

# Prolegomena to an Empirical Philosophy of Science

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**Abstract** We identify and address a set of foundational questions relevant to the project of an empirical philosophy of science, the most basic of which is the nature of the empirical. We review the task of distinguishing empirical from non-empirical questions by providing examples from our analysis of cognitive and learning practices in biomedical engineering laboratories. We emphasize that the empirical should be understood as rooted in the instrument, and that the instrument comprises the researcher, which includes elusive factors such as disciplinary identity, disposition, and values. The implications of this claim are examined in relation to three empirical approaches to the philosophy of science: historical, qualitative, and experimental.

**Keywords** Method · Foundations · Epistemic values · Historical analysis · Ethnography · Experimentation

## 1 Introduction

The title of our paper, though tongue-in-cheek, harkens back to Kant on purpose. The questions we are asking were present in some form in 1783, and it was with the same basic questions Kant was wrestling: How can we understand the empirical? What are its preconditions and limits? How do we move from the empirical to concepts? In essence, what are the grounds of possibility of science (for him, natural science) and social science?

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It is not our intention to definitely answer the questions we pose, but to open a discussion and call for an effort to confront some preliminary questions and problems attending the use of empirical methods in philosophy of science. The closest we will come to providing an answer for the most basic questions concerning the nature of the empirical (and thus the nature and possibilities of an empirical philosophy of science) is this: *The empirical* is rooted in *the instrument* and cannot be understood apart from it. The instrument, of course, consists of relevant technology and established and reliable methods suited to the using the technology to address a question of interest. However, the point we will develop here is that on a more fundamental level, the instrument comprises also *the researcher* who actively selects and analyzes data.

The researcher's central role in science is easier to appreciate in relation to the non-empirical questions that are part of any investigatory project. By emphasizing the central role of the researcher in empirical questions as well, it might be thought that we risk collapsing empirical with non-empirical questions. But on the contrary, the delineation of empirical from non-empirical questions is the most basic issue in play, for science as for an empirical philosophy of science. We do not have a formula for delineation of empirical from non-empirical questions, yet we can provide examples of delineating efforts and the outcomes of these efforts. To do so we draw from the history of efforts at delineation in the discipline of psychology, where the debate has been long and vigorous. As illustrations, we draw upon our own investigation that entails years of collecting and analyzing historical and ethnographic data to address philosophical questions about the nature of science practice in physics and in bioengineering science. After a brief introduction, we provide two examples of questions prerequisite to the empirical study of science that are not themselves empirical questions and two examples of empirical findings that in our case *have* informed our understanding of the sciences we study, which have broader implications for our understanding of science practice in general.

## **2 Non Empirical Questions in an Empirical Investigation of Science**

The first non-empirical problem that confronts us is a hornet's nest of troublesome categories upon which the whole enterprise of an empirical philosophy of science can be said to rest. Among the most difficult is 'empirical' itself, though 'empirical' connects with or is embedded in a cluster of interconnected terms and fuzzy categories such as 'method', 'science', and ultimately, 'reality'.

The difficulties surrounding the meaning of 'empirical' infrequently find their way into discussions of empirical methods in philosophy. This is itself problematic. An example is evident in relation to the recent trend of adopting empirical methods from psychology to inform philosophy, including philosophy of science. This "experimental philosophy", is something of a curiosity, because nowhere has the

ambiguity of “empirical” created more problems than in the discipline of psychology. Psychology’s fraught history and fragile conceptual edifices should stand as a warning rather than a beacon to philosophers when it comes to adopting appropriate methods for philosophy of science. There is a risk of borrowing psychological methods too hastily to inform philosophical questions while ignoring the more than century long debate over their range, fit, and adequacy. At the same time, there are lessons to be gleaned from the history of psychological science.

One lesson concerns the grounds for adopting a particular empirical method, for choosing one method over another. A prominent view is that the method(s) should be appropriate to the empirical reality, the ontology of what is to be investigated. Thus, for example, as concerns psychology, social or collective processes such as myth-making demand interpretative inquiry; study of differences in individual reaction time requires precise measurement and experimental control. This is, in very rough form, Wundt’s view (1901). But differences in method and differentiated units of analysis can arise in relation to the same phenomenon when differing perspectives are taken on the phenomenon. At the end of the 19th century, a scant 20 years after the opening of Wundt’s laboratory, Titchener described a division within the science of psychology between its structural and functional aspects, between the concern for the ‘plan of arrangement’ in the mind’s ‘mass of tangled processes’ and concern with the “system of functions” that enables mind to “do” things for us or equips us to “do” (1899, p. 290). The emphasis on structure entailed a reduction; the emphasis on function, a systems view (Ahn et al. 2006). The difference in perspective or emphasis accompanied differing sets of questions, different methods, and different levels of analysis in relation to the same subject matter—consciousness (James 1890; Titchener 1898). The focus of structural psychology called for controlled, laboratory based experimentation; the focus of functional psychology required analysis of how processes function, what mind does and what it allows people to do, sometimes in the laboratory but often *in the contexts of their natural activity*. Titchener acknowledged the differing emphases to be complementary, as reflecting the structural and functional concerns of the science rather than as attributable to ontological dispute or convention (“turf wars”). Nevertheless, what might have remained a removed recognition of different aspects of the science became a point of contention and social positioning, a “violent controversy” (Boring 1929/1950, p. 314) prompting distinct “schools,” “systems” or disciplinary provinces by the early 20th century (Angell 1907). Functional psychology all but disappeared as William James and John Dewey abdicated for philosophy, and John Watson sounded a call for replicable, publically verifiable data and an elimination of consciousness as the focus of psychological science. The differences between structural and functional psychologists reflect not the investigation of different psychological processes (phenomena) but different perspectives on the same psychological process—consciousness.

Differences in emphasis are not limited to the period in which the contours of the new science of psychology were in negotiation. In the latter half of the 20th century, computational and situated approaches to cognition proffered different perspectives on the nature of cognition and the kind of empirical phenomena

required to study it, which lead to different starting assumptions and investigative methods, and so to different research programs. The original *physical symbol system* view of cognition (Newell 1980) focused on more professional cognitive tasks such as chess playing and disease diagnosis, while the more recent *situated* perspective (Lave 1988) focused on mundane tasks such as arithmetic use by grocery shoppers and dieters (see, Bredo 1994 for a succinct summary).

## ***2.1 Non-empirical Question #1: What Counts as an Empirical Approach to the Study of Science?***

The point we wish to emphasize is that across psychology's history as a formal discipline there has been little agreement about what is to be counted as an empirical approach, included as legitimate data, as the 'facts' of the science, let alone how the facts should be evaluated. Of course, the question of what is to be counted as the empirical reality is informed by the theoretical assumptions in play, by the model from which one is working. But these assumptions and models are themselves influenced by a complex set of factors that include *disposition, identity, and value*.

An emphasis on disposition and value is embedded in Angell's emphasis on function offered in the preface to his textbook on psychology: "Psychologists have hitherto devoted the larger part of their energy to investigating the structure of the mind. Of late, however, there has been manifest a *disposition* to deal more fully with its functional and genetic phases. To determine how consciousness develops and how it operates is *felt* to be *quite as important* as the discovery of its constituent elements" (Angell 1904, p. iii).

The feeling of what is "quite as important" is, we claim, a matter of *epistemic value*. The source of differences in epistemic value is itself a hugely complicated question. In disciplinary practices, such as adopting a particular method or evaluative approach (e.g. a reductionist vs. a systems approach), surely disposition implicates not only a process of socialization to a specific academic community in which that approach is favored but also cognitive style, such that one is more readily drawn to and embraces the values, attitudes, and epistemic assumptions sanctioned within the community of which one becomes a part. That is, value intertwines with academic identity. Likewise, identity has social and personal dimensions, a personal story line and a social history by virtue of the groups with which one actively and passively identifies, along with the history of these groups. These are important aspects of what accounts for the general theoretical models used and stances taken—the scientific perspective (Giere 2006). As is the case in psychology, such differences are a force to reckon with in relation to the emerging empirical philosophy of science.

There are three main approaches to empirical inquiry in philosophy of science that carry differences in identity and epistemic value: historical, observational/ethnographic, and experimental that we examine below. The main point we wish to

make, and what we intend by the idea of rooting the empirical in the instrument is this: What is to be counted as empirical in an empirical philosophy of science is hardly itself an empirical question. Rather, what is taken as adequately or appropriately empirical represents a choice and a commitment, either an explicit choice made on grounds that are largely philosophical, or an implicit choice based on disposition and membership in a community that shares a set of epistemic values.

For our purposes, then the empirical question of how epistemic values are formed may not be as important to ask as how they function, what affordances and constraints they offer in relation to inquiry in general and the philosophy of science in particular. In the interest of specificity, we now examine how questions of epistemic values and identity are implicated in relation to the three prominent approaches to empirical inquiry in philosophy of science: historical, and more recent observational/ethnographic, and experimental approaches. We examine the affordances and limitations of these identities and their associated attitudes and values. Each of these empirical approaches is rooted in traditions of analysis that have longer histories and broader scope than just philosophy of science. Note that by implicating values and dispositional factors we bracket ontological considerations relevant to the three approaches, i.e., what is appropriate to the subject matter. Instead, we focus on differences among empirical approaches, and, focusing on the relations between approach, disposition, and value, we specifically note the importance of rooting the ‘empirical’ in the instrument, i.e., the researcher.

**Historical Analysis.** Using historical data and methods of analysis has a very long history in empirical philosophy of science, with accounts too numerous to list. The scientific status of historical inquiry and analysis has been in dispute at least since Dilthey (1910/2002), with radically different approaches to historical interpretation emerging in the twentieth century. Despite differences in assumption and approach, the limitations of historical analysis in general are easy to identify. A certain level of vagueness and indeterminacy is acceptable and inescapable. Although there are methods for historical analysis it is simply not possible to codify procedure rigorously, even for historical work deemed positivistic. Hence training or education in historical analysis is distinguished by the substantial role played by apprenticeship. One develops a “feeling for analysis” under the guidance of a mentor.

The absence of a prescribed method in historical analysis offers the advantages of relative autonomy. There is a great deal of flexibility in relation to one’s research questions. Affordances include freedom in relation to the selection of cases or episodes for analysis and the kinds of data to examine, and freedom in relation to analytic procedure. One can be more inventive with one’s methods. It is of course the degree of freedom involved that has served as the point of contention among different approaches to historical interpretation (e.g. Beiser 2011). Historians have long used the resources, insights, and analytical methods of many other disciplines to deepen historical understanding. Historians of science, too, frequently draw on the resources of other disciplines—anthropology, economics, political science, literature, sociology, cognitive science—to further their analyses. What resources outside of history one draws upon in any given analysis depend on the questions one is asking.

In practical terms, historical analysis can be conducted without the benefit of a research team. This is also a potential limitation: Typically historical analyses are carried out by individuals, even if through mentorship. Although not by any means a necessary component of historical analysis, it is a norm. A historical analysis is typically a single perspective on a data set. Finally, the available data are a fait accompli. There is no opportunity to collect further data to inform a question arising in the middle of analysis. For example, in the absence of any records on the subject, one can only make reasonable conjectures about, for instance, what was the problem that led to an 8-month lag between the first two parts and the third part of Maxwell's 1861-2 analysis of "physical lines of force" (Maxwell 1861-2; Nersessian 1984, 2008). Thus it is safe to say that there are questions that cannot be definitively answered by historical analysis alone.

**Qualitative Analysis.** By qualitative analysis we primarily refer to analyses based on observational, interview and ethnographic investigations of scientists in real world contexts of practice. This kind of data collection and analysis is relatively recent in philosophy of science. In addition to our own research, recent exemplars are Calvert and Fujimora (2011), Kastenhofer (2013), Knuuttila and Loettgers (2011). The affordances of ethnographic study are in many ways similar to those of historical analysis, but there some important differences. Like historical analysis, ethnographic analysis affords the opportunity to enter into and evaluate the fullness of the life-world, the lived complexities of scientists and the irreducibly rich contexts of their problem solving, thereby avoiding an artificial abstraction away from these complexities.

A good deal of freedom is afforded with qualitative analysis, the nature of which differs in some aspects from that of historical analysis. One can decide on the form of data to collect, what are the sufficient and important data needed to understand the science, rather than having to rely on the data that are left behind. One is "there" in a way that opens opportunities to make spontaneous decisions about what might be interesting and important. There is a much better possibility of collecting the kind of data that suits and informs ones research questions, than is the case with historical data. One can create new data at will with new observations and interviews.

On the other hand, as with historical analysis, one is somewhat at the mercy of the participants (the scientists), what they are willing and able to provide. In an example from our study, in the first lab we investigated we asked to see researchers' laboratory notebooks. We had assumed that laboratory notebooks, as with our experience with historical analysis, were an important part of any experimental/laboratory practice. We thought, in this case in particular, that these would provide us with a record of the development of the physical models currently in use. However, when asked to produce them for our study, the principal investigator asked "what notebooks?" They did indeed keep relevant information about specific experiments in documents on their computers, but these were largely strings of numbers devoid of any comments or reflection. Thus there is still the problem that scientists engaged in real world contexts of practice might not offer the kinds of data we feel are important. There are constraints on top down analysis, that is, because the data might not be available to answer the questions we have.

In terms of training, one can dabble a bit with qualitative analysis just as one can with historical analysis, to a degree that is not possible with experimental or quantitative approaches. That is, much of the learning is “on the ground,” honed through apprenticeship of various forms. One can get ones feet wet in qualitative inquiry and analysis without a great deal of detailed preparation. There is not a rigorous canon of procedure which one should master before becoming involved in a research project. Proponents of qualitative analysis, however, unlike historical, have devoted considerable attention to methods. Here we focus on debates within psychology, but the issues have been raised in sociology and anthropology as well. Among psychologists, the use of qualitative and interpretive methods have incited controversy within a science designed to achieve independence from philosophy by means of positivistic methods to address questions about mind. Various forms of naturalistic inquiry and exploration of experience were met with criticism from the very beginning, in large part because they were regarded as problematically importing philosophical assumptions into psychological inquiry (as if controlled laboratory experimentation did not do so!): “(A)nything approaching a complete and permanent divorce of psychology from philosophy is surely improbable so long as one cultivates the functionalist faith” (Angell 1907, p. 90).

Challenges to the legitimacy of qualitative analysis as a foundation for knowledge and questions concerning their generalizability and predictive utility have accompanied their use historically. Of late there have been increasing efforts to name and describe various systems of qualitative coding and analysis. Qualitative methods books are proliferating, as are systematic attempts to distinguish the different approaches from one another and occasionally to analyze their common fundamentals (e.g., Wertz et al. 2011, is a recent example of this effort). We suspect that psychology’s continuing obsession with method has had a great deal to do with the emphasis on distinguishing qualitative approaches and attending to their unique forms of rigor. Part of the justification of procedure comes from its belonging to a recognizable and named category of procedure, despite the fact that new research contexts and questions might call for innovations in procedure. Concerns with establishing inter-rater reliability in developing codes and similar matters reflect the same trend. By contrast, anthropology has not had to justify its methods in the same way. The focus has been on the researcher as instrument, as tool. It is enough that the researcher is “there,” in the setting in which the inquiry takes place; there is implicit trust in the veracity of the observations of an eyewitness. Traditionally, ethnographic research is carried out by an individual, although within philosophy of science (including our own research) there has been a trend towards what could be called “team ethnography,” in which multiple perspectives of several researchers are brought to bear on an interpretation.

Among the reasons for concern with legitimacy is that much of what passes for procedure entails seemingly irreducible acts of insight; thus much is not amenable to “neutral” description, let alone replication. Qualitative analyses cannot pass the kinds of reliability tests established for the purpose of evaluating quantitative data, prompting charges that qualitative analysis represents “mere storytelling.” In addition to requiring a willingness to engage methods that remain controversial in

some corners, qualitative analysis, like historical analysis, suits some dispositions better than others. It requires the ability to abstract from reams of data stemming from various sources to form insights, make and hold tenuous connections, generate possibilities. One must have a high degree of tolerance for ambiguity and uncertainty, must be comfortable with messiness and feeling out of control, leaving things open, being surprised, not knowing where one is going. Qualitative analysis does, however, offer the possibility of collaboration and bringing multiple perspectives to bear on an interpretation. Our interdisciplinary research group has had considerable, fruitful experience with perspective sharing or exchange. All of what we are calling the affordances of qualitative analysis are, from another perspective, intractable limitations. One relinquishes control, precision of description, predictive ability.

**Experimental/Correlational/Descriptive Statistical Analysis.** That which is lost with qualitative analysis might be gained with explicitly quantitative approaches, and even more so approaches that incorporate controlled experimentation to inform scientific reasoning. The recent development of an “experimental philosophy” has largely been confined to the philosophy of mind. Contemporary work in experimental philosophy harnesses the methods of social science, especially psychology, to investigate and challenge prevailing assumptions in the context of philosophy of mind (Deutsch 2009; Knobe and Nichols 2013; Machery et al. 2004). Within the methodological traditions from which experimental philosophers draw, the most robust efforts have all of the weight of experimental logic behind them, providing a grounding for inferences that can never be matched by historical and qualitative approaches. Abstraction, precision, control, detail, and statistical power are formidable allies. The corresponding dispositional qualities are not difficult to identify. Not surprisingly, precision comes at a price. Vagueness is not acceptable. In psychology, at least, the training required to do this kind of analysis well is extensive; it requires rigorous training in experimental design and statistical analysis. One cannot dabble in it. It is not enough just to learn some statistics. A significant limitation, though, is that the range of questions that can be asked is much narrower in scope. One must be comfortable with a restricted range of questions and possibilities for addressing them.

*Summary.* Beyond training considerations, the important point is that all three approaches lend themselves to different *dispositions*. They should be regarded as complementary, not in competition, because they are aimed at different questions or different levels of question, which the observational and analytic powers of different researchers equip them to address differentially.

Of course, in suggesting that different empirical approaches suit different dispositional qualities, we risk reducing epistemology to psychology. That is not our aim. We aim only to point out that in practice it is not *merely* ontological considerations that determine empirical approach. But of course ontological considerations should play the major role. Thus we return now to the question of ontology as relevant to the philosophy of science, namely the unit of analysis chosen for the investigation of science practice.

## 2.2 *Non-empirical Question #2: What Is the Appropriate Unit of Analysis for an Empirical Investigation of Science?*

The question of the unit of analysis appropriate to an investigation is not an empirical one. That is, it is not empirical apart from the sense in which history provides a guide to decisions that have been made in previous efforts to investigate phenomena of the kind in question. The considerations are here are ontological, because the unit of analysis concerns the nature of the object under investigation in philosophy of science. Unit of analysis determines perspective: it involves a decision about where to look, how widely to extend the gaze. Decisions about where and how widely to look, in turn, implicate a set of constraints on what can be “seen,” along with the aspects of the phenomenon to which one will be “blind” (Giere 2006). Decisions about where to look, or what to look at, are then followed by decisions about how best to organize analysis in relation to the level of complexity of the subject matter. In short, the epistemological considerations follow from the choice of unit of analysis. The choice of unit of analysis can be influenced by many things. It is the cannon of good science that methods should follow from, not lead to one’s questions. The unit of analysis, however, may be co-implicated in a question, may lead to a question, or may follow from it.

In our own work, our choice of unit of analysis follows from our problem formulation. We have identified what we have called an “integration problem” in science studies (and in psychology, for that matter). The majority of cognitive studies of science have proceeded in relative isolation from social and cultural studies of science, while the latter have largely ignored the need to address cognitive dimensions Both Longino (2002) and Nersessian (2005) separately have pointed to the implicit acceptance of a rational–social dichotomy in both philosophy of science and science studies. There are conceptual problems with any such dichotomy, as Vygotsky (1978) and scores of others have made clear. Therefore the unit of analysis for an adequate understanding of science must be one that does not perpetuate such a divide. For our purposes, we have found it very useful to select as our unit of analysis the *acting person*. The acting person is a social, cultural, and cognitive being with a particular experience, disposition, and identity.

What is implied in ascribing the label of “person” is a longstanding problem with a great deal of baggage, usually relating to intentionality, rationality, language-use, rule-following, or individuality/particularity, depending on the context and purpose. The choice of person as unit of analysis for the study of science may seem peculiar. Michael Polanyi acknowledged the seeming tension, even contradiction, between ‘persons’ or ‘the personal’ and science in his preface to *Personal Knowledge* (1958), noting the impersonal and universal features typically emphasized in relation to science and assumed to be necessary to a proper understanding of its authoritative grounding. In turn, ‘the personal’ is associated with variation, deviation, difference, contamination (Titchener 1912).

However, if our focus turns to the empirical dimensions, to science as it is actually practiced in real world settings, rather than as an idealized conception of

methods, logic, and products, attention to the particularity and experience of the person, the scientist, is a necessary complement to social, historical, and cognitive dimensions of analysis. Thomas Kuhn appears to have arrived at something like this insight:

Just because the emergence of a new theory breaks with one tradition of scientific practice and introduces a new one conducted under different rules and within a different universe of discourse, it is likely to occur only when the first tradition is felt to have gone badly astray. That remark is, however, no more than a prelude to the investigation of the crisis-state, and, unfortunately, the questions to which it leads demand the competence of the psychologist even more than that of the historian. What is extraordinary research like? How is anomaly made law-like? *How do scientists proceed when aware only that something has gone fundamentally wrong at a level with which their training has not equipped them to deal?* Those questions need far more investigation, and *it ought not to be all historical.* (1962, pp. 85–86, emphasis added)

Kuhn's remarks point to the need for enhanced understanding of the overall function of personal factors in the hows and whys of scientific practice—such as how a scientist's awareness of her own shortcomings in relation to a new direction might influence her readiness or resistance to change. This is a question of learning history and identity, of positioning, and broadly speaking, perspective. There are also implications of emotional involvement. Adequate characterization of science practice must at some point come to terms with the problem of the personal, with the fact that people bring different levels of cognitive ability, different interests, goals, desires, problems, experiences and collaborative relationships into any laboratory, no matter how systematic its proceedings. There has been insufficient effort to carefully theorize how these differences impact the “organized, artful practices” that constitute rational achievements in real world settings (Garfinkel 1967, p. 34). A variation on this question is whether “the personal” dimension might be understood not merely as a source of impurity or impediment but as a set of processes that enhance and indeed, enable science.

We have argued that emphasis on the *acting* person encompasses both the intentional quality of action and the social meaning or force of acts accomplished through the actions, for the intentional performances of persons (actions) always take place within socially negotiated or inherited contexts of social meaning (Osbeck et al. 2011). An understanding of scientific practices as normatively structured by sanctioned methods, communal ideals, and field-specific projects does not alter the fact that science consists in activities of persons, nor even does the recognition that economic and political controls are driving scientific agendas in a broad scale way. Persons act to collaborate with other persons using the tools available to them, always in relation to goals, desires, aspirations, and values both collective (values held by the scientific community at large, such as advancing knowledge) and particular (advancing one's career, solving a problem, obtaining closure). The acting person as an analytic unit then integrates intentionality, creativity, and social normativity; it represents an inherently integrated focus of analysis.

Given our focus on the acting person, one promising direction is to concentrate more intently on ways in which identity is implicated in scientific reasoning. The utility of identity for the present purposes lies in the fact that the category historically has involved both personal and social dimensions, an experience of one's own unique history, place, aspirations, and meanings and the groups or social formations to which one claims belonging, prompting the rather clumsy distinction between personal and social identity (Turner 1982). More strongly, relational identity has been suggested as a precondition for the experience of personal identity (e.g. Mead 1934).

Identity is a notoriously ambiguous category, but it implicates a constellation of concepts important for understanding science: value(s), emotion, embodiment, the anticipated and experienced gaze of the other. It is a form of enactment despite the experience of continuity and permanence. The close relation of identity to social positioning means that identities can be seen to establish the possibilities of action. They have epistemic effects, are integrally related to problem solving, influencing what and how one feels able or entitled or do within the wide range of practices that constitute science (see Osbeck et al. 2011, Chap. 5). Such considerations are increasingly important with the growing trends towards interdisciplinary and transdisciplinary collaborations in science.

### 3 Empirical Questions in an Empirical Investigation of Science

The purpose of our remarks so far has been to identify aspects of our analysis that constitute non-empirical questions in the empirical investigation of science. We turn now in the other direction, to give examples from our own practice that *have* been directly informed by empirical investigation of science practice. We draw on two empirical investigations. The first is the multi-year cognitive-historical study Nersessian conducted of the formation of the electromagnetic field concept. Interpreting the historical data leading to the development of various electromagnetic field concepts from the mid-1800s to the early 1900s required her to develop a reflexive method of analysis (Nersessian 1984, 1995). "Cognitive-historical analysis" examines historical records in light of cognitive science investigations into mundane reasoning and representation and feeds back the analysis of scientific cognitive practices into the development of cognitive theory. The second is the multi-year ethnographic study we have been conducting of bioengineering sciences laboratories and, more recently, integrative systems biology labs. Our attention is both to the cultural organization of each laboratory setting and the participatory stance of each researcher in relation to biological phenomena, cognitive tools (e.g. models) and instrumentation central to the science. We regard cognitive processes as *system phenomena*, that is, as distributed across persons and artifacts and situated in physical and cultural contexts (e.g. Hutchins 1995a, b; Greeno 1998; Clark 2003; Nersessian et al. 2003) with cognitive activities made possible (afforded) or

constrained by the specific properties and composition of environments in which reasoning takes place. We have elsewhere described the laboratory as a cognitive-cultural system in that cognition and culture are co-implicated (Nersessian 2005).

### ***3.1 Model Based Reasoning***

When Nersessian began investigating the archival records of Faraday, in particular his Diary, she was struck by the abundance of sketches along the margins and elsewhere that seemed to be playing some role in his reasoning about the phenomena he was investigating at that time. Up to that point she had been indoctrinated as a scientist and then as a philosopher of science with the idea that scientific inference is inductive or hypothetico-deductive reasoning over propositional representations. Although her science teachers sometimes drew diagrams, they never discussed why this might be important. There was virtually no discussion of visual representations in the philosophical literature, and what there was pointed to their role as “mere aids” to reasoning (which is logic-based). The historical literature had likewise tended to ignore them, but just at that time some accounts emerged that looked primarily at the communicative role they serve (e.g. Rudwick 1985). Faraday however appeared to be reasoning through or by means of his sketches and she could tie the articulation of his concept of field directly to specific visual representations. Maxwell, too, seemed to be reasoning by means of a visual representation of what he called a “physical analogy” (Maxwell 1861-2) and with this and other diagrams in his papers, he gave instructions for how the observer should simulate motion of the elements of the diagrammatic representation their imagination. He also wrote several accounts on the importance of physical analogies as a method of discovery. However, his analogies were noted explicitly in both the philosophical and historical literatures as “merely suggestive (Heimann 1970), of “slight” heuristic value” (Chalmers 1973) and at worst as post hoc fabrications, while “the results were known to him by other means” (Duhem 1902). The exception was Hesse (1963) who tried to develop an account of analogy and discussed Maxwell, but was curiously silent about the 1861-2 paper where the physical analogy seemed to be playing a generative role in Maxwell’s initial formulation of the field equations. To make a long story short, Nersessian began to think these data should not be considered ancillary, but that these visual representations, analogies, and thought experiments (prevalent in the records of the practices of other historical scientists as well) constituted a form of creative reasoning—what she called “model-based reasoning.” It took another 20 years of philosophical, historical, and cognitive science research to articulate the nature of model-based reasoning, including its cognitive basis and how it produces conceptual innovations (Nersessian 1992, 2002, 2008). Expanding from the insights deriving from historical data, our bioengineering laboratory studies over the past 12 years have been looking into the creative roles of model-based reasoning more broadly than conceptual innovation, now focusing on physical and computational models and simulations.

### 3.2 *Relation of Emotion to Problem Solving*

We began our investigation of bioengineering laboratories with the explicit goal of characterizing the nature of the cognitive and socio-cultural practices exhibited by researchers across each lab. While coding interview text along these lines we were struck by a good many passages with a decidedly affective tone. Others seemed clearly expressive of desires, goals, and aspirations. We began to assign codes for affective and motivational content and found that it quite naturally sorted itself into three categories of expression: (1) overt expressions of excitement and frustration; (2) metaphorical and figurative expressions in scientists' descriptions of practice; and (3) anthropomorphisms involving an attribution of emotional states to objects, artifacts, and devices. We described these as three classes of affective expression. The important overall point of this analysis is that it demonstrated how closely intertwined affective expression and problem-solving efforts seem to be. We realized how our data implicated ways emotion figures into cognitive acts, that one cannot be entirely disentangled from them without considerable abstraction away from the real world phenomenon of science practice. In turn, we were able to analyze the functional significance of emotion in the overall situation of the laboratory, of which the anthropomorphic expressions are most interesting and significant. In brief, the functional benefits of anthropomorphism are of two related kinds: First, the attribution of emotional states through anthropomorphism reflects implicit emotional processes that contribute to the motivation, interest, and attention of the researcher in relation to the objects and entities central to the laboratory's research projects (Osbeck and Nersessian 2013). Secondly, the attribution of emotion carries attributions of agency. That is, objects central to the practice of the scientist are imbued with agency (functionally so) through anthropomorphism, such that they are transformed into working partners with the research scientist in cognitive practices toward shared and individual problem solving goals. We have construed this process of transforming objects into "partners" in problem-solving practices as "cognitive partnering" (Nersessian et al. 2003; Osbeck and Nersessian 2006). Of course the emotional expressions in the interview text, including anthropomorphisms, did not speak for themselves; they required interpretation and analysis. The point is that these insights concerning scientific reasoning would not have been possible in the absence of the empirical analysis. They would not have occurred to us.

*Summary.* In this section we provided two examples of questions concerning the nature of scientific reasoning that were informed explicitly by empirical research. We first discussed an example from historical analysis, namely Nersessian's discovery that Faraday and Maxwell appeared to be "reasoning through" models of various forms; that these model-building and manipulation processes were integral to their most important discoveries. Secondly, in an ethnographic study of bioengineering science, we discovered that as researchers frequently and quite consistently use anthropomorphic expressions when referring to the physical and computational models that are central to their problem-solving, providing a connection between affective and inferential processes. Although in each case the

findings did not interpret themselves, we were sufficiently “surprised” by them to consider them to be matters of discovery.

We turn now to a more detailed discussion of our current empirical project to provide a context for thinking about the dynamic interplay of empirical and non-empirical questions.

## 4 Empirical Philosophy of Science in Practice

For illustrative purposes we briefly describe our own approach to empirical analysis that has been informing our investigations of five research laboratories in the bio-engineering science over the past 12 years in order to exemplify the rich affordances of empirical methods for informing philosophical questions about science practice, in our case interdisciplinary science. We draw from the part of our investigation that was situated in two biomedical engineering (BME) research laboratories located on the campus of a major research university in an urban setting. Biomedical engineering may be characterized as an *interdiscipline*, meaning that “melding of knowledge and practices from more than one discipline occurs continually, and significantly new ways of thinking and working are emerging” (Nersessian 2006, p. 127). The labs merge resources from both biology and engineering in the form of researchers, concepts, materials, and methods. In addition to blending academic domains, the labs tend to attract persons with diverse interdisciplinary interests and experiences.

Our study of these interdisciplinary laboratories has also drawn a diverse interdisciplinary team comprising researchers from cognitive science, philosophy of science, psychology, psychoanalysis, linguistics, history of science, and computer science, to understand the learning, reasoning, and problem-solving practices. It has been challenging to draw from these varied influences in such a way that represents adequately disciplinary and dispositional differences while achieving a unified ‘voice’ for our analysis. We have reconciled these difficulties through regular weekly meetings at which we compare observations and compare and develop interpretations of interviews. Further, our interpretive codes were developed in dyads and refined in the larger group, ensuring that no one interpretive perspective was put forward; rather, we aimed for an integrative perspective.

Our investigation began with the framing assumption that the cognitive practices of each laboratory are both *situated* in the laboratory and *distributed* across systems of interacting persons, artifacts, instruments, and traditions. The situated approach to cognition construes the features of intelligent behavior as arising within and depending upon the constraints and affordances of particular settings, in contrast with a view of cognition as a context-independent abstract set of functions. We understand the laboratory as the physical space, its artifacts, the instruments and devices used for investigation, including technologies specially designed for these purposes, and also as an organized social group that shares an agenda that is to

some extent collective. The broader collective agenda underlies and supports the problem-solving goals and strategies of any single researcher at any given time.

The principal investigator of each lab is most obviously involved in setting the collective agenda; however, our analysis has shown that the agenda is *dynamically* influenced by contributions from all members of the laboratory community. We thus construe the laboratory as an “evolving distributed problem-space”—comprising researchers, artifacts, and practices—with permeable boundaries, in that it enables researchers to move between its physical boundaries and the wider community to which the work is connected (Nersessian et al. 2003). Researchers in both labs actively seek new ideas and applications at the cutting edge or frontier of knowledge in their respective fields. They are therefore *creative* environments, which in previous work, Nersessian (2006, 2012) has characterized as distinguishing the study of these laboratories from other problem-solving environments in which the goal is not novelty but precision, such as Hutchins’ studies of navigation processes undertaken in landing a plane or steering a ship to harbor (Hutchens 1995a, b). The laboratories are *evolving* systems, with problems, goals, methods, and technologies transforming in response to the activities of its researcher-learners, the entry of new researchers and the departure of others, and with outside collaborations.

Central to the cognitive practices in both laboratories is what we have labeled *traversing the in vivo–in vitro divide*. Research in biomedical engineering must devise ways to emulate selected aspects of in vivo phenomena to a degree of accuracy sufficient to warrant (to the extent possible) transfer of simulation outcomes to the in vivo phenomena. As a result, researchers in both labs design, build, and experiment with hybrid physical in vitro simulation models composed of both living and engineered materials that selectively instantiate what the researchers deem significant features of in vivo systems. Experimentation with these models requires bringing biological and engineering practices together in an investigation into a “multifaceted modeling system” (A-10).

A more detailed description of the purposes and practices of each lab will help to situate our approach to analysis.

## 4.1 Lab A

Lab A is a tissue engineering laboratory that dates to 1987. During the period of our investigation the overarching research problems were to understand the mechanical dimensions of cell biology, such as in the behavior of endothelial cells in response to shear forces, and to engineer living substitute blood vessels for implantation in the human cardiovascular system. The dual objectives of this lab explicate further the notion of an engineering scientist as having both traditional engineering and basic scientific research goals. Examples of intermediate problems that contributed to the daily work during our investigation included designing and building living tissue —“constructs”—that mimics properties of natural vessels; creating endothelial cells

(highly immune sensitive) from adult stem cells and progenitor cells; designing and building environments for mechanically conditioning constructs; and designing means for testing the construct's mechanical strength.

During our study, the main members included a director, one laboratory manager, one postdoctoral researcher, seven PhD graduate students (three graduated while we were there, and the other four graduated after we concluded formal data collection), two MS graduate students, and four long-term undergraduates (two semesters or more). Of the graduate students, two were male and seven were female; the postdoctoral researcher was female. Additional undergraduates from around the country participated in summer internships, and international graduate students and postdocs visited for short periods. Usually the graduate student researchers work on individual projects, often with assistance from undergraduates.

## **4.2 Lab D**

Lab D is a neural engineering laboratory. During the period of our research the overarching research problems were to understand the mechanisms through which neurons “learn” in the brain and, potentially, to use this knowledge to develop aids for neurological deficits. The assumption that guides research in Lab D is that advancing understanding of the mechanisms of learning requires investigating the network properties of neurons. Examples of intermediate problems that contributed to the daily work included developing ways to culture, stimulate, control, record, and image cultured “dishes” of living neuron arrays; designing and constructing feedback environments (robotic and simulated) in which the dish of cultured neurons could “learn;” and using electrophysiology and optical imaging to study plasticity.

During our study the main members included a director, a laboratory manager, a postdoctoral researcher, four PhD graduate students in residence (one left after two years, and three graduated after we concluded formal data collection), a PhD student at another institution who periodically visited and was available via video link, one MS student, six long-term undergraduates, and one volunteer for nearly two years, who was not pursuing a degree (already had a BS) but who helped out with breeding mice. Of the graduate students, two were female and three were male; the postdoc was male. The backgrounds of the researchers in Lab D were more diverse than those in Lab A and included mechanical engineering, electrical engineering, physics, life sciences, chemistry, and microbiology; some were currently students in a BME program. As an institution, the neural engineering laboratory had been in existence for only a few months and was still very much in the process of formation when we began data collection. Because all the projects centered around the “dish” of living neuron here was significantly more interaction among research projects than we witnessed in Lab A. Unlike the traditional independent configuration of Lab A, Lab D is embedded in an open space that is shared by seven faculty members and their postdoctoral researchers, as well as graduate and undergraduate students.

### 4.3 Data Collection

As noted above, qualitative approaches to inquiry are proliferating in the social sciences. The sheer variety and alleged differences among approaches (e.g. grounded theory, discourse analysis, narrative analysis, phenomenological inquiry) can be daunting. But the basic issues concern the question, problem, or analytic focus, which then have implications for the decision about the particular method most appropriate to use.

For our purposes, an analytic focus on *the acting person*, the scientist, or more specifically *the acting person in normatively structured contexts of practice* (the science laboratory), is an inherently integrated focus. It thus invites an analysis not, e.g., on neural mechanisms in the brain but on acts of coordination or coordinated practices across persons and artifacts occurring in the context of the biomedical engineering research laboratory. Coordinated achievements occur and are demonstrated in both the interviews (conversations) with research scientists and through the practices which are described in detailed field notes on our observations.

**Individual Interviews.** The question might well be raised why we focus on *interview text* rather than video recordings of laboratory practices. In the learning sciences, video recordings are often used to provide grounds for analysis of complex interactions of persons with one another and with the objects of their practices; enabling consideration of the interrelations of verbal utterances (talk), gestures, use of tools and artifacts, and both routine and novel practices (Jordan and Henderson 1995). We audio and video recorded interactions, but have focused most of our attention on analysis of interview data. It was not possible to record research activities in the labs.

Additionally, we worry about the possibilities of eliminating the affective, motivational, and cognitive particularity of contributors to the collective practice of knowledge construction through accounts that focus solely on interaction. We have no easy solution to the problem of adequately understanding the contribution of the particular to the collective without resorting to an individualistic framework, but the inclusion of the personal dimension of science is necessary to any effort to move beyond the artificial separation of the social and cognitive realms that has dominated accounts of science to date.

Moreover, the use of interviews is a methodological implication that follows from the acting person as an analytic focus. The study of persons should include treating them as persons, which entails enabling them to give reasons, to provide accounts of their activities (Parfit 1984). Scientists do not speak of their subjective or personal investments in their formal reports; research is described as if subjective effects have been eliminated. Yet when scientists discuss their own practices more informally, including in the context of an interview, they include highly personal accounts of their aspirations, influences, accomplishments and failures. That is, the personal dimension emerges as critical to their theoretical commitments and discoveries. Thus, although study of persons in science may well include observation and analysis of their conversational exchanges, it seems also to require talking to

them, enabling them to give *reasons* for their specific activities and describe what their practice means to them, to *account for* their practices and research interests. Of course we do not suggest that interviews provide us with an x-ray of our participants' inner world, and an account of what occurs in practice must be compared with ethnographic observations. However, the account provided by an interview (especially a "situated interview" that takes place with the environment—the laboratory—in which the cognitive activities of interest occur), is essential for analyzing the personal contributions of the scientist to the research process.

Through the use of individual interviews with researchers with different levels of expertise, from different disciplinary backgrounds, and at different phases of research, we are able to analyze how the particular learning history, relational networks, affective style, sources of motivation, and epistemic values contribute to what takes place in their own research trajectories and in the relational dynamics of the interdisciplinary space. The interview provides insights into how each scientist understands her work, what it means to her, and how she experiences it. These aspects have tended to be excluded from analysis of science practice to date. Moreover, following Rouse (1996), we regard the interview conversation as part of the wider conversation of science. That is, the felt demand to clarify and explicate their problem solving to a novice outside of their field has been described by some of our participants as contributing to new ways of understanding what they are doing for themselves. Directors of both labs reported that they found that our interviews of their researchers made them more reflective about their practices.

**Field Observations.** Several members of our group became participant observers of the day-to-day practices in each lab. Each ethnographer "hung out" in a lab, observing and having informal conversations, and attended official laboratory functions (meetings, presentations, dissertation defenses). We estimate that the total time spent in observation of these two labs across our research team is over 800 hours. Team members took field notes on their observations, audiotaped interviews, and video- and audiotaped research meetings (full transcriptions have been completed for 148 interviews and 40 research meetings). We used fieldnotes from the observations to compare with interview data to arrive at our interpretations.

**Coordination of Field and Interview Data.** Our interdisciplinary investigatory team held regular weekly meetings that allowed us to compare interview data with field notes. We developed and refined coding categories during these meetings. Naturally, the changing composition of the team affected both the style of working together and the specific categories that emerged or received emphasis. Codes that emerged through grounded theory analysis (described later) were "tested" for their applicability and conceptual fit with data recorded as field notes and with a sample of additional interviews. In coordinating interview and field observation data we view ourselves as analytic instruments, relying on the basic human capacities of insight as we engage with the accounts of our participants. Through our dyadic and group coding and refinement of codes we hone these insights by considering multiple perspectives and engaging in discussion, even argument.

## 4.4 Data Analysis

**Development of Codes.** We used a coding procedure informed by Grounded Theory (Corbin and Strauss 2008) inasmuch as we attempted to approach the data openly, not looking to confirm the presence of theoretical categories we held to be salient before our research began. Of course, the extent to which we are influenced by our pre-existing theoretical commitments is an open question; the point is that we did not deliberately seek to identify particular themes in the data. We were guided by our research questions but left ourselves open to surprises.

We began by coding a subset of interviews selected to represent different research problems, disciplinary backgrounds, and levels of expertise. Each selected interview was examined line by line, from beginning to end, with the intent of providing a descriptive level for most passages. Tentative codes developed were discussed in larger group meetings. We held detailed discussion about the possible significance and alternative interpretations of the text.

We then grouped codes together under headings that seemed to capture as much as possible their important main theme. For example, model-based understanding and model-based reasoning were included along with model based-description or explanation, which seemed to express situations in which the model was invoked principally for the purposes of explaining a concept to the interviewer. Codes that did not fit easily into one of the main headings were analyzed further for possible overlooked meanings or their fit with other categories. We repeated the process until we could draw no further important distinctions. We then developed and revised a written description of main code categories, with examples of text passages assigned to each category. Main categories, descriptions, and examples were brought to the main research team for feedback and were revisited and in some cases revised after the feedback was received. An Exemplar of the highest level codes that emerged is *Seeking Coherence (sense-making)*, which includes subcategories of modeling, framing, positioning, and offering narrative (lab history and personal history).

**Case Study and Cognitive-Historical Analysis.** In addition to sampling interviews across researchers, another strategy was to focus coding and analysis on interviews with one particular lab member over time, analyzing chronologically one researcher's developmental trajectory from a point very soon after she first entered the laboratory. We used a coding system similar to that used for the analysis across interviews.

Finally, we made use of also of the cognitive-historical method to determine how the representational, methodological, and reasoning practices have been developed and used by researchers in the BME laboratories. Cognitive-historical analysis involves tracking the human and technological contributors to a cognitive system on multiple levels, including their physical shaping and reshaping in response to problems, their changing contributions to the devices developed in the lab and the wider community, and the nature of the concepts that are central to the practice at hand. As with other cognitive-historical analyses, we used a variety and range of

historical records over time spans of varying length, ranging from shorter spans defined by the activity itself to spans of decades or more. Although historical in perspective, the focus is on facilitating an understanding of cognition, as well as developing an historical interpretation (Nersessian 1992, 1995, 2008). For this dimension of our study, we collected the publications, grant proposals, dissertation proposals, PowerPoint presentations, laboratory notebooks, emails, materials related to technological artifacts, and interviews on lab history.

#### ***4.5 Rigor and Accountability***

Although we fully embraced the idea of putting our faith in the instrument of analysis (the researcher) by trusting the interpretations made, we were equally concerned about rigor and holding ourselves accountable for the match or fit between data and interpretation. For instance, we attempted to maximize coding rigor in three ways or phases:

**Collaborative Coding.** Coding initially took place between two or more members of our research team, ensuring that codes reflected interpretations that seemed plausible to at least two people, usually with different disciplinary backgrounds. Where possible, one of the coders was a person with more advanced knowledge of biosciences to provide help in interpreting specifics of the science.

**Group Code Refinement.** Updates on coding were presented at the research team's regular weekly meetings, in the context of discussions that occasionally became heated arguments. However, codes were only retained when they seemed plausible and accurate to all team members present. Other codes were adjusted or abandoned to reflect group feedback.

**External Audit.** After the initial coding scheme was developed, we enlisted an external auditor to review codes and to check them against a data sample. The auditor had expertise with qualitative methods of analysis but was not involved with the project except as an auditor. Thus he had no vested interest in the study's outcome. We provided the auditor with a sample data (interviews), a description of our procedure, and our initial coding scheme (higher and lower order categories). He met with us and provided very favorable feedback on our procedure and interpretations.

Overall, to ensure the "trustworthiness" (Lincoln and Guba 1985) of the findings, we followed Eisner's (2003) three principles: structural corroboration, referential adequacy, and consensual validation. Structural corroboration requires that a sufficient number of data points converge on a conclusion to support the arrived at interpretation. This principle calls for triangulation among different data types, in our case, interviews, field notes, lab meetings and documents. Referential adequacy addresses the richness of the description and interpretation and whether it aligns with member understanding of the same phenomena. It is important to clearly and succinctly explain the properties of each coding category for the sake of transparency. And finally consensual validation refers to the level of inter-rater

agreement that can be reached among two or more team members using the coding schemes (as discussed above). Failure to achieve such validation means that the coding scheme is not well corroborated in the data or adequately described. To further ensure the trustworthiness of our findings, a methods consultant advised on procedures for collection and analysis of qualitative data, including interview format, coding procedures, and synthesis of coded material.

## **5 General Conclusion: Rooting the Empirical in the Instrument**

We have attempted to provide some conceptual grounding relevant to the project of an empirically informed philosophy of science. We identified questions important to this grounding, and although we did not attempt to answer them definitively, we provided a guiding framework for understanding the complexities involved. We focused principally on the delineation of empirical from non-empirical questions in an empirical philosophy of science. Although we found ourselves unable to supply a formula for such delineation, we were able to provide examples of what we consider empirical and non-empirical questions in our own work, that help to inform the question of how best to understand “the empirical” in an empirical philosophy of science. Our examples and reflections on both empirical and non-empirical aspects of an empirical philosophy of science point to the same conclusion, namely that we must root our understanding of the empirical “in the instrument.” By this we mean to emphasize especially that at the deepest level the instrument comprises the one who engages in the collection and analysis of data. We commented on ways that differences in value and identity, even disposition or temperament (personality), interrelate to the epistemic demands and affordances of three empirical approaches: historical, qualitative (e.g. ethnographic), and experimental analysis. We have tried to make clear that all forms of empirical analysis require *reliable* instruments, including persons who can be trusted to collect adequate data and to analyze it with insight and integrity. We suggest the acting person as a unit of analysis not only as the focus of our investigation of science but as the instrument of empirical philosophy of science regardless of methodological approach.

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