EPISTEMIC, COGNITIVE PRACTICES IN STATISTICAL CONSULTATIONS:

AN ACTOR NETWORK APPROACH

By

Kenneth Allen Wright

Dissertation Submitted to the Faculty

of the Graduate School of Vanderbilt University

in partial fulfillment of the requirements

for the degree of

DOCTOR OF PHILOSOPHY

in

Teaching and Learning

December, 2012

Nashville, Tennessee

Approved:

Rogers Hall

Kevin Leander

Richard Lehrer

Norbert Ross

Leona Schauble

DEDICATION

For Koura, Forest, Julia and Clara, thank you for coming into this world, for giving me the chance to raise you and for inspiring me along the way

and

for Carol and David, my parents, whom I wish were still here to see this day

ACKNOWLEDGEMENTS

Research reported in this dissertation was supported by NSF grant REC-0337675 to Richard Lehrer, Leona Schauble, and Rogers Hall.

I thank the faculty on my committee for their encouraging words, insights and many earnest prods to follow through with this work. From each I have learned so much. Special thanks to my advisor, Rogers Hall, for the many enlightening conversations, for the (usually) friendly arguments and for all his efforts to see me through this process.

Thanks to my spouse, Jamie Webster and to her parents, Bill and Cindy Webster for their forbearance and love. Thanks as well to my siblings Tina, Sally, Jay, Charlie, Jim, Lisa and Fred, their spouses and children, for their many encouraging words and support.

TABLE OF CONTENTS

TABLE OF FIGURES

CHAPTER I

THEORIZING EPISTEMIC, COGNITIVE PRACTICES

Introduction

Recent scholarship in the Learning Sciences has made progress in adapting literature in sociology and philosophy of science into theory and practice of pedagogy. Of special note for purposes of the aims of this investigation, is the substantial work in science education research around the topic of epistemology (Duschle, 2008; Elby & Hammer, 2010, Sandoval, 2005; Ford, 2008a, 2008b, 2012), the work in science and mathematics educational research in modeling (Lesh & Doerr, 2003; Lesh, Hamilton & Kaput, 2007; Lehrer & Schauble, 2006a) and the work in mathematics educational research around the topics of agency and dispositions (Boaler & Greeno, 2000; Hall & Greeno, 2008; Gresalfi & Cobb, 2006; Gresalfi, 2008). Despite this progress more could be done. The progress of scholarship and theory in the Learning Sciences might be enhanced further by sociological or philosophical work more focused on learning and cognition than has usually been the case. Such is the task of this investigation.

Generally speaking, the sociological and philosophical literature has had little interest in the embodied, real-time, talk and interaction of knowledge production, a “scale” of activity referred to here as an epistemic, cognitive practice. The investigation reported here attends to such an epistemic, cognitive practice in the sciences for the purposes of informing our understanding of learning and development within a scientific practice. Such an improved understanding may help realize the often explicitly-articulated intention in the Learning Sciences literature to foster learning environments in schools in some kind of image of professional scientific practice.

To illustrate the problem of poor imaging, and as I have argued in my Major Area Paper (2009), the general scholarship in modeling has been hampered by an undo focus on models as such, a focus more in keeping with dominant philosophical scholarship on models as abstract representations. Few sociological investigations have directed their attention to modeling practices as a primary topic of investigation, which I characterized as a situated practice of people and performative objects. I argued that, as a result, few scholars in the Learning Sciences have explored modeling as a practice and few students have encountered models as performative objects that might fail to or successfully predict events subsequently made to unfold.

As will be argued here, we should think of knowledge as something that takes shape and is made in practice into something more durable by means of some activity. Such activity is not necessarily "local" in that it does not necessarily unfold mainly within talk and interaction. Whether talk and interaction is or is not a propitious scale for witnessing the production of knowledge in a given conversational setting depends upon the purposes for which the parties to conversation have convened. The parties convening in this investigation do so for the purpose of designing research plans or for analyzing data. For this reason the meetings are rich in talk about knowledge. That said, as grounded as this investigation may be in talk and interaction, other situations of actions in other times and places are incessantly queried, problematized or reinterpreted by those who have assembled in order to talk and interact. These conversations are never only about the here and now.

This investigation is intended to meet a need for more fleshed out images of scientists struggling with the same *philosophical* questions that perplex us as we design learning environments: Is this plan of action legitimately scientific? Is this finding of ours true? Are the assumptions incumbent to this model appropriate to the situation at hand? We need more elaborate images of scientists struggling with the same *sociological* questions as do teachers striving to foster authentic, collaborative learning in the sciences: What actions taken by us will make this or that statement of fact sufficiently convincing? How do we achieve adequate consensus on matters of fact so that we may collectively conduct our work or report our findings? Are we acting like legitimate scientists?

Having better images of scientists doing the work of knowledge production can only enhance our understanding of what might be drawn from professional practice as we design learning environments. After all, as Lehrer and Schauble note in their review of current links between our understanding of "scientific thinking" (in professional practice) and "scientific literacy" (in pedagogic practice), "Images of the nature of science set the stage for the study of development [in pedagogic settings]. They inform what researchers choose to study and suggest appropriate means of study" (2006b, p. 156). Of particular relevance to our understanding of the nature of science in the practical sense of informing pedagogical design, would be investigations that follow scientists as they engage in the cognitive practice of concerted talk about knowledge claims. Such investigations might help us to understand the process, at a level of interactional detail more amenable to translation from professional practice to pedagogical practice, whereby an emergent sense of knowledge is made to be fixed and enduring. By focusing upon what scientists do in such circumstances, we might come to better understand cognition in epistemic terms. Such a focus on epistemic, cognitive practice directs our attention in directions of interest to linguistics, Cognitive Science and to the sociological schools of conversational analysis and ethnomethodology. One could productively focus upon a wider range of scales. Indeed, I have been engaged in investigations of learning as an ontogenetic process situated within multiple scales of cognitive change (Hall, Wright & Wieckert, 2007; Hall, Wieckert & Wright, 2010). The focus here on epistemic, cognitive practice mainly as a phenomenon within talk and interaction, should be seen as complementary to such investigations.

The concept of learning for this investigation derives from the primary focus upon epistemic, cognitive practices. It will usually be framed in terms of Hutchins' (1995) cognitive ethnography of the development of people as they become more familiar with, and attuned to, cognitive resources available to them. These resources may be textual traditions, representational forms, machines, or computers—the infrastructure with which people interact. According to this view, learning is situated at the interface of internalized structures "inside the skin" and infrastructural resources, which mutually adapt over time.

For the term, knowledge, I borrow from historical studies of epistemic practices (Shapin & Schaffer, 1985; Netz, 1999) and from Latour's metaphysical sense that relations among actors make for claims about our world (2005). Actors may be people or objects. According this view, knowledge is the outcome of a process within a complex system. Knowledge production is the forging of new relations among actors whose actions in the moment are emergent. As people produce knowledge they come to understand it in an embodied sense while also and necessarily encoding it in ways that assimilate it into their infrastructure, which is to say they learn it. The cognitive ethnography of Hutchins and the ethnomethodological sympathies of Latour differ in their assumptions and the kinds of tasks to which they have traditionally been applied. Nevertheless I feel that integrating these two perspectives will have purchase on the focus I will take in this investigation.

The laboratories of this investigation are sites organized for knowledge-production where cognitive practices lead to learning (presumably). Schools are sites organized for learning where knowledge production could occur if the cognitive practices cultivated there were designed for it. The majority of this investigation portrays epistemic, cognitive practices among researchers. In the final chapter I portray these epistemic, cognitive practices as images of what learning could be like in schools. I do so by means of commentary on literature in the Learning Sciences. The specific and respective meanings of cognitive practice, learning and knowledge production emerge from my interpretation and analysis of scholarship in cognitive ethnography, sociology, history and ANT. This scholarship also informs my use of the terms, performance, objects, internalized structures, necessity and generality. None of these terms are new. However, the combination of these numbers into an investigation of knowledge and learning may well be new. I employ a graphic organizer for these terms at the end of each of the selected episodes in the investigation.

Questions for this Investigation

The first aim of this investigation is to describe epistemic, cognitive practices in interactive and material terms—that is, to characterize how objects in nature are made to speak and how scientists voice them in constructing an accountable connection to nature. For a preliminary view on this topic, I draw from historical studies of epistemic, cognitive practices (mainly, Shapin & Schaffer, 1985 and Netz, 1999) and from Actor network theory (mainly, Latour, 1993, 1999, 2005). From this literature, I develop an interpretation of object agency at an interactional level of talk as researchers engage in knowledge-producing activity. I identify some typical aspects of epistemic, cognitive practice as situated within the cognitive activity of people in interaction with artifacts. This is the main task of the literature review below. Translating this aim into a question, I ask, *How do objects typically perform or how are they typically made to perform in cognitive, epistemic demonstrations?* I explore this question by means of historical and interpretive analysis.

The second aim of this investigation is to portray researchers conducting demonstrations as a means of convincing one another of the truth they perceive as visible within some representational form. I am not aware of other investigations that have attempted to explore this level of epistemic activity in terms of object agency as promoted by actor network theory (ANT). In order to characterize the cognitive activity of people in interaction with artifacts, as well as related learning, I draw from cognitive ethnography (mainly, Hutchins, 1995). The main body of this investigation is dedicated to this simple task of characterizing the cognitive activity of leaning to invoke object agency in these demonstrations. Translating this aim into a question, I ask, And, *How are people and objects recruited into the infrastructure of demonstrations so as to make things hold together as new knowledge?* The historical studies tell us (especially if we read them as Latour does) that we should expect people to be dealing with artifacts, borrowing rhetorical forms from other settings, jostling or fighting for authority, but also positioning objects to do things of themselves so that the thing to be argued can be demonstrated to have transpired without direct intervention. We are to understand that scientists have implicitly imbued objects with agency to do the things observe them to be doing without interference. But whether this interpretation holds up in ordinary, everyday practice, in real time as researchers engage in routine discussions is the primary empirical question of this investigation.

The third aim of this investigation is to point the way toward a translation of these descriptions about recruited objects, from professional settings to educational ones. The suggested translation is to characterize both as settings for epistemic, cognitive practices. Stated as a question, I ask, *How can a better understanding of epistemic, cognitive practices in professional settings be made relevant to the development of practical, epistemic, cognitive competencies in educational settings?* This is the primary task of translation from the literature in the philosophy and sociology of science to the Learning Sciences research. I would like to think of this investigation as something of a hybrid, an explicit attempt to investigate a professional practice for the purpose of informing a pedagogical one. This third question will be the subject of the final chapter.

Recruiting Scholarship into ANT

We now turn to a selection of literatures that will help frame the terms for this investigation. As mentioned, the primary kind of epistemic artifact to be presented here is comprised of representational forms designed to demonstrate things, such as graphs, tables, equations or "narrative assemblies" (Hall, Wright & Wieckert, 2007). My concern is with the cognitive practice of such demonstrations, which entails several dimensions of activity, including the agency of participants and objects, metaphysical questions about how things relate to each other and epistemic questions as to how we come to know things. I have adopted actor network theory or ANT (Latour, 2005) in order to interpret this activity, because it has grown out of a desire to deal with such a multi-dimensional perspective.

It might be more accurate to say that I have adopted Latourian actor network theory, as Latour is far and away the most influential party in the loose collective of scholars who identify themselves with ANT. It is not really a discipline or a field so much as it is a movement. Versions of sociology, philosophy, anthropology, political science and many other disciplines could be categorized as influenced by ANT. Latour selectively assimilates ideas from others, and then writes about his own synthesis, as will become apparent in the literature review. In my own synthesis of this literature I have found myself imitating this assimilationist technique, reading from authors' explicit arguments, sometimes reading beyond those explicit arguments and sometimes reading against what they say.

Performing Objects in Talk

This part of the section on recruiting scholarship into ANT explains how objects are animated in talk and interaction and how they can be discursively positioned. An account as provided by Ochs, Jacoby and Gonzalez (1994), provides some insight into ends to which talk and interaction are employed in natural sciences research. It depicts physicists in front of blackboards as they integrate language, gesture and representational artifacts in order to construct versions of hypothetical or imagined experimental events. The linguistic resources exploited are embodied and interactional. Participants use their bodies to play out speculative dramas as to how antiferromagnetic materials behave. A primary linguistic resource is to interweave the identities of materials with the identities of physicists. Collaborative, discursive activities such as these constitute the means for coordinating activities in laboratories. We should assume people convene and talk for a reason. From this perspective on such collaborative talk, we should ask what they accomplish in them?

Part of the answer to that question may come from reading against or beyond the authors' explicit argument. The antiferromagnetic objects of their talk may well have identities interwoven or fused with the identities of physicists, but these objects are also active in their own right. They mediate activity. Courses of action in the physicists' collaboration are contingent upon the performance of these objects as they are subjected to magnetic fields and variations of temperature, as made manifest in the participants' talk around graphical representational forms. It is helpful to regard these objects as present but displaced (Latour, 1990). A performance of these objects, related in talk or in graph, might confirm a state of affairs that joins previously published work to current work. Scientists' talk facilitates collective agreements as to which knowledge claims might be contingent upon which kinds of performances of objects. This collective agreement is hard won and is often only achieved as scientists go "down to the wire" once time becomes scarce (Ochs & Jacoby, 1997). Yes, the linguistic means for achieving this agreement is crucial. In this investigation I will document similar means. That said, the ends for this activity are to position the performance of artifacts so as to be tantamount to the establishment of knowledge—a knowledge product. In summary, I appropriate two principle ideas from these authors' work. The first idea derives from a fair reading of the authors' explicit argument—the cognitive practice of demonstrations is comprised and constituted by a richly embodied discourse. The second derives from reading more deeply into the authors' argument—material objects appearing in talk are performative (Pickering, 1995).

In studies of talk and interaction in which I have participated, colleagues and I have directed attention to discourse at both the level of talk and interaction as well as at the level of narration, stories and parables (Hall, Wright & Wieckert, 2007; Hall, Wieckert & Wright, 2010). We have described "narrative assemblies" of people, representational artifacts, wild objects and instrumentation as people anticipate changes in future epistemic, cognitive practices. These are collaborative achievements of talk in which new ways of organizing collective scientific work are entertained. These narrative assemblies are epistemic in that they entertain reconfigurations of work so as to sustain durable claims about knowledge. They show that in practice researchers take the agency of things into account even as they contemplate their own capacity to judge and evaluate. In this investigation I continue this line of work on collective talk and interaction, and nonhuman agency, but turn more specifically to the performance of objects within historically developed argumentative forms in which such objects form into stable knowledge relations.

The sections that follow will review three principal works of scholarship. These works have three things in common. Each is of book-length. Each describes cognitive activity in social and material terms amenable for others to build upon, marking each as an important work in modern scholarship of learning and development. Finally, and precisely for this reason, each has been appropriated by Latour for the cause of ANT. The authors as a group are somewhat ambivalent about this endorsement.

At the Interface of Internalized Structures and Infrastructure

This part the section on the recruitment of scholarship into ANT develops an interpretation of Distributed Cognition (Hutchins, 1995) for an epistemic, cognitive practice. In Hutchins' original comparative ethnographic account of way-finding at sea, *Cognition in the Wild* (1995), cognition is described as a process occurring across the boundary of body and as including infrastructural resource available to the body. Hutchins's field of Cognitive Science relies upon the reductionist assumption that learning may be respecified as the integration of simpler tasks. A variant on this theme within Artificial Intelligence is to formulate these tasks in terms of computational processes. An extremely simplistic outline of Hutchins' argument is the following. Suppose that cognition were reducible to a computational structure of simpler computations. An investigation of cognition in actual settings where people "compute" something important to them (navigating competently at sea and thereby surviving) shows that this computational structure would be distributed among people, artifacts and the universe at large. So there is something fundamentally inaccurate with the notion that cognition is adequately situated as principally "inside the skin." Some (Polynesians) find their way by watching the stars, counting days and observing fine distinctions in the color and movement of water waves. Others (in the American Navy) quickly trace lines over maps as they assimilate reports about land positions.

In either cultural setting, cognition operates at the interface of the body and artifacts in the surround. What do bodies do especially well? They speak and gesture as a primary means of coordinating actions with other bodies. They integrate sight with movement to discern positions of things so that they can grasp, avoid, or approach them. People fabricate instruments at a collective level, even as they come to integrate embodied modalities to them. Habits of action and ways of engaging with artifacts become internalized so as to make actions more efficient and automatic.

It should be noted that Hutchins does not dwell upon the structure of internalized processes themselves, though he does make plausible arguments about some aspects of them. There are few presumptions made as to the form of these internalized structures. The manifest routine adaptation of people to everyday tasks suggests that these structures change considerably in some fashion. Those who do dwell on such internalized structures have increasingly come to describe cognition in commensurate terms. Such is case with increasing research and theory into Embodied Cognition or Grounded Cognition (Barsalou, Simmons, Barbey & Wilson, 2003) and as summarized by Barsalou (2008). Accumulating evidence indicates that cognition (1) is constituted in part by simulated reenactments of experience, including introspective ones, (2) is situated so as to couple perception with goal-oriented action, (3) includes the capability of describing whole systems of actions by means of language and (4) is amenable to introspective imaginings of activity that simulate language use internally. This latter capability arises in part upon symbol use and manipulation as mental processes that emerge from modal systems of the body. Barsalou cites evidence that putatively amodal systems in the brain (these would be processing centers that compute offline from inputs from the rest of the body or environment) in fact remain on line throughout, taking input from, for example, simulated reenactments of experience. In other words, just as cognition at sea is distributed between body and environments, cognition "inside the skin" is also distributed across structural boundaries. The emerging picture from such work confirms that it may be generally helpful to depict cognition as a process unfolding at the interface of structures.

Anthropologists too have argued that minds and cultural objects come into a kind of coordination across the boundaries of bodies and the world of artifacts or across the political boundaries between communities. People's ways of categorizing and characterizing biological kinds in the environment, develops in coordination with how they interact with, manage or exploit these kinds (Atran, Medin & Noss, 2005). These authors and others see cultural stability as at least partly a consequence of cognitive representations shared, distributed and transmitted within populations. Others argue as well that our shared human "cognitive architecture" of mental modules explains the ubiquity of certain artifactual forms across cultures. Examples include religious belief and the use of masks or portraits (Sperber & Hirshfeld, 2004; Note that Barsalou would insist these so-called modules depend upon, and integrate with, embodied modalities). In both these examples, the world at large is brought into coordination with internalized structures that develop at ontogenetic, sociogenetic and even phylogenic scales of time (Cole, 1996). In a more recent essay, Hutchins respecifies Distributed Cognition as a phenomenon applicable to systems "inside the skin" or beyond it in terms similar to those of Grounded Cognition or Cognitive Anthropology.

Hutchins argues (in *Cognition in the Wild*, 1995) that the progressive internalization of more efficient task-oriented coordinations with the environment becomes an important dimension of our understanding of ontogenesis. After all, many people can trace lines or locate a symbol on a map, but only trained crewmen could trace a workable intersection of lines according to the exacting space and time constraints for locating a rapidly moving vessel at sea. Some people could sustain the exacting physical labor of rowing in the open sea, some could align positions of stars while bobbing in the water and some could even discern subtleties in the colors of waves, but only sea-faring Polynesians could integrate these embodied actions well enough to survive. It takes time for individuals to integrate the various modalities of the body in such a way that the required action unfolds efficiently enough. Incidentally, it also requires time and an infrastructure of learning and development for an organization to cultivate new practitioners so as to sustain itself across generations.

Hutchins is specific about how to construe learning from this distributed perspective on cognition. "The proper unit of analysis for talking about cognitive change includes the socio-material environment of thinking. *Learning is adaptive reorganization in a complex system*" (p. 289, italics in the original). This notion of learning as adaptive reorganization makes sense as long as the salient complex system appears to be fairly stable, as long as it remains recognizable as roughly the same complex. To illustrate by way of counterexample, Hutchins' definition would have been problematic for exploring learning over the decades following his study. This is because the complex system to which a learner comes to adapt has been subject to fundamental and radical transformation. The fundamental technologies of location have mostly migrated to GPS navigational techniques. Presumably, the shift to this technology entailed a dramatic shift in the complex, distributed, cognitive system. For different reasons, the Polynesian cognitive practice of way finding has also mostly been lost.

This investigation presents three primary professional settings for epistemic, cognitive practice. I will argue that in two of them there is an identifiable complex system to which the parties adapt. These parties amend this complex system as they interact with it but not to a degree that it changes in any fundamental way. Thus, learning as adaptation to complex systems provides leverage into the epistemic, cognitive practice in these settings. However, in the third professional setting the parties are engaged in a concerted effort to build the very complex system to which they might adapt in the future. I argue that

Hutchins' notion of learning has less leverage in helping us to understand that practice.

In this investigation, the focus is on the production of knowledge within demonstrations. With the possible exception of one of the professional settings just mentioned, my view is that we can make some headway into epistemic, cognitive practices by at least provisionally taking demonstrations as complex systems comparable to the work of locating and projecting movements of a vessel at sea. Demonstrations constitute a cultural form of participation in communication for which members have been conditioned. There is a speaker who commands the floor. There is a visual scene where some action occurs and to which all parties have an unobstructed view. The audience has visual access to the scene, but only the speaker has control of it. The representational forms depicted in this investigation have their own historically developed means of structuring and systematizing. Within the contours of such a complex system, the question for learning may be respecified as a question of mutual adaptation of bodies and infrastructure.

This description of embodied cognition reflects what Stevens (2002) classifies as a "conceptualist" standpoint. "[I]ndividual human beings have recurrent shared physical experiences and common biologically given bodies and thereby develop common internal concepts and conceptual systems based on these experiences" (p. 338). It should be emphasized that shared conceptual systems require mechanisms for making experiences stable across people. These mechanisms are themselves infrastructural. In a distributed conceptual system, internalized structures or "internal concepts and conceptual systems" are made to be common across people. So much is at least implied in Hutchins' presentation of people in generic terms, according to their roles in the activity and not so much according to biographies. Some of Hutchins' more recent work on conceptual blends (2005) extends this conceptualist standpoint both inward in terms of mental conceptual blends (Fauconnier and Turner, 2002) "anchored" to material artifacts as well outward to expansive distributed conceptual systems whose mechanisms of stability are explicitly described in terms of formal analysis of information (2012). For example, he mentions "mechanisms of dimensionality reduction, filtering, and constraint satisfaction" and "conceptual entropy" as possible "sources of [conceptual] order" (p. 314).

Stevens describes an "interactionist" standpoint in contrast to the conceptual standpoint (2012). In interaction, the worlds people encounter vary or only become shared due to collective work in making this world as sharable. "The modalities in which the body produces meanings and actions, often in multimodal combination, are diverse and include tool use, gesture, pointing, prosody, intonation, physical orientation, gaze, and talk" (Stevens, p. 338). One distinction as I see it between these two standpoints is that whereas the conceptualist standpoint presumes that concepts (however they may be defined) are stable across people, the interactionist standpoint presumes that concepts take form and are emergent in activity. Hutchins has sometimes entertained what appears to be such an interactionist standpoint, characterizing "action as cognition" (Alac & Hutchins, 2004) and by describing multimodal embodied interactions as irreducibly complex (2006).

As Stevens mentions, few have tried to integrate connectionist and interactionist standpoints. One can read Hutchins as trying to do just that while retaining a primary commitment to the conceptualist assumptions of Cognitive Science. My personal view is that these standpoints are theoretically incompatible as long as we dichotomize these standpoints according to the strongest historical traditions from which they arise. As will be discussed in Chapter II, the interactionist standpoint has strong roots in ethnomethodology, a program of sociology whose subject of study is the local production of order in ordinary, everyday conversation. According to this view, whatever order is to be found in conceptual activity is only discoverable as a problem with which participants themselves contend. Order is itself a practically discoverable phenomenon within ordinary talk and interaction. Furthermore, concepts become instantiated within the activity in question. The connectionist standpoint has strong roots in Cognitive Science and scientific psychology. According to this tradition, order might arise from constraints other than those with which participants contend and concepts might be describable in terms independent of the activity in question. This incompatibility of standpoints requires a more pragmatic approach if there is a desire to work with both. In this investigation my primary sympathies and commitment extend to an interactionist standpoint. Nevertheless, I believe that the focus in Distributed Cognition upon the interface between embodied modalities and infrastructural artifacts is just the right one for most of the demonstrations presented here.

One attractive feature of Hutchins' account of Naval sea-faring is the attention given to the professional development of people as they move through time to encompass progressively more experience with the distributed cognitive task of finding the location and bearing of the ship. This is an account of ontogenesis from a standpoint of a rather stable infrastructure. Hutchins more or less takes the development of this infrastructure for granted, as if it, unlike the officers, required no development. This critique points to a sociogenetic dimension of body/artifact coordination that, in Latour's view (1996b), is oddly lacking in Hutchins' explicit account. In his view, Hutchins does not treat bodies and artifacts symmetrically. He does not take the distribution of cognition "all the way" (p. 60). Latour argues that just as the distribution of internalized structures enlisted for a given task depends upon the affordances of technological artifacts (reading from Hutchins), the distribution of technological artifacts depends upon the affordances of bodies (reading into but against Hutchins). For instance, the culturally-defined semiotic structures incumbent to the use of maps together with the physical placement of road signs and other markers (artifacts upon which bodies depend), evolve in tandem with the learning of language and embodied manners for manipulating and recognizing things (internalizable structures upon which the shapes of artifacts depend). In this sense Latour's critique is consistent with the more recent work in psychology and anthropology in that it echoes a sense that whatever is cognitive of the mind must evolve in concert with the greater world with which it interacts. In response to Latour, Hutchins invites us to read his work as specific to Cognitive Science and not as expansively as Latour would like (1996).

We can nevertheless follow Latour's lead in interpreting Hutchins more expansively. The demonstrations presented here are forums for the mutual viewing of a collection of objects on display, usually a graph or some other representation of quantities, for the purpose of collaborative talk. This display may take on any of several forms: paper sheets, screen of a computer monitor, projection screen, or narrative assembly. These representational forms have a history of adaptation to people. But we should be reminded that the greater world, including laboratories especially, upon which these representational forms are brought to bear, have been shaped so as be representable in these forms.

Ancient Necessity and Generality

This part of the section on the recruitment of scholarship into ANT explains the primary categories for epistemic, cognitive practice as will be used in this investigation: necessity and generality. These terms are adapted from Reviel Netz's account of early Greek mathematics, *The Shaping of Greek Deduction* (1999). The title seems simple enough. But common presumptions surrounding mathematics and Greek history may bias us from making a plain reading of it: (Ancient) Greek deduction took shape at a particular time. This shaping resulted in a practice of deducing that had not existed previously and that was specific to those Mediterranean people at that time. That is to say, formal mathematics and deduction share a common historical origin. Furthermore, Greek deductive practice was importantly different from its successor practices in the Mediterranean (Netz, 2005). Greek deduction was a cognitive practice with its own coordination of internalized structures and environment, to phrase things in terms of Distributed Cognition. It is now gone. The discursive forms employed by the Greeks were supplanted. The technological artifacts changed and so did the typical pathways toward becoming a practitioner in its successor practices. Today we perceive, talk about and index points, lines and areas with different instruments and infrastructure than did the very few Ancient Greeks who studied mathematics at the time.

Netz uses multiple, converging, lines of evidence to reconstruct the cognitive practice of ancient mathematicians. For example, he compares mathematical texts to contemporary literary sources, identifying features borrowed from Homeric poetry. The oral history of reciting poems from memory can be inferred from written texts that maintain traces of the earlier medium of communication within it, namely “formulae” (a literary term that has nothing to do with the use of the term in modern mathematics). These are stock organizational forms, oft repeated within oral histories that provide a framework for recounting long tales from memory. This gives us one avenue for understanding the shaping of this cognitive practice—mathematics borrowed from available discursive forms.

Greek mathematics texts took the repetition of formulae to an extreme not seen elsewhere. They were also unique in having an extremely limited vocabulary and in being accompanied by diagrams. Diagrams usually appeared at the end of scrolls of text. Because the scrolls could be folded, it is presumed that text and diagram were simultaneously made visible while reading. Text and diagram must be regarded together as they mutually constitute the mathematical description. For example, a text may describe the radius of some circle as a consequential object for the progressive elaboration of a proof, but provide what appears to us as “underspecified” notational guidance for locating that relevant radius on the diagram. The sense of the progression of the proof to its end, its “necessity,” can only be preserved by presuming to identify the relevant radius. Conversely, an explicitly-defined relationship in a diagram may go unmentioned in the text but must be presumed in order to provide warrant for a conclusion in the text that could only have been reached by referencing this relationship. For example, a text may provide notational guidance for finding two distinct radii of a given circle in a diagram, but never clearly state warrants for knowing that they are congruent. The reader of the text may be required to impute this sense of congruence into the text in order to follow the preservation of necessity.

Netz argues that what might look to us now as underspecifications, were in fact legitimate, adequate, specification of the relevant state of affairs. To a modern viewer, these "underspecifications" look like "mistakes" in the proof. In my own personal reading of such modern proofs, it would seem that the text is supposed to stand alone in the sense that diagrams are there to illustrate or visualize but do not constitute the essence of things to be argued. This is certainly the case with introductions to geometric proofs in high school curricula. These introductions have their own history, and their own origins having little to do with Ancient Greek practices of proving (Herbst, 2002). Whether in practice the modern proofs could make sense or whether in practice they could be performed without diagrams is another matter. Netz cautions us against imputing our own sensibilities upon Greek mathematics but to rather understand its practices on its own terms.

The textual grammar suggests a cultural practice of pointing to diagrams, and to a literal "first order discourse" where the features of diagrams (e.g., line segments or points) figured as primary, literal objects of activity. The import of the argument, its "generality," relied upon a "second-order discourse" wherein the force of the argument pertained to any similar forms constructible in similarly arbitrary manners (e.g., a proof regarding a specific rectangle drawn arbitrarily, would pertain to any rectangle one could draw). Reading into Netz's book, I take a "proof" to be a kind of demonstration and will mostly use the latter term from now on. The Greeks invented forms of performativity that have endured since. By this I mean, reading into Netz's account, that objects are staged so that as the argument plays out, new stable arrangements among objects emerge. Modern bodies perceive new aspects of objects the Greeks never encountered. The Greeks encountered aspects we never will. Some objects we now encounter are ostensibly similar, but we may regard them differently. Yet despite the profound differences, we still talk and point as we demonstrate in order to persuade others of the preservation of necessity and of the generality of some mathematical argument. We could say the Greeks invented a form of argument that has endured despite the loss of their specific cognitive practice.

It is informative to consider Livingston's interpretation on the modern "culture of proving"

(1999). His terminology and mode of analysis are very different from those of Netz, but he nevertheless underscores a distinction between practices in a manner consistent with the categories of necessity and generality.

> The proof of Figure 2 does not literally reside in the visual description of the proof: the reasoning involved in the demonstration is neither 'in' nor not 'in' the proof figure. Provers must 'find' the proof in the figure. (Livingston, 1999, p. 869)

In chapter II we will revisit this notion of finding with respect to phenomena, as it derives from Livingston's collaborations with Garfinkel. Here Livingston makes a distinction between the visual description of the proof figure (it was an arrangement of dots) and the "finding" of the proof. Livingston's visual description is comparable to Netz's first-order discourse as both are grounded in the materiality of things jointly seen by necessity to pertain. Livingston's finding of the proof is comparable to Netz's second-order discourse of things one is supposed to follow in practice. In this comparative reading, Livingston confirms that modern practices maintain an argumentative form similar to those of the Ancient Greeks. I should caution that Livingston goes on to describe modern mathematicians' metaphysical sense about the nature of their generalized objects in terms very different from the way Netz describes ancient mathematicians' metaphysical sense about their generalized objects. As discussed above, Mediterranean mathematicians regarded generalized objects as belonging to a group of objects. These objects appear within a similar material, embodied activity repeated under similar circumstances. In Livingston's view, modern mathematicians regard generalized objects as belonging to idealized groups that have nothing to do whatsoever with any particular activity.

For each level of discourse order in Netz's account, there is a kind of performance of mathematical objects we could say is typical of mathematical cognitive practices even to this day. The first order discourse (for necessity) is only intelligible as the assembling of objects that together align to make for turning a doubtful outcome into an ineluctable one. After all, the lack of obviousness in the thing drawn out to become necessary is what makes demonstrations interesting in the first place. Over time it is safe to assume that practitioners come to see many things as obvious that novices could only arrive at by means of a succession of necessity-preserving steps. There is a sense of surprise that accompanies the cognitive practice of following the preservation of necessity to its end, and a sense of innocence lost once it becomes obvious. Within this sense of surprise I say that we engage our modalities as if we were to engage with other kinds of objects whose performance we might find uncertain or ambiguous—objects we would take as more real, tangible or material.

The second order discourse (for generality) is more tacit. “The theory which explicates and validates a practice may be only partially understood by those who follow that practice” (Netz, 1999, p. 240). We might elaborate upon this sense of the tacit, as a conceptual sense that emerges and is sustained within a community of practice (Lave & Wenger, 1990). Practitioners experience the making of the demonstration as speaking both to particulars here and now on this page and in this text, but also to other pages and texts that one could make. Once demonstrations become intelligible in this way practitioners find they can converse with other mathematicians. A novice unable to follow this second order discourse would experience confusion until he or she did. Netz argues later (2004) that Medieval Mediterranean scholars shaped a new mathematics by making a second order discourse more explicit in order to integrate and extend upon the mathematics inherited from the Ancients. As mentioned, modern mathematicians might state things in different metaphysical terms than the ancient Greeks. It is possible all the while that they nevertheless promote generality in similar cognitive terms. That is, they may very well reason metaphorically about these objects performing in similar ways under similar material and embodied circumstances while nevertheless expressing a view that they are really talking about transcendent objects.

For purposes of situating the sense of the obvious with respect to contingencies in mathematical arguments, it should be noted that mathematicians in more modern times eventually directed attention to demonstrating the necessity of some of things the Greeks saw as obviously self-evident. For example, the Greeks thought their descriptions of regular solids were obviously sufficient; later mathematicians did not (Lakatos, 1976). Continuity for graphical representations of functions developed in more modern times had not been taken as problematic, but later was (Lakoff & Núñez, 2000; Núñez, 2005). In the course of such endeavors, some of these obvious things become valid only under specific, new constraints. I mention this to underscore that putatively “purely conceptual” objects may eventually become encumbered with contingencies. In this way they are no different in principle from objects taken as material.

Netz’s notion of a mathematical practice revealed by means of a “cognitive history” is consistent with an ANT “obsessive attention to the material, historical, and practical conditions necessary for the discovery of new cognitive skills,” as Latour writes in his review of Netz’s book (2008). Latour announces that Netz does for the origins of mathematical argument what Shapin and Schaffer (mentioned next) did for the origins of scientific argument. Netz expresses ambivalence about the larger project of science studies, insisting that his work not be read as the "Shapin of deduction." Despite his lack of comfort with science studies generally, his frustrations about the contemporary perception of Greek deduction can be read as supportive of Latour's greater political argument (see especially, Chapter 8 of *Pandora's Hope*, 1999). Inasmuch as Latour's greater argument is somewhat convoluted and not especially germane, I will not discuss it here except to point out that he makes the argument that Socrates, through the medium of Plato's writings, turned reason into a facsimile of geometric proof.

In Netz's view, Plato was not a mathematician and therefore could not appreciate mathematical cognitive practices. He invented a notion of reason that was not in keeping with the cognitive practice of Greek deduction. He was able to lose track of the moorings, the diagrams, the literally "drawn" proofs, that made for authentic mathematical practice at the time, yet insisted his reasoning was just as preserving of necessity and just as productive of generality. Netz sees it as flawed to project contemporary Platonic and idealist metaphysical views about mathematics backwards in time, as this diverts from appreciating the very materially oriented discourse of Ancient Mathematics.

Modern Necessity and Generality

This part of the section on the recruitment of scholarship into ANT explains the historical origins of the goal of much scientific work, to produce knowledge that is contingent only upon the regular and predictable behavior of objects. The take on reason adopted by Netz and Latour above provides insight into the dispute between Thomas Hobbes and Robert Boyle as depicted by Steven Shapin and Simon Shaffer (1985). As the authors document, Boyle invented a novel form of argument. In light of the above works described, we could say he invented a concomitant cognitive practice. In this cognitive practice, knowledge outcomes were positioned so as to be contingent upon how objects performed in demonstrations. For example, a feature of air that would otherwise be elusive, its "spring," became contingent upon the performance of a visible level of fluid in a glass tube.

As with any cognitive practice, the infrastructure and modifications of apparatus developed in tandem with modalities of bodies. Witnesses who could see a level of the fluid within an enclosure evacuated by an air pump could testify to its descending, but only once they had been instructed as to how to recognize events, once a sufficiently large glass enclosure was fabricated, and once the means for sealing the air-pump had become reliable. All the while, Boyle exploited available infrastructural resources to complete his argumentative form. As with the Greeks who borrowed storytellers' formulae to help establish the mathematical form of argument, Boyle borrowed from judicial practices of achieving assent in order to help establish the scientific argumentative form.

By a series of arrangements of apparatus, pump operators, and the accumulating accounts of witnesses, a composite description of the spring of air arose. We may see Boyle's form of argument in terms Netz makes about Greek deduction. First, Boyle constructed arguments that preserved something like necessity in that a collection of related recordings of witnessed events could be arranged and ordered so as to make the spring of air the only plausible outcome among other possibilities. Second, air in other places would also be expected to exhibit spring because of the generality of the procedure Boyle explicated. The force of this argument was tenuous because air pumps were difficult and expensive to fabricate. The force of generality only became more profound as other air pumps came on line years later. In this investigation, I entertain the premise that though this particular cognitive practice has changed, a few aspects of scientific argumentative form, as articulated by Boyle, have endured. These aspects are (1) the positioning of objects as providing the ultimate warrant for knowledge claims and (2) the staging of the performance of laboratory objects as speaking to the nature of other objects or entities not manifestly identifiable. For example, Boyle (1) positioned movements of levels of columns of fluid as warrants for claims and (2) made these laboratory objects speak to the nature of a quantity that is literally invisible and whose properties of spring and weight might otherwise be elusive and even fantastical. How could something unseen and as ephemeral as air perpetually push with crushing force? Boyle developed a form of argument that has convinced us of an answer even to this day.

In comparing this scientific form of argument with the mathematical form as described by Netz, we observe a trade-off. We lose the simplicity and elegance of an autonomous world, because real objects come inevitably with greater contingencies—necessity here is less-convincingly preserved. On the other hand, we gain more expansive collectives, including objects taken as more real than mathematical objects—generality here more expansively encompasses objects completely unlike the performing ones.

For example, the necessity-preserving steps of this argumentative form positions witnesses, glass enclosures, valves and levels of fluid to speak to the actions of the spring of the air, which is a completely different kind of object. The spring of the air is meant to be general. It is not only in the here and now, but anywhere and anytime. The preservation of necessity in the argumentative form is always tenuous and contestable at multiple points, as Hobbes showed. The force of the generality of the argumentative form might become overpowering, as Hobbes feared.

Hobbes objected in principle to a form of argument dependent upon contingencies. He essentially insisted (putting things in Netz's terms) that Boyle's argumentative form did not make for the preservation of necessity as tight as, nor for the production of generality as expansive, as Hobbes' own faculties of reason. But as we have seen, reason as the term had come to be used by the descendents of Plato, is likewise insufficiently grounded to preserve necessity or to produce generality as well as Greek deduction. We can identify Boyle historically as the originator of this form of argument, not because he was the first to experiment, but because Hobbes forced him to make so much of the infrastructure of this incipient argumentative form more explicit. In answer to Hobbes, Boyle had to show that (1) Hobbes' arguments were also dependent upon contingencies, and that (2) Boyle's contingencies could be delimited whereas Hobbes' could not. Whatever doubts we may have about the contingencies of the air pump, we can at least bracket them. We can openly discuss how varied they may be or how uncertain we may be about them.

In Latour's reading, by making the air pump's contingencies visible, Boyle gave voice to objects that since the time of Plato had been mostly silenced.

> Here in Boyle's text we witness the intervention of a new actor recognized by the new Constitution: inert bodies, incapable of will and bias but capable of showing, signing, writing, and scribbling on laboratory instruments before trustworthy witnesses. These nonhumans, lacking souls but endowed with meaning, are even more reliable than ordinary mortals, to whom will is attributed but who lack the capacity to indicate phenomena in a reliable way. (Latour, 1993, p. 23)

In the above Latour reads an interpretation into Shapin and Schaffer's account that they are unwilling to make. As in the case of Hutchins' unwillingness to "go all the way," he criticizes these authors for failing the test of symmetry: They do not take the actors of nature Boyle recruits as seriously as they take the actors of power Hobbes recruits (Latour, 1993, pp. 24-25).

Indeed, they retreat from an acknowledgement of Boyle's epistemology, grounded in the reliability of meaning-endowed actors of the laboratory and towards Hobbes' epistemology, grounded in unseen social forces. For them, “all questions of epistemology are questions of social order” (Latour, 1993, p. 25), which order for them precludes nonhumans. They actually conclude their book with the parting claim that, “Knowledge, as much as the state, is the product of human actions. Hobbes was right” (Shapin and Schaffer, 1985, p. 344, and as quoted by Latour, 1993, p. 26). Here, following Latour, I read against this last turn by the authors. Their scholarship more plausibly supports a view that questions of epistemology are questions of a social order only as long as objects are included in it. Knowledge is a product of the collaboration of human actions and object actions.

Object-Relations as the Cause

This part of the section on the recruitment of scholarship into ANT explains the present state of the sub-discipline of ANT and how its attention to object-agency informs this investigation. ANT is the progeny of the sociological subdiscipline of Science Studies, a field that originally grew out of a concern for how people produce knowledge in the modern settings we have come to call scientific. Scholars in Science Studies in the 1970's though the 1990's inevitably tackled questions about the philosophy of knowledge, or epistemology, in order to explain themselves or to extend the ramifications of their novel approach. ANT certainly does not represent a consensus answer to those questions, but its focus on these foundational kinds of question has shaped it into something of a cross between sociology and philosophy. For Latour (1999) the challenge is to tackle epistemic, psychological, moral and political questions “all at once.” Because ANT concern itself with the methods of others, it cannot escape the reflexive problem as to how to describe its own. To pursue this reflexive program, ANT scholars have found it necessary to inspect the foundations of sociological theory. This foundational inspection can be seen as having its own genesis in the ethnomethodological tradition begun by Garfinkel (1967) and continued by Lynch (1993). Though Latour’s earliest ethnographic work was decidedly materialist and semiotic (1979), he eventually portrays his theories as an extension of ethnomethodology (1996b).

There are several intellectual traditions from which to read Latour. Because this is an investigation of talk and interaction, it is most relevant to read him from the tradition of ethnomethodology (EM, Garfinkel, 1967; Lynch, 1993). From this tradition he assimilates the principle that, in ordinary activity, people necessarily, collectively and interactionally enact versions of a sociology in order for anything they say or do to become intelligible to one another. But he reads into EM and against it by adding that these enacted sociologies necessarily imbue objects with agency (1996b) even in, or especially in, scientific enterprises (see especially, 1993, 1999, 2005). Though people are very good at making themselves accountable in interaction, they may not be very good at describing the various resources they recruit to make this intelligibility happen. This may be especially true regarding the performance of objects.

It might be helpful to focus a bit on the particular importance Latour places on the idea of agency generally, independently of the particular concerns here for integrating agency and epistemology. One way to enter into the ANT perspective on agency and artifacts is to review, *On Interobjectivity*, Latour's essay on simian "society" (1996b). What follows is my own paraphrase, put as concisely as possible after a close reading.

> Opposing ethnomethodologists and sociologists have missed an important point that renders their debate about the advantages of a "micro" account of sociology and a "macro" account moot. Both have misunderstood the role of objects in activity. Simian society provides a convenient forum for observing what both have misunderstood, precisely because technological objects are absent. Simians life constitutes (1) an "ethnomethodological paradise" of interacting intelligent beings who incessantly rebuild any society-wide relationships and (2) a refutation of the sociological assumption that the coordination of interactions requires contributions from prior social structures. By living together, simians confirm the premises of ethnomethodology but refute the premises of sociology. We should therefore retain ethnomethodology as a starting point for investigations of human activity and discard the rest of sociology.
>
> But ethnomethodology is only a starting point and will not get us as far in our quest to understand human activity without a major revision: we must include objects in our analyses of interactions. Doing so requires us to situate interactions within multiple locations and times. Putting our human experience to bear, we might say that the absence of objects in simian life (1) prevents simians from building enduring society-spanning relationships and (2) forces sociologists to abandon social structure as a prerequisite for living together, as simians manage to live together despite having to incessantly rebuild social relations. Conversely, the presence of objects in human life (1) allows us to build society-spanning relationships between humans and humans, between humans and objects and even between objects and objects, and (2) accounts for the illusion of social structure—some objects are mediating contributors to activity the effect of whose participation, when ignored, seems to emanate from inscrutable causes requiring structural explanations. (My own paraphrase of Latour, 1996b)

I have a few other comments to make in addition to the paraphrased summary above. Latour based this essay on collaborations with the primatologist Shirley Strum. For them, other primatologists tend to make a mistake while characterizing dominance among simians. Other primatologists propose kinship relations or other social structures to explain why some simians differ and others arrogate. But as Latour and Strum argue, simians do not reference a power structure when interacting (Strum & Latour, 1987). Instead, dominance is enacted within the interaction, as embodied actions such as vocalizations, gestures and movements of the self. Whatever we see of power is continuously remade in the daily dealings among beings, which is to say that the category of power does not serve as an explanatory structure. Strum and Latour sought to reverse the prevailing view of cause and effect within the sociology of simians. Power is among the things that the ordinary activity of simians explains, not the thing that explains ordinary activity. Latour wants to extend this reversal of cause and effect within the sociology of primates generally. But to realize this reversal we have to come to terms with the agency of objects.

On Interobjectivity is probably Latour's most cogent explanation for how he situates his own work with respect to ethnomethodology and sociology. For Latour the relationship with ethnomethodology is critical, but sympathetic and foundational. With sociology, the relationship is critical and caustic. But this is not the theme that most bears on this investigation. I am interested in the activity of researchers and their surroundings. How do internalized structures come into coordination with infrastructure? As we shall observe, researchers sometimes index objects while other researchers express confusion—they fail to follow or they express disagreement—they follow a different route as it were. In Garfinkel's terms (1967) this is a situation where the discursive feature of the ambiguity of indexicality becomes visible to participants. This ambiguity may be at times be productive and even "rational," inasmuch as a more specific kind of indexing might engender its own class of misunderstandings. Conversely, the same ambiguity might result in confusion. The members' task, if they elect to take it up, is to orient themselves on repairing this disconnect between the speaker's intent and the listener's understanding or to reconcile their divergent understandings. How do they do this? By invoking other objects to organize those in question or dispute. These other objects are often the representational forms that are featured prominently in this investigation.

But we have to be alert so as not to reverse cause and effect, as some primatologist-sociologists have done. Representational forms do not by themselves tell us who stands on the side of truth or who stands on the side of falsehood. They do not by themselves determine how two opinions might be reconciled. They do not stand alone as an explanation for how people conduct themselves in cognitive practice. In Latour's terms these objects connect members engaged in local cognitive practice to cognitive practice more globally construed with respect to such objects as textbooks, journals, professional development, or the allocation of credit. If members do not concur, they might engage collaboratively in a process of first reconciling how to reconfigure the various objects related to their dispute. A representational form may help them do so by providing a forum of organizing manners of talk, gesture and body positions with objects shared more globally within the discipline. In other words, representational forms mediate the coordination of internalized structures and infrastructure. Admittedly, this description invites some critique. The representational form is itself a piece of infrastructure. I will even argue in one of the chapters that a representational form is a critical piece of infrastructure to be modified and with which to be engaged. There is a pragmatic way to deal with this problem of confounding: Make a distinction between stable representational forms whose use in practice is not contested or problematic versus unstable forms whose use is contested or problematic. In either case, the representational form is infrastructural, but in the former case it more seamlessly mediates the coordination of internalized structures with infrastructure. Here we restate the same distinction made in our discussion of Distributed Cognition between stable complex system and unstable ones, but in terms of ANT.

Between science studies generally and ANT there is a schism that bears mentioning because the dispute can be traced to misunderstandings of the ANT conception of object-agency. This dispute, ostensibly about reflexivity but really about object-agency, is now two decades old. It can be traced to an exchange between H. M. Collins and Steven Yearley on the one hand and Bruno Latour and Michel Callon on the other (Collins & Yearley, 1992a; 1992b; Latour & Callon, 1992). Collins and Yearley (1992a) accused Latour and Callon of playing a sophomoric game of "Epistemological Chicken." They describe chicken as a game whose goal is to be the last one to cross a road as a speeding car approaches. Being more reflexive than others about one's analysis is akin to being the last one to cross the road, a pointless maneuver that shows nothing substantial about the enterprises of road crossing or sociological analysis. They argue that sociologists can leave what scientists know about the natural world to the scientists and concentrate on what they as sociologists can add to accounts of scientific activity from a sociological perspective. Reflexivity for them is an endless game of regress. One can begin to write a reflexively oriented account of activity only to discover that one cannot justify even this account without reflecting upon one's own activity of reflecting. They characterize the way Callon and Latour depict the agency of objects as vacuous. They accuse Callon and Latour on the one hand of deferring to the scientists in the sense that they abdicate taking a privileged sociological position for interpreting activity. On the other hand, they accuse them of arrogating the place legitimately reserved for scientists to talk about natural things.

Callon and Latour respond (1992) that Collins and Yearly take the most important categories to be explained, the formation of nature and society, for granted. They characterize their counterparts as working as if all analysis operates along a one-dimensional pole with nature at one extreme and society at the other. For their counterparts, scientists have the requisite skills to orient themselves along the polarity towards nature and the unique authority to speak from this pole. Conversely, sociologists have the most developed skills to orient themselves along the polarity of society and the unique authority to speak from that pole. Callon and Latour problematize this one-dimensional division of labor as they introduce an orthogonal dimension wherein the degree of settlement of a scientific idea varies. They argue that when ideas are not settled there is no scientific authority to which the sociologists might appeal for an explanation. Conversely, when the very terms of organization of the scientific enterprise are in dispute, there is no settled society that the scientist could appeal to the sociologist to explain.

In their reply to a reply, Collins and Yearley express a concern that newcomers to the field of science studies are becoming increasingly swayed by the cleverness of the Frenchmen's rhetoric despite the lack of substance in their writings (1996b). Their essays represent an attempt, as it were, to reveal that the emperor has no clothes. For their part, Collins and Yearly freely admit that they operate along a single pole between society and nature. They say that, following Wittgenstein, you can only ever follow along a form of life. Their form of life is constituted in part by the odd way that sociologists become attuned to shifting realities. They have a sociological view of events that is unique to their discipline. They dismiss the attempt to open up new (metaphorical) dimensions of analysis as something akin to the fascination in popular culture with taking a "Journey into Space" (1992b). They thus make a pun of the Frenchmen's illustrative depiction of a two-dimensional space. So, once again, the Frenchmen are depicted as unprofessionally childish.

My own take on this dispute is that Collins and Yearley misunderstand the ANT approach to object-agency. I think they are put off by Gallic humor. I think they resist, ignore or are ignorant of the ethnomethodological assumptions that inform Latour, at least. I think they take object agency as implausible because objects do not have intentions. The latter view reflects Pickering's (1995) take on the dispute just mentioned. But objects do not have to have intentions for their properties to be emergent, for people to take them as performing in cognitive activity (one of my main points here) or for new alliances and configurations of them to shape what society becomes (Latour's usual description of object agency, for example, 1990).

Knowledge as the Effect

This part of the section on the recruitment of scholarship into ANT explores contingency as a routine dimension of cognitive practice in science. In this investigation, I will often point out that researchers make decisions about what to do next or make knowledge claims, contingent about how objects perform. This idea of contingent performance derives from Pickering (1995). He introduces the performative idiom for the description and interpretation of scientific and mathematical activity. His work perhaps falls more squarely into social studies of science, but we can position his work within ANT if we choose from it selectively. Artifacts have agency or latitude of performance beyond the scope or purview of humans. Most compelling is his report on a team of scientists looking for evidence of fractional units of elementary charges. In terms of Pickering's performative idiom, oil drops may hover in an electric field sufficient for fractions of the elementary charge to emerge—or they may not. Making this sufficiency or insufficiency visible entails a vast array of devices and graphical forms. What comes next in the research program depends crucially upon how these oil drops perform on this stage with all manner of machines and representational forms as supporting cast. Scientific enterprises organize or reorganize according to the contingencies of the performing objects. In other words, these material objects have agency, material agency.

An alternative to the performative idiom is what might be called a retrospective idiom, one that continually evaluates or explains scientific work in terms of a rationality achieved literally after the fact. The retrospective idiom, as I am calling it, is captured in the practice of providing accounts of "ready-made-science" and the performative idiom is captured more or less in accounts of "science-in-the-making" (Latour, 1987). The retrospective idiom dominates most discussions about science, its agreed-upon truths, settlements and findings.

The retrospective idiom not only reverses the temporality of events, it also obscures the agency of artifacts, because it only considers artifacts under circumstances wherein their performative possibilities are severely constrained by arrangements with other artifacts that have since become stable over time. We are left with a residual state of affairs where only people act. Artifacts do nothing more than intermediate—they transfer the intentions and actions of others but are never mediators in their own right (Latour, 2005). As with most sociological or psychological investigations, those pertaining to learning and development have been hobbled by this idiom. This idiom does not prepare us to see that people do not just learn about states of affairs, about how settled artifacts are arranged, they also learn common strategies and means for arranging them. The classic way to deal with this aspect of work is to characterize these means in terms of a scientific method, or in terms of methods for proving in mathematics. What educators usually imply by either of these is a prospective protocol that will more or less eventually guarantee a favorable evaluation by critics in the future. But empirical studies suggest that methods across disciplines are highly idiosyncratic between disciplines and within disciplines over time (Knorr-Cetina, 1995). These idiosyncrasies make it difficult to sustain a sense that the development of an epistemologically sophisticated practitioner is simply or mostly a matter of learning standard methods well.

Looking more broadly at the metaphysical idea of knowledge, we can think of it in object-relational terms as the establishment of truth relations. I will call this the metaphysics of relations: a relation among things is true because these things are made to be held together (Harman, 2009). A common sense and prevailing metaphysics holds the opposite: relations are regarded as holding together because they are true. I will call this the metaphysics of truth. The metaphysics of truth can be seen as another version of the reversal of cause and effect mentioned above. As mentioned, objects reconfigure in ways that make for new orderings in society. The particular kind of ordering here has to do with truth relations. The cause of these truth relations is the various relations established among performing objects. The effect of these new relations is for knowledge to become settled upon.

People collaborate and employ several instruments, devices, technologies and infrastructures in order to make relations hold together. Self-reports of such activities by scientists in journal publications create the impression (illusion) that things hold together because they are true. Such accounts may be relevant to communities of natural scientists. The metaphysics of truth they invoke may be (or may not be) productive for organizing work within this community. But such accounts are less ideal for the community of learning scientists. This is because the Learning Sciences focus on the development of people and things, on the genesis of competence among participants and the concomitant progression of ideas, a progression in which relations among entities is always in flux. So, an account relevant to understanding the co-development of people, concepts and techno-scientific truths should remain within the metaphysics of relations.

There is an additional aspect of knowledge production that is worth mentioning. Knowledge does not have to take on a unitary form. It can be enacted in several ways that hold together in rather elaborate ways. This sense of the term, enactment, derives from the work of John Law (2004) and Annemarie Mol (2002). Latour, citing these authors, also takes it up (2005). Mol documents the multiple ways in which the disease atherosclerosis is enacted in medical practice. There are several atheroscleroses: one for the patient whose mobility is restricted by leg pain, one for the clinic, dominated by a dialogue between the clinician and patient, several more for each of the various imagining technologies, another for the operating room where surgeons must locate an artery within a mess of tissues, and yet another for the pathology lab where a cross section of an artery taken from a severed leg can be examined. These various enactments are brought into some kind of coordination within the medical field.

But Mol shows that despite this work at coordination, these enactments are often inconsistent in the local interactions she observes. Atherosclerosis enacted in clinical interviews is often not enacted consistently by an imaging technology—that is, they make differing diagnoses. Physicians must then privilege some diagnoses over others. With some irony, Mol shows that the only enactment accepted without reservation is an examination of tissues from a severed leg—a diagnosis too late to benefit the patient. Mol wishes us to understand that what is problematic for knowledge of atherosclerosis is problematic for knowledge in general. Knowledge is not out there to be pulled in to the local scene intact without concerted work to make it stable. It is only made stable by the work that we do to enact it in a particular instance. She describes her work as “empirical philosophy.” By this she means to subsume the philosophical concern for epistemology into a concern for how artifacts are assembled.

The Main Ideas Informing this Investigation

In the previous section we recruited a wide range of ideas into the cause of this investigation. The main purpose was to find support in historical, sociological and scientific scholarship for an investigation into an epistemic, cognitive practice. Put another way, the task was to show that a large body of literature suggests that cognitive practices should be epistemically rich. The main, recruited ideas are listed below. This list will then be further organized into a graphic in this section. This graphic will then be used to summarize many of the episodes of the empirical chapters of this investigation.

1. Internalized structures coordinate with infrastructure (coordination). This involves the scope of demonstration activity, which is characterized here as occurring at the interface of internalized structures of persons and the technological infrastructure with which they interact. For this notion, I draw upon Hutchins (1995) and others. As people engage with infrastructure they come into coordination with it. As they do so, they revise resources, be they internalized structures or infrastructure.

2. Objects are animated in talk and gesture (animated performance). Routine and mundane cognitive practices in these epistemic fields are rich in animistic turns of phrase and descriptions for the objects that make up the narrative worlds of researchers. This suggests at the very least that important aspects of reasoning are grounded in such metaphoric, conceptually blended terms (Lakoff and Núñez, 2000; Fauconnier & Turner, 2002). This is not to say that researchers will declare aspects of their graph as literally alive. But it is to argue that this animism is the vehicle by which internalized structures engage with infrastructure (Hall, Wieckert & Wright, 2007).

3. Decisions or conclusions are contingent upon object performance (contingent performance). Consequential decisions and conclusions—new forms of knowledge—are contingent upon how some of these performances play out (Pickering, 1995). Often these demonstrations invoke acts of performance that go well beyond figures of speech: a decision as to what to do next or as to what a representational form reveals is contingent upon the performance of objects.

4. Demonstrators strive to preserve necessity and promote generality (necessity/generality). Epistemic, cognitive practices include an argumentative form with historical roots. This argumentative form may be productively characterized in terms of what Netz calls respectively, necessity and generality. Briefly, necessity is the sense that a new result or claim must be true because of other things already understood or conceded. Generality is what gets promoted as a collective follows beyond the necessity of a

specific demonstration to other situations different from it in only incidental ways.

5. Relations are made to hold together in order to make something true (knowledge production). Perhaps the most important idea is that by means of these kinds of communications, researchers may come to collective agreement that relations of objects hold together to make things true (Latour, 2005, Harmon, 2009). Or they may fail to do so. As demonstrations unfold, participants are to varying degrees brought into the conversation and brought into following the presentation of events. As they follow the necessity of what unfolds, the generality of the demonstration to some other state of affairs becomes clear and the possibility of agreement emerges. I conjecture that two things happen as this process unfolds. First, people internalize to some degree the actions they undertake as they interact with infrastructure, which is to say they develop more sophisticated and efficient cognitive abilities. But the infrastructure develops too in the sense that the methods employed to make the demonstrations become adapted to the task, recorded and stored for future routine use and remembered within the group as the way to do things. These descriptions help to put some substance to the claim that knowledge is an outcome of relations coming into a stable form. These relations include both people and artifacts.

It might be helpful to illustrate some of these descriptions as in the graphic organizer of Figure 1 below. This graphic organizer will be included repeatedly in my analysis to come. For each part of this generic graphic organizer, the number of the main idea as listed above is included in parentheses. It shows the activity of performance and knowledge production occurring at the interface of internalized structures and infrastructure. Both animated performance and contingent performance occur so as to preserve necessity, eventually reaching a point where researchers may follow beyond the thing necessary in the moment to the general idea that is the ultimate reason the argument was advanced in the first place. Meanwhile, participants may use infrastructural or internalized resources in novel ways. As a result of this process a new state of affairs is achieved and new knowledge has been produced.

⇒ Coordination (1) ⇒

New or available infrastructure appears or is appropriated in new ways.

Animated performance (2) & contingent performance (3) ⇒ Preservation of necessity (4) ⇒ Promotion of generality (4)

Internalized structures accommodate new ways of seeing, manipulating or interacting

⇒ Coordination (1) ⇒

⇒

Revised resources (1)

Knowledge Produced (5)

Figure 1: A Generic Synoptic Graphic Organizer

The Organization of this Investigation

This chapter has begun to address the aims described at the beginning and as articulated in the form of questions. (1) *How do objects typically perform or how are they typically made to perform in cognitive, epistemic demonstrations?* And, (2) *How are people and objects recruited into the infrastructure of demonstrations so as to make things hold together as new knowledge?* The answer to the first question, in part, as made evident by ethnographic, historical and interpretive scholarship, is that there is some structure to epistemic, argumentative forms. This structure includes specific references to objects performing so as make nature accountable within the argument. For researchers to perceive these performances, they have to develop more sophisticated embodied ways of interacting with the infrastructure for perceiving, modeling or communicating. As a result of this process, a new state of affairs within the activity is achieved, which includes new coordinations of internalized structures and infrastructure, as well as new configurations of relations to make something true, that is, new knowledge. The answer to the second, in part, is that learning is mediated at the interface of bodies and infrastructure. In the chapters that follow I will progressively address these questions in greater detail.

Chapter II considers the methodological approach I adopt so as to be able to find meaningful answers to the questions posed. Because ANT is rooted historically in ethnomethodological methods and theory, I review the ethnomethodological literature for insights into the conduct of an investigation such as this one, given that few studies in the tradition of ANT have been organized at such a level of talk and interaction as is pursued here. Following a program outlined by Lynch (1993), I identify “epistopics” germane to the empirical investigations of professional settings of Chapters III, IV and V.

In Chapters III, IV and V, the themes of object recruitment, the conditioning of people at the interface of internalized structures and infrastructure, and the argumentative forms of necessity and generality are continuously explored and interrogated. These themes are all relevant to answering the first two questions of this investigation. In each chapter I thematically explore a different pair of epistopics and relate them to chapter-specific answers to the two primary questions. Chapter III explores the relation between the local production of agreement in relation to the production of knowledge. The professional setting has to do with a study of devices for measuring metabolism. Of course knowledge production is a general theme for the entire investigation. What makes the attention to knowledge production special in the setting is the more elaborate attention paid to the social organization of this knowledge as it becomes locally produced in talk and interaction.

Chapter IV explores the local production of necessity and generality at a greater level of detail than in other chapters. I have argued that necessity and generality are historically rooted relevant themes for making demonstrations in epistemic fields. Chapter IV explores these themes more elaborately as epistopics. It concerns a mathematical table and calculation for estimating the number of people infected with influenza. Chapter V is a shorter chapter. It explores the epistopics of retrospective and prospective analyses. These are common concerns in statistics.

Chapter VI considers the third question posed near the beginning of this chapter. (3) How can a better understanding of epistemic, cognitive practices in professional settings be made relevant to the development of practical, epistemic, cognitive competencies in educational settings?

CHAPTER II

METHODS FOR INVESTIGATING EPISTEMIC, COGNITIVE PRACTICES

Introduction

This chapter considers methods for the analysis of epistemic, cognitive practices. First, it considers the rationale for invoking ethnomethodological and actor network theoretical ideas for investigations such as this one. Second, it explores an ethnomethodological "program" specifically designed for studies of epistemic fields such as those of this investigation. Third, it explains how actor network theory (ANT) productively expands upon the themes of ethnomethodology so as to provide a better opportunity to understand how to undertake a project to answer the questions posed in chapter I. Fourth, it describes the particular steps that have been take to acquire the field data that is the basis of the empirical analysis discussed in the Chapters III, IV and V.

Method as Theory

This section explores methodological ideas as expressed in the literature on ethnomethodology (EM) and actor network theory (ANT). EM is appealing because of its original "indifferent" stance to scientistic methodological presumptions of standard sociology and, by extension, to its indifference to the belief that science is special. Now, in Chapter I science and mathematics were depicted as special in the sense that they employ argumentative forms with distinct historical origins. They took shape at rather definite historical settings and have remained with us since. But the embodied, distributed and material practices brought to bear upon the cognitive practice of doing scientific or mathematical argument are themselves not special. Rather, it is the organized way that these mundane practices are enacted that makes the argumentative forms special. My supposition is that there is a recognizable, distinct, argumentative form special to the sciences. This supposition may not be in keeping with the most "radical" versions of ethnomethodology, but it is in keeping with the more pragmatic approaches taken in actual published works by self-identified ethnomethodologists.

In his deconstructive analysis of the ethnomethodological cannon, Arminen (2008) argues that a radical ethnomethodological stance appears to insist that the only adequate account of an ordinary practice would be the actual endogenous lived-world experience of participating in this practice. That is, no text could stand in for this endogenous experience and so, if held to this standard, ethnomethodology would have nothing to say. The thrust of Arminen's critique of radical ethnomethodology is anticipated very early by Latour (1986) in his review of Lynch's account of "shop work" in a research laboratory (1985). As I will argue below, the more radical approach advocated by Lynch in his later methodological work, *Scientific Practice and Ordinary Action* (1993) is mitigated by his more pragmatic "outline" of a "program" of investigation of "epistopics" in that book's penultimate chapter. In other words, as Arminen points out, radical ethnomethodologists must take more pragmatic stances in practice.

In founding ethnomethodology, Garfinkel argued that sociology implicitly relies upon a formalist vision for science, one for which methods of inquiry had acquired an operational form describable in terms independent of the particular, mundane, material and interactive happenings in the scientific "shops" otherwise known as laboratories (1967, 1996). In my understanding of the notion of indifference, it has at least three implications. First, it implies that ethnomethodology does not concern itself with the complaints of sociological colleagues as it pursues its own programs of research.

The second sense of indifference stems from Garfinkel's observation that sociology's concerns for methods of inquiry can be viewed itself as an endogenous, practical activity to which members are held to be accountable and that produces its own sense of practical order. For sociologists a method is a *resource* for discovery things. For the indifferent ethnomethodologist, this same method becomes a *topic* of inquiry. For this reason, ethnomethodologists refer to this as the distinction between topic and resource.

To illustrate this in practice, Garfinkel examined a coding procedure for documenting record-keeping practices in a health clinic. Sociologists treated this coding procedure as a resource for making general claims about the organization of a health clinic (1967). But Garfinkel turned this resource into a topic. When treated as a topic it became clear that ad hoc judgments and other local judgments were necessary, and in a practical sense, required, in order to implement the coding procedure of patient records. Garfinkel showed that in order to implement the procedure, the coders were obliged to draw upon their familiarity with the everyday, routine practices of inscribing patient information in records within the clinic's files. In so doing, the coders necessarily invoked an endogenous, common sense understanding of the method of record keeping. But this common-sense understanding was itself the subject of the sociological study and the thing supposedly to be better understood by means of the coding procedure-resource. This attitude of indifference could be seen in positive terms inasmuch as it encourages the sociologist to interrogate the methods employed and perhaps to make them better. To notice that the enactment of sociological methods is a practical, ordinary activity does not necessarily undermine the utility of explanations forthcoming from implementations of it.

Third, the stance of indifference can inform a transformation of natural scientific methodological resources into potential topics as well. An opportunity arose to address such a topic when Garfinkel obtained a tape-recording of scientists' talk in an astronomical observatory at the time of the "discovery" of an optical pulsar by means of methodological resources as reported in Nature (Cocke, Disney & Taylor, 1969). In this short paper the authors "report the discovery" of an astronomical object by means of instrumentations and methodological techniques. These astronomers do not mention "methods" as such, but they do explain steps taken as a consequence of things recorded in such a manner that the text only becomes intelligible as a report of method even as it reports a finding. Now, we are permitted to take the methodological resources for granted if it is stars we care about. We may adopt a reverential stance for the method as we marvel at the exotic astronomical objects described. But Garfinkel and his students took an alternative, indifferent stance to stars and pulsars as they analyzed audiorecordings in order to investigate the means by which the astronomers did the work of discovering them (Garfinkel, Lynch & Livingston, 1981). My investigation is ethnomethodological in the sense that it likewise turns its attention to the practical means by which scientific methods are enacted. It is indifferent to the local claims of what is found or claimed to be known.

As explained in Chapter I, at least some of the origins, sympathies and theoretical commitments of ANT can be traced to the ethnomethodological work of Garfinkel and his students. Of these students, Michael Lynch is perhaps the most notable for this investigation because of his work on the ordinary activity of scientific practice. EM ideas on method can be invoked in order to help answer the questions posed in Chapter I. As Lynch argues, in order for EM to become relevant to answer such questions, some work must be done to tackle the problem posed by the various interpretations of terms. For example, what is knowledge and what is a demonstration? When these terms are used in conventional ways, they tend to act as a sociological-analytical gloss that may or may not pertain to the lifeworld of participants. The term “gloss” and is a favorite EM term. It is used in a pejorative sense, conveying a sense of bias on the part of a conventional sociological investigation, one that purports to summarize, categorize or define an activity in ways that lack grounding in the lifeworld of participants.

The literature in ethnomethodology already provides a substantial basis for the analysis of scientific talk and interaction. What then does ANT add to the analytical task of investigating the epistemic, cognitive practices of medical researchers? Unlike any ethnomethodological work of which I am aware, ANT invites us to consider a broader array of actors in activity, including both humans and nonhumans. My point can be summarized as follows. As researchers include objects in their cognitive practice, they and their objects inhabit a lifeworld in which these objects are imbued with agency. These objects exhibit agency in the sense that researchers make consequential decisions for analysis contingent upon how these objects perform. They also exhibit agency in the sense that their manners of performance constitute essential linchpins that make for the establishment and maintenance of knowledge products.

The general notion of object agency has been both controversial and daunting. No self-identified ethnomethodological or ANT investigator, as far as I am aware, has taken up the challenge of exploring agency of objects as an aspect of ordinary talk and interaction. Latour for his part tends to talk in grander terms of object agency, as is the case with the agency of the anthrax bacillus acting first as an object that appears in history and eventually insinuating itself progressively into broader networks of humans and artifacts (1983). Latour often alludes to and asserts the ubiquity of object agency at several scales of activity, including talk and interaction (2005), but has not attempted to explore object agency in these terms of talk and interaction. Lynch for his part has been generally skeptical of Latour’s writings on object agency and depicts Latour as beset with excessive and grandiose ambitions (Lynch, 1996).

Rather than attempting to reconcile or bridge the theoretical work of these two scholars, my approach here is to propose an interpretation of object agency (extending Latour’s argument as I read into his work) and of talk and interaction (extending Garfinkel’s and Lynch’s arguments as I read into them) that will help us to understand something more about the epistemic, cognitive practice of doing scientific research. I would not characterize this effort as bridging or reconciling because doing so would require a greater exposition of the sociological and philosophical literature than I am prepared to conduct. That said, it is fair to say from a basic survey of EM and ANT, that both emerged from contentious debates about methods in social science research. Protagonists for both argue against relying upon foundational assumptions about the proper conduct of science as a warrant for the proper conduct of sociology. These protagonists insist that because we choose to be indifferent to the proper conduct of science in order to study it, we must not purport to build a sociology that imitates its methods. Any relevant sense of "method" emerges as a demonstrably visible aspect of the work endogenous to the lifeworld of the participants under investigation. Both traditions are deeply critical of, and reflexive about, their own methods as a result of this general skepticism about methodology. It should be said that building a study under the rubric of ANT and EM has been somewhat daunting. Under the exacting, reflective standards of EM analysis or ANT scholarship, it becomes very difficult to produce an account of activity that is both interesting to a greater audience and deemed as an adequate sociological account by established practitioners in these fields.

I think a few short explanations will suffice to make my point. Lynch's book length treatment of ethnomethodology and scientific activity (1993) is an explicit attempt to articulate a program of action for doing ethnomethodological studies of work in laboratories or similar sites of ordinary scientific practice. It stands to this day as the most thorough account of an attempt to theoretically make ethnomethodology and social studies of science more compatible, or at least to clearly delineate the major fault lines between them. The main text is 319 pages long. Most of it consists of a survey of the sociological ancestry of ethnomethodology and of a somewhat parallel history of the development of science studies. It is not until page 299 that Lynch pronounces the following.

> At long last, I am ready to outline a "program" of investigation that combines ethnomethodology's treatment of ordinary practical actions with the sociology of scientific knowledge's interest in the "contents" of scientific practices. The investigations I have in mind will concern the primitive structures of accountability that make up the instructable reproducibility of social actions. (p. 299)

By this measure it seems that in order to conduct a study informed by EM, one might be obliged to spend 93 percent of one's effort in clearing away the theoretical brush so as to begin an inquiry about the primitive structures of accountability with the last 7 percent of the available effort. As of this publication in 1993, only four studies had passed muster as producing a "uniquely adequate" account of scientific activity, as judged by Garfinkel (Lynch, 1993, footnote, p. 302). These four studies had all been completed by

Garfinkel himself or by his students over the three previous decades since the founding of EM. My point here is not to bemoan the challenges I face in producing a uniquely adequate account of object agency. Rather, I merely wish to explain the difficulty of presenting my method as a sufficiently valid instance of ethnomethodological analysis in the same way that the researchers I depict here might present their methods as sufficiently valid cases of statistical modeling or in the way that other qualitative researchers might index a method whose parameters might be less incessantly problematized. I can only say that upon careful reading of Garfinkel and Lynch, neither of whom are easy to understand, I feel as if I have a more or less accurate sense of what kind of project they promote. I back this up by stipulating that my accounts are uniquely adequate to the extent that you the reader will understand that the account I present is a plausible rendition of the researchers' lifeworld.

As for the challenges of informing a study with ANT, it is only necessary to point out that Latour devotes one chapter in his latest programmatic text on doing interpretive research (2005, *On the Difficulty of Being an ANT: An Interlude in the Form of a Dialog*, pp. 141-158) to the impossibility of "applying" ANT in order to conduct an investigation. He depicts this problem by way of a dialog with a fictional graduate student who wanders into his office in search of advice, only to discover that ANT does not provide anything like methodological guidance, or not quite the kind of guidance one might expect based on a general experience with sociological methods. Latour's tone is ironic and flippant, an attitude that infuriates the graduate student and with whom I can deeply sympathize for obvious reasons.

Epistopics for an Ethnomethodological Program

In the latter 7 percent of his book, Lynch (1993) outlines a guide for conducting an investigation of epistemic, cognitive practices at the level of talk and interaction. Except for perhaps my commitment to regard object-agency as an essential aspect of ordinary practice, his programmatic outline stands as a useful guide for this investigation. What follows is a synopsis of this program with respect to my own investigation as it pertains to meetings among medical scientists and statisticians. Lynch begins with a discussion of the topics of investigation of the sciences. In most circumstances of sociological investigations of the sciences, we are drawn to them precisely because we are interested in what purportedly at least makes these activities special, namely, their epistemic concern with knowledge and how it is produced.

> The starting point for such investigations is the "epistopics"—the discursive themes that so often come up in discussions of scientific and practical reasoning: observation, description, replication, measurement, rationality, representation, and explanation. The epistopics provide foci for classic epistemological and methodological discussions, but they are no less relevant to vernacular inquiries. (p. 299)

As Lynch explains, these epistopics as discussed in the general philosophical or sociological literature, have an uncertain or ambiguous relation with the ordinary practices as enacted by researchers. Latour, for example, in some of his earliest work, documented the central role of qualifiers of statements of fact in the work of isolating a hormone in an endocrinology laboratory (Latour & Woolgar, 1986). These qualifiers attenuate statements of fact. The more qualifiers, the less a given statement is to be taken as a fact. An unqualified statement constitutes a measure of knowledge after the various laboratory specimens have been discarded, the machines have been turned off and the technicians sent home.

As Lynch explains in his own critique (1993) this account is interesting but it is not "uniquely adequate" in the sense that it is not so well grounded in the lifeworld of the researchers and in the sense that we do not experience as readers the practice of making statements progressively less qualified. After all, Latour adopted the stance of a stranger, or outsider, in order to conduct his investigation. Such as stance is decidedly not in the ethnographic mode. A uniquely adequate vernacular account of the progressive attenuation of statement-qualifiers is not made to be visible in his account. We do not get a sense of this phenomenon as an accountable aspect of practice among researchers in their daily ordinary work. My guess is that a later Latour, the version of Latour espousing a "theory" of an actor network, would concede as much. So I would like to underscore that this example should not be seen as to imply that EM and ANT are incompatible. As explained in Chapter I, ANT can be seen as an extension of EM, rather than as a competitor or as coming from a distinctly alternative viewpoint.

Lynch lists at least seven points in this programmatic outline. I will discuss each in successive paragraphs by first quoting selectively and by then elaborating upon the ways that this investigation realizes or effectuates each point. The first point concerns the pragmatic choice of epistopics, linking theoretical concerns of the academic literature (here, that of the Learning Sciences) with the everyday, ordinary lived experience of participants.

> Begin by taking up one or more of the epistopics. The epistopics have a prominent place in the large literatures in the history, philosophy, and sociology of science, but in this case our aim will be to break out of the academic literature by searching for what Garfinkel has called "perspicuous settings": familiar language games in which one or another epistopic has a prominent vernacular role. (p. 300)

Two primary epistopical terms germane to the entirety of this investigation are representations and demonstrations. As discussed in Chapter I, demonstrations have a long history. Arguably, they are foundational to what makes for a natural science. I argue throughout this investigation that researchers routinely invoke representations in order to demonstrate things. The appearance of representations is obvious from any cursory analysis of these meetings. But why do people display so many of these representations? Once we begin to look carefully at these representational practices we will observe that they often exhibit some of the epistemic, historical, argumentative structure of science and mathematics demonstrations as discussed in Chapter 1.

Another pair of epistopics is agreement and knowledge production, a major focus of Chapter III. Obviously, any epistopic would involve the idea of knowledge in some sense, but with the epistopic of knowledge production, the focus is upon the metaphysical idea of what makes for knowledge. If it is a product, what do researchers take it to be made of? The related epistopics of necessity and generality as discussed in Chapter I will be the focus of Chapter IV. Finally, Chapter V focuses on two epistopics of great interest to statistical science: retrospective and prospective analyses.

The second point in his programmatic outline concerns the criteria one might use to choose episodes of talk and interaction that will elucidate the point one is trying to make via the voices and actions of participants. He suggests to, "Search for primitive examples" (p. 300). By this he explains in part that we need not become experts in the scientific domain of the participants of our studies. We should expect that ordinary activity would be sufficiently recognizable for determining perspicuous settings, regardless of our level of expertise. That said, the more one understands the practice as a practitioner the better, as Lynch points out repeatedly elsewhere. Sitting in upon and enrolling in various biostatistics courses have augmented my own expertise in statistical practice. One of my statistician-participants was the instructor in one of those courses. This not only brought me into greater familiarity with the personnel of the Biostatistics Department but it also helped to familiarize me with some ways of talking, use of software tools and with more of the mathematics of statistical inference. Lynch's second point only makes sense if we assume that ordinary action provides opportunities for illustrative examples of episodes of talk. Implicit perhaps to this suggestion is the advantage of choosing examples that are not overly esoteric. Many of the episodes for this study are not primitive in a technical sense. They require a fair degree of background explanation in order for them to become intelligible to the reader, that is, to make them less esoteric.

His third point borrows from Wittgenstein. It draws upon the notion that any correspondence between participants' utterances, statements or embodied indexings and any objects referenced occurs within the situated practice itself. "Follow the epistopics around and investigate actual cases in detail. An ethnomethodological transformation of Wittgenstein's approach would be to search for "naturally occurring" primitive language games and to investigate them in detail." This point reminds us that our understanding of this epistopic will be mediated and perhaps transformed by the trouble we take to investigate it in practical action. He argues that the cumulative effects of several such investigations of an epistopic, as in this investigation's several depictions of demonstrating over representations, would broaden our understanding of what it means to demonstrate and to represent. The suggestion then for this investigation would be to decipher or elaborate upon the recurring implicit or tacit game of demonstrating over representations.

The fourth point is an elaboration upon Garfinkel's "unique adequacy requirement." As Lynch explains, "the requirement has to do with a method for demonstrating what a description says about a practice by enabling readers to see what is said by entering into the phenomenal field of that practice" (p. 302). In this investigation this point is met by providing some details so that the reader can interpret the sense of the reasoning or representational dilemmas or insights that the researchers encounter. This does require that the reader invest some time learning about some of the specific practices of the researchers' scientific work. Each chapter includes some discussion of what I call infrastructure and cognitive infrastructure, commonly exploited artifacts and ideas. But it also includes sufficient reproductions of critical representations (photographed images or facsimiles) so that the reader might reenact some of the key reasoning challenges encountered by the participants.

The fifth point is to relax the sometimes-tacit assumption that the conduct of science and mathematics involves some special epistemic, cognitive resources, which is for the analyst to adopt the attitude of "ethnomethodological indifference" (p. 303) as to the claims among scientists that their methods are special. Such claims of uniqueness only constitute one perspective among many, only one aspect of the scientists' lifeworld. Lynch elaborates on this by turning to the ordinary action of non-scientific or scientific work.

> Without denying that scientific and mathematical practices, no less than fixing a car or preparing a dinner, require specialized training along with a disciplined use of some commonplace skills and routines, I am recommending that we not assume that these rare and specialized competencies discriminate a uniquely coherent set of methods for making true observations, constructing unquestionable proofs, and achieving discoveries. (p. 303)

My own argument about the uniqueness of science is that the cognitive resources brought to bear upon ordinary demonstrations in the collaborative meetings between scientists and statisticians are not special to science. However, as explained in Chapter I, ordinary competencies are invoked to perform language games with historical roots that are special to science or mathematics. My argument is that scientific practices tend to orient members' non-specialized competencies into specialized language games. These specialized games might in fact be shown to constitute something like a "uniquely coherent set of methods." But such a thesis would have to be argued through the detailed inspection of primitive examples and should not be presumed to pertain before such an analysis begins.

The sixth point is to apply the same sense of ethnomethodological indifference "to the special epistemological status associated with social science methodologies" (p. 305), in keeping with Garfinkel's original interest in the attitude of indifference. Lynch here is searching for alternative grounds than would be found in traditional sociology for making something akin to generalized claims about scientific practice. In striving to do so, he elaborates upon the features of a uniquely adequate account of a practice that is itself specifically methodological.

> Since the epistopics are both thematic objects and analytic "instruments," to be uniquely adequate (in the sense outlined earlier) any analysis must be subject to a kind of "double transparency." The language games examined, for example, of "describing the appearance of an object or giving its measurements," must be transparently recognizable to readers, and that transparency must be made thematic. This radical reflexivity of accounts is not a question of "observing oneself observing" with all its regressive implications but of bringing the transparency of an action under examination by composing descriptions that enable an "adequate" reproduction of the action. In order to partake of this transparency, a description must both enable the practical reproduction of an intuitively recognizable action and provide a notational index of the transparent details of that action's performance. (p. 305)

Here is an example of how this investigation strives for such double transparency. In one of the episodes I present, a statistician computes a result for a formula using terms and values that appear within a table. The table is shown to have incompatible values in a sense that the reader is free and able to inspect or “verify” the theme of this incompatibility. The arithmetic calculation may likewise be practically reproduced. In this sense the description is an experienced phenomenon in its own right for the reader. The description becomes doubly transparent as it is shown by means of a notational index of screen shots and utterances that the practical consequences of getting the table wrong in some sense impedes the ability of the researchers to quickly work out an ostensibly simple arithmetic computation. So the readers’ thematic sense of the difficulty as well as the participants’ accountable and witnessable difficulty coincide.

The seventh and final point has to do with informing the literatures from which the epistopic might have been drawn, or perhaps to inform literatures that had not taken up this epistopic in the past.

> Relate the “findings” back to the classic literatures. The epistopics are collecting rubrics, but particular “findings” about their situated enactment are likely to hold differentiating and therapeutic implications for classic epistemological and methodological versions of observation, measurement, and the like. (pp. 305—306)

Some of the more specific differentiating and therapeutic implications I have in mind for the Learning Sciences are discussed in Chapter VI.

The lesson I take from the epistopic of demonstrating is that it exemplifies a manner of enacted practice that makes scientific knowledge more intelligible. Without a sense of demonstration as is described here, students might not “grasp” the incumbent implications of a scientific principle or fact. I argue that the epistopics of necessity and generality point to a performative sense of mathematical ideas that expands and complicates a growing literature on human agency and dispositions in mathematical learning. My point is that practitioners of mathematical demonstrations, in cognitive practice, engage as a matter of course with object actors. Doing so entails something very much like a mathematical version of a “grasp of practice” (Ford, 2008a). I also argue that the growing interest in models and modeling in education has been informed too strongly by some philosophical perspectives that take the historical development of models within a practice for granted. Contemporary models might seem to be the only sensible summary of the relevant natural phenomena, but there was a time in the past when this common sense was not common. In keeping with the general theme of grasping practice, students need a more authentic experience with the tentativeness of models and with the disquiet of “losing the phenomenon” as Garfinkel describes it (2002).

By this he means that, while in development, scientific principles do not have a durable or sustained presence in any community's lifeworld and might only tentatively or provisionally pertain to a given phenomenon.

Extending to Actor Network Theory

Chapter I discussed how Latour's ANT might enhance Hutchins' (1995) concept of Distributed Cognition. Cognition for Hutchins unfolds among artifacts on either side of the skin of the person. It is a process that integrates thoughts, mutually mediated sensory inputs and motor outputs, and historically layered artifacts. Cognition for Latour unfolds among artifacts that may or may not be coupled to a sentient, human agent. The idea of the extension of agency to nonhumans invites us to consider dimensions of cognitive processes that unfold beyond the interface of bodies and artifacts, as Hutchins has eventually conceded is necessary in order to make sense of collaborative interactions among people and things (Alac & Hutchins, 2004; Hutchins, 2006). In other words, artifacts other than the body mediate important aspects of cognition. Conversely, as argued in Chapter I, contemporary theories of Grounded Cognition (Barsalou, 2008) explore the structure of mental processes while, under the assumption or prospect that the environment, body and brain are coupled. Grounded Cognition takes up the mechanism of this grounding as a problem to be solved.

In the Cognitive Science and in the Artificial Intelligence of Hutchins's day especially, cognition was typically specified as a process that unfolded within the confines of the brain or within the confines of a computer environment. The outside world was a source of inputs as well as a target object for the execution of outputs. More sophisticated versions might incorporate parts of the outside world into a coupled system with the computational entity (brain or computer). But this incorporation only redraws the boundaries of the container. I think that Latour is correct to point out that Hutchins equivocates or "hesitates" on this question as to the boudedness of cognition.

ANT invites us to respecify cognition as a process that unfolds among actors that may invoke activity at other times and places. The spatial or temporal delimitation of bounds of cognition is only a practical or pragmatic maneuver from this perspective. These actors may or may not be reflecting, self-aware sentient beings able to monitor, guide or administer activity. In this sense, cognition should be regarded as an emergent phenomenon that includes embodied actions in which people have control over some mediating artifacts but not over others.

I think that respecifying cognition this way informs recent developments in cognitive linguistics and embodied cognition that have explored some of the structure incumbent to the use of metaphors and conceptual blends (Lakoff & Núñez, 2000; Fauconnier & Turner, 2002; Hutchins, 2005). The authors in this literature often appear to treat the metaphoric structures or conceptually blended structures they have identified, as fossilized components of cognition. They do not appear to adequately consider that metaphors might be contingent upon particular historical events and that they are subject to the possibility of modifications or revisions as new historical events occur, which events could include concerted pedagogical reforms. For instance, (Lakoff & Núñez, 2000) appear to believe that only a few available metaphors for particular mathematical operations (e.g., addition as movement to right, subtraction as movement to the left) have emerged within human consciousness over history. Furthermore, they also appear to believe that these metaphors are themselves logically organized with respect to each other, an unnecessary and dubious assumption. Without denying that their attempt to reorient mathematical cognition away from Platonic assumptions is a welcome development, I have to say that in my own experience the diversity of metaphors is far richer than they let on. These various metaphors do not necessarily integrate very well. An ethnomethodological investigation of metaphors of mathematical operations might complicate and enrich the work these authors have begun.

The attention to cognition and agency here has as its purpose to present Latour's main argument about the agency of objects in terms most germane to the problems of this investigation. Ethnomethodology appears to take it as a foundational assumption that activity is mediated uniquely by humans and that activity is necessarily a phenomenon local to the immediate surround of humans. The persistence of the assumption of humans-as-unique-agents means that even ethnomethodology retains some allegiance to the camp of "the sociology of the social" (Latour, 2005). In contrast, Latour's "sociology of associations" takes as its explicit foundational assumption that activity is mediated by any actor whose performance is emergent. Furthermore, no principled distinction between kinds of actors (humans, nonhumans, objects, infrastructure) is warranted because almost no activity occurs without some entanglement of actors traditionally categorized as sentient and others traditionally categorized as inanimate.

In this investigation I will discuss contingencies made manifest in talk among researchers. An important characteristic of scientific demonstrations, I argue, is that objects are positioned via representational artifacts to perform, by which I mean that outcomes of action are indeterminate until an action is staged and then witnessed by a collective that can then form an agreement as to is seen. This collective can then consider how this agreement informs an understanding of the relevant phenomena related to knowledge. In ANT terms, these objects are actors. That researchers learn to treat them as actors as an endogenous aspect of ordinary action is one of the primary contentions of this investigation. If this is right, then it suggests that we might construe the development of scientific or mathematics understanding in part as a progressively advanced sophistication with the staging and witnessing of object-actor performances.

Meeting to Model

The settings presented here are videotaped meetings between medical scientists and biostatisticians. From these meetings I have selected episodes of people interacting, usually around graphical user interfaces, tables or graphs, in order to demonstrate something, to show or view performances of objects (Pickering, 1995). There are other possible levels of analysis to investigate scientific activity—following journal publications and noting citations among authors, for example. But the intent here is to view activity as people interact with their surround. I show that people routinely animate objects in talk and that they suspend action as they make the next decision or claim. Any decisions or claims are contingent upon how these objects perform. I depict these events as part of a process of forging new and stable relations among objects in order to construct new assertions about states of affairs. Such assertions about states of affairs is called knowledge. I argue in the last chapter that these aspects of epistemic, cognitive practice are graspable in pedagogical settings.

The research meetings usually include two to four people involved on a project. In all the meetings presented here, there are one or two biostatisticians and one or two medical scientists present—hereafter, statisticians, scientists or researchers (either). Such meetings are routine and sometimes mandated in the medical school where they take place, usually in the statistician's office or in a conference room. Conference room meetings are semi-public affairs where others can stop by and observe. By design, these meetings (of either kind) were selected as a venue that would likely elicit conversation and interaction on modeling and the production of knowledge. This is because the participants convene precisely because doing so provides a forum for explaining understandings so that research programs may move forward.

In all of the settings for this investigation, scientists are trying to publish knowledge claims in journal publications. This forum merges the collective interest to broadcast knowledge claims with the personal interests of researchers to garner credit. We might expect that knowledge-producing enterprises such as these self-organize in varying ways but that they nevertheless achieve configurations wherein the needs and interests of the members come into a kind of balance with the making of durable knowledge claims. One can read Knorr-Cetina's comparative ethnography (1999) of high-energy physics and molecular biology in this way.

Making knowledge claims in medical science studies that rely on statistics is an explicitly uncertain enterprise in the sense that any knowledge claims are explicitly qualified as estimates and therefore less than certain. This statistical kind of uncertainty is perhaps special because it is itself explicitly calculated, as in, for example, the publishing of estimates with accompanying confidence intervals. But as will become clear in the various case studies, new knowledge that researchers advance or attempt to advance here are often contested or problematized in many additional ways. The researchers usually work with data that is costly to assemble. This means that they feel pressure from protocols that might require them to eliminate data, or that raise the bar for making inferences (e.g., lower type 1 error thresholds). We also might expect that some in the field would reflect upon this state of affairs and attempt to shift professional standards one way or the other. This is certainly true of biostatistics. Without going into depth here, I will simply point out that several critics within biostatistics identify a lack of durability of knowledge claims as a problem and suggest ways of remedying the situation (e.g., Sterne & Smith, 2001).

The focus of this investigation is at the level of cognitive practice as people work face to face. Making such epistemic, cognitive practices visible in a form amenable to the Learning Sciences has always been a challenge. This is partly because the technics are typically difficult to grasp. It is also difficult to obtain access to scientists doing work together on projects, work with which Learning Sciences researchers or practitioners might make comparisons. But there is another problem I think is more acute. Even when such access is granted, the categories ethnographers have chosen may not line up well with categories the Learning Sciences might find most relevant. The tactic employed here is to make the analysis of primary epistopics described above "differentiating and therapeutic" to the Learning Sciences.

After any given meeting was videotaped, we made a transcription of talk. For two of the meetings discussed in this investigation, the meetings were discussed and analyzed within a group consisting of myself, Rogers Hall and Karen Wieckert. The meeting featured in Chapter V was analyzed mostly by myself, though Rogers Hall has independently reviewed the meeting and has discussed portions of it with me. Our analysis usually consisted of some work to understand the technics of the meeting. We wanted to have at least a rough appreciation of the topics discussed as understood by the participants. Next, two to three short episodes were selected for use in follow-up interviews with the primary participants. During these follow-up interviews, we played a video playback of the episode and then asked a series of questions about it. We also invited participants to volunteer any commentary. These interviews were then transcribed and read through so as to inform our interpretation of the original meetings.

CHAPTER III

AGREEMENT AND KNOWLEDGE-PRODUCTION

Introduction

The primary questions to which we seek answers in each of the empirical settings, depicted sequentially beginning in this chapter, are as follows. (1) *How do objects typically perform or how are they typically made to perform in cognitive, epistemic demonstrations?* And, (2) *How are people and objects recruited into the infrastructure of demonstrations so as to make things hold together as new knowledge?* Any answers to these questions will be tied contextually to the specific setting under investigation. In other words, the answers to these questions will be somewhat idiosyncratic. The analysis of each professional setting in each of the next three chapters will be broken up into episodes. At the end of each episode you find a graphic organizer that summarizes the mutual coordination of internalized structures with infrastructure as objects are recruited to perform so as to form into new relations that speak to novel states of affairs. That is, objects are recruited to produce knowledge.

As explained in the preceding chapter, the special "epistopics" for this chapter are agreement and knowledge production. The additional epistopics of representations and demonstrations will also be explored in this chapter, as they will be explored in all three of the empirical chapters. Because the topics of necessity and generality figure into the argumentative form that makes for the production of knowledge, they also figure into this discussion, though they will be explored more elaborately in the next chapter. One may also read this chapter in terms of the epistopics of prospective and retrospective analyses, though this comparison of epistopics will not be explored in depth until Chapter V.

The demonstrations depicted here are especially informative of the contingencies of knowledge in the sense that we are left to wonder as we come to the end, whom to believe, the collective of the assembled for this meeting, the collective of the three main collaborators, or just the lead statistician? The achieved agreement among the various collectives will be tracked here in local talk and interaction. The term, "achieved agreement," derives from Michael Lynch's investigation of scientists studying brain physiology (1985). The term evokes the local, collaborative and interactive work to reach consensus as evident within talk. In this meeting we observe the relations that together make for an achieved, agreed-upon knowledge claim even as we conversely observe relations that together make for an alternative, contradictory, achieved, agreed-upon knowledge claim. We also observe the researchers carrying on without any particular urgency to resolve the apparent discrepancy between the incompatible states of affairs. Because both achieved agreements stand on their own as local accomplishments of what participants take to be logically based argumentation, the contrast between them stands as an instructive lesson on the relativity of knowledge with respect to the relations among objects and people that comprise it.

As for learning, we will follow how the interactions with representational forms appear to induce participants to perceive new aspects of them, and new realities of the laboratory nearby. By doing so we will track learning in the sense of adaptation to a complex system (Hutchins, 1995) as discussed in the introductory chapter. But because knowledge is contested across groups and because the constitution of what gets taken to be knowledge is so clearly tied up with the social organization of people, it will be informative to also consider learning from a perspective of legitimate peripheral participation (Lave & Wenger, 1991). We can track those aspects of epistemic, cognitive practice that become visible to the assembled. We can at least document the possibility of these aspects being taken up by these scientists in the future.

In this chapter I first describe the available infrastructure for assessing "agreement" or "disagreement" of devices. Then I discuss a series of episodes within the meeting. Each episode considers performances of objects, necessity, generality and knowledge-production. The specific instantiations of activity relevant to these terms are summarized with a synoptic graphic organizer. Some of the episodes are reported at a finer grain size than others. Such episodes are accompanied by transcripts or panels showing screenshots (in Appendix A). Each episode considers the animated performances and contingent performances of objects as well as what, if anything, people come to see or are encouraged to see as necessary and general. As this process unfolds, new infrastructure becomes available for researchers even as new relations among objects are forged for constituting knowledge. After considering these episodes in sequence I will review knowledge production and learning across episodes.

Steve discusses devices for measuring metabolic activity with the assembled. Sally and Lev are collaborating researchers on this project. Mary and Brian, scientists unaffiliated with this project, speak occasionally during selected episodes.

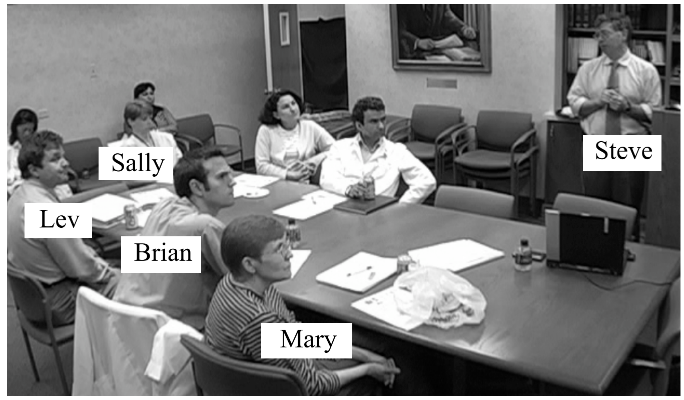


Figure 2: Researchers comparing the agreement or disagreement of devices

People and Infrastructure

The setting for this part of the investigation is a study of devices that measure energy expenditure in human metabolism. The figure above depicts the rooms, pseudonyms and locations of major participants in this presentation of this case. The researchers (a generic term that includes both statisticians and scientists) met in a room designated for attendance on a walk-in basis. The atmosphere was collegial, with drinks and lunch provided. Researchers typically brought in data acquired in their laboratories or clinics. Those assembled learned about colleagues' studies as well as about statistical analyses related to them. In attendance were eight researcher. Steve had arranged to present his analysis of data from Sally and Lev. Sally's study involved an analysis of an armband device for measuring the "energy expenditure" of children. Steve had prepared his part of the analysis in advance. In fact, it was almost ready for Sally to present at an upcoming conference. Sally was the main scientist on this project and Lev was her mentor.

Steve had been consulting for about 20 years and had been recently hired by the Biostatistics Department at the medical school where all the meetings presented in this investigation took place. He had marketed a book to medical scientists teaching them to be more successful at getting published. The department hired him partly because of the success of this book. It is fair to say that Steve had built his career on being practical about making knowledge claims so that he and the people who listen to his advice get credit that comes with real value. He consulted regularly with members of various research units at the medical school. He explained in interviews that he did not have time to perform routine data manipulation and statistical analysis with his many clients. He promoted SPSS®, statistical software built around graphical user interfaces (GUI), because it is easier for his clients to use on an occasional basis than other software.

In other meetings I have witnessed, Steve frequently instructed scientists to organize measurements acquired in the laboratory into spreadsheets (Excel®, it seems almost exclusively) with a structure amenable to transfer to the database format of statistical software. He contrasted horrible spreadsheets that made collaboration difficult as “spreadsheets from hell” and those that facilitate statistician contributions as “spreadsheets from heaven.” In the latter format, each row is dedicated to a unique subject of the study (usually a person and made anonymous with an identification number matched to a person on a list kept separate from the spreadsheet). Each column is dedicated to a different variable associated with that subject and is standardized with consistent units. Steve advised researchers to enter data directly into well-formatted spreadsheets so as to streamline work. He advised scientists not to use spreadsheets for statistical analysis but to use a program specifically designed for that purpose. Interestingly, in the later part of this meeting Steve shows that he transferred statistical results from SPSS® back into a spreadsheet for the purpose of making graphical displays for Sally’s upcoming presentation. Apparently, though SPSS® had the requisite analytic capabilities, it lacked some of the graphical capabilities that Steve liked to use in Excel®. This part of the meeting is not otherwise discussed here. However, it should be mentioned that the considerable effort and work that Steve had already invested in this project has bearing upon how willing he might be to consider alternative modeling approaches introduced in this meeting and that might require him to revise his analysis.

This experiment, like many others I have observed, was a planned study with a sample size so small that it strained the capabilities of statistical models to produce reportable patterns from them. Because it is time-consuming, resource intensive and expensive to acquire human subject health data, scientists find it valuable to get good statistical advice so as to draw the strongest inferences possible from the least data. In this meeting, Steve spoke about and displayed graphs, and tables pertaining to measures on children. Concerns about statistical significance arose several times in the meeting, particularly when Steve made modeling choices that even marginally reduced the prospective “power” of the analysis, a term for the probability that a valid population characteristic would become revealed as a significant factor in the analysis.

Two of the measures in the database, energy expenditure as determined by an armband device and energy expenditure as determined by a metabolic chamber, were subject to special scrutiny and comparison. The metabolic chamber was a closed room in which patients resided usually for a span of at least 24 hours. It modeled metabolism primarily as a function of oxygen intake and carbon dioxide outtake.

The armband device modeled metabolism partly as a function of movement as sensed by accelerometers. Its precise algorithms were proprietary and not available for the researchers to inspect. Its main advantages were portability and low cost. In other words, it was both immutable and mobile (Latour, 1990). It had several disadvantages. First, it presumably measured energy expenditure with less validity or reliability than the metabolic chamber, which was taken as the "gold standard." Second, its algorithms for computation were proprietary and not made available by the manufacturers for inspection. The algorithms for the metabolic chamber, on the other hand, were available for inspection in great detail. Third, the armband was designed and presumably calibrated upon adults, a more restricted population than these researchers wanted to consider. In other words, its validity, reliability, transparency and applicability were insufficiently immutable for transfer to their laboratory. Their research here was directed toward making these features more immutable and therefore more mobile in order to extend the use of the armband to meet their interests.

In Steve's telling, Sally's research question was, "whether this armband, uhm, measures the energy expenditure as well as the gold standard, the, uh, metabolic chamber." Based on measurements, a profile of energy expenditure for that patient emerged. Twenty-one (21) children used the armband device while housed within the chamber for their study. This allowed for direct comparison of devices. The episodes to follow provide a running narrative of the extended portion of the meeting where Steve recounts his work, first in terms of building a model for comparing devices and second, in terms of building a model that uses the armband measure to "predict" the chamber value. The approximate start time for each part of the narrative is included within parentheses at the beginning of each paragraph.

Making Device Disagreement Invisible (Doing it Wrong)

(2:00) As the meeting begins, Steve sits at the conference table. He promises to illustrate how to avoid some “simple mistakes” that “a lot of investigators … make” and how “to make the data a little more sophisticated for publication.”

(3:00) He walks over to the conference room computer at the lectern. He opens Sally’s SPSS® file putting the “data view” on display. He mentions that simply comparing devices based on correlation is a common mistake in the literature. To get published in a good journal you have to avoid this mistake. He mentions that the chamber measures energy expenditure. Lev interrupts to insist that the chamber and the armband measure different things: the “chamber measures oxygens that you use and carbon dioxide that you produce.” Steve interrupts him in turn, insisting, “they’re trying to predict the same.” As Lev begins to explain more, Steve interrupts him once again in order to continue with his prepared narrative. From this exchange, we learn from participants that the chamber measures gas in some way and that both devices are endowed with will and with foresight, as they are “trying to predict.”

The multiple interruptions prevented Lev from making others perceive more about the distinction between measuring and predicting. Thus the authority for framing terms to be invoked within the cognitive practice of demonstrating is mediated in part by the physical arrangement of people in the room and by the interpersonal arrangement of authority to speak. Steve and Lev enact a situated aspect of interactive practice in our culture, wherein the voice of the standing speaker overrides the voice of those sitting. Other situated practices contribute further to the channeling of most of the speaking and demonstrating to Steve. He stands in front with privileged access to the keyboard, which in turn is of course a required passage point for control of the display. He also has unique access to the display screen, which may further augment his unique ability to make things visible.

Correlations

		chamber exercise	sensewear exercise
chamber exercise	Pearson Correlation	1	.886**
	Sig. (2-tailed)		.000
	N	21	21
sensewear exercise	Pearson Correlation	.886**	1
	Sig. (2-tailed)	.000	
	N	21	21

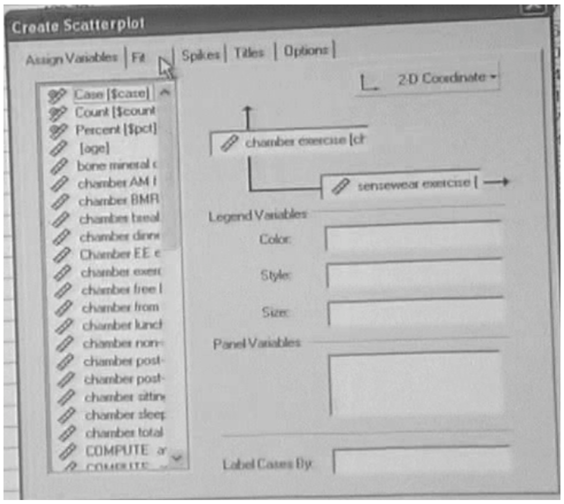
Figure 3: Facsimile of of the software’s analysis of correlations

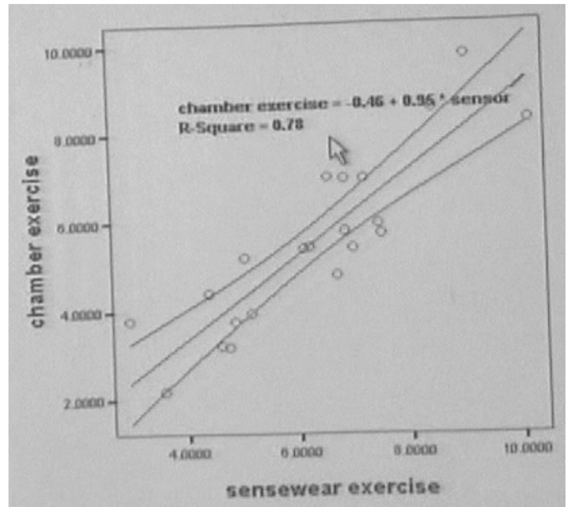
(5:00) Steve produces an analysis of correlations, a facsimile of which appears in the figure above. It shows a high correlation (.886) between the two devices and a significant finding that this correlation is different from zero (double asterisk after ".886" and a significance value of .000, which is vanishingly small in comparison to the conventional type I error rate threshold of 0.05). But he cautions that this strong significance is insufficient for agreement. "So, a lot of investigators will look at this and say, well, well, you know, that's really good proof that these two measures agree. But it's really flawed…" Note that in Steve's talk, a viewer might read into the tabular format that the devices agree. This imaginary reader imputes the metaphor of people agreeing to the devices. This particular reading is flawed because it uses the information provided in tabular display wrongly.

To explain why this is an incorrect reading, he returns to the data view (not shown) and points the cursor to the column heading for the chamber energy expenditure values and to the adjacent column heading for the armband energy expenditure values. As he moves his cursor from the chamber column to the armband column, he explains a counterfactual situation as follows.

> This armband could be predicting half of what the chamber's predicting. You could just put half in here (pointing to a cell in the armband column). And you would come up with a perfect correlation. Because, every time this goes up (points to cell in chamber column), the half goes up also (points to adjacent cell in armband column). But, a perfect correlation for something that's half is obviously not, um, good agreement. You wouldn't wanna say, "Armband agrees with chamber," if it always gives you half the number. (Steve)

The various positionings of performance and judgments in this counterfactual narrative is richly varied. First, "you" create the counterfactual narrative. You do this by imputing a value into each cell in the right column that is exactly half the value the cell to its left. In this counterfactual narrative, we are looking at two columns of data. You are to then imagine yourself noticing that each datum in the armband column is exactly half the value of the datum to its left in the chamber column. If that were so, you are to understand (1) that these two columns *would* correlate perfectly and (2) that these two devices *would* manifestly disagree. Steve continues with the counterfactual scenario. You then make a judgment that you would not want to make, presumably because such a claim would make you look foolish. Steve alluded earlier to flaws, but the flawed reasoning was not visible in the correlations tabular display of Figure 3. The high coefficient of correlation in the table is an insufficient indicator of agreement inasmuch as it only reflects a consistent proportional relation between the devices. We need Steve's counterfactual narrative in order for this insufficiency to become manifest.





GUI. List of variables on the left field. Visible in upper right field is "chamber exercise" and in lower right field, "sensewear exercise."	Scatterplot of sensewear exercise on horizontal axis and chamber exercise on vertical. Equation reads: chamber exercise = -0.46 + 0.95 * sensor R-Square = 0.78

Figure 4: GUI and scatterplot for the comparison of devices

(6:45) Steve then selects the following path line in the dropdown menu: Graphs ⇒ Interpretive ⇒ Scatterplot. This brings up a GUI for a "Create Scatterplot" (above left). He drags the "chamber exercise" variable to the vertical axis and "sensewear exercise" variable to the horizontal axis. Under the "Fit" tab, he chooses the method of "Regression." Steve then invokes the graph (above right) to show again that the two devices share a high square of correlation coefficient of 0.76. Note that this is the rounded value of the square of 0.886, the coefficient of correlation mentioned earlier. He then describes the graph as follows.

> And it certainly looks like, um, the armband and the chamber have good agreement, but, um, this is really not the way you want to send it into a manuscript, uh, it's a, it's a, reviewers are at all sophisticated, they'll, they'll pick up that this could be one half and we, we, you know, we'd have that flaw in the analysis … (Steve)

In this viewing, we see a visual pattern that shows high correlation this time as a progression of dots that more or less hang around an ascending straight line. Steve takes it for granted that his viewers have come to recognize such patterns as describing a high correlation. In the scenario he describes, a reviewer will respond negatively to your submitted manuscript if they too think counterfactually about the information given, if they imagine a line with lots of points hanging around the line but with the armband values being only half the chamber values. Steve, then posed a question for the next episode. “So, if this is the wrong way to do it, what, what would be the appropriate way that’s stronger to get it into a journal?” This question is relevant to our understanding of this episode inasmuch as it reveals that his motives in illustrating the wrong way here is to provide a contrast for the right way to make comparisons of devices.

The synoptic graphical organizer below summarizes some of the aspects of the activity described. Since this is the first such graphical organizer to be presented in this chapter, it is worthwhile to note that it is only a summary. Much detail is of course erased in its production and I have had to be extremely selective in choosing what goes in. On the left we have performances being made visible at the interface between embodied modalities and infrastructure. These performances speak to or facilitate the preservation of necessity that, once achieved, promotes the generality of the case shown for those who can follow that tacit implication. Meanwhile neither side of the interface remains static. Researchers are directed by Steve to see things differently now and in the future. The infrastructure is revised in the sense that a conventional mode of comparison is no longer to be seen as adequate for these scientists. Steve alludes to the historically changing infrastructure of publishing wherein a “good journal” would no longer accept submissions that compare devices by correlations.

So just as the SPSS® infrastructure is being made to come into coordination with the interests of the researchers, the researchers are also coming into coordination with it. For this group of researchers, the various relations for device agreement have been dismantled so that a new set of relations might be built. It is not known to what degree the various assembled already knew about the flaws (as Steve portrayed it) of relying only upon correlation. Nor can we directly assess how successfully Steve manages to convince his clients. We can say that Steve felt it necessary to be very didactic about this problem and that only the most senior member of his collaborative team for this project had anything to say as commentary—and that even he was spoken down to. The following synoptic graphic organizer summarizes this episode.

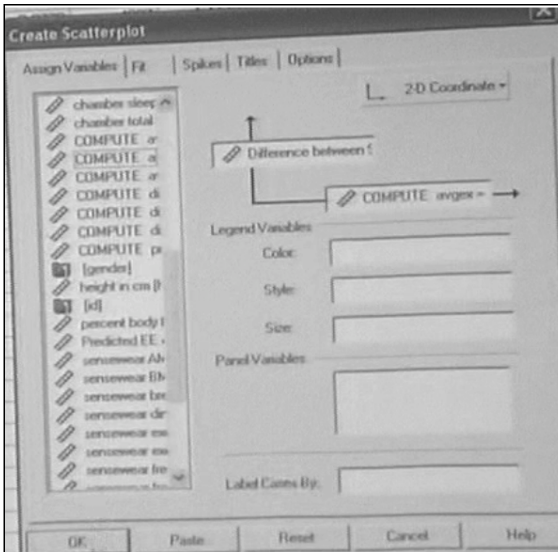
<table>
<tr><td colspan="5">⇒ Coordination ⇒</td><td rowspan="5">⇒</td><td rowspan="5">Revised resources: correlation is the wrong way to assess agreement even though some (bad) journals continue to accept it.

Knowledge produced: correlation once was seen as revealing agreement, but now does not. This is illustrated by means of a counterfactual, half-measuring device. It seems that some improved alternative is pending.</td></tr>
<tr><td colspan="5">Available infrastructure includes, scatterplot module in SPSS® with line of best fit, linear equation and square of correlation coefficient.</td></tr>
<tr><td>Devices can agree, they might measure, they try to predict (animated performance). Unauthentic agreement is contingent on data correlating well while disagreeing dramatically by a factor of 2 (contingent performance)</td><td>⇒</td><td>Preservation of necessity that in this case, a high correlation dramatically hides disagreement</td><td>⇒</td><td>Promotion of generality that in any such case, correlation may hide disagreement</td></tr>
<tr><td colspan="5">Internalized structures accommodate the recognition of a pattern of dots as suggesting linearity, the noticing of numbers for squares of correlation coefficient and the assessment as to what such a number says about the phenomenon of correlation.</td></tr>
<tr><td colspan="5">⇒ Coordination ⇒</td></tr>
</table>

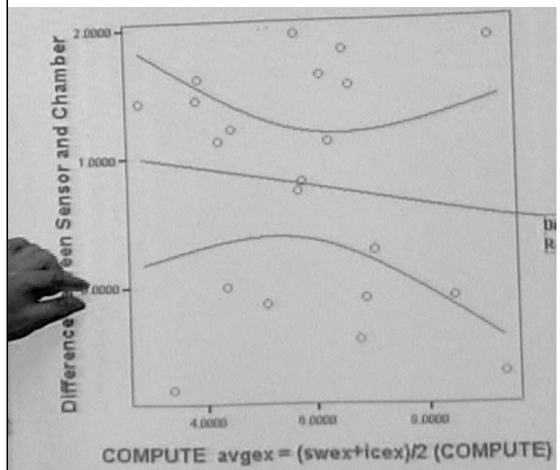
Figure 5: Synopsis for Making Device Disagreement Invisible (Doing it Wrong)

Making Device Disagreement Visible (Doing it Right)

(8:45) For Steve and for any “good journal,” the right way is an alternative known as the Bland Altman technique. To prepare for it, he returns to the scatterplot GUI to switch to a computed variable for device difference into the vertical axis and to a computed variable for device average on the horizontal axis (below, left). Steve explains that he has computed these two variables in advance.



GUI. List of variables on the left field. Visible in vertical axis field is “difference between” and in horizontal axis field, “Compute avgex.”



Scatterplot of the average of the device measures on the horizontal axis and the difference between measures on the vertical.

Figure 6: GUI and scatterplot for Bland-Altman technique

(10:45) Steve then invokes the scatterplot (above right) and asks, “Do you think that there’s good agreement now?” Lev answers that the armband device is sometimes “overpredicting,” sometimes “underpredicting.” Steve goes to the screen and points to places on the scatterplot that appear to show dots for situations where children wore underpredicting armbands (lower on scatterplot than the level of his opposing fingers in Figure 6 above) and others showing overpredicting armbands. He goes on to say that, “we’d like to see most of these differences right around zero here, right?” (note placement of fingers at this precise moment around “0.0000” on the axis in upper right). This directs attention to zero difference as equivalent to the desirable goal of agreement. Dots that fall above this zero level (most) are “overpredicting” and this is instantiated as a pattern of dots near an animated line.

(12:00) He then reads the extant graph against a hypothetical one not observable at the moment: “We’d like to see this trend across these differences stay pretty flat,” gesturing a flat line with his hands. Because this gesture did not follow the sloped straight line of the graph, his utterance of what “we’d like to see” indexes a counterfactual situation. After noting that, “We have a lot more points above zero,” he concludes that, “it looks like in general the armband overpredicts.” By stating this interpretation, he overwrites Lev’s earlier pronouncement that sometimes the armband underpredicts. The armband is clearly described here as an agent that predicts, with identifiable direction of error, thanks to the comparative measure of the metabolic chamber taken to be the standard for judgment. Steve gestures in an attempt to make this variation on direction of disagreement visible as a dislocation in graphic space.

He then reveals something more about Sally's project: she is looking for demographic factors that might contribute to such overprediction. He asks, "If these do disagree, does that disagreement relate to any characteristic of the child?" A decision as to how to incorporate the armband into their work would be contingent upon the upcoming performance of the demographic variables in the statistical tests.

It bears mentioning that again Steve takes charge as how to interpret the state of affairs of armband disagreement. His talk remains very didactic and he once again corrects Lev, though with much more subtlety than in the previous episode. Thanks to the new relations forged by means of the Bland Altman technique, the assembled now know about the right way to analyze device disagreement. This is not to say that they could do this analysis themselves, but that they now know something about the subtleties of such comparisons. It may be that all they now know is to rely on a statistician more when device comparisons are to be done. What I can say is that the eventually published paper included a similar Bland Altman plot. Sally is first author and Lev is a contributing author on this paper. Thus, these two effectively and eventually gave their imprimatur to this interpretive scheme. This brings us to the end of the episode and to its graphic organizer.

<table>
<tr><td colspan="5">⇒ Coordination ⇒</td><td rowspan="5">⇒</td><td rowspan="5">Revised resources: Bland Altman technique. It has an additional role beyond merely assessing disagreement. It is a means for people to assess the possibility of some underlying variables contributing to disagreement.

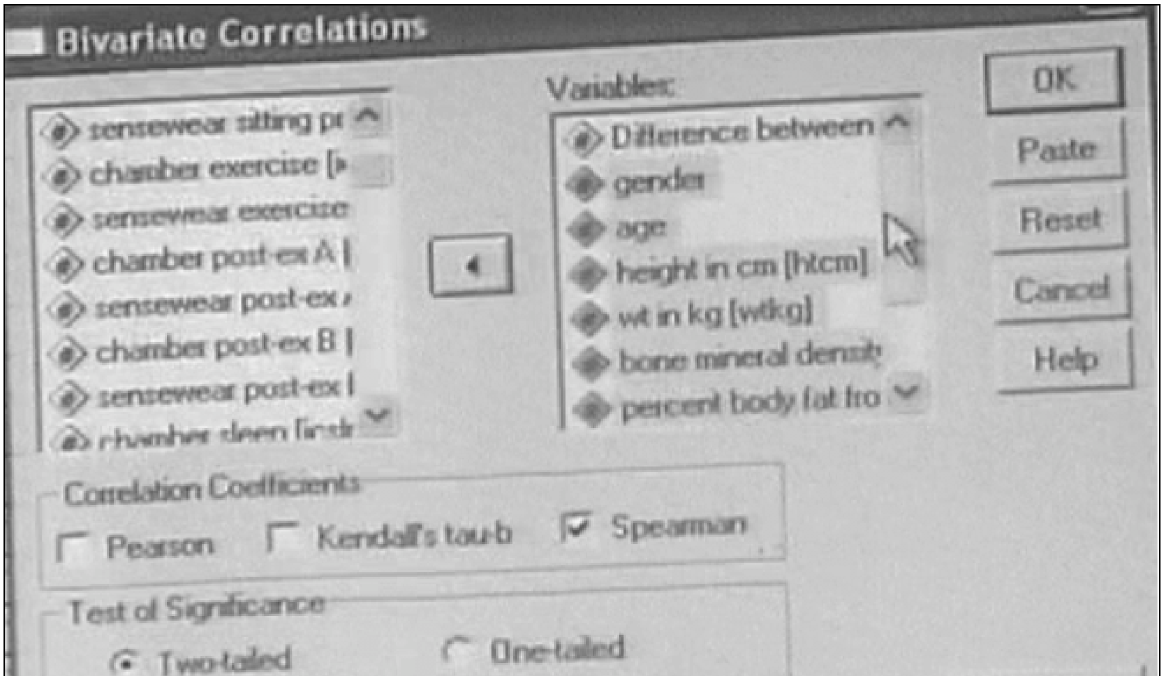
Knowledge produced: armbands by this manufacturer generally overpredict for children. Some underlying cause may be responsible.</td></tr>
<tr><td colspan="5">New infrastructure includes Bland Altman technique for assessing the structure of disagreement.</td></tr>
<tr><td>Armband animated as predictor. Systematic, graphic "direction" of prediction (over or under) as contingent upon plurality of points on one side of horizontal zero energy line.</td><td>⇒</td><td>Preservation of necessity that in this case the armband overpredicts overall.</td><td>⇒</td><td>Promotion of generality that the armbands would overpredict energy expenditures for samples of other children.</td></tr>
<tr><td colspan="5">Internalized structures accommodate seeing a horizontal zero energy line, overpredicting as above, underpredicting as below.</td></tr>
<tr><td colspan="5">⇒ Coordination ⇒</td></tr>
</table>

Figure 7: Synopsis for Making Device Disagreement Visible (Doing it Right)

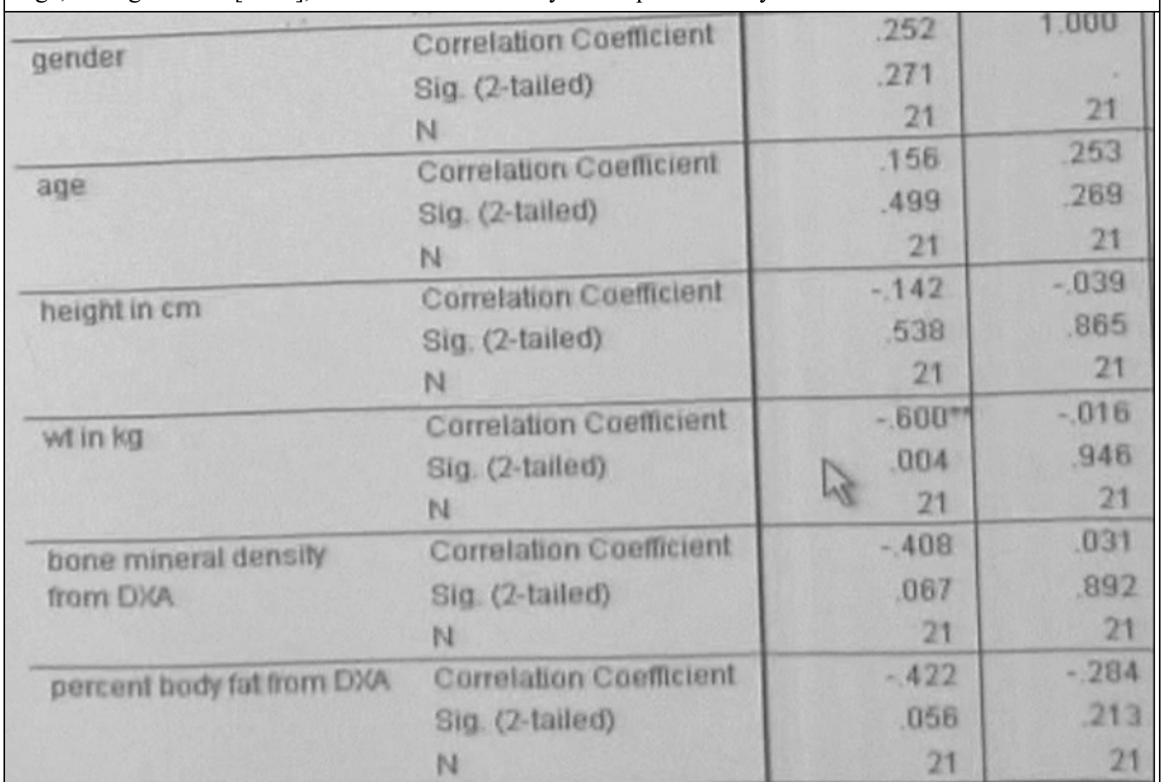
Making Weight's Significance Visible

(16:00) When prompted for predictions as to which "characteristics of the child" might be shown to be correlated with disagreement, Brian and Mary (neither is involved with the study) guess that body fat might correlate with difference between devices. From the menu Steve chooses Analyze ⇒ Correlations ⇒ Bivariate to produce a "Bivariate Correlations" GUI. Steve drags the following variables from the database list on the left into the model Variable field: difference between devices, gender, age, height, weight, bone mineral density and percent body fat. Steve then runs the test and the screen displays a tabular array of correlation coefficients between each of variables. Each variable appears once along the column headings and once along the row headings. He directs attention to only the leftmost column. Because device difference is in the column header, this column contains all the comparisons between device difference and other variables. In talk as he directs attention with the cursor, he discounts percent body fat as "not quite significant" (top of Figure 8, below). Mary and Brian are not part of the project, so the failure of their prediction only puts them in a position of ignorance, not incompetence. We observe them predicting wrongly, but without a lot of ancillary consequences. Here we might infer something about how the embodied activity of people might be folded into the agency of objects. People can predict. These predictions have uncertain validity and so must be tried out. Sometimes they fail; sometimes they succeed. Later in this meeting, predictability will be assessed for objects too.

(18:00) Next Steve explains, "So what is this telling us? Somehow the difference between the armband and the actual value is a function of how much the child weighs." Meanwhile, he hovers his cursor over the double asterisk of "-.600**" (bottom of Figure 8, below, indicating a significant correlation) on the row for weight. Steve has placed the Bivariate Correlations module into the role of an epistemic agent that tells. He employs his cursor to direct attention to a double asterisk in order make this telling clear—it tells us of the knowledge produced that device difference is a function of weight. The inclusion of the child's characteristic, weight, in the upcoming linear model is contingent upon its performance here as a significant factor.



GUI. List of variables on the left field. Visible in upper right field is “Difference between,” “gender,” “age,” “height in cm [htcm],” “bone marrow density” and “percent body fat from.”



gender	Correlation Coefficient	.252	1.000
	Sig. (2-tailed)	.271	.
	N	21	21
age	Correlation Coefficient	.156	.253
	Sig. (2-tailed)	.499	.269
	N	21	21
height in cm	Correlation Coefficient	-.142	-.039
	Sig. (2-tailed)	.538	.865
	N	21	21
wt in kg	Correlation Coefficient	-.600**	-.016
	Sig. (2-tailed)	.004	.946
	N	21	21
bone mineral density from DXA	Correlation Coefficient	-.408	.031
	Sig. (2-tailed)	.067	.892
	N	21	21
percent body fat from DXA	Correlation Coefficient	-.422	-.284
	Sig. (2-tailed)	.056	.213
	N	21	21

Tabular array showing a significant value for weight in kg correlation (double asterisk) and a “not quite” significant value for percent body fat from DXA (“.056”). Column heading (not visible) above cursor is “Difference between Sensewear and Chamber.”

Figure 8: GUI and tabular array of correlation tests

So far Steve has demonstrated that correlation can act as a deceptive epistemic artifact, if mediated by humans with unsophisticated interpretive skills. A more sophisticated approach is to compare device average with device difference by means of a scatterplot. This analysis, mediated by human interaction with a graphical pattern, revealed a state of affairs that the armband generally "overpredicts." Steve spends some time disciplining the perception (Stevens & Hall, 1998) of his clients, animating his own professional vision for seeing and perceiving this state of affairs in the laboratory (Goodwin, 1994). In this latest episode, Steve positions the Bivariate Correlations module as an epistemic agent that tells the assembled which variables might be responsible for overprediction. This analysis, mediated by human interaction to notice a significant variable, revealed weight to be the only statistically significant characteristic related to device difference. Steve is now about to produce yet another scatterplot, positioning SPSS® as an epistemic agent to reveal how weight becomes a significant factor with respect to device disagreement. That is, he queries whether the problem is with heavy kids or light ones? We come now to the synoptic graphic organizer.

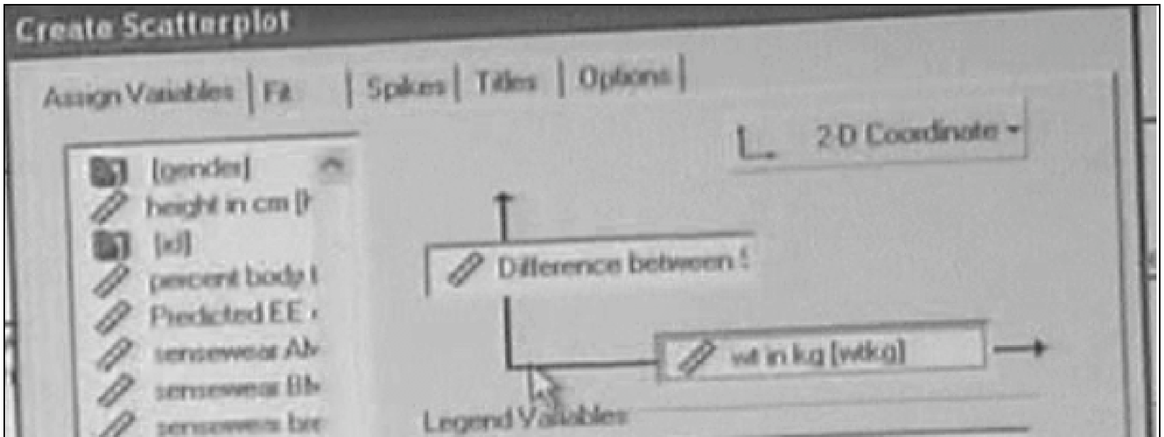
⇒ Coordination ⇒					Revised resources: bivariate correlations as means of getting told the variables of which a pattern of disagreement is a function. Knowledge produced: device difference is a function of weight as revealed by a bivariate correlations module applied after using Bland Altman technique.
Available infrastructure includes Bivariate Correlations GUI, its tabular array of tests of significance as well as the custom here to regard significant estimates as detections of real effects.					
The table produced by the GUI tells us device difference is function of child weight. The inclusion of weight in the upcoming model is contingent on its performance here as a significant variable among multiple correlations.	⇒	Preservation of necessity that in this case weight is a real factor of which device difference is a function.	⇒	Promotion of generality that in all armbands from this manufacturer, device difference is a function of weight for children.	⇒
Internalized structures accommodate the selection and dragging of variables into a GUI field, recognition of the relevant column of the generated tabular array.					
⇒ Coordination ⇒					

Figure 9: Synopsis for Making Weight's Signifcance Visible

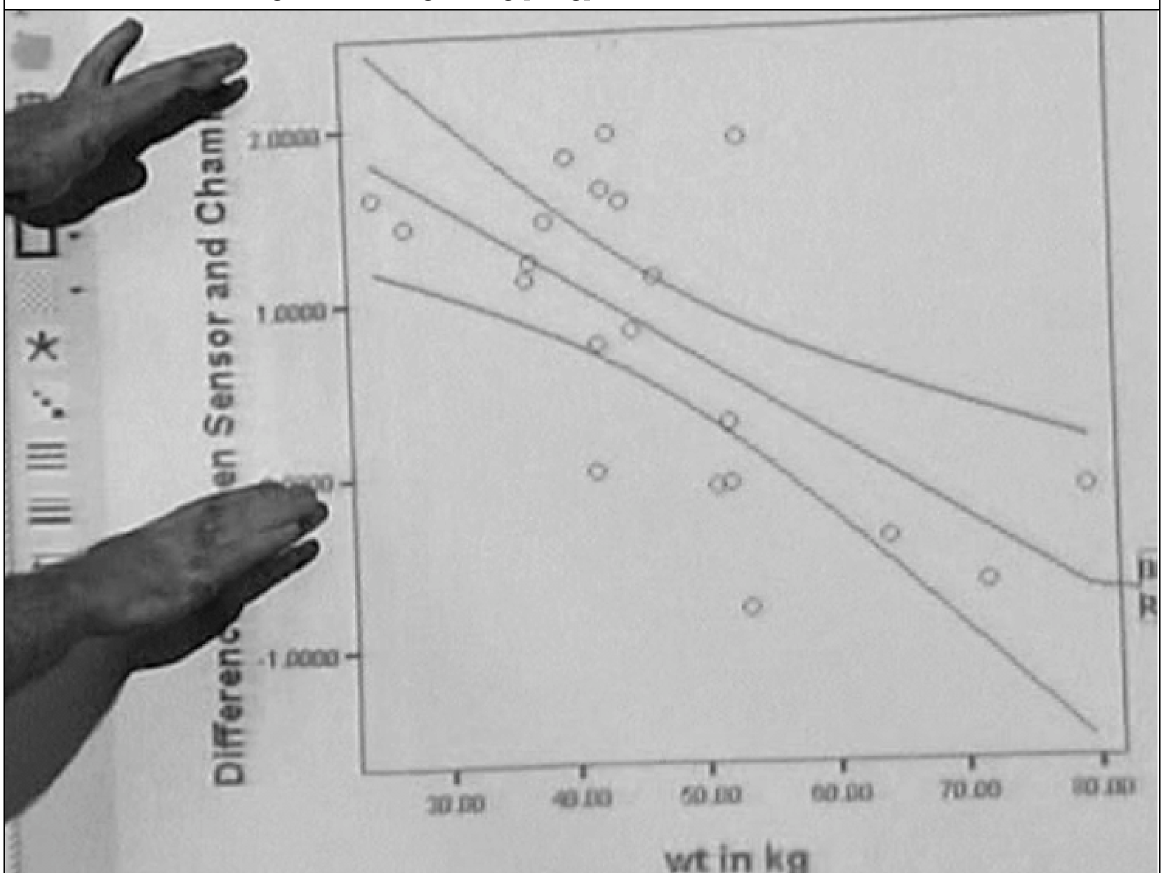
Making Weight's Role Visible

(19:45) At this point we continue with the meeting, but prepare to follow it more closely in a transcribed episode. You will recall that Steve has just made weight visible as a characteristic related (in some way) to device disagreement. He then asks, “So now we want to know, is it heavier children that overpredict?” In other words, he now directs his audience’s attention to modeling the manner in which weight and device disagreement are related. To answer this question, he visits the scatterplot GUI once again, this time switching to “weight” as the horizontal variable but leaving device difference as the vertical variable (top of Figure 10, below). At this point we arrive at episode proper. The transcript for roughly the first half of this episode is presented in Appendix A, Making Weight’s Role Visible, as a series of sequential panels. The latter half of spoken utterance, a monologue, is presented as text near the bottom of this transcript. A picture of a screenshot of the graph is presented at the bottom of Figure 10, below. It is suggested at this point that readers read the panels of the transcript in the Appendix before continuing.

This transcript includes some particular notations and conventions that will be mentioned here and which are repeated below the text of utterances. First, the uttered word spoken at the time of the screenshot for a given panel is indicted in bold white font. Second, words spoken with emphasis are shown in CAPITAL letters. Third, overlapping speech is marked [with brackets]. Fourth, arrows toward/away from hand indicate movement immediately prior to/after the screenshot. With sufficient attention and successive rereadings, it is possible to more or less get a sense of the experience the activity. If a richer experience is desirable, it helps to read the transcript out loud and to gesture over the large screenshot of the scatterplot in the figure below.



GUI. List of variables in the left field. The “legend variables” are visible in upper right field is “Difference between” and in lower right field, “weight in kg [wtkg].”



Scatterplot of “wt in kg” on horizontal axis and “Difference between Sensor and Chamber” on the vertical axis. The horizontal axis is marked off as 30.00 kg, 40.00 kg and so on. The vertical scale is marked off as -1.0000, 0.0000, 1.00000 and 2.0000 units of energy. In later publications this variable is called “energy expenditure” while specified in units of energy per time, kilocalories per minute.

Figure 10: GUI and scatterplot for Making Weight's Role Visible

As with all the transcripts to be presented in this investigation, it is preferable to have a transcript available for inspection separate from the analytical text that follows. For example, if reading this document as a PDF file, you might open two versions and reserve one for viewing the transcript on a separate window. You may recall that the Ancient Greeks encountered a similar need to regard text and diagram simultaneously. Because a scroll may be folded, it is technologically superior to a bound volume in this respect. Now, at this time, please read Transcript A, Making Weight's Role Visible.

Now that you have read the transcript, it is worth reflecting for a moment upon what Steve may wish to accomplish as he talks, gestures and moves about. In other words, what does Steve intend to add to the reading of a textual object that is plainly visible without any further intervention from him? How does he help his clients to see the scatterplot better by gesturing in front of it? It seems that some features are easier to notice if his hands and arms sometimes obscure them. It appears that objects not present (energy, weight, children) must be discursively bound to features of the scatterplot. By paying attention to the particular manners by which Steve binds these non-present objects to present features of the scatterplots, we will gain some insight into the cognitive practice of demonstrating, to the performance of objects and to the production of knowledge. In a rough sense, Steve's discursive, embodied, interactive work converts a mere representation into a demonstration of actors. Now that we have reminded ourselves of the discursive means by which objects may be made to perform and of the knowledge-producing ends to which these discursive means are employed, we should consider some details of this demonstration.

Steve first directs attention to some objects one might take naively as literal: the graph itself and weight as an axis (panel 1), device difference as an axis (panel 2), zero as a position on that axis (panels 3 & 4), the literal direction from zero as a prefix for prediction ("over-," "under-," panels 5—7). Indeed, when Steve announces, "this is our graph," his mode of reference might be taken as literal. But he also qualifies this object as something into which we may see or remember a past view of correlations already observed. The correlation is a prior event upon which this graph here is contingent. Given the educational background of the audience, everyone is presumably able to notice *that* this thing observed is a graph or *that* it has these particular axes (panels 1 & 2). Steve would certainly know this. So what is the purpose of stating these obvious and mundane facts? Maybe stating the obvious prepares everyone to pay attention, to enter a state of intensive observation in preparation for seeing some less obvious but impending thing. That less obvious thing might be the zero value on the horizontal axis indexed by Steve's gestural cut in panel 3. On many other graphs, the value of zero is located at the bottom of the graphical space. Steve might anticipate that his audience might not notice this.

At this point it may not yet be clear how a perspective on object agency enhances our understanding of this cognitive practice. Maybe Steve is only helping his audience to pay sufficiently close attention in order to see inanimate objects manifestly there. Maybe it is only important to coach them *to see*, rather than to help them learn to interactively engage with objects in order to animate them so as *to make them see-able*. Maybe the discursive means employed here only propel people to see what is obvious more quickly than they would have if left to themselves. But his next comments in panels 4—7 start to make this naïve realist view problematic. Steve merges objects in the literal view with objects of the scientists' laboratory. He deliberately builds relations between things local and things global. In Steve's professional vision this graphical domain is a scene where objects from Sally's laboratory are made to perform: the armband might overpredict and might underpredict, not in the laboratory, but here before our eyes either up here or down there. By simply attending to what Steve has said so far, it starts to become difficult to view this talk as merely a local activity. It starts to become apparent that some work must be done in order to span the referential gap between the space of the screen and the space of the laboratory.

Next, Steve solicits his audience's understanding of the "conclusion," or logical consequences incumbent to seeing the graph in terms of the features just highlighted (panel 8). Brian succinctly answers with his interpretation that the armband works worse for children of lower weight (panel 9). At this point Steve begins to delineate more features of the graph in order to set the stage for the object performance he wishes his audience to observe. He repeats Brian's interpretation to communicate his agreement (panels 10 & 11). But he then qualifies this agreement somewhat by asserting that the coupling of lighter children to worse-working armbands originates at a particular location on the horizontal axis (panels 12—14). In order to delineate this originating location, he directs attention to a location on the graph for "a certain weight" (panel 13).

The critical value of zero energy on the horizontal axis discussed above could be interpreted by some as a manifestly obvious feature of the graph, something that anyone who pays sufficient attention could find. In this interpretation, no special work is needed to make the zero energy value to become visible, as this visibility has been sufficiently taken care of by the conventions of graphical form, conventions that are built into the software. Anyone who knows the grammar of these forms should be able to just see the location of zero energy. People should just see the zero mark on the vertical axis. The fact that Steve makes a deliberate effort to highlight this location of zero, suggests that in Steve's experience, researchers often miss such details. So, we might doubt that, in fact, ostensibly obvious features of graphs are readily apparent, even to highly educated readers.

But even if we disregard the necessity of making the zero value more apparent, we must concede that the critical value of a certain weight requires undeniable work in order to be made to be seen. In order to instantiate this critical weight, Steve first finds zero energy along the vertical axis and slices horizontally from there to the right over to the regression line (panel 12). Steve appears to see a need to make his audience see this property of zero energy as a feature that extends across horizontally from that zero point he had located by first scanning along the vertical axis, as he gesturally cuts across the space in panel 12. That is, Steve does not appear to take it for granted that his audience will coordinate the ordering of things vertical with things horizontal, which is to say that he does not assume his audience will appreciate the primary structural property that makes the Cartesian plane such a powerful technology. He then holds a vertical span down from the regression line to the horizontal axis, at which point the certain weight can now be observed by all to have been found (panel 13).

Locating the certain weight required the coordination of three primary objects: zero energy on the vertical axis, the regression line and the horizontal axis. The certain weight is a unique location on that horizontal axis that enjoys certain properties that no other point on it has. For this process of locating to be generally intelligible, the assembled must attend to and recognize the regression line as a feature relevant to the coordination of things horizontally arranged with things vertically arranged and must disattend to the curvilinear lines around it. The exhaustive attention to detail here may be instructable for us in the Learning Sciences. It might remind us of the complexity incumbent to the competent use of the Cartesian plane. If we assume that Steve attends to such details in order to address common misreadings that in his experience medical scientists tend to make, then we can only conclude less sophisticated users must also be continuously reminded how to read these graphs.

It bears mentioning that in panels 13 and 14 Steve might seem to misspeak when he states that, “after a certain weight, the armband starts to overpredict.” The word “after” might by convention orient overprediction to the right. Thus, following such a convention, we might interpret Steve as saying that dots to the right of a certain weight illustrate situations of overprediction. This would contradict his assertion that armband does “worse” (panel 11) at locations the zero line. Recall that above the zero energy line, the armband overpredicts (panel 5). But he is probably orienting himself to the left. It is likely that Steve has coupled Brian’s sense of the progression of weight variation (“The lighter you are, the worse it works?”) to a leftward spatial orientation for what goes on “after.” In fact, as Steve immediately then makes clear, to the right something other than overprediction is happening. To the right of this certain weight and below zero energy the armband works better (panels 15 & 16). He then locates a position above zero and to the left of the critical weight as the location where overprediction happens and where the trouble with the armband can be circumscribed (in keeping with the idea that positions to the left are “after” a certain weight; panels 17—19).

This process of seeing and illustrating things is progressive. Each time he finishes highlighting and coding one feature (Goodwin, 1994) he builds upon this newfound sense of graphical reality to highlight and code still others. He begins by highlighting the zero energy position for delineating halves of the graphical planar surface where overprediction and underprediction occur, in the upper and lower regions, respectively (panels 1—7). He then uses this important feature of zero energy as an anchor to make a certain weight become visible (panels 12—14). Next, he connects a discrepancy between levels of agreement in the laboratory to a discrepancy between quadrants on the planar expanse of the graph (15—19). Good agreement in the laboratory is made to be connected to the lower right quadrant. Bad agreement of the overpredictive kind is made to be connected to the upper left quadrant.

Thus we observe that by this time an extensive amount of additional work has been done to make features of the graph, as well as practices in the laboratory, visible. This work is much more than can be presumed to be carried out by graphical conventions, or by mere grammars of reading. This additional work constitutes one avenue for observing that the graphical analysis carried out in this meeting is realized by something other than mere *representation*, as the term is usually understood. I characterize this in contrast as the work of *demonstration* in the sense that it requires concerted work to make states of affairs of the laboratory play out in the conference room. This work is of a kind with the construction of air pumps (Shapin & Schaffer, 1985), though effected in much shorter time and with much less sweat. Just as air pumps and flasks are positioned and observed to perform so that the spring of air could be made real, critical value, dots and lines are discursively positioned to make device agreement and disagreement real.

As this episode comes to a close, Steve suggests yet another statistical analysis, to “make some adjustment and sort of downplay the weight of the armband and also take into account the weight of the child” (panel 20). The weight of the armband to be downplayed is not literal in the way that the weight of children is literal, but refers to a forthcoming diminishment of its role as predicting true energy expenditure. In the forthcoming “linear regression” the armband will share the role of predicting with the weight of the child. The synoptic graphic organizer below summarizes this episode.

<table>
<tr><td colspan="5">⇒ Coordination ⇒</td><td rowspan="4">⇒</td><td rowspan="5">Revised resources: using scatterplot as a means of assessing the direction of influence that a variable might have on device difference.

Recognition of critical “certain” weight and nuanced judgment in seeing underpredicting yet working better in one location but working worse and most off in another location.

Knowledge produced: armbands overpredict among lighter children as revealed by recognized pattern in scatterplot.</td></tr>
<tr><td colspan="5">New infrastructure includes scatterplot graph in the role of querying the direction of influence of a variable (weight) on device difference.</td></tr>
<tr><td>Armband underpredicts after a certain weight and is most off among lighter children (animated) and its inclusion in the upcoming linear regression model is contingent upon its performance here as a variable that makes the armband worse for lighter children.</td><td>⇒</td><td>Preservation of necessity that in this sample, the armband works worse for children weighing less than a critical threshold weight.</td><td>⇒</td><td>Promotion of generality that such armbands will consistently overpredict energy expenditure within the population of lighter children.</td></tr>
<tr><td colspan="5">Internalized structures accommodate recognizing critical values along the horizontal and vertical axes.</td></tr>
<tr><td colspan="5">⇒ Coordination ⇒</td><td></td></tr>
</table>

Figure 11: Synopsis for Making Weight’s Role Visible

Folding Measures into Data

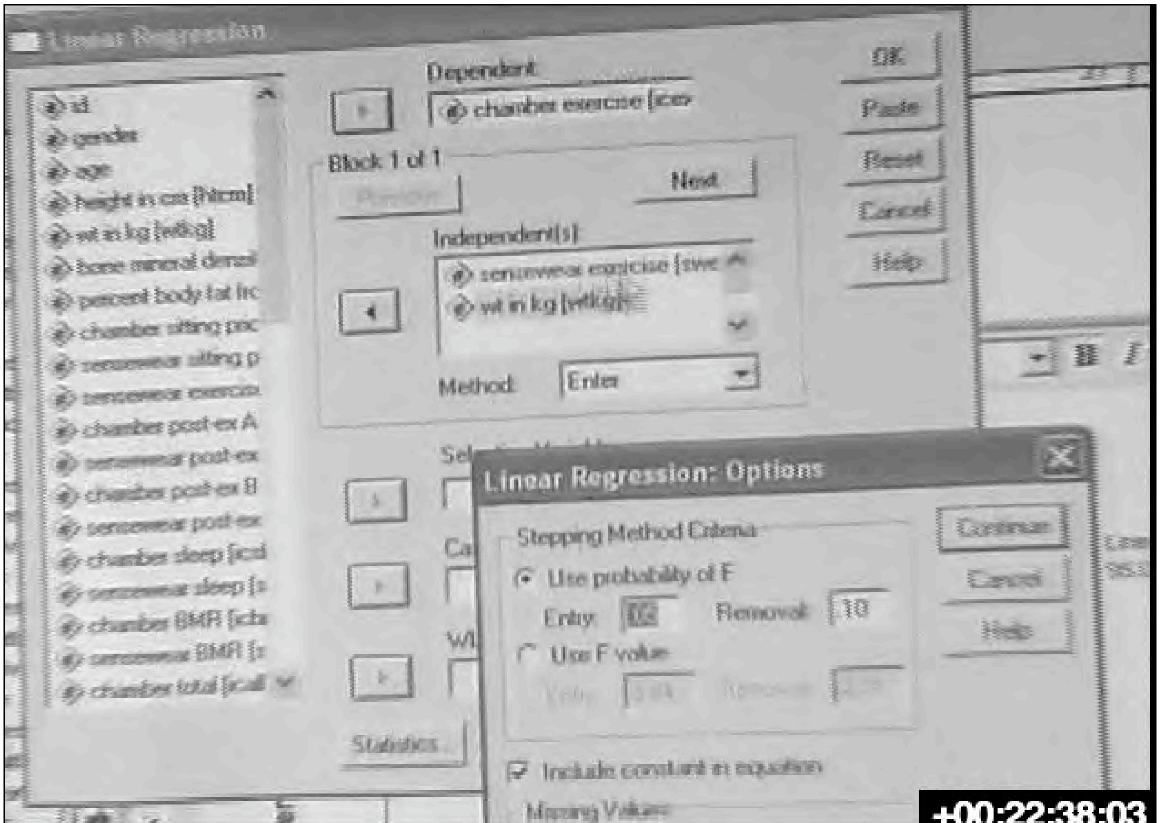
(21:30) Steve selects the following pathway in SPSS, Analyze ⇒ Regression ⇒ Linear. This brings up a new GUI entitled “Linear Regression.” Note that because his adjustment now includes two independent variables, weight and armband, he can no longer use the scatterplot GUI discussed earlier. That earlier GUI reserved room for only one (independent) variable on its horizontal axis. The several graphs produced so far with that module were created as scatterplots. He chose to include the optional linear regression fits in those scatterplots. The iconic use of intersecting axes in that scatterplot GUI suggests that in order to investigate a relation graphically, you are limited to a single independent variable. The upcoming analysis here differs in that it has two independent variables, which is why Steve selects a new menu path and GUI. Ironically, though this analysis is categorized within SPSS® as "linear," in this version of practice, it is invoked only in circumstances for which no line could be produced to express it. If this model were depicted graphically, it could express itself as a planar surface within a volume if certain conventions are adopted available software other than this one. Statisticians regard such models as "linear," even if they include too many variables to be depicted spatially as lines, because in the algebraic form of their equations there are only constants or coefficients of variables of degree one. A prototypical equation of this form having only one independent variable would be expressible as a graphed line on a plane.

(22:30) Steve selects armband and weight as independent variables ("sensewear" and "wt in kg," Figure 12, below) in order to predict the one dependent variable ("chamber energy expenditure") during exercise. He explains as follows.

> Normally you stick a constant in your equation here. But in this case I am going to take this out (pointing with cursor to the default option of the GUI to "Include constant in equation," the box has yet to be deselected in Figure 12). Because it's really not logical if there's, if there's no energy here, that we should expect something here (pointing respectively to "sensewear exercise" and "chamber exercise"). Uhm, it'll just make it a little bit cleaner also. (Steve)

Steve's rationale is both logical and aesthetic. He states that doing otherwise would not be logical. He depicts a situation wherein one observes, perhaps in the laboratory, the armband reporting a zero measure. In such a circumstance, one should expect that the chamber also reports zero. One common terminology for such a model is "restricted," though it is not used here. This latter term evokes the idea that "excluding" the constant in the model in effect actually includes it but in the restricted form of the constant having the specific value of zero. The counterpart to a restricted model (constant "not included") is a full model (constant included). Doing it this way is cleaner as well, by which he appears to express an aesthetic sense that the fewer constants or coefficients, the better. It should be noted that beauty is in the eye of the beholder, as is suggested by the alternative aesthetic implicit to the terminology of restricted model.

Restricting would be to put an aesthetically unnecessary and arbitrary constraint upon the full model. In any case, the upcoming decision as to how to downplay the effect of the armband and to adjust for the weight of the child will be contingent upon how this statistical model performs once Steve selects the “OK” button in the upper right corner of the Linear Regression GUI of Figure 12, below.



Linear Regression GUI and Options GUI. In the Linear Regression GUI, the list of variables is in the left field. Visible in upper right field for the “Dependent” variable is “chamber exercise [icex].” Below that in the “Independent(s)” field are “sensewear exercise [swex]” and “wt in kg [wtkg].” In the pop-up Options GUI, Steve is about to deselect the check box for “Include constant in equation.”

Figure 12: Screenshot of Linear Regression GUI and Options GUI

(23:00) Steve then produces a table showing the consequences of the particular modeling decisions made by Steve, as shown in the facsimile in Figure 13, below. At the top of that figure is an extended quote of Steve. He recites a formula that he makes to emerge from his reading of the table.

But really what we were after is this part here (hovers over coefficients, ".637" and ".034" with cursor). So, we could use this to have a formula (moves to screen and points to the unstandardized coefficients under the column head "B" with finger) that says the predicted energy expenditure is whatever value you get from the armband (points to "sensewear exercise") times point six (points to ".637"), because we know we need to reduce it down a bit, we don't want to take the full amount, plus whatever the weight is (points to "wt in kg") times this coefficient (points to ".034"). So we can have a little formula there that would try to make an improvement in that armband for children.

Coefficients [a,b]

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	sensewear exercise	.637	.089	.715	7.175	.000
	wt in kg	.034	.012	.286	2.872	.010

a Dependent Variable: chamber exercise
b Linear Regression through the Origin

Figure 13: Steve's recitation and facsimile of table without constant (restricted model)

By reading over the table in this way, Steve instructs his audience as to which features to attend to as relevant. Much detail is thus deliberately ignored, such as the distinction between unstandardized and standardized coefficients, the strength of the t-statistics or the comparison of significance values to type I error thresholds, to name just a few that others might find to be relevant in a statistical practice. So it should be noted that Steve exemplifies a method of ignoring within his exemplification of his method of reading. Because the formula is recited, it has little endurance as an object for inspection. It only persists in the collective awareness of the assembled as a shared memory.

It should also be observed here that much work in translation must be undertaken for this recitation to be intelligible as an equation. There are at least two salient translations. First, we "reduce" the value of the armband from "the full amount" of 1 to the value of ".637." Second, one must translate Steve's coupling of the utterance "whatever you get from the armband" with the deictic reference to the text "sensewear exercise" as a reference to the imputation of a specific value into the variable of a formula. The statistical software has not provided the researchers access to a text for this formula. The absence of such a textual formula is notable in the sense that we have already seen in the first of the scatterplots shown earlier that the software is capable of re-expressing models in the forms of an equation. Using this evidence we could argue that the designers of the software in some sense chose not to include the equation here. However, we might access such a text for the purpose of appreciating the cognitive challenge of following

Steve's recitation. The equation below is a version eventually printed in a journal article.

$$\mathbf{EE\ [kcal*min^{-1}] = 0.637EE\ (SWA)\ [kcal*min^{-1}] + 0.034 * body\ weight\ [kg]}$$

In this article, Sally is listed as first author while Lev is listed as a contributing author. Interestingly, a colleague of Steve's is listed as second author and Steve is not listed at all. Nevertheless, you can see that the values that the software has produced above appear in this equation. Reading this formula as text requires some extra work of interpretation: units of measure are in brackets, the dependent variable is EE and the independent variables are EE (SWA) and body weight. As Steve recites, he alludes to the imputation of a specific measure of armband energy expenditure into a variable such as EE (SWA) and to the imputation of a specific measure of weight into a variable such as body weight in order to calculate a value for a variable for the "predicted energy expenditure" such as EE. The point here of alluding to an equation that no one sees (yet) is to underscore the considerable cognitive work these participants must perform in order to translate textual information in the table into an intelligible formula. The formula is difficult enough as it is to follow even when written out as an inspectable, permanent text. In the ensuing episode we shall see that participants face the yet more daunting challenge of carrying out the cognitive work of collaboratively discussing features of this barely present formula.

By stating the formula, Steve relates the ultimate goal of Sally's project: using plausible values generated from statistical software suitable for recalibrating the armband device. These formulas would be useful for future studies of metabolism with children and will be publishable as findings in their own right. Prior to this episode, the process of investigating disagreement has been fairly symmetrical towards its treatment of the roles of the two energy-measuring devices. Only the choice to subtract chamber measures from armband measures in the calculation of device difference belied any privilege granted to one device over the other. But this latest statistical model expressly frames the armband in a subservient role to the chamber. It is modeled here as providing only part of the information needed to predict energy expenditure: you will have to know the weight of the child as well.

(23:45) Lev then asks for a rationale for taking the constant out of the model:

> But, um, could you, could you explain again this, ah, skipping of this, ah, constant, why it's cleaner? Because usually in all these equations you always have constant. And constant has, ah, some always, um, like a t-value that is significant that is usually very high. (Lev)

Here Lev brings up an argument that is statistical. The criterion for keeping the constant in the model is that using it results in a higher t-statistic. The t-statistic, you may recall, is one of the textual features Steve ignored while discussing the linear regression table above. In answer, Steve begins to use a criterion that is not statistical but technological and biological. If the armband were measuring zero, "you shouldn't be expecting, um, any energy expenditure." A model that includes a (non-zero) constant is illogical for him because it would imply an impossible circumstance. Brian interrupts with strong disagreement about this configuration of things biological and things technological. He argues that a sleeping person uses substantial energy. In his hypothetical scenario, the armband would report zero energy expenditure but the chamber would report, "like 60 percent of your daily caloric intake." Thus for Brian the same situation that Steve finds impossible and illogical, he finds to be possible, even required and therefore logical.

For the first time in this meeting there is open and sustained disagreement about the modeling decisions presented for consideration. For the first time objections have been raised and they have not been overridden. Lev and Brian challenge Steve on different grounds. Lev challenges on grounds of conventional statistical practice as he has learned from people like Steve: making fewer a priori assumptions and giving greater credence to models that produce higher t-statistics. In other words, he is challenging Steve to justify his statistical reasoning. Steve has folded the armband and weight into a model that predicts energy expenditure. Lev attempts to unfold this model on grounds that standards in statistics for doing things the right way demand it. After all, this entire lecture so far has been about doing things the right way. Lev provides the possibility of a higher t-statistic as a possible warrant for unfolding the model without the constant and for refolding into a new model with a constant. This argument relies upon warrants of convention from statistical practice. As mentioned, this project is too far along for Steve to seriously entertain a radically different model. Lev seems to be mostly interested in having Steve explain his aesthetic sense that excluding the constant is "cleaner," and not so much in seriously requesting a major revision. Brian on the other hand is challenging Steve to justify his scientific reasoning about devices and body metabolism. If Brian's challenge wins the day, Steve will have a lot of work to do before Sally travels to her conference. At this juncture let us consider the existing state of affairs as summarized by a synoptic graphic organizer.

⇒ Coordination ⇒

New infrastructure includes a GUI for a linear regression model, a generated table of coefficients without a constant and with a t-statistic and significance estimates.

Devices animated by Steve within a hypothetical illogical world as one reporting zero but the other not. Devices animated by Brian doing the same but as a logical consequence in our world. ⇒ Preservation of necessity not achieved within the assembled group. The assembled as a collective do not see these coefficients as necessary consequences of reasonable assumptions. ⇒ Promotion of generality might still be contingent upon some resolution of the dispute.

Internalized structures accommodate recognition of values on table and recounting of facts about metabolism.

⇒ Coordination ⇒

⇒

Revised resources: Using GUI of linear regression to set up model for prediction as opposed to earlier models of comparison.

Knowledge produced: contested. There is a new arrangement to make for knowledge but its status is made contingent upon the resolution of one statistical objection and one scientific one.

Figure 14: Synopsis for Folding Measures into Data

Unfolding Data into Measures

This transcript picks up where the last episode leaves off. It is illustrated through a transcript for Unfolding Data into Measures in Appendix A. One segment of this transcript is depicted as a series of panels. A dispute is in the air and the participants are now engaged in the task of finding logical or aesthetic grounds for their points of view. We are going to observe a rather complicated relation between the epistopics of knowledge production and agreement. We will also observe that the act of demonstrating is somewhat less effective here. This lack of effectiveness will be seen as at least partly the consequence of what the representation does not do and a consequence of what Steve chooses not to do. The warrant for alluding to things not done is that similar things had been done in this meeting. In this sense the parties are making the choice to do otherwise than they had done. As before, the particular details by which parties demonstrate, forge agreement or fail to forge agreement, are relevant to the general points to be made about the epistopics of interest. Thus, attending to the details is important for understanding the grounds for the conclusion taken in this part of the investigation. Please attend to a few conventions for the transcript, as repeated at the bottom of it but summarized here. Overlapping speech is in [brackets]. Interruptions of turns are indicated with a double slash //. The uttered word at instance of screenshot is <u>underlined</u>. A turn is a more or less continuous utterance without notable interruptions. Some of these turns stretch from one panel to the next. Turn 15 spans from a panel to a line of the transcript. Some panels include multiple turns. At this point, it is suggested that the reader stop and read through the transcript for Unfolding Data in Measures in Appendix A.

Steve first explains that zero energy expenditure is probably not possible (turn 1), a way of problematizing the situation he has posed and that Brian has critiqued. It may well be that the controversy has compelled Steve to think more literally about what a situation of zero energy expenditure would be like, in the chamber or around the armband. He then turns his attention to remaking the model, if only to amuse his audience (turn 1). This part of reworking is not hard to do. He simply retraces the menu path, opens the GUI and re-selects the box to "Include constant in the equation" (this activity is not illustrated here). Meanwhile, Brian turns to Lev to weigh in on the scientific point of view, asking him to confirm that the two devices would presumably not come close to agreement (turn 2).

Lev then relates to Brian a narrative assembly (Hall, Wright & Wieckert, 2007, as discussed in Chapter I) involving the resting metabolic rate, raw data, algorithms, measures, and the mathematical operations of subtraction and addition. This part of the episode is depicted in panels (turns 3—16). Lev first places his hand on the table (obscured by Brain) to indicate a baseline, to show what "they" (the armband manufacturers) predict the "resting metabolic rate" to be for a given person (turns 3—5). By doing so, Lev connects a situation in the laboratory to a location on the table that both he and Brian can see. The grammar of this talk is akin to the grammar of a graph in that things elsewhere are depicted in spatial terms here. For this reason I characterize this narrative assembly as a representational form whose deployment in use serves the purpose of demonstrating the performance of objects.

In turn 5, Lev bundles several demographic measures into the laboratory prediction that has been bound to the level of the table. In this way, Lev reverses the work just completed by Steve to subsume the armband measure into a model. For this reason, I characterize his work here as unfolding the armband. What the armband detects from the movement is animated with his arm (turns 5—7). Note that even as Lev unfolds the linear prediction model so as to make the armband salient again, he folds several variables into it. Thus, what is already folded away into the armband is taken as the baseline, and the child's movement is taken as a vertical displacement above that baseline (turn 7). In the cognitive practice of illustrating how a machine works, Lev invokes resources that are both spatial and metaphorical. In turns 6 and 8, Brian agrees with Lev and even co-completes with Lev his description of movement going "on top." Steve attempts to interrupt (turn 10) but this time Lev appears determined to finish his comments about the folding of measures into predictions (turns 9—11).

Lev then mentions subtracting something from the chamber measures and relating this in some way to the raw data from the armband (turns 11—13) in order to compare (turn 13). There is an additional amount re-animated as a vertical displacement (turns 13—15). At this point the narrative becomes more difficult for me to follow. Nevertheless, Brian, by agreeing in turns 12 and 14, seems to follow wherever Lev is leading. It is possible to interpret Lev's narrative assembly as either confirming Steve's point of view or Brian's. It is also possible that there is no intelligible confirmation of anyone's viewpoint. Lev does not seem to be quite clear on his own message, as he immediately turns to Sally to ask whether the raw data he had just assembled into his narrative can legitimately be assembled into it after all (turns 15—17). In this sense, his narrative assembly is most intelligible as a means of thinking out loud, as the expression goes, rather than as an attempt to recruit other people into perceiving as he does.

For what it is worth, it seems to me that he is trying to describe energy expenditure above the resting metabolic rate for both the armband and for the chamber, using data that he might be able to obtain or calculate from the armband and data he has access to from the chamber. If so, then both the independent variable and the dependent variable in the model of "a different story" would be energy-expenditure-above-resting-metabolic-rate. Whether in this interpretation Lev's narrative assembly confirms Steve's point of view or Brian's is unclear. Brian seems to act as if Lev's narrative assembly confirms his own point of view, but Lev almost certainly did not believe Steve's model was so radically wrong. The implications for his own future research and for the prospects of Sally getting her paper published were too great for him to defer to Steve so blindly against his own intuitions. After all, we can observe that Lev is not automatically deferential to Steve. After all, he is able to keep the floor while Steve attempts take it from him (turns 9—11).

In later commentary we will return to this episode for a more complete discussion. Briefly, we can note that Steve and Lev have represented scenarios in narrative forms that, by their nature, are more ephemeral and mutable than their textual counterparts. Steve recited the mechanics of substituting values into an equation and of performing calculations, but he did so without anchoring his narrative to a textual form. Thus he invoked a common representational form without expressing it in its most common instantiation as a written equation. Lev narrated a more elaborate integration of laboratory objects for the purpose of assembling a plausible account of how armbands function. Neither the narrated equation nor the narrated assembly of artifacts exhibited affordances anything like those of the graphs put to use so far. The people assembled could not easily collaboratively witness and discuss the structure of these narrated representations. Steve did produce one textual form relevant to this episode, the linear regression table (and he has produced another one that we are about to see). But this textual form has less structure to be exploited for discursive analysis than the graphs. It does not seem that the parties found this table to be very relevant to their dispute, as they by and large ignore it.

Steve then immediately announces the results for including the constant (you can read the text of his talk and look over the facsimile of table in Figure 15, below). He animates a reading of an equation as he points to named labels and values on the table in a fashion similar to his narration in Figure 13, above. Steve suggests that having a negative constant is too weird, presumably because it invites us to contemplate situations where the predicted energy might be (absurdly) negative. As Steve neared the end of his explanation, Lev co-completed his thought about this absurdity, suggesting some understanding of, agreement with, or deference to, Steve's approach.

Steve: (Standing at the screen) So, if you stick the constant in there, you get a slightly different formula. It's negative two point one seven (points to "-2.171") plus whatever value you get here (points to "sensewear exercise"), which is downweighted a little (points to ".792"), plus whatever the weight is (points to "wt in kg") times this (points to ".057"). I just thought that was a little weird (walks away from screen to face the audience) because, I mean, now you have a formula with a negative, uhm,

Lev: constant

Steve: constant in it. (Screenshot depicts the scene at this point in time.)

Coefficients a

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	-2.171	.589		-3.685	.002
	sensewear exercise	.792	.081	.739	9.816	.000
	wt in kg	.057	.011	.387	5.139	.000

a Dependent Variable: chamber exercise

Figure 15: Steve's recitation and facsimile of table with the constant (full model)

You will recall that Brian expected the armband to report zero energy while the chamber reports lots of energy. Steve's characterization of weirdness within the with-constant table appears by extension to characterize Brian's perspective as equally weird. Brian asserted that a state of affairs in the laboratory would confirm the appropriateness of including the constant in the model and implied that the chamber would report energy while the armband did not. Because Brian raised his objections in the context of a dispute about the model, acceding to his demands puts the statistical model in the position of speaking for Brian. As a result, the model puts Brian in the position of essentially claiming that while resting the chamber will report you as using up energy at a negative rate (if you impute zero values for the variables into the model in the manner Steve had described). Except for characterizing the situation as weird, Steve does not dwell on this. It is hard to know for sure whether or not Steve was just being coy about making too much about the contradiction between what Brian asserted or about what the model said on his behalf.

As for Lev's statistical argument, we can read the full model (with the constant) as providing support for Lev's (implied) position that including the constant proves to be better on statistical grounds. In comparing the two tables, you will notice that the full model has smaller standard errors, higher t statistics and equivalent or lower significance values. Interestingly, the researchers do not explicitly mention these comparative details. We are left to wonder whether they noticed them. As we shall see in the next short quotation, the parties do not appear to regard the inclusion of a constant as decisively refuted or its exclusion as decisively validated. At (25:30) they all come a kind of agreement, to agree to disagree as it were, as indicated by the following coproduced statement:

Steve:	To truly test this, you'd probably need additional data …
Lev:	additional data to look at how they actually could do it. Use the data from other children to see which …
Brian:	which is better, yeah.
Lev:	which is better.

Figure 16: Short transcript of researchers agreeing to disagree

In this last exchange, the researchers collectively positioned the present sample of data as insufficient to make any performances upon which the knowledge production of the better model would be contingent. Here is a synoptic graphic organizer for this last episode.

<table>
<tr><td colspan="5">⇒ Coordination ⇒</td><td rowspan="5">⇒</td><td rowspan="5">Revised resources: Two ways of generating tables. One has no constant, the other does.

Knowledge produced among the assembled: contested or doubted. Steve avers an outcome but there is no consensus or clear support voiced for his position.</td></tr>
<tr><td colspan="5">Available infrastructure includes Lev's narrative assembly of devices, baseline metabolic rates, manufacturer's machinations, raw data and what he might do with it if it were available, also revised table showing negative constant.</td></tr>
<tr><td>For Lev, movement gives you energy measure above a baseline metabolic rate; armband does not give raw data, or does it? For Steve, the exclusion of the constant is contingent upon its sign being weirdly negative.</td><td>⇒</td><td>Preservation of necessity that in this case, for Steve at least, exclusion of the constant is more logical, not weird, and cleaner. Maybe for Lev including the constant is statistically superior.</td><td>⇒</td><td>No promotion of generality. The parties appear to agree only that the validity of neither of the two models can be assured.</td></tr>
<tr><td colspan="5">Internalized structures accommodate the following of talk and gesture in Lev's narrative assembly.</td></tr>
<tr><td colspan="5">⇒ Coordination ⇒</td></tr>
</table>

Figure 17: Synopsis for Unfolding Data into Measures

(27:00) Steve seemed eager to move on. He immediately began to talk about the poster he had prepared for Sally to present at an upcoming conference. After discussing Sally's poster he gave them "pop quizzes" on "the scientific method" and shared advice for writing submissions to journals.

Achieving Agreement to Disagree

These episodes have largely been about agreeing or disagreeing in one way or another. The devices were first depicted as ostensibly agreeing (using correlations naively) yet possibly disagreeing (viewing correlations skeptically). The devices were then folded into new variables that allowed for comparison in an inverted sense. This inverted comparison amplified any disagreement, however small, and allowed for inspection of the structure of disagreement (Bland Altman scatterplot). Then the data were subject to new questions about the structure of this disagreement resulting in the identification of weight of child as a contributor (bivariate correlations) followed by inspection of the structure of weight's contribution (scatterplot). Finally the armband and weight were folded into a new model that took disagreement for granted, but mitigated it in the form of a linear regression whose algorithms minimize disagreement overall, by design.

It is hard to avoid the pun that at the end, the mixture of agreement and disagreement among the assembled is as richly nuanced as it is for devices in laboratory. As mentioned at the outset, the term used in this chapter, "achieving agreement," is a reference to Michael Lynch's ethnomethodological study of a psychobiological laboratory (1985). It is informative now to briefly discuss this term in more detail. Lynch contrasts "achieved agreement" with "implied agreement," the sort of agreement that might be inferred by means of surveys, for example, that involve little or no collaborative efforts directly between the parties being said to agree. Achieved agreement can only be evaluated by means of an "immanent analytic" grounded upon accountable features of talk among participants. It is a local achievement that is "provided *via* the asserted relevance of one statement or activity to another in the actual setting of its production" (p. 188, italics in original). It may be independent of the criteria particular parties use for asserting the correctness of statements, as people may collectively agree to things with which they privately disagree. Achieved agreement can be summarized as, "identical with its production and recognition by interacting parties" (p. 189).

By these standards, the collectives here achieve two agreements that are clearly different in kind. One the one hand, the parties agree that they need more data, which is to say that neither model is known to depict the true state of affairs better than the other. On the other hand they immediately discuss an upcoming presentation in which one of the models will be presented as depicting the known true state of affairs. These two depictions appear to contradict. How do the parties reconcile them? Or, in what sense are these contradictory agreements assimilated? Since these agreements or disagreements have to do with knowledge claims, questions of agreement are tantamount to questions of what is true about states of affairs in the world at large, that is, about knowledge. The apparent contradiction is resolved, or assimilated, not through the elaboration of an argument effected upon a page, through computer code or by means of some representational form. Rather, it appears to be resolved within the organization of expertise, authority and self-interest of the people in the meeting.

In this section we discuss the local production of the agreement to disagree. In the next section I speculate somewhat on an agreement to the generality of restricted model at a much larger collective level. In order to consider this local production of agreement to disagree, let us refer again to the short conversation quoted at the end of last episode.

Steve:	To truly test this, you'd probably need additional data …
Lev:	additional data to look at how they actually could do it. Use the data from other children to see which …
Brian:	which is better, yeah.
Lev:	which is better.

Figure 18: Portion of transcript as researchers agree to disagree, repeated

What Steve offers to test, you will recall, is the advantage of choosing his restricted model (without a constant) over Brian and Lev's suggestion to choose a full model (with a constant). The researchers agree in the sense that they collectively concur that only the performance of more children, weights and armbands could settle the dispute—they would need more data. It seems that they nevertheless continue to disagree about which model they think will win out once these later data are assembled. It is not clear which side Lev stood on at this point. Above he articulates a position that the question as to which model is better is still an open question. But we do not know whether or not Steve's explanation has convinced him that the restricted model is more logical or cleaner. It does not seem that the "weird" negative constant produced in the full model was germane to his concerns for following conventional statistical procedure. And as we saw, the full model appears to show superior statistical values, but it is unclear whether the participants took the effort to make this comparison. Brian, for his part, has given no indication that he sees his position differently after the full model was made. As observed above, Steve does not appear to impress the point that the full model disconfirms Brian's point of view and Brian may not have followed the weirdness of the negative constant to its logical conclusion. So, except for Steve, there is some uncertainty as to how the other parties are leaning with respect to which model they would prefer. But we do know that they concur that despite what one might believe personally about the situation, they cannot collectively construct an argument that would be convincing to everyone. It is in this sense that they agree to disagree about two interpretations they take as incompatible.

In Lynch's terms, whether some people in attendance might declare otherwise in other settings, is not germane to the question of agreement in the local scene just described. In a follow-up interview, Steve insisted that he still considered the inclusion of a negative constant to be weird. This later view is consistent with his view before this agreement to disagree. His view then and later was that the present data were in fact sufficient for excluding the constant. His argument was informed by a sense of what is logical when considering technological devices and biological systems together and by an aesthetic sense of cleanliness of technique.

We have a situation where a comparison of tables clearly shows that on statistical grounds, including a negative constant makes more sense. Given Steve's concerted efforts throughout this meeting to discipline the perception of the scientists to notice statistical evidence in tables, it is not plausible that Steve ignores the stronger statistical evidence that is manifestly presented in the full model. He even implies that he had produced this very table before the meeting (see Figure 15), which would suggest that he thought long and hard about the full model, but rejected it on grounds that were not statistical. Steve describes the restricted model as "cleaner" (21:30). It would seem that he holds the restricted model as good enough statistically while being decisively simpler. Having yet more data that reveals more strongly the same preponderance of statistical evidence will not undo its quality of being weird.

We should then ask ourselves what this agreement to disagree achieves? Brian does not have much interest in a professional sense one way or the other. If his standing as a knowledgeable scientist is important to him, then agreeing to disagree may satisfy his ego at least, as he has demonstrated that his point of view can be taken on a par with other's. As for Lev, his interests are more complicatedly tied up with the collaborative group that is conducting the study. This group is the subject of the next section. Steve, for his part, has a full agenda of things to present. He immediately changes the subject and moves on to the next topic. So for him, agreeing to disagree appears to close an argument that he does not need to win. Steve is not concerned with winning Brian over to his side. His main concern is with keeping his collaborators on board and with sending Sally off to the conference with something to present.

What should we make of the agreement of the audience for the need to seek more data? Clearly, the researchers did not wait for more data, but rather immediately plunged ahead as if they had never even discussed getting more. This situation is only intelligible if the audience shares a perspective that either model is likely to be good enough in the sense that it is likely to be better than the state of the art situation.

Achieving Agreement of Generality

It should be noted that Steve's interaction with the others is usually more like a lecture than a collaborative meeting. Because he has already completed the analysis, he is able to run through explanations quickly. He has already collaborated with Sally and Lev and we are witness to an advanced stage of their collaboration. Immediately upon agreeing to disagree, Steve begins talking about graphics that Sally will soon use in a presentation. These graphics display energy expenditure using the restricted model. The assembled appear to take this in stride, as if it were sensible to continue on as if, in fact, there already were sufficient data to use the restricted model. There are a few possible reasons for this. One, the collective of Sally, Lev and Steve has the most intimate knowledge of the devices and of the statistics. Others might care about getting the model right, but do not have the resources to intervene even if they think that a full model were better. Furthermore, only Sally, Lev and Steve have much direct interest in the outcome. Only they will suffer consequences if their paper is rejected or if their model applied later to future studies of children are erroneous. The silence of the parties who have less expertise and have no special interest in the outcome requires no other special explanation.

But what about the silence of Sally and Lev? One might think this lack of urgency for unfolding the model adequately so as to resolve the dispute indicates that the most invested parties, Sally, Lev and Steve, had already settled on this model, understood it, and were already comfortable with it. However, Lev himself had asked for clarification of the rationale for deselecting the constant. So, it is more plausible that Lev and Sally deferred to Steve on this matter even if they did not completely understand his point of view.

Within this milieu of partial understandings and deferrals, Steve, Sally and Lev collectively agree that the cleaner, restricted (without constant) model is preferable to the statistically stronger full model (with constant). They agree in the sense that they let it continue to be used in the meeting, to present it at a conference as a valid expression of states of affairs in the laboratory and for other laboratories. It seems to be good enough for their purposes. What are their purposes? They want better ways of measuring energy expenditure among children. They appear to believe that the model they have produced may well be much better than the previous state of the art technology of using the armband alone. It was also “cleaner” than the alternative. By this Steve appears to mean that the restricted model only requires two terms (each with a coefficient) whereas the full model requires three (two with coefficients and one constant). In this sense, “cleaner” means to have fewer terms in the formula.

They also want credit for publishing. As it turns out, Sally’s paper eventually was accepted and published in refereed journal. The paper included some of the same graphs you have seen here. As previously mentioned, it also included an equation for predicting the energy expenditure of a child during exercise using the same coefficients as appear in the restricted model of Figure 13 above.

The researchers, Sally and Lev, together with a statistician, collectively interacted with a journal. The mode of interaction with this journal was quite unlike the mode of interaction reported so far in the meeting. Presumably, it entailed a certain amount of correspondence electronically, for instance. By the standards of what counts for knowledge and for what counts for credit, this collective was successful at recruiting objects to advance their preferred, restricted model. They were able to convince referees to accept their version as necessary about these children, these children’s weights and these armbands. They were able to convince them that this specific situation spoke generally about similar armbands dedicated to similar studies of children. In this sense, the collaborators were able to achieve generality by the standards of their profession.

By speaking of necessity and generality this way, I stray from Netz's original account somewhat. Deductive progress for Netz follows necessarily from one statement to the next with seldom opportunities to unmake the progress. In other words, other objects or performances cannot easily undermine or reverse the geometer's statements. Generality likewise is the jump from specific actions to similar actions different in only incidental ways. Here to some extent what gets presented as necessary relies upon personal judgment. One has to be trained to see things in Steve's way in order to form an opinion as to how lesser weights mess up the armband's accuracy or to effect a tradeoff between statistical strength and model-cleanliness. We should keep in mind that the professional prerogative to state what is relevant can be contested in practice or contested in principle by an analyst of that practice (Goodwin, 1994). Note that though Lev brings up some questions, he does not intervene to demand, for example, that Steve use his expertise to compare models. Nor does he ask Sally to put off her presentation so that they might have more time to collect data from children. In the end Lev's name appears as a coauthor on the published paper, an explicit statement of his consent to the restricted model.

I use the term, necessity, in a pragmatic way because I want to underscore the appeal here to principles of logic. There is more going on here than researchers merely reaching a state of achieved agreement as might occur in non-scientific settings, such as a family agreeing where to go out and eat. In this practice participants clearly frame their arguments, and agreements in logical, deductive terms and hold each other accountable to do so. Steve justified the restricted model in terms that he specifically claimed were more "logical" (21:30). Even Steve's aesthetic sense of what is "cleaner" can be seen a logically pragmatic choice to make things simpler. In later episodes we see that Steve's interpretations are contested. But they are contested on grounds of logic, in keeping with my asserted sense here that logical persuasion is the primary legitimate form of argument. The collaborators do not have to comprehensively follow the logic of each and every contributor to the collaboration in order for the collective, in a practical sense, to have achieved a conclusion as a uniquely necessary one. In this sense, the three collaborators preserved the necessity that energy expenditure be modeled without a constant and promoted the generality that similar devices strapped on to similar children can be productively modeled in the same way.

The achievement of agreement on generality within this collaborative group appears to be mediated by the prerogative Steve enjoys to choose a model. Steve's prerogative is maintained in part by Lev and Sally who defer to him. We can only speculate as to the means of achievement of agreement of generality between this group and reviewers of the journal. However, it is clear that at a collective level of communication, the collaborators, together with the journal did in fact publish a general testament to the relevance of the restricted model to states of affairs involving weights, armbands and predictions of energy expenditures.

Recruiting Objects so as to Produce Knowledge

In this and in the sections that follow we return to the questions posed at the outset of this chapter. This section concerns itself with both questions, while focusing upon the production of knowledge. Again, the questions are: (1) *How do objects typically perform or how are they typically made to perform in cognitive, epistemic demonstrations?* (2) *How are people and objects recruited into the infrastructure of demonstrations so as to make things hold together as new knowledge?* The details of the episodes above should make it clear that researchers routinely animate armbands, weights and other objects in their attempt to make their arguments accountable to how objects behave independently as to how they behave. In most of the episodes, some object or thing is recruited to perform so as to tell the researchers what to do next or to tell researchers about some state of affairs in the world of children, weights and armbands. I think there might be a bias, or an urge, to equate the mundane and the routine with the unimportant. I want to suggest the opposite. We have witnessed common means that are essential and fundamental to producing knowledge. We witness here how people assemble graphs, tables, computers, a shared table, a projector, screen, data from some colleagues, expertise and software to forge new knowledge relations or to contest knowledge relations in development. If we want to understand the cognitive practices such as these as epistemic practices as well, we have to attend to the means people employ to deploy the agency of objects. Looking across the episodes, we can comment more broadly about the primary question of this investigation: *How are objects recruited into scientific demonstrations so as to make things hold together as new knowledge?*

First of all, the armbands, chamber, children, children's weights, body fat and energy expenditure must be recruited through the mediating work of the statistical software, its data view, its many GUI's, tables and graphs, but also through the mediating work of Steve and some of the scientists. Stating the obvious perhaps, the infrastructure together with embodied interactions, allow for the animations and contingent performances discussed above to happen.

Second, the objects have been recruited for a purpose. In the synoptic graphical organizers I have listed a few of the kinds of internalized structures and infrastructure relevant to each episode. Knowledge production, if any, depends upon the performances of the recruited objects. They are the result of a whole process and may become undone if part of that process is contested or fails. For instance, Brian attempts to undo a process of modeling that leaves out a constant. We can imagine that if he can be persuasive, then Steve's work will have to be redone, not because Brian has authority to boss Steve, but because he will have succeeded in undermining the combined agency of the armband, weight and this restricted model to predict energy expenditure.

Third, objects are sometimes recruited to fold into new models. Early on (at 3:00), Lev tried to open a conversation about the distinction between measuring and predicting. He was trying to explain that the chamber measures gas concentration and then processes these measures through algorithms that calculate or "predict" a value for energy expenditure. Steve, intentionally or naively, was using terminology that folded the various devices of the chamber into one measurement. Lev was arguing to keep them unfolded somewhat in order to keep a view of the chamber as comprised of embedded devices that measure gas intake and outtake. As the meeting continued under Steve's direction, more folding ensued. The armband and the chamber were folded into new calculations of device difference and device average. These new computations were then folded into an analysis of disagreement through the Bland Altman technique. Later the weight and the armband were folded into a model that predicted energy expenditure (the precise form of that model remained contested at the end). Weight was a characteristic that ostensibly would not figure in directly with energy expenditure. Yet here it is influencing what energy we think a child might expend. Conversely, the armband was ostensibly an adequate report of energy expenditure on its own, but now it is only one of two players in a prediction.

It should be mentioned that in the context of the phenomenon of folding predictions into new ones, the distinction that Lev makes early on between a measure and a prediction takes on a new possibility of meaning that is perhaps ripe for practitioners in this practice to appreciate more generally. The phenomenon I have in mind is the indeterminate regress of measures. We find that in practice all measures come down to material apparatus and embodied modes of observing (Stengers, 1999). If the regression is not infinite, it is certainly long. Lev insists that there are things called measures different in kind from devices that merely predict using algorithms from these measures. But what are we to make of the apparatus that "measures" oxygen concentration? It is certainly comprised of components some of which interact with oxygen molecules and some of which amplify or display the signal provided by that component. So does it measure the true oxygen content or does it predict the oxygen content from a signal? And what do we make of the construction of that very component or what do we make of the vast infrastructure necessary to calibrate any such devices so that the signals generated in one device can be made commensurate to signals generated in others? The physicists Stengers encountered found themselves dwelling incessantly upon such questions. It may be that Lev and Sally are having to dwell upon this regression more often or more profoundly, which might explain why Lev feels compelled early on to lecture Steve and others about the distinction between a measure and a prediction. We can only speculate as to the extent that this focus on this distinction might open up Lev or Sally to appreciate the recursive notion of unfolding in manners similar to the experience of physicists.

Fourth, sometimes objects cannot be recruited. This at least is one way to interpret the controversy that goes unresolved at the end. I identify three representational forms in the "Unfolding Data into Measures" episode. The first form is the table of coefficients of which there are two instantiations (the restricted model, that is, without a constant, and the full model, that is, with a constant). The second is Steve's recitation of the equation for energy expenditure as read from these tables, as in the Figure 13 above for the restricted model and in the Figure 15 above for the full model. The third is Lev's narrative assembly in the transcript for Unfolding Measures into Data in Appendix A. Steve, Brian or Lev were each in his own way trying to unfold the devices in this model back to some hypothetical situation that would validate their point of view. Steve described a hypothetical, and for him *illogical* situation where the armband reports no energy while the chamber reports some energy. For Brian, this very scenario was *logical*.

What did Steve have available to recruit his illogically disagreeing devices? The table would not do, as it had only coefficients and other statistics, nothing on it to follow the armband measure to a zero reading and to coordinate that reading with a chamber measure. His recited equation might have been persuasive, but it is hard to persuade with an equation available to the assembled as a remembrance: nowhere was this equation inscribed. Recall the elaborate gestures, the visual and tactile work Steve performed to help people coordinate relationships between variables on graphs, including coordinations with zero as a salient value. Recall that points for a linear regression running two independent variables could only be graphed if charted along a two-dimensional plane in three-dimension space, a format not available in the software they were using. Steve could not recruit the necessary objects with these representational forms to make people see patterns in spatial terms analogous to his exposition of features of the scatterplots in earlier episodes.

What did Brian have available to recruit his logically disagreeing devices? He had some knowledge of metabolism and asserted it. But he needed more than bluster to convince this assembly. He turned to Lev as an expert-resource. Lev complied to the extent that he supplied an explanation based upon his knowledge, but it is unclear what kind of situation he was trying to recruit his objects for. As mentioned, it is possible that Lev himself did not know. In our earlier work we have found that narrative assemblies can adequately recruit objects for the purposes of persuading or reaching consensus (Hall, Wright & Wieckert, 2007). Here, however, the narrative assembly was not adequate to the task. This (negative) example illustrates the limits of representational forms in doing epistemic work. Without an adequate forum, it is difficult to achieve consensus.

Was there, is there, an adequate forum? I want to avoid a normative analysis that goes beyond what people did here to impute a judgment about what they should have done. Instead, I want to situate what people could have done in terms of what they had been doing already. The entire meeting thus far has been concerned with multiple modes and means for making comparisons. Steve is clever about calculating, folding and displaying to get people to see how to see structure within comparisons. Yet here, despite the resources available to him, he does not try to explore the structure of disagreement between a without-constant equation to a with-constant equation. He has recited the equation for both models. So he could certainly define variables in the statistical software to calculate with these equations. Presumably, some form of comparison could be at least tried. So the decision to stop the modeling process of comparison here can be interpreted as a deliberate one that has nothing to do with the unavailability of resources. Objects *could* be recruited in this case, but the deliberate choice is not to bother.

The questions left begging are, Why does Steve avoid comparing the models and, Why do the assembled let him get away with it? These questions cannot be answered by the particular organization of software. They can only be answered in terms of the interpersonal organization of the assembled.

Learning as Adaptation to Complex Systems of Modeling

This section mainly concerns itself with the second question only: (2) How are people and objects recruited into the infrastructure of demonstrations so as to make things hold together as new knowledge? Here we see that they are recruited to adapt to the infrastructure of measures, data and software. There are a few things to notice here about the participants' adaptation to a complex system or at least about what becomes available to them to take up if they have an interest. Steve enacts a version of modeling. There are certain choices that may be made within a menu of options. There are decisions a user may make, and others one may not make within the structure of this software. It seems that Steve's ongoing task is to discipline his collaborating scientists to choose the appropriate pathway of decisions within the software. To do so requires frequent assessments of states of affairs of a laboratory setting further back along the chain of circulating reference (Latour, 1999) as revealed in the output of the software. He teaches his clients to recognize these states of affairs and to make further decisions as necessary. The analysis he portrays here is one extended decision tree comprising both the menu-structure of the software with decisions enacted by humans disciplined to organize their own analyses similarly.

Even from this brief account, we may characterize the coordination of embodied, internalized structures and infrastructure in historical terms. From a perspective on the embodied side of the interface, it seems that the delegation of assigned work (scientists doing much routine tabular and graphical analysis with Steve in the role of instructor) organizes statistical analysis around a menu-driven decision tree structure. That is, the enormity of the task of doing analyses within this institution directs Steve to promote this particular software product. To use this product within a regime of delegated work requires that scientists internalize novel, embodied modalities for interacting and perceiving. Symmetrically, from the perspective of the infrastructural side, we could say that the menu driven software facilitates a delegation of work for doing routine analyses better than its competition. It makes such delegation more appealing and feasible. As it extends its reach, its proprietors acquire more wealth and, perhaps, reinvest so as to make the software even more appealing and insinuating. In this way SPSS® insinuates itself more deeply into the interface between embodied, internalized structures and infrastructure in research institutions around the world.

As mentioned in the episodes, Steve frequently directs the attention of scientists to critical values within graphs or to markers and values within tables. These various attempts to discipline the perception of scientists can be seen as a cross-section of ontogenesis. We witness an attempt to make the scientists more sophisticated interactors with graphs, to recognize things within them that would otherwise be unremarkable. The purpose for noticing these things within graphs is to then make claims about knowledge. So he is not merely teaching his scientists to read graphs; he is teaching them to read them in an epistemic manner, to enact within them versions of how objects in the laboratory interrelate not just in this particular instance (as we preserve necessity) but also in other similar instances (as we promote generality). Steve also disciplines his scientists to think strategically about building knowledge from one analysis to the next. It is not as if one runs a single statistical test and then sets about writing it up. We saw that the process of making the publishable, predictive model required several stages of analysis, several foldings of data into new configurations.

Learning as Participant

This section continues with some discussion relevant to the second question: (2) How are people and objects recruited into the infrastructure of demonstrations so as to make things hold together as new knowledge? Here we consider and speculate as to some of the tacit lessons that one might draw as a legitimate participant in activity. We learn early on that journals can publish models that are wrong, as illustrated by the misuse of correlation. There is a right way to do the particular modeling of device agreement, which is to invert the comparative logic, to model the structure of disagreement. In this instance, it requires a statistician to notice and inform the scientist that an ostensibly legitimate practice is actually not. So, the first tacit lesson for the scientist is that current statistical practices, even those appearing in refereed journals, need to be critically examined by a collaborating statistician. This lesson is tacit in the sense that Steve never says this explicitly, yet those in attendance can only wonder about the implications of an established method in the literature being disallowed by a collaborating statistician. Scientists here have to consider the reality that in order to recruit the cooperation of the statistician, they may have to trust this statisticians judgment about existing statistical practices within one's discipline. Steve for his part finds that when encroaching upon scientific knowledge, his views may be contested. For example, Lev contests his understanding of the epistemic role of devices: for him they only predict or calculate, they do not measure. Brian contests his understanding of metabolism.

There is a second, pragmatic lesson here about knowledge claims within an epistemic, cognitive practice. They can be good enough. Exhaustive study as to which model is best is not necessary as long as we can arrive upon one that serves one's purposes. As mentioned before, statistically based claims are made to be explicitly uncertain. The uncertainty is modeled mathematically and therefore delimited in perhaps the most sophisticated possible way. But here we have a sense of uncertainty or doubt that could be more challenging to accept. There is no escaping a relativistic sense of knowledge as contingent and uncertain within this practice. Because the participants on the one hand agree they need more data to determine which model is better, yet proceed immediately to use one of the models; they enact in practice a pragmatic and relativistic approach to the production of knowledge. They cannot maintain a positivistic sense that the restricted model is to be preferred because it is the one and true model to apply. In the end, what should we make of the question posed at the beginning as to whom we should believe, those assembled or the smaller collective of the three collaborators? The researchers appear to provide the following answer: believe either one or both. We cannot know which model is better but the one model we have already invested in will be good enough.

Third, and perhaps most important, is that what can be claimed as knowledge is emergent from what the weight, armbands and chamber do, as revealed through the infrastructure of spreadsheets, data files, statistical software and expertise of participants. Because Steve presents a version of events retrospectively, we have to infer from it the prospective sense of surprise that accompanies the practice of running it through the first time. In this sense we do not get a complete picture here of the researchers doing the work of discovering. That said, we still can read the performance of the many nonhuman actors into the narrative that Steve tells.

According to the metaphysics of truth (see Chapter I), weights, armbands and the chamber inhabit a prior state of affairs that become revealed in the process of making measurements and doing statistical analyses. We usually find this metaphysical stance compelling. We rely on it every time that we read recommendations for using a prescription drug or every time we read an announcement of a new astronomical event such as new pulsar (see Chapter II). You could read Sally and Lev's article that way. But this metaphysical stance is not as compelling when it come to the epistemic, cognitive practice of doing the work of discovering. As Steve does the work of discovering how weights, armbands and the chamber relate, there is no state of affairs upon which he can rely to tell him what that relation might be. Instead, he enters a metaphysical world wherein the software tells him things, it reveals things about some state of affairs that had not been determinant until that very moment that the telling was made. In other words, Steve and his scientist colleagues routinely enter into a metaphysics of relations every time they take upon the epistemic, cognitive task of doing a demonstration. If this is right, then to become proficient as a scientist is to learn how to read such performances into or through the mediation of the representational form, taking on a metaphysical perspective of relations in order to do so.

In the professional setting observed here, we have observed participants as they contended with scientific demonstrations, with a complex relation between making agreements and advancing knowledge claims and with several examples of representations being recruited into sometimes elaborate demonstrations of activity of objects in the laboratory. In the next chapter we will again observe elaborate demonstrations occurring over the tableau of a representation, but within a more mathematically oriented setting.

CHAPTER IV

NECESSITY AND GENERALITY

Introduction

The primary questions of this investigation as described in the introductory chapter are to explore the following questions. (1) *How do objects typically perform or how are they typically made to perform in cognitive, epistemic demonstrations?* And, (2) *How are people and objects recruited into the infrastructure of demonstrations so as to make things hold together as new knowledge?* As with chapter III, the meeting is divided into episodes. A synoptic graphic organizer summarizes the episode in terms of performance, argumentative form, recruitment and knowledge production. Thus, aspects of performance and knowledge production are continuously and systematically reviewed and made salient.

In this chapter we investigate a mathematical demonstration, the knowledge produced within it and learning as a process of mutual adaptation of internal structures within persons and representational infrastructure. We will continue to explore the epistopics of representations and demonstrations. We will investigate the specific epistopics of necessity and generality in greater detail than in the other empirical chapters. The chapter is organized as follows. First I describe the available infrastructure for the participants' model for assessing the "burden" of influenza. Then I discuss a series of episodes within the meeting. Each episode considers performances of objects as well as descriptions of necessity and generality and summarizes these terms with a synoptic graphic organizer. After considering these episodes in sequence I will review knowledge production and learning across episodes in a series of additional sections.

People and Infrastructure

The setting is research on influenza prevalence within a county local to the university medical school that houses the scientists and statisticians appearing in this investigation. The man depicted to the right below (Ted, a statistician) sought to convince the man on the left (Alberto, scientist) that a calculation along one line of argument confirmed a result explainable within a second line of argument. Alberto was interested in finding a better method for estimating the number of children infected with influenza. He had been put into contact with Ted by the primary investigator of his research project. She had heard that Ted had completed his PhD on methods for analyzing Capture-Recapture data. The Capture-Recapture method derives from wildlife management but has spread over the years into other fields, including epidemiology.

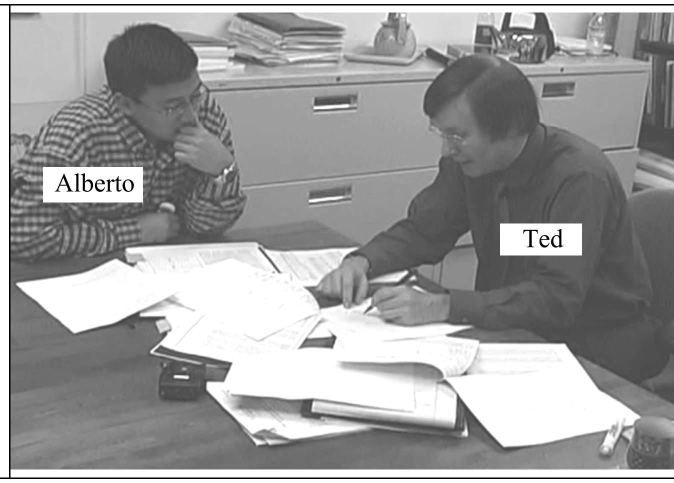
Alberto (scientist) and Ted (statistician) explore a special case for the use of a formula to estimate the number of children having influenza in the local area.	

Figure 19: Researchers exploring the prevalence of influenza

Alberto had two systems for counting children with influenza. He hoped to combine them. The first was a system that tested hospital patients at intake. It operated four days a week. The other system operated essentially all day every day in the sense that it consisted of retrospective reviews of patient records. As Alberto explained in the meeting, the two systems operated independently in the sense that the people running one system did not communicate with people running the other. Ted then explained that even if operators are blinded, the two surveys might still depend on each other in some unforeseen ways. Ted pragmatically suggested that Alberto argue for and assert a stronger, statistical sense of independence when writing up his study.

The researchers believed that both systems missed some children. Could a statistical method be used to combine the information from both systems to provide an estimate better than simply consolidating the lists? Alberto hoped the answer was yes. He had read other papers in epidemiology and had reviewed equations and tabular formats for organizing sample counts. Others had combined lists to obtain estimates of prevalence of disease. He had tried to work out some estimates with the same techniques. However, he ran into a difficulty of a kind that earlier publications had not documented. Unlike earlier situations documented in the literature, the two lists he obtained were generated by systems that clearly operated sometimes concurrently, sometimes not. Alberto thought he might be required to restrict his lists to children enrolled in either system during times of concurrence only. He identified this as a problem because he would have smaller sample sizes (bad for statistical inference-making) and he would have to work meticulously through the lists in order to match up concurrent times (logistically impractical). He raised these concerns with Ted.

Before we get to Ted's explanation, let us first consider some of the infrastructure of the Capture-Recapture technique so that we have a better grasp of the topic, to at least catch up a bit on the researchers who were already fairly well versed in these conventional technical forms. Being familiar with the existing infrastructure will also provide a basis of comparison as later we explore the modifications in this infrastructure introduced by Ted. A version of the table Alberto presented in his eventual publication in a refereed journal appears in the figure below. This has been adapted here. The actual numbers appearing in his paper for counts of children with influenza are used. Similar tables appear in several of the publications using the Capture-Recapture technique that Alberto and Ted brought to the meeting.

	Intake screen Enrolled	Intake screen Missed	Total
Hospital records Enrolled	11	23	34
Hospital records Missed	18	*m*	
Total	29		*N*

Figure 20: Facsimile of Capture-Recapture table

The figure above shows that 11 children were enrolled into both the intake screen and in reviews of hospital records. That is, combined lists of names and other specific identification information such as birth date revealed 11 names in common. 23 children appeared on the hospital records review list but not on the intake screen list. Conversely 18 children were did not appear on the hospital records review list but did appear on the intake screen list. The table also displays row totals (34 listed overall in hospital records review) and column totals (29 listed overall in the intake screen). Disconcertingly, since each list shows that the screen for the other list is missing people, there is presumably some number of children missed by both screens, *m*. If we do not know *m*, we cannot know *N*, the overall number of children infected with influenza. This value *N* may be modeled as the sum: 11 + 23 + 18 + *m*. But it may also be modeled as a factor in a proportion, which is the way these researchers discuss it.

The conventional, metaphorical illustration of this technique derives from wildlife management. These researchers had used this metaphor in the past. Both Ted and Alberto talk about this metaphor in interviews some time after this meeting. It is hard to avoid encountering it given the name of the technique. The diagram depicting this metaphor can be found in the figure below. It displays the same numbers as in the figure above. There is an initial state of undifferentiated fish in the wild (upper left). You capture 29 fish with your first net and mark them in some way (upper right). You now release them. The fish again swim randomly and intermingle but are now differentiated: you have introduced a ratio of marked fish into their habitat (lower left) and have essentially created a new state of affairs in the wild. If the technique is conducted well, the fish will not experience a change in the state of affairs of their world, but the scientists will in theirs. You do not know how big the true number of fish in population *N* is, but you do know that whatever it is, a ratio of 29 out of *N* are now marked. Next you capture 34 fish with your second net in order to measure that ratio (lower right). You notice that 11 of those fish had been marked. So this second capture identifies the ratio of 11 previously captured fish out of 34 generic fish in the population.

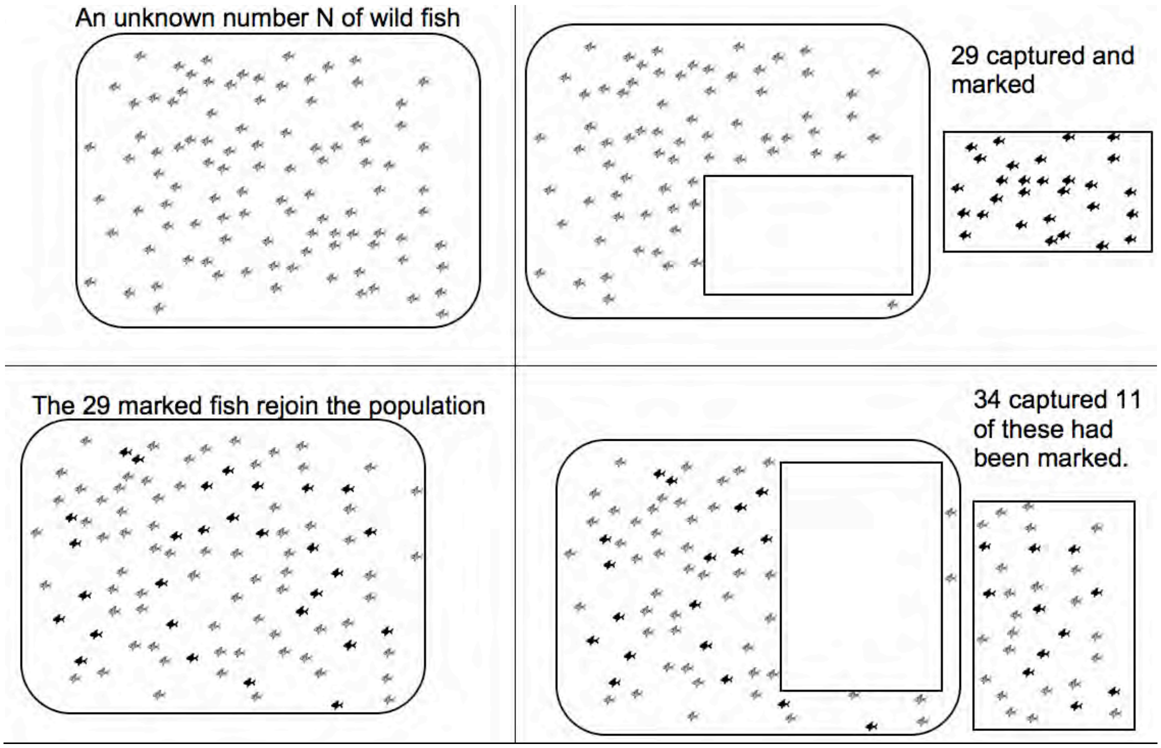


Figure 21: The Capture-Recapture model within wildlife management.

The indeterminate ratio and the new determinate one may now be equated by means of a proportion: 29/*N* = 11/34. By means of conventions of arithmetic, you may reconfigure the equation to look like this: *N* = 34*29/11. *N* calculates to be about 90. A version of this equation used by Ted in the demonstration can be found in the figure below. The equation comes from a text that Alberto brought to the meeting (Regal & Hook, 1984). On the left of the figure is a general table that appears on the same page as the equation. Thus the Capture-Recapture technique employs notational schemes that explicitly build correspondence between this table and the proportional equation. So this tabular structure and equation are related components of the conventional cognitive infrastructure of the technique into which these two researchers had become acclimated.

		Source B		
		Present	Absent	Total
Source A	Present	X_{11}	X_{12}	X_{1+}
	Absent	X_{21}	—	
	Total	X_{+1}		

$$\hat{N} = \frac{X_{+1}X_{1+}}{X_{11}}$$

Figure 22: Copy of table and equation from Regal & Hook, 1984, p. 288

The syntax of the terms in the table may seem abstruse. The leftmost subscript of the variable X tells you the state of presence for source A: "1" is for present, "2" is for absent and "+" is for either. The rightmost subscript is for source B. Thus, within this syntactical form, "X_{+1}" refers to the number that could be either present or absent in source A, but present in source B, which is a roundabout way of expressing the total number for source B. This feature of this table, its explicit place holding for source sums, will become a relevant aspect later when we compare this table to Ted's hybrid table produced here.

One other feature of the existing infrastructure bears mentioning in light of the hybrid table to be introduced by Ted. The epidemiological use of the technique differs from its use in wildlife management in that the designation of the capture screen and the recapture screen is arbitrary, unnecessary and maybe even unhelpful. Either one could be regarded as the capturing list if anyone cared to keep track. This arbitrariness is reflected in the symmetry inherent to both the table and equation. This sense of symmetry is not invoked in any explicit manner in the episodes to follow. I only mention it because it is an interesting and conspicuously available feature of the infrastructure and because it may help the reader sense this arbitrariness of designation. In Figure 20 either screen may be entered into column headings and either screen entered into the row headings with no consequence at all for the estimate *N* obtained. Put another way, either screen may be designated as "Source A" or "Source B" in Figure 22. Likewise, the equation computes the same if you merely switch the labels of "+" and "1" in Figure 22, as becomes evident by then invoking the multiplicative property of commutation as shown in the figure below:

$X_{+1}X_{1+}/X_{11}$	$X_{1+}X_{+1}/X_{11}$	$X_{+1}X_{1+}/X_{11}$
Original	Switch labels	Commute numerator terms

Figure 23: Symmetry of Capture-Recapture equation

Alberto knew how to do this kind of calculation and how to coordinate it with the Capture-Recapture table. But as mentioned, something seemed wrong or suspicious about enrolling children into one screen at a time when the other screen was clearly not operational. The lists for these other studies had been compiled from records kept over concurrent time periods. So whereas the two screens were necessarily sequential in wildlife management, they were generally concurrent in epidemiology. As mentioned, this difference in temporal structure did not prevent epidemiologists from appropriating the Capture-Recapture technique because the designation of the “capture” screen and the “recapture” screen was arbitrary. What made Alberto’s situation different, and it may have been the first such case in the literature, is that one of the lists clearly operated sometimes within and sometime outside of the time frame of the other, on a regular basis, week by week. Neither historical application of the technique appeared to exhibit this temporal structure. This mismatch made borrowing from existing infrastructural resources problematic.

Coming From the Science

The episodes to be explored below involve a mathematical demonstration whose purpose is to support a recommendation offered by Ted for Alberto’s scientific program. Ted advised Alberto not to worry about the temporal mismatching and to include all the hospital records into his analysis; “As far as the method’s concerned, I mean the, it doesn’t care about the fact that one method has a smaller probability of capture than the other.” He suggested that Alberto think of the intake screen as “making crosschecks” on the hospital records system in order to “estimate the imprecision of of the seven-day method.” In his explanation, it did not matter that the probability of “capture” by either screen was different from the other. Once you know the imprecision of the 7-day method, you can apply your knowledge of it across all the days, even on those days when the intake screen is dormant.

Entering the Mathematics

The precipitating event prompting the demonstration illustrated in the following episodes, was a conjecture Alberto then posed: if the intake screen were to operate for only one day, would the Capture-Recapture technique still apply? Ted assuredly said it would and commenced to direct Alberto and himself to concentrate on a piece of paper, in order to demonstrate the preservation of necessity along a deductive trail that would eventually confirm this conjecture. As Ted demonstrates, he refers to the same form of the equation illustrated above in Figure 22. The episodes are presented as a series of panels in Appendix B. The episodes relate the entire conversation over a period of about 8 minutes. To the left of each panel is a facsimile of Ted's inscriptions. This affords a progressive view of the page as visible to Ted and Alberto. Highlighted text within the facsimiles indicates those locations to which Ted points during the given panel utterances. At the middle of many panels are screenshots and to the right is text of speech. Other conventions for these transcripts are [brackets] for overlapping speech and the use of the abbreviation H&R for the Hook and Regal Equation of Figure 22.

Before looking at the episodes, it is worth considering some of the additional infrastructure recruited for this demonstration. The two of them are sitting side by side for ready access to the paper. Either can point at aspects on it or write on it, though Alberto sits a bit further away. Over previous days, months and years, they have internalized modes of embodied interaction with tables, metaphors of trapping animals and equations as discussed above. This experience was itself a resource. They were also accountable in this meeting in important ways that facilitated the organization of their interaction. Alberto was mostly accountable for providing information about his project and for attending to advice offered by Ted. Conversely Ted was accountable for listening and for given relevant, useful advise. Up to this point, they had been discussing the case for almost an hour. Alberto had talked a lot about his project. Ted had already instructed Alberto about the need to model and promote the two systems as independent. He cautioned not to rely upon a standardized distribution for drawing inferences (as others in wildlife management and epidemiology had done), but to use "profile likelihood-based" methods and nonstandardized distributions for determining the range of uncertainty in the estimate. Now that Alberto had raised a new problem, he took a listening role as Ted adopted a lecturing one. Alberto was being demonstrated to, but as we shall see, the demonstrator became more attuned to some of the unforeseen entailments of his own hybrid table as he talked.

The episodes to follow will explore the epistemic, cognitive practice of a mathematical demonstration. In order to trace the consequential embodied, discursive means by which the preservation of necessity is advanced, it becomes necessary to follow the talk and interaction in great detail. The transcripts will facilitate a detailed account of the sequential production of two lines of argument and of an eventual, emphatic merging of these lines of argument. In the course of the demonstration, Ted constructs a page containing an equation, a calculation and a table that is a modified or hybrid form of the tables appearing in

Figures 20 or 22 above. The screenshot in the figure below shows what this hybrid table looks like near the end of the demonstration. As suggested in the previous chapter, it is recommended that the reader keep a copy of the transcript beside the text of this chapter while reading it. It is also recommended that one read the transcript of a given episode before reading the analysis of that episode.

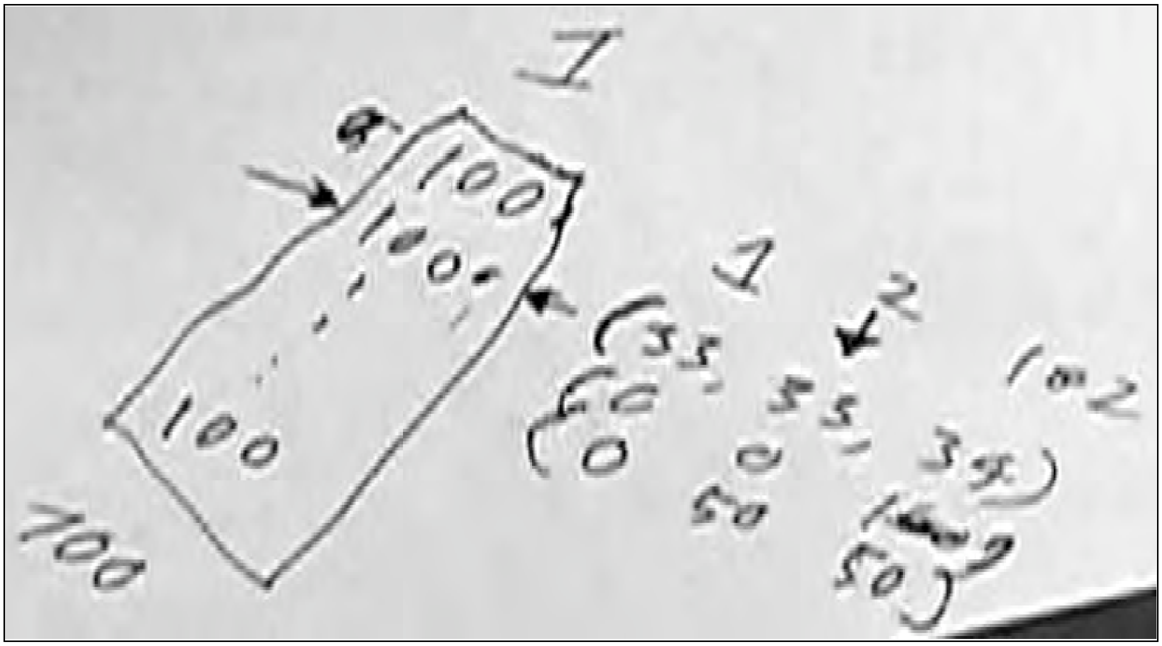


Figure 24: A look ahead at the completed hybrid table

No Generality

You will recall that Ted’s task in this demonstration is to illustrate that the hospital records data for all seven days of the week would apply even in the extreme case wherein the intake screen only operated one day per week. The table Ted is about to sketch incorporates some aspects of the conventional table illustrated in Figures 20 and 22. It lists some numbers that could appear within designated cells of the conventional table, if anyone cared to make this translation. But this new table also includes an explicit temporal dimension. For this reason, I characterize it as a hybrid. By making the unusual temporal structure of Alberto’s project explicit, Ted is positioning the calculations that draw upon this structure to speak to the relationship of this study to those of field biology or epidemiology. Alberto fears that his study’s unusual temporal structure will impede the adoption of the Capture-Recapture technique. Ted has self-assuredly asserted that the technique “doesn’t care” about this study’s unusual temporal structure, and that essentially this study is functionally equivalent to others. In this episode, the initial version of this hybrid table fails to convince Alberto and seems to actually confuse the demonstrator. In other words, generality is not achieved, hence the name of this episode. Let us now consider it in detail.

In Appendix B, panels 1—11, Ted sketches his first version of the hybrid table. The 11 panels of this episode span three pages. Ted first lists patients captured by different methods (“methods” is the predominant term for “screens” in this episode) in a column (panels 1 & 2). He “supposes” that there are 100 patients per day, presumably because that is an easy number to work with. By supposing he marks the number as arbitrary. He could have supposed another number. The number, 100 patients per day, is instantiated as a column of numbers (panel 1). Some of the repeated values of 100 are indicated with only a dot. Thus, time transpires as you move down the page. He inscribes an arrow to designate the second value from the top as the number-captured-by-one-method-on-that-particular-day. That particular day is the one on which the other-method operates (panel 2). This arrow marks the-number-captured-by-one-method-on-that-particular-day (100) as distinct from the-number-captured-by-one-method-in-that-week, which we might already guess is the sum of the values in the column (700). This day is arbitrary in the sense that it is designated only by their say-so (“let’s say …,” panel 3). The row contains cells for the number of patients captured by either method or subgroups of them. Ted writes particular numbers into the cells of the table (panels 3 & 4). So both methods, and subgroups of them, appear along the horizontal dimension. You will find a summary of his descriptions as of panel 4 in the figure below, as Ted describes them.

Description	Patients detected each day by one-method	Patients detected by one-method-only	Patients detected by the other-method-only	Patients detected by both-methods
No generality	100 100 100	33*	33	34
* In a follow-up interview, Ted concedes this value should be modified for purposes of making this table consistent. For consistency, it would be 66.			The sum of the two rightmost values, the (total number of) patients detected by the other method, is never computed nor mentioned in this episode.	

Figure 25: Ted's hybrid table as of the No Generality stage

In this analysis I would have preferred to avoid mentioning or commenting on any "errors" that emerge from a perspective of someone reviewing the activity with the advantage of expansive resources of time. This is because this investigation concerns itself with those cognitive aspects that researchers identify in themselves or each other as they hold one another to account. Nevertheless, it is difficult to follow this demonstration as an instance of object-agency in deployment if the demonstration itself is confusing to the reader. For this reason I mention at this time that these numbers are not mutually compatible according to resources available using existing infrastructure: the patients detected by one-method-only plus the patients detected by both-methods should add up to the number-captured-by-one-method-on-that-one-day. Here they do not add up ($33 + 34 \neq 100$). In a later interview, Ted concedes as much. The value marked by the asterisk in the figure above should be 66 if the other values hold steady. Because the researchers are used to situations where these numbers are consistent, the lack of consistency here may become a source of confusion.

Another possible source of confusion is a missing value, one that is found in the conventional table but not in this hybrid one. Though Ted provides room in his hybrid table for the number-captured-by-one-method-on-that-particular-day in the far left cell on any day) he does not provide room for the other method total. As we shall see, this total for the other method may be inferred as the sum of the two rightmost values. We can see that Ted himself appears somewhat unsure of himself as he puts numbers into the table, as evidenced by: (1) The several pauses in his monologue, (2) Use of the word "method" when he clearly means "people" in panels 3 and 4, (3) Several "uh's" that buy for time as he speaks and (4) An explicit expression of doubt about the rightmost number in panel 4.

The two discrepancies between a conventional table and this hybrid one are possible reasons for these expressions of doubt or confusion. Still, it is an empirical question as to whether any "errors" or "omissions" are consequential to the local production of a necessary sequence leading to some knowledge product. Let us see if Ted or Alberto can dissipate this apparent sense of confusion or doubt as we continue on to panel 5 and beyond.

In panel 5, this horizontal row of numbers constitutes a "case" and this case tells you something, which is to say that it serves the role of an epistemic agent, which I characterize as an actor that reveals a true state of affairs (panel 6). In panel 6 "this method" is marked with an arrow to its left. What does the case tell you? It tells you that the one-method misses people. Whom is it missing? By his finger position in panel 7, it appears that it is missing people that are in the other-method-only cell (panel 7). How does it know this? It knows this because of a particular scenario of capturing. In Ted's description, 33 people are captured by the other-method-only and so (by definition) not by the one-method (panels 8 & 9). In other words, the hospital records screen is missing (at least) 33 people each day even as it enrolls 100. If you are following this scenario so far you might notice that earlier we discussed a class of people that are missed by both methods. There is no location in this hybrid for the number of such people to be represented. In the conventional table of Figure 19, it is represented by the cell with the variable m. In the Hook and Regal table of Figure 21, they are represented as a dash. If there were such missed people in this scenario, then we would add them to the 33 in order to account for how many are missed overall by the one-method.

Why dwell on so much detail? We do so because the task here is to follow how a particular line of necessity is laid out in practice or not. We have already seen at least four sources of confusion or expressions of doubt. Here we add a fifth one that further explains why this first version of the hybrid table will not be sufficient for convincing Alberto. Ted is introducing an ontology of missed people that appears to be unusual. The conventional table does not ordinarily include an account of how many people are missed overall by a given screen. You will notice in Figure 20 for instance that there is no number or variable to the right of the second row and no number or variable below the second column. These would be the locations for an accounting of the total missed by either screen. But as you can see, the conventional technological nexus of representations, categories of captured entities, and metaphors does not account for this particular category of people. Ted is introducing a form of infrastructure with which neither he nor his listener is well coordinated.

As Ted continues, he suggests that you could “run it through the numbers” on the computer (panel 10). The value for the one-method can be inflated appropriately as an aggregate (panel 11). In a mundane, even routine way, Ted describes objects as performing some substantial tasks. The case is an epistemic agent that reveals things you might not have known. This case reveals something about the performance of still other objects—methods that find some patients but miss others.

Necessity has not been preserved here sufficiently to promote a sense of generality requisite for confirming Alberto’s conjecture. We are told something, but there is nothing in what we are told that leads to a necessary result that would convince us. Indeed, no actual estimate for the number of children infected is produced. The calculation is apparently too complicated to work out by hand. Even Ted does not seem to be persuaded. Most relevant is the fact that Alberto says nothing, despite the opportunity Ted provides as he stops talking and looks to him (panel 9). This reaction of stillness suggests reserve and doubt, and little sense of being convinced. As Ted continues, he expresses further doubt of his own by qualifying that, “I think you’ll find” (panel 11) that the calculations will do something appropriate. He mentions inflation, but neither what gets inflated nor the amount of inflation is made visible by Ted. Even if it were, it is unclear how its degree of appropriateness could be assessed. It seems that even Ted has not come to terms with how to align his new table with his own internalized modalities of interaction with groups of caught and not-caught people, and it is very unlikely Alberto has either. The researchers have not collaboratively achieved a sense of conviction.

We could characterize the apparent regress from Ted’s sense of self-assurance to doubt as a case of “losing the phenomenon” (Garfinkel, 2002). We could say that Ted knew for certain that the extreme situation of a one-day second screen works, but that here he is unable to make it work here, and that as a consequence he may now be less certain. In order for us to find the phenomenon so as to make more sense of this episode, let us match the values on the second row of the hybrid table to the conventional table in the figure below, which is a revision of Figure 20. Before doing so, let us remind ourselves that our participants do not engage in this off-line effort in mapping from one table to the next. Our task is not to critique the quality of Ted's teaching or the competence of Alberto as he studies. Rather, our task is to understand the demonstration as an attempt to preserve necessity. If the demonstration fails, our task is to show where necessity is not preserved. By looking at this episode in the context of conventional infrastructure we may gain some insight.

	Intake screen Enrolled (other method)	Intake screen Missed (other method)	Total
Hospital records Enrolled on 2nd day (one-method)	34	33*	100
Hospital records Missed on 2nd day (one-method)	33	*m*	
Total	?		*N*

Figure 26: Finding possible phenomena for No Generality

It would be worth your effort as a reader to take a moment to verify the mapping from Figure 25 to Figure 26. The translation is complicated by the shift in language. For instance, Ted describes "thirty-three patients detected by one method only," which in translation would mean that thirty-three patients are enrolled in the hospital records screen but missed by the intake screen. The relevant cell is marked with an asterisk. Now, recall that the technology of this array includes conventions for recording sums of rows to the right of those rows and sums of columns below those columns. This technology provides a window into the source of Ted's hesitation. It may now be clear enough that the value marked with an asterisk is not consistent with the values to its left and right. In a limited sense we have now found this phenomenon, we can even figure out what the value "should" be. But we must be cautious. It is not clear that the phenomenon we have found in this conventional table is even relevant to the demonstration. This remains to be seen. We may readily find a second phenomenon on the page, which is the total enrolled in the intake screen, designated by the question mark. We are going to see, no, we are going to *experience*, that finding one of these phenomena will be crucial for successfully completing the demonstration. The other phenomenon will be sidelined and made to be essentially irrelevant—until we the investigators bring it up in interviews later. I will not tell you yet which, the asterisk or the question mark, will become crucial and which will be made irrelevant. The following table summarizes this first episode.

<table>
<tr><td colspan="5">⇒ Coordination ⇒</td><td rowspan="4">⇒</td><td rowspan="4">Revision of resources: possible recognition of dissonance on Ted's part at least. This dissonance may induce a search for a new way of perceiving numbers from the table.

Knowledge produced: nothing coherent as no relations have been made stable.</td></tr>
<tr><td colspan="5">New infrastructure includes a hybrid table with some familiar categories of groups as appear in standard representational forms, specific numbers 100, 33, 33, 34 for this case, the vertical dimension for days to unfold.</td></tr>
<tr><td>This case tells you something, methods catch or miss people, running through numbers will inflate something.</td><td>⇒</td><td>Necessity that something inflates by an appropriate amount, not preserved.</td><td>⇒</td><td>Promotion of generality not feasible.</td></tr>
<tr><td colspan="5">Internalized structures accommodate the recognition of categories of patients in the hybrid table. These internalized structures might find dissonance because they may be maladapted to the hybrid table.</td></tr>
<tr><td colspan="5">⇒ Coordination ⇒</td><td></td><td></td></tr>
</table>

Figure 27: Synopsis for No Generality

De-Generality

A preview: we will see here that Ted presents numbers for a new case as well as a set of inscribed labels for cells in the hybrid table (Appendix B, panels 12—17). He then calculates the estimate for the number of children infected without recourse to pen and paper. Apparently in an effort to convince Alberto, he then calculates on paper while making connections among three forms: the Hook and Regal equation, his hybrid table and his calculation for this case. These three forms are summarized below in the figure below. It shows a facsimile of the three forms as of the end of this episode.

I		1	2	1&2
100				
=100	⇐	(33,	33,	34)
.		(0	0	100)
.				
.				
100				
700				

Hybrid Table

Hook & Regal Equation $\frac{X_{+1}\, X_{1+}}{X_{11}}$

$\frac{700 \text{ x } 100}{100} = 700$ Calculation

Figure 28: The three representational forms brought into correspondance

In this episode and after, Ted reverts aware from the term, “methods” back to the term, “screens.” While bringing these forms into correspondence, Ted appears to discover that the screen-two value is needed and that it is expressible as a sum. Recognizing this sum and indexing it appears to be difficult under this new, albeit temporary, infrastructure of the hybrid table. Sometimes Ted ambiguously or contradictorily indexes this sum, creating possible, additional confusion. By the end of the episode, Ted at least seems convinced by this calculation. However, the case presented is an extreme case that would presumably be of very little interest to Alberto. Sometimes mathematicians use the term “degenerate” (with an etymology of *de-generate*) for situations that collapse to configurations that do not appear to allow for making general comments. This case would be such a situation, hence the moniker, “De-generality” for this case. The figure below summarizes how Ted renames each of the four values and how he inscribes the names of these cells. Note that he is introducing alternative values for the second day in the third row, not values for the third day. To the terms introduced explicitly, I add the term, “screen-two” for the total of two-only and one-and-two when appropriate to do so in commentary.

New description	screen-one	one-only	two-only	one-and-two
Inscribed cell names	I	1	2	1&2
	100			
No Generality	100	33*	33	34
De-Generality	.	0	0	100
	.			
	.			
	.			
	100			
	700			

* As mentioned this value would be 66 if made to be consistent with other values.
"Screen-two" would be the sum of the two rightmost values.

Figure 29: Hybrid table at De-Generality stage

We now consider the panels 12—17 in Appendix B in sequence. Ted first marks up the sheet considerably, adding cell names to the four cells, as summarized in the figure above. He also inserts a row of four new numbers. As before, these are arbitrary numbers in the sense that are imputed by merely saying, "let's say" (panel 12). He verbally indexes a "one-day crosscheck" while deictically indexing the one-and-two cell (panel 13). Up to this point only screens have been discussed as being able to do things. If we were to take Ted at his word and gesture, he would appear to mean that the *tabular cell* crosschecks screen-one. This is the one-and-two cell that expresses the combined effect of both screens. Making out this cell as if it were a screen makes for a new ontology with respect to existing infrastructure. This cell has an epistemic role in finding a number of people, which in this case happens to be the same as in screen-one. Ted redescribes the comparison in terms of "your estimate of missing patients." As mentioned in the previous episodes, using one method for finding out how many patients the other misses might be problematic. There are two senses of the category of missing people. There is the number people that a given screen misses. Then there is the smaller of equal number of people missed by both screens. Ted seems to be describing the former, a sense of missingness that is not formally considered in the conventional Capture-Recapture method. It is not recorded on the conventional table and there is no variable for it in the standard equations. The conventional technology for Capture-Recapture only dwells upon how many patients are missed by both screens. It will be instructive to map the De-Generality numbers to the conventional table.

	Intake screen Enrolled (screen-two)	Intake screen Missed (screen-two)	Total
Hospital records Enrolled on 2nd day (screen-one)	100	0	100
Hospital records Missed on 2nd day (screen-one)	0	*m*	
Total	?		*N*

Figure 30: Inspecting possible phenomena for De-Generality

Note first that the phenomenon of the asterisk has disappeared. The number zero in the upper right of the bordered box is consistent with the numbers to its left and right. Furthermore, the phenomenon of the question mark can be lost for a different reason than before. If one were to identify the question mark as 100, it might be easy to confound it with the value 100 at the top of the column. This kind of confounding is always the danger you run into with degenerate examples. There is another sense of degeneracy relevant to the phenomenon of screens missing patients. The number for *m* would be the same as the number that would belong in the empty cell to it right. That is, the number missed by both screens *m* is the same as the total number missed by the hospital records screen alone. This location to the far right of the second row of Figure 30 is the place Ted's description of "your estimate of missing patients" (panel 12) would presumably be mapped. We shall see that this number is zero. But one could map this value of zero to the cell for *m* just as well. Thus, the distinction between these two senses of missingness could easily be lost in this example. So again, it is all too easy to confound one object with another. Let us now move on to the remainder of the episode.

Ted calculates without recourse to computer or paper that the resulting estimate would be 700 (panel 13). Thinking out loud, Ted decides that is worth it to make an explicit calculation to verify the calculated number (panel 14). He first records the sum of the left column at the bottom of that column (panel 14). By means of pointing and simultaneous descriptions, he puts the value 700 into correspondence with X_{+1} (panel 15). Here Ted appropriates the Hook & Regal equation that he wrote onto the sheet previous to these episodes. Ted puts "this total here" (pointing to one-and-two cell value of 100) into correspondence with X_{1+} (panel 15). By stating, "this total here," he verbally indexes the sum of some values while deictically indexing the single value of the cell to which he points.

This is a demonstratively confusing statement. It indicates that Ted himself has not fully accommodated himself to relevant values in the table, or at least not to a level where he is ready to design his talk for the benefit of the recipient of that talk. He did not mention this total in the first episode and may now be only just noticing its relevance as he is forced to identify specific factors to be inserted into his calculations. In other words, Ted searches to make the total visible to himself because it is called for in the equation. This contradictory indexing indicates that Ted himself is only now becoming aware of the phenomenon of the total. This prompts him to then query as to where it is to be found. This phenomenon is the question mark of Figure 30. As mentioned as a possibility in discussing this Figure 30, Ted seems to be confounding one value of 100 with another one.

Ted then puts the number 100 for the calculation into correspondence with X_{11} and calculates the result to be 700 (panel 16). This calculation is one line of argument. He then traces another line of argument, describing the value of 700 as "exactly what you would to do" (panel 17). He thus alludes to criteria beyond the calculation itself for knowing this result. Alberto weakly agrees to this specific case but still appears reserved (nod is slight). He is literally reserving judgment. How do they know this number 700 pertains anyway to the true number of infected people for the week? Following Ted's gestural indexing, he argues that the one-and-two cell with a value of 100 "finds no evidence" that screen-one misses anybody (panel 17). This restates his assertion in panel 13 that screen one misses zero patients. The sense of rightness to the overall argument is completed by a successful matching between a line of argument from a calculation of the true population of sick people and a line of argument that tells us this true value by means of a narrative of what one would hope to do. At the very end of this episode, Alberto indicates some agreement.

As we reach the end, indications are that Ted feels certain he is on the right track. He seems convinced that his deductions lead by necessity to a result of 700. He brings calculations into coordination with the expectation that they will. The equation comes out to be 700 and the process of making that happen is dominated by spatial links mediated by talk and gesture. But he checks this calculational result against an additional criterion: the scenario suggests that neither screen misses anyone. Thus, the "estimation" is nothing more and nothing less than a thorough and perfect count. The calculation is one track along which necessity is preserved, but having this calculation be consistent with the crosscheck (inflated over seven days) validates his method of estimating. Left unstated by Ted is the idea that the one-day crosscheck speaks to all seven days, including the six days when screen-two is not operating. To follow the necessity of this argument, you have to infer this.

Alberto appears to be convinced of the necessity of this particular demonstration. However, this case does not plausibly entail a sufficiently wide sense of generality. I characterize this as a degenerate case because there is no realistic scenario in which both screens would count perfectly and because important distinctions become lost. If both screens did count perfectly, the Capture-Recapture technique would not be considered, as there would be no problem for it to solve. The Capture-Recapture technique is used in epidemiology precisely *because* systems for counting patients are known to be imperfect. Verifying that the formula works for this degenerate case provides at best only provisional assurance it would work in realistic circumstances.

<table>
<tr><td colspan="5">⇒ Coordination ⇒</td><td rowspan="4">⇒</td><td rowspan="5">Revision of resources: links have been forged between and among cells in the table, variables in the equation and factors in the calculation.

Ted has only now seemed to identify a total as a relevant value. His own ability to recognize this appears to be incipient and tenuous.

Knowledge produced: one line of argument, a crosscheck, verifies another line of argument, a calculation for a degenerate case of dubious value for generality.</td></tr>
<tr><td colspan="5">New infrastructure includes headings on table, the equation and the calculation. An alternative set of numbers is introduced: 100, 0, 0, 100. A new epistemic role and ontology for the tabular cell. It has for agency in finding and crosschecking, just as do screens. A new mention of the total but a confusing reference as to where to find it.</td></tr>
<tr><td>Cell one-and-two finds people and cross checks screen-one. The validity of the proposal to use a single day as a crosscheck is contingent upon the calculation deriving a value commensurate to the crosscheck estimation.</td><td>⇒</td><td>Preservation of necessity that in this case the calculated screen-one estimate is exactly 7 times the single day estimate obtained independently by crosschecking.</td><td>⇒</td><td>Promotion of generality is problematic because the crosschecking situation is a degenerate case.</td></tr>
<tr><td colspan="5">Internalized structures accommodate visually attending to and tracking corresponding aspects of three different representational forms. There are new dissonances between conventional and hybrid ontologies and between kinds of indexings of a cell.</td></tr>
<tr><td colspan="5">⇒ Coordination ⇒</td></tr>
</table>

Figure 31: Synopsis for De-Generality

Finding our Question Mark

We will see in this episode that Ted moves to an example that is more realistic (panels 18—31). In these panels he creates a third set of numbers. He then begins to rewrite the equation in the context of this new set. However, he does not at first obtain consent from Alberto that a value in the denominator belongs there. He then directs attention back to the page to clarify terms until he obtains consent. The key stumbling block appears to be the recognition of a total as a value to be inserted into the equation, the very total that Ted has been struggling with in the earlier episodes. This total alludes to the phenomenon of the question mark in Figures 26 and 30 above, hence the title of this episode. The figure below depicts the updated hybrid table for this episode.

New description	screen-one	one-only	two-only	one-and-two
Column heading	I	1	2	1&2
	100			
No Generality	100	33*	33	34
De-Generality	.	0	0	100
Question Mark	.	0**	50	50
	.			
	.			
	100			
	700			

* As mentioned, this would be 66 if made to be consistent with other values.
** Would be 50 if made consistent with other values

Figure 32: The hybrid table as of Finding our Question Mark

Be aware that once again Ted has placed a value into the one-only cell that both he and Alberto will later explain would have to be amended in order to be consistent with the other values as indicated above by the double asterisk. Note that when Ted pulled values into the calculation during the calculation in the previous episode, he did not pull any value from that cell. In this way, this “errant” value may not be consequential to the forthcoming calculation either. This “error” is mentioned here only to make this demonstration more intelligible to the reader so that we can then focus on the deployment of object-agency.

Before considering Ted's demonstration of this third scenario, let us update the map to the conventional table. We do so for our own purposes of following the phenomenon that is the total for screen-two. As we consider the figure below, it should be noted that the question mark in the figure below is *our* question mark. For Ted and Alberto, our phenomenon of the question mark takes on a different form than for them. Ted has to find the phenomenon of the total, just as do we in order for the cognitive practice of this episode to become intelligible, but Ted's means for making this total a relevant and recognizable object must be found within his hybrid table, as he has chosen not to invoke this conventional one. The figure below depicts what this conventional table would be like.

	Intake screen Enrolled (screen-two)	Intake screen Missed (screen-two)	Total
Hospital records Enrolled on 2nd day (screen-one)	50	0**	100
Hospital records Missed on 2nd day (screen-one)	50	*m*	
Total	?		*N*

Figure 33: Finding our question mark in the conventional table

What do we find in the convention table above? We find that the total for screen two (100) will be different than the number for one-and-two (50, upper left of bordered cells). Whereas the total for screen-two is the sum of 50 and 50, the number for one-and-two is only 50. It becomes much easier here to maintain the distinction between these two values than in the previous degenerate case.

Once Ted has clearly articulated this total and Alberto perceives it, the two of them will appear ready to follow the preservation of necessity going forward. In this episode we observe Ted and Alberto struggling with this final hurdle. Ted begins by stammering once again, as if he is not sure that the number 50 belongs in the cell (panels 18—19). He then describes values as he points to them, obtaining Alberto's agreement (panels 20—21). He then begins to write the equation (panel 22). He puts the column sum, 700, into correspondence with the left factor of the numerator (panel 22). This is the position he had established for X_{+1} "as before" by pointing and describing in the previous episode. He then puts the number 100 into correspondence with the right factor of the numerator, qualifying it, "as before, because fifty and fifty is a hundred" while pointing to both numbers (panel 23).

He then elaborates, perhaps sensing that Alberto does not agree that he is performing "as before" in the previous case. Recall that in panel 15 of the previous episode, Ted conflated the value in the one-and-two cell with the screen-two total. The explanation here is clearer than before inasmuch as he clearly articulates the total as a relevant phenomenon. However, in panel 24 Ted confusingly elaborates: "There are a total of a hundred patients found in the second screen." Looking carefully at his deictic references, we see that he immediately reverts to describing the 1&2 label as what is "found in the second screen" (later part of panel 24). That is, he *verbally* describes the total, while he simultaneously and *deictically* describes a single value that is not equal to that total. Alberto's tepid response in panel 24 suggests that he still does not see the phenomenon of a total in the hybrid table.

If we consider the process occurring here in terms of an adaptation to infrastructure, then it seems that Ted progressively comes to integrate his internal structures with the recognition of the total as a value findable by combining two cells. But this coordination with infrastructure is tenuous. He nevertheless continues to display a prior sense that the total is findable in only one cell. Because he still articulates things confusingly, he does not provide the means for Alberto to come into coordination with the infrastructure of the hybrid table.

As Alberto appears to be unconvinced, Ted leaves the calculation aside as he directs his and Alberto's full attention to the hybrid table. He describes the value 50 as detected by one-and-two (obtaining consent, panel 25) and to 50 others detected by two-only (possible consent by eye-contact off camera, panel 26). He then describes patients detected in the two-only cell in a partially negative way, as detected in screen-two, but *not* in screen-one (panel 26). Thus Ted re-describes the two-only value as a group of guys you look for in two places: You find them within the one-and-two cell but not within the screen-one cell (panels 27—28). Furthermore, the sum of two-only and one-and-two is the total for screen-two (panels 29—31). To this Alberto assents to as well (panel 31).

In these latest explanations, Ted finally unambiguously articulates the view that the total for screen two is the sum of two cells in the hybrid table. This total can only be located in the hybrid table as an enacted process of summing two numbers. It seems that this effort to articulate things may finally have convinced Alberto. In this demonstration, Ted cannot direct Alberto to an explicit place for this sum, as it had no designated place to be. Interestingly, Ted once again directly animates the two rightmost cells as detectors of guys rather than as mere subsets of guys detected by screens. In the conventional infrastructure this is an unusual ontology. Consider Figure 20. The intake screen is labeled as enrolling or missing people in the column headings. The hospital records screen is labeled as enrolling or missing people in the row headings. The cells have the ontology of being what becomes enrolled or missed, or what becomes estimated in the case of the variables *m* or *N.* But here Ted states that cells enroll or miss people. As noted above, this shift in ontology may have been confusing for Alberto. For Ted this shift in ontology appears to have been awkward, as he seemed to lose his way on several occasions.

It requires considerable work to get Alberto to recognize an invisible value of 50 plus 50 in the table and to see it as the relevant element to be included as a factor into the calculation. This work is facilitated in part by animating the cells themselves as detectors and by posing the value to be included into the calculation as contingent upon the calculation of the sum of two detected amounts. The preservation of necessity here leads us to see that the screen-two value is 50 plus 50 (panel 23). But the researchers clearly promote its generality in panels 29—31. In future work, using similar methods of calculating, screen-two would be the sum of those two cells, whatever numbers happens to be there.

As a collaborative achievement, the two researchers have now arrived upon a shared re-alignment of internal structures with the novel, hybrid table in front of them. They have found a version of the phenomenon of the question mark of the conventional table within the hybrid table. It appears that the alignment of screen-two in the calculation with the sum of the two rightmost cells was the most difficult hurdle for both Ted and Alberto. Over the course of these episodes, Ted has completed a demonstration. For each horizontal array in this table, he has completed or has attempted to complete a smaller demonstration, one unconvincing (No Generality), one convincing but not compelling (De-Generality). He has interrupted the present demonstration of the present more realistic case in order to demonstrate a phenomenon within it: the total for screen two. Now that this phenomenon has been found, it will be possible to complete the demonstration in the next episode. The synoptic table for this episode is as follows.

<table>
<tr><td colspan="5">⇒ Coordination ⇒</td><td rowspan="5">⇒</td><td rowspan="5">Revised resources: the total for screen-two is now unambiguously the sum of the two rightmost cells. This sum corresponds to a factor in the calculation.

Knowledge product: the relevant factor for the equation is generally determinable by summing two cells in the hybrid table.</td></tr>
<tr><td colspan="5">New infrastructure includes a new set of alternative cell values: 100, 0, 50, 50.</td></tr>
<tr><td>The cells in table detect or miss guys. The substitution of the value of 100 as a factor into the numerator of the calculation is contingent upon the cells in the table plausibly performing as detecting a total of 100 guys.</td><td>⇒</td><td>Preservation of necessity that the number relevant to the calculation, the number detected by screen-two, is the sum of the 50 detected by the two-only cell and the 50 detected by the one-and-two cell.</td><td>⇒</td><td>Promotion of generality is that the number detected by screen-two is the sum of the two-only cell and the one-and-two cell for other such scenarios.</td></tr>
<tr><td colspan="5">Internalized structures accommodate recognizing two cells as the location of screen-two in the table.</td></tr>
<tr><td colspan="5">⇒ Coordination ⇒</td></tr>
</table>

Figure 34: Synopsis for Finding our Question Mark

Generality Promoted

There is a pause at end of panel 31. It may be that off camera Ted and Alberto exchange glances and that from cues so given, Ted feels confident Alberto is ready to move on. In this episode (panels 32—41) we see the completion of a case that is convincing to both parties and sufficiently generalizable so as to speak to Alberto's project. We will see the desired generality successfully promoted.

Ted continues the articulation of correspondence between table and equation (panels 32—34). From this point on, Alberto becomes more verbal and participates more in the demonstration, suggesting that this case has indeed suddenly become intelligible to him. As Ted continues to calculate, Alberto anticipates putting 50 into the numerator as Ted does so (panel 34). Ted then writes the computed result as a product of the factor 2 and the factor 700 (panel 35). He then explains that the one-and-two cell tells you something: it tells you that screen-two "only found half the guys" screen-one found (panels 36 & 37). Here we have a clearer explanation of the epistemic role played by this cell. It tells us something about the comparison of how well screen-two captures compared to how well screen-one captures. In this sense, Ted appears to be reading a "telling" of how the screens are working comparatively into the one-and-two cell. By reading it in comparison to the screen-one cell, you obtain a ratio, here, of 50 to 100 or one-half.

It appears that Ted is borrowing from the infrastructure of the Capture-Recapture technique as follows. Screen-one is akin to the "capture" screen in the wild. 100 people are marked by the hospital records screen as having been infected. Of those 100, 50 are recaptured in the intake screen. Thus, there is a ratio of 50 to 100 or one-half between those marked by screen-one and the greater population of those infected. This finding of one-half makes sense (panel 38) in that this one-half ratio is the inverse of the factor 2, determined earlier (panel 39). This sense making is linked to a "blowing up" enacted within the equation (panel 40). Alberto repeatedly offers assent and co-completed talk (panels 38—40). Ted becomes as animated as his objects as he links sense making with "blowing up" (panels 39—41). Both Ted and Alberto are engaged and in concert as this demonstration comes to an end. Both appear to follow a series of related statements that, strung together, demonstrate to them something that was not so obvious at the outset. So necessity has been preserved along a deductive path to the end they sought.

The preservation of necessity for us readers may not be as easy to maintain, as it is somewhat difficult to follow the cognitive practice from a place one-removed. Nevertheless, it is feasible to articulate some of the key aspects of the cognitive practice to which these two researchers bound themselves. At the broadest level, Ted brings two performances of the screens into comparison, two independent lines of argument. These lines of argument were also completed in the De-Generality episode, but with a less-convincing outcome. Because these lines are each necessity-preserving, it is possible to promote an independent sense of generality for each line of preservation prior to uniting them. The uniting of these two lines of necessity-preservation is itself an emergent outcome that makes this a demonstration of something true and durable. The uniting of these lines is tantamount to the production of knowledge. That knowledge product would be this: even in the extreme case of a one-day intake screen, all the data from the passive, continuous hospital records screen may be retained.

Let us now review the uniting of these two lines of necessity preservation. First, Ted demonstrates that the calculation applied over a week will produce an estimate for infected children: 2 x 700 or double the total amount detected by screen-one over the week (panels 32—35). We examined above how Ted brought values visible on the table or inferable from it into correspondence with an equation so as to convince Alberto. We will see that leaving the expression in terms of factors facilitates an impending comparison. We shall see that the factor 2 is made to compare with its multiplicative inverse of one-half.

This presentation of the expression therefore appears to be deliberate on Ted's part. He appears to decline to compute the product, which is not plausibly difficult, for the purpose of designing the expression for the benefit of the recipient of his talk.

Thus, it would appear that Ted at least is marking this calculation in a strategic way so as to facilitate the promotion of generality. The first general truth promoted is that the equation, the hybrid table and the calculation are all to be put into reliable correspondence with whatever appropriate numbers or recognizable combinations of numbers that happen to appear in the hybrid table. Let us recall that the forging of these correspondences was not at all trivial. It took three attempts to extract a reliable correspondence between the two cells in the hybrid table and the rightmost factor in the numerator of the calculation. This completes the first line of argument.

Second, he demonstrates that screen-two crosschecks on screen-one. It discerns that screen-one misses half the patients on that given day. We have not considered in detail how necessity was preserved along this line. Ted invokes the conventional sense of the Capture-Recapture technique in epidemiology: the case tells you that screen-one finds half (and misses half) of the patients on the day of *concurrently running screens* (panels 36 & 37). Both screens are operating on the same day over completely concurrent time frames—for that day. So, the second general truth promoted is that a crosscheck determines a ratio that compares the screen-one number to the true number of infected patients: here, one-half, but it could be any ratio. This truth is only part of the second line of argument.

To complete this line of argument, Ted takes the knowledge for that one day just mentioned, one step further. The knowledge for one day speaks to a state of affairs that pertains to all days, *even when screen-two is not crosschecking.* In this way you can proceed to inflate the half proportion over the course of the whole week. The number detected in screen-one over the course of the week will be half the number of patients truly infected with influenza over the course of a week. Thus, the third general truth promoted is that there is an "appropriate amount" of inflating (panel 11) that is a factor of 7 here, but might be taken more generally as a ratio between days of operation of screens.

At the terminus of the first line of necessity preservation (calculation), double-700 was the true number. At the terminus of the second line of necessity preservation (single day crosscheck, followed by inflation), the number 700 was 7 times half of the true number for that one day. The true number for that one day would be whatever the value, 100, is half of. The descriptions just made are convoluted, but they are only as convoluted as Ted made them. It is tempting to jump to the calculation that along each line of argument, the true number is 1400. But this does not seem to be how Ted reconciled the two lines of argument. Instead, he appears to have reconciled them by means of the factor of 2 he left in the expression for the calculation and by comparing this factor to its inverse of one-half determined via the crosscheck. Rather than comparing answers for true numbers of people infected, he appears to have compared by means of factors or ratios. The researchers appear to take it as obvious and shared that doubling (in calculation) and half-taking (in crosschecking) are inverse processes that make a sensible link between the two lines of argument. This is an example of what it may mean to make sense in mathematics, to see through to a particular result and to also see it as true along independent lines while also seeing a link between those two lines.

We are not done with the episode. We must attend to the subtle but important extension to generality for this case as an exemplary case that speaks to all similar cases. Alberto will clearly not be satisfied with a technique applicable only to cases with proportions of one-half or with cases showing exactly 100 patients per day enrolled into the hospital records system. The researchers need to promote the generality of this demonstration considerably. Now, the work of promoting generality has already begun and it could be done if participants take it as done. After all, generality is more tacit. It is the conclusion that you are supposed to draw from the specific case observed.

But there may still be a need to promote the generality of the entire demonstration as a composite. Ted has demonstrated a situation for a special circumstance that is viewable as arbitrary. The case shows 100 patients per day regularly found by screen-one. But we are supposed to see this as arbitrary. It could be some other number. It could vary by day. It could play out over a whole month, not just a week. The crosscheck could happen on any day, not just day 2. It could happen on several days per week. These various extensions from the particular necessity to the general may not be obvious. Most important, the making of sense by uniting two lines of reasoning for one case, speaks to states of affairs for unions of lines of reasoning pertaining to all cases interpretable as different in only arbitrary ways. Ted appears to sense a need to make some of these forms of generality a little more explicit.

Still hunkered over the page (panel 41), Ted links elements on this page to probabilities and crosschecks he had mentioned just prior to entering into the mathematics of this series of episodes (see Coming from the Science above). The specific role played by the number 50 in the one-and-two cell in crosschecking the imprecision of the screen-one cell number 100 in the specific case is a general one in that the value in the one-and-two cell will check against the value in the screen-one cell for any similar cases that may have different specific numbers in them. This sense of generality is still, in a sense, locatable on this page—you would just have to imagine lots of other cases inscribed and calculations done in similar ways on this page or a similar one.

The reason for doing this demonstration in the first place is to promote a form of generality that translates beyond this page and beyond imagined similar pages. The reason for this demonstration was to travel into the delimited region of the page and return with assurance of the validity of actions within the science of Alberto's project. We will see Ted and Alberto move more completely back into this science in the next episode. Here is the synoptic table for this episode.

⇒ Coordination ⇒

New infrastructure includes an unambiguous, nontrivial case. The calculation from values in cells is now reliably enactable.

The one-and-two cell tells you how well screen-one cell performs. The making sense for the case is contingent upon the calculation deriving an estimate commensurate to an alternative derivation made by crosschecking and inflating.	⇒	Two lines of preservation of necessity are completed and then joined. The integration of the crosscheck and the blowing up achieves a sense that the results are commensurate.	⇒	Promotion of generality is that in other such cases, the two lines of deduction would join.

Internalized structures accommodate links between cells in table and factors in the calculation, the perception of a proportion emergent from a crosscheck and perception of inflation.

⇒ Coordination ⇒

⇒

Revised resources: an additional completed calculation with a denominator factor different from the right factor of the numerator. This makes the distinction between values to be drawn from the table more clear. A clearer articulation and enactment of crosschecking.

Knowledge produced: a stable relation among the three mediating representational forms. These include an equation, cells in the hybrid table, and a calculation. This stable relation itself relates to stabilized epistemic roles for a cell value or for screen-acting-within-cell value to cross check. These epistemic roles and the integrations of representational forms are mediated by researchers' internalized structures that have taken shape over time.

Figure 35: Synopsis for Generality Promoted

Back to the Science

This chapter has been about preserving necessity and promoting generality in a mathematical demonstration. As these episodes began, Ted leaned over the page. Ted is no longer hunched over the paper (panels 42—46). He is no longer constrained to interact with it, so he takes a more comfortable position away from the table. He is not symbolizing his act of completion so much as he enacts it. The mathematical demonstration is over. Ted now talks about things related to the paper, but not on the paper, things they had been talking about before the mathematical diversion. Note that he mentions “the second screen” in panel 43, but appears to refer to the hospital records system that filled the role of the first screen in his demonstration. Here he may speak this way unintentionally. Alternatively, from his new perspective away from the page, he may very well regard the hospital records review as a secondary screen compared to the intake screen. In this interpretation, the misalignment of terms between the mathematics of the page and the science of the larger project only confirms how separate these two worlds are. It indicates that the terms need not be aligned because the worlds are distinct. Ted specifically recommends including all numbers on the smaller screen (panel 43) in order to have non-small numbers for analysis (panel 45), a statistical concern that has nothing at all to do with the demonstration just completed, but has everything to do with Alberto’s larger scientific project and is one of the reasons Alberto was worried about the matched-days problem in the first place. They are back to science.

Generality in this Page

The calculation described in these episodes entailed some very specific values, yet the researchers clearly took the calculation to promote an acceptable scope of generality. We have discussed their subsequent success in realizing a kind of generality beyond the time and location of this meeting (Hall, Wieckert & Wright, 2010). But here the emphasis has been upon a sense of the general located within the world that has been made to be delimited at the interface between people and paper. This world is made to be delimited in the sense that restricting the relevant global topics to the interface of local bodies with the local page is itself an achievement. It constituted the setting for what I have called, entering into the mathematics.

These researchers arrived eventually at preserving necessity and promoting generality to the satisfaction of both. It was not easy, however. Ted's tabular tool for preserving necessity here was different from the one conventionally employed in this practice. Rather than using a conventional table for illustrating his cases, he used a hybrid one. He did so in order to contemplate a novel modeling problem. As a result, things located in the hybrid table and things to be operated upon in the equation were no longer in coordination with the internalized structures of the researchers. Recall that in Figure 22 we saw that the entities within the conventional table could be explicitly brought into correspondence with entities of a conventional equation by means of a symbolic syntax. Whereas they both had been competent at identifying and manipulating objects relevant to this modeling environment, they no longer were. They required several calculations to configure correspondences between objects so as to preserve the necessity they sought.

Learning While Recruiting Objects

This section focuses on the documentation of learning across episodes while producing knowledge, a theme that speaks to both of the questions posed at the beginning of the chapter. They are as follows. (1) *How do objects typically perform or how are they typically made to perform in cognitive, epistemic demonstrations?* And, (2) *How are people and objects recruited into the infrastructure of demonstrations so as to make things hold together as new knowledge?* Many knowledge objects were produced in these episodes. The major knowledge product of this chapter here might be summed up as: A single-day screen legitimately crosschecks a seven-day screen.

With a view toward tackling the main questions, we will consider the various ways people recruit objects and then use these findings as windows into how people are conditioned and into how people learn to navigate within settings of such epistemic uncertainty.

Typical Discursive Manners

First, objects are recruited to perform. Above we have seen that at each stage of a journey toward achieving consensus within the collaboration, objects were animated. This animation of objects, as well as the contingent performance of them, is routine. It should not surprise us to find that interactive work of this kind involves metaphor and conceptual blending of objects and actions ordinarily reserved for humans. However, an open question is the extent to which the peculiar way of framing objects as performing epistemic roles reflects a specialized understanding of, or advanced participation in, communities of practice in knowledge-making disciplines. My conjecture is that indeed, people uninitiated to such talk will not be able to follow it. They do not follow the implicit, situated practice that frames objects as performative in order to build a stable relation that will then constitute knowledge. Those who lack this implicit, epistemic understanding cannot develop the connected sense of knowing mathematics that both facilitates present success in learning while also promoting future participation in mathematics. To achieve something like recognizable success, such epistemically uninitiated students are relegated to faithfully following formal procedures (Boaler & Greeno, 2000).

Importantly, whenever Ted had trouble coordinating with infrastructure, either intrapersonally or interpersonally, he resorted to animating objects as a means of reasoning his way along. Animation of objects is not a mere epiphenomenal aspect of reasoning, but an integral and essential part of what reasoning is. Epistemic, cognitive practice is a discursive activity. Ted is struggling to find out how his hybrid table works in relation to the Hook and Equation and in relation to the calculation he is trying to execute. The examples above provide evidence or stand as examples of cognition as action situated in activity, partly dependent upon internalized structures "inside the skin," but certainly not adequately explained or reducible to such structures.

From these episodes it seems that for a participants to learn within an epistemic, cognitive practice, they must become familiar with and conversant with the peculiar manners of animating objects in activity. An additional aspect of object animation should be explicitly summarized. Ted and Alberto recruit particular objects on a page in order to preserve necessity along some line of argument, but with a view towards promoting generality to similar cases. They both know this implicitly. They hover intently over a sheet of paper. Ted had promised that on this piece of paper Alberto's hopes for a successful project would be realized. For this short time it was central to Alberto's project. But once they were done scribbling, they were ready to toss the sheet aside. They were focused on a particular object but only to make provision for a type of object. Once this type of object, this generality was promoted, they had no more need of the particular one.

The first aspect of learning in an epistemic, cognitive practice that emerges from this line of analysis is summarized as follows: (1) Learning within an epistemic, cognitive practice depends upon participants becoming conversant with typical discursive manner by which objects become animated in talk.

Struggle is Productive

Second, objects are recruited as a means of coordinating internalized structures with infrastructure. Concerted attention has been applied to the often-tedious documentation of the emergent recognition, and means for articulating, the sum of the two-only and one-and-two cells as constituting the total number for screen-two. This attention has been worth the effort because it illustrates materially and interactively the process of coming into coordination with infrastructure. This episode depicts two researchers who are well-versed and well-coordinated with existing infrastructure. We are fortunate enough to observe them here struggling to integrate their knowledge and intuitions with this novel representational form. How do we interpret the many false starts, the hesitations and self-admitted errors of Ted? He studied Capture-Recapture presumably with great intensity as part of his PhD work (albeit many years prior). He is a successful teacher and professional. In the larger context of this work he shows that he can program code and critique others in the field for following suboptimal methods. It is implausible that Ted carries around deep misconceptions about the technique or about proportional reasoning. It is more plausible to locate the problem as one of poor coordination with infrastructure that improves progressively as Ted and Alberto strive to use the hybrid table as a means of bringing groups of people, an unusual temporal structure of gathering lists, cells in a table and factors of a calculation into correspondence.

As recounted above, in the unconvincing, No Generality case, Ted did not mention the number for screen-two, nor did he even mention that it would be determinable. He provided no indication to us that he perceived it at all as a relevant entity. Furthermore, he seemed unconvinced and tentative about his own demonstration. I have argued that this was because he could not bring his hybrid table into coordination with his own intuition about one screen cross-checking the other. He already knew he was right about assigning one screen to crosscheck another, but he could not get his new infrastructure to demonstrate this rightness. His response was to put his infrastructure to the test in an alternative example, a degenerate case that nevertheless gave him some reassurance that this infrastructure could be made to work. In order to achieve further reassurance, he needed to run a calculation. Running the calculation in turn required that elements in the table be made to correspond to its factors. Where was Ted to find the rightmost factor in the numerator, the screen-two number? Not in a single cell, he discovers, but in two of the cells. As I have argued, his "discovery" was transient and he subsequently lost it before finding it again. Now, having found it himself, at least once, he set about making this sum durable for himself and visible to Alberto. Only then could they proceed to complete the deductive path to achieve necessity and generality.

Ted's process of bringing his own modalities of recognizing, gesturing, writing and describing into coordination with his hybrid table can be traced as a learning experience. The learning experience for Alberto was different, but he too had to come into coordination with the same objects as did Ted. This example gives credence to the idea that students need to struggle with various representational forms as they construct arguments about them (Lehrer and Schauble, 2003). The justification is that this struggle makes it necessary to explore structure within the representational form. This indeed is exactly what happened to Ted. He did not notice the absence of the screen-two number until he needed to appropriate it as a factor in a calculation. The modeling approach for teaching mathematics and science (Lesh & Doerr, 3003; Lesh, Hamilton & Kaput, 2007) can find in this example confirmation of the benefits of this approach. Ted and Alberto's modeling can be seen as a means toward developing a more sophisticated understanding of proportion. It can also be seen as and end in itself inasmuch as the two of them have witnessed the utility of invoking novel representational forms when existing forms are not available to depict the situation proposed. Understanding or believing in that utility may be instrumental for either researcher at a later time.

This example also provides another lens onto the difficulty learners may have with discerning states of affairs from standard representational forms. These forms appear to the initiated as so obviously depicting these states of affairs. But we already have reason to question the simple transparency of representational forms. Several studies have documented that experienced users of graphs in one specialized practice have difficulties when confronted with graphs that depict a related, but different situation. For example, nurses in a pediatric ward routinely made consequential decisions based on interpretations of typical blood pressure and of variations in blood pressure from graphical readouts of instruments (Noss & Hoyle, 1999). However, when confronted with graphs depicting distributions of blood pressure within populations over time, the nurses appeared to be unable to make intelligible readings of them. Only with concerted training were they able to see variation as a population-wide phenomenon. The nurses even had trouble integrating formal statistical concepts that ostensibly pertained to their work. For example, the concept of variation in an individual's blood pressure as distribution around a mean did not appeal to them. For them, the variation (increase or decrease) of blood pressure due to the onset of some medical condition is what mattered. The authors concluded that though their work was richly mathematical, it might not be a very useful context from which to draw for teaching standard statistical concepts.

This study suggests in contrast that there is something worthwhile to extract from investigations of reasoning around representational forms (here, a table and calculation using an equation). But rather than borrowing from the level of topic (unwanted situated use of variation among nurses there, problem of matching days here), we could borrow from the level of epistemic argumentative form. Ted treats his table as a thing to be probed and interrogated. He tries things and does not appear to be especially uncomfortable with failure (as in No-Generality). He employs an epistemic trick that from my experience is rather common and well worth teaching. This trick is the invocation of a special degenerate case. He comes into improved coordination with his representational form by using this special case in order to obtain some assurance there is something right about the approach being taken (De-Generality). Finally, he is able to enact a more realistic situation (Generality Promoted).

The second aspect of learning in epistemic, cognitive practices that emerges from these episodes is as follows: (2) Learning within an epistemic, cognitive practice is facilitated as participants struggle to coordinate their internalized structures (understandings, concepts) with representational forms that organize and present performances of object—the struggle can be productive for learning.

Promotion of Generality is Interactional

There is much that Ted and Alberto do not appear to have to say explicitly but that must be taken implicitly for them to have found the demonstration to be convincing. For example, as mentioned, the true number of patients did not vary by day in the demonstration, but the participants must have implicitly assumed it could. Otherwise, it would not be plausible that either took the demonstration as speaking to Alberto's project. Because so much of the promotion of generality may be tacit, a demonstration may range from excessively tediousness to cryptic. It would be tedious for the demonstrator to explicate all the forms of generality he or she could think of. Conversely, it would often be cryptic for the demonstrator to assert promotions of generality without filling in some of the details. I have argued that Ted selectively promotes terms in general form in panel 41 in order to assist Alberto. To be selective suggests strategically choosing details for inclusion. As an interlocutor, Alberto has provided several hints that give Ted a sense of what he does follow. These are especially evident in the co-completion of talk as well as his shift toward the end in giving frequent and unreserved assent. Ted for his part could derive from these cues the information he may need to explicitly state details that might remain necessary for Alberto to notice. This is an intrinsically interactive aspect of sense making. This brings us to a third aspect of learning in epistemic, cognitive practice: (3) Learning within an epistemic, cognitive practice is situated in collaborative work to monitor and assess interlocutors' taking up what is general from what is necessary from particular material, social situations.

Taking Contingencies and Performances into Account

Fourth, objects are recruited for the purpose of forming new knowledge relations. As we have seen above, the various knowledge products are contingent upon the particular performances of objects. During a follow-up interview, Ted was asked about the lack of consistency among the numbers used in the convincing case. You will recall that this lack of consistency forced us to find the phenomenon by substituting the number 66 for 33 in Figure 25. Likewise we were forced to substitute the number 50 for 0 in Figure 32 of Finding our Question Mark. In the latter episode, Ted attempted an explicit calculation, but never put the number 0 into correspondence with any number in the calculation. In this way, the phenomenon we experienced was irrelevant to the researchers. The phenomenon we experienced as lost but then found was never missed by them. That is, it was never missed until we reported it as missing in a later interview.

When we asked Ted about these discrepant values, he assessed the situation and then affirmed this inconsistency. He immediately looked over his calculations to confirm that the general truths promoted earlier were still valid. He then explained, “In terms of getting across the logic of how the method works, what I did was fine.” I interpret his quickness in searching for an affirmation of the irrelevance of his mistake as evidence of his awareness of the negative implications: the stability of the knowledge of the appropriateness of crosschecking was contingent upon the workings of this table. Though Ted was visibly confident throughout the follow-up interview that his method was correct, a mistake nevertheless called for immediate analysis inasmuch as it was disconcerting to think that, however unlikely it might be, changing this one little detail might bring down the whole project like a house of cards. Some of the objects in his hybrid table were recruited to form new knowledge relations. In Ted’s understanding, this knowledge was contingent upon the viability of the relations among these objects. This interpretation is confirmed by the attention Ted paid to assuring himself and us that these objects remained viable, despite his error.

It follows that in order to become proficient in a knowledge producing enterprise, you must bring internalized structures into coordination with the epistemic roles that objects are typically posed to play. You also have to see in a wide sense how particular performances play into a larger process that makes for knowledge. A fourth aspect of learning in epistemic, cognitive practice is as follows: (4) Learning of knowledge produced within an epistemic, cognitive practice is to be deeply aware of the relevant contingencies and performances.

The Hybrid Table as an Actor

For each of Ted’s examples, we saw for ourselves what a conventional table would look like. This allowed for a comparison between phenomena that were routinely findable in conventional use and phenomena that became lost in the hybrid table. As mentioned, I have portrayed the total for the second-screen as one such phenomenon. I labeled it as a question mark on the conventional table. Another phenomenon was the consistency (or inconsistency) of the one-only number with its left and right neighbors on the conventional table. This too is a phenomenon. I labeled it with an asterisk or double asterisk on two of the conventional tables and on two of the hybrid tables. Interestingly, Ted loses both of these phenomena as he constructs his hybrid table. Only one of those things lost became important for him to find.

I have resisted portraying agency of objects in elaborate terms, as Latour sometimes does. It is hard for me to work with the notion of a hybrid table as an agent that would inhabit an identity like that of a human. However, I think it is productive to portray the hybrid table as exercising some agency. It has agency to bewilder Ted and/or Alberto under certain conditions. This condition for which it created bewilderment was the act of extracting a number from the table for the purpose of inserting it into the calculation. Ted finally found the total for screen-two in the table. The circumstances revealed in this episode suggest that Ted was looking for this phenomenon in the sense that he kept trying examples. He was not satisfied with one satisfactory calculation. He may have intuited that something was wrong and that he should just keep trying examples until he found that something. This leads to the fifth and final aspect of an epistemic, cognitive practice. (5) Learning to competently use representational forms within an epistemic, cognitive practice is to develop the habit of interrogating them for possibilities that they might reveal.

I conclude with a few comments about the metaphysics of relations. Many scientists and teachers may find this relativist epistemic stance to knowledge (production) disconcerting. They might think that to point out contingencies, shifts in agency or the performance of many actors, promotes a discourse that undermines the authority of science (or even mathematics). I think this case illustrates the opposite. The contingencies point to how objects, not people, exercise agency so as to construct knowledge claims. Mathematics and science may garner greater support to the extent that they articulate findings as contingent upon the performance of objects with agency independent of scientists. For teachers, this chapter provides support for a view that epistemic authority derives from claims that are accountable to nature (Ford, 2008b). Mathematical and scientific ideas are dialogic in the sense that to understand them as concepts you have to already understand them in epistemic terms as well, as explanations of nature and as more plausible than alternative ideas (Ford, 2012). This general point will be elaborated upon in greater depth in Chapter VI. This chapter puts a finer point on what accountability to nature and plausibility entail. It entails making objects perform and arranging events so that their performances may inform consequential decisions or conclusions. The more mathematical and scientific ideas appear to arise from the performance of objects, the stronger they are.

CHAPTER V

RETROSPECTIVE AND PROSPECTIVE ANALYSIS

Introduction

So far we have dealt with situations where researchers sought to produce specific knowledge claims. They usually had an idea of what they wanted to demonstrate. They employed representational forms in order to convince someone else. In this chapter, two scientists want to produce knowledge claims but do not yet know specifically what these claims might be like. They try to demonstrate with graphs and tables, but do not quite know what they are trying to get these forms to reveal. As a result, the researchers come back again and again to questions of method. Both the scientists and statisticians wanted to report knowledge claims as contingent upon the performance of objects demonstrated by means of models. But they had differing ideas about how to do this. The scientists wanted to retrospectively determine models with acquired data in hand. The statisticians wanted to prospectively determine models in anticipation of data to be acquired. Their opposing standpoints can be seen as a difference over where the epistemic roles for objects and for people belong. As we look across this collaboration, we will trace a process of negotiation over how to assemble the epistemic roles of objects with the epistemic roles of people.

As a reminder, the primary questions for this investigation, as posed in Chapter I were the following. (1) How do objects typically perform or how are they typically made to perform in cognitive, epistemic demonstrations? And, (2) How are people and objects recruited into the infrastructure of demonstrations so as to make things hold together as new knowledge? With these questions and epistopics of prospective and retrospective analyses in mind, we will focus on interactions that pertain to talk about methods for analyzing data. It turns out that the analytical sequence, whether things are arranged prospectively or retrospectively, becomes a prominent feature of these settings.

The notion of an adaptation to a complex system (Hutchins, 1995) gives us less leverage on learning within this meeting than in the ones discussed earlier. This is because the very grounds for what should make up the complex system of knowledge production is what is at stake in the discussion among researchers here. Their collaboration here is very much about making for a workable epistemic system that they can join together. However, we can suppose that the researchers share a common culture or situated action over the course of the meeting. For this reason learning will depicted here in terms of participation in a community of practice (Lave & Wenger, 1991). We will ask ourselves what becomes available for people to experience or enact, in order for us to get a handle on learning within this situation. Also for this reason I will not employ the synoptic graphic organizer that has been featured extensively so far.

Phillip discusses methods for a study on the prevalence of HPV in Asia. Clockwise from left: Janet (Masters-level statistician), Naresh (Senior scientists) Lynn (medical student scientist) and Phillip (PhD statistician).

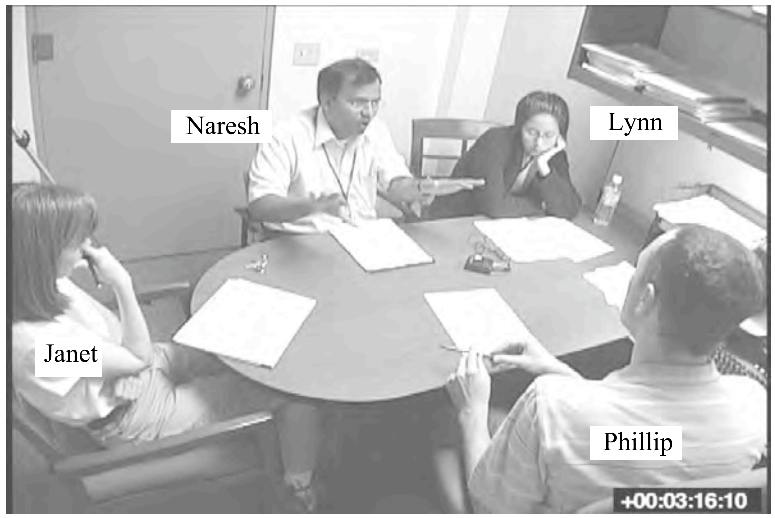


Figure 36: Researchers discuss how to model HPV prevalence

The figure above shows a picture of the researchers. Janet and Phillip are statisticians, and the two scientists are Lynn and Naresh. All names are pseudonyms. Janet held a position in the Department of Biostatistics as a Masters Degree level faculty. At the time, she had been at the medical school for only a few years, having arrived shortly after obtaining her Masters Degree at another biostatistics department in the United States. Masters level biostatisticians in this department are often assigned to work under the direction of a PhD level statistician, as occurs in this consultation. Phillip was an Assistant Professor in the Department of Biostatistics, was on a tenure track and had also recently joined the medical school after obtaining his most recent advanced degree (PhD) at another biostatistics department in the United States. Janet and Phillip were assigned by the Department of Biostatistics to work with Naresh's research group. Lynn was a medical student who intended to participate in the study under discussion here the following year in Asia as part of her graduate training in medical research. Students at this medical school are required to complete a study as part of their program.

Naresh was an epidemiologist of sexually transmitted disease in third world countries who also had recently come to the medical school. He held a research faculty position in an interdisciplinary department concerned with international health. He was serving as mentor to Lynn's work. In addition to facilitating Lynn's research, it seems that he sought to conduct related studies of his own in association with her work at this clinic. His prior work as well as the proposed work under consideration here had focused in developing countries where problems of health fall on large populations of the poor. The private and public support from the developed world for scientific work of this kind can be seen as the merging of two kinds of interest. Their concerns may be characterized as both charitable, but also self-interested. Besides dealing directly with the concerns of the poor, governments are concerned about the transmission of disease in all segments of society, including the middle class and wealthy. When diseases flourish among the poor, they imperil everyone.

It is worth looking ahead at what these episodes are going to reveal in broad terms. The designated epistopics of retrospective and prospective analyses are so chosen because, from my general experience with statistical practice, the distinction between them is a common distinction made. It has important epistemic aspects that matter to statisticians and that will be shown to be relevant here as the researchers discuss the sequencing of analysis and sometime use the very terms, retrospective and prospective. As we enter more deeply into the lifeworld of these researchers we will come to understand that the distinction between the two epistopics may be subtle. We will inspect the means by which one might learn to understand the local production of a method of research.

We will see first that Naresh and Phillip have a kind of established practice from past collaboration. In that collaboration, Phillip substantially revised a model that Naresh had anticipated using after Naresh had already completed the clinical work of acquiring the data. From this model they reported important inferences. In the episodes we view here, Naresh repeatedly seeks to plan on making similar analyses and model decisions in a "retrospective" fashion, which he takes to be the fashion of analysis he and Phillip had conducted previously. Phillip repeatedly resists. It seems that Naresh has drawn a lesson from past collaboration that is somehow not in keeping with the lesson Phillip has drawn from it. When the difference in lessons taken becomes consequential to their future collaboration, how do these contradictory lessons come to be reconciled? Part of the answer, one again, is that participants recruit objects to demonstrate scenarios that make methods of prospective or retrospective analyses visible.

Previous Retrospective Analysis

In a study he had recently published with Philip, Naresh examined the relationship between high-risk Human Papillomavirus (HPV) types and cancerous cervical lesions among women in an African city. All these women carried the Human immunodeficiency virus (were HIV-positive). HIV is the pathogen responsible for Acquired Immune Deficiency Syndrome (AIDS). Most of the women were on Highly Active Retroviral Therapy (HAART) during the study.

Because the participants in these meetings often refer to this recent paper and to their collaborating working practices leading up to it, one of its principal findings, as well as some discussion of the their collaborative work are worth a brief mention. They found that the logarithmic odds of severe lesions increased with age until about age 40 years, after which the odds decreased. Naresh explained in a later interview that the decreased risk after age 40 years was probably due to hormonal and metabolic changes after menopause. The graphical expression of this relationship was a curvilinear path having a peak at about age 40. It was modeled using a technique called restricted cubic splines. This finding was the subject of a story Phillip and Naresh told at the end of one of the meetings of this case.

As Phillip explained, he first considered a statistical model that dichotomized patients into older and younger groups. The model found a significant difference between groups. But Phillip frowns upon techniques that do so, he explains elsewhere, as it unnecessarily erases information. It is likely that such dichotomizing is common in Naresh's field of epidemiology and that Naresh wished to imitate this practice. So Phillip may have performed the analysis for purposes of demonstrating its faults or he another statistician at the university at which Naresh conducted the field trials had done it. In any case, Phillip then modeled the trend using a linear model. But here he found that *un-erasing* the information had the effect of *erasing* the finding. In ethnomethodological terms, the linear graph lost the phenomenon of significance that the dichotomous graph had found. Phillip characterized this story as a "funny" because of the irony of that the better method erased an important finding. The resolution of the affair came about when Phillip modeled the trend as curvilinear, a resolution that used yet more information while finding a trend again. The figure below shows the participants pantomiming the graphical trends.

Phillip: If you dichotomized it, it was significant	Phillip: But if you kept it linear, it was insignificant	Naresh: that was like a bell shaped curve

Figure 37: Naresh (on left) and Phillip discuss past retrospective modeling

Of greater import to us than the findings themselves, is the sequence of actions taken. Phillip joined the project to analyze data rather late in the game. Naresh had only recently arrived at the medical school, having conducted the study initially when employed at a different university. Phillip was not involved with the planning of the study. It is possible that Phillip intervened to change the planned analysis from a dichotomized age variable to a continuous one. In later episodes we shall see that Naresh apparently takes a lesson from this story that Phillip does not intend or condone. He seems to think that a statistician might want to intentionally wait until the study is mostly completed before setting upon a model for analysis of the data. Phillip's intervention was apparently very helpful to Naresh and his colleagues. He mentions just after this episode that a colleague of his had just recently presented this paper at a conference. The sophisticated analysis introduced by Phillip apparently made these researchers' work look good. As we shall see, Phillip on the other hand seems to take a different lesson. Intervening in this way is simply pragmatic. Though he might have preferred to be involved from the beginning, his task as a statistician was to employ the soundest principles possible under the circumstances.

The General Natural History Model

The episodes we will investigate in detail took place in the second of two meetings, one week apart. The preliminary meeting was somewhat of a bust. Because Naresh was tied up with a family concern, Lynn was obliged to explain the research plan on her own. After they had talked for a while, they agreed that the researchers, or Lynn at least, did not have a sufficiently firm grasp of the intended study for continued talk to be productive. Lynn left and then so did Cathy. But soon Naresh arrived after all. He and Phillip then talked for a while before agreeing to reconvene after Naresh had a better idea of what his research questions were. It was at the end of this meeting that Phillip and Naresh told the funny story about past collaborative practice discussed above.

In this second meeting all parties are more prepared. Naresh brought along a “Protocol Schema,” a short write up of his plans for the study. Over the past week he has talked to colleagues who run a health clinic in an Asian city. It is at this clinic that they want to do the study and where Lynn will be going to oversee it. Lynn has brought in a notebook filled with copious details of epidemiological findings from earlier studies in this Asian region. She has also brought along prints of most of the articles from which she had gleaned these details. Both Phillip and Cindy have been communicating with Naresh and Lynn by email over the weeklong interim since the last meeting. At Naresh’s request Cindy has created what Naresh will call a “sample size estimate” in the episode of the next section. It is a report that explores the relationship between statistical power and the sample sizes they might be able to acquire in the study.

The title of this section is a quote of Naresh, who summarizes his own verbal description of the his research plans as “the general natural history model.” This section prepares us for the episode that will be focus of the next section. Neither biostatistician had a chance to read the Protocol Schema before the meeting. Here is how Naresh verbally summarizes it to them.

> This will be a cohort study on three eighty-five or five hundred, whatever we get the final indication of our sample size. Uh, of those N number, will be followed by time point A and time point B, six months apart. And, this will actually estimate the incidence, regression or persistence of HPV types and SIL lesions, squamous intraepithelial lesions. Which means if we screen the five hundred or three eighty-five women at base line, we get a baseline prevalence of all the HPV types, as well as the prevalence of the SIL lesions in these patients. We follow them over, to six months. Some patients may have had some HPV types, newly developed. Some may have the current types go away, and some may have the current types also present six months down the line, and the same thing can be said with SIL, some new, some gone, and some the same. (Naresh)

In order to appreciate Naresh's explanation, it will be helpful to clarify the standard descriptions of a few terms, drawing from the field data mostly, but also from standard statistical or medical texts:

A *cohort* is a group of people from whom numerical measures are taken at two or more time points. Here Naresh imagines two time points only, designated as A and B. Under standard procedures as indicated in introductory texts, the unit of statistical analysis would then be the pair of numbers associated with each person.

A *sample size* is the number of women (the "N number") recruited from the population into the investigation. Alternatively, it could also be the number of pairs of numbers generated in the investigation. The greater the sample size, the more precise estimates may be inferred regarding characteristics of the population of people represented by the sample. Naresh's contacts in Asia tell him to expect a sample size of 385, based on past records of the number of women typically seeking treatment at their clinics. This is why Naresh mentions the number ,385. Naresh is hoping to obtain 1000 HPV testing kits as a donation from the manufacturer. This would allow him to perform tow tests on each woman within a cohort of as many as 500 women, if such a number became available.

The first of two such population characteristics he mentions is the presence or absence of *Human Papillomavirus (HPV)*. Based on the other comments, he refers here only to specific oncogenic (cancer-causing) types of HPV—those known to cause cervical cancer in women. There are hundreds of types of HPV in humans and in animals, many of which are comparatively benign. Only a few are known to be associated with cervical cancer.

The second characteristic is the presence or absence of s*quamous intraepithelial lesions (SIL's)*. These are growths on the surface of the cervix that vary in seriousness, from "low-grade" to "high-grade." SIL's may be precursors to cancer. Oncogenic HPV types first give rise to low-grade SIL's, which may then grow progressively more serious.

Prevalence is the percentage of women in a population statistically estimated to have a given characteristic, such as presence of oncogenic HPV or SIL's. This estimate is inferred from the percentage of such women in the sample.

Incidence, *regression* and *persistence* refer respectively to a statistically estimated increase, decrease or maintenance of the prevalence measured at time point B compared to time point A. Note that a cohort may provide evidence of both incidence and regression, as some percentage of women may "develop" HPV (incidence), but for another percentage HPV may "go away" (regression) even as in another percentage “current types” may still be “present six months down the line” (persistence).

<table>
<tr><td rowspan="3">HIV infected women recruited at the Asian clinic</td><td>Measure at outset</td><td>Measure 6 months later</td></tr>
<tr><td>Particular HPV types
Severity level of lesions</td><td>Particular HPV types
Severity level of lesions</td></tr>
<tr><td colspan="2">Compare changes in measures across time.</td></tr>
</table>

Figure 38: A summary of Naresh’s Protocol Schema.

The figure above summarizes the Protocol Schema, as described by Naresh. Upon completing his description, Naresh turns in the next episode to the topic of “the primary hypothesis.”

Basing our Primary Hypothesis

The talk for this episode is presented in panels in Appendix C in two parts labeled A and B on consecutive pages. A description of the following conventions is listed at the bottom of part A and at the bottom of part B. The transcript is presented in rows and columns, in the reference style of cells in a spreadsheet. The default speaker is listed in the row headings. Thus, unless the panel refers to someone else uttering, assume that it is the person labeled in the row heading. Each screenshot was captured while the speaker was uttering the last word of the panel. When the word being uttered for a given screenshot, it will be underlined. Overlapping speech is indicated by [brackets].

Now for the episode. It is quite short. The purpose for the extreme detail of the recordings and markings of this few moments of talk and interaction is to explore this epistemic, cognitive practice in a way that gives a window into how people in this practice think about the epistopics of prospective and retrospective analysis. Objects get recruited. Which ones and how so is the subject of the episode. It is recommended that one read at this time all five rows of talk in the two pages of the transcript.

The transcript consists of four turns of talk with five rows of panels, one turn in the first two rows and then alternating turns for each row after that of talk after that. In panels 1 and 2, Naresh makes an assertion about the basing of a primary hypothesis. In panel 3, Phillip asks a question to get Naresh to clarify his assertion about this primary hypothesis. In panel 4 Naresh clarifies his original assertion in terms of how he will choose to keep it focused. In panel 5 Phillip recontextualizes this assertion, as he now understands it. That, in a nutshell, provides a summary of the turns as a rhetorical exchange of information. Let us now turn our attention to this information more particularly.

In panel 1A Naresh begins to cross and sweep his arms back and forth in front of himself and over the table. He continues to do so in panel 1B as he utters, “Now, if that’s like the general natural history model.” How should we interpret this sweeping of the arms. First, we should not expect that it have a referential meaning in the same way that we can take a word from text and locate a statement in the dictionary (though it might). On the other hand, it is not plausible that he does so for no discursive, communicational reason. Instead I would suggest, following the ethnomethodologists, that it is an indexical expression whose meaning is a matter local to the interaction (Lynch, 1993). We commonly use words in indexical ways that are beneficially ambiguous, as in, “What are we doing here?” Does “here” mean the room everyone occupies, the building or the country? Any or all depending perhaps on how other talk plays out. But even if this ambiguity is never resolved, usually no one complains or strives to clarify the ambiguity. This ambiguity is a rationale feature of talk inasmuch as any more “specific” reference, such as, “What are we doing in this room?, might encumber the speaker in unwanted ways. For instance, it might induce interlocutors to compare doing things in this room in comparison to doing things in the next room, which in turn might elicit innuendo of some sort.

Likewise, with such a gesture, we should not expect even Naresh necessarily knows how to reference his sweep to a statement of text that one might express offline. As analysts we can at least consider some of the possibilities at to what this sweep might accomplish rhetorically. One interpretation is that the sweeping arms clear the metaphorical space of his prior discussion so that a discussion about something new may ensue. Another interpretation is that the sweeps instantiate the very natural history model he is talking about: rather than evacuating the space, he may be filling it. Still another possibility is that these gestures are only loosely bound to utterances and that therefore definitive meanings should not be assigned to them.

The stance I adopt here entertains all these possibilities. I suggest that people do integrate talk with gesture in purposeful manners in order to convey intentions and meanings more richly. But people are not always conscious of exactly how their words and actions might be taken up by interlocutors or even by their own selves. So it is even possible that Naresh begins to sweep away the general natural history model for the purpose of moving on to the next topic, but as he is doing so, opens a space for considering the relationship between his general history model and the primary hypothesis. In panel 1C, he asks upon which kind of base "we" might metaphorically place a primary hypothesis. It is not clear whether that plural first person pronoun should include just the two epidemiologists or all four collaborators.

Note that he lowers his hands abruptly on the word "exactly," clearly integrating exactness with a gesture that mimics basing the primary hypothesis at a particular location within the space he has associated with his general history model. Here the relation between limbs moved and words uttered is fairly clear. The base of the primary hypothesis is clearly instantiated as a place in front Naresh and the action of basing is clearly articulated as something Naresh and company do.

In panel 2A, he extends his right hand toward Janet as a literal location "where" the "sample size estimates" to be mentioned panel 2B are to be found. In so doing, he binds sample size estimates to her and in turn to the sample size estimate she has brought to the meeting. In panel 2C, on the consecutive words "drive" and "that" he pulses his hand toward Phillip. Naresh here suggests by gesture, a metaphor in which these sample size estimates constitute an agent that will have the force to drive the exact placement of this primary hypothesis. As he completes this speaking turn he looks to Phillip, implicitly offering him the opportunity to take a turn.

In panel 3 Phillip takes up his turn, asking a question for the purpose of clarification. He modifies the noun "hypothesis" with the second person possessive pronoun "your." Thus, for Phillip, Naresh's spoken "we" in panel 1C is restricted to the two epidemiologists. This marks the labor of basing a hypothesis on them alone. While Phillip talks, Naresh joins with Phillip to co-complete the word, "hypothesis." He thereby partially clarifies what he meant even as Phillip seeks clarification: Naresh does indeed mean that sample size estimate will drive the basing of the primary hypothesis. In panel 3A on the word, "what," Phillip points his pen toward the table. He bounces his hand in panel 3B at "hypothesis." In panel 3C Phillip continues the rightward trajectory of the hand as he talks of the possibility of a hypothesis coming into being.

This last phrase, "is gonna be," might appear perfunctory at first: perhaps Phillip is just finishing the sentence he had intended before Naresh anticipated him. But on closer inspection, Phillip deliberately temporalizes what the sample size estimation will drive, in the future. The phrase, "is gonna be," when added to "going to drive," suggests that for Phillip, prior to this discussion, no primary hypothesis yet exists. Naresh and Lynn (the referent of Phillip's "your") must make a choice to bring a hypothesis into being. In composite, Naresh and Phillip relate distinct ontologies for the primary hypothesis. In panels 1 and 2 the composite of talk and gesture reveals that for Naresh there already is a primary hypothesis for which some first person plurality of people must only make a choice, akin to choosing where to place a stake in the ground. Naresh has arranged for personnel, equipment, expertise on HPV and SIL, as well as a field site where patients will show up. The primary hypothesis is just another piece to be arranged for. But for Phillip there is not yet a primary hypothesis and it is incumbent upon his clients alone to bring one into being.

In panel 4 Naresh takes a turn to clarify more. In panel 4A Phillip's "Okay" lets Naresh know that it is now clear to him what Naresh means for the sample size estimates to drive. Naresh appears to anticipate this "Okay" to also signal his turn to have the floor, as his first word, "Yeah" overlaps Phillip's "Okay." Naresh now elaborates that the sample size estimates will drive a choice between two possibilities, one focusing on HPV or one focusing on SIL. Lynn joins him with an affirmative nod that binds her collectively into his project. In Panels 4B and 4C we observe that Naresh indicates by gesture that keeping the study focused on HPV is like taking a path to his right and that keeping it focused on SIL is like taking a path to his left. In this image, it is as if he stood at a fork in the road where the sample size estimate will have a consequential effect on which direction the study will traverse. Again, his first person plural "we" is ambiguously assigned.

In panel 5 Phillip again asks a question for clarification, but this time rephrasing Naresh's statement in panel 4 as tantamount to being able to "see something." Which form the primary hypothesis takes will depend upon what the sample size estimates reveal about which form will allow them to detect a reportable finding. Naresh and Lynn confirm that his appraisal is right. Note that Phillip again demurs to include himself explicitly within Naresh's first person collective: he does not say, "we can see," but rather "you can see." Of course, participants might take the pronoun, "you," to be generic. So it is not so clear here whether this term does the work of putting the onus for determining which focus to take solely upon the epidemiologists alone.

Let us summarize what statistical modeling methods have been enacted up to this point. Collectively, participants have demarcated aspects of method work that belong uniquely to epidemiologists and others uniquely to statisticians. For example, the primary responsibility for basing or for creating the primary hypothesis belongs to epidemiologists. The responsibility for bringing forth sample size estimates to facilitate subsequently consequential decisions about such basing or creating belongs to statisticians. There is also a hint here that Phillip does not approve of letting a sample size estimate drive the basing of a primary hypothesis. He twice asks for clarifying information, suggesting that he finds something unusual about Naresh's approach.

In terms of the epistopics of retrospective and prospective analyses, this short discussion reveals a relation with respect to time and ordering relevant to the epistopics of retrospective and prospective analyses. Here Naresh has already conferred with his colleagues in Asia. He knows approximately how many patients to expect to be enrolled in the study. Now he wants to hear from Janet and then make a decision about the primary hypothesis. Phillip suggests surprise and insinuates disapproval. It seems that he expected Naresh to have come to the meeting with a primary hypothesis ready to put to use. Indeed, later in this meeting he chastises Naresh for, "doing it backwards," explaining that, "We got a sample size and then we're picking a hypothesis." You will note that, with word, "we," Phillip now identifies himself implicitly as going along with Naresh by this later point in the meeting. He is a reluctant participant. In this later short quote we learn explicitly that Phillip is instructing Naresh that basing or picking a hypothesis should precede getting a sample size.

As we shall see, Phillip wants to pick a hypothesis before doing almost anything else. This desire is consistent with normative accounts of prospective analyses as is evident in most introductory statistics textbooks, for example. We shall see that Naresh wants to do almost everything else before picking a hypothesis. This desire is not even consistent with normative accounts of retrospective analyses. That said, Naresh appears to be attuned not to normative texts but to his experience in past practice with Phillip. In that situation, he did in fact do most everything before the final hypotheses were settled upon. It appears that many of the original hypotheses were essentially modified late in the game.

Retrospective Modeling as in the Future

In the previous episode we observed the researchers negotiating who is responsible for picking a hypothesis. They also considered the temporal order of planning sample sizes and choosing hypotheses. As this next episode begins in Appendix C they discuss the temporal order of analyses. At this time it is suggested that the reader read the translation.

Let us begin with a rough account of this episode. As mentioned before, Phillip has been brought along the path of doing things backward somewhat reluctantly. In this episode Naresh first explains that the study they are planning could be like the previous study in many respects. At this Phillip does not object and the others listen impassively. Janet looks at Phillip. Lynn searches for information in her pile of journal prints. But when Naresh tries to put the name, “retrospective,” to the method they might undertake, everyone in the room expresses disapproval or surprise. Both Janet and Lynn look at Naresh. Phillip’s face turns sour. The situation becomes extremely awkward. Naresh quickly apologizes, offers an excuse and revises his presentation of what is actually trying to do. Let us now direct our attention to the transcript in detail.

Naresh imagines that their collaborative work in this study might be similar to their previous one in Africa (turn 1). He first explains this similarity in terms of “studying changes later.” As he speaks Lynn is intently looking over one of the many documents she has brought to the meeting, searching for more information to share with the others. Janet appears to be looking directly in front of or perhaps towards Phillip. Philip leans in toward Naresh with a rather stern look on his face. Just as Naresh finishes turn 1, Phillip moves his body away from Naresh as he sits up straight (note the new position in right pictures in turns 2—4).

Phillip’s verbal response at turn 2 is noncommittal. He indicates assent but his movement away from Naresh indicates dissent. Phillip is no longer with Naresh at the table, but apart from him. This could mean that Phillip is feeling ill at ease with Naresh’s proposal and may be at a point where he no longer wants to keep doing things backwards with Naresh. The top row of pictures shows two simultaneous views of the meeting, from different angles of the room. Both time codes show the precise time at 00:30:42:22. As Naresh explains, he gives a specific name to his idea, “a retrospective cohort study” (turn 3). The reaction of everyone is immediate. As Naresh finishes his suggestion, Lynn looks up to Naresh from her papers with a quizzical expression, Janet turns her head to look directly at Naresh, and Phillip interrupts with overlapping speech, uttering "Well," before puckering his lips into an even more serious and grave expression (turn 4). The lower pictures are also simultaneous at 00:30:50:28, showing that eight seconds elapsed over the duration of turns 2 to 4.

The word, "retrospective," has the characteristics of an offensive word as judged by reactions of all. Just as Naresh utters the word, he seems to freeze, trailing off by holding the fricative consonant "v" for 3 seconds. Meanwhile Phillip is puckering his lips. Lynn appears bewildered. Janet is relatively impassive as she gazes at Naresh. Phillip appears to be extremely uncomfortable. Phillip is usually affable and friendly, but this word has touched a nerve. He is now almost surly. Up to now Phillip has agreed to work with the scientists to help them come pick a hypothesis even though he considers hypothesis picking to be their responsibility and even though it should have been picked before sitting down to meet like this.

It seems that in drawing a comparison to the previous collaborative work on the African data, Naresh is asking Phillip to wait until Lynn returns from Asia with data before he goes about planning upon a hypothesis or a model for analysis. By implication, they would defer on making specific plans now. Phillip may be taking an implication that Naresh wants him to also analyze the data before deciding upon a model for it. By affect Phillip seems stern and reserved while Naresh spoke in turn 1. At turn 2 he becomes more distant still, literally. But still he is reserved. Up to this point, Naresh could have been asking to do things as they had done before. In telling of their past collaboration, Phillip seemed at ease. So even if Phillip thought that doing things as before was not feasible now, he might at least regard Naresh's proposal as a the kind of naïve misunderstanding he expects from scientists.

But by using the word, "retrospective," Naresh seems to be baldly proposing to pick a hypothesis or to choose a model later. Using this word puts his proposal to "be studying the changes later" in a new light. He seems to propose as well to peek at the data before modeling it. Peeking is especially criticized in my experience among statisticians. Following acceptable protocols has a rather heavy moral valence in this profession. The weight of this moral valence could be felt mostly poignantly over these three seconds of Naresh's fricative pause after uttering the word, retrospective.

Naresh's response to this collective expression of surprise was to appeal to common understanding (turn 5). But Phillip interrupts him to make it clear he has no sense of common understanding on this point (turn 6). His is no longer with Naresh. Everyone is now looking at Naresh (turn 6).

Naresh immediately apologizes, complaining of confusion rather than ill intent (turn 7). He expresses a wish to only “salvage something” and by implication not to suggest that anyone bend the rules in his favor. His demonstrative apologizing appeals to the kindness of both Phillip and Janet. They both offer encouraging affirmations (turns 8 & 9, 11) that allow Naresh to save face. At the end of turn 12 there is a long pause of 5 seconds. This pause is longer than the previous but it is not burdened with the tension of that previous one. It is an opportunity provided by all for Naresh to formulate a revised proposal to collaborative work.

Prospective Salvaging

What he states is a capitulation. This episode begins immediately upon the completion of the five seconds of reflection that Naresh takes to gather his thoughts. It is suggested that one read this transcript now.

Naresh now proposes a different way of arranging people, methods, data, patients and clinical diagnoses. In turn 1 he describes a process of gathering the data in the clinics and puts Phillip in a position of having a goal to see what he can get out of it. This description so far seems to be very much like the experience they had with the previous paper. Phillip affirms that he is listening and by implication has no major objections so far (turn 2). In turns 3 and 5, Naresh persists in suggesting that they look retrospectively rather than prospectively. Here Naresh has explained what he thinks “salvaging” is like. It is a matter of extracting whatever information you can after the prospective analysis might have failed to generate a finding. In this telling Naresh is coming to Phillip with the data in hand, ready. By implication, Phillip has not had access to these data up to this point.

He continues to depict a scenario where all the data are assembled and he has them now to hand over (through turn 9). He adds that these results are not available for Phillip to look at (turn 11). He then asks Phillip to now suggest a model that would be appropriate in anticipation of results to be handed over to him in the future (turn 13). By saying so, he instantiates what Phillip would be looking at “from the tables’ perspective" (turn 13 & 15).

This latest version removes some of the problematic implications or insinuations put forth in the earlier proposal. It is not a completely coherent proposal. At first he seemed to be describing a situation such as Phillip had encountered when he took over the project, trying to see what he can get out of a set of data that had already been accumulated (turns 1 – 5). But by characterizing Phillip as blinded (turn 11) and by asking him now what kind of analysis he would do then (turn 13) he appears to have switched course and to have described an epistemic practice more in keeping with Phillip's level of comfort. He is asking Phillip to pick a model now that would be used later to salvage data. From later talk in this meeting, it appears that this proposal never gets taken up. It does not seem to be coherent on its face because it appears to mix a prospective analysis (choosing a model now) within a context of salvaging, a metaphor that could only imply a process of making the best out of a process that failed to go as planned.

Naresh's proposal in this latest example takes on the form of a narrative assembly. He positions HPV types and pap smears and colposcopies from two time points into a collection of data. Naresh then handles these data until they were ready to be choose a statistical model. The data would be blinded for Phillip so that he would choose the model, presumably based on methodological grounds rather than upon the opportunistic grounds that might one might be tempted to use if one were to peek at the data. The basis format of this narrative assembly is remarkably similar to a case we reported earlier (Hall, Wright & Wieckert, 2007, p. 112). This similarity suggests that the metaphor of blinding as a proxy for preserving an analysis in order to avoid the temptation to peek and thus draw opportunistic or errant conclusions is a common one.

Timing is Everything

Regardless as to whether this modeling choice has a coherent sense to the assembled, the positioning of agency is relevant to our discussion of epistopics and recruitment of objects. In this latest episode, Phillip gets to pick a model in advance that makes sense from the perspective of the study design they settle upon (if ever they do settle on one). In this orientation of agency, Phillip takes on a professional role. In this role, he determines whether or not a modeling design makes sense logically. During the meeting, Phillip asks about several things. He asks about sample sizes, hidden causal pathways, associations and kinds of data that will become available. This probing for information reveals the kinds of things Phillip assembles in order to fabricate a model that is justifiable prospectively. Given that Phillip wants to know so many of these details, we can presume that he wants to use this information to plan models in advance. Naresh and Lynn have a professional role in coming up with clinically interesting questions. Phillip is willing to help them out somewhat but only suggests questions when he happens to think of them. He mostly waits to hear what his clients have to say about what is clinically interesting.

Objects have a role here too. There are modeling objects and there are laboratory objects. The activity reported here gives us a window into how people interact with these objects. You might say that Phillip wants to choose a model in advance because he wants to preserve the integrity of the performance of the objects in the laboratory to speak through the model. If he chooses a model retrospectively, then reviewers, funders or colleagues may question the authenticity of the model to present the performance of objects. They may wonder if he merely chose the first model that happened to portray the objects as performing in some way that would be reportable as a finding.

It is notable that Naresh comes around to this more conventional orientation on his own rather than having to be coached. If Naresh is able to come up with an acceptable revision so quickly, it would seem that he already knows that models have to be selected in advance so as to preserve the integrity of the models to depict their performances. So how do we explain his persistent attempts to do things backwards? Naresh has expressed great satisfaction to Phillip for helping him publish papers. So it would seem that he is under some pressure to publish again. Data are difficult and expensive to gather. A sample size of 400 or so subjects sounds like a lot of people. You have to wonder if he simply believed that somewhere amidst all these choices that there must be something publishable to write about. So Naresh is under pressure to publish. Statistical criteria for limiting what kinds of things he can ask of his data may well appear to be arbitrary to him. Think of manners for driving cars. How much is acceptable speeding in the company of decent people? How much is too much? The boundaries for many behaviors are never as simple as standard protocols or traffic laws might suggest.

Naresh has experience working with Phillip. Here Naresh learns that he has read the lesson from the first collaboration in a manner differently than had Phillip. They cannot plan on having to salvage, or plan on having to pragmatically change modeling decisions. If they are going to collaborate together, they are going to have to work on determining a hypothesis or model in advance. It is in this sense that modeling practice is learned in practice. There are several related topics in statistics that all have to do with sequencing in one way or another. One may explore data or do hypothesis testing. The sequence of stating a hypothesis, gathering data and analyzing seems to be a common topic and bone of contention in some other meetings I have observed but have not reported here. Exploring data is sometimes permitted, but then claiming to have found something by such exploration becomes complicated. Here I have tried to open this temporal aspect of statistical practice with an example or two from one meeting.

Throughout the two meetings that we recorded with these researchers, Phillip often advised his client scientists to forego making a major finding of difference between one group and another. The researchers were relentlessly trying to find a configuration of likely differences and adequate sample sizes to likely find such differences. Apparently, such major findings bring more prestige and credit while also perhaps providing more useful information for the field of epidemiology. Over the next few years these researchers did publish a few papers together. Lynn published a paper based on data gathered at the site they were planning on, but on a totally different subject. Naresh and Phillip were coauthors. Naresh published a paper based on a related subject but in a different Asian city with Phillip as coauthor. In both studies, the scientists published findings of prevalence, but did not report any major findings of differences across groups.

CHAPTER VI

EPISTEMIC, COGNITIVE PRACTICES FOR LEARNERS

Introduction

As discussed in the Chapter I, the third aim of this investigation is to translate descriptions about the recruitment of objects in the laboratory to the school. I posed the question, *How can a better understanding of epistemic, cognitive practices in professional settings be made relevant to the development of practical, epistemic, cognitive competencies in educational settings?*

The preceding chapters relate events as they occur in very specific practices for particular people at particular places and times of their careers as researchers. I have argued that the scientists use argumentative forms that have historical origins. In this investigation I have also borrowed from ethnomethodological methods and warrants for characterizing some aspects of activity. As I understand ethnomethodology, it stakes its relevance as a form of sociology on the assumption that in ordinary activity people draw upon shared resources for making sense of what they are doing. Are these resources shared across the whole culture? Not necessarily. But some aspects of shared life must be similar across situations to account for the routine establishment of a sense of being together and interacting among people. Ethnomethodology begins with the assumption that these aspects become visible as people interact. As a method it tends to be modest about general claims.

I have tried to characterize epistemic, cognitive practices in technical terms that are understandable for anyone who cares to dig into them. I have also tried to characterize epistemic, cognitive practices in terms that make the process of knowledge production visible in order that one might then imagine how other knowledge production enterprises hold things together to make for knowledge. I have also tried to follow cognition from the perspective of adaptation to a complex system, when appropriate, in order to characterize one feature of learning that both scientists and educators care a lot about—representational forms, especially graphs, tables and equations.

This has led me sometimes weave together both “conceptualist” and “interactionist” standpoints (Stevens, 2012), two senses of cognition, to make a rough distinction I alluded to and developed in Chapter

I. Into this classification scheme I have placed the Distributed Cognition of Hutchins (1995, 2005, 2010) on the conceptualist side and ethnomethodology, ANT and situated cognition on the interactionist side. As a pragmatic move, I resist the urge to dwell upon the discoverable structure of contradiction between these senses of cognition. I have argued that is it useful to characterize learning as an adaptation to complex system as long the relevant system is fairly stable. If so, then it is productive to follow the interplay between internalized structures and infrastructure with which knowledge-producing learners contend. It is not essential that we model the specific form of these internalized structures in order to productively make use of the idea *that* these internalized structures change. Of course it would be wonderful to understand these mental structures in greater detail, but I think that it is reasonable to argue that internalized structures must be changing in some ways as people come into coordination with artifacts.

This chapter explores a few ways that aspects of epistemic, cognitive practices as observed in scientific settings might inform endeavors dedicated to the cultivation of learners. The cases presented in this investigation were all situated within meetings between statisticians and medical scientists. The statistical, mathematical and scientific topics, as well as the infrastructure relied upon, are more sophisticated than would occur as similar topics are discussed in schools. Nevertheless, I argue that with an appropriate adjustment to infrastructural scale, a focus on the epistemological aspects of cognitive practice may make for a legitimately authentic introduction to scientific practice in schools. I make illustrations among three themes in the Learning Sciences in order to draw some tentative comparisons.

The first theme concerns modeling as an epistemic, cognitive practice. In my major area paper (Wright, 2009a), I surveyed research on learning using models and modeling to structure learning. My conclusion at the time was that modeling practices depicted in this body of work were promising but disappointingly unimaginative of the more performative aspects of scientific practice as depicted in science studies scholarship. I revisit this survey again in light of some ideas developed in this investigation. The second theme is concerned with the ontology of a concept. What kind of a thing is it? By way of answering this question in a very limited way, I argue against making a distinction between things conceptual and things material. This theme also extends upon commentary in my major area paper regarding conceptual practice (Pickering, 1995) as it has come to be understood in the mathematics education literature. I suggest that we mainly appropriate Pickering's primary idea of performance but not his distinction between material and conceptual practice. Third, and finally, I comment on some questions in science education that are directed specifically to learning to engage in epistemic, cognitive practices.

Modeling as an Epistemic, Cognitive Practice

This section considers modeling in education. I compare and contrast three instantiations of modeling as it relates to education, extending on analysis from my major area paper (Wright, 2009a). I focused then upon two major volumes of modeling in education (Lesh & Doerr, 2003; Lesh, Hamilton & Kaput, 2007). These works showed that exploring models is a propitious means for learning subject area concepts and should be regarded as an end in itself. That is, leaning to use models flexibly is itself a learning outcome to be valued. I pointed out that despite the promising, explicit focus on modeling, important aspects of modeling practices as found in science studies literature appeared to be mostly lacking in the accounts presented. In most of the model-based learning activities discussed in these volumes, little attention was paid to modeling in performative terms. Students tended to have presented to them models made by others for them.

Now, perhaps I should have noted at the time that models serve other purposes than the performative function of prediction. For instance, they might more efficiently frame a general body of knowledge that is already pretty well understood in one sense, but whose structure might be better grasped by means of a model. Of special significance would be what the statisticians in the professional settings of this investigation sometimes call “toy models.” These models are characterized as relatively simple in the sense that they are not complicated by the contingencies incumbent to the translation of laboratory measures into data. Such contingencies might have to do with, for example, doubts about the appropriateness of attributing assumptions inherent to the model to the population in question, with controversies about missing data, or with uncertainties about the reliability or validity of measures. Furthermore, they exhibit behaviors that they are able to more or less control. Arithmetic operations or statistical principles of distribution and chance may also be modeled in manners that facilitate investigations of structure of the topic. In these senses, many of the models depicted in the educational literature on modeling have a valid place in pedagogy. But I have argued, especially in Chapter IV, that even mathematical models have performative features that we would do well to explore as opportunities for the design of learning environments. More to the point, practitioners of modeling already understand this sense of performativity implicitly. Becoming an adept modeler may well be a function of acquiring this performative sense to modeling activity.

The figure below is a reprint from my major area paper in which I characterized three depictions of modeling practices in these volumes of educational literature. For each of the three, I pulled details from a particular article in order to illustrate comparisons across depictions.

Activities	Model-eliciting (e.g., Lesh, Yoon & Zawojewski, 2007)	Model-evolving (e.g., Lehrer and Schauble, 2003).	Model-conflicting (e.g., Goldstein & Hall, 2007)
Scope of the longitudinal development of practices. Mathematization.	A few weeks. Students had no historical practice of attempting to form fair volleyball teams for which the construction of a mathematical model could be seen as an improvement. They did not substantially mathematize a previously weakly mathematized practice.	Several years. Students engaged in early activities to describe plant growth involving little or no mathematics or attention to measure, followed by the invention of inscriptions supporting increasingly mathematized models, in order to improve the collective goals.	A decade. Groups differed regarding the validity of the existing model for healthy lizard habitat, offering conflicting alternative models and undermining opponents' models by attacking their motives, their assumptions and the utility of their models' inscriptional conventions.
Generating measures relevant to an epistemic goal.	Students had no hand in determining what attributes of the summer campers were most essential to forming a predictive mathematical model of their performance relevant to selecting balanced teams. Students could not anticipate discovering features of the "world" as the authors describe it.	Students engaged in collaborative efforts (with teaching guidance) to determine which features of plants were most relevant to measure in order to make growth visible. Students made measures while anticipating a future use in graphing and other representations.	Regulatory biologists contested the relevance of measures of reflected radiation obtained by satellites over a restricted terrain and time period, to the question of how to locate acceptable lizard habitat. Their collective goal was pragmatic compromise, as reflected in the notion of habitat land reallocation as a process of "give and take."
Interrogating the reliability of measures as enacted in practice.	Students had no access to measurement activities, nor to the making of measurements commensurate across campers (e.g., How would you consistently note the highest point of a vertical leap?)	Students engaged in measurement activities, in which uncertainties and ambiguities became evident (e.g., Is plant height, width or the best indicator of plant growth?).	Regulatory and local biologists explored the operational definition of measures, such as the conventions for converting bands of reflected infrared radiation to optical "red" on the map of the terrain.
Interrogating the validity of models as enacted in practice.	Students did not participate in any subsequent practice of model evaluation with data from performances of teams selected according to rules of any model. That is, students had no empirical basis for arguing that one model performs better than another at selecting balanced teams.	Students engaged in the collaborative creation of mathematical models for the predicted growth of plants, followed by eventual empirical evaluation of that model, and followed in turn by subsequent cycles of prediction and evaluation.	Groups could not agree on grounds for validating models with existing information. Additional information, generally agreed upon as needed, was not forthcoming within the necessary time frame for relevant decisions to be made.

Figure 39: Three depictions of activities in the literature on modeling

Model-eliciting activities

Most of the modeling practices in this literature on modeling in mathematics education would fall under the leftmost "model-eliciting" column of the figure above. As pedagogy, to effect a model-eliciting strategy is to pose a problem that could only be solved by translating its terms, its questions and its phenomena into a model. These models have a great deal of logical structure and many are translated or translatable into mathematical, symbolic form. The prevailing assumption appears to be that if students are given a good model and are shown how to put it to use, they will acquire a sense of the practice of modeling. Now, students may indeed acquire a somewhat better sense of the "subject matter" within such pedagogical settings, but I argued then and now that they will not acquire a sense or "grasp" (Ford, 2008a) of the practice. In these model-eliciting examples, students do not have substantial experience with (1) a timeframe for an epistemic, cognitive practice to develop, (2) generating measures relevant to an epistemic goal, (3) interrogating the reliability of measures as enacted in practice, or (4) interrogating the validity of models as enacted in practice. These aspects of time, measures, reliability and validity are described in the "Model-eliciting activities" column of the figure above.

In terms of the theme of embodied interaction developed in this investigation, the interface between bodies and infrastructure in model-eliciting activities was rather sparsely populated with links, especially in the sense that students had little or no access to relevant measures and little experience with the modeling situation as an ordinary activity. The illustrated case appears in Lesh, Yoon & Zawojewski, 2007. It describes a model for selecting balanced volleyball squads from a pool of players based upon mock data of players' abilities. I argued that the students dealt only with data, not with measures inasmuch as they had no access to the practice of acquiring the physical information of players.

In terms of the theme of performance, any models developed by students may have been performative in the sense of being able to run the mock data under various scenarios. However, they could not have been performative in the stronger and more meaningful sense of the term as developed here. That is, these models could not be inserted into a practice wherein consequential decisions would depend upon how these models performed under actual scenarios. In the end, the evaluation of the better model was at the prerogative of the instructor, which is to say that the validity of the models was a matter of the agency of that instructor. The network of relations that held this knowledge in place as a legitimate account of some state of affairs in the world, depended crucially upon the judgment of that person and upon the collective agreement that his or her judgment be respected. The expressed goals of most science instruction holds as an ideal that knowledge does not hinge upon the authority of any influential person's say-so. Thus the meta-lesson about knowledge advanced by this approach was not in keeping with the ideals of modern science, as we would probably wish to portray them. The synoptic graphic organizer summarizes this instantiation of modeling.

⇒ Coordination ⇒

Infrastructure includes texts for explaining the assignment, mock newspaper articles to provide a context, calculators and spreadsheets.

Students could create models but there were only limited ways to "run" them or to otherwise compare how they performed.	⇒	No scenario could be constructed to preserve the necessity of the superiority of one model over the other.	⇒	No generality, only an asserted authority in its place.

Internalized structures accommodated interacting with spreadsheets or calculators, interacting with peers in small groups

⇒ Coordination ⇒

⇒

Revised infrastructure: some new models come to be developed and shared with the class, students entertain the idea of modeling to solve a problem.

Revised internalized structures: a process of sense-making for a problem characterizable as applied and interesting to students.

Knowledge produced: depends entirely upon one person's say-so, as no stable configuration of performative objects has emerged.

Figure 40: Synopsis for model-eliciting activities

Model-evolving activities

We now consider a more sophisticated pedagogical practice, as least as far as epistemological concerns go. These are what I called, "model-evolving activities" (middle column in Figure 40, above). They typify an alternative approach to modeling that more fully realizes the potentials of learning that we can discern from science studies literature and from this investigation. In such modeling activities, students have access to the development of models and are able to test their validity (Lehrer and Schauble, 2006a). The illustrative case appears in Lehrer and Schauble, 2003. It involves the progressive development of a model for characterizing general aspects of growth of plants. The students have a problem to be solved that can then be mathematized or modeled in another form amenable to the kind of analysis they wanted to do (mathematization or model-development). In terms of the terminology of this investigation, the students in the illustrative case participated in the building of new representations ("inscriptions") as they built rich interactions at the interface of bodies and infrastructure. Because these models were predictive of future events in the students' laboratory (classroom) the model was subject to the possibility of failure, amendment or validation. These models were performative, then, in the stronger sense because the consensual judgment in the classroom as to their validity, was contingent upon how the models performed as a predictor of events. By posing the evaluation of the model as a consequence of how events unfold, agency for holding together the knowledge of its validity resided visibly with many things, such as with the performance of objects being observed, the collective agreement of the class, as well as with the evolving representational infrastructure. The synoptic organizer below summarizes this kind of pedagogical, modeling activity.

<table>
<tr><td colspan="5">⇒ Coordination ⇒</td><td rowspan="5">⇒</td><td rowspan="5">Revised resources: various inscriptions, from daily notes to tables, graphs, tables, a community of fellow researchers, more knowledgeable instructors.

Knowledge produced: the phenomenon of slow initial growth followed by a spurt followed by slow growth, becomes visible through the production of graphical forms. As this knowledge stabilizes for the particular fast-growing plants, conjectures as to its generality to the growth of other life forms arise.</td></tr>
<tr><td colspan="5">Infrastructure includes plants that reliably grow sufficiently fast for tracking in timeframes for school projects, epistemically-sophisticated instructors, graphical representational forms such as tables and graphs.</td></tr>
<tr><td>Performance: Students could run models and verify that they work as predicted (or not). The acceptance of a model was contingent upon it performing in manners consistent with prediction.</td><td>⇒</td><td>Necessity: Specific data for growth shows an "S"-shaped pattern.</td><td>⇒</td><td>Generality: the "S" pattern is plausibly common for growth of other fast-growing plants.</td></tr>
<tr><td colspan="5">Internalized structures accommodate new ways of seeing patterns in graphs, new dispositions for observing, recording events, and attention to measuring as a practice.</td></tr>
<tr><td colspan="5">⇒ Coordination ⇒</td></tr>
</table>

Figure 41: Synopsis for Model-Evolving Activity

Model-conflicting activities

The last contrast is described under the heading, "model-conflicting activities" (right column, Figure 8). The term derives from the study, Goldstein & Hall, 2007, and is summarized in that column. In the setting in question, community members, landowners and professional biologists contended with models for the impact of human activities upon wildlife in the local environment. The outcome of the activity is instructive to concerns for schooling in the sense that it speaks to the possibility that in epistemic, cognitive practices, people may not be able to come to consensus. In other words, conflicting knowledge claims may persist. Reasonableness of a kind may well have resided with one side or the other, but only if you adopt the perspectives and assumptions of that side to incorporate a sense of things holding together to make something true. This investigation has explored the forging of stable relations among objects in order to make things true. Sometimes these relations cannot congeal and the kind of knowledge sought for does not emerge. There should be room in my view for lessons in modeling practices that fail to wrap up with a neat consensus, that extend a controversy beyond the practical time frame to explore it.

<table>
<tr><td colspan="5">⇒ Coordination ⇒</td><td rowspan="5">⇒</td><td rowspan="5">Possible revised resource: as a collective practice, the building of the collective practice of discussing and arguing about land use might be sustained.

Knowledge produced: contested. Existing land use practices either imperil animal habitat or are benign.</td></tr>
<tr><td colspan="5">Infrastructure includes satellite photographs, imaging technologies and maps, wildlife monitoring data, documentations of historical land use, and the biology of animal behavior and habitat.</td></tr>
<tr><td>Patterns in color on photos emerge from conversion algorithms that translate infrared frequency into visible light colors.</td><td>⇒</td><td>Necessity: colors depict particle sizes of sand for this area (but this is contested).</td><td>⇒</td><td>Generality: colors would depict sand particle sized for other situations in the future so that changes in habitats can be documented (but this is contested).</td></tr>
<tr><td colspan="5">Internalized structures accommodate ways of seeing sand particle size in the color of photos.</td></tr>
<tr><td colspan="5">⇒ Coordination ⇒</td></tr>
</table>

Figure 42: Synopsis for Model-Conflicting Activities

The practical implication for students who experience such controversies is that the science sometimes remains “in the making” (Latour, 1987). There is an understandable fear that taking such an avowedly relativistic stance toward knowledge may be unproductive because students may interpret science as just a matter of opinion. But anecdotally it would appear that the general public is already selectively relativist about much established science. The fault as I see it is not with whatever elements of relativism that has seeped into curricula, but rather with the continued insistence on personal authority in these curricula as a warrant for making claims.

In this section I have revisited some of the arguments I made in an earlier paper in the context of some of the ideas developed in this investigation. I had characterized some of the literature as relatively weak in epistemic terms. Under terminology developed here and explored in epistemic, cognitive practices in medical research, this weakness can be characterized more specifically in terms of agency of objects. In more robust learning model-evolving or model-conflicting situations, students have ample opportunity to experience the performances of objects.

If we regard model-evolution as a kind of instruction, we could say that it is typified by a concerted attempt to develop student understanding by having them contend with epistemic problems. Students learn to coordinate variables, on a graph because they want these compared variables to speak to some claim they are developing about some state of affairs in the world. For instance, they may discover slope as a salient aspect of the graph that speaks to events related to growth. Within the evolution of models, critical topics of conventional mathematics and science knowledge becomes interesting and problematic for students. If we regard model-conflict as a kind of instruction, it may be typified by a concerted instructional attempt to keep a controversy alive in order to teach important mathematics and scientific understandings. This is important to be sure, but it is also important for its own sake to give students a sense of controversies about open questions as an inevitable feature of public life.

If we regard model-conflict as a kind of instruction, we might imagine controversies as avenues for inducing students to focus yet more strongly upon getting their objects to perform more reliably in order to convince others. It gives them an experience and sense of epistemic activity that is almost impossible to convey otherwise. I have characterized this sense as one of contingency and in terms of organizing arguments so that the thing you maintain be both necessary and likely to promote a scope of generality, is seen to pertain.

In this investigation a focal object has been knowledge. I have argued first from historical and interpretive literature and then from empirical case studies, that knowledge is always contingent. With a focus now on learning it is perhaps important to spell out more precisely why this metaphysical point of view may be relevant to educators. Model-conflicting activities as described above involving multiple stakeholders whose interests become bound up with epistemic, cognitive practices are common. As our lives become increasing bound up with questions of science, it becomes increasingly obvious that cultivating a more epistemically sophisticated citizenry will facilitate participatory. Empirical investigations such as this might shed some light on how to characterize the interrelation between learning and knowledge production in ways that educators may use. This is because the goal of inquiries within this technoscientific field of medical science is to produce knowledge and because the practice is depicted in terms of plausible interest to educators. In this investigation we observe the collaboration among researchers and interaction with representational forms. Both collaboration and representational practices are of keen interest to current research into learning in the sciences.

Concepts as Epistemic Objects

Out of a concern for facilitating the learning of mathematics, Learning Sciences researchers have paid increasing attention to dimensions of agency in learners' experience within mathematical settings, such as classrooms. Greeno and a number of other researchers have introduced and explored an especially propitious line of theoretical work around what they describe as "conceptual agency" (Greeno, 2006a; 2006b; Gresalfi & Cobb, 2006; Engle, 2007; Krange, 2007; Hall & Greeno, 2008). By this they appeal to a sense of efficacy or disposition students might acquire toward mathematical concepts, a feeling of connection to them, a belief in one's ability to work productively with them and a sense of emotional security when communicating with others about them. This general sense can also be summarized in terms of a specific legitimacy "to appropriate, adapt, question, and modify conceptual meanings in the discipline" (Hall & Greeno, 2010). In this section I describe how a focus on the agency of mathematical objects may enhance the relevance of this line of work.

The idea of object agency is not entirely lost to those pursuing this line of scholarship. Greeno and Hall (2010) have characterized concepts as taking shape within communities and as migrating across communities, elaborating on changes in concepts over sociogenetic scales of time and space, a view Hall, Wieckert and I developed in the context of concepts in epidemiology (Hall, Wieckert & Wright, 2010). So the concepts with which one connects have a history and are embedded into infrastructure within technoscientific workplaces and pedagogical settings. These conventions have a life of their own in the sense that they propagate outside the direct control of users or collectives of users to modify them.

Drawing on Pragmatist ideas, we presented a view that no sentient agent need direct the spread of such concepts, that they might spread much as viruses propagate. Virus propagation is a useful analogy because its mechanism of dissemination rests upon an integration of information encoded in its RNA with the biological activity of its hosts. The RNA does not "carry" all the information necessary for the virus to reproduce. So, whatever internalized structures related to a concept we may carry in our minds, they are not homologues of that concept and they do not provide for us sufficient means to enact the concept in practice. They are something more akin to viral RNA that relies on the situated setting of its host in order to live and to propagate. Concepts live and spread within situated settings of minds, bodies, human collectives and technological infrastructures.

Except for perhaps in Greeno's collaboration with Hall, Greeno and others do not quite say that concepts are agents, as one might suspect from a plain reading of the term, conceptual agency. Instead, they say that learners can engage with concepts in more ways than we educators usually countenance. Learners might engage deeply with mathematical concepts, taking initiatives when reasoning about them in a generally active stance or attitude. Such learners exercise a high degree of "conceptual agency." One who mostly follows procedures without a sense of connection with the mathematical ideas is not exercising much "conceptual agency." We have much to learn from this and future work on agency among learners of mathematics, together with related work on dispositions and efficacy. But here I will pursue a more literal interpretation of the notion that conceptually related things have agency. However, I will direct my attention to mathematical *objects* rather than to the notion of mathematical *concepts*.

Getting to this more literal interpretation by way of Greeno and others is a bit convoluted. Greeno and others cite Pickering (1995) as the source of the term, conceptual agency. Pickering uses some similar terms, but never puts these two words into consecutive order. He mentions some similar terms that teasingly suggest that the term, "conceptual agency," belongs somewhere. He contrasts "conceptual practice" to "material practice." In the material practice of investigations of cosmic radiation using bubble chambers and of fractional elementary charges on electrostatically suspended oil drops, he makes much of the "material agency" of these objects. By this he means that consequential practical reactions of scientists and their conclusions about the generality of events are contingent upon how these objects perform in practice. What we eventually come to know of the regularity of these performances amounts to "captured" agency, that is, it amount to the production of knowledge. He then discusses "conceptual practice" in the development of the mathematics of quaternions by Hamilton. In this context he only mentions "disciplinary agency" and "human agency" as regimes in which agents perform.

The absence of "conceptual agency" in Pickering's work is conspicuous in the sense that one might reasonably look to complete the following analogy.

material practice: material agency :: conceptual practice: X

Despite explicitly stating an intention to develop an understanding of conceptual practice in terms similar to an understanding of material practice, Pickering never develops a notion of conceptual objects performing in terms comparable to how material objects perform—he never puts "conceptual agency" into the position of X in the analogy. I can only speculate with Frohmann (2004), that the implications of regarding concepts as real, performative things, as objects analogous to material things, was just too much of a metaphysical stretch even for Pickering. Likewise, I can only speculate that Greeno and others made an otherwise sensible completion of the analogy without having found the precise term in the text.

That said, it appears that though Greeno and others completed an otherwise sensible reading of analogical terms, they did not complete a sensible interpretation of analogical meaning. In material practice, materials operate as agents whose performance informs as to how to reconfigure apparatus or how to construe new understandings of physical particles—knowledge is contingent upon the performance of these objects. Conceptual agency, if it were to be derived from Pickering's language use, could only refer to the performance of concepts and to mathematical knowledge as contingent upon subsequently reliable repeated performances of such concepts. But for Greeno and others, conceptual agency appears to refer to discretion enjoyed by people, not concepts. Again, it is worth noting that the cultivation of this human-oriented sense of agency is important, and sadly lacking in too many educational settings.

In my reading of Greeno and others, it seems that Pickering's notion of "human agency" within conceptual practice comes closest to the sense of agency they seek to explicate. In Pickering's illustrative example, Hamilton engages as an active human agent in order to make new connections among geometric objects and algebraic ones, between lengths and numerical symbols. In submission to his mathematical discipline (in Pickering's telling), to "disciplinary agency," he engages passively to the derivational outcomes of novel, tentative arrangements of figures and symbols, waiting to see how these objects get manipulated until a definitive knowledge outcome can be obtained—or not. Exploiting a metaphor of knowledge-generation in geographic terms, Pickering describes a three-step temporal sequence of stages in the exercising of human agency in conceptual practice.

Step one. Human agency of "bridging" between one domain of concepts (from an algebra of numbers expressible as spatial transformations of two-dimensional forms to a prospective algebra of numbers expressible as spatial transformations of three-dimensional forms). Humans here are active, the discipline is passive.

Step two. Disciplinary agency reflected in the member's "transcribing" terms that require some adjustments in order to be sensible within the new domain (from algebraic rules rigorously validated for 2 "couplets," extended to an algebra of "triplets." Hamilton here is passive.

Step three. Human agency of "Filling-in" the new domain with the many new and interesting features concomitant to applying the analogy to it. Hamilton is active, the discipline passive.

For Hamilton, this three-step process was complicated by many "resistances." For example, the triplets never succeeded in his calculations to produce evidence for the geometric relations he wanted them to exhibit. He was forced to "accommodate" to this resistance by inventing a four-part number he dubbed the "quaternion." Incumbent to this new kind of number were some new algebraic relations. For example, quaternions multiplication does not have the property of multiplicative commutation: the product, *ab*, does not have to equal the product, *ba*, in all cases. So quaternions, not triplets, then filled in the new domain space of step 3 with new kinds of mathematical relations.

Pickering depicts scientists in step 2 as merely deriving things out of habit or merely channeling what the discipline insists they must do. My view, as developed in this investigation, is more nuanced. The temporal sequence is one helpful way to think of human agency in processes of knowledge dissemination. But his notion of the disciplinary member passively enacting disciplinary practices is not the only way to interpret this important historical event. In my view, mathematical objects perform most dramatically in the very place where Pickering has the discipline acting through Hamilton. Hamilton is not like some unthinking, "sociological dope" (Garfinkel, 1967), who just does what he is expected to do without knowing why. Rather, Hamilton is an actor in deep coordination with an evolving infrastructure. This infrastructure is populated by novel mathematical objects whose performative possibilities have yet to be explored.

Pickering chooses to illustrate his version of activity with this example because Hamilton left an inspectable narrative record of his discoveries. Let us directly inspect this narrative record ourselves. I extract the following quote from a letter Hamilton wrote to a friend; recounting some of his early encounters with tentatively formed mathematical objects he called triplets (1843). It will not be necessary to follow the specific mathematics, as the comments to be made here on agency are based mostly upon the semantics of clauses only.

> Calling the old root, as the Germans often do, i, and the new one j, I inquired what laws ought to be assumed for multiplying together a + ib + jc and x + iy + jz. It was natural to assume that the product
>
> = ax - by - cz + i(ay + bx) + j(az + cx) + ij(bz + cy);
>
> but what are we to do with ij? Shall it be of the form $\alpha + \beta i + \gamma j$? Its square would seem to be = 1, because $i^2 = j^2 = -1$; and this might tempt us to take ij = 1 or ij = -1; but with neither assumption shall we have the sum of the squares of the coefficients of 1, i, and j in the product = to the product of the corresponding sums of squares in the factors. (1843)

In Pickering's terms, Hamilton actively exercises human agency in order to borrow the root i from the old couplet algebra and to make a bridge to his new, prospective triplet algebra. He crosses this bridge by introducing the object, j, an additional kind of square root of negative 1 distinct from i (first sentence). It would seem that indeed Hamilton then passively succumbs to the learned behavior of his discipline as he found that, "It was natural to assume that the product…" Here he invokes the understanding of the distributive property applicable for the algebra of couplets for the prospective algebra of triplets (as well as some other properties).

But something else occurs that does not fit into Pickering's analysis: Hamilton lets us know that he has let the novel object, ij, perform. To see this performance, notice that Hamilton first poses a problem of action, "what to do with ij? Shall it be of the form $\alpha + \beta i + \gamma j$?" By posing this question he asks what form ij could have so that the product will have an algebraic form of a triplet. A decision as to what to do with ij will be contingent upon how it performs. So, he invokes the agency of the mathematical statement, ij = 1, (itself a mathematical object). By this I mean that he subjects it into the role of initiating the completion of the calculation, not to confirm what he already understands it will do, but observe it to see what it will do. As it happens, the object performs as failing to initiate a calculation that would make for the triplet to be the kind of mathematical object he sought it to be. He then invoked the agency of the statement, ij = -1, obtaining a similar, disappointing, epistemic outcome. Note that in his letter he recounts actions anterior to the time of writing it. At that anterior time, ij = 1 was a statement with uncertain prospects. Would it perform so as to preserve the hope that we shall have, "the sum of the squares of the coefficients of 1, i, and j in the product = to the product of the corresponding sums of squares in the factors"? No. No for the mathematical statement, ij = -1, as well.

Hamilton leaves it to the reader to (literally) figure out how the mathematical statements fail. It is easy to miss the agency of performing objects in his text. If I may insert some information from my own experience, I can relate that the derivations are tedious. They require a few pages of scribbling. You have to have a good sense of error checking and organization to produce anything intelligible. Even Hamilton had to do this. Making these objects speak requires a great deal of coordination with infrastructure. There is indeed a great deal of disciplinary structure to this, so Pickering's description of disciplinary agency is not totally off the mark. However, I would prefer to use other language for the work performed by representations. Agency in this practice is indeed greatly shaped by conventions, and by the various means of organizing and demonstrating. That is, representational forms common to the discipline have agency too. Nevertheless, there is no epistemic outcome without posing the task in contingent terms: what comes to be understood as knowledge is contingent upon how specific mathematical objects such as the product, ij, perform in practice.

We can infer from Hamilton's account that he must have thought at that anterior time that the statements might perform satisfactorily or might not. Hamilton's modeling work, his knowledge of his prospective new algebra, was contingent upon the specific to-be-determined performance of these statements. Hamilton does not have agency to whimsically aver where derivations will lead. Neither did the discipline of mathematics. Upon a close, but plain reading of Hamilton's account, some aspects of agency can only plausibly reside in the mathematical objects themselves within the context of his prospective algebra. The following synoptic graphical organizer summarizes the new knowledge relations described here.

⇒ Coordination ⇒					⇒	Revised resources: new algebraic properties are contemplated and introduced. For instance, the operation of multiplication is not necessarily commutative. Knowledge produced: the triplet does not perform as a geometrically interesting mathematical object under the direction of competent infrastructural resources of computation. However, its failure is productive as it induces a search for what becomes the mathematically interesting quaternion.
Infrastructure includes algebraic properties presumed to pertain to new numbers such as distribution, an existing algebraic-geometric numerical system in two-dimensions.						
The object ij fails to initiate a calculation that would make for the triplet to have geometrical properties analogous to the geometric properties of couplets.	⇒	The necessity of the failure of ij is preserved through a series of calculations conducted by Hamilton or off-line by the reader.	⇒	The generality of this failure pertains to any value for ij, not just to ij = 1 or ij = -1.		
Internalized structures accommodate attention to many mathematical objects in tight formation but organized according to inscriptional conventions and they accommodate geometrical analogues to operations.						
⇒ Coordination ⇒						

Figure 43: Synopsis of the Agency of the Mathematical Object

I think it would be a mistake to conclude that mathematical objects only have agency in the special case of cutting edge mathematics. I want to make a generalization that is far more expansive than this very delimited realm of activity. I have argued that for learners of mathematics, the cognitive practice of deriving is replete with consequential decisions that are contingent upon the specific performance of mathematical objects within the context of arithmetic, algebra or geometry. Obviously, learners tend to encounter mathematics that is more or less settled from the professional perspective. But this settlement, however embedded it may be in computational technologies, takes little away from the prospects learners experience of the possibilities that are contingent upon the specific performance of conjectured mathematical relations.

In Greeno and Boaler’s originating reference (2000) of Pickering’s writings on conceptual practice they quote extensively from Pickering’s three-part geographical description of human agency in conceptual practice. In this article they describe the relevant agencies for learners in terms of human agency and do not use the term “conceptual agency” that Greeno and other later take up. They make special reference to the first and third stages, those in which human agency is active. Thus it would appear that what they what Greeno later dubbed “conceptual agency” more accurately indexes what Pickering calls “human agency” in conceptual practice. To the extent that studies of scientific practice are relevant to theories of learning, getting this intellectual lineage right might matter. Greeno and others have cast their arguments about learning in terms derived from studies of science, so the origin of these terms should be of concern to those wishing to develop these ideas further.

The term “conceptual agency” is manifestly invalid from the point of view of lineage, as Pickering never writes these two words consecutively. The term, “human agency in conceptual practice,” would be a more valid appropriation of Pickering for describing the dimensions of agency Greeno and others explore. However, we have reason to reject the very idea that it is useful to distinguish conceptual practice from material practice, as I argued in my Major Area Paper (Wright, 2009b). This might leave us with just “human agency in material practice,” if we were willing to include the previous conceptual practices into material practices. I suggest that we abandon this lineage altogether, at least as far as appropriating terms goes.

Here is a summary of where we now stand with respect to agency, Hamilton’s story, Pickering’s story about Hamilton, Greeno’s and others’ story about Pickering, and my retelling of all of them. Hamilton recounts his discovery of quaternions, a process that includes several obstacles and failings, including the particular one cited above. Pickering describes a contrast between the material and the conceptual. In material practice, human agents actively rearrange material objects. Then material objects perform while humans passively wait. Eventually we come to know how these material objects will perform reliably and thereby know something about the material world. In conceptual practice, humans actively rearrange conceptual (non-material) objects. The conceptual discipline (not the concept!) then actively works to derive the consequences of these new arrangements while humans passively submit to previously learned practices of the discipline. Yes, Pickering actually argues that humans are passive while deriving. The conceptual practice has a three-part geographic structure. Eventually what we come to know amounts to an expansion of concepts from one geographic domain to a new one.

Greeno and others point to a sense of human agency that the Learning Sciences needed to have described. Unfortunately, they construct a dubious lineage for the idea. Rather than calling it “conceptual agency,” they might have called it “human agency in conceptual practice” even if this latter term is a bit unwieldy. This is because it is humans who exercise, choose not to exercise or are prevented from exercising agency as they mostly describe it. Concepts are not agents in this view.

Greeno and Hall then explore the idea that concepts might enjoy some agency after all. Concepts spread without the concerted orchestration of such spreading from an individual human or collective with an agenda for making that concept spread. Collectives of humans use such concepts to meet local objectives without necessarily intending to propagate the use of such concepts onto a broader scale of activity. But it is hard to exploit the idea of concepts as agents more than this because the idea of a concept presents metaphysical challenges. Greeno and others take positions within theories of learning that would construe as antithetical the idea that concepts are merely mental images, merely textual objects or merely abstract objects existing nowhere in particular. From a situated or sociocultural perspective, a concept or other knowledge object would be necessarily amorphously embedded in its context of use.

If we wanted to take up the intuition that conceptual practice should include active non-human objects just as material practice includes active non-human material objects, concepts make for a problematic candidate. What we need instead is a level of analysis that indexes material forms that people use when doing conceptual work. Almost invariably, when people collaborate over ideas, they pull out paper, point to computer screens or pantomime scenes as they talk about future possibilities. The objects of such worlds are symbols, lines, points, speech, gesture, or the infrastructure such as whiteboards, desks, electricity, or computers. One way to frame this is to say that Pickering was right all along—until he was wrong: there is no reason to mark a distinction between material and conceptual practice. It is all material and it all performs. His illustrative example of the "conceptual" practice of the mathematics of quaternions is just as materialistic as cosmic ray research. Why not just say that in mathematical practice, objects have agency, that people do not wait passively upon what the discipline performs as its intentions are channeled through people, but rather upon what these objects do?

To summarize, Pickering's notion of the contingency of performance (his central idea, in my view) has largely been lost to those who investigate learning and development (for at least one exception, see Ford, 2008). This is partly due to an unnecessary distinction Pickering makes between material practice and conceptual practice. This distinction created an understandable confusion that has limited the scope of influence that Pickering otherwise might have had. Some scholars in the Learning Sciences have taken up Pickering's notion of conceptual practice, but this notion actually undermines his greater argument about the contingent performance of nonhuman agents (Wright, 2009a). The substance of Pickering's distinction is that though material artifacts such as oil drops have agency to perform, mathematical artifacts such as "couplets" or "triplets" do not, presumably because they are deemed to be simply the product of human imagination in a manner that material artifacts are not. For Pickering, numbers only perform in the hands of competent disciplinary agents who manipulate them. I have argued that this distinction is unnecessary. In an effort to rectify this state of affairs without abandoning contingency of performance, I argue that couples and triplets are just as artifactual as oil drops, which is to say in terms of ANT, just as real. Whether a given activity is contingent on their performance depends on how they are positioned and whether anyone has an interest in them. It does not depend upon the relative portions of material essence or conceptual essence we attribute to them.

That said, Pickering's move toward a performative idiom could direct our attention to the contingencies of cognitive practices of modeling. By this I mean that objects do not just perform, people anticipate how they might perform and respond accordingly. It is common, for instance, for researchers to review a plot of data and to interpret it. They make decisions according to what emerges on the graph. Performative objects need not be material. As I have argued, mathematical objects can be performative too. The consequences for this perspective in learning situations are to design instruction so that the performative aspects of mathematical relations become salient. This experience with performativity is one way to characterize the connected sense of knowing mathematics that some students may experience in classroom that cultivate such connections (Boaler & Greeno, 2000) or to characterize the "*procedural*, *conceptual*, and *critical* engagement with content in order to develop *dispositions* towards engaging with statistics" (Gresalfi, 2008, p. 576, italics in original). What makes this emphasis of the performative distinctive from these interesting works just cited is that it decenters agency from the human learners to some extent. In so doing, it allows for a richer sense of human agency in epistemic, cognitive practices as intrinsically about interacting with performative objects.

Developing Practical, Epistemic, Cognitive Competencies

Epistemology and related concerns about agency of learners in science or mathematics has been an ongoing concern in the Learning Sciences (Chinn & Malhotra, 2002; Sandoval, 2005; Duschl, 2008; Ford, 2008a; 2008b; Elby & Hammer, 2010). These concerns derive from what is more or less an emerging consensus that theories of learning should be informed by authentic images of epistemic, cognitive practices within these knowledge-building fields. That said, there are especially few ethnographic studies of scientific reasoning occurring within the real time of talk and interaction, few images that might give greater insight into knowledge building as a materially and socially situated activity. The work in which I have participated has been oriented toward filling this gap (Hall, Wright & Wieckert, 2007; Hall, Wieckert & Wright, 2010). In this investigation I have continued this work with a view towards exploring the agency of objects as constituted within the talk and interaction of researchers engaged in concerted epistemic work. My argument has been that in practice, researchers routinely position objects as doing things or telling things. I suggest that this way of talking is typical in science and that it is more than an epiphenomenal side effect of experimental work. To develop as a researcher is to become increasingly attuned to these ways of speaking and listening. I focus on demonstrations within these meetings as researchers talk over various representational forms such as tables and graphs, but also over embodied, figurative forms we have called narrative assemblies.

One way to pose the importance of understanding agency better, and its relevance to learning and development of epistemic, cognitive practice, is to consider talk about the natural world in scientific work and its constitutive role in the understanding of scientific ideas (Ford, 2008a; 2008b). As Ford explains,

> Through participation in practice, scientists come to *know that* scientific knowledge is held accountable by explicit connections to nature, to *know how* to play the roles of constructor and critiquer appropriately, and to *know that* the interaction of these roles in practice yields reliable knowledge. (Ford, 2008b, p. 405, italics in original)

Ford argues that constructivist commitments and intuitions about learning are appropriate to a point: students must learn to make their own sense of ideas. But he goes on to argue that in science at least, arguments about ideas are held accountable within a community whose communication is organized around making connections to how nature behaves. It is not enough to make one's own sense; one must also form a scientific one. Ford further argues that you already have to have acquired a sense or feel for the construction of scientific knowledge, a "grasp" of practice, in order to construct an appropriate personal sense of the subject area content presented to you in classrooms. This grasp includes a sense of material practice which includes two aspects: setting up contrived and very unnatural devices in order to get natural material phenomena to happen in an observable way and a more rhetorical sense of staging things so that others can discern things as you do.

> Whereas the first aspect of material practice is more about what the scientists do to get nature to "speak," the second aspect is more about the way nature's "voice" gets portrayed, to convince the peer community of the existence of a pattern in nature. (Ford, p. 408, 2008b)

The aim of this investigation has been to describe knowledge-producing activities in interactive and material terms. That is, the aim has been to characterize how objects in nature are made to speak and how scientists voice them as an accountable connection to nature.

This investigation has been carried out as an attempt to make the agency of objects visible for purposes of better understanding the practice of learning to do technoscience but in such a way that we might also understand "doing technoscience" as something that students can also do with the somewhat limited infrastructural resources available to them. In the activities reported here I have recounted instances of mathematical modeling, scientific modeling or statistical method making. Because technoscientific practices rely on vast infrastructures, successful researchers must insinuate themselves into them, learning to hold their own by communicating with others within them. I have focused upon a specific form of communicating within a specific setting within these infrastructures: interactive talk and demonstrations in collaborative research meetings. Here researchers argue or demonstrate that objects in medical clinics or laboratories do things independently of what researchers can impel them to do and that what these objects do, matters. These researchers tell about what these things do, giving voice to objects that would otherwise not have anything to say about public health.

The Learning Sciences could contemplate the agency of objects as a feature of cognitive practices. At present, in the Learning Sciences, people mostly do things to objects, , but objects do not tend to perform. Because the Leaning Sciences do not see objects acting, they do not see people learning to interact with them. In the meetings reported here, I have focused on the rhetorical means that researchers invoke in order to make the performance of objects visible to others. These means usually include equations, graphs, computer graphics and computer code. But they also include more mundane discursive tools such as stories, body positions and gestures of arms and hands. This investigation has framed activity from a perspective of actor network theory (ANT). From a philosophical standpoint, ANT focuses on objects and strives to reimagine them: it is a theory of object-actors connected by networks of relations. In my view this perspective on learning and knowledge production may be appropriate for learning in schools. If so, it may open the study of epistemic learning to more elaborate and interesting comparisons with authentic epistemic, cognitive practices in the sciences.

APPENDIX A

TRANSCRIPTS FOR AGREEMENT AND KNOWLEDGE-PRODUCTION

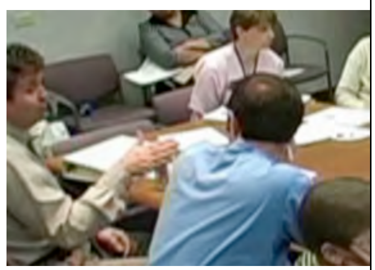
Time codes in mm:ss.s format. White font on picture refers to **word uttered** or word just completed at time of photograph. Capital letters mark INTONATIONAL EMPHASIS. Brackets mark [overlapping speech]. Question marks ?? signify uncertain or ambiguous utterance. Arrows toward hand indicate prior movement; arrows away from hand indicate future movement.

Figure 44: Making Weight's Role Visible

1	Steve	It's probably impossible to ever get zero energy expenditure. Well, let's try it. You, you could do it both ways, um. (turns to his keyboard and monitor)
2	Brian	It's not even close though. (pauses, turns to face Lev). Right? I mean //

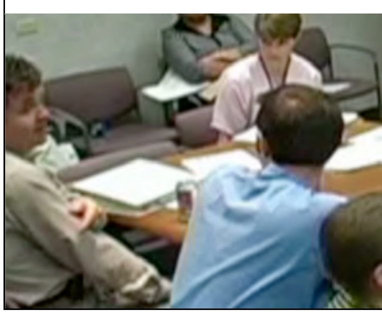
3 Lev: Yeah, because, what, what they do, with this armband
4 Brian: Yeah
5 Lev: They predict, what you said, this resting metabolic rate

5 Lev: from your weight, height, and age,
6 Brian: Yeah.
7 Lev: And then what you get from the movement



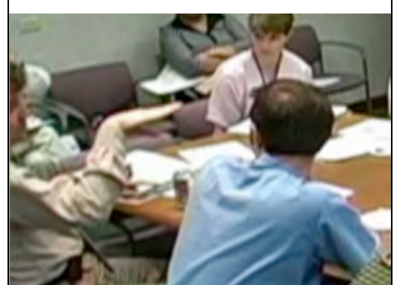
7 Lev: goes [on top of it.]
8 Brian: [On, on top] of it, right
9 Lev: Yeah, and //
10 Steve: [Okay, so] //
11 Lev: [And in] the chamber you can, you can subtract

11 Lev: and just deal with the, if you would use the raw data
12 Brian: Yeah
13 Lev: from the arm, armband.



13 Lev: and compare with this

13 Lev: addition.
14 Brian: Yeah.
15 Lev: Then it would be a different story.



15	Lev	But, but this armband actually does not give you the raw data (looks to Sally) that is accessible… (glances at Brian)
16	Brian	Um
17	Lev	like minute by minute. (looks to Sally, as shown to right) It does? Or it doesn't?
18	Sally	You have to do some calculations.
19	Lev	Calculations.
20	Brian	Calculate it.

Conventions for these texts of utterances are as follows. Overlapping speech is in [brackets]. Interruptions of turns indicated with double slash //. Uttered word at instance of screenshot is underlined. Some turns continue from panel to panel.

Figure 45: Unfolding Data into Measures

APPENDIX B

TRANSCRIPTS FOR NECESSITY AND GENERALITY

Conventions for transcripts in this chapter are as follows. In most panels, the left side is devoted to facsimiles of the hybrid table, equation or calculation. Text in the facsimiles that is inscribed or toward which anyone points during the panel, is highlighted. Overlapping speech is indicated in [brackets]. H&R refers to the Hook and Regal form of the equation (see Figure 22).

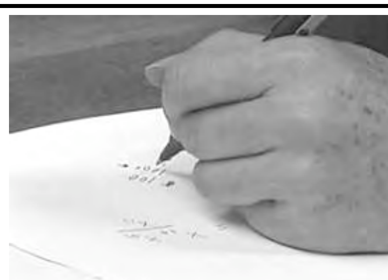
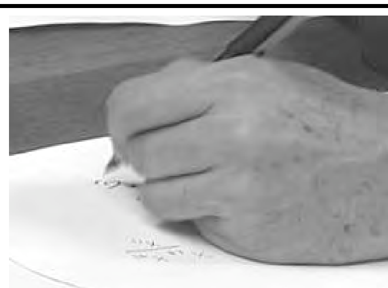
100 100 . . . 100		Ted: Um. I mean suppose (leans to write) let's say that you ended up with a hundred patients. Say you have a hundred patients captured a day (pause) for say seven days. 1
100 100 ⇐ . . . 100		Alberto: Mm hmm. Ted: Okay. And the other method is only available on one day (writes). 2
100 100 ⇐ (33 . . . 100		Okay? And so let's let's say say this is the day and on this particular day um, (pause) you have a you know this breaks down into uh, (pause) let's say um thirty three methods (writes) detected by one method only (points to 100) 3

Figure 46: No Generality panels 1—3.

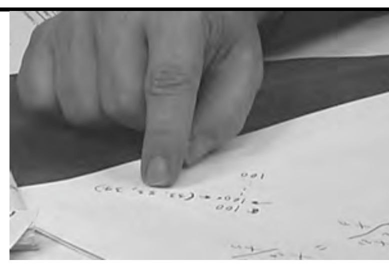
100 100 ⇐ (33, 33, 34) . . . 100		Ted: thirty three method, thirty three detected by the other only (writes) and, uh, (pause) and 34 or however many detected by both (writes, then looks to Alberto). 4
100 100 ⇐ (33, 33, 34) . . . 100		Alberto: Mm hmm. Ted: Okay. Well, if that's the case (traces finger along row) 5
100 ⇒ 100 ⇐ (33, 33, 34) . . . 100		then then what that's telling you (places finger on 34) is that this method (marks with arrow) your bigger method 6
100 ⇒ 100 ⇐ (33, 33, 34) . . . 100		is missing quite a few people (points to 33). 7
100 ⇓ ⇒ 100 ⇐ (33, 33, 34) . . . 100		Because there are, you know, there are, there are thirty three people (marks arrow) who were not caught here (points to 100) 8

Figure 47: No Generality panels 4—8

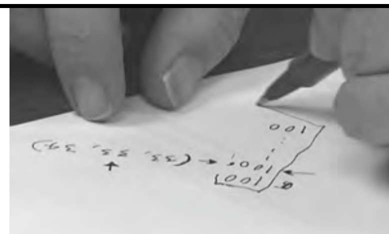
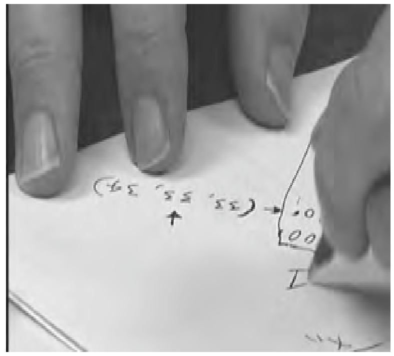
100 ⇓ ⇒ 100 ⇐ (33, 33, 34) . . . 100		Ted: who were caught there (looks to Alberto who remains unmoved.) 9
100 ⇓ ⇒ 100 ⇐ (33, 33, 34) . . . 100		So, um, I mean you know you can play around with this (wiggles finger and pen erratically over the row), and run it through the numbers (gestures toward the computer) 10
100 ⇓ ⇒ 100 ⇐ (33, 33, 34) . . . 100		but but but I think you'll find that that's going to do (begins to sketch box) is it's going to inflate all of these numbers (traces border around left column) by by an appropriate amount. 59:56 11

Figure 48: No Generality panels 9—11

	I		1	2	1&2
	100				
⇒	100	⇐	(33,	33,	34)
	.		(0	0	100)
	.				
	.				
	100				



Ted: Whereas if what you get here is let's say let's say this is screen one (writes "I"). And so, here we have one only, two only and one and two. (writes) And if on this one day your data comes out to be one hundred zero zero. (writes) Okay? [So

Alberto: [Mm hmm]

	I		1	2	1&2
	100				
⇒	100	⇐	(33,	33,	34)
	.		(0	0	100)
	.				
	.				
	100				

Ted: [your] your one day cross-check (wiggles finger along the row, then points to 100 at right) finds exactly the same number of people that you have here (points to 100 in screen-one, phone rings) then your estimate of the missing patients (points to paper from a distance at indeterminate object) is going to be zero (points from afar as he moves toward phone) and your answer is going to be seven hundred (points from afar, answers phone) Hello? Yes? Hey Maggie I I'm I'm in a meeting can I call you back? Bye. (Hangs up phone, rolls back on chair.)

13

	I		1	2	1&2
	100				
⇒	100	⇐	(33,	33,	34)
	.		(0	0	100)
	.				
	.				
	100				
	700				

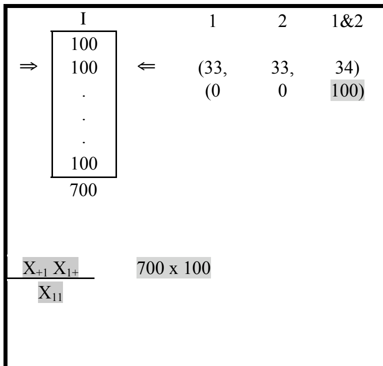
$$\frac{X_{+1}\,X_{1+}}{X_{11}}$$

Alberto: Mm hmm.

Ted: Um. I you know is it worth worth actually running these things through um, you know if through the tables you know to make sure that this (points to row from well-below that row) is in fact in fact correct? But for instance you know here (points to Hook & Regal equation) we have, um, the total number (points to I) detected by method one (points to screen-one) is seven hundred (writes 700 at bottom of column, looks to Alberto).

14

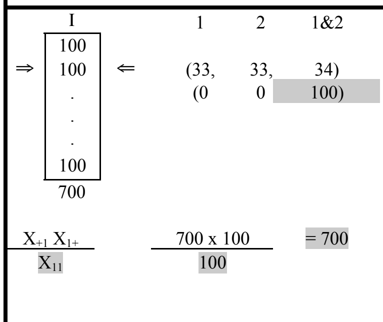
Figure 49: De-Generality panels 12—14.



Alberto: Mm hmm.

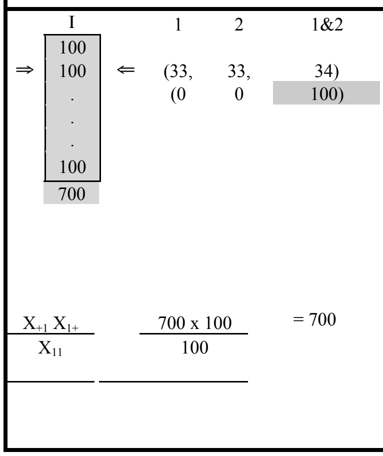
Ted: Okay. So the, um, the the estimate here (points to H&R equation) is going to be (holds finger at H&R equation) this is seven hundred times one hundred (writes 700 x 100) which is this total here (points to 100 on table).

15



Ted: Divided by (writes bar) the number detected by both (points to X_{11}) which is one hundred (writes 100) which is seven hundred (writes = 700),

16



which is exactly what you would hope to do. (Looks to Alberto) Right?

Alberto: (Nods slightly)

Ted: Cause 'cause what this (points to 100) is telling you is that it finds no evidence that you're missing (points to screen-one column) anybody.

Alberto: Right.

17

Figure 50: De-Generality panels 15—17

I: 100, 100, ., ., ., 100; 700 ⇒ ⇐ 1 2 1&2 (33, 33, 34) (0 0 100) 50	Ted: Whereas if um (pause), if let's say uh (pause) uh let's see (pause) let's see how to make the (pause) let me say that this one here is fifty (writes). Um (pause) and uh, (pause) 18
I: 100, 100, ., ., ., 100; 700 ⇒ ⇐ 1 2 1&2 (33, 33, 34) (0 0 100) (0 50 50)	let's say I have fifty here (writes) and let's say zero there (writes) 19
I: 100, 100, ., ., ., 100; 700 ⇒ ⇐ 1 2 1&2 (33, 33, 34) (0 0 100) (0 50 50)	Okay so so I have fifty guys detected by both (points to 50). 20
I: 100, 100, ., ., ., 100; 700 ⇒ ⇐ 1 2 1&2 (33, 33, 34) (0 0 100) (0 50 50)	Um, and fifty guys detected in the second screen (points to 50) just in this one day who are missed here (points to screen-one, the far left column). Okay? Alberto: Mm hmm 21
I: 100, 100, ., ., ., 100; 700 ⇒ ⇐ 1 2 1&2 (33, 33, 34) (0 0 100) (0 50 50) 700 x 100	Ted: So so now my estimate becomes seven hundred (writes 700) as before (points pen to screen-one, the far left column) times a hundred (writes 100) as before, 22

Figure 51: Finding our Question Mark panels 18—22

I ⇒ 100 100 . . . 100 ⇐ 700 1 2 1&2 (33, 33, 34) (0 0 100) (0 50 50)		(wiggles finger between the 50’s) because fifty and fifty is a hundred (points left to right sequentially to each 50). 23
I ⇒ 100 100 . . . 100 ⇐ 1 2 1&2 (33, 33, 34) (0 0 100) (0 50 50)		There are a total of a hundred patients (points to the 50’s) found in the second screen (points to 1&2). (Pause) You agree? Alberto: Mmm. 24
I ⇒ 100 100 . . . 100 ⇐ 700 1 2 1&2 (33, 33, 34) (0 0 100) (0 50 50)		Ted: This number fifty is the number detected by both (points to 1&2) screens. Alberto: Right. 25
I ⇒ 100 100 . . . 100 ⇐ 700 1 2 1&2 (33, 33, 34) (0 0 100) (0 50 50)		Ted: And this is the number detected by the second not detected by the first. (points pen at 2 while holding finger at 50) 26

Figure 52: Finding our Question Mark panels 23—26

	I		1	2	1&2
	100				
⇒	100	⇐	(33,	33,	34)
	.		(0	0	100)
	.		(0	50	50)
	.				
	100				
	700				

So so these guys (writes circle around 50)

27

	I		1	2	1&2
	100				
⇒	100	⇐	(33,	33,	34)
	.		(0	0	100)
	.		(0	50	50)
	.				
	100				
	700				

are found here (pen points at the 1&2) but not found here (pen points at the heading of left column, pause)

28

	I		1	2	1&2
	100				
⇒	100	⇐	(33,	33,	34)
	.		(0	0	100)
	.		(0	50	50)
	.				
	100				
	700				

Ted: And so the total number (wiggles finger between 50’s) detected

29

	I		1	2	1&2
	100				
⇒	100	⇐	(33,	33,	34)
	.		(0	0	100)
	.		(0	50	50)
	.				
	100				
	700				

by the second screen (points to 1&2) is this number (points to left 50 as shown)

30

	I		1	2	1&2
	100				
⇒	100	⇐	(33,	33,	34)
	.		(0	0	100)
	.		(0	50	50)
	.				
	100				
	700				

plus that number.

Alberto: Mm hmm. (Pause)

31

Figure 53: Finding our Quesiton Mark panels 27—31

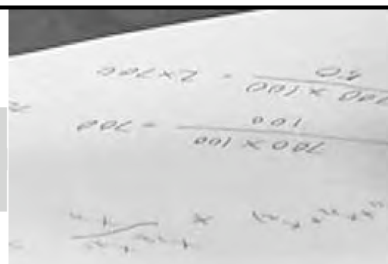
700 x 100		Ted: Okay. Uh, so I have seven hundred times one hundred. 32
$\underline{700 \times 100}$		Alberto: Right. Ted: Now divided by = (writes) 33
$\frac{700 \times 100}{50}$		Alberto: =Fifty. Ted: By fifty. (writes) 34
$\frac{700 \times 100}{50} = 2 \times 700$		Alberto: Mm humm. Ted: Which is equal to two times seven hundred. (writes) 35

Figure 54: Generality Promoted panels 32—35

	I		1	2	1&2
	100				
⇒	100	⇐	(33,	33,	34)
	.		(0	0	100)
	.		(0	50	50)
	.				
	100				
	700				

Ted: But you’ll note that what this is telling you here (points to 50) is that, um,

Alberto: [They are captured]

Ted: [on the second] screen (points to 1&2)

36

	I		1	2	1&2
	100				
⇒	100	⇐	(33,	33,	34)
	.		(0	0	100)
	.		(0	50	50)
	.				
	100				
	700				

Alberto: Right

Ted: we only found [half the guys]

Alberto: [Right, Mm hmm]

Ted: screened here. (Points to screen-one column)

37

	I		1	2	1&2
	100				
⇒	100	⇐	(33,	33,	34)
	.		(0	0	100)
	.		(0	50	50)
	.				
	100				
	700				

Alberto: [So probably the first]

Ted: [So it makes sense] =

Alberto: =Right right =

38

	I		1	2	1&2
	100				
⇒	100	⇐	(33,	33,	34)
	.		(0	0	100)
	.		(0	50	50)
	.				
	100				
	700				

Ted: =that that this screen (points emphatically to screen-one)

Alberto: [is missing]

Ted: [is missing] half of the patients (raises fist)

39

Figure 55: Generality Promoted panels 36—39

$\frac{700 \times 100}{50} = 2 \times 700$		Alberto: [right right]. Ted: And so by blowing these this up this way (points to left side of equation), this makes sense (points to right side). Alberto: Mm hmm. 40
$\begin{array}{cccccc} & I & & 1 & 2 & 1\&2 \\ & 100 & & & & \\ \Rightarrow & 100 & \Leftarrow & (33, & 33, & 34) \\ & . & & (0 & 0 & 100) \\ & . & & (0 & 50 & 50) \\ & . & & & & \\ & 100 & & & & \\ & 700 & & & & \end{array}$		Ted: So the the fact that the probability of a person being captured in the sec in the second screen (points to 1&2) is in this case much less than the probability of being captured here (points to screen-one column) doesn't interfere with the usefulness of this (points to 50) as a way of cross checking that. (Points to calculation) 41

Figure 56: Generality Promoted panels 40—41

42	Alberto: Mm hmm.	
43	Ted: Which is why I would use all of your numbers on on the second screen. (Leans back) I mean it's simpler. (Hands outstretched)	
44	Alberto: Right.	
45	Ted: And it's you know there there's nothing wrong with it. And it's also going to somewhat get around your your small numbers problem.	
46	Alberto: (Smiles) Yes.	

Figure 57: Back to the Science lines 42—46

APPENDIX C

TRANSCRIPTS FOR RETROSPECTIVE AND PROSPECTIVE ANALYSIS

	A	B	C
1 Naresh	 (Facing Phillip). Now, if that’s like	 the general natural history model,	 where exactly are we basing our, uh, primary hypothesis?
2 Naresh	 (Turns to Janet) And I guess that’s where	 (Turns to Phillip) I think the sample size estimates	 are going to drive that.
Default speaker noted in row headings. Frames are noted in document text by row-column. Each picture corresponds to the last word, unless otherwise indicated by an underlined word. Overlapping speech indicated by [brackets].			

Figure 58: Basing our Primary Hypothesis, Part A

	A	B	C
3 Phillip	 They’re going to drive what your	 primary [hypothesis] Naresh: [hypothesis]	 is gonna be?
4 Naresh	 Phillip: *[Okay.]* [Yeah]	 right, I assume. Because whether we keep it HPV focused	 or we keep it SIL focused. (Lynn nods)
5 Phillip	 Okay, it depends on if you can see	 something?	 Lynn: Mm hmm. (As she and Naresh nod in unison)
Default speaker noted in row headings. Frames are noted in document text by row-column. Each picture corresponds to the last word, unless otherwise indicated by an underlined word. Overlapping speech indicated by [brackets].			

Figure 59: Basing our Primary Hypothesis, Part B

1	Naresh	I mean, like all three eighty-five. We see the types here and there. And on, on, on a retrospective, like what we did for our [African country] study, we saw the differences later. I guess that’s what we’re doing right now, even though, you know, whatever sample size calculations we do, we’ll actually be studying the changes later, whether HAART has had any impact on the six months, follow up (Pulls elbows off table, sits upright).	
2	Phillip	Mm-hmm.	 
3	Naresh	Right? So, in effect, it’s, it’s more of a retrospective (makes a trailing vvvv sound, 3 s) [cohort study.]	
4	Phillip	[Well] (Puzzled look on face, puckers lips)	 
5	Naresh	Well, not really, I mean, yeah, but, you know what I’m getting at is, uhm,	
6	Phillip	I guess I don’t. (Turns head, makes contorted smirk)	
7	Naresh	I’m sorry, I’m, I’m, I’m, I’m trying, trying to have too many thoughts in my head at the same time. I’m trying to see if, first of all, my goal is to salvage something out of it. [I mean, I want to]	
8	Phillip	[Yeah, no]	
9	Janet	[(nodding head affirmatively)]	
10	Naresh	[That’s clearly], I mean, I want to get something [whether]	
11	Janet	[Mm hmm]	
12	Naresh	we do it or not. I mean, uhm. (5 second pause).	

Figure 60: Retrospective Modeling as in the Future

1	Naresh	Assuming we do the HPV types and pap smears and colposcopies on all patients at these two time points. For example, if you were given a data set with all these results, and now your goal was to go back and see what you can get out of it,
2	Phillip	Okay.
3	Naresh	rather than, you know, prospectively
4	Phillip	Uh huh.
5	Naresh	looking at what you may be able to design and find out. What would be the best approach? If you, for example, I'm coming at, coming to you six months down the line with everything ready, with [you know],
6	Janet	[Mm hmm]
7	Naresh	all [the results ready],
8	Phillip	[Okay]
9	Naresh	all the HPV types ready.
10	Phillip	Mm hmm.
11	Naresh	You don't know the results. It's kind of blinded to you.
12	Phillip	Okay.
13	Naresh	What would you be looking at from your, you know, from the tables' perspective; from, from the tables that we are to put in the paper? Uh, will we be looking at a multi-variable analysis? You know, I'm, I'm really going only, [only]
14	Phillip	[Uh, huh]
15	Naresh	at the output of it, which is a paper, which means a table which, which is where I'm [actually ??]

Figure 61: Prospective Salvaging

REFERENCES

Alac, M., & Hutchins, M. (2004). I See What You Are Saying: Action as Cognition in fMRI Brain Mapping Practice. *Journal of Cognition and Culture 4*(3), 629—661.

Arminen, I. (2008). Scientific and "Radical" Ethnomethodology: From Incompatible Paradigms to Ethnomethodological Sociology, *Philosophy of the Social Sciences 38*(2), 167—191.

Atran, S., Medin, D. L., & Ross, N. O. (2005). The Cultural Mind: Environmental Decision Making and Cultural Modeling Within and Across Populations, *Psychological Review 112*(4), 744—776.

Barsalou, L. W. (2008). Grounded Cognition. *Annual Review of Psychology*, *59*, 617—645.

Barsalou, L. W., Simmons, W. K., Barbey, A. K., & Wilson, C. D. (2003). Grounding conceptual knowledge in a modality—specific system. *Trends in Cognitive Science, 7*(2), 84—91.

Boaler, J., & Greeno, J. G. (2000). Identity, agency, and knowing in mathematics worlds. In J. Boaler (Ed.), *Multiple perspectives on mathematics teaching and learning* (pp. 171—200). Westport, CT: Ablex Publishing

Chinn, C.A., & Malhotra, B.A. (2002). Epistemologically authentic reasoning in schools: A theoretical framework for evaluating inquiry tasks. *Science Education, 86*, 175—218.

Cocke, W. J., Disney, M. J., & Taylor, D. J. (1969). Discovery of Optical Signals from Pulsar NP 0532. *Nature, 221*, 525-527.

Cole, M. (1996). Cultural psychology: *A once and future discipline.* Cambridge, MA: Harvard University Press.

Collins, H. M., & Yearly, S. (1992a). Epistemological Chicken. In A. Pickering (Ed.), *Science as Practice and Culture* (pp. 301—326). Chicago: University of Chicago Press.

Collins, H. M., & Yearly, S. (1992b). Journey into Space. In A. Pickering (Ed.), *Science as Practice and Culture* (pp. 369—389). Chicago: University of Chicago Press.

Duschl, R. (2008). Science education in three—part harmony: Balancing conceptual, epistemic, and social learning. *Review of Research in Education, 32*, 1-25.

Elby, A., & Hammer, D. (2010). Epistemological resources and framing: A cognitive framework for helping teachers interpret and respond to their students' epistemologies. In L. D. Bendixon & F. C. Feucht (Eds.), *Personal Epistemology in the Classroom: Theory, Research, and Implications for Practice* (pp. 409—434). Cambridge: Cambridge University Press.

Engle, R. (2007). Leadership in the dance of agency during productive disciplinary engagement. *Cultural studies of science education, 2,* 210—218.

Fauconnier, G., Turner, M. (2002). The way we think: Conceptual blending and the mind's hidden complexities. New York: Basic Books.

Ford, M. (2008a). "Grasp of practice" as a reasoning resource for inquiry and nature of science understanding. *Science & Education, 17*(2&3), 147—177.

Ford, M. (2008b). Disciplinary authority and accountability in scientific practice and learning. *Science*

Education, 92, 404—423.

Ford, M. (2012). Dialogic Framing of Scientific Content for Conceptual and Epistemic Understanding. *Science Education 96*(3), 369—391.

Frohmann, B. (2004). *Deflating Information: From Science Studies to Documentation*. Toronto: University of Toronto Press.

Garfinkel, H. (1967). *Studies in ethnomethodology.* Englewood Cliffs, NJ: Prentice Hall.

Garfinkel, H. (1996). Ethnomethodology's Program. *Social Psychology Quarterly, 59*(1), 5—21.

Garfinkel, H. (2002). Ethnomethodology's Program: Working out Durkheim's Aphorism. Lanham, MD: Rowman & Littlefield.

Garfinkel, H., Lynch, M., & Livingston, E. (1981). The Work of a Discovering Science Construed with Materials from the Optically Discovered Pulsar. *Philosophy of the Social Sciences, 11*(2), 131—158.

Goldstein, B. E., & Hall, R. (2007). Modeling without end: Conflict across organizational and disciplinary boundaries in habitat conservation planning. In R. Lesh & H. M. Doerr (Eds.), *Beyond constructivism. Models and modeling perspectives on mathematics problem solving, learning, and teaching* (pp. 57—76). Mahwah, NJ: Lawrence Erlbaum Associates.

Goodwin, C. (1994). Professional vision. *American Anthropologist, 96*(3), 606—633.

Greeno, J. G. (2006a). Authoritative, Accountable Positioning and Connected, General Knowing: Progressive Themes in Understanding Transfer. *The Journal of the Learning Sciences, 14*(4), 537—547. MA: Harvard University Press.

Greeno, J. G. (2006b). Toward the Development of Intellective Character. In E. W. Gordon & B. L. Bridglall, (Eds.), *Affirmative Development: Cultivating Academic Ability* (pp. 17—37). Lanham, MD: Rowman & Littlefield.

Gresalfi, M. (2008). Designing for Dispositions. *Proceedings of the International Conference of the Learning Sciences (ICLS 2008),* (pp. 576—583). Utrecht, The Netherlands.

Gresalfi, M., & Cobb, P. (2006). Cultivating Students' Discipline—Specific Dispositions as a Critical Goal for Pedagogy and Equity. *Pedagogies: an International Journal, 1*(1), 49—57.

Hall R., & Greeno. J. (2008). Conceptual Learning. *21st Century Educational Handbook.* Sage (pp. 212—221).

Hall, R., Wieckert, K., & Wright, K. (2010). How does cognition get distributed? Case studies of making concepts general in technical and scientific work. In M. Banich & D. Caccamise (Eds.), *Generalization of knowledge: Multidisciplinary perspectives* (pp. 225—246). New York: Psychology Press.

Hall, R., Wright, K., & Wieckert, K. (2007). Interactive and Historical Processes of Distributing Statistical Concepts Through Work Organization. *Mind, Culture, and Activity, 14*(1&2), 103—127.

Hamilton, W. R. (1843). Letter to Graves on Quaternions; or on a New System of Imaginaries in Algebra (17 October 1843). *Philosophical Magazine 25*, 489—495. Reprinted in *The Mathematical Papers of Sir William Rowan Hamilton, Volume 3* (pp. 106—110). Cambridge: Cambridge University Press.

Harman, G. (2009). *Prince of Networks: Bruno Latour and Metaphysics.* Melbourne, Australia: re. press.

Herbst, P. G. (2002). Establishing a Custom of Proving in American School Geometry: Evolution of a the Two-Column Proof in the Early Twentieth Century. *Educational Studies in Mathematics ,49,* 283—312.

Hutchins, E. (1995). *Cognition in the wild*. Cambridge, MA: The MIT Press.

Hutchins, E. (1996). Response to reviewers. *Mind, Culture & Activity, 3*, 64—68.

Hutchins, E. (2005). Material anchors for conceptual blends. *Journal of Pragmatics, 37*, 1555—1577

Hutchins, E. (2006). *Imagining the Cognitive Life of Things*, presented at the symposium: "The Cognitive Life of Things: Recasting the boundaries of Mind" organized by Colin Renfrew and Lambros Malafouris at the McDonald Institute for Archaeological Research, Cambridge University, UK 7—9 April, 2006. Downloaded on May 23, 2012, at http://liris.cnrs.fr/enaction/docs/documents2006/ImaginingCogLifeThings.pdf.

Hutchins, E. (2010). Enaction, imagination, and insight. In J. Stewart, O. Gapenne, & E.A. Di Paolo (Eds.), *Enaction: Towards a new paradigm in cognitive science* (pp. 756—798). Cambridge, MA: MIT Press.

Hutchins, E. (2012): Concepts in Practice as Sources of Order, *Mind, Culture, and Activity, 19*(3), 314-323.

Knorr Cetina, K. (1999). *Epistemic Cultures: How the Sciences Make Knowledge*. Cambridge. MA: Harvard University Press.

Krange, I. (2007). Students’ conceptual practices in science education: Productive disciplinary interactions in a participation trajectory. *Cultural Studies in Science Education, 2*, 171—203.

Lakatos, I. (1976). *Proofs and Refutations: The Logic of Mathematical Discovery.* New York, NY: Cambridge University Press.

Lakoff, G., & Núñez, R. (2000). Where mathematics comes from: how the embodied mind brings mathematics into being. New York: Basic Books.

Latour, B. (1983). ‘Give me a laboratory and I will raise the world’, in K. Knorr—Cetina & M. Mulkay (Eds.), *Science Observed* (pp. 141—170). London: Sage.

Latour, B. (1987). Science in action: How to follow scientists and engineers through society. Cambridge, MA: Harvard University Press.

Latour, B. (1990). Drawing Things Together. In Lynch, Michael; Woolgar, Steve (Eds.), *Representation in Scientific Practice* (pp. 19—68). Cambridge, MA: MIT Press.

Latour, B. (1993). *We have never been modern.* Cambridge, MA: Harvard University Press.

Latour, B. (1996a). Cogito ergo sumus! Or psychology swept inside out by the fresh air of the upper deck. Review symposium: Cognition in the wild. *Mind, Culture & Activity, 3*(1), 54—63.

Latour, B. (1996b). On Interobjectivity. *Mind Culture and Activity, 3*(4), 228—445.

Latour, B. (1999). *Pandora’s Hope: Essays on the Reality of Science Studies.* Cambridge, MA: Harvard University Press.

Latour, B. (2005). Reassembling the social: An introduction to Actor Network Theory. Oxford, UK: Oxford University Press.

Latour, B. (2008). The Netz—Works of Greek Deductions—A Review of Reviel Netz's The Shaping of Deductions in Greek Mathematics, *Social Studies of Science, 38*(3), 441—459.

Latour, B., & Callon, M. (1992). Don't Throw the Baby Out with the Bath School! In A. Pickering (Ed.), *Science as Practice and Culture* (pp. 343—368). Chicago: University of Chicago Press.

Latour, B., & Woolgar, S. (1986). *Laboratory Life: The Construction of Scientific Facts*. Princeton: Princeton University Press.

Latour. B., & Strum, S. (1986). Human social origins. Please tell us another origin story! *Journal of Biological and Social Structures, 9*, 169—187.

Lave, J., & Wenger, E. (1990). *Situated Learning: Legitimate Peripheral Participation*. Cambridge, UK: Cambridge University Press.

Law, J. (2004). After Method: Mess in Social Science Research, London: Routledge.

Lehrer, R., & Schauble, L. (2003). Origins and evolution of model—based reasoning in mathematics and science. In R. Lesh & H. M. Doerr (Eds.), *Beyond constructivism: A models and modeling perspective on mathematics problem—solving, learning, and teaching* (pp. 59—70). Mahwah, NJ: Lawrence Erlbaum Associates.

Lehrer, R., & Schauble, L. (2006a). Cultivating model-based reasoning in science education. In K. Sawyer (Ed.), *Cambridge handbook of the learning sciences* (pp. 371-388). Cambridge, MA: Cambridge University Press.

Lehrer, R., & Schauble, L. (2006b). Scientific thinking and science literacy. In W. Damon, R. Lerner, K. Anne Renninger, & I. E. Sigel, (Eds.), *Handbook of Child Psychology, Sixth Edition, Volume Four: Child Psychology in Practice* (pp. 153-196). Hoboken, NJ: John Wiley & Sons.

Lesh, E. Hamilton & J. Kaput, (2007). (Eds.), *Foundations for the Future in Mathematics Education* (pp. 315—348). Mahwah, NJ: Lawrence Erlbaum Associates.

Lesh, R., & Doerr, H. M. (Eds.), (2003). Beyond constructivism: A models and modeling perspective on mathematics problem—solving, learning, and teaching. Mahwah, NJ: Lawrence Erlbaum Associates.

Lesh, R., Yoon, C., & Zawojewski, J. (2007). John Dewey Revisited—Making Mathematics Practical versus Making Practice Mathematical. In R. Lesh, E. Hamilton & J. Kaput, (Eds.), *Foundations for the Future in Mathematics Education* (pp. 315—348). Mahwah, NJ: Lawrence Erlbaum Associates.

Livingston, E. (1999). Cultures of Proving. *Social Studies of Science, 29*(6), 867—888.

Lynch, M. (1985). Art and artifact in laboratory science: a study of shop work and shop talk in a research laboratory. London: Routledge & Kegan Paul.

Lynch, M. (1993). Scientific practice and ordinary action: Ethnomethodology and social studies of science. Cambridge: Cambridge University Press.

Lynch, M. (1996). DeKanting Agency: Comments on Bruno Latour's "On Interobjectivity," *Mind, Culture and Activity, 4*(3), 246—251.

Lynch, M. (2001). Ethnomethodology and the Logic of Practice. In T. R. Schatzki, K. Knorr Cetina & E. Von Savigny, (Eds.), *The Practice Turn in Contemporary Theory* (pp. 131—148). New York: Routledge.

Mol, A. (2002). *The Body Multiple: ontology in medical practice*, Durham, NC, Duke University Press.

Netz, R. (1999). *The Shaping of Deduction in Greek Mathematics.* Cambridge: Cambridge University Press.

Netz, R. (2004). The Transformation of Mathematics in the Early Mediterranean World. Cambridge: Cambridge University Press.

Noss, R., Pozzi, S., & Hoyles, C. (1999). Touching Epistemologies: Meanings of Average and Variation in Nursing Practice. *Educational Studies in Mathematics, 40*(1), 25—51.

Núñez, R. E. (2005). Do real numbers really move? Language, thought, and gesture: The embodied cognitive foundations of mathematics. In F. Ilda, R. P. Pfeifer, L. Steels & Y. Kuniyoshi (Eds.), *Embodied artificial intelligence* (pp. 54—73). Berlin: Springer.

Ochs, E., & Jacoby, S. (1997). Down to the Wire: the cultural clock of physicists and the discourse of consensus. *Language in Society, 26*(4), p. 479—505.

Ochs, E., Jacoby, S., & Gonzales, P. (1994). Interpretive journeys: How physicists talk and travel through graphic space. *Configurations, 1*, 151—171.

Pickering, A. (1995). *The Mangle of Practice: Time, Agency, and Science.* Chicago, IL: University of Chicago Press.

Regal, R. R. & Hook, E. B. (1984). Goodness-of-Fit Based Confidence Intervals for Estimates of the Sie of a Closed Population. *Statistics in Medicine, 3*, 287-291

Sandoval, W. A. (2005). Understanding Students' Practical Epistemologies and Their Influence on Learning through Inquiry. *Science Education, 89*(4), 634—656

Shapin, S., & Schaffer, S. (1985). *Leviathan and the air—pump: Hobbes, Boyle, and the experimental life*. Princeton, NJ: Princeton University Press.

Sperber, D., & Hirschfeld, L. A. (2004). The cognitive foundations of cultural stability and diversity, *TRENDS in Cognitive Science, 8*(1), 40-46.

Stengers, I. (1997). *Power and Invention: Situating Science*. Minneapolis: University of Minnesota Press.

Sterne, J. A. C., & Smith, G. D. (2001). Sifting the evidence—what's wrong with significance tests? *British Medical Journal, 322*, 226—231.

Stevens, R. (2012). The Missing Bodies of Mathematical Thinking and Learning Have Been Found. *Journal of the Learning Sciences, 21*, 337—346.

Stevens, R., & Hall, R. (1998). Disciplined perception: learning to see in technoscience. In M. Lampert and M. Blunk (Eds.), *Talking mathematics in school: Studies of teaching and learning* (pp. 107—149). Cambridge, UK: Cambridge University Press.

Strum, S., & Latour, B. (1987). The meanings of social: From baboons to humans. *Information sur les Sciences Sociales, 26*, 783—802.

Wright, K. (2009a). *Model-Enacting Activities and Modeling Practices.* A major area paper for qualification of candidacy for doctoral program, Vanderbilt University.

Wright, K. (2009b). The Performance of Concepts, Especially Mathematical Ones. Paper presented at the annual meetings of the *American Educational Research Association*, San Diego, California.