TOWARDS A GENEROUS* DISCUSSION OF INTERPLAY BETWEEN NATURAL DESCRIPTIVE AND HIDDEN MACHINERY APPROACHES IN KNOWLEDGE AND INTERACTION ANALYSIS

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This chapter explores different orientations to research practices that are present in both Knowledge and Interaction Analysis. Rather than rehearsing the familiar controversy between individual and sociocultural theories of knowing and learning, we analyze differences associated with what we call “natural descriptive” (ND) and “hidden machineries” (HM) orientations. The writing was undertaken as a conversation among the authors, following a series of lively (and sometimes difficult) conversations with others in the KAIA community. The text came in pieces, with many gaps and alternate paths taken that are not reflected here. We have tried to smooth the remaining pieces together in a way that reflects some of our discoveries along the way, starting by characterizing the work of hidden machineries and natural description in scientific practices, and then illustrating the latter in two historical cases. Next, we pose a series of generous* questions for exploring differences and possible interplay between these two orientations, using cases from previous studies in the learning sciences. We end with an invitation to think differently – in generous* ways – about these differences, in efforts to integrate Knowledge and Interaction Analysis in studies of learning and teaching.

Hidden machineries approaches postulate a domain operating either above the phenomena of interest – such as cultural scripts in anthropology, generative rules of universal grammar in linguistics, or relations of production in Marxist history, or beneath the phenomena of interest – such as interactions between atoms and light producing color patterns or mental structures in cognitive psychology. The
main goal of HM approaches is to identify structures and processes in these hidden domains that have predictive and causal powers over the phenomena of interest. Natural descriptive approaches, on the other hand, focus on close examination of the phenomena of interest, noticing unexpected relationships and documenting previously unnoticed cases and circumstances. It is key to note that we are not mapping HM approaches to “theory” and ND approaches to “observation.” Theorizing and observing are germane to both. Theories in HM tend to be models of postulated mechanisms, whereas theories in ND are means to highlight phenomena and relations that had previously remained unnoticed or without broad significance.

We argue that: (1) natural description and hidden machineries are both legitimate enterprises with their own traditions, methods, and diverse professional communities; (2) rather than a hierarchical (i.e., one being “better”) or developmental (i.e., description comes first, explanation later) relationship, they are parallel strands of work that benefit from interplay with one another; and (3) in order to enable this interplay it is crucial to acknowledge and grasp the cultural and historical roots of natural description and hidden machineries perspectives.

Interplay Between Natural Descriptive and Hidden Machineries Orientations

Method? What we’re dealing with here is not, of course, just method. It is not just a set of techniques. It is not just a philosophy of method, a methodology. It is not even simply about the kinds of realities that we want to recognise or the kinds of worlds we might hope to make. It is also, and most fundamentally, about a way of being. It is about what kinds of social science we want to practise. And then, and as a part of this, it is about the kinds of people that we want to be, and about how we should live (Addelson 1994).

Method goes with work, and ways of working, and ways of being. I would like us to work as happily, creatively and generously as possible in social science. And to reflect on what it is to work well.

(Law, 2004, p. 10)

We intend to be generous*, but we set out in this exploration with an extensive background of writing about what science properly consists of and what it is for. Normative or idealized accounts of scientific practice, as we have learned from the social studies of science, may not adequately describe what scientists know, do, or learn. As Law (2004) points out, scientists inhabit their own cultures, with different personal commitments and broader, public values. In this section, we consider historical cases of the interplay between ND and HM orientations. Our purpose is to explore a more open relation between these
orientations, in order to further a discussion about how these differences can be leveraged in new ways.

Our paper is a reflection on the philosophy of sciences, which we hope will enrich the ongoing exchanges in the KAIA community. This reflection arose as we wondered how differences between practitioners of Knowledge Analysis and Interaction Analysis could be productively grasped and described. On the face of the KA and IA labels, their corresponding differences seem to make little sense. How would it be possible to examine knowledge if not through the interactions among knowing people and their materials? Conversely, can there be a way of making sense of interactions regardless of what participants know, as evidenced by their talk and skilled actions? Another difference commonly discussed in our field is between individual and social; is that the one that applies here? We think that the prevalent discourse about individual/social distinctions has become stereotyped or fossilized. For instance, the same cognitive structures proposed by a learning researcher can, in principle, be ascribed to each of the tested individual minds, or to the individuals’ internalization of structures historically established by their society or their professional communities. In this paper, we attempt to articulate another distinction that seems to offer, in our opinion, a language for a generous and inclusive treatment of differences – the one between what we call “hidden machineries” and “natural description” approaches.

Before we characterize these approaches, let us be clear that we are not trying to establish a foundational or a priori dichotomy. The distinction we propose is provisional and open to refinement. In fact, it is easy to find cases that do not fit, that fit both sides of the distinction, that seem indifferent to it, and so forth; the value of this distinction is that it may open up fresh questions and provoke a generative and respectful exchange.

In drawing these distinctions, we intend to describe scientific worldviews that can be different and equally productive. We do not believe, for example, that what we call a hidden machineries orientation is deeper or more fundamental as a guide to scientific knowing than are natural description approaches. While these approaches may lead investigators to different epistemic stances, we believe they can be mutually informative, and we will give historical examples of this. The sense we want to invoke with the term “hidden” is not so much that the layer of reality causing the phenomena of interest is out of sight, but that it is “behind” or “above” observable phenomena, on another relatively autonomous stratum. For instance, cells, which are nowadays directly visible with proper instruments, have their own dynamics and are regulated by specific biological rules and principles — which is what “machinery” alludes to. The latter can causally explain, say, symptoms reported by a person construed as a supra-cellular phenomenon.

In natural description approaches, on the other hand, practitioners strive to discover phenomena that had previously remained unnoticed or without broad significance. Goodall’s studies of a community of chimpanzees at the Gombe Stream Park in Tanzania are a popular example of natural description research. The term
“natural” indicates that practitioners struggle to verify that observed phenomena are not merely artifacts of their own choices, methods, and instruments. For example, Goodall’s observations about chimpanzees’ aggression have been criticized as a result of feeding stations used at the Gombe Stream Park, suggesting that war-like behaviors observed by Goodall were not “natural” among chimpanzees. This controversy was explored further by other studies using natural description. Note that this use of “natural” is different from the one invoked by the phrase “natural science” which is often taken as a synonym for “non-social science.” The quality of natural description studies we want to foreground is that they do not posit layers of causal explanation hidden behind the realm of observed phenomena. Avoiding this kind of explanatory stratification could be said to make natural description approaches “ontologically flat” (i.e., explanations remain meshed with the observable ontology).

To illustrate diverse and complex relationships between natural description and hidden machineries approaches, we chose two examples from the work of highly accomplished scientists: Charles Darwin and Ramón y Cajal. The broad pattern of discovery that we find in these two cases is consistent with the ideas that: (1) natural description and hidden machineries co-exist, and (2) they are not related by developmental sequence.

**Darwin: The Mockingbirds**

On September 15, 1835, the research sailing vessel, HMS *Beagle* reached the Galapagos island chain. It visited several islands over a period of five weeks. Darwin, who was then 26 years old, worked to gather specimens and observations on the geology and biology of the archipelago. His servant hunted 65 birds, Darwin dissected the specimens and wrote notes, and in most cases only their skins were brought back to England. At the time Darwin did not have in mind the geographical distribution of species as a major focus of study. Looking back, Darwin remarked how he came to notice:

> [T]he most remarkable feature in the natural history of this archipelago … the different islands to a considerable extent are inhabited by a different set of beings. My attention was first called to this fact by the Vice-Governor, Mr. Lawson, declaring that the tortoises differed from the different islands, and that he could with certainty tell from which island any one was brought. I did not for some time pay sufficient attention to this statement, and I had already partially mingled together the collections from two of the islands. I never dreamed that islands, about 50 or 60 miles apart, and most of them in sight of each other, formed of precisely the same rocks, placed under a quite similar climate, rising to a nearly equal height, would have been differently tenanted.

*(Darwin, 2011, p. 162)*
Even though Darwin had “mingled” the birds from different islands, there were four of the “mockingbird” type, for which he had recorded their island of origin. Three of these four birds were very similar in appearance, and Darwin was unsure whether they represented different species (Sulloway, 1982, p. 349). In 1837 Darwin delivered his collection of birds and mammals to the Zoological Society of London. The birds were subsequently studied by an expert ornithologist, John Gould, who found that most of the Galapagos land birds were “forms new to science and confined exclusively to the archipelago” (p. 359). In particular, he described the “distinct insular form” (p. 379) of the mockingbirds.

Although no unique event or insight during the voyage of the Beagle, or its aftermath, can be said to have prompted Darwin to develop the theory of evolution, he wrote in his autobiography how he was struck by:

the manner in which they [species] differ slightly on each island of the [Galapagos] group; none of these islands appearing to be very ancient in a geological sense. It was evident that such facts as these, as well as many others, could be explained on the supposition that species gradually become modified; and the subject haunted me.

(Darwin, 2006, pp. 71–72)

The Origin of Species, published 22 years later, includes Darwin’s (2009) remark that in the Galapagos archipelago:

almost every product of the land and of the water bears the unmistakable stamp of the American continent. There are twenty-six land-birds; of these, twenty-one, or perhaps twenty-three are ranked as distinct species, and would commonly be assumed to have been here created; yet the close affinity of most of these birds to American species is manifest in every character, in their habits, gestures, and tones of voice.

(pp. 384–385)

Darwin compared these observations with the Cape Verde archipelago, which bears resemblance to the Galapagos in terms of latitude, climate, size of the islands, etc., but in which the inhabitants are closely related to African species. Species differences could be explained by assuming that the fauna and flora of the two archipelagos were descendent “with modifications” from American and African ancestors. Darwin proposed descent with modification, coupled with the geographic isolation of the islands, as the origin of new species in these archipelagos.

While his explanation can be understood to involve a “mechanism,” it is important to realize that it does not invoke a subjacent domain, or a distinct ontological layer, causing new species to appear. In other words, Darwin’s account of evolution did not depart from his natural description of geological and biological phenomena. Only much later were hidden machineries of evolution proposed,
in the disciplines of molecular biology and genetics, emerging from the work of new communities with expertise in technologies and laboratory settings that flourished during the second half of the twentieth century. In this historical case, hidden machineries approaches followed the discovery of an integrative theory of evolution using a natural description approach.

**Ramón y Cajal: The Neuron Doctrine**

The case of Ramón y Cajal and the neuron doctrine illustrates how technical developments, in many cases generated by theories embedded in hidden machineries approaches, allow for the opening of new realms to natural descriptive work. Based on extraordinary developments in optics and histology, Ramón y Cajal pursued tantalizing natural descriptive explorations of the nervous tissue.

In 1673 Antonie van Leeuwenhoek mailed his first letter to the Royal Society’s Philosophical Transactions describing observations with his self-made microscopes. Over the next 50 years, he kept reporting to the Royal Society, becoming famous all over Europe and gaining recognition as a leading scientist. But his microscopes were difficult to use – they required steady hands holding them at a correct angle with respect to a light source and great visual acuity. Van Leeuwenhoek kept secret the specifics of microscope fabrication and “following his death in 1723, there was little scientific use of the microscope until Joseph Jackson Lister (1786–1869) developed the achromatic objective during the 1820s” (Reeves & Taylor, 2004, p. 1103). Different colors refracted differently in microscopes, creating blurry images with optical artifacts. Achromatic lenses were constructed by joining halves made of two different materials such that they neutralized, to a large extent, differences in refractive deviation. Initially used successfully in telescopes, achromatic lenses in microscopes took much longer to develop because of difficulties posed by their small size.

The dismissal of microscopy among anatomists and medical professors was not only rooted in technical hindrances, but also in prevalent cultural traditions. Ramón y Cajal described how, as a student of medicine in 1877,

> I was completely surprised by the almost total absence of any curiosity on the part of our professors, who spent their time talking to us at great length about healthy and diseased cells without making the slightest effort to become acquainted visually with those transcendental and mysterious protagonists of life and suffering. What am I saying! – Many, perhaps the majority of professors in those days, despised the microscope, even considering it prejudicial to the progress of biology! In the opinion of these academic reactionaries, the marvelous published descriptions of cells and of invisible parasites were pure fantasy.

*(Ramón y Cajal, 1937/1989, p. 252)*
As a newcomer to the field – open to different and emerging research practices – Ramón y Cajal described his fascination with the new worlds he discovered:

There was presented to me a marvelous field for exploration, full of the most delightful surprises. With the attitude of a fascinated spectator I examined the blood corpuscles, the epithelial cells, the muscle fiber, nerve fiber, etc., pausing here and there to draw or photograph the most captivating scenes in the life of the infinitely small.

(Ramón y Cajal, 1937/1989, p. 252)

While microscopic studies of plant and animal tissues had led to wide acceptance of the theory that all living organisms were primarily made of cells, “there was plenty of room for exceptions to the rule. Foremost among these appeared to be the nervous system” (Shepherd, 1991, p. 25). The main reason was that “gray matter is formed by something like a very dense felt of excessively fine threads; and for following these filaments thin sections or completely stained preparations are worthless” (Ramón y Cajal, 1937/1989, p. 310). Camillo Golgi’s discovery of silver nitrate staining in 1873 provided Ramón y Cajal with a tool for close descriptive exploration of what had previously appeared to be a uniform fabric of felt-like tissue. A surprising benefit of the Golgi stain was that it impregnated only a few (1 to 5%) of the nerve cells:

To follow the entire course of a long nervous process, it is necessary to prepare very thick sections. If the technique had impregnated all the nerve cells present in such a section, it would have been impossible to follow individual nerve processes among the inextricable tangle of all the others.

(Pannese, 1999, p. 133)

As Ramón y Cajal set to work with silver nitrate stains, he favored the view that nervous tissue was composed of fully formed and self-contained cells, rather than a network of continuous protoplasmic connections, which was a competing theory favored by Golgi. Working with less dense, embryonic nervous tissue in microscopic studies over a period of about four years, Ramón y Cajal published a collection of papers describing cellular structures that were visible using the new stain:

The continuity of substance between cell and cell being excluded, the view that the nerve impulse is transmitted by contact, as in the junctions of electric conductors, or by an induction effect, as in induction coils, becomes inescapable…. If the method is applied before the appearance of the myelin sheaths upon the axons (these forming an almost insuperable obstacle to the [Golgi] reaction), the nerve cells, which are still relatively small, stand out complete in each section; the terminal ramifications of the axis cylinder are depicted with the utmost clearness and perfectly free.

(Ramón y Cajal, 1937/1989, pp. 322–344)
Santiago Ramón y Cajal’s papers included careful illustrations of the visible structure of nerve cells and their physical connections, but his efforts of natural description were finally cemented when he traveled to meetings of the German Anatomical Society at the University of Berlin. He did not present a paper, but instead offered demonstrations using several microscopes set up to explore tissues using the Golgi stain. Despite his halting mastery of French, Ramón y Cajal managed to demonstrate the fine, cellular structure of nervous tissue to leading professors who attended the meeting. Despite their initial skepticism, “when there had been paraded before their eyes in a procession of irreproachable images of the utmost clearness … the prejudice against the humble Spanish anatomist vanished and warm and sincere congratulations burst forth” (Ramón y Cajal, 1937/1989, p. 356–7). Ramón y Cajal’s success with a natural description approach eventually resulted in a Nobel prize, which he shared with Golgi (controversially) for work on the fine structure of the nervous system.

The two cases, Darwin and the theory of evolution and Ramón y Cajal and the neuron doctrine, illustrate the diversity of natural descriptive approaches across disciplinary fields in the natural sciences, and their interplay with hidden machinery types of work. At the same time, and while the distinctions we discuss are fluid – there are surely boundary cases and counter-examples – it also seems reasonable to recognize that there may be differences in how the two approaches mesh with diverse scientific practices. One apparent difference is that hidden machineries in the social sciences – examples include Piaget’s schema, Lakoff’s conceptual metaphors, diSessa’s p-prims, Chomsky’s rules of generative grammar, or Fauconnier’s blends – remain a concern of relatively small scientific communities, and in this way are distinct from ideas which originated in the natural sciences that have become “normalized” by society at large, such as the various kinds of forces, bacteria, or molecules that are commonly invoked in public discourse. Another difference is that hidden machineries in the natural sciences have often become examinable, through various tools, in multiple and mutually independent ways (e.g., viruses, DNA). It is difficult to find clear precedents in the social sciences for hidden machineries that could be examined apart from the social behavior that expresses them.

One of the great hopes of the cognitive revolution initiated in the 1950s, based on the premise of a foundational similarity between brains and computers, was that the hidden machineries of cognition were to be found in the form of programmable codes (Gardner, 1986). In similar fashion, a recurring hope is that progress in cognitive neuroscience will allow for the discovery of the hidden machineries of psychology, education, and culture in expressive capacity of neuronal populations and their connections. Neuroscience findings, particularly in the form of brain imaging, are increasingly positioned in our society as precursors to a causal explanatory framework for why humans and animals behave the way they do.
It is possible, of course, that future hidden machineries emerging from the social sciences will be scrutinized apart from social behavior in multiple independent ways and that they will be widely accepted as explanatory, but the question of why this has not been the case so far remains open and relevant. Certainly it is not because of the lack of effort on the part of many practitioners of the social sciences. In order to open up discussions about this asymmetry, we will reflect on three explanations that have been offered: (1) the human sciences are younger, (2) the mathematics needed for the articulation of hidden machineries in the human sciences have not yet been developed, and (3) culture is inherently holistic and does not allow for an analytic stratification in which one layer causally explains another.

**Youth**

A frequent argument is that the human sciences are newer, which explains why they are still unprepared for work in hidden machineries. This argument assumes that there is a developmental sequence culminating in the articulation of hidden machineries that has to be traversed by scientific disciplines. This thesis contradicts the simple historical fact that the social and natural sciences emerged around the same historical period (Cohen, 1994). We have tried to dismantle this developmental model through some of the preceding examples.

**Mathematical Support**

This argument is based on the observation that formalism in general and mathematics in particular are often crucial for the articulation of hidden machineries. Sometimes a branch of mathematics has been developed for the sake of a hidden machinery approach (e.g., Newton’s invention of calculus); at other times a corpus of mathematical ideas has been found already in place for this purpose (e.g., Einstein’s use of non-Euclidean geometries in relativity theory). The inference is that the mathematics needed for the articulation of hidden machineries in some of the human sciences is still to be invented (or discovered, depending on one’s philosophy of mathematics). Perhaps stemming from this perspective, the explosive growth of non-linear dynamics – a relatively new branch of mathematics over the last 60 years – prompted some psychologists to develop new lines of research to explain psychological development. Similarly, the “social networks” approach to studying the dynamics of social organization has been fed by mathematical and computational advances in graph theory.

Economics is a fascinating case for the relationship between mathematics and hidden machineries. For many of its practitioners, economics is an exact science. This conclusion is sometimes supported by the observation that quite a few mathematicians work for companies operating in financial and investment...
domains, or by noticing that a crucial target of work in economics is the mathematics of decision making (e.g., game theory). But on the other hand, many economists see their subject as shaped by cultural and historical forces that do not appear amenable to formal laws, such as Weber’s well-known thesis linking the rise of European capitalism to the work ethics of Protestantism.

**Cultural Holism**

We group in this category those views according to which culture is inherently holistic in the sense that it does not allow for a stratification of layers, some operating as causally explanatory of others. The associated conjecture is that hidden machineries approaches necessitate such stratification in order to be workable. For example, according to this holistic view, postulated principles regulating the economy could not causally determine cultural dynamics, because culture and history themselves shape economic phenomena. Or, given the extraordinary plasticity of the nervous system, it seems impossible to discern whether animals’ behavior is dictated by their neuronal networks or whether these networks emerge from life in certain environments and communities, or whether they co-emerge in non-striated ways, which amounts to posing again the age-old nature/nurture debate. It is consistent with the thesis of cultural holism that the social sciences could be the “advanced” ones, in the sense that the natural sciences might encounter, at some point in the future, holistic edges undermining the consensus about stratified hidden machineries.

What we have called the interplay between natural descriptive and hidden machineries approaches to scientific work has historically been both more diverse and more productive than adherents to either Knowledge or Interaction Analysis might typically acknowledge. As our conversations around this chapter have continued, we have tried to remain open to what we perceive as a driving or motivational force for researchers involved in this attempt at integration, particularly around what typically counts as an explanation in the research that we do. In the remainder of this paper, we take up a series of generous* questions that we hope will make this space of possibilities for interplay easier to appreciate and inhabit as forms of research practice.

**Generous* Questions About Interplay Between Natural Descriptive and Hidden Machinery Approaches**

There are many productive discussions possible in the space we are trying to open. In this section, we pose four questions that have emerged in our work and sketch our progress thus far. Our approach has been to consider concepts developed in research in the learning sciences that have been widely influential, asking how forms of interplay between ND and HM orientations are present...
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(or not) in each. These include concepts of socio-mathematical norms (Yackel & Cobb, 1996), inscription devices as material for constructing knowledge in Actor–Network Theory (Latour & Woolgar, 1979), meta-representational competence as an explanation for productive diversity when inventing representations (diSessa, 2004), and legitimate peripheral participation as a theory of situated learning developed in studies of apprenticeship (Lave & Wenger, 1991). Each of these cases can be used to think about the generous* questions we pose, though in what follows we locate each case within a particular question. For each, we summarize these concepts and focus on interplay and problems of method that have been important in our continuing conversation.

(1) Whichever approach we take (ND, HM, or deliberate interplay between them), how can we best remain open to what the actual details of human activity can show us?

As Charles Goodwin put it at the Marconi meetings, “You could not make this stuff up, sitting in your office.”

We start from a particular interview on a particular day between two identified persons in the presence of a child, a camera and a cameraman. Our primary data are the multitudinous details of vocal and bodily action recorded on this film. We call our treatment of such data a “natural history” because a minimum of theory guided the collection of the data.

(Bateson, 1971, p. 6)

First, go to the site where the event being studied normally occurs. Second, show up on the occasions at which it would happen anyway. Third, view experienced participants who know each other. Fourth, take all possible measures to avoid changing the situation. And fifth, observe rather than participate directly in the event under study.

(Schefflen, 1973, pp. 313–314)

These quotes from Bateson (1971) and Schefflen (1973) draw from what many consider to be the origin of the field of communications studies (within which methods of Interaction Analysis developed) – the Natural History of an Interview (NHI) project, started as the Center for Advanced Study in the Behavioral Sciences was opening its doors (1955 and 1956), but never published in wide circulation. The recording analyzed most closely in the NHI project features one of the analysts (Gregory Bateson) lighting a mother’s cigarette, in an interview about her son, in the context of ongoing family therapy.
That something consequential might be found in activity convened and conducted by people other than the analyst remains as a central commitment of Interaction Analysis (Jordan & Henderson, 1995). But we are also committed to the possibility of discovery in studies of designed environments in which investigators deliberately intervene to change things, something that is not entirely alien to IA traditions – e.g., consider the elaborate series of breaching experiments reported in the second chapter of Garfinkel (1967). Learning unexpected things in design studies is a hallmark of research in the learning sciences. So again, how to remain open to discovery?

Discovery of new foci of attention is actually quite common in design experiments, if it is not absolutely necessary. Failures or surprising successes not infrequently push toward, and sometimes enable, new lines of inquiry, possibly involving new ontologies. In some other cases, typically in initial failures, we manage the gap by patching enough to get by, without pulling the surprising occurrence into the core scientific program.

(diSessa & Cobb, 2004, p. 86)

In discussing this question, the concept of “socio-mathematical norms” (SMNs) developed by Paul Cobb and his colleagues (Cobb, Stephan, McClain, & Gravemeijer, 2011) seemed particularly interesting. As Cobb notes (above), their team did not set out to study this concept or to create it in classrooms. Rather, recurring patterns of talk in classrooms led them to pay careful attention to how a teacher talked about the content of valued mathematics with her students and how these conversations developed over time. We give a brief description of SMNs and then make some comments related to how investigators might remain open to discovery.

**Socio-Mathematical Norms**

SMNs occupy a middle region of specificity in what we now understand as a trio of related concepts developed by Cobb and his colleagues to describe the development of micro-cultures in mathematics classrooms. The first, social norms, “refer to obligations and expectations regarding classroom participation” (Bowers, Cobb, & McClain, 1999, p. 27). Examples of social norms include justifying solutions and listening to others’ explanations, and they may be established in classrooms for any subject area. SMNs are similar, but specifically relate to mathematical activity. Typical examples include what counts as an acceptable justification or explanation, and what counts as mathematically different. The third concept, classroom mathematical practices, “focus on the taken-as-shared ways of reasoning, arguing, and symbolizing established while discussing particular mathematical ideas” (Cobb et al., 2011, p. 126). Our conversations have focused on SMNs, which we
treat as interactive structures of knowledge-in-use that shape expectations about what counts as knowing or doing mathematics in particular classrooms.

SMNs were discovered in the work of a particular teacher, as she pushed for mathematical content under investigation by Cobb and his colleagues. Talking with students about how to talk about mathematics later became a research design goal, in an effort to further understand how processes of interaction supported the development of particular (highly valued) versions of knowing and doing mathematics. Now, years (and many publications) later, the field (as we read the literature) has realized that something like SMNs exist in any mathematics classroom, in that what will count as mathematical knowledge (and who has agency for working on and with that knowledge) can vary dramatically across classrooms, teachers, and schools. The relation between individual activity and SMNs is reflexive, in that individuals contribute to the ongoing negotiation of SMNs, while at the same time SMNs shape, support, and constrain their learning. As a consequence, SMNs can (and often do) develop in ways that lead away from what many (not all) in the education research community value as mathematical knowledge-in-use. As an object of design, SMNs that support making and arguing about mathematical conjectures in ways that are preferred by educational reformers may be very difficult to achieve, depending on the institutional contexts of teachers’ work.

Analyses of SMNs used interactions in the classroom community as a unit of analysis rather than individuals or individual learning. The notion of “taken as shared” was interesting in this regard, since participants in the classroom did not “tell” the researchers what they took to be normative. Analysts identified SMNs by noticing when students committed violations of what was expected and were held accountable by others in the classroom community. Given a provisional description of an SMN, if actions the analysts expected to be violations were treated as legitimate in the classroom (going unremarked or accepted after negotiation), their description of the SMN was revised. Making SMNs visible, then, required making inferences about what was hidden or explicit for participants in the classroom.

Cobb described SMNs as part of “the hidden curriculum of mathematics classrooms,” part of “what students had to know” and do to be successful in the classroom (diSessa & Cobb, 2004, p. 94). This hidden layer of mathematics classrooms was treated both as a phenomenon to be investigated (the work of analysts) and as having mechanistic properties that drove the development of students’ dispositions and learning about mathematics (the work of classroom participants). SMNs that treated computational results as a sufficient justification, for example, promoted a computational view of mathematics. Contributions by students and the teacher shaped, reproduced, and changed SMNs, either explicitly (e.g., through discussion or other forms of instruction) or implicitly (e.g., through holding each other accountable). In this sense, SMNs arose within the phenomena under investigation, and their consequence as a concept (and object of design) did not depend on a form of machinery that was hidden inside people.
or in some other way that was different, in kind, from the phenomena in which they were originally discovered.

As research using this concept has continued, our field now sees SMNs as a range of agreements, built through interactional work between students and teachers that specifies how to engage with particular kinds of mathematical activity. The concept of SMNs has shifted from a discovery (something found, ongoing within the relevancies of people being studied), to an object of design (something made, brought forward as an intervention to reorganize classroom talk and action), to something that is (arguably) always already there, though of varied quality, depending on what one values.

The case of sociomathematical norms seems particularly analogous to that of germ theory. Just as germs are tiny, invisible biological elements, so we no more observe sociomathematical norms directly than we can directly perceive students’ conceptual processes. Instead, we have to infer the norms established in a classroom by identifying patterns and regularities in teachers’ and students’ classroom interactions…. Further, just as germ theory is an interpretive perspective that enables us to make sense of certain biological phenomena, so sociomathematical norms are constituents of an interpretive framework that enables us to make sense of certain social phenomena. In both cases, the primary motive was to understand while remaining vigilant that proposed theoretical constructs do useful work.

\[\text{(diSessa & Cobb, 2004, p. 98)}\]

\(\underline{2) \text{How can an explanation or a theory be consequential, most importantly in ways that reduce suffering and provide for more humane and equitable forms of pedagogy?}}\)

If the question \textit{what to do} no longer depends on \textit{what is real}, then what else might it be linked up with? I suggest that if we can no longer find assurance by asking “is this knowledge true to its object?” it becomes all the more worthwhile to ask, “is this practice good for the subjects (human or otherwise) involved in it?” If faithful representations no longer hold the power to ground us, we may still seek positive interventions. Thus, instead of truth, goodness comes to the center of the stage. Or rather, not \textit{goodness}, as if there were only one version of it, but \textit{goodnesses}. Once we accept that ontology is multiple and reality leaves us in doubt, it becomes all the more urgent to attend to modes and modalities of seeking, neglecting, celebrating, fighting, and otherwise living the \textit{good} in this, that, or the other of its many guises.

\[\text{(Mol, 2002, pp. 165–166)}\]
Our second question concerns the kinds of explanations or theories we might hope to develop in an integration of Knowledge and Interaction Analysis. In particular, what would we like the consequences of these to be as explanations are taken up and used by other researchers, in other settings? As the quote from Mol (2002) suggests, this process of uptake both changes the concept and creates new arrangements that may serve very different interests. In our brief description and analysis of the developmental trajectory of SMNs, for example, further study and design using that concept has revealed a variety of structural relations, enacted in talk and activity over time in classrooms, concerning what would count as valued mathematical knowledge. If many versions of mathematical knowledge are possible in classrooms (we believe this is possible; others may not), then how do explanatory or theoretical concepts help to produce versions that are (perhaps multiply) “good” for learners and for teachers?

To help think about this question of how more humane or productive forms of pedagogy might be possible, we have discussed how “inscription devices” (IDs), a particular concept describing how representational technologies are used to fix evidence or technique in scientific practice, have been taken up and used in learning sciences research. One aspect of our discussion has been to see IDs as constructive media both for building up knowledge claims and as critical resources for dissent or building alternative claims about what counts as (or is valuable about) particular versions of knowledge. In choosing IDs as a case, we intend to highlight that the reflexive nature of the concept – creating IDs to do research that is useful – could lead to interplay between ND and HM approaches that serve different meanings (held vigilantly) of “useful.”

**Inscription Devices in Actor-Network Theory**

Actor-Network Theory (ANT) is rooted in the discipline of science and technology studies, particularly as it has been developed by Michel Callon (e.g., 1986, 1991), Bruno Latour (e.g., 2005), and John Law (e.g., Law & Hassard, 1999). While the decades of work in this tradition are diverse, ANT can be characterized broadly by the conviction that socio-technical objects, actors, and institutions are the relational outcomes or effects of heterogeneous assemblages. In other words, ANT treats scientific knowledge as contingent and assembled, not as an outcome of an already present, singular, and transcendent Nature. Some knowledge claims are more stable than others (more realistic) because the network assembly that makes them stable holds up to dissent.

Latour and Woolgar’s (1979) notion of **inscription devices** provides an early example of the kind of socio-technical assemblages posited within the ANT tradition. An ID is an arrangement of materials, humans, technologies, and practices that transforms some material object or observation into a usable inscription, where inscription is construed broadly to include diagrams, traces, text, or drawings that might figure in a scientific publication. An ID in a neuroendocrinology
laboratory, for example, could be a bioassay composed of recording technologies and human practices that convert animals or their cells into drawn curves whose peaks and valleys can be inspected by members of the lab and used as figures in their papers. IDs as a concept were discovered in ethnographic observations of human and machine activity that was neglected or actively suppressed in scientists’ accounts of their own work (deletion of human modality makes some scientific claims stronger or “harder”). In this sense, IDs grounded in ethnographic description had the (still) somewhat radical effect of making scientific fact-making visible as a process of assembly.

Taking IDs in ANT as our exemplar, how can we understand the discovery of this concept (and its later use by others) as the possible interplay of ND and HM approaches to life in laboratories and the knowledge used and produced there? Prima facie, certain aspects of the ANT framework lend themselves to a HM approach. Images of complex arrangements producing socio-material objects, analytic choices that radically decenter human agency, materialist leanings that foreground laboratory apparatus, methodological restrictions on “cognitive explanations” until after the network has been exhausted (Latour’s “Rule 7,” 1987, p. 268), and injunctions to study the “mechanics of organization” (Law, 1992, p. 389, emphasis in original) abound in the ANT literature and certainly evoke a machinic imaginary. Indeed, an introductory text on science and technology studies defines (albeit crudely) IDs as “machines that … allow the scientist to deal with nature on pieces of paper” (Sismondo, 2004, p. 67, emphasis added).

Considering the extent to which actor-networks are hidden complicates the initial temptation to classify ANT as a hidden machineries approach. On the one hand, there is a sense in which assemblages can be hidden, forgotten, or elided in practice. In Latour and Woolgar’s (1979) neuroendocrinology lab, the processes by which IDs produced their effects were quickly bracketed once scientists had the inscription in hand. Instead of acknowledging the complex arrangements required to make diagrams, spectra, and numerical tabulations, the scientists treated inscriptions as transparent pointers to their objects of investigation. It is for this reason that scholars working with ANT varyingly refer to socio-material processes that are “forgotten or taken for granted” (Latour & Woolgar, 1979, p.63), to black-boxed or punctualized (Callon, 1991) actor-networks, to hinterlands (Law, 2004) of pre-established practices and assumptions, or to “invisible work [that] lies below the waterline” (Law & Singleton, 2005, p. 337). On the other hand, a critical feature of ANT’s assemblages is that their practical elision is forever tenuous; “punctualization is always precarious” (Law, 1992, p. 385). The neuroendocrinologists may un-bracket the socio-material conditions of scientific literary production, for example, when IDs don’t perform as expected or when a scientific claim is in doubt. So the heterogeneous assemblages of ANT perennially flir with the boundary between hidden and unhidden: their endless complexity may be in full view or neatly punctualized; they may be elided or center-staged; they may recede into the hinterland or come charging back out of it. In this sense, IDs are
useful for both building up and dissenting from knowledge claims, providing a leverage point for creating alternative versions of knowledge.

Understanding ANT as a hidden machineries project becomes even more doubtful in light of its commitment to a radically *flat* ontology (Latour, 2005). That is, ANT resists the a priori deployment of social science’s usual arrangement of scales – for example, in micro–macro debates about bottom-up agency versus top-down social determinism. In the present volume and KAIA project, this might take the form of locating individual cognition within social interaction (e.g., as “context”) without exploring generative relations between the two. ANT’s flatness invites us to wonder how dialogues in educational research might be transformed if we jettison ontologies that nest psychological states, individuals, and social groups like matryoshka dolls. At any rate, if we take the hallmark of hidden machineries approaches to be the appeal to a sub- or supra-personal stratum of invisible, causally potent structures, then IDs and the associated ANT framework are not consistent with the use of hierarchical strata as explanation.

But if theoretical and empirical work in ANT seems imperfectly captured by the language of hidden machineries, it is not for want of structure. Latour and Woolgar (1979) are quite explicit, in fact, that the notion of IDs provides crucial theoretical leverage by which the non-scientist ethnographer can organize and make sense of the apparent chaos of laboratory practice: “At this point, the observer felt that the laboratory was by no means quite as confusing as he had first thought … the laboratory began to take on the appearance of a system of literary inscription” (pp. 51–52). Moreover, ethnographic work informed by ANT uses extended participant observation in order to identify principles and regularities that characterize actor-networks and their processes of formation or dissolution. In this way, although ANT does not appeal to hidden, sub- or supra-individual causal layers, the theoretical framework can be seen as a search for structure by meticulously tracing connections as they appear through close empirical analysis of ethnographic data.

**3) What is the appeal of mechanism in theories or explanations for human activity?**

If by mechanism we mean structural regularities that influence patterned, human activity, what sorts of mechanisms, at what levels of aggregation or scale are consequential (see Question 2)? Are there advantages to an explanatory approach that is “ontologically flat” (i.e., without using super- or sub-ordinate entities and relations to explain the observed phenomena)?

The major result of our retrospective analysis was the “discovery” of meta-representational competence or simply MRC. MRC, as an ontological innovation, implicates a specific body of knowledge that students
have, and which can be developed, lying behind students’ abilities to create, critique, and adapt a very wide range of effective scientific representations.

(diSessa & Cobb, 2004, p. 88–89)

All we can observe is interaction, and we can only see one interaction at a time. An interaction is an example of process, which is always visible. The guidelines underlying the choices made at the level of process are what can be called structure, but structure remains invisible. We know of its existence only because of the regularities we find in interaction. This means finding logical sets of behaviors to study, and paying attention to the regularities found to exist in these sets. This is why researchers study conversational openings or closings, or service encounters, or civil inattention — the goal is to find a type of communication behavior, and then examine many different examples in an effort to begin to understand how that type works.

(Leeds-Hurwitz, 2005, pp. 143–144)

In any of the studies explored in our paper, and in our own work as well, it is never possible to see processes of learning and teaching as things that are just “lying around” in the moment. These processes always involve multiple perspectives on (and in) activity, they extend over durations of time that are difficult to capture and organize effectively for close analysis, and there are many details that have uncertain relevance to participants and could hide structural regularities of interest to analysts (what is relevant to members and analysts may diverge, we understand). The discovery and subsequent pursuit of “meta-representational competence” has been a productive case for our conversations.

Meta-Representational Competence

Meta-representational competence (MRC) is a concept diSessa and colleagues (diSessa, Hammer, Sherin, & Kolpakowski, 1991) developed to describe the capacity of children (or anyone) to notice that some representations work better than do others, to be critical of different representations proposed for use in the same task, and to tinker with representational systems to make them more effective. They conjecture that these capacities are grounded in underlying knowledge structures that support inventing or designing new representations, comparing and critiquing the adequacy of representations, understanding how representations work, explaining these things to others, and learning new representations quickly. These underlying knowledge structures are said to be a form of “native competence” (diSessa, 2004, in the title, “Metarepresentation: Native competence and targets for instruction,” and throughout the paper), and this might reasonably invite a view of MRC as part of the innate endowment of the human cognitive architecture. Instead, this competence is “gradually developed through cultural practices in and out of school” (p. 294) and, since it is not taught (again, not to be
confused with an impoverished stimulus, understood as input to a mental faculty), MRC is a “free resource.” It may be available in formal learning environments and could be a resource for learning in a broad array of conceptual domains (e.g., kinematics, mathematics, or computation), depending on how settings are organized (or designed).

MRC was developed at a time (late 1990s, early 2000s) when there were a number of studies discovering flexible use of “informal” or everyday representational media to manage (or to solve) problems associated with use of “formal” or schooled representational forms. These included Sylvia Scribner’s (1986) studies of “thinking in action” among workers in a dairy, comparative analysis of “oral” versus “written” arithmetic in the informal economy of street sellers in Brazil (Carraher, Carraher, & Schliemann, 1987; Saxe, 1991), gap-closing practices of quantity use in shopping and cooking (Lave, Murtaugh, & de la Rocha, 1984), and use of informal representations and strategies to solve school algebra problems among engineering and computer science undergraduates (Hall, Kibler, Wenger, & Truxaw, 1989). Three lines of research developed out of this work: (1) studies of the organization and development of representational practices, (2) studies of the “representation effect” from different affordances of representational forms, and (3) the discovery that learners invent representational forms that, with direction, come to resemble conventional (and powerful) historical conventions. The first focuses on the social organization of technical practice (hewing closely to science studies), the second on task and instructional design for purposes of school instruction, and the third on trajectories or pathways for conceptual domains in particular disciplines (e.g., the statistical concept of variability, the concept of speed or velocity in kinematics).

After some discussion, we remain interested in what an account of native competence that lies hidden beneath the conventions of formal instruction can tell us about representational practices, and we certainly would not argue that these practices are (instead) lying around “in plain sight” for straightforward study by means of Interaction Analysis. The discovery of MRC was made possible (diSessa et al., 1991) by iterative analyses of records of classroom talk over time, as learners were encouraged by their teacher and diSessa’s team to explore alternative representational displays for describing motion. As with his provocative use of “native” in the later summary article (diSessa, 2004), diSessa places “inventing graphing” in the title of the original report, but he (and co-authors) quickly replace this with a more familiar description of schooling – students settled on a preference for conventional Cartesian graphing in conversations orchestrated by their teacher. MRC may consist of previously hidden knowledge structures, but the analysis that discovers them as a form of invention is conducted primarily (as far as we can tell) from a natural description perspective. The results of that analysis are attributed to structures that are doubly hidden, first by the politics of conventional schooling (we agree and value this meaning) and second by a
causal explanation from hidden mechanisms (knowledge structures, in pieces or otherwise).

The claim to “invention” (later, “native competence”) foregrounds the agency of learners, while putting into the background (not really hiding, we think) the agency of teachers, researchers, and their suitcases full of tools, tasks, video cameras, and assessments. What is hidden in MRC is a kind of subversive learning, done in private or in activities that are either unsanctioned or actively eliminated in conventional practices. In approaching this subversive learning (i.e., not school; see Sefton-Green, 2013), and similar to the account of discovery we gave for SMNs, we see researchers looking for structures of interaction that drive particular values in what counts as knowledge (i.e., Cartesian graphing rules over local inventions). We would expect that comparative talk about representations, reaching a critical consensus concerning adequacy, and the very idea that students could “invent” forms in a typical classroom would also require concerted efforts on the part of students, teachers, parents, and school administrators to keep practices of invention going in typical schools. A similar set of arguments can be found in focal analysis chapters and commentaries from a recent, multiple analysis project in which students were described as “inventing representations” of statistical concepts in classrooms studied (and designed) by Rich Lehrer and Leona Schauble (Koschmann, 2011).

We could be wrong about what will be a consequential explanation or theory of these kinds of representational fluency. Our desire to continue asking what these activities mean for their participants, or how relations between people and material at different levels of organization enable (or dissolve) these activities, may just delay new cycles of design and refinement along a developmental path that makes invention a commonplace occurrence in classrooms (Hall, 2011). Positing mental structures as an ontological layer behind these more complex, but primary (we think), phenomena reflects a desire for a particular kind of theory and explanation.

(4) What kinds of comparative analysis are productive and ethical, and what kinds should we avoid?

With schooling as the template, questions about apprenticeship were framed mostly in negative terms. True, the binary distinctions between “formal” and “informal” education posited some definite characteristics for formal education. Mainly, however, things present and important for carrying out schooling were presumed absent in apprenticeship – teaching as the central prerequisite for school learning, for example, and with its absence, a lack of effective organization to the learning aspects of apprenticeship. This tenacious perspective clearly directed the questions I asked in Happy
Corner [tailoring enclave in Monrovia, Liberia] to begin with and defined my expectations about what should be happening if the apprentices were going to learn. It explains the difficulties I had in coming to see that other things that were happening were crucial matters of learning.

(Lave, 2011, p. 59)

Most research in the learning sciences treats a particular site (e.g., a physics classroom, an after-school club, a museum exhibit) as a type of setting for studying learning or teaching. Whatever develops in the particular site is described and explained as the kind of thing that happens (or might happen) in this type of setting. In framing the role of comparison in this way, we do not mean to invite more careful forms of inference from sample to population (evaluation studies and education sciences are doing a bang-up job with that argument). Rather, we mean to ask how to draw comparisons across studies, or to conduct comparative analysis as part of a single study, in ways that may be enhanced by the interplay between ND and HM orientations. This is a form of inference from case to theory (Yin, 2011) rather than from sample to population. In our conversations, Jean Lave’s development (with Etienne Wenger, Lave & Wenger, 1991) of the concept of “legitimate peripheral participation” has been very helpful in thinking about productive and ethical comparative analysis. The introduction of this concept, of course, came on the heels of a thorough critique of information-processing psychology as a hidden mechanism theory of cognition, learning, and meaningful human activity.

**Legitimate Peripheral Participation**

The concept of legitimate peripheral participation (LPP) provides a structural, explanatory framework for a theory of “situated learning” (Lave & Wenger, 1991) that is made up of changing social relations to ongoing practice. These changing relations shape practice-linked identities – who one is understood by others to be – as much as they change what one knows or is able (or allowed) to do in practical activity. The unit of analysis is the relation between person and setting, understood as part of ongoing (perhaps changing) cultural activity. This theory and explanation for learning, often without explicit forms of teaching, is based on comparative analysis of apprenticeship relations across a wide variety of cultural practices. But the concept (LPP) has probably been most influential for critical and design-oriented studies of schooling, extending more recently into research on the relation between learning in schools and in other valued cultural practices. Even in settings where individual instruction and testing are dominant activities, and even if these are understood by participants to involve expert knowledge transmission to individual learners, these activities (and interpretations of learning) also can be understood as structured in social relations, with a history and the potential for both reproduction and for change.
LPP as a concept describes regularities that may not be lying around “in plain sight” for participants in the cultural practices being studied, or for analysts who visit the practice to study learning. In this sense, there is a structural account that could be read as a “mechanism” or “machinery” (Lave does not use these terms), which is not immediately visible or even sensible to the observer (particularly an alumnus of Western schooling) but must be revealed through comparative analysis of naturally occurring human activity. It is possible but not particularly responsible, Lave suggests, to study these things without becoming involved in their politics. These structural regularities, once adequately described, might serve as models for designing activities in or around schools, and many have attempted to do this (Gutiérrez & Vossoughi, 2010; Taylor & Hall, 2013).

The theoretical pathway here, as we argued at the outset in this paper, is not from natural description to hidden machineries. Instead, there is careful analysis and description of phenomena in historically specific, cultural situations, and a persistent effort to understand the activity from the social actor’s point of view. The structural regularities that are developed as a theoretical concept (LPP) hold up to comparative case analysis, by suggesting which cases to study next (in terms of developing grounded theory, “theoretical sampling”) and providing for structural refinement and a better understanding of conditions under which the concept operates. Through a form of interplay between ND and HM (describing structural influences), comparative analysis of ethnographic cases can be quite rigorous but still open to discovery.

Looking back on her initial fieldwork with Liberian tailors, Lave (2011, quote above) describes a series of misadventures in trying to understand learning in apprenticeship, treated as a “negative space” formed by comparing tailoring activities to seemingly more deliberate (or formal) practices of Western schooling. By trying to understand the historically particular setting of tailoring shops as a type of some other, idealized cultural form (Western schooling), her fieldwork produced a series of absences that neither satisfied Lave nor made much sense to her study participants (i.e., masters could perform didactic instruction, when asked in interview, but they never did so on their own). Eventually, by attending instead to the organization of tailoring work, the path of learners through this work, and to forms of assessment concerning apprentices’ capacity for sewing garments in the service of producing sellable clothes, Lave arrived at a positive, structural description of apprenticeship learning in its own terms. In this sense, she struggled to remain open to what the particular practice under study could tell her, as an intact activity system with its own logics of production and learning.

If a comparison with idealized models of formal instruction (whether to valorize or critique apprenticeship) was not initially productive, the subsequent analysis leading to the concept of LPP did involve productive forms of comparative analysis. The monograph with Wenger (Lave & Wenger, 1991) describes structural regularities across a deliberately diverse collection of ethnographic cases. We will not rehearse that history here, but it is worth noting that the dimensional terms
of LPP provide for structural descriptions of settings that both support and inhibit learning.

Participation, as a basic mode of engagement in cultural life, varies in ways that are influenced by two other dimensions. The learner can be seen and treated – with dramatic consequences for access to practice – as being a legitimate or illegitimate participant. Similarly, participation can range in centrality from peripheral to full. The particular configuration of legitimate, peripheral participation describes a pathway towards full participation. This configuration (LPP) tends to be used as if it were the theory, and in a romantic genre (i.e., Learning in this place is LPP, and therefore it is good!), to the exclusion of other possibilities available in the theory. We sometimes joke that freeloading contemporaries in difficult teamwork are legitimate, central, non-participants, for example. The single, negative case reviewed in the monograph concerns butchers in large-volume, middle-class supermarkets, who can spend years wrapping routine cuts of meat in plastic, without gaining access to the broader array of butchering activities available to their peers working in smaller, working-class stores (Marshall, 1972). There are very few studies of how LPP accounts for a failure to learn in the extant literature, but as this concept is extended to new cases, whether in or out of schools, we expect there will be further discoveries and refinements to the theory.

We end this discussion of a generous* question with a comment on three forms of comparative analysis. In the first, one form of cultural activity about which relatively little is known (e.g., craft tailoring apprenticeships) is analyzed using comparative terms drawn from a theoretical account of some other activity, sometimes a normative or idealized explanation for how a universe of activities of a general type work (or more normatively, should work). This describes, we think, the initial misadventures Lave described in Liberia, and so raises the question of when and how to avoid these problems of creating “negative (comparative) space.” In the second, multiple particular cases of cultural activity are studied and compared in hopes of identifying what they have in common, usually to outline a new type of cultural activity that will be of general interest (e.g., adolescent and professional use of location-aware communication devices; see Hall & Leander, 2010) or that could be used for some other purpose (e.g., designing learning activities in which adolescents learn new things about STEM concepts enabling these devices; see Taylor & Hall, 2013; Ma, this volume). In the third, comparative analysis focuses on transitions experienced by study participants as they move between cultural activities, or even as they assemble interstitial spaces that support new forms of learning (e.g., children’s ways of talking around the dinner table by comparison with their structural access to conversation in classrooms as they begin formal schooling; see Erickson, 2004). Differences between these forms of comparative analysis (and others) run in parallel with King Beach’s (1999) theoretical account of different types of “consequential transitions” in studies of learning and transfer. As he pointed out, not only are sites of study and participation juxtaposed, but each involves differently inflected values about the future of changing practices, as well.
A Concluding Note on Doing Being Generous*

If Latour and subsequent ANT explorers have captured an important aspect of using/creating scientific knowledge-in-use, the stability of what in retrospect we might call a scientific contribution based on a ND (or a HM, or their combination) approach is tenuous until the dust (and network making) has settled. Some of the concepts, treated as cases in an as yet unwritten history of the learning sciences, have not been settled. We intend the conversation about Generous* questions to be a conversation about methods, and about what we desire to make as researchers engaged in research on learning and teaching in conceptual domains that we care deeply about.

We have at this point had a lot to say. In the spirit of the events bringing the KAIA community together in the first place, we invite readers to take up these questions, and we look forward to further conversation and comparative analysis of cases as themselves generous* activities in an effort to develop more productive methods and concepts.
Commentary

“OPENNESS” AS A SHARED RESEARCH AESTHETIC BETWEEN KNOWLEDGE ANALYSIS AND INTERACTION ANALYSIS

Mariana Levin

Thinking about the four generous* questions that Hall, Nemirovsky, Ma, and Kelton pose provided a convenient impetus for focusing and sharing some of my ongoing reflections on our agenda. Their piece covered a lot of terrain, and in this reflection I’m only going to try to follow up on one thread from it – that of what it means to have an “open” perspective on research. I hope this brief reflection is only the beginning of a longer conversation, both within the KAIA community and more broadly.

One of the key moves made by Hall, Nemirovsky, Ma, and Kelton was to suggest a reformulation of the discussion in terms of commonalities and differences across research practices and aesthetics as opposed to the surface differences in attributed topic of theorizing (e.g., knowledge or interaction). I found this move intriguing, productive, and “integrative” in spirit. In the piece, the authors propose a distinction between “natural descriptive” (ND) and “hidden machineries” (HM) approaches to theoretical work in science, and they point to several cases that they feel are illustrative of these two approaches. While I think it would be good at some point to return to a broader discussion of the sensibility of ND and HM as categories, in this essay I wanted to focus instead on the second part of the paper, in which the authors pose four generous* questions to provoke discussion.

I found all four of the questions Hall, et al., posed interesting and, each in their own way, provocative (in a good way – as in generative and stimulating). The first question, “Whichever approach we take (natural descriptive or hidden mechanism), how can we best remain open to what the actual details of human activity can show us?” in particular spoke to me. It seemed to point to something I feel is a shared value across our (KAIA) community. I wanted to take the opportunity in this turn* to begin to explore the theme of openness in our research, especially in relation to work done in this community.
Explorations Under Rocks and in Outer Space

In his plenary lecture at the KAIA conference in Marin, Chuck Goodwin talked about the importance of being open to what the world can show you – and then looking systematically to characterize what might be going on. The line “You couldn’t make this up, sitting in your office” was taken as emblematic of the kind of open aesthetic we should have to our research. This line was quoted in the statement of the four questions in Hall et al., as well as referenced in our later discussions in Vancouver.

At a follow-up meeting in Vancouver, Rogers Hall reiterated this idea of openness in the search for the unexpected aspects of activity to notice in a story he told about how as a kid growing up in Texas he used to turn over rocks and find “really weird stuff.” He likened the experience of doing research and finding unexpected things about how people think and learn as they engage in activities to being sort of like turning over those rocks in Texas. I was struck by Rogers’ description of the personal joy that he felt in discovering the “really quite beautiful things that you can find in human interaction.” I was struck not because openness to seeing new things was a foreign idea, but rather because it was an idea that at least in some ways deeply resonated with me. As I listened to this story of Rogers in Vancouver and the general discussion concerning maintaining an open stance in research, it reminded me of a story, the allegory of the Jungle on Mars, that Andy diSessa sometimes tells when he is introducing Knowledge in Pieces and Knowledge Analysis.

The story is this: Two scientists journey to a new planet (“Mars”) for the purpose of studying the flora and fauna of the new planet. The first scientist comes ready to use the wealth of analytic methods and measurements he has developed for categorizing the vegetation on Earth. He immediately embarks on matching what he is observing on “Mars” into these existing categories, using the methods that he knows from his experience on Earth. In contrast, the other scientist also comes with a breadth of knowledge about plant life and methods of studying it on Earth, but the very first thing that she does is take a good look around the planet and just observe what is there before attempting to categorize. Even as new categories of classification emerge, this scientist retains a skeptical stance towards applying the categories – checking to make sure they really apply or whether a different category system entirely might be more apt and need to be developed.

Andy used the allegory to make the point that we as learning scientists need to appreciate the complexity and beauty of human knowledge. He argued that a basic part of research should involve questioning the very ontological categories we use to describe knowing. Characterizing the nature and form of knowledge is an empirical endeavor that involves observing both knowledge-in-use and knowledge-in-development. While we do build upon categories that have been...
created in previous analyses in our ongoing empirical studies, we must retain a skeptical stance to those categories and we must always remain open to what the data can reveal about how knowledge (knowledgeability? knowing?) can be seen to function in activity. From a methodological perspective, the allegory can be understood as encouraging an open and evolving perspective in research. Not all studies can be perfectly designed a priori to shed light on an already conceptualized phenomenon. Not only is it not possible, but such a perspective is not always the most illuminating. In line with Goodwin’s exhortation, it isn’t possible to just make this stuff up sitting in our offices.

**A Personal Turn Toward a Version of Openness**

Indeed, I first heard this parable of the scientists and the jungle on Mars at the very first AERA symposium I co-organized (on Knowledge in Pieces perspectives on the role of representations in mathematical cognition). This was the first substantive opportunity I had to interact personally with Andy and others using KiP in their work. For context, leading up to that conference, based on my own reading and work, a goal I had for an analysis was to use coordination class theory to trace the development of a student’s understanding in an unexpected learning event that I had happened to capture as part of a study I was doing for a completely different project. Despite a lot of struggle, my initial analysis wasn’t really getting off the ground because I didn’t really have the right idea of what a coordination class was, much less how to recognize one or use the theory to guide my analysis. But more germane to this discussion on openness, before that AERA, it hadn’t occurred to me that perhaps coordination classes were not even what I should expect to see in my data and that in order to determine that, I should work from the other direction: to look with fresh eyes at the data and see them for what they were instead of trying to see them in terms of some particular construct.

This personal turn toward a more “open” research attitude was sparked both by Andy’s telling of the parable of the jungle on Mars in his discussant remarks at our symposium and also by a conversation before the symposium with some of the other participants. Given my struggles with the analysis I was trying to do, I was especially eager to get to talk to Andy and the others about coordination classes. After letting me explain the data and discussing them for a little bit, Andy said what at the time I thought was a most surprising thing: (something like) “You know, I actually tire quickly of this game – people always asking me ‘Is THIS a coordination class or is THAT a coordination class?’ Whether or not coordination class theory applies is actually what you might discover in the course of the analysis of your data. Is it about coordination classes? I don’t know. Is it? Or is there something else going on?” In the moment, I was a bit taken back by how unhelpful Andy seemed to be in my quest to “see” coordination classes in my data! I turned at that point to Joe Wagner, another one of the symposium participants,
who continued the discussion with me for a bit and helped me to understand a bit more what the function of coordination classes as a model of a particular kind of concept was.

Eventually it turned out that coordination class theory did serve a useful role as a kind of reference model in the later analysis that emerged. However, the way it was involved in the analysis was in a much more organic and generative way than a typical top-down, “identify and apply” model of the relationship between constructs and data. Of course, actually figuring out how to develop an interpretation of the data that is both grounded in open, “naturalistic” observation, and also guided by a heuristic epistemological frame (that includes an evolving “toolkit” of precise, yet malleable and extendable constructs like coordination classes) is a learned art in and of itself (and could be a topic for further discussion…). An important aspect for our ongoing work that I haven’t tried to unpack here concerns the importance of naming the underlying epistemological assumptions and guiding metaphors for knowledge and learning that would even make the “negotiation” between data and a particular theory (e.g., coordination class theory in this case) sensible. I do think we need to articulate those assumptions and be aware of those metaphors because they can be both an asset in pushing our theoretical work forward and also a constraint in “blinding” us to other ways of seeing.

Concluding Remarks

Returning to the original question of how we can remain open to the details of human activity – there seems to me to be (at least on the surface) a tension between on the one hand remaining open and “neutral” in noticing surprising, unexpected, and unexplored aspects of knowledge/knowing of learning interactions in data, and, on the other hand, recognizing previously explored conceptual categories when we see them in data. One heuristic for remaining open is to be sure that our theories are actually “doing real work” in their application to particular data. Painting the data with different theoretical perspectives and constructs can almost always be done. We need to make sure that theoretical categories not only apply but are insightful. Another heuristic for openness is assuming that we simply don’t know much about cognition and how it functions in activity.

As Rogers pointed out in Marin – one thing that unites us as researchers working on the KAIA agenda is that we are a community of learning theory builders. As such, we have to learn to expect the unexpected, notice it when it presents itself, try to understand it, and then build on those insights in further studies. If we only went out and saw what we expected to see, we would never generate fundamentally new insights, as our job would be more concerned with assessing and measuring empirical data against our a priori theoretical expectations (like the first scientist in the allegory of the jungle on Mars).
I think we do need both an open and a skeptical attitude to the interface between previous theoretical categories and new data – we must always be prepared to see something new in the data. However, I think that we can (and do) practice more of a form of “disciplined openness.” Obviously, given our differences in intellectual histories and research aesthetics, we have some differences in where we are looking in the data and what we’re attentive and prepared to be open to. I think this is a point of potential strength in the agenda of bringing KA and IA together. At the follow-up meeting in Vancouver, we talked a little bit about stewardship of our ways of doing research and what kinds of researchers we want to be. On a personal note, one of the things I hope to get out of the interactions with researchers in this broader KAIA collaborative effort is a collection of new ways of seeing and interpreting data and new ways to being open to what it can show us.
Commentary

HOW SCIENCE IS DONE

Andrea A. diSessa

As one of the initiators of the Knowledge Analysis/Interaction Analysis (KAIA) agenda, I was gratified to find a chapter such as this one, offered by Hall, Nemirovsky, Ma, and Kelton. The chapter is, indeed, generous — in the everyday sense, and also in the narrower generous* sense — in its treatment of some knotty issues. I find it to be without partisanship, and the intention to be neutral and balanced is everywhere visible. The reviews of histories of accomplishments, both within the KA and IA communities and outside them (Charles Darwin, Santiago Ramón y Cajal, Jane Goodall), are, in themselves, interesting, revealing, and good fodder for discussion. But, I’m most grateful that the chapter explicitly raises a meta-scientific perspective. It is more than possible (but I would not take it for granted) that what has separated IA and KA — or what might unite them — is as much meta-scientific as it is scientific. Predilections for ways to do science are almost certainly consequential, and the case of connections or disconnections between IA and KA really ought to be examined from that perspective.

Let me start with a sketch of what I think Hall et al. accomplished and then proceed with a sketch of my commentary. The generous* chapter centers on two modes of doing science: the hidden machineries mode (HM) and the natural descriptive mode (ND). HM seeks to find and defer explanation to things that are “behind” (below) the phenomenology of our investigations on an ontologically distinct level. It also projects a nature to this initially (but not ultimately) invisible level: It is, in some sense, like a machine (hence the term “machineries”).

A workable and historically pregnant metaphor is that the HM level is something like “how a clock works”: assembled of elements that interact in a kind of lock-step mechanism that produces the behavior that we see. However, the behavior — hands that go around and tell time — constitutes a distinct ontology from gears and springs. Indeed, we can know this because there are clocks that have hands that go around that have nothing like gears and springs in them. We can even dispense with hands and going around completely with a digital clock. The attitude that Hall et al. project on the HM mode is that, ultimately, scientific explanation is in discovering the gears and springs and how they work, and then there is nothing more to clocks.

The ND mode, in contrast, aspires to, or actually enacts, an ontologically flat science. One sits firmly within the level of the “phenomena of interest,” and employs
extensive and careful observation. Instead of seeking hidden machineries, the ND mode seeks such things as “noticings” that have not been previously taken into account, or whose wide import has been neglected. In complementary manner, one also seeks to prune accounts of the phenomena of interest of spurious observations through careful and rigorous attention to the details of the relevant phenomena, or by taking into account the accumulated body of work, specifically using the breadth of occurrences of the phenomena of interest.

It seems evident to me that the HM and ND modes might be abstracted from prominent strains in the KA and IA agenda, respectively. Indeed, my own construct of p-prims (presumably representative of the KA approach) is cited along with historical examples like Piagetian schemata and information-processing structures as prototypical HM science. On the other side, prominent contributors to IA, such as Charles Goodwin (“you could not make this stuff up,” as cited by the authors) motivate and ground the ND perspective.

It’s just at this point that generosity enters the argument prominently. Rather than pursuing the HM and ND modes as “KA vs. IA,” Hall et al. recognize the legitimacy of each mode, and inquire as to when and how each can be valuable, often in concert with the other. In particular, in addition to rejecting any absolute primacy, they reject simple versions of articulating HM and ND, such as stage theories that contend that, for example, extensive ND exploration is always necessary as a precursor to HM modes of inquiry.

I’ve used the unassuming description of “mode” for HM and ND, although I believe that Hall et al. have inclinations of a particular scale both in time and in terms of community distribution in mind for these perspectives. They have “their own traditions, methods, and diverse professional communities.” So, traditions and methods, and even whole professional communities, accumulate separately around HM and ND. In other places in Hall et al.’s chapter, HM and ND are referred to as “worldviews” and “ways of being.” It seems that one can go a long time working within one or the other perspective: As described in the chapter, Darwin’s ND work is followed years later by the HM of DNA; Ramón y Cajal’s extended ND work followed the completion of HM work that elaborated optics and matter–light interactions. Similarly, the authors’ “professional communities” comment (just above) suggests that one might not even meet adherents of the contrary perspective on an everyday basis, as one does within one’s immediate professional cohort.

After this meta-scientific set-up, Hall et al. continue by asking an important and interesting set of questions, which I will not review (see the explanation offered, just below). We are invited to consider these questions in the light of recognizing both the HM and ND modes, while continuing to seek to understand their sensible relationships.

My comments on Hall et al. center on the HM and ND modes as constructs. In particular, I have to admit that I had a rather large set of quick reactions that problematize these as cogent categories. There is a lot to explore before I, at least,
feel I can use these ideas productively as analytical tools. In addition, exploring the ideas of HM and ND in some detail will give priority to some important and potentially impactful meta-scientific issues that may otherwise not have a voice here.

I recognize that this choice is not exactly polite, deferring an offered avenue of continued consideration. However, such are many conversations where offered continuations are deferred or reformulated in succeeding turns, or even within turns. And, in giving even partial due to the constructs of HM and ND, a lot is in play, to which I don’t believe I can do any justice while simultaneously continuing with the generous* questions.

A Pithy Account of My Meta-Science

The following is a set of reflections on my own experience as a scientist, and also reflections on the nature of science more broadly, supported by cases in the history of science with which I have some acquaintance (just as Hall et al. did). The points were stimulated in their contrast to (or at least in relation to) many distributed points in the generous* chapter. However, I present them together, in the order of my own choice, and only occasionally and minimally with reference to the context of the related points made in the chapter. I do this in order to help show systematicities among the elements. I have chosen a personal approach in part because I don’t want to project these ideas on the whole of the KA community, much less pretend that they have some authority over how everyone must view science. I don’t want to go too far in the direction of “how things are.” “How things seem to me” is good enough for present purposes.

While I believe that many elements I put forward are commonplace, a few are more idiosyncratic, and I expect it’s safer just to put forward this little system of ideas as mine, rather than trying to be scholarly about the nature of science, which space here would not allow in any case. Even then, my exposition will be elliptical in various degrees, depending on the predilections of the reader. What I believe will become most clear, however, is that these elements contrast vividly with those that support and justify the HM and ND analytic.

1. Diversity and generativity in our toolkit for doing science: kicking and screaming.

I think that all scientists, especially those in the learning sciences, have or should have a wide variety of tools to try out in any given inquiry. Speaking more carefully, I should call these tool sketches, because I believe any “tools” we use are always subject to further invention and refinement for the cases that we pursue. So, we schematize what we’ve found successful in the past, but our schematizations are always open to reinterpretation and development.
Empirical methods constitute such tools (tool sketches), but there are also sketches of bits and pieces of theories. Collins and Ferguson (1993) suggest “epistemic forms and games” to describe the final forms of scientific inquiry and related practices for developing them. I’ll adopt their terminology, if not everything in their theory. An element of Collins and Ferguson’s point of view that I do strongly support, and which will become important here, is that there are a lot of such forms and games. Looking at science from the standpoint of only two “games” (say, HM and ND), and even “doing” only one of those for extended periods of time, seems a rather severe simplification. The essential diversity of the category of “theory” (here represented by various forms) becomes a main element in my continuing discussion. Incidentally, the Collins and Ferguson form-and-game type that best matches my p-prims theory is called “primitive elements,” as in the periodic table of elements.

With a bevy of epistemic forms and games at our disposal, what determines which to use? The easy, but simplistic, answer is “the ones that best fit present circumstances.” The first bit of complexity is that it is likely or certain that individuals or groups have preferences and expertise for some games over others. But, also, the open nature of the set of forms and games is, for me, essential. And above many other things, I believe that science, done well, will inevitably drag us — kicking and screaming if necessary — toward non-preferred, but more apt forms and games. That process is not much like free choice, nor is it like being able to know in advance how things will work out, nor even like having on the table the particular form or game that will work best. Scientists should be aware of the kicking and screaming phenomenon, and they should cultivate a sensitivity to the ways that science should be allowed to drag us, including both sensitivities to problems and limitations in our current understanding and also sensitivities to possibilities that are not just trotting out things that have worked in the past.

The next two items in my list implicate a lot that is familiar, even conventional, concerning the nature of science, starting from the top-level categories: theory and observation. That fact certainly does not mean that these concepts are not in need of refinement and development. But they are serviceable as a starting place, and they constitute a benchmark that improved views of the nature of science should surpass.

2. Theory is never on the surface of things.

Pretty much everyone I know subscribes to the fact that theory is important. In my prior life as a physicist, this was not even slightly contentious. Even the most ND of physics researchers, experimentalists, recognized theory’s central importance. Interestingly, the importance of theory is sometimes contested in the learning sciences, or it is even characterized as harmful. But I don’t think very many in the KA or IA circles contest the importance of theory. What gets more complicated is the nature of theories.
It may be more controversial to say that theory cannot be found on the surface of things. What supports this belief, for me, are endless examples of theories—and only questionable counter-examples—where theory does not look much like the world that it purports to explain. The world does not look like quantum mechanics. For example, impenetrability of solid matter is only a statistical fact in quantum mechanics, not a basic principle; there is a chance that a book on a table will fall right through it without breaking it. Similarly, space does not look curved. In fact, for many people, this is literally an unimaginable possibility. Yet, Einstein says it is curved, and he also says that the curvature of space replaces the notion of gravity as any kind of force, such as what is immediately salient in holding a heavy object in our hand. Gravitational force, per se, is literally gone in general relativity. Touching on a point to be developed later, Darwin transformed species from categories of similarity among animals and plants to categories of essentially diverse products (each individual) of very long-term processes that are beyond any sensible version of “observable.”

The reasons that I support the non-superficial nature of theory in science also include theoretical ones, such as the basic fact that science is, after all, a human construction, made up to help us do things like explain and predict. My basic epistemological convictions also include the counterintuitive fact that, while the science we create will cleanly surpass our naive ways of thinking, it will always bear some earmarks of those ways of thinking. “The whole of science is nothing more than a refinement of everyday thinking,” Einstein said. Although he did not in this quote mention the remaining earmarks of everyday thinking, he did so in other parts of his commentary on the nature of his science.5

3. Observation is never theory-free.

Auguste Comte (1855) said:

> If it is true that every theory must be based upon observed facts, it is equally true that facts cannot be observed without the guidance of some theory. Without such guidance, our facts would be desultory and fruitless; we could not retain them: for the most part we could not even perceive them.

(p. 27)

While I don’t endorse much of Comte’s positivistic philosophy, this quotation contains, for me, the nub of a highly consequential truth. I think allegiance to this idea is widely shared. My guess is that the spirit of this idea is broadly shared in KA circles, and probably a little less so in IA circles. But see, for example, the interesting spin on this point in Hall (2000).

Now, I need to make a few modifications and extensions of this observation to make it maximally helpful. “Theory” in this quote is problematic in a number of respects. I do not think everyday observation implicates scientific theories, per se.
We need to extend the meaning of “theory” for these purposes to make contact with the inarticulate and non-scientific frameworks that lie behind observations of all sorts. Being able to see everyday things imputes conceptions of objects and space that are certainly not articulately formulated nor explicitly tested. I would say they are just not of the same fabric as scientific theories.

It is also relatively common and convenient to separate the observation theory from the focal (“substantial”) theory, that which is in development. Optics might constitute an observation theory for Galileo’s telescopic investigations of the celestial sphere; then, one might inquire about the substantial theory, what are the markings on the moon’s surface: mountains, craters? Optics, per se, doesn’t tell you what’s on the moon, but it allows you to interpret what you see through a telescope. Consistent with my felt need to extend the meaning of theory to make sense of the idea that observation is always theory-based, another rendering of “theory” is as the vocabulary of observation: a collection of ontologies of things we believe we can just “see” in the world (even if we often don’t recognize the theoretical/conceptual infrastructure of “seeing”).

4. Science “rethreads” the universe.

Science consistently, if not always, rearranges the things we take as “going together” in the world. Here’s a homely example. Let’s say we are interested in the phenomenon of flames, such as a candle flame. As a matter of fact, one needs at least two perspectives, theories let’s call them, to understand flames. First, a flame is an example of an exothermic (it gives off energy) chemical reaction. Now, naively, flames are not much like the things that are joined under the rubric of exothermic chemical reaction, such as rusting or explosions. It is easy to imagine (if it is not inescapable) that the pre-scientific construction of “the phenomena of interest” would leave out both rusting and explosions, since their qualitative features are so distinctive (violence rather than steady state; absence rather than presence of discernable heat and light).

The second theory of relevance to flames is fluid dynamics: in this case, steady state or chaotic flow of “fluids” such as air and vaporous hydrocarbons. From this perspective, flames go together with laminar (seen as layered symmetry in the flame) or chaotic flow (flicker), which create friction against a moving car. And flames go together with things that saliently display viscosity, such as considering certain properties of honey versus water, which properties are hardly perceptible as consequential, naively, in flames. Science “rethreads” flames from “a phenomenon of interest” to something with at least two major interwoven threads that each connect to bizarrely different things, at least as perceived by the pre-theoretical mind. In my micro-essay on approximate modularity (diSessa, this volume), I used the term “causal threads” to describe the general class of things exemplified here by exothermic chemical reactions and fluid mechanics within the phenomenon of flames. I believe that, in the learning sciences, nearly every phenomenon of
interest will turn out to be multi-threaded in ways we mostly do not have a good grasp on right now, or in ways that are often construed as alternative accounts, like culture and individual cognition, rather than contributing threads.

I have not been able to think of any counter-examples to the rethreading principle. Some of the phenomena and theories mentioned under “theories are not on the surface,” naturally and for understandable reasons, also count as rethreadings – from initial or surface-based threadings to alternative ones.

5. Levels are legitimate, if complexly and diversely related.

The best and easiest starter example of levels that I know concerns digital computers. First, one has the surface phenomenology of interacting with a modern computer. Then, below that, there’s the level of programming. Below that, lies the level of device circuitry. And finally, below that, there’s the level of the physics of the devices (often, quantum mechanics, which is necessary to understand transistors and larger-scaled devices built out of them). Now, one striking fact about these levels is their apparent independence. One doesn’t have to know about programming to operate a computer (although whether one should is a different matter). Programmers, in turn, don’t need to know anything about circuits. I believe that circuit designers don’t, in general, know much quantum mechanics.

Part of the specialness of these particular levels is that they are designed, and they are designed, in part, so that people acting at one level do not need to understand the lower levels (or much about the higher ones). Levels that are not designed might not often have such clean modularity relations; that’s just a fact of life.

In general, the phenomenon of levels gives me essentially no pause in physics and even less in the learning sciences. We certainly don’t know everything about levels as a general phenomenon in science, but the rough landscape seems clear and unproblematic. Here are some representative observations, mostly having to do with relations between levels.

- **Homomorphic levels** – One of my favorite levels examples comes from physics, and I think it is scientifically unassailable. It turns out that there is an axiomatic formulation of thermodynamics. My first graduate textbook on thermodynamics took this approach (ter Haar & Wergeland, 1966). Shockingly (especially to many educators), one does not need to know anything about little randomly moving particles to develop axiomatic thermodynamics. In fact, that version of thermodynamics “doesn’t care” whether the world is Newtonian or quantum mechanical. The world turns out to be quantum mechanical, but one doesn’t have to know that to learn axiomatic thermodynamics. It doesn’t in any obvious way do a learner good even to know quantum mechanics before learning axiomatic thermodynamics.
So, the basic phenomenon is that one can know a lot about thermal phenomena (not all that is possible, just a lot) without the details of the lower level. This reminds me of homomorphism in mathematics, where a slice of the structure of a system happens to be a perfectly understandable and complete “theory” all by itself, even maintaining a lot (or all!) of the vocabulary and structural relations of the “lower,” more complex level. Hence, I’ll appropriate “homomorphic” for this kind of levels relationship.

A different kind of homomorphic-level relationship involves approximate or rough models. Bohr’s atom is one such example. It came before the full machinery of quantum mechanics, but got the essence of what quantum mechanics could explain that classical treatments could not: discrete spectra. At the same time, I believe everyone knew from the very beginning it was “wrong,” at least in its details. Then, after quantum mechanics was more developed, Bohr’s model was discarded, although it had played a critical role in the process that led physicists “kicking and screaming” into the quantum mechanical world.

Approximate but theoretically productive models are frequent in physics, and I think they may be more frequent in the learning sciences – except that I seldom find the equivalent epistemic attitude in the learning sciences. Learning scientists seem to me to be fixated on right or wrong, using the things a theory cannot do just to reject it (and thus not learning from the things it can do), rather than as opportunities to advance understanding.

I regard popularizations of scientific theories, also, sometimes as honorary homomorphic levels. They may use the same words as professional scientific discourse, but may not be sufficiently precise or detailed to constitute science proper. However, they are a proxy for the more subtle and often inaccessible theories in the highly consequential crucible of, let’s say, educational application of theories of cognition.

- Implementation relation – The original computer example of multiple levels displays canonical implementation relations. For example, one doesn’t need to know anything about circuitry to program a computer. Yet, if we could not realize the abstract structure of the programming level in physical devices – that is, if we could not implement it – we would have no computers. In some cases, we are in a good position to know that there must be some implementation relations to an underlying level, even if we do not even understand that level much at all. So, realizing the detail of the implementation will always remain a good scientific project, no matter how good the implemented level is for our practical purposes. I think we can create good theories of knowledge* (good, modern theories of knowledge; see diSessa, Sherin, & Levin, this volume), yet I recognize that it would be good to know how those get implemented in biological mechanisms (as mysterious as the mechanisms of the brain still are). It might be useless to understand the implementation if
our theories at the knowledge level are good enough. But it still stands as an important scientific thing to do.

On the other hand, I think the right default assumption is that understanding implementation will probably add useful detail, at least. To take a perhaps controversial example, we might propose that culture (or interactional structure) is implemented within a cognitive level; then I would strongly suspect that the cognitive level would be quite illuminating of the higher level. Said differently, I would guess that culture is not as independent of cognition as one finds, for example, between programming and circuits. This contrasts with my guess that, at this stage, we do not have a lot to gain by reducing cognition (knowledge*) to brain processes.

- **Function/structure relation** – This is a special kind of implementation relation. Any act of design generally has a functional “top” level. That does not mean the top level has no structure in the sense of an articulation of entities, relations, and even processes (consider, again, programming). Anticipating later discussion, one might say that the functional level may also have “machinery,” even if one is more tempted to use the term for the underlying level, the one that implements the upper level.

I believe that it is fair to say that whatever ontology function belongs to, it is immaterial. One “sees” things like the function of tiger claws in their ability to catch and hold prey only metaphorically.

I close this section with a negative tone. Since I believe in levels and the value of understanding their complex and diverse relationships, I personally find the idea of eliminative materialism abhorrent. In cognitive science, eliminative materialism comes down to the claim that since the mind is a biological machine (neural mechanisms, let’s say), all explanation must, ultimately, be at this material level. Within the eliminative perspective, we must categorically throw out all the “folk” beliefs that we have inherited (such as belief in “concepts,” or even “mind,” as separate from brain), both because folk ideas are just wrong (which is easy to show; consult the huge literature on “misconceptions”) and because they are of the wrong type, wrong ontology: They are not brain processes. John Searle (1980) and various neural scientists such as Paul and Patricia Churchland (e.g., Churchland, 1981) have argued for versions of this position. I believe that there is no basis to rule out important homomorphic levels before trying to find or create them. Ruling them out might greatly stall worthy and practical advances that could even provide hints and constraints in aid of understanding the implementation level. If statistical thermal physics had occurred first in the history of science, would the axiomatic version have been ruled out? Should we have refused the more phenomenological roots of thermodynamics because they were independent of quantum mechanics? For a congenial critical view of reductionism (a more general form of eliminative materialism), see Midgley (2011).
Illumination and Critique of HM and ND

Most of the real work for this commentary is done. The astute reader has noticed anticipations and probably filled in a lot of what is to follow, which is a set of observations concerning, on the one hand, the meta-scientific point of view laid out by Hall et al. in their concepts of HM and ND, and, on the other hand, the comparable view laid out here. Mostly I find misalignment and difficulties assimilating the HM/ND view into mine. In what follows, I use the principles announced above singly or in combination, developing them a bit when the need arises, in order to consider the nature of HM and ND as cogent and helpful in construing cases of scientific inquiry.

General Considerations: HM and ND Versus Theory and Observation (First Pass)

I put forward the constructs of theory and observation, augmented by the concept of levels, as a kind of competitor to the HM/ND dichotomy. It's not exactly a replacement, but it deals with similar phenomena. Indeed, I think it deals better with some phenomena explored in the Hall et al. paper than the HM/ND frame. After unveiling relevant considerations at this level, I will turn specifically to what I find difficult to assimilate individually about HM and ND.

Let me rehearse more of what I take to be useful lore concerning theory and observation. First, “everyone knows” that theory and observation are both necessary and important in science, even if, at various times, one may be highlighted more than the other. After a strong, well-formulated theory is put forward, one seeks to test it via observations. Theory also can emerge from extended, careful consideration of a large body of observations. This is a mode with which I have a particularly strong connection (e.g., Parnafes & diSessa, 2013). Examples of diverse short-term and long-term interactions between theory and observation and examples of different sequential relationships are easy to generate. This is the same epistemic game that Hall et al. propose to play concerning HM and ND. I mention a few more useful such relations below.

Theory and observation are taken by many to be intimate, reflecting Comte’s formulation. So, extended phases of working on one without also working on the other should be relatively rare. Phases of doing one without the presence of the other should be rarer, or possibly non-existent. I don’t see any sensibility in theory and observation being alternative world views (as HM and ND are put forward) nor for professional communities to settle around one or the other, exclusively (though, perhaps, mathematics, which is so important to the infrastructure of certain theories and to the “processing” of some classes of observations, might be an exceptional case). I don’t perceive any of my immediate colleagues, including the authors of the generous* paper, to be markedly unbalanced with regard to theory and observation. Perhaps that is a great unrecognized resonance between KA and modern IA.
Observation theories, per se, offer another model of the relation between theory and observation. Some theories are necessary for our very observations, but they often or always are unconcerned about the key theoretical elements of emerging new theories – the theories for which observation theories produce observations. Recall that optics, as a kind of observation theory, can justify interpretations of what Galileo saw in his telescope; then one can go on and investigate matters concerning what is observed. Optics, per se, has nothing to say about the structure of the moon. Being relatively independent of the theories on which their observations bear is, in fact, one of the main epistemological points of observation theories. The theories of light and interactions with matter (the case of Ramón y Cajal in Hall et al.’s chapter) are precisely of this form – they serve as observation theories – a point not noted by Hall et al. So, relatively (or extremely) independent trajectories of development for, on the one hand, theories of particular domains and, on the other hand, observation theories, are not at all surprising. Darwin’s case is decidedly different. DNA, identified as a HM phase in Hall et al., most definitely intersected the theory of natural selection. It filled in details of inheritance and also of the causes and kinds of variation that Darwin knew he needed but could not resolve. It seems this particular relation could be rendered well by saying that DNA constituted an implementation level for some aspects of natural selection that were inaccessible to Darwin.

Observation theories are sometimes applied over extended periods of time and in composition with other theories to produce workable technologies of observation. Hall et al. make a similar point in the second half of their paper. This form of development in science seems to me very insightful of what Ramón y Cajal was doing with microscopy and tissue staining. More generally, the degree of interaction and time overlap between technology development and observation theory development are generic and fairly open parameters of interest in recounting any case of developing observation technology.

Already we’ve seen some appropriation of HM/ND phenomenology into the more familiar scheme of theories, observations, and their relations. To the extent that HM/ND is like theory/observation (and I think there are some strong similarities: ND looks very much like observation; HM looks like a restricted type of theory), other characteristics of HM/ND begin to be suspect. For example, the assumption that HM and ND can exclusively occupy scientific pursuits for extended periods of time and that they may also be exclusive distinguishing characteristics of professional communities might be called into question.

Implausibilities of the HM Mode

I find that HM makes implausible assumptions about how often, even whether, reduction between levels can work in understanding complicated systems. First, at a general level, my own dispositions include a high degree of diversity of modes of scientific study, as marked by Collins’ epistemic forms and games and also
considering the multiple dimensions of meta-science that I’ve broken out. This is the “rich and generative” principle in my initial meta-scientific sketch. The extent to which cases can match the HM and ND prototypes will be an issue in some of the details below. In particular, I will show some substantial commonalities between Darwin’s characterized-as-ND work and HM work. And, I will also put forward some elements of my own work on p-prims, characterized by Hall et al. as HM, that seem to match elements of what Darwin did.

Let us start by pursuing reduction (or eliminative materialism) in the simple context of clocks as an example that might well inspire the idea of the HM mode more generally. The big picture, which I’ve already introduced, is that the way Hall et al. portray the HM model appears to me a form of reductionism, or even eliminative materialism. That doesn’t work for me.

If clocks represent the HM mode then, we must, as previously noted, take cognizance of the fact that clocks can be built out of very different technologies — gears and springs versus digital electronics. Looking very carefully at gears and springs or other technology may tell you some things about clocks, but there is so much in that level of machinery that it seems plausible that clockness, per se, may always be clouded in the complexities of that underlying level. How do we pick out what is essential for clockness from knowing the implementation technology? In the end, even if we can explain clocks at the implementation level, the (homomorphic) higher-level view of clocks might still be immensely important in sorting out what is clock-relevant about gears or digital electronics and what is not.

Coming at things from the other direction, imagine an alien archeologist from a different universe arriving on Earth after our civilization has gone. They study gears and springs, or digital electronics, and figure out their principles of action. Yet, what do they then know about clocks and whether examples of gears and springs are “the same” in any respect (they might be clocks) as another set of examples of digital electronics?

Here is what I think the alien should come to know about clocks. Clocks do chronometry. They should realize that one might want to measure time, or locate oneself in time, and it might also be important to allow easy access to multiple grain-sizes of measurement over which we might need to do chronometry, such as hours, minutes, and seconds. These different scales are implemented as hands on a mechanical clock, and in other ways (e.g., separate displays) on most digital clocks. “Doing chronometry” could be regarded as a functional level for clocks. Then, the technology is at the implementation level, which might be necessary to understand some things about particular clocks (their behavior in varying temperature, for example), but it does not call out the top-level and most important thing about clocks in a perspicuous way, or possibly not in any way.

At this point, I make an observation in anticipation of later discussion. In the case of clocks, the designer intervenes in creating the specifications for clocks (and also, likely, in relevant implementation of these specifications, respecting the characteristics of the implementation systems). This introduces a plainly different
causality, a kind of teleological causality, separate from that which one finds in the implementation technology. Now, Darwin’s natural selection does not have a designer per se. But, the process (evolution) that results in the creation of a particular “design” (a species) is also at least somewhat independent of the causality within the system as implemented (how the species achieve certain functions). In other places, I call the “design” causality long-loop causality, to mark that it is an extended process (in time and space) that creates configurations that are locally (in time and space) causal in the “designed” thing that serve the functions at issue.

The Problematic Rendering of Visibility in the HM Model

Hall et al. make the following point about visibility of machinery in the HM model: “[T]here is the expectation that the postulated entities (e.g., electrons, genes) will, eventually, become observable in multiple and independent ways.”14 I do not think this is a good assumption for any decent meta-science. Certainly small (or, in general, hard to see) physical entities may become visible, as they did in the Ramón y Cajal case, but this is a small class of scientific entities. I believe that, even in the Ramón y Cajal case, a lot of importance resides in the underlying “mechanisms” within and between cells, which mechanisms are not visible. The chemical processes in cells or electrical interactions across them are not things that can just be directly seen, like cells themselves. In the final quotation of Ramón y Cajal cited by Hall et al., he seems to make abundantly clear that the importance of seeing actual cells is that the mechanisms of signaling along nerves then must be of a different sort than previously hypothesized. Concerning mechanisms and processes, of course, we can make representations of them, which are visible, but a stronger microscope, per se, doesn’t particularly help. Instead, we build firm inferential pathways from things we can see (perhaps meter readings or other measurements) to the central entities and processes of our science. In the case of cells, we need to understand diffusion, metabolism, inter-cell electrical influence and the like, and in the relevant inferential pathways, of course, theory is strongly implicated.

One might think that the idea of visibility of underlying mechanisms is inherited from physics: “the clockwork universe.” But nothing could be further from modern physics than visibility. No one has ever observed a quantum field, which constitutes the bottom level of explanation for a lot of modern physics. Indeed, because of the nature of quantum mechanics, quantum fields are in principle not directly observable. That is no problem, however, because strong inferential pathways lead us to and from observable things. Quantum fields are consistent with essentially every observation we have been able to make in physics, and they explain some details that are inaccessible to any higher level of explanation. The question of whether electrons (mentioned by Hall et al. in the quote above) are visible might be a boundary case. Electromagnetic waves (visible light, or even X-rays) are too coarse to see even atoms, let alone electrons. One uses “matter”
waves (ironically using electrons) to see atoms instead of electromagnetism. But electrons are literally infinitely smaller than atoms, since the best modern theory has electrons as point particles. How can one see that? One can’t. One only finds that the assumption of point particles is consistent with other theory, and also consistent with inferred and observable consequences. On the whole, then, I do not think that electrons are visible.

A similar story about visibility can be told about general relativity (and, between quantum fields and general relativity, we’ve pretty much covered the bottom level of explanatory frameworks in modern physics). My instructor of general relativity in graduate school, a Nobel laureate, gave his last lecture saying that “bent space” was just an effective metaphor, and particle exchange was what is “really” happening: Gravity is a force (particle exchange is the modern model of “forces”), after all. One simply cannot observe bent space in order to determine if it is there. One just observes implications of that theoretical construct.

Thinking on a grander scale, one should be skeptical of insisting – or even expecting – that the modality of vision has any claim to exclusivity in defining science. Vision is a very particular, evolutionarily endowed capacity, and one implemented with a very particular and limited technology. The proper replacement is inferential access, as suggested above. I am opposed to insisting on visibility as a basic assumption behind any scientific mode. To the extent such a claim is present in the idea of a HM mode, I consider it suspect.

To sum up, theoretical descriptions often or always do not look like the familiar world of observations we make as humans. Meta-scientific projections that depend on unquestioned observation principles can’t take us very far into the secrets of the universe.

Implausibilities of the ND Mode

I continue by considering implausibilities that I see in the ND mode, taking off pretty much at the final point made just above. ND science supposedly enacts an ontologically flat science, one that sits firmly within the level of the “phenomena of interest.” Furthermore, Hall et al. seem to insist that this level is mono-ontological. “Explanation remains with the observable ontology” (emphasis added). Is this sensible? I previously argued that science rethreads phenomena, so it seems unlikely that scientific scrutiny of a phenomenon can remain ontologically flat. But there is a prior problem. What is the status of observation before and possibly without relevant scientific theory?

Here, we return to Comte’s observation and my amendment of it. One cannot observe without a “theory,” where “theory” includes implicit and intuitive frameworks that have not had scientific scrutiny. Observation that one might do “within the ontology of the phenomena at issue” is not free from such “theories,” and they are almost certainly not mono-ontological. For example, concerning
Hall et al.’s narrative on Goodall, she and her detractors made observations about the spatial locations of particular creatures and other things, such as feeding stations. So far, we may be within one ontological field, having to do with objects in space. But what is the status of other observations that they made? For example, consider this sentence from Hall et al.’s chapter: “Goodall’s observations about chimpanzees’ aggression have been criticized as a result of feeding stations used at the Gombe Stream Park, suggesting that war-like behaviors observed by Goodall were not ‘natural’ among chimpanzees.” What are “aggression” and “war-like behaviors”? They are certainly not spatial. Instead, these are something like projections of psychological categories that are familiar to humans; so enters another ontology. Things get rapidly more complicated. One imputes psychological states from facial expressions and gestures, so there are “theoretical” links for these things (whatever their ontology) to psychological or bodily states. Relevant “theories” also impute consequences for psychological/bodily states (anger breeds aggression; aggression breeds fear in others; aggression and physical conquest breed status).  

I do not believe that any theory is mono-ontological. To take a familiar example where we might have a clearer view of theories and ontologies, Newton’s laws, \( F = ma \), involve both force and acceleration. Acceleration is within the (ontological) category of space–time geometry. Force is quite different. It connects tangentially to space (forces have a place and a direction), but essentially new features get involved, some almost certainly proprioceptively grounded rather than the dominantly visual grounding of geometric categories. Historically, we can see the important distinctness of these categories. Galileo did quite well with the space–time concepts of speed and acceleration. He did not really get (the ontology of) force figured out.

All in all, I think what Hall et al. describe as mono-ontological levels are better rendered as mono-theoretical levels; they involve all the relevant ontologies from one “causal thread.” These are perfectly plausible from my meta-science perspective, but somewhat rare. As I explained, more typically, especially during the scientific development of a phenomenological domain, multiple threads are more likely. The sort of ND “observation” portrayed by Hall et al. may be even more problematic in not recognizing the “theories” behind observations. So, pure ND observation may be “ragged,” involving an unrecognized collection of observation types and implicit “theories,” and it may be unprincipled in that no analysis of the relevant ontologies and theories is even broached. Our tendency to naturalize familiar observations as direct and unproblematic hides what we must do as scientists. We must expose our potentially hidden and unrecognized “theories” (whether they are observation theories or more “substantial ones”), and improve or replace them as necessary.

A final note: What can Hall et al. mean by (paraphrase) “bringing to the fore the wider significance of previous observations,” which they take to be a typical function of ND research? I think the way that wider implications are created is
typically to build a new theory or significantly alter a previous one. So, more or
different ontologies are even more likely.

To reprise the argument, the ontologically flat assumption seems to me implaus-
ible for any rich set of observations; it is implausible for any theory, even intuitive
observation “theories,” and it is even more implausible in doing something like
“exposing wider impact.” The only plausibility I see for ontologically flat observa-
tions’ building significant scientific results is that for some (few) purposes, implicit
and possibly everyday “theories” might be good enough. I believe that sometimes
happens, but it is never a “restful” place to remain in scientific inquiry.

Darwin’s Theory

This section backgrounds general concerns about ND, but tries instead to look at
what Darwin did with a different eye. I propose that there is a lot in common in
his work, characterized as ND by Hall et al., with HM (to the extent that we can
ignore some of the inherent difficulties I see in the concept of HM). To anticipate,
we will see multiple levels, new ontologies, and even “machinery,” all of which are
ruled out in a characterization of Darwin’s work as ND.

Hall et al. emphasize observations in their account of Darwin’s work, and
they de-emphasize the nature of his theory. When we look a little more carefully,
I think what he did looks very different from ND. This is not to say that Darwin’s
acuity and breadth of observation do not make him singular. It is just that obser-
vation alone is only part of the story.

To embark from a previous point, function is front and center in Darwin’s the-
ory of natural selection. The concept of “fitness” of a species to an environment is
functional. That is, features or structures of the species serve critical life-sustaining
functions…or they don’t, which ends in evolution or extinction. As noted before,
function is not a material ontology. We can describe it as a homomorphic level
above the physical structure (phenotype) of members of the species.

So, Darwin’s accomplishment is in this very important way at least bi-ontological
in a familiar way: function and structure. But there is another ontological innov-
ation in Darwin’s theory that, to me, is even more important. It is an innovation
of a type (“species”) within the ontological category of “causality.”

To my mind, Darwin gave the first account of *ensemble causality* in the history
of science. I will sketch what I mean by this, but the ideas, if not the name, should
be entirely familiar.

In Darwin’s account, species are really an ensemble of individuals, each of
which have little or no ability to change; said differently, there is no causal thread
of essential change (where “essential” means with respect to heredity) within indi-
viduals.20 Change — in particular, change that might lead to the creation of a new
species — enters in at the ensemble level. (Note the level split, here, between indi-
viduals and ensembles.) That causal thread is still quite interesting, even if it is now
familiar. Individuals differ in many ways from one another, and this diversity is
essential to ensemble causality. The fit of each individual to its environment determines its likelihood of survival and procreation. Thus, the future of the ensemble (or, its “average,” if one allows that conceit) shifts. The future ensemble is the future of only some individuals, and succeeding generations repeat the process. Succeeding generations start with a different set of variations, around a new “average” (continuing that conceit). In net, over eons, initially unimaginable variation can take place, including the creation of entirely new and apparently essentially distinct species stemming from the same “original.” As Darwin (1859, 1976) put it, “natural selection … accumulate[s] all profitable variations, however slight, until they become plainly developed and appreciable to us.” (p. 175)

Deep difficulties of students in grasping ensemble causality echo the incredible novelty of Darwin’s creation. One essential difficulty is merely keeping track of the two levels and where change enters in. Another difficulty is in the role of chance and randomness. Yet other difficulties lie in leaving behind (a) “the designer” in this case of natural “design” (to specifications set by the environment), and (b) common tropes of elliptical causality (“ducks have webbed feet so that they can swim”), or even agency-based accounts (“ducks change themselves so that they can swim” — like body builders change themselves so that they can lift huge weights).

Ensemble causality constitutes, in my mind, a new and important ontology in the pantheon of science, all by itself. I think it lies manifestly beyond the vocabulary of observation accessible before Darwin created his innovations. It is not the material sort of level imagined by Hall et al., but I have already argued that material reductionism is a simplistic and limited view of multi-level explanation.

With the exception of “containing things that must be directly seen” (as opposed to inferentially accessible), I do not see any objection to referring to ensemble causality as “machinery.” It has: a relevant set of things (e.g., individuals and species); a set of parameters of these things (e.g., structural variation, “fit” with the environment); and, most importantly, a dynamic that can be “run” to see how evolution might proceed — taller animals had a great advantage over shorter ones in the savanna, hence giraffes! One doesn’t need the implementation level proper, DNA, to imagine roughly how things might develop over time.

The autonomy of the level constituted by ensemble causality is attested to in Darwin’s immense accomplishments in understanding the development of individual species. And yet, we know that important implementation-level contributions were to come. DNA, as noted by Hall et al., added some things, most particularly explaining elements of heredity that Darwin knew he could not explain, and also introducing new phenomena, such as genetic drift (understood as a shift in the frequency in a particular allele in a population), the effects of which Darwin did not, and probably could not, notice. The part of diversity of individuals due to sexual procreation also became clearer and more refined. Darwin also could not anticipate genetic engineering, where we can actually intervene at the implementation level (DNA). Yet, natural selection and ensemble causality stand to me
as a “substantially autonomous level” (which Hall et al. explicitly and literally rule out of ND inquiries), almost a mathematical one (attested to by how easy it is to write a computer program to demonstrate the essence of natural selection; see “the blind watchmaker algorithm” in Dawkins, 1986).

Two final points: Darwin simultaneously changed the very ontology of species, from an immutable essence explaining similarity to a constantly changing and shifting ensemble. As I do, Darwin himself put this change explicitly at the center of his theoretical innovations. And Darwin explicitly lists in the last paragraph of The Origin of Species (1859, 1976) the various threads that he had to discover and intertwine to come up with his theory: growth and reproduction, inheritance, variability and its causes, ratio of increase (geometric growth of unchecked populations), struggle of life, natural selection, divergence of character or extinction. I cannot see these, individually or collectively, as ND accomplishments.

Natural Selection and P-prims

Since p-prims were explicitly called out by Hall et al. as HM creations, I would like briefly to present my own view. First of all, p-prims are not things we can ever expect to see. Or, at least, I am quite agnostic about their ultimate disposition. At this stage, I offer them as a model, a homomorphic level, that, like natural selection, has its own insights to give (if rather more modest ones than Darwin’s). I don’t expect that there are necessarily any corresponding “things” at the neural level. However, I am open to the possibility that there might be some semblance of this. I hypothesized that p-prims operate as kinds of recognitions. To the extent that the brain implements recognition in very particular ways, details there may help with understanding the phenomenology connected to p-prims.

P-prims have their functional aspects; they contribute feelings of confidence and naturalness, or, on the contrary, feelings of something’s being wrong. Hence, the same non-physical ontology (function) plays a central role. In other corners of my work, function is even more prominent. Coordination classes are in essence completely functional things. To be sure, things like p-prims can play a role in implementing the functions of coordination classes, but the concept of coordination class, itself, is functional. If any part of my work is strictly in HM modes of doing science, I don’t see it.

In other things I have written, I have proposed a version of evolution as a general framework for understanding knowledge and intelligence (diSessa, 1994). I begin by rejecting reductionist claims, such as those by Searle, who claims that “brains cause minds” (Searle, 1984, p. 39). I then move to the evolutionary perspective that “the history of their development causes minds.” The distinction between local causality (brain mechanisms) of minds and the long-loop causality of evolution is central here. The details are not relevant, but, to the extent that Darwin was doing something different than HM-based science, so am I. Of course, according to my view, neither of us is doing (exclusively) ND- or HM-based science. Within
the meta-scientific perspective I have put forward here, it is difficult for me to see any stark differences between us.

While Hall et al. minimize the importance of theory and ontological innovation in Darwin’s work, I emphasize it. Theory, per se, is particularly important in my meta-science, and also in the science that I do. Again, I don’t believe there is much difference between the way that I work and the way Darwin did, at this level of consideration. In generating the p-prims model, I iterated for several years on a rather large (though puny, in comparison to Darwin’s) database of observations, looking for ways to understand it. Here is how Darwin (1859, 1976) describes his process:

When on board H.M.S. Beagle, as naturalist, I was much struck with certain facts in the distribution of the inhabitants of South America, and in the geological relations of the present to the past inhabitants of that continent. These facts seemed to me to throw some light on the origin of species – that mystery of mysteries, as it has been called by one of our greatest philosophers. On my return home, it occurred to me, in 1837, that something might perhaps be made out of this question by patiently accumulating and reflecting on all sorts of facts which could possibly have any bearing on it. After five years’ work I allowed myself to speculate on the subject.  

(p. 65)

This does not sound much like the dispassionate ND observer, collecting and dispatching observations for no systematic reason. It sounds like a methodical search for a new theory, which eventually replaced his starting theory. The search was stimulated, in fact, by incongruences he had observed between his expectations (cultivated common sense about the distribution of variations of species) and what he glimpsed might be contained in some of his observations on the Beagle. Importantly, Darwin describes that this search for relevant facts began in earnest after he had made all of his observations in the field. Like Comte’s “desultory facts,” much that he observed in the field was abandoned, and only a select part, suitably refined to best relate to his evolving theory, remained.22 Darwin, in my eyes, is a grounded theorist through and through, who was patient enough to bother to create new ways of seeing, new ontologies in the form of new concepts (e.g., a new version of “species”), and even a dramatically different “machinery for the universe,” which I’ve called ensemble causality.

Finally, Hall et al. give the impression that individual scientists and communities can get stuck with their predilections to do one kind of science (such as HM-based science) or another. I am more optimistic, based on the “kicking and screaming” phenomenon. In reflecting on the development of the p-prims model (diSessa, 1994), I described myself as a “disappointed physicist” (p. 2). Many of the initial expectations that I had about naive physics turned out not to work. I liked the simplicity of compact and timeless expressions of theories one
frequently finds in physics, such as Newton’s laws, Einstein’s general relativity, or Maxwell’s laws. Though “kicking and screaming” is overly dramatic, I nonetheless found myself pulled into a very evolution–like regime, where accidents of history created a wonderful, if essentially complex, variety of species (p-prims) that we need to know individually in order to understand present–day cognitive ecology (diSessa, 2002). I didn’t sign up to become a biologist in changing from physics to the learning sciences. But, Nature taught me a relatively painless lesson on fairly short order.

In the last paragraph of his own introduction to his monumental volume, Darwin (1859, 1976) also remarked that he had been dragged “kicking and screaming” to an initially very implausible conclusion:

I can entertain no doubt, after the most deliberate study and dispassionate judgment of which I am capable, that the view which most naturalists entertain, and which I formerly entertained—namely, that each species has been independently created—is erroneous.

(p. 69)

Later work compelled him to an even more counterintuitive conclusion, one against a central fabric of his society, and one with which he struggled himself and within his household: Man descended from apes. Could there be any more graphic illustration that the epistemic forces of science well accomplished can overcome initial predilections, however strong these might be?

**Conclusion: Where Are We? Where Can We Go from Here?**

When I read Hall et al., I found myself swamped with intuitive reactions that ranged from puzzlement to feelings of disbelief. “Hidden machineries,” which was to describe at least some of my science, entailed a brand of material reductionism that seemed unambiguously outside of my own experience with science and the way I understood parts of the history of science with which I have an acquaintance. “Natural description” seemed to me to focus exclusively on things that can be naively seen, as if to deny that science essentially always uncovers hidden worlds that don’t look like the overt one. Natural description seemed to naturalize naive observations and put them into the category of “unencumbered by theory.” That struck me as awkwardly naive realist. Descriptions of science that I know something about (Darwin), but also science of which I was only barely aware (Ramón y Cajal), put some phenomena in strange boxes (treating sequences of sciences, which were about different things, as if they were some natural sequence within one domain). And some things that I felt were absolutely central (the essence of Darwin’s great theoretical accomplishment) had essentially no analysis.
So, then, this commentary is an attempt to unpack my own intuitive reactions, to systematize them a bit, and to make them sensible to others.

What is the outcome of these efforts? In a certain sense, we have not come very far. I believe my exposition has made clear that there are different predilections concerning “how science works.” But, as I said early on, I don’t want to go so far as to say mine is a “better” or more faithful to what “really” happens. Meta-science is murky, unsettled, and contested. Furthermore, I don’t think we know how my dispositions and those of Hall et al. are distributed across the IA/KA “divide.” I am acutely aware that some of my predilections are personal, and I have no real basis to support my feeling that some are very widespread, at least in certain communities.

But, here’s the revised generous* inquiry that I offer in reaction to that of Hall et al. I would be interested to understand the distribution of some relatively full range of meta-scientific ideas across the KA/IA “divide” as it now stands. It seems certain that other bases for describing science would need to be added to the list created here. Then, we might be in a better position to find a core out of which we could build a more synthetic view. Or, at least, we could become clear on where we stand and have targets to settle in further progressing toward synthesis. Another potential usefulness would be to look carefully at emerging synthetic research, as represented in quite a number of chapters of this volume, to see how it plays out meta-scientifically. Are we all being dragged “kicking and screaming” toward a more similar view? Or are we, in fact, playing the rapprochement/synthesis game in different ways, depending on our previous dispositions? All of this does pick up the “game” of closely examining cases of the development of science to see what they look like, which was suggested by Hall et al. and which I most heartily endorse. That may be the best game we can play at the present time with respect to meta-scientific issues, and it stands out as a strong commonality across my views as expressed here and those of Hall et al.

I don’t know how instrumental this inquiry can be in moving IA and KA toward some more definitive rapprochement than has been achieved. However, “how science works” is certainly an inquiry for the ages. I personally find examining others’ science, and my own, infinitely engaging – and at least potentially important in changing and improving what is done under the banner of scientific inquiry concerning learning.

Notes

1 Generous* questions are designed to invite attention to our assumptions and commitments, in ways that make them visible for discussion, further study, refinement, and use.

2 We do not have access to the work of discovery in any of these more recent historical cases, and at this point, the investigators may not either. Still, we offer our understanding of how the interplay of ND and HM approaches might have contributed in each case.
3 Explaining structural regularities in interaction in terms of unobserved mental structures, while backgrounding analysts’ work practices that were involved in finding these regularities, may be similar to deleting modalities when claims about Nature are made by scientists in a realist rather than a contingent repertoire (Callon & Latour, 1992). This practice would be consistent with an epistemic framing of natural description as preliminary to hidden mechanism accounts. Explanation and desire go hand in hand.

4 The authors revised their chapter after my commentary was finalized and sent to them. However, I believe that essentially all of my comments remain productive if a few are reinterpreted as “One might think …” explorations, rather than “These authors have committed themselves to ….” The logic in the argument stands, independent of who affiliates with what particular ideas. I will add “in-press” notes on relevant points in this contribution.

5 Einstein maintained that in doing science one searches among our everyday ideas (such as rulers, clocks, and measurement) for ones that are suitable for axiomatizing (rulers, clocks, and measurement, it turns out). The resulting axiomatic system, then, is science.

6 The concept of coordination classes was developed in very substantial degree in order to give an account of the underlying “theory” behind many observations, which may not count as scientific theories in any restrictive sense. In coordination class theory, the inferential net is the equivalent of the relevant “theory” for observing particular “things” in the world.

7 I find Lakatos (1970) illuminating on the status and function of observation. However, there is a huge literature on theory and observation that can easily be found online.

8 This essay was written over a few days after a careful reading of Hall et al. I am sure it has many faults for this brevity of consideration.

9 I once tried to convince Jean Lave that she should not care about certain implementa-
tion levels at all, at least, as regards social and cultural levels and the sketch of a possible implementation level provided by Newell and Simon’s physical symbol systems hypothesis. Her levels of interest were autonomous enough not to need much from this particular potential implementation analysis, or maybe even profit much from it. On the other hand, I stand for the need to incorporate aspects of the knowledge level, where I mostly work, into considerations of culture and activity.

10 I use conventional terminology for function/structure relations even if it is terribly confusing. It should be called something like function/implementation to distinguish between this meaning of structure in “function/structure” and the more generic one, of having a discernable, patterned analysis. Function in itself has “structure” in the more general sense, but its “structure” is, by definition, divorced from implementation.

11 In-press revision (see note 4). The authors added this explicit disavowal to their chapter: “It is key to note that we are not mapping HM approaches to ‘theory’ and ND approaches to ‘observation.’ Theorizing and observing are germane to both.” As I make clear here in other parts of my commentary, I do not put forward theory and observation as one-for-one replacements for HM and ND. Instead, I think systematic consideration of theory and observation and their role in science can replace — with what I feel are clear improvements — some of the interpretations made using the HM/ND language. In addition, I feel that their notion of ND is at least inspired by the category of observation, and the nature and role of theory in ND remains, to me, unclear in their presentation. Similarly, some aspects of HM (not all of them) are, in my view, aspects of essentially all theories, and should thus be found in accounts of ND science, even if we might have to look carefully to see them. In this regard, see my comments in the text proper concerning new ontologies in theories, and, in particular, my later examination of Darwin’s characterized-as-ND work.

12 One can ask questions about clocks that are more specific to particular technology. For example, one can ask “what makes the hands go around?” or “what happens when the
hour digit flips from 1 to 2?” with a digital clock. But I will ask a set of questions that better fit later considerations and relevant examples in the history of science.

13 If I thought I could make it comprehensible, this is precisely the description of clocks with which I would start with a young child. All the rest is details, many of them implementation details that have no generality concerning clocks.

14 In-press revision (see note 4): The claim that entities associated with HM typically become visible was removed from the prior version of the chapter. While its removal is quite congenial to my point of view, as expressed just here in my text, issues of visibility – what and how things are observable – remains a core concern that requires more extensive treatment than is given in the HM/ND analytic frame. In addition, the authors retain the claim that, in the natural sciences, hidden machineries usually become “examinable,” apparently unlike what has happened in the social sciences. If “examinable” means anything like “visible,” then my comments on visibility in physics believe the distinction between social and natural sciences that they tentatively explore. If “examinable” means “inferentially accessible,” then the authors appear to be saying merely that theories in the social sciences are not as well developed as in some of the natural sciences (the inferential network involving theoretical terms is less secure). I agree.

15 It’s actually easy to “find” invisibility in modern science. Features of the world that are small enough (in a way that is determined by specific physical parameters) are intrinsically quantum mechanical. So, our everyday assumptions about “things that are visible” (our implicit “theories of observation”) are inapplicable. There are no things (“particles”) that have particular places to which we can point, for example.

16 The history of physics here is illuminating. When electrons began to get traction as particles, a huge furor arose concerning their reality. On one side, august scientists like Millikan maintained that electrons were simply there. Irate in the face of denials, Millikan said (something like), “I can see the little buggers” jumping from oil drop to oil drop (referencing his famous experiment). I take that statement to be metaphorical. On the other side, highly respected scientists like Ernst Mach rejected electrons as “mere mathematical creations.” Mach never accepted their reality. As time went on, however, physicists got used to electrons’ nature and treated them as “really there.” There was no point, in any case, at which electrons were observed – no new type of microscope allowed them to be seen. Reality for such things is all in robust inferential pathways to observations.

17 “Ontology” is, in my view, a seriously problematic term. I don’t believe it has any agreed meaning to the point that we will be able to firmly settle the question of whether something constitutes an ontology or not. Nevertheless, I will not give voice to those skepticisms here and, instead, I will try to use the term in a way that I expect those who use it will not find immediately objectionable.

18 In-press revision (see note 4): The revised phrasing is “explanations remain meshed with the observable ontology” (emphasis added). This change forestalls the most direct criticism that I had of the former phrasing. However, I do not consider it helpful concerning the principle issues. “Meshed with” is vague. What else, beyond the observable ontology, is involved in explanation? Might there be new ontologies, revised causal threads, or even new theories? These are where the action is. That the observable ontology (presumably, unchanged) remains within the larger scientific and explanatory pursuit might well just respect the fact that scientific explanation also retains some common ways of talking about observations. Einstein’s views (note 5) would seem to guarantee that everyday ideas (e.g., measurements) would appear in scientific explanation. However, their logical status (they are now axioms) has changed radically.

19 I strongly expect – hope – that ethologists have surpassed common sense and anthropomorphic projection in their study of animals; thus would enter more theory development, which would abrogate, or at least challenge, naïve observation.
There is a causal thread of change between generations, via procreation. However, this thread is predominantly random and not specifically directed toward adaptive change. With regard to “elliptical causality,” see my micro-essay on modeling and reality (diSessa, this volume).

Notice that, in the quotes cited by Hall et al., Darwin makes clear that the importance of the (possibly) ND observations that he made early on is precisely that – suitably refined and augmented by other theoretically related considerations – they eventually became part of the grand theory that he created. Notice that “wider significance” is here critical, and the wider significance came about because of theoretical development entailing, as I see it, ontological innovation and new, “substantially autonomous” levels (ensemble causality).

References


Natural Description & Hidden Machinery


