013 to July 2015, with more intermittent observations in the time afterward. The SLRC drew my interest as a field site in early 2012, when I came across the first press releases from the Centre. There were several reasons why it struck me as an apt case study. Earlier, I had done fieldwork on political and social dimensions of forest management and environmental knowledge in the Shouf Mountains of Lebanon, which was subtly informed by insights from cognitive anthropology. Planning my next research project, I decided to explore the interface between scientific knowledge, cognition, and materiality in more detail.

Generously funded by The Research Council of Norway as a Centre for Research-based Innovation, the SLRC combined basic biological research with an applied angle, constituting a vibrant space involving a wide cast of different actors and epistemic interests. As I further engaged with the project, it also became clear that the SLRC offered an occasion to examine both the material cultures of science, and its role in the cultural transmission of knowledge, as it represented the genesis of an entirely novel experimental system. As a scientific institution, there was a stable membership of experts, routines for introducing newcomers into the epistemic community, and systematic documentations of the community's changing material and ideational culture through time. Of anthropological interest, the laboratory also presented a task-oriented, spatially bounded setting for the articulation of knowledge, guided by a diverse set of implicit and explicit rules.

A more personal motive for selecting this field site came from growing up in the coastal city of Ålesund in Western Norway. Here, I went through vocational training and entered the food industry, working some years in fine restaurants as a *chef de parti*e, before gradually transitioning into the academy. Trained in the culinary arts, I was attuned to the importance of embodied skills in gastronomy, and the necessity of augmenting and distributing tasks to one's work environment. This insight is captured in what professional cooks refer to as *mise en place* (literally "putting in place"), the act of setting up one's workstation properly, as a curated environment for culinary action. This experience piqued my interest in questions about how the material cultures of experimental scientists influence the production of knowledge. As a chemist friend once brought to my attention; experimental science at the workbench can be remarkably similar to what goes on in a kitchen. Having grown up in an affluent coastal region built on seafood, I also appreciated the massive transformation brought about by the ascent of marine domestication. The case of SLRC provided an opportunity to peak behind the curtain to see how cutting-edge bioscience gets applied in aquaculture and contributes to a knowledge-intensive industry of great importance to our food system.

The possibility of carrying out "proper" anthropology in such familiar contexts has caused much debate. While this issue has become less of a concern in ethnographic studies of science, it was not uncommon for the first laboratory studies to use rhetorical devices that exotified and made the assumed familiar strange. By conjuring imagery of the anthropologist as a visitor among an alien "tribe" of scientists in the strange lands of the lab, early ethnographies of science attempted to demonstrate that decades of history and philosophy of science had failed to offer a realistic appraisal of what happened in these spaces (see Doing, 2008 for a critical assessment).

Still, when anthropology takes place "at home," in the investigator's own cultural milieu, it is unavoidable that research subjects organize their knowledge about the world in ways that overlap with the anthropologist's. It is fair to say that I shared with my informants both a naturalistic ontology about what entities exist in the world, a belief in the merits of empiricism as a guiding theory of knowledge, and a subscription to those loosely knit norms of argumentation and reasoning often called "rationalism." There are two common assumptions about such ontological overlaps. They can either positively contribute to an enriched understanding, or negatively affect the study by adding only "unnecessary mystifications" that render the commonplace complex (Strathern, 1987: 17). While I recognize the worry that ethnography risks losing its unique characteristics in such homely projects, I think that whether such concerns are warranted greatly depends on the phenomena being examined and the study's execution. Skepticism toward insufficient exoticism and distance is motivated by a concern that the ethnography will become interview-driven, rather than observation-based. But far from it, cognitive ethnography as a discursive practice involves a *sharpening* of the observational focus, through an emphasis on micro events and disciplined reflexivity about the theoretical import of interactional phenomena.

Furthermore, scientific practices and laboratories are now so specialized and alien, compared to folk-knowledge, that the exotic and unknown can still act as guiding principles. Like in other field sites, an ethnographer of a molecular biology lab must enter a long period of communal socialization to acquire new ways of seeing and articulating the world (Rabinow, 1996: 2). As we account for the "particularity of practice" in these settings, my goal has been neither "glorification or unmasking" (ibid.: 17). Arguably, the strength of cognitive ethnography lies in the interactional data it obtains from long-term, systematic field observations. When we zoom in closely on these situations, even the textures of mundane things may offer surprises. Here, familiarity with the larger cultural domain where practices take place becomes a key asset for grasping the phenomena in question.

To build rapport, I first approached the Centre's director in spring 2013, via my academic supervisor, since they were professional acquaintances. The two of us were then cordially invited to present a research proposal at a staff meeting in May 2013. Here, I explained my approach to studying interaction in the laboratory and sketched some suggestions for topics to explore ethnographically. Fortunately, the Centre administration and research community found my proposal sufficiently intriguing to let me accompany them over their next years of work. In early August, I was generously provided with access keys to a shared office space at the Centre's facility, hosted by the University of Bergen's Department of Biology. My identity was negotiated around a dual status as a social scientist studying scientific reasoning and a doctoral student within the same University. Sharing an office space with other doctoral students and postdoctoral candidates, and establishing rapport with them through "legitimate-peripheral participation," I could access interpretations of practices and perspectives on the field that were not necessarily shared by more established scientists (Mody & Kaiser, 2008). During my research, I participated in laboratory work, social events, attended lab meetings, and audited lectures on topics in molecular biology, aquaculture, and fish health biology. To become conversant about details on the ethical, legal, and epistemological principles behind the experimental use of fish, I also passed an exam on laboratory animal science.

In addition to learning about scientific practices through participant observation, I carried out formal interviews and informal conversations with members from the community, as well as industrial representatives and public administrators to learn about the economic, political, and ecological context of salmon lice. The parasite also made regular appearances in media, which provided an additional source of information. Numerous conferences, internal and public meetings attended to by SLRC staff, provided access to events that gave insight into how research findings were disseminated, and the paradigmatic problems that were on the agenda.[1]

During my participation in daily life as an ethnographer, I was invited to present my own work to the scientific community on several occasions. One forum were the internal lab meetings, where all members of the SLRC were expected to present their work each semester. These were occasions where I could raise topics and questions of my own interest, based on my observations, gauge my understanding of issues, and spawn discussions with the larger lab collective. I was also invited to present my work on the cognitive anthropology of science to three cohorts of Ph.D. students at the annual Molecular and Computational Biology Research School. While some of the concepts I used were alien to my interlocutors, they willingly engaged in stimulating discussions to correct misunderstandings and sharpen my perspectives. I also collaborated with one of the SLRC's senior scientists, Sussie Dalvin, on a presentation at the tenth International Sea Lice Conference in Portland (Maine), August 2014. Named "Communicating and framing salmon lice on the web," our talk

[1]As in all fieldwork there were occasions, like board meetings and other events with special-status participants, were I found it inappropriate to intervene or ask to attend, due to my status as a guest at the Centre.

offered a content analysis of how salmon lice and their associated risks are framed by different actors in the aquaculture landscape by surveying discourse in online materials.

For someone deeply interested in biology, but without formal training in molecular life science, acquiring sufficient knowledge about the parasitology of salmon lice and high-tech laboratory work implied a steep learning curve. There is no clear-cut answer to the question about what level of competence on a subject matter that an ethnographer of science must acquire, since this depends on the problems being investigated. As Philip Kitcher points out, the important thing is to have the necessary information that is pertinent for understanding the scientific activities in question (1998: 34). I was therefore aiming to acquire "interactional expertise" (Collins, 2004). Lodged between propositional knowledge, and embodied skills, interactional expertise makes it possible to converse with experts to learn about their practice but stands in contrast to the "contributory expertise" necessary to carry out experiments, publish papers in the field, and so on. This meant getting familiar with relevant topics and being able to sufficiently describe these in ways recognized as sensible by members of the scientific community.

No method that science has at its current disposal allows us a privileged, direct view of cognitive processes inside people's heads in the wild. It is even dubious to assume that we actually have *direct measurements of cognition*, even in the laboratory. Brain imaging technologies do not directly picture cognitive processes but detect physiological states that are used as proxies for inferring about higher order cognitive states. The distributed view on cognition tells us that scientists create and augment their cognitive powers partly by building the problem-solving environments whereby they exercise their powers (Hutchins, 1995: xvi). Cognitive ethnography, which I capitalize on here, assumes that the ethnographer can literally step into such sociocultural cognitive ecosystems and observe cognition in action. In total, I collected around 30 hours of high-definition digital video of different events with a handheld video camera. These data are mainly explored in Chapters 5, 6, and 7. As there is a potentially infinite stream of parallel events to record, video-assisted cognitive ethnography entails the risk of a kind of data deluge. Analyzes of video-recorded interactions are also very time consuming, which adds to the urgency of sampling relevant episodes for detailed examination.

Decisions about what to record were based on background knowledge about the relevance of various practices to the Centre's overall mission. When filming sessions in the lab I positioned the camera to capture the broadest view of the action possible. Sometimes, if the interaction unfolded over a larger area, e.g., multiple rooms, I would use an additional audio recorder, or an iPhone camera as a supplement. When scientists were busy on a specific area on the lab bench, I would position myself behind them with the camera, or place the camera to record the scene from a sideways angle to capture as much of the situated interaction as possible. Since the camera was small, I could move it around to follow the action.

Salient events were then indexed and transcribed to capture fine-grained details of human interaction, using a simplified transcription scheme.[2] This was an iterative process where I moved between other resources, such as notes, documents, pictures, scientific reports, etc., looking for connections between phenomena of interest. Here, video recordings made it possible to "save the phenomena," and a resource for resisting the tendency to *decontextualize* ethnographic observations prematurely (Sormani et al., 2016: 126).

In *Handling Digital Brains*, Alač writes that she initially planned to study the fMRI center at the heart of her ethnography qua its organization as a research center (2011: 12–13). However, her work gradually centered on smaller units of practice, such as the collaborative sense-making routines that occur between colleagues doing situated work to make and interpret scientific visuals. In this study, I zoom in and out from collaborative micro-interactions to capture different levels of activity in the lab. The goal is to understand how representations propagate within a "pipeline" for research, where epistemic activities were organized around problem-solving in a local experimental system.

According to the Norwegian Personal Data Act, the use of video makes the project subject to the "duty to notify" the Norwegian Centre for Research Data, which recommended the project on the condition of

[2] Alač adopts a variation on Jeffersonian notation (2011). I use a simplified version here.

informed consent. While I use the names of prominent researchers in the field in the historical narrative, I have anonymized the identity of my informants, for their convenience, in the more intimate setting of micro-analyzes. I asked for permission to film on occasions where video offered a relevant aid for my inquiry. Other than a few joking remarks about the camera's presence ("surveillance!"), I did not receive complaints about the camera's intrusiveness. Interlocutors were usually filmed while performing familiar tasks. As these were attention demanding, my experience was that they quickly lost interest in the camera's presence and got on with their work. I was fortunate not to have the same experience as Nersessian, who had to discard the camera as a research tool, since her participants found it intrusive (2009: 733–734).

When selecting events for further inquiry, I was guided by Hasok Chang's "Checklist for Activity-Based Analysis," or what he humorously refers to as a "Recipe for the Transformation of Boring Philosophical Issues" (2014). Chang starts his methodical recipe with a rather indisputable premise; namely, that "a serious study of science must be concerned with what it is that we actually do in scientific work" (ibid.: 67). This requires a shift of emphasis from proposition to actions; who is doing what, why, how, and in what context? Chang's checklist suggests that the first thing is to characterize the activity; what is being done? Secondly, we should look at its purpose and external function; what are its aims? The third element is the systematic context of activity; is it singular, or routinized and thus part of something extensive? Studying systems of practice, we must always keep in mind that systems have goals beyond the purpose of the activities constituting the system (ibid.: 74). We must also attend to the agents; who do things to, and with, whom? And by which resources and capabilities do they do it? How free are the agents to make epistemic choices, and what constraints are in place? Finally, there is, like Barth reminded anthropologists (2002), always a set of metaphysical principles at play; what kind of world does it take for the activity to make sense, and who decides about its sensibility?

In this book I use cognitive ethnography to flesh out how these questions pertain to SLRC's experimental system, its history, and some everyday operations. Like all exegesis from the native's point of view, this requires strenuous balancing between doing justice to the world of insider conceptions through which my informants think, know, and act in the world, and using a meaningful vocabulary for lay readers. I have tried to avoid flooding readers with technical terms used by my interlocutors, but in a Malinowskian spirit I do consider *some* insider language to be essential when accounting for meaning-making from their point of view.[3] I apologize for any nuisances this may cause.

Outline

This book is organized into two complementary parts. This chapter sets the stage in terms of what the case study is about, my approach to the field, and the scope of an anthropology of knowledge that takes cognitive and social dimensions seriously. It introduces a handful of conceptual issues in cognitive anthropology, elaborates on the framework of distributed cognition, and shows its relevance for the study of experimental practices. Grasping the cultural and material dimensions of scientific cognition and meaning-making in experimental bioscience requires a larger unit of analysis than the individual agent. I show how an emphasis on experimental systems enables us to take seriously the materiality of science, and clarify epistemological questions raised by this project, like the issue of "cognitive bloat" and the nature of epistemic agency.

The next three chapters situate the Sea Lice Research Centre and its experimental system through an ethnographically and historically informed account. Examining the Centre through the lens of distributed cognition, requires undertaking several journeys: through historical time, through physical and social space, and through conceptual space. I begin my story at the macro level in Chapter 2, with a wide shot that situates the science in the larger world of salmon aquaculture and lice management. Here, I examine the deep history of Norwegian salmon farming, looking at parasitism as a wicked management problem in

[3]This is not to be conflated with the contentious distinction between internal and external exposition in science studies, which has been treated with much dubiety (see Chang, 2016 for a recent discussion).

animal domestication, not only on land, but also in the sea. Management of pathogens has become a hot-button issue, profusely shaping the trajectory of salmon domestication, and will be a decisive factor determining its future path. A meaningful analysis of experimental science in the laboratories at the SLRC necessitates an appreciation of how scientific management of fish health through parasitology came to indirectly shape the coevolutionary, interspecies process that is marine salmon farming.

Chapter 3 tackles the historical background for the Research Centre and describes its social organization and scientific goals, by moving through physical, social, and conceptual spaces. I focus on the emergence of a novel system for probing the biology of salmon lice, how new technologies changed the nature of the experimental practice, and look at the division of epistemic labor. Critical for this story is how the parasite was domesticated in the lab, and the development of robust technologies for experimentation, such as a novel system for maintaining lice and hosts. My analysis of these "technologies of the mind" is informed by distributed cognition, along with historical and philosophical work on experimentation.

Expanding on this topic, Chapter 4 examines the conceptual space of central biotechnology in SLRC's experimental pipeline, namely, RNA interference (RNAi). Here, I show how RNAi was adapted as a key tool for screening the salmon lice genome for potential therapeutic targets. I draw on recent work on regulatory RNA research and related biotechnologies, which exemplify distinct modes of epistemic practices at play in the life sciences. It is shown how experimentation is not just for testing hypothesis but may serve other important epistemic goals as well. I capitalize on the ideas of the "New Experimentalists," who began rethinking experimentation as practice in the 1980s, and subsequent work on "exploratory experimentation" to discuss the epistemic role of RNAi in the science of salmon lice.

Chapters 5, 6, and 7 offer a series of situated micro-analyzes of everyday practices in the lab that shows how the extended experimental system constitutes a vehicle for thought and action. Here, I track different laboratory events and map the traffic of representations within the Centre's pipeline for research. By framing experimental systems as *cognitive ecosystems*, we see how small-scale practices link up into larger interactive elements that constrain how, where, when, and in what form information travels, and gets interpreted by cognitive agents. I show how cultural practices within the experimental system link up cognitive resources, and how the sources of organization for ordered scientific practices originate outside of the individual performer. Depending on the epistemic phenomena in question, the most suitable unit of analysis can occasionally be found at the level of a situated, individual agent, while at other times the analysis must be expanded further beyond the individual skin and skull into the social and material environment.

In Chapter 5, I examine events sampled from the initiation and termination of RNAi experiments. These functional screenings probe the effects of specific genes on salmon lice biology. RNAi initiation and termination are socially and cognitively complex affairs, whose execution require the choreography of a collective of researchers. I look at how these situations set up epistemically rewarding relationships between samples of lice, instruments, and various representational artifacts.

How are valuable tissues from RNAi trials cared for, and endowed with biological meanings within the experimental pipeline? Chapter 6 addresses these questions by examining how patterns of gene expression become visible using a technology known as *real-time quantitative polymerase chain reaction*. Here, I follow the downstream benchwork of one particular researcher and examine how her situatedness within the lab's cognitive ecosystem makes such measurements possible. Through everyday operations, scientists opportunistically use artifacts to execute various creative actions that render patterns of gene expression visible. I analyze these epistemic activities as "ecological assemblies," cultural practices that orchestrate arrays of resources in the agent's environment to house and extend cognitive processes beyond the individual agent. By changing the arrangement of her external surroundings, the agent creates novel opportunities for knowledge and insight.

As icons of science, microscopes occupy a prominent place in epistemological debates about scientific realism and that which is invisible for the naked eye. Tapping into some of these, Chapter 7 examines how the anatomical structure, distribution, and development of salmon lice exocrine glands are collaboratively described through explorative microanatomy. Offering an ethnography of the microscopical study of tissue samples (histology), this chapter shows how mundane artifacts and sophisticated imaging techniques help practitioners create spatial reference and thus biological meaning from microscopic phenomena. Spatial language, and a range of other semiotic resources, are intricately deployed to reason and achieve consensus about such biological entities. I show some of the cognitive practices that microscopists use to establish spatial reference to salient phenomena, and how representational states are propagated through embodied interactions in front of the microscope, via transformations to scribbled notes on paper, and eventually through the systematization and dissemination of findings in scientific publications.

In conclusion, Chapter 8 draws together threads from preceding chapters and sketches some recent developments in the science of salmon lice, both as it pertains to SLRC's experimental system and to the general trajectory of salmon aquaculture and lice management. I also spell out some implications of my study for future work on distributed cognition, cultural transmission in science, and the contribution of material culture to the evolution of scientific knowledge.

Primer on Cognitive Anthropology

Before presenting my roadmap for integrating cognitive and social studies of scientific meaning-making, I offer a brief primer on cognitive anthropology. After probing some limitations in how this field has conventionally approached cognition, I introduce "distributed cognition" as an alternative framework for rethinking the fundamentally cultural nature of cognition and action. Understanding scientific cognition in fields like molecular biology requires a larger unit of analysis than the individual agent. Works by historians and philosophers have identified "experimental systems" as a critical working unit for understanding contemporary science. This label describes heterogeneous arrangements of apparatus, material infrastructure, technical expertise, conceptual models, theoretical constructs, and cultural assumptions that govern research fields. The concept draws attention to the fact that knowledge in experimental science is a collective, cumulative endeavor. It is governed by a stream of activity that explores the phenomena in question from many angles, rather than single, "decisive" experiments for hypothesis testing, performed by individuals working in solitude. I argue that this approach productively dovetails with the framework of distributed cognition, and other research on the situated, embodied, and extended character of mind and knowledge. Attending to material and distributed aspects of scientific reasoning raises questions about the locus of agency in distributed cognitive systems. I clarify these toward the end.

Distributed cognition, as an analytical framework, was introduced by the anthropologist Edwin Hutchins in *Cognition in the Wild* (*CiTW*), a landmark ethnographic study centered on large-ship navigation aboard a US Navy vessel (1995). Among other things, this work compared the representational assumptions of modern navigational culture in the US Navy with those of traditional Micronesian navigation. Based on a detailed ethnography of a hierarchical military culture, Hutchins specifies how cognition situated naturally is thoroughly distributed, socially and materially. A big idea was that cognitive scientists had attributed to the individual person many computational processes that are better understood as being performed by larger, heterogeneous systems. According to Hutchins, the computations that cognitive science had assumed were occurring inside people's heads frequently crisscrosses the boundary of the skin in ways that bestow humans with many cognitive powers. *CiTW* then argues for a perspectival shift and a new unit of analysis of cognition that carves out space for the role of cultural practices and materiality. Hutchins had trained in the tradition of cognitive anthropology, sometimes known as "the New Ethnography" or "ethnoscience", which emerged from the linguistic and cultural branch of American anthropology in the 1960s. But his case study represented a radical conceptual flip from conventional approaches to intelligible action within psychological anthropology and cognitive science. Why was this flip necessary?

Studies on the relation between mind, behavior, and language have a long-checkered history in anthropology and adjacent disciplines. In nineteenth-century Europe, German romanticists like Wilhelm von Humboldt explored the connections between languages and worldviews. Humboldt, and his contemporaries, believed that languages differed in how suitable they were for describing how the world was (which, incidentally, explained the superiority of Indo-Europeans). In early American anthropology, Boas, Sapir, and Whorf pursued similar topics, and introduced the concept of linguistic relativity based on field research in Native American communities. While linguistically minded anthropologists agreed with the romanticists that structural aspects of different languages could uncover the roots of cultural differences, they proposed both weaker and stronger versions of the relativity hypothesis. Furthermore, the anthropologists disagreed about ranking languages in terms of their suitability for intellectual pursuits.

In the late 1950s, Claude Lévi-Strauss famously launched structuralism as a naturalistic program to compare the cultural products of the mind. Inspired by Roman Jakobson's structural linguistics, Lévi-Strauss claimed that human thought organizes information primarily as binary contrasts that form combinatory, abstract patterns that generate the concrete cultural variations found in the ethnographic and historical record. Lévi-Strauss' universalist approach to the production of cultural forms such as myths, exercised a huge influence across the humanities and parts of the social sciences, not only due to its positive contributions, but also because of strong reactions *against* the structuralist program.

As Lévi-Strauss developed his elaborate schema, the so-called "cognitive revolution" swept across the behavioral sciences, in disciplines such as psychology, linguistics, and philosophy, along with the nascent field of computer science. Many in this new vanguard also considered anthropology to be a crucial piece of the puzzle (Boden, 2008: 516). But although structuralism had been a "proto-cognitive" approach in some respects, few proponents engaged thoroughly with these developments (Sperber, 1985). Structural linguistics and particularly phoneme theory, a theory about the smallest units of significance which Levi-Strauss based his reasoning on, soon faced heavy criticisms from Noam Chomsky and others's generative grammar (Bloch, 2012: 54–59).[4] Through the

[4]While Lévi-Strauss proposed a comparative and naturalistic approach to culture, he did not, for various reasons, engage deeply with the cognitive program, instead aligning his project with Piaget's developmental psychology. Some anthropologists abandoned the enterprise as it offered no method beyond intuition to identify the minimal contrastive symbolic elements of cultural productions.

argument from the "poverty of the stimulus," for example, Chomskyan generativists claimed that structural linguists did not tell a plausible developmental story about how children learned languages at the speed they did. Instead, they hypothesized a biologically specialized mental faculty disposing humans to language acquisition, and that this innate module enabled the development of a universal grammar constraining language variation.

Early American cognitive anthropologists approached cognition from a rather different vantage point than such nativist, generative deep structures. Instead, they first tried to wed the anthropology of cultural particulars with formal linguistics by looking for semantic equivalents of the finite phonemes that were widely believed to characterize natural languages. Kinship terms, for instance, were assumed to have a paradigmatic structure that could be deduced by extracting semantic features from genealogies (see D'Andrade, 1995a). Such native mental categories and structures could not be observed directly. Instead, they had to be discovered through elicitation methods. This led to the development of stringent procedures for studying lexical items, known as componential analysis.

The resulting "ethnoscience", which equated culture with knowledge and its organization, was quite productive empirically. But there were major concerns about the psychological reality of formally elicited semantic structures. Keesing, a specialist on Melanesia sympathetic to the project of a science of culture, offered a harsh verdict of "messianic promises" to identify this "heart of cultural structure": "The new ethnographers have been unable to move beyond the analysis of artificially simplified and delineated (and usually trivial) semantic domains, and this has discouraged many of the originally faithful. Ethnoscience has almost bored itself to death" (1972: 307–308). Cognitive anthropologists had borrowed their conceptual framework from linguistics, but Keesing asserted that Chomsky's generative grammar had literally "destroyed" the foundational paradigm of ethnoscience, such that it "no longer made sense."

By this point some cognitive anthropologists, disillusioned with the old framework, had begun novel research on the formal properties of taxonomic and classificatory models. And soon, topics like the universality of color terms and the structure of ethnobiological knowledge became matters of intense debate. Among ethnobiologists, for example, a pervasive disagreement ensued over the relative importance of utilitarian versus innate drivers of environmental knowledge and natural classification (see Hunn, 1982). Evidence indicated that most lexical domains were not organized taxonomically, with a few special exceptions in ethnoscientific folk knowledge. Nor could culture be conceptualized analogous to an integrated "grammar" or "code". The proposals from cognitive anthropologists had "failed to gel into a comprehensive, agreed upon new theory of cultural meaning" (Quinn, 2011: 34).

Eventually, the "new ethnographers" developed elicitation techniques, imported new methods like multidimensional scaling from psychology, and co-opted theoretical tools of greater sophistication. But it was still clear that a comprehensive understanding of how cultural knowledge and meaning was organized, required a rethink of fundamental issues: "In short, it cannot simply be assumed that distinct semantic domains are structured in the same way. Until independently assessed domains can be shown similar, meaning should be assumed to be a motley, not monolith" (Atran, 1993: 57).

Soon, new theoretical accounts from experimental psychology, including prototype models and schema theory, led to the emergence of the "cultural models school." In this approach, meanings were considered not as the product of simple checklists of features but determined by a complex organization of different mental representations. The notion of schemas was introduced to account for meaning construction in general. These cognitive–semantic structures were built up from experience, both conscious and unconscious, as well as from sensation and emotion. As experientially derived constructs, schemas could also give structure to future, novel experiences. Both individual meanings and shared, public representations could be understood in these schematic terms, hence the notions of "cultural schema" and "cultural model."[5] Later, research

[5]These concepts are used somewhat interchangeably in the literature, with some using "cultural schema" as the generic term, and "cultural model" for describing more general mental structures. Shore introduced the term "foundational schema" for widely shared and abstract conceptual structures, and reserve "models" for particular instantiations (1995: 53).

suggested that these schemas and models had directive force, created motivations, and oriented people toward certain outcomes and meanings, providing evaluative standards of what is good or bad, thereby driving behavior in culturally specific ways (Quinn, 2011). This work was further refined throughout the 1990s, with a wave of studies on the dynamic nature of cultural models and cultural representations. These indicated that the first wave of cognitive anthropologists had subscribed to an overtly ideational, language-like concept of culture.

Critically, the old paradigm had failed to address cultural transmission as *process* (Bloch, 1998; C. M. Keller & Keller, 1996; Lave & Wenger, 1991; Shore, 1995; Strauss & Quinn, 1997). In addition to growing dissatisfaction with the theoretical impasses of cognitive anthropology, failures to engage mainstream sociocultural anthropologists in the ongoing interdisciplinary conversation provided additional impetus for rethinking the "cognition and culture" field. In European social anthropology, structural-functionalist accounts had reigned supreme well into the 1960s, and the various approaches to social phenomena that followed, marginalized the space for cognitive perspectives even more than in American anthropology. One exception to the European trend was a small but influential group of scholars who began thinking about the distribution of cultural phenomena in terms of domain-specific, evolved cognitive mechanisms. These works developed around Dan Sperber's notion of an "epidemiology of representations" (1985, 1996). Sperber took a lead from Gabriel Tarde, one of Durkheim's detractors, by marrying Tarde's diffusionist approach to cultural diversity with theories about evolved cognitive dispositions. For Sperber, a naturalistic approach to culture should investigate the regulation, acquisition, variability, and use of mental and public representations and performances. Similar to medical epidemiology, an anti-reductionist discipline in search of mechanisms involved in distributing health and disease, anthropological studies of culture and society should attend to the irreducible *ecological* patterns of psychological phenomena (Sperber, 1996: 31).

In a landmark study, Scott Atran cashed in on Sperber's proposal, demonstrating how traditional precursors to modern biological science, like natural history and natural philosophy, were institutionalized byproducts of an innate, pan-human cognitive propensity for reasoning about living kinds (1990). “Folk-biology” is an evolved disposition, he argued, that afford people across cultures the ability to reason about living entities by intuitively attributing them with essences, and by structuring representations of living kinds in terms of species-like groups organized in hierarchical relations. Building on comparative analyzes on ethnobiological classification, Atran suggests that this leads to a naïve, essentialist notion of species that conforms to a particular “generic” rank in folk taxonomies (like the generic label “tree”). These conceptions sometimes come into conflict with Darwinian and scientific species concepts (such as interbreeding). But intriguingly, adoption of a Darwinian species concept does not eliminate everyday intuitions about the generic level and an essentialist bias. Instead, such intuitions provide a cognitive resource for meta-representational reflections on biological information in ways that allow scientists and others to go beyond spontaneous, naïve intuitions, and reach new conclusions.

This work culminated in a series of comparative field investigations that productively combined experimental and ethnographic approaches in a variety of societies to understand environmental reasoning among different groups (Atran & Medin, 2008). Similar to what Chomsky had proposed for language, there were evolved special-purpose tools of the mind adapted for reasoning about natural kinds. Varieties of cultural knowledge emerge from these domain-specific, pan-human cognitive mechanisms when they get implemented in different ecological contexts. Details about how exactly such habits of the mind develop, and their relation to perennial anthropological issues like essentialist social categories, are still debated among specialists (Regnier & Astuti, 2015).

Interpretative anthropologists committed to *sui generis* views of culture criticized this agenda. A narrow focus on a few select domains of social life, a commitment to methodological formalism, and hubristic ambitions to *causally* explain social phenomena, was misguided as it simply failed to grasp what was special about human culture. David Schneider, for example, had early on criticized the application of cognitive and formal approaches to kinship studies (1965). Clifford Geertz also took issue with the mentalistic and individualist notions of culture proposed by the cognitivist program (1973), which he believed married "extreme subjectivism" with "extreme formalism." In this view, the epistemic goal of ethnography and anthropology was not an explanation of social phenomena per se. Instead, the anthropologist's aim was interpretations of shared, public meanings; the Weberian webs of significance spun by humans. The goal of ethnography was "thick descriptions" of the social; an approach which could not be formally articulated and left little room for systematic data elicitation using other methods. Later, as evolutionary-informed analyzes gained traction in cognitive circles, these were seen to advocate a troublesome reductionist agenda. This also coincided with a displacement of epistemic virtues in parts of anthropology, like searching for objective models, favoring instead what D'Andrade called "moral models" (1995b). As Bloch describes, cognitive approaches fell on the wrong side of a spurious epistemological and ontological nature–culture divide, where sociocultural anthropology "declared itself the champion of 'culture' against a 'nature' which includes a consideration of the working of the mind" (2012: 6).

The social-reductionist alternative of Geertz and his followers effectively culturalized the mind, but simultaneously resisted any form of cognitively nuanced apprehension of culture (Shore, 1995: 35). In a mutual gesture, many cognitivists dismissed mainstream anthropology as succumbing to an untenable holism lacking methodical and theoretical rigor, effectively being incompatible with naturalistic accounts of culture. As Margaret Boden shows in her history of cognitive science, these internal disagreements about fundamental questions, sidelined the analysis of generative cognitive mechanisms that could account for both diversity and pan-human patterns of culture (2008). Consequently, anthropology became the "missing discipline" in debates about the mind.

Critiquing this development, Strauss and Quinn argued that Geertz's and other interpretivists' insistence that cultural meanings were only interesting *qua* their status as *publicly* shared representations, built on an inadequate "fax model of internalization" (1997: 23). The Geertzian claim that "culture is public *because meaning is*" (my emphasis), assumed that culture was an integrated, shared, and coherent symbolic system (1973: 315). But the notion of a unified symbolic system, an idea that was shared by early cognitive anthropologists, was notoriously ambiguous and lacked empirical warrant. People did not always attend to publicly accessible symbols in the same way, and symbols did not straightforwardly determine how people understood and attributed meanings to things and events. Strauss and Quinn also took issue with poststructuralist attempts to explain away cultural meanings as "constructed." Neither were historical-materialist accounts of exploitative "hegemonies" of meaning plausible, they argued, in light of knowledge about the properties and organization of human mental faculties (see also Vike, 2011). Foucauldian concepts, such as *discourse* and *episteme*, appeared to dissolve any boundary between people's inner workings and their social world in ways that lacked empirical warrant (Strauss & Quinn, 1997: 26–41). Rather than dissolving the culture concept altogether and replacing it with more opaque terminology (see Shore, 1995: 45), there was a need to refine cognitive theory and accommodate more holistic analyzes of local meanings.

Strauss and Quinn found practice theory, a widely adopted approach to social phenomena which emphasized the implicit character of knowledge (Bourdieu, 1977), as somewhat compatible with the kind of cognitive approach to cultural meaning they proposed. But while practice theory offered a step in the right direction, it was nonetheless flawed since it refused to specify the cognitive mechanisms involved in the internalization processes of cultural learning. As Bloch has observed more recently: "By stressing the need to understand individual motivation and the processes that lead to action in living people, Bourdieu takes us to a point where we cannot do without the work of cognitive scientists, but he himself seems unwilling to take the further necessary step" (2004: 152). In conclusion, these objections implied that metaphors commonly used to make sense of the culture concept, and its role in the production of meaning, had not only been misleading, making anthropologists look in the wrong places for the wrong things, but lacked empirical justification. Culture could no longer be conceptualized as something transferred between humans like bodies warm to rays of sunshine. People were not sponges soaking up cultural stuff, and the notion that culture is like a pair of glasses through which we view the world, was at best misleading. New frameworks were called for.

Distributed Cognition

The culturalist position minimized the role of innate human dispositions and strategically overstated cognitive variance. But neither could the cognitivists hope to understand human nature by "factoring out dimensions of local variation" to expose a stripped-down, essentially *acultural* being (Shore, 2011: 148). The challenge was to articulate an approach that accommodated anthropological sensitivities to detect and understand local meanings and practices, with a view toward human meta-culture, the cognitive conditions that make observable cultural variation possible.

Being deeply committed to methodological individualism, cognitive anthropologists had long considered the enculturated agent as a natural unit of analysis. But treating cultural knowledge as a mind-internal and language-based phenomenon, had some unfortunate implications. Roy D'Andrade, for example, once suggested a division of labor between cognitive scientists, who study the general mechanisms by which the mind operates, and anthropologists, who study the range of cultural content of minds across social worlds: "Cognitive anthropology and cognitive psychology are both concerned with the interaction between processing and information, except that the cognitive anthropologist wants to know how cultural information is constrained and shaped by the way the brain processes such information, while the cognitive psychologist wants to know how the machinery of the brain works on all types of information, including cultural information" (1981: 183). While this was a nuanced proposal at the time, the separation between "cultural" and other forms of information, along with the distinction between content and process, is problematic in retrospect (Bender et al., 2010: 377). Not only does recent evidence suggest that even basic domains of perception are culturally malleable (Henrich et al., 2010), but an a priori separation of content and process also seems to ignore the material dimensions of culture, along with non-declarative knowledge like skills and practices. Cognitive anthropologists could no longer pursue their project by cramming everything cultural inside the native's head.

Hutchins' *Cognition in the Wild* offered a conceptual flip in this intellectual landscape by respecifying action as cognition, pleading for an anthropological reexamination of the enculturated mind as an emergent product of interactions between material artifacts, cultural practices, and cognition as the computation and propagation of representational states (1995). Building a case against reigning internalist models of the mind, Hutchins describes how early cognitive science defined bodies (sensory motor systems), emotion (affect), and social context as too difficult problems to tackle with standard computational approaches. Pioneers in the field accepted that these phenomena instead would have to be integrated later, when the field had matured. But even three decades after the cognitive revolution, Hutchins could still observe that much more was known about cognition "in the captivity of the laboratory" than cognition in "culturally constituted settings" (ibid.: 370–371). This was not simply a critique of the dominant cognitive paradigm, but also a critical commentary on cultural theories that had failed to engage with the naturalistic study of the mind.

Hutchins identified the malaise in cognitive science as a set of problematic and unexamined assumptions about minds as "physical symbol systems" (PSS). Basically, a PSS consists of a set of physical patterns that can be attached to each other to make a structure (an expression), and a set of processes that operate upon these symbols according to specific instructions (creating, altering, copying, destroying), and which is located in a world of real objects that include more than just symbolic expressions alone. Cognitive science was built on the assumption that symbolic representations, a class of things that exists in the world around us, could be located inside people's heads as constituent elements of the mind. In his original formulation of how an intelligent system could work, the mathematician-logician Alan Turing proposed an abstract model system, a Turing machine that manipulates a strip of tape according to rules, using the image of a mathematician at work, busy manipulating symbols in order to solve formal problems. Hutchins reminded us that this idealization actually interacts with a material world, using hands and eyes to manipulate symbols and perform computations: "The heart of Turing's great discovery was that the embodied actions of the mathematician and the world in which the mathematician acted could be idealized and abstracted in such a way that the mathematician could be eliminated. What remained was the essence of the application of rules to strings of symbols. For the purposes of producing the computation, the way the mathematician actually interacted with the world is no more than an implementational detail" (ibid.: 362).

The first digital computers were based upon this metaphor and proved that it was possible to build "universal" or "Turing equivalent" machines that could formally manipulate symbols to compute any exactly specified function via a set of rules. Such a formal system would encode phenomena in the world as symbols put together into symbolic expressions. By manipulating a string of symbols following syntactic rules, it was possible to create newly formed strings that entailed some particular meanings about the world. Hutchins considered these formal systems to be so powerful that they were "the key to modern civilization" (ibid.: 360). Eventually, abstract symbol manipulation became a model for human thinking and was eventually refined into what today is recognizable as the computational theory of mind. The PSS hypothesis suggested that the mind–brain was best understood as an information-processing system operating on abstract symbols to perform computations. The computer, a mechanical system for rule-based symbol manipulation modelled on an idealized human agent, was replaced with the brain, which effectively placed the symbols Turing identified in the external environment into the head, the locus of brain-internal information processing.[6] As Wilson and Clark observe, this individualistic conception of thought and action resulted in a sandwich model of the mind, where cognition is "wedged between perception (on the input side) and action (on the output side)" (2009: 56).

While this was an extremely productive guiding idea when the field of cognitive science coalesced, elevating it as a central dogma had some unfortunate consequences. Internal symbol processing came to carry the entire explanatory burden in accounts of the mind. In his ethnographic study, Hutchins lays out the case of ship navigation on a large US navy

[6]The term 'computer' used to describe a person performing calculations in fields which required joint work teams to solve complex problems. Each participant usually worked on a subset of the problem.

vessel, which involves taking bearing readings and turning these observations into formal manipulations of numbers, symbols, and lines drawn on a chart to satisfy the constraining principles of nautics. He shows that many representations that are being manipulated to answer navigational questions are not in the head of any individual navigator, but out in the environment; being operated upon by human beings engaged in practical tasks, acting and communicating with each other to answer the general navigational question of "where are we?" How could these cultural activities, which were so evidently computational in nature, become invisible for cognitive science, and how could they be made visible again? Hutchins suggests that this requires a conceptual figure-ground reversal of a Kuhnian sort. Due to their incommensurability with the standard paradigm of cognitive science, Hutchins even finds his own words unruly (1995: 356). So, before his words can assume its intended meaning, he must reverse engineer the assumptions behind the computational metaphor of mind to expose its limitations.

We saw that when machines capable of manipulating symbols were created, these soon became model exemplars of intelligent systems. But the model that Turing had in mind when he first conceived the idea of the universal machine was an actual physical human being interacting with the world, manually manipulating symbols with a writing instrument, paper, and other tools. Turing's universal machine was based on abstracting away the human agent, her body, equipment, and the rules, which were all parts of a distributed system. This move would be unproblematic if the goal was simply to push the boundaries of humanity's cognitive accomplishments. But it offered an impoverished model for describing how flesh and blood human beings engaged with cognitive tasks in natural contexts. The cognitive properties of a human agent equipped with only the bare brain, according to Hutchins, did decidedly not have the same properties as those of the same agent equipped with a suite of tools, material symbols, and an external medium in which computations can be implemented. As with bare-handed carpentry, bare-brained thinking simply does not get us very far (Dennett, 2000). The physical symbol system hypothesis, Hutchins radically suggested, had reproduced the properties of the wrong system, and was no fitting model for individual cognition: *"It is a model of the operation of a sociocultural* *system from which the human actor has been removed*" (1995: 363, italics in original).

A skewed view on the nature of information processing was the result of inappropriate conceptual surgery that replaced the biological brain with a computer. Unfortunately, says Hutchins, while the procedure seemed remarkably successful from a computational perspective, the role of body and environment to cognitive processes was forgotten in the operation's aftermath. Cognitive science then reshaped the image of the human mind on basis of a new but impoverished model; putting symbols, manipulation, implementation, and everything else into an abstraction, insulated by the skull's hard boundary. The provocative conclusion to Hutchins' line of reasoning is that the computer was *not* made in the image of a human agent, but rather in the image of what for Turing was a sociocultural system to begin with, one developed to solve certain kinds of problems; a human agent, the mathematician-logician, immersed in an actual environment seeded with physical symbols and the tools to manipulate them. An enskilled agent participating in a material culture emerging from a long chain of cultural evolution and selection in the mathematical domain.

The framework of distributed cognition proposed that the intracranial boundary was no longer a tenable demarcation for truly "cognitive" phenomena. While these boundaries were put up mainly for reasons of tractability, Hutchins proposed that the implementational details of symbolic manipulation, mattered a lot more for our understanding of cognitive systems than previously recognized. Think about the now-classic example of performing "long multiplication" using pen and paper to multiply two three-digit numbers (Magnus, 2007: 277). Some gifted individuals can solve such multiplication tasks by relying on mental imagery alone, without externally representing the problem. But most of us either have to use a tool, such as a calculator, or orchestrate our hands in other specific ways by manipulating a writing instrument to make inscriptions on paper or some other medium. In the latter cases, only some parts of the task are performed by the individual brain, while other major parts, such as representation and memory, are outsourced to external media that can be manipulated using specific rules. Clearly, the cognitive properties of an agent calculating three-digit numbers with just the naked mind are different than those of an individual equipped with pen, paper, and the procedural rules for manipulating symbols by hand to construct an external representation of the problem. Although the output, the solution, remains the same, information is being processed in different ways in the two systems. This is also the case for many other familiar tools that litter our environments and which we frequently use to solve analytical problems large and small. Structure in the world does more than simply augment our memory capacities; it also changes the nature of the tasks we try to accomplish by facilitating coordination between the inside and outside of the agent.

Hutchins suggests we can rectify this erroneous conception of the mind by extending the unit of analysis beyond individual heads, to include the enculturated functional environment, or "cognitive ecology," where processes of cognition take place in the wild (Hutchins, 2010). In the above example, the *cognitive system* which performs the pen and paper computation is actually the person with its internal resources *plus* the inherited tools used to accomplish the task externally. But while this example indicates that we must broaden our unit of analysis, Hutchins maintains that we can *use the same language* that was previously reserved for describing internal mental events to account for the cognitive accomplishments of larger sociocultural systems. This means "computation" in the wide meaning of the word, realized through creating, transforming, and propagating representational states. The difference is that the media where this process unfolds is no longer restricted to a hundred billion neurons that are wired together in the human brain. For Hutchins, talking about cognition and computation in the case of extracranial events is therefore not an unwarranted metaphorical extension, as sceptics might object (see Adams & Aizawa, 2001).[7] Instead, this conception follows from the original source model that gave rise to the physical symbol system hypothesis; a wider information-processing

[7]Adams and Aizawa suggests that Hutchins only studies "naturally occurring computation" rather than true cognition (2001: 58–59). Their argument hinges on the importance of "non-derived meanings" for what they see as truly, intracranial cognitive processes, as opposed to "derived meanings" of external computations (meanings that we attribute to things). This is a technical argument that I cannot pursue here. See Clark (2008: 93–99) for a refutation of these objections.

system where human individuals are just one (special) component among several constituent parts. So, whatever turns out to be true about the implementational details of computational processing inside the head, Hutchins suggests we at least can be sure there are physical symbol systems, out in the world that is used by enculturated agents. Our use of these representations must certainly be accounted for.

In justifying his conceptual flip, Hutchins appeals to David Marr's classic levels of description for any information-processing system. A neuroscientist working on visual processing, Marr was concerned with how physical systems could accomplish computation. While there were many possible levels of description for any system, he identified three salient ones. Marr's dubbed his first level the computational level; a specification of what problem the system solves, and why it does it. This account must specify the constraints satisfied by the system's operation. Marr's second level, the representational or algorithmic, specifies the representations that are used and the algorithm by which representations are transformed; it must account for "logical organization of the structures that encode the information and the transformations by which the information is propagated through the system from input to output" (Hutchins, 1995: 50). The third level is the *implementational level*; the material substrate or architecture in which the algorithm and representational level is physically realized.

In the pen and paper example of long multiplication above, we see these three levels clearly coming into play (Magnus, 2007: 298–299). Computationally there are normative answers defined for the input of natural numbers during multiplication, the algorithm is specified by the stepwise transformations to be performed on the input and output, and the implementation is carried out using pen and paper (although exceptional individuals can execute the steps using only mental simulations). In the context of scientific practice, however, we can often simplify the scheme into a distinction between task, an abstract description of the computational goals the cognitive system must satisfy, and process, which specifies how this is accomplished and implemented (ibid.).

Studying these different levels of cognitive distribution in the wild required a descriptive enterprise for investigating the natural history of cognitive systems, to paraphrase Hutchins (1995: 371). He proposed that ethnography was uniquely positioned to attend this new unit of analysis, which spanned beyond individual minds to the propagation of representational states through various representational media in larger interactive social systems, and even through historical time. A companion method to distributed cognition was therefore proposed. This "cognitive ethnography" would track, in the naturalistic contexts where cognition takes place, how events unfold in different communities of practice. Conceptualized this way, the ethnographer could literally "step into" cognitive systems to observe them in action.

The novelty of Hutchins' ethnographic project was using the *same* computational language that was usually reserved to describe internal, individual cognitive processes to account for what the anthropologist observed in the external world. Particularly, his own case study examined Western navigational practices in the US Navy as it was implemented in *pilotage*, determination of a ship's position relative to known geographic locations close to shore. He also compared such practices with the representational and algorithmic assumptions of Micronesian navigation, like celestial maps and other cultural resources and frames of reference employed in the famous *etak*-system. As a test case, navigation was well suited for analysis as these traditions. Despite variations on a common theme, all basically try to answer questions like "where am I, and how do I get to where I want to go?" In his comparison, Hutchins shows that even if two navigational systems basically solve the same computational problem, traditions can diverge profoundly in the representational assumptions that they bring to the problem-solving table. With respect to Marr's three levels of analysis *CitW* offered an ethnographic account of the second (representational) and third (implementational) levels of distributed cognitive systems. The reason for this is that any computational-level account is a formalized abstraction that is near impossible to convey in meaningful terms to an audience unfamiliar with the technical domain in question. So, while a computational-level account could theoretically be formalized for practices like navigation, many cognitive activities like those unfolding in the laboratory, are not sufficiently well-defined to be formally abstracted. In the ethnography of experimental science that follows, I will also keep the representational and implementational level centered.

This reconceptualization allows a reassessing of core assumptions about the minds and activities of enculturated agents, and to rethink what the source of this organization might be. Hutchins refers to this as "the attribution problem" (ibid.: 355). A byproduct of neglecting the cultural nature of cognition, the attribution problem may lead to an erroneous identification of boundaries in whatever intelligent system we are observing. Consequently, we may attribute the correct properties to the wrong system or, in the worst case, invent erroneous properties and spuriously attribute them to the wrong system. Distributed cognition therefore asks analysts to suspend judgment about the individual agent, and avoid over-attributing cognition to internal processes, so that one ends up with the wrong unit of analysis for explaining phenomena. Instead, we should first ensure that the phenomena under investigation are not caused by sociocultural practices which orchestrate interactions of brain, body, and culturally organized environments to produce higher level cognition (Hutchins, 2008).

Applying this externalist perspective to cultural systems, three features about cognitive processes come into view. First, cognition can be distributed across a social group. Secondly, cognitive processes may extend beyond the skin into the world so that internal and external structures, including materials like cultural artifacts and bodies, co-produce cognitive outcomes. Third, cognitive tasks can be distributed through time so that earlier events may transform later events (such as by propagation of media that encode representations). Consequentially, even a complete theory of internal processes cannot give us a complete account of many cognitive phenomena, since their dynamics are historically and socially contingent.

These three features have consequences for how we define the unit of analysis and the range of phenomena that can legitimately be invoked in accounts of cognitive processes. When applied to the cognitive life of experimental systems in laboratory science, as I do in the following, they also reveal intriguing features about the role of epistemic resources like artifacts and instruments in the production of knowledge. Hutchins calls such instances of material culture "cognitive artifacts" (1999), and in the laboratory they play a critical role in mediating scientific cognition by improving the informational environment of agents using them.

Cognitive artifacts are instances of material culture that are engineered to function as representational media, not simply by amplifying the cognitive powers of users, but often by transforming how tasks get accomplished. By crystallizing cultural knowledge and practices in physical structure, cognitive artifacts constrain action and embody invariant features of the world. Such artifacts range from a simple string tied around the finger for remembering, to lists tables and formulae, as well as specialized scientific diagrams and other tools. Even structures assembled for entirely different purposes may acquire cognitive functionality when humans interact with their environments and other agents in opportunistic ways. In subsequent chapters, we shall encounter a range of ethnographic examples that highlight the epistemic functions of diverse cognitive artifacts in the laboratory.

Related Germinations

The intellectual roots of distributed cognition are diverse. Hutchins points out that *Mind in Society*, a work spelling out the cultural-historical activity theory of Russian psychologist Lev Vygotsky, was published for an English audience in 1978 (Hutchins, 2001). Seven years later, American computer-scientist Marvin Minsky published his *Society of Mind*, a book title mirroring Vygotsky's. While Minsky used the language of social groups to account for what happens in the mind, Vygotsky's used the language of mind to account for the properties of social groups. For Hutchins, the timing of these two works suggested that "something special might be happening in systems of distributed processing" (ibid.: 2068).

There were other precursors, too. In 1964, the anthropologist John Milton Roberts published an essay on "The self-management of cultures," comparing patterns of informational management among four Native American groups. Roberts suggested that political and social organization in these groups could be conceptualized as information economies, where information could be received, created, stored, retrieved, transmitted, utilized, and lost (1964). Another precursor to the distributed view can be found in Gregory Bateson's notion of an "ecology of mind," who saw informational loops extending from the mind, through the body and the environment, informed by the nascent field of cybernetics (1972).

Other germinations are found in "connectionism"; an influential approach to modelling intelligent systems in terms of artificial neural networks, developed by the UC San Diego-based Parallel Distributed Processing Research Group beginning around 1980. Here, simplified models of natural neural systems were constructed from the weighted interconnections among units (analogous with neurons and synapses). By using weighted connections, it was possible to study the effects of synapses that link up neurons through differentiated activation patterns across processing units. In *Culture and Inference: A Trobriand Case Study*, Hutchins applied connectionist concepts to analyze land litigation among Trobriand Islanders (1980). Drawing on fieldwork data, Hutchins showed how reasoning in land litigation was derived from propositions about land tenure. Natives used these propositions to make inferences to new disputes via a set of transfer formulas. Comparing these reasoning strategies with Western thought styles Hutchins found that similar logical principles governed both. Connectionism was also embraced by the Cultural Models school in psychological anthropology, as a basis for how cognitive schemas could be constructed, operated, and interrelated (see Quinn, 2011). These ideas were also adopted by Maurice Bloch, in an influential critique of conflations between language and culture among anthropologists, and a failure to adequately distinguish between implicit and explicit knowledge in accounts of social behavior (1991).

Also foreshadowing a distributed approach, were the ideas of experimental psychologist James Gibson (2014), who developed an idiosyncratic "ecological" approach to a vision where perception was considered a form of action, rather than a passive process (see Shapiro, 2011 for an assessment). Human perceptual systems, in Gibson's view, derived all necessary information from invariants in the agent's environment, which could be utilized directly as a sufficient basis for action, without internal representations. This approach of "direct realism" clashed with foundational ideas about information processing in early cognitive science. Gibson's embodied account complemented that of philosopher-scientist Michael Polanyi, who popularized the importance of tacit, implicit knowledge in human experience, in contrast to propositional, explicit knowledge (Polanyi, 2005). In a telling example, Polanyi invokes the image of a junior physician learning to read x-ray pictures. A competent reader of x-ray imagery possesses perceptual and conceptual skills that are difficult to articulate verbally, but which afford the ability to see phenomena that others cannot (ibid.: 106). Elaborating on this theme, Pierre Bourdieu later developed his theory of practice around the idea that tacit competencies were unevenly distributed among members of various strata of society (1977).

Classical computationalism was also challenged by embodied accounts of knowledge emerging from phenomenological philosophy, which gained some prominence in anthropology (Csordas, 1990). Maurice Merleau-Ponty's phenomenology, for instance, inspired an influential critique of "standard" cognitive science based on the observation that cognition happens in the intersection between body and world, where bodies are both lived experiential structures and the milieu of cognitive processes (Varela et al., 1993: xvi). Like Gibson's, this body of work stressed the entanglements between perception and action through "enaction," motor activity and a suite of structural couplings and emergent dynamics between organisms and environment. Combining Gibson's ecological approach, phenomenology, and theories on embodiment, Tim Ingold further developed an "anti-representational" anthropology of knowledge (2000). In contrast to standard accounts in psychological anthropology, Ingold suggested that perception and action should not be seen as *culturally mediated*. Instead, he argued that humans perceive the world in a direct relationship, by moving about and making use of its many affordances through active, situated, and skilled engagement. This placed Ingold in the odd position of being both an "anti-cognitivist" and an "anti-culturalist."[8]

Hutchins' work on distributed cognition also developed in parallel with an influential "embodiment" thesis about language use and meaning construction, as a response to Chomsky's generative program (see Fauconnier, 2006 for an overview). This work in "cognitive linguistics"

[8]Tim Ingold's dismissal of representations makes his framework ill-suited for more detailed interactional analyzes of action. In science there is abundant interplay between internal and external representations.

tackled a diverse range of representational phenomena, based on the view that cultural meanings arise from, and are conceptually constrained by, the kind of bodies we possess as corporeal human beings. I return to the relationship between distributed cognition and embodied meaning construction in Chapters 6 and 7.

Other scholars turned to material culture. Drawing out the implications of Gibson's notion of affordance in new directions, cognitive scientist and designer Donald A. Norman investigated the cognitive consequences of artifacts, and the role of representational technologies in social systems (1992).[9] In opposition to the intracranialist orthodoxy, the philosopher Daniel Dennett also articulated an influential "transcranialist" position (1996). Minds, as he writes, are "composed of tools for thinking that we not only obtain from the wider social world, but largely leave in the world, instead of cluttering up our brains with them" (Dennett, 2000).

Another widely discussed conjecture on the constitutive role of external resources for cognition was offered by Andy Clark and David Chalmers in "The Extended Mind" (1998). Rather than empirical demonstration, Clark and Chalmers provided a thought experiment involving Inga, a woman with normal cognitive function, and the Alzheimer-impaired Otto, who meticulously kept his memories in a notebook. Here, they argued for dissolving artificial boundaries between internal and external cognitive processes, based on a principle of parity: "If, as we confront some task, a part of the world functions as a process which, *were it done in the head*, we would have no hesitation in recognizing as part of the cognitive process, then that part of the world *is* (so we claim), part of the cognitive process" (1998: 8).

Later, Clark introduced the "principle of ecological assembly" (PEA). The PEA, which we will revisit later, says that when cognitive agents are facing a task, they will recruit problem-solving resources eclectically and indiscriminately to achieve an acceptable result, with minimal effort (Clark, 2008: 13). It does not really matter whether these resources are neural, bodily, social, or environmental. The important thing is that our

[9]Norman founded the Department of Cognitive Science at UCSD, an intellectual ground zero for several key works in this tradition.

inner environment aligns with designed portions of our external environments. A tool-using cognizer must be sufficiently intelligent to recognize and use different tools, which in turn endow users with powers that were unavailable before the tools came into use.

Here, it should be noted that Hutchins' idea of distributed cognition and Clark's notion of extended mind significantly overlap, but that some differences in emphasis are worthwhile to unpack here. First of all, distributed cognition *is not a theory of a special type of cognition*, but a *framework for the study of all kinds of cognitive processes*. These may span from low-level neural processes, up to entire sociocultural assemblages that develop over large timespans, such as languages, writing systems, or other representational technologies. It tackles questions about the elements involved in producing cognition, in addition to developing hypotheses about the relation and interactions between elements. Distinctions between distributed cognition and the extended mind primarily concern the emphasis placed on the role of cultural transmission for the constitution of cognition, as well as demarcations of the scale and units of analysis (Hutchins, 2011). Hutchins suggests we may consider "extended mind" as a specific hypothesis nested within distributed cognition, with the latter being a more overarching framework for dissecting cognitive phenomena. Accordingly, the extended mind picks out "a particular class of distributed cognitive systems that operate on a spatial scale somewhat larger than an individual person," and on a "temporal scale typically completing operational cycles on the order of seconds or minutes" (2014: 37). At this mid-level scale, resources internal to an individual are coordinated and coupled with external elements in an agent's close social and material environment to produce certain cognitive outcomes. Clark calls these proximate interactions "ecological assemblies," while Hutchins prefers the term "functional system". However, distributed cognition does not only aim to account for cognitive events in an individual's immediate surroundings, but also to characterize cultural ecosystems at larger spatial and temporal scales. While extended mind hypothesize that there is usually a center for cognitive activity, distributed cognition does not presume a focal point in the traffic of representations a priori. The distributed view simply states that questions about the legitimate boundaries for cognitive systems must be determined empirically, based on the density in the propagation of representations between elements that make up the system.

Differences in explanatory scope notwithstanding, both frameworks remain agnostic about the constitutive role of internal representations (i.e., sequences of abstract symbols) in human cognition, or whether our faculties are better described as products of connectionist networks and other dynamical systems (Rupert, 2009). For example, Hutchins considers the thesis on "modularity of mind," which emphasizes certain types of cognition as products of evolved biological structures with specific functional circuits dedicated to information processing for particular domains, to be a "clear example of taking the distributed cognition perspective" (2014: 37). Likewise, the architects of extended mind consider the hypothesis to be "compatible with both connectionist and classical views, with computational and non-computational approaches, and even with internalism and externalism in the traditional debates over mental content" (Clark, 2008: xv–xvi). But while Hutchins is explicitly convinced that "humans actually process internal representations of symbols," he does not accept that "symbol manipulation is *the* architecture of human cognition" (Hutchins, 1995: 370, my emphasis). This agnosticism about the implementational-level details is partly methodological. Cognitive ethnography is based on the principle that it is difficult to infer lower level constituent processes from higher level, emergent phenomena by observing cognition in the wild. Rather, the framework redresses an artifact of intellectual history, where symbol processing was assumed to be inside because "we took the computer as our model of mentality" (ibid.).

To summarize, the works surveyed here make up a diverse research agenda for exploring cognitive phenomena, guided by far broader ontological commitments than classical approaches (Shapiro, 2011). Drawing on a helpful typology by Robbins (2009), the gist of these claims can be outlined as follows. First, cognition does not just depend on the brain, but also the body in terms of causality and constitution (*embodiment*). Secondly, cognition routinely recruits structures in the environment (*embedding*). And third, cognition extends beyond the individual organism (*extension*). The goal of extending computational processes into the material and cultural environment, is not to establish a *unified* theory of cognition. Rather, the hope that we can "sift the wheat of computation from the chaff of individualism" (Wilson & Clark, 2009: 61). Let us now see how this is relevant for the ethnographic study of scientific practices.

Connecting Cognition, Materiality, and the Social in Studies of Scientific Practice

Inquiry into the nature of scientific knowledge obviously has deep philosophical roots. In cognitive science, the study of scientific reasoning was launched by Herbert Simon, an early pioneer who envisioned that artificial intelligence could help explore the process of scientific discovery as a model for understanding human reasoning in general (see: Giere, 2008). But although Simon's ambitions to unveil the true nature of scientific reasoning through AI was unsuccessful, it spawned numerous studies on the cognitive dimensions of science, spanning topics like models and visual representations, reasoning, judgment, and conceptual change. These efforts resonated with Willard O. Quine's program of "naturalized epistemology," an attempt to bring philosophy and the empirical sciences into a close dialogue about the nature of human knowledge (Godfrey-Smith, 2009: 150–151). A similar ambition was visible in Kuhn's work on the nature of conceptual change in science (2012), which also grappled with the interplay between conceptual representation and perception (Kaiser, 2016; Nersessian, 2003).

One challenge for realizing Quine's program today is that cognitive and social studies of science is regularly conducted in relative isolation from each other. As Nersessian observes, such studies tend to "line up on either side of a perceived divide between cultural factors and cognitive factors in knowledge construction, evaluation, and transmission" (2006: 125). On one hand, cultural explanations of scientific development seem to black box cognitive dimensions. One the other, cognitive studies seldom make cultural factors an integrated part of the analysis, despite paying lip service to their importance. But any incongruity between these perspectives is illusionary and artificial.

One promising route to productively integrate cognitive and social studies of scientific practice, lies in the application of distributed cognition. This is not a new proposal. Ronald Giere, for example, has clarified the epistemological basis of a distributed account of science (see Giere, 2010 for a summary), while Nersessian and colleagues have operationalized the framework in historical case studies of physics and long-term ethnographic engagements with biological laboratories (see Chandrasekharan & Nersessian, 2015 for a recent interpretation). Additionally, there have been productive exchanges about the explanatory value of such applications (Brown, 2011; Magnus, 2007; Magnus & McClamrock, 2014; Toon, 2013; Vaesen, 2011). Approaching this subject matter from the view of anthropological linguistics and interactional analysis, Charles Goodwin has also studied the multimodal, communal character of scientific practices (Goodwin, 1994, 1995). Pushing this sort of interactional analysis in novel directions, Morana Alač mobilized distributed cognition for a series of ethnographic laboratory studies that show how works on fMRI scans acquire meaning in the hands of experts through embodied, social, and material interaction with scientific visuals (2011).

In addition to these attempts at respecifying cognition as action, there have also been efforts to carve out an anthropologically informed account of scientific practice that synthesizes the distributed framework with theories about evolved cognitive faculties (Heintz, 2004, 2007). Through case studies from the history of mathematics, Heintz develops an "integrated causal model" that combine theoretical tools from cognitive science with a naturalistic approach adopted from the Strong Program in the sociology of science. To move beyond the impasses of past debates about rationality, Heintz points to the human ability to engage in meta-representation. The production and use of *representations of representations*, he argues, is central for creating new scientific knowledge and conceptual change. Being an evolved disposition that all humans share, he proposes that meta-representations are critical for the evolution of distributed cognitive systems in science, as they enable humans to assess the epistemic status of the output from any innate cognitive dispositions. In this view, scientific culture is predicated on vigilant reasoning about one's intuitions and beliefs about phenomena. Scientists accomplish such reasoning by propagating representations that arise from modular minds across diverse social and material loops. This distributes and transforms information beyond what Clark calls the "biological skin-bag" (2003: 5).

For Heintz, a satisfying description of science must consider both its social embeddedness and other cognitive constraints. Advocating a strong version of the "continuity hypothesis" about the relation between everyday thought and scientific reasoning, Heintz suggests that it is not possible to identify an absolute criterion for demarcation (2004: 396). Science in this respect, builds and depends on "common sense" or "human meta-culture," which is "innately grounded, and species-specific, apprehensions of the spatiotemporal, geometrical, chromatic, chemical and organic world in which we, and all other human beings, live our usual lives" (Atran, 1990: 2). Still, as underscored by ethnobiologist Roy Ellen, scientific knowledge is nonetheless both more efficacious than common sense, and enjoys a very different status, so that the assertion that it is "no more than common sense in a specialized institutional setting" comes close to saying nothing at all (2004: 432). Clearly, science is different from common sense. But how? From an anthropological perspective, the answer is twofold. First, the transformational powers of science are derived from institutionalized mechanisms for meaning-making through the "establishment, shaping and maintenance of intersubjectivity" in a community of practice (ibid.: 433). Secondly, scientific cultures do not only belong to the realm of ideas but encompass a range of material practices. The significance of materiality for scientific cognition is especially visible when we enter the experimental laboratory.

Beginning in the 1980s the field of science studies, broadly construed, underwent a "post-Kuhnian move away from the hegemony of theory" (Rheinberger, 1997: 1). One contribution was Ian Hacking's "Taxonomy of elements," a conjecture about the interplay between ideas, things, and marks in laboratory practice (1992). In this context, Hacking refers to fields that partially create the very phenomena they scrutinize, which "seldom or never occur in a pure state" and whose interference require isolated instruments (ibid.: 32) (Table 1.1).

Two aspects of Hacking's typology deserve brief comment. First, his elements do not include people, nor the building the experiment takes place in, and institutions. Neither does it account for authors

Table 1.1 Elements of laboratory practice, summarized after Ian Hacking (1992)

IDEAS	
1. Questions	Research questions of all kinds
2. Background knowledge	Seldom systematized but taken for granted both in the experimental process and in the write-up of results. Fuzzy boundary with 4 and 5
3. Systematic theory	High-level theory does not have direct experimental consequences and is seldomly revised on basis of experimental outcomes
4. Topical hypotheses	What physicists call "phenomenology." Connects systematic theories to observations within the experiment. More open to revision than systematic theory
5. Modelling of apparatus	Theories and background knowledge about instruments and equipment. Seldom equivalent to what is being pursued in the experiment
THINGS	
6. Target	Preparations and modification of the object of investigation; a tissue section, modification of cell with a foreign substance, and so forth
7. Source of modification	The apparatus that interacts with a target, such as a biological molecule delivered by microinjection
8. Detectors	The thing that measures the interference or modification of the target, like a DNA sequencing machine or similar instrument
9. Tools	"Humble things" that experimenters rely upon; off-the-shelf devices like micropipettes, test tubes, et cetera. Context-dependent and overlaps with 8
10. Data generators	The thing that counts; generators transfer representations of one kind into a different medium. May overlap with 8
MARKS	
11. Data	Outputs from detectors and data generators; not yet interpreted inscriptions. Some call this "raw data"; others say these are already interpreted and perspectival

(continued)

Table 1.1 (continued)

12. Data assessment	The first of three kinds of data processing; calculations of probable errors and other supposedly theory-neutral statistical techniques
13. Data reduction	Large quantities of unintelligible data requiring transformations to be meaningful
14. Data analysis	Events under scrutiny can be chosen, analyzed, and presented computationally. These are not theory-neutral statistical techniques, but relate to 1, 4, and 5
15. Data interpretation	Requires background knowledge combined with 3, 4, and 5

and audiences of scientific works, and issues of power. It is simply a typology over the "internal" epistemic resources found in experimental practices. External resources used to promote experimental results, or those involved in the politics of funding and allocating research priorities, have no place in the typology. Hacking thus defends the "conservative" internal–external heuristic in science studies (ibid.: 51). He does so against those who would argue that *stabilization* of a given result only becomes fact when the internal resources of experiment and laboratory get recruited into an alliance with external ones. Secondly, although Hacking considers these configurations to be epistemically "self-vindicating," the scheme does not deny the possibility of mission-oriented science, where techniques and devices developed in the laboratory move outside its boundaries for practical applications. Self-vindication simply implies that laboratory sciences become epistemically stable and consistently true to phenomena as theories and instruments become mutually adjusted to each other.

A problem with Hacking's inventory is that it does not tell us much about the structure of how these fifteen elements interact in a vibrant laboratory environment. In Hasok Chang's words: "It is as if he gave us the vocabulary of scientific practice, without a grammar to go with it" (2014: 69). Aspects of this grammar can be found in Hans-Jörg Rheinberger's work, who invoked the concept of "experimental systems," the basic unit of activity that propels the growth of knowledge in bioscience (1997, 2010). Here, experimental systems are driven forward by the interplay between two elements. The elusive, unknown objects of scientific inquiry are "epistemic things," which result from a choreography of "technical things," the stable context of experimental work that includes instruments, laboratory techniques, concepts, and social resources.

An emphasis on experimental systems as the prime loci of epistemic action presents us with a view on scientific practice that deeply resonates with a distributed account of science and its attention to the role of material culture in cognitive processes (see Rheinberger, 2010: XVI). In this book, my goal is *to explicitly flesh out the implications of taking the distributed view on these units of knowledge production*. By looking ethnographically at how experimental systems, as complex cultural-cognitive ecologies, come to life, we may truly integrate social and cognitive understandings of science. Since no discipline has yet taken full ownership of the cognitive life of epistemic things, their character remains relatively unknown, with ample room for novel contributions. While historiographic accounts of experimental systems must contend with mapping the epistemic properties of a given system in retrospect based on written source materials (Rheinberger, 1997: 223), cognitive ethnographies of scientific practice allow us to collect data on the epistemic character of embodied interactions and material engagements in approximately real time.

We can now see more clearly that the gap between the social, cognitive, and material is not insurmountable. This task is also greatly helped by the fact that historian-philosophers like Rheinberger and Hacking have *implicitly* framed their descriptions of experimental science in accordance with principles from distributed cognition, thereby facilitating an integrative project. Hacking, as we saw, conceptualized laboratories as input–output devices which transform, reconfigure, and coordinate ideas, things, and marks. Similarly, Rheinberger emphasizes the constraining power of experimental systems. Rather than seeing experimental outcomes as byproducts of internal cognitive processes at work in the experimenter's brain, experimental systems provide a "space of representation," which scientists can use to "think with" (1997: 105). Giere's notion of "scientific perspectivism," which takes the outputs of scientific instruments to be fundamentally perspectival, further extends this line of reasoning (Giere, 2010). Accordingly, much scientific observation and reasoning is only possible due to the support of material and conceptual aids, like models and theories. These afford scientists with an ability to manipulate phenomena of interest. Such a "laminated picture" of an intercalation between theory, experiment, and instrumentation also stand out from Galison's work on subcultures in physics (1997: 138).

Conceptualizations of scientific knowledge production as a kind of distributed cognition also appears elsewhere in science studies. In *Epistemic Cultures*, for example, Knorr-Cetina explicitly invokes a vocabulary similar to Hutchins' at least six times in her account of knowledge-making in high-energy physics and molecular biology (1999: 25, 165, 174, 179, 180, 242). On two occasions she employs the qualifiers "sort of," and "something like" to convey how material artifacts aid scientific work. Additionally, she introduces the concept of the "laboratope" (ibid.: 278), an artificial environment where knowledge evolves. This analytical unit is similar to what Hutchins' later described as "cognitive-cultural ecosystems," systems of constraint satisfaction that settle into a subset of possible configurations through stable, coherent practices (Hutchins, 2014).

It is, however, hard to assess whether Knorr-Cetina considers knowledge production in these fields to be *literally* distributed and extended, or whether she intends a deflated, *metaphorical* reading (Giere, 2002). Her interpretation is also problematic from the perspective of distributed cognition. For instance, Knorr-Cetina appears to claim that epistemic processes in molecular biology primarily occur at the level of individual subjects, while reserving truly distributed knowledge-making to what happens in the large experiments of high-energy physicists at CERN. However, an analysis in terms of distributed cognition would consider a single molecular biologist interacting with spreadsheets or pen and paper to calculate reagents, just as much a product of a distributed cultural-cognitive ecosystem as the epistemic labor of thousands of physicists around the globe, collaborating on a particle detector. The difference lies in the *density* of connections between elements in the distributed system, which must be specified by asking what information goes where, when, and in what form. Distributed cognition, in other words, implies that cognitive resources are *literally* distributed among the elements in a cultural-cognitive ecosystem. Instead of specifying the traffic of representations involved, Knorr-Cetina instead invokes Durkheim's notion of "collective consciousness" to carry the explanatory burden of how knowledge comes to life.

Cognitive Bloat and the Question of Agency

If the cognitive anthropology of scientific knowledge must be widened to include material culture and situated practice, as I have proposed, what does this imply for our conception of individual agents as knowing subjects? There is no shortage of studies on technoscience that propose a rethink of rationalist intuitions about the loci of agency, by radically moving beyond anthropocentric analyzes and achieve analytical symmetry by equally weighing contributions from human and nonhuman entities in shaping epistemic outcomes. Andrew Pickering, for example, has articulated a position "where science and technology are contexts in which human agents conspicuously do not call all the shots" (1993: 562). His account seeks to move away from an understanding of science as primarily a *representational* activity. Instead, Pickering encourages us to think about the world encountered by scientists as one filled with agency, and not primarily littered with representations like facts and observations. In this view, our world is continuously doing things (he invokes weather as an analogy), and science extends how humans cope with this agency, by enlisting a wide variety of tools and other resources.

Pickering sketches two main positions on how to conceptualize agency for science studies. One is the fundamentally *asymmetrical* position that considers scientists to be agents who provide accounts of material agency in the world, like physical laws, biological mechanisms and so forth. These scientific accounts can then be studied as products of human activity. Alternatively, it is possible to tackle material agency itself. But this puts scholars of science studies in a position where they must defer analytic authority to the natural sciences to explain how material agency *really* works. These are the stock positions of traditional humanist approaches, both pragmatist and symbolic interactionist, as well as interest theories, including certain flavors of the "Strong Program" in the sociology of science.

Alternatively, Pickering suggests a more semiotically performative conception of agency that may engender analytical symmetry between human and nonhumans, that could help us move beyond representationalism. But this is problematic. Although it would take material agency seriously by factoring in the performance of technical apparatuses in the material world, it risks a retreat to an image of "science-as-representation." This can be avoided, says Pickering, by looking at material agency as being "temporally emergent" in practice. Scientists cannot know how the material agency will behave and must develop devices to probe it by "tuning" into signals that cannot be known in advance. Also, to avoid "whiggish" accounts of science we should only draw on those epistemic resources that are available to the scientists themselves. Here, Pickering appears to make the strong claim that those who aim for a real-time understanding of science as practice, are on an equal footing with respect to the material agency of nature, like the scientists we study. According to Pickering, the latter does not (when working in the present), have the benefit of hindsight about what will, after all, be established as facts by future research (1993: 563). A second problem with the performative semiotic conception of agency, is the sticking point of intentionality. While humans have intentionality, most nonhumans, apart from some higher animals, do not. There is no material counterpart to intentionality, notes Pickering, but the intentional structure of human agency is always, like material agency, emergent from real-time activity where a dialectic of resistance and accommodation between the material and human creates a "mangle of practice."

Scientists cannot know in advance whether their attempts at intervening and understanding the world will succeed or fail, and it is only through trial and error, unfolding over time, that the "contours" of this agency can be known. For Pickering, this is not a technological determinist view of science, where material agency "forces itself upon scientists" (1993: 577). Instead, such resistances co-exist alongside human goals and plans. Resistances in science thus have a hybrid quality, irreducible to neither material agency nor human agency. This "mangle" pulls material agency into the pathway of human agency, structuring it so that in effect neither material nor human agency has "its own pure dynamics" in the co-production of knowledge. According to Pickering, scientific activities link up existing cultural practice with future goals, but he does not want to say the relation is mechanical or causal. When human scientists accommodate material agency, they must revise intentions, plans, and goals. This becomes, in Pickering's terminology, a "dance of agency" between the materiality of nature, apparatuses, theories, models, and techniques. Similar conceptions of agency can, with differences in emphasis, be found in the works of Bruno Latour (1999b). Also here is agency considered a network effect of heterogenous associations between humans and nonhumans, one that is very different from the kind of "causal agency" that gets exercised when a physical entity affects the scientist's sense organs by refracting light through a microscope, for example (see, for instance, the exchange by Bloor, 1999a, 1999b; Latour, 1999a).

From the perspective of distributed cognition, these radical attempts at destabilizing the human agent operate on a level of abstraction that leaves out critical information about the sociology of interaction, microstructures of representational cascades, and the relevant cognitive divisions of labor among scientists. Details about these matters would be necessary to flesh out true examples of nonhuman agency in science. Latour, for example, argues against separating the mental from the material environment in ways that *appears* to harmonize with a distributed perspective (see J. D. Keller et al., 1996). But for Latour, it seems that if cognitive processes can somehow be identified outside of the embodied brain, then they cannot be inside at all. So rather than reconsidering the boundaries of the unit for cognitive analysis, Latour wants to sweep clean the psychological agency of human actors in its entirety. From the perspective of distributed cognition, such a radical, "mind-blind" conclusion about the loci of agency does not follow. Human cognition certainly moves across the boundary of the skull, but this does not mean that what occurs on the inside is of no importance to understand the traffic outside. Neither Pickering nor Latour offers the reader detailed empirical descriptions of how artifacts and other nonhuman entities can exercise "agential" behaviors in the absence of human interaction. For instance, nowhere in their accounts do nonhuman entities appear to intentionally change the informational character of the environment, like epistemically minded flesh-and-blood scientists try to do. In fact, even Pickering and Latour's analyzes appear to accommodate minor roles for human representational agency in their performative accounts, by acknowledging scientists as intentional agents that use language, plan, model, theorize, write, and so on. Humans therefore still appear to play a special role in the case studies of scientific knowledge production we are confronted with, since only humans appear to have a capacity for instigating certain classes of action.

In the debate on distributed and extended cognition the problem of locating agency outside the boundary of the human has primarily been framed around the issue known as "cognitive bloat." Cognitive bloat is an imagined consequence of the two-way coupling between brains and environment, in which everything people interact with somewhat absurdly becomes part of their mind. Bloat raises the challenge of identifying and demarcating functional relationships between human agents and their environment that imply true instances of cognitive extension. Fortunately, this challenge has been addressed by outlining a set of "trust and glue" conditions for what constitutes genuine examples of extended cognitive systems (Clark, 2008). These state that the resources in question must be reliably available and typically invoked (*availability*). Furthermore, retrieved information must be endorsed by default (*trust*), and easily accessible, as and when required (*accessibility*).

Distributed and extended cognition views humans as biological agents with a natural and cultural history that has endowed us with capacities for interaction with our environment that fulfill these conditions. Humans and other organisms have not just evolved through natural selection so they are better adapted to their environments, they also engineer their environments through a process of "niche construction" that can transform the effects of natural selection (Sterelny, 2004). Beaver dams provide a telling example of this process, as the environmental transformations carried out by beavers may have fitness consequences on their descendants. One form of agency that is intimately related to niche construction is *epistemic agency*; the capacity of certain biological agents to engineer their own environments to acquire information that is not ready at hand. A predator that moves into elevated terrain to have a better view of its prey, while remaining partly hidden in the bushes, can be said to exercise a low-level form of epistemic agency (other animals may demonstrate more sophisticated forms). A human that writes down a shopping list on a post-it, and sticks it on the fridge as a reminder, exercises a higher level epistemic agency involving the use of an epistemic artifact as a mnemonic aid. Experimental systems in the laboratory scaffold more complex cases of such agency, as we shall see. While low-tech epistemic agency is ubiquitous among animals, humans rely on higher level epistemic agency, whereby they attempt to improve their informational environments and create meaningful representations by using sophisticated epistemic artifacts to represent the world in ways nonhumans do not (Sterelny, 2004: 240).

Here, another asymmetry between humans and nonhumans come into view, namely, our ability to engage in trusting relationships with both conspecifics and nonhuman entities. In accordance with Clark's "trust and glue" conditions for cognitive extension, Heintz points out that trust is the "cement" of distributed cognitive systems in science (2007: 319). Representations about what are trustworthy components in an open-ended endeavor like scientific research is what keeps these extended cognitive systems together (Miller & Freiman, 2020). Changes in representations about who or what is trustworthy with respect to knowledge acquisition, what Wagenknecht dubs "epistemic trust" (2015: 162), can subsequently change the division of labor in the cognitive system. Such trusting relations with nonhumans are expressed through everyday statements like "the qPCR-machine gave accurate readings," and "the electrophoresis yielded positive results." In Chapters 3 and 4, we shall see how a gradual development of a new experimental system depended on the research community learning to trust the epistemic outcomes of new apparatus and techniques, while cultivating epistemic vigilance as good scientists.

By prematurely extending agency to all kinds of nonhuman entities we risk obscuring fundamental cognitive asymmetries between humans and other entities, such as the capacity to engage in representational activities for epistemic reasons, and to form trusting relations. Instead, we should refine our accounts of how the material cultures of situated practice support the propagation and transformation of representations. Cognitive ethnography is uniquely suitable for this task, and can help us go beyond the limitations of framing of science through social-reductionist categories (Creager, 2002: 319–320). The view I advocate here thus acknowledges the contributions of nonhuman entities to scientific practice, as the difference between human and material agency is surely one of degree, not kind. But I reject a more radical metaphysical interpretation, to maintain human exceptionalism for certain representational activities (which humans, as far as we know, alone are capable of). The proposal to build an entirely new metaphysics of agency is simply unattractive, and a gambit for which there is little empirical support. It entails adopting metaphysical commitments whose epistemic costs for science studies are simply too great to justify (Giere, 2004, 2007).

Instead, I argue that humans are central as semiotic and epistemic agents in distributed cognitive systems. A true understanding of what is internal to the epistemic agent hinges on first specifying the computational and representational work that is being performed on the outside. Scientific knowledge production, then, should be considered a continuous process of representation and re-representation, where material artifacts participate in the traffic of cognitive representations across various material media in an open-ended process of meaning-making. By focusing on how scientists use tools and social structures outside the epidermis of skin and skull, we may, to paraphrase historian Jürgen Renn, avoid playing off against each other the cognitive, social, and material dimensions of science (2015: 39).

Justification for adopting a distributed perspective on experimental systems comes from its empirical and theoretical productivity, and not from pressing metaphysical needs to revise what we mean by an agent. If necessary, we can carve a space for material agency as a relational property by following the tempered advice of Malafouris, who advises us to not insist on asking *what* an agent is, but rather *when* an agent is (2013: 147–148). By viewing the world as activity-centered and not intrinsically human-centered, we can see scientific practices as projects for material engagement between people and things, without losing sight of cognitive accomplishments. In this view, agency becomes a "relational and emergent" product of material engagement with the world (ibid.).

This is supported by a simple fact. Ours is a species that scaffold its own thinking and meaning construction in unimaginably ingenious and recursive ways. As astutely observed by Andy Clark, we are not only self-engineering better worlds to think in, but also design worlds in which to build better environments in which to think, filling them with ever better-thinking tools, using these to fine-tune our utensils even more, educate ourselves in their use, and further refine our cognitive tools by building even better environments to cultivate them even more (1998: 59).

Perhaps nowhere are such instances of cognitive and epistemic scaffolding through engagements with our material world more ubiquitous than in scientific laboratories. To make sense of the cultural practices of cognition in the molecular science of salmon lice the first step will be to examine the context from which these thinking tools emerged. We must ask why and how such organisms were domesticated in the laboratory as objects of research for experimental biologists.

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