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THE ACTING PERSON IN SCIENCE PRACTICE

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I. The Acting Person as Unit of Analysis

In any effort to establish the parameters of a psychology of science we face preliminary questions that are not empirical in a strict sense, beginning with how to demarcate the core concepts in play (science, psychology). A related question is one we take up in this paper: What is the appropriate *unit of analysis* for a psychology of science? This question relates to but can be distinguished from that which concerns the appropriate unit of measurement (Meehl, 1978) or even the framework of investigation (Milton, 2010). The unit of analysis concerns the nature of the object under investigation in psychology of science; it is a question about where to turn ones gaze and how widely to extend it, and how best to organize analysis in relation to the level of complexity of the subject matter. In this paper we propose a unit of analysis we find promising for the psychology of science, namely the *acting person*. We situate the conceptual advantages of this unit of analysis against what we identify as *the integration problem* in both psychology and science studies. We also examine what is required methodologically. We then demonstrate how we have employed the acting person as a unit of analysis to interpret the psychological practices of biomedical engineering research scientist in two innovation-focused laboratory communities. We focus on two dimensions of activity that have relevance to the integration problem we identify: emotional expression and identity formation through social positioning. We then return to a discussion of the conceptual limitations and advantages offered by adoption of the acting person as an analytic unit for the psychology of science.

The Integration problem in Psychology and Science Studies

The importance of the question of the unit of analysis, but also difficulties faced in addressing it, are amplified by the state of the two academic contexts in which the psychology of science is situated. Both psychology and science studies have emerged historically as internally divided disciplines. The problematically fragmented state of the discipline of psychology has attracted much theoretical attention and prompted calls for various strategies of unification (Henriques, 2003; Staats, 1983, 1991,1999, 2004; Sternberg and Grigorenko, 2001). Concerns about the "string of raw facts" produced by experimental psychology were expressed early on by William James (1892), the "disturbing absence of that *cumulative* character that is so impressive in disciplines like astronomy, molecular biology, and genetics" by Paul Meehl (1978), and the lack of a coherent paradigm by Stephen Toulmin (1972).[i] Staats goes so far as to label the situation a "crisis" "because the disunification feeds on itself and, left unchanged, will continue to grow" (Staats, 1991, p. 899). Others have been more skeptical about the possibility or desirability of unification in psychology (Koch, 1981; Kukla, 2002; McNally, 1992).

Although internal fragmentation in psychology complicates the issue of the unit of analysis for a psychology of science, our own interest is not in the unification of psychology *per se*, but rather with an adequate conceptualization of science. Academic study of science polarizes around differing emphases. At one end are accounts reifying an essential inner logic common to all forms of science, a foundation of enduring rational structures to which the norms of particular sciences ultimately appeal (e.g. Carnap, 1935; Hempel, 1952). More recent efforts ground these structures in the sound operations of mechanisms detailed by cognitive science and neuroscience research. At the other end are views of science as a fundamentally social, economic, or political enterprise, structured by institutional and economic forces that infiltrate representational practices and ultimately determine questions asked, explanations offered, and forms of analysis and justification that sanction some activities as "science" (Latour and Woolgar, 1986; Feyerabend, 1975, Rorty, 1979). Thus both Helen Longino (2002) and Nancy Nersessian (2005) point to the science studies community's implicit acceptance of a rational-social divide, such that science tends to be interpreted as *either* a cognitively *or* a socially powered phenomenon, and accounts of science typically are rational/cognitive *or* social/cultural accounts. Of concern is that any such divide between social and rational accounts of science is at odds with the complexities of science revealed by historians, biographers, and ethnographers of science, the intricate interdependency of cognitive, social, affective, material, and agentive dimensions in a 'mangle of practice', as Pickering (1995) has called it. We are thus concerned with the *Integration Problem* in the psychological study of science. By integration problem we refer to an ongoing challenge to adequately characterize the functional interrelationships of social and cognitive domains in everyday moments of actual scientific practice. It is in view of this challenge that we offer an explicit statement of the unit of analysis we find most promising: *The acting person*. We examine below what is entailed in each aspect of this conception and examine historical precedents that inform it, then discuss our application of this unit of analysis in reference to our study of biomedical engineering research laboratories.

The Person

Mark Bickhard (2008) and William Smythe (1998) are examples of contemporary psychologists wrestling with the concept of person and its implications. As Smythe points out, "the concept of person has never received much sustained systematic treatment by psychologists, and is becoming increasingly problematic in contemporary psychology, with its steady advance toward depersonalized views of its subject matter" (1998, p. xi). He views the depersonalization as coming from both an emphasis on mechanisms and extreme versions of cultural/constructionist psychology that view the person as " faceless node in a network of social and cultural relations" (p. xii). But what is it to 'personalize' rather than depersonalize our subject? The nature of "person," what is implied in ascribing this label to some objects and not others, is a longstanding philosophical problem of particular difficulty, overlapping with questions about the nature or essence of human beings, which notoriously leads to forms of muddle. Yet that "talk about persons" in comparison to other things -- "person-talk" -- has unique features is illustrated by Toulmin in what he calls a "classic protest:"

> "Don't treat me as a type-cast student (black/foreigner/cipher/anonymous statistic): for God's sake treat me as a *person"* (1974, p. 212).

The distinctive nature of person-talk leads to the claim espoused by personalistic theorists that a "person" is a metaphysical primary (Brightman, 1943) involving a differential "field of projection" (Stern, 1906/2010, p. 11), a unified set of capacities and phenomenal point of view. Philosophical "person-talk" typically includes reference to one or more of the following: intentionality, rationality, language-use, rule-following, or individuality/particularity, depending on the context in and purpose for which "person" is invoked. The meaning and function of each of these can be debated endlessly, complicating the project of "personalizing" psychology.

Agency and discovery

Further complications are introduced by the nature of science practice. Consider the familiar suggestion that "person" implies intentionality, such that consciousness is always "about" or directed toward something, and the related idea of agency, such that "a person" deliberately directs at least some of what she does and says toward goals or ends subjectively experienced as chosen. In all domains of human activity, but most obviously in science, constraints on agency are called into question by an inflexible and recalcitrant material reality: "To hold a natural law to be true is to believe that its presence will manifest itself in an indeterminate range of yet unknown and perhaps yet unthinkable consequences. It is to regard the law as a real feature of nature which, as such, exists beyond our control" (Polanyi, 1964, p. 10). Similarly, scientific discovery is seldom a matter to be willed into occurrence:

> The conditions in which discovery usually occurs and the general way of its happening certainly show it in fact to be a *process of emergence rather than a feat of operative action* (p. 33)... In this light it may appear perhaps more appropriate to regard discovery in natural sciences as guided not so much by the potentiality of a scientific proposition as by *an aspect of nature seeking realization in our minds* (Polanyi, 1946/1964, p. 35).

Polanyi is an especially important authority here precisely because he is best known for his effort to ground scientific knowledge in the particular capacities of persons; hence his title *Personal Knowledge* aims at a "fusion" of the personal and the objective. Polanyi's analysis makes clear that "personhood" and "personal" are not equated with a notion of agency limited to explicit, deliberate, predetermined goals, to conscious experience, or a subjective reality independent of the natural world: "Comprehension is neither an arbitrary act nor a passive experience, but a responsible act claiming universal validity. Such knowing is indeed *objective* in the sense of establishing contact with a hidden reality; a contact that is defined as the condition for anticipating an indeterminate range of yet unknown (and perhaps yet inconceivable) true implications" (Polanyi, 1958, p. viii).

The earlier and less well-known "Scientists are Human" by D.L. Watson (1938) similarly affirms 'The Scientist' as the principal problem of science, the nature and value, that is, of what is produced as scientific knowledge. Like Polanyi, Watson consults Gestalt psychology to assert that the 'unverbalized discerning of intrinsic resemblances' or similarities of form or idea are 'the chief kind of knowledge we have.' For Watson, what the mind adds to nature "to organize what it sees" includes contributions of the psyche of which the scientist is not aware, *unplanned* operations such that new configurations seem to jump out toward the solution of problems. Watson calls these "unconscious operations" a "rich storehouse of potential insights into the impersonal behavior of the *non-human world*, and therefore an aid for the problems of science" (1938, p. 189, emphasis original). These views of implicit processes are consistent with classical accounts of discovery (Poincaré, 1914; Wallas, 1926) and thus we must say that implicit or tacit or unconscious operations are part of the "person-talk" applicable to the psychology of science, despite the fact that the "automatic" nature of these processes seems to call into question the very agency traditionally regarded as definitive of persons.

Acting, Actions, Activities

Yet spontaneous reorganization and emergence rely on activities that *are* effortful: "The intuitive phase of natural discovery and extra-sensory perception have it in common that they rely on an effort of mental concentration to evoke the knowledge of a real thing never seen before" (Polanyi, 1946/1964, p. 35). Additionally, once evoked, the scientist must recognize "knowledge of a real thing" as having some value, must decide on its importance in relation to an overarching goal or problem of interest, and, ultimately, its relation to truth, even before it is presented to the scientific community for validation:

> Viewed from the outside as we described him the scientist may appear as a mere truth-finding machine steered by intuitive sensitivity. But this view takes no account of the curious fact that he is himself the ultimate judge of what he accepts

> as true… ."It is like a game of patience in which the player has discretion to apply the rules to each run as he thinks fit. Or, to vary the simile, the scientist appears *acting* here as detective, policeman, judge, and jury all rolled into one (Polanyi, 1946/1964, p. 39).

The active nature of all cognition is an important feature of the pragmatist tradition and activity theory, for which reason Jean Lave establishes "persons-acting" as central to her analysis of cognition in "just plain folks" engaged in everyday reasoning practices (1988).

In relation to science, concentrating in a way that enables discovery and judging the value of what is discovered are activities of persons. Thinking and imagining are forms of activity for Dewey (Dewey, 1926; Dewey & Bentley, 1949), as for James, who notes: "Cognition, in short, is incomplete until discharged in act… the active nature asserts its rights to the end" (James, 1896, p. 85). The meaning of active for both Dewey and James is akin to a stance of engagement in relation to the natural world. This engagement positions the actor as a *participant with* the active natural – and technological – world toward the construction of scientific knowledge. We have elsewhere called this active participation "cognitive partnering" (Osbeck and Nersessian, 2006). This idea of participation is similarly tied to the practice framework in science studies, as articulated clearly in McGuire and Tuchańska's *Science Unfettered*:

> Being a subject of scientific activity is to participate in a particular subpractice that belongs to the entire practice of certain historical societies. This participation is a form of being-together-in-the-world: through participating in research an individual takes part in social relations that form the structure of being-together and with other scientists and with the life-world of science (2000, p 150).

For Dewey, knowledge involves an active transformation of a situation rather than a passive reception of the world, as is expressed most clearly in his scathing critique of the "spectator" view of knowledge (Dewey, 1930), but also in his concept of experience:

> An experience always is what it is because of a transaction taking place between an individual and what, at that time, constitutes his environment, whether. . . persons with whom he is talking...the subject talked about...the toys with which he is playing, the books he is reading or. . . *an experiment he is performing.* The environment, in other words, is whatever conditions interact with personal needs, desires, purposes, and capacities to create the experience which is had.
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> John Dewey, *Experience and Education,* 1938, pp. 43-44, emphasis added.

Thus one reason it is important to include "acting" with "person" as a unit of analysis is that it underscores the dynamic, directed participation of the scientist. Such participation, in addition to tacit apprehension of patterns, inevitably includes an affective or motivational component, as Polanyi explains that the scientist

> apprehends certain clues as suspect; formulates the charge and examines the evidence both for and against it, admitting or rejecting such parts of it as he thinks fit, and finally pronounces judgment. While all the time, far from being neutral at heart, he is himself passionately interested in the outcome of the procedure. He must be, for otherwise he will never discover a problem at all and certainly not advance toward its solution. (p. 39, emphasis added)

Polanyi's appeal to the emotional involvement, the passionate commitment of the scientist in problem solving is important to the conception of "acting" as "participating." Examining theorists for whom activity is central to the conception of knowledge reveals an additional feature of "act" that is important to the specification of "acting person" as a unit of analysis: the coordination of operations evident in the direction of efforts toward problem solving. Of course affective processes are engaged in problem solving, not as tacked on to reasoning but as integral to it. This coordination of processes in real practice is essential to pragmatism and activity theory. Even in Dewey's critique of the reflex arc concept we see the act portrayed as a coordinated event, in his charge that psychology treats the reflex arc not as "a comprehensive, or organic unity, but a patchwork of disjointed parts, a mechanical conjunction of unallied processes" (p. 358). The "defect" in psychology's theoretical treatment of the reflex is the assumption that "sensory stimulus and motor response as distinct psychical existences, while in reality they are always inside a coördination and have their significance purely from the part played in maintaining or reconstituting the coördination" (Dewey, 1896, p. 360). Subtext to the emphasis on coordination is an assumption that through a turn to action/activity we are able to overcome dichotomized conceptions of subject and object, mind and world, body and mind. This is summarized effectively in an excellent overview of activity theory in Russian psychology:

> To be able to analyze such complex interactions and relationships…there is a demand for a new unit of analysis. Activity theory has a strong candidate for such a unit of analysis in the concept of object-oriented, collective, and culturally mediated human activity, or activity system. Minimum elements of this system include the object, subject, mediating artifacts (signs and tools), rules community, and division of labor" (Engeström & Miettinen, 1999, p. 9).

Acting implicates sociality

The fuzzy conceptual relation between "acts," "actions," "and "activity/activities" can cause some confusion, despite the implication of coordination expressed by each of these. A rough classification of act and actions as solo, goal-directed performances and activities as collective stems from the roots of "act" and action in Aristotle (Anscombe, 1957) and "activity" in Marx (Engeström and Miettinen, 1999). Yet counter-examples complicate this classificatory scheme[ii] and it is important to recognize the social dimension inherent in the concept of acting. Tolman (1998) traces in Aristotle and Hegel the idea that "acting," even though attributed to a person, has an irreducibly social connotation by its connection to the moral order. More recently, actions are viewed "behaviors that are intended performances" and acts are the social meaning of the performances (Louis, 2008, p. 25). In other contexts, "activity" is owned as belonging to a single person engaged in a goal-directed "doing:" for example, Bridgman's claim that **"**Science is activity. Science does not begin until my activities begin" (Bridgman, 1950, p. 50).

The claim that the acting person is the unit of analysis appropriate for science studies includes the qualification that acting always takes place within normative contexts established by the scientific community and its historical claim to epistemic authority. Norms also include those relevant to a particular field of specialization, and the local community of each laboratory, wherein peculiar patterns of language use and social interaction are established.

Relation to practice theory

The centrality of normative structures is important to the concept of practice, which has important similarities to the *act* and *activity* in psychology, and is indeed overlaid on the concept of practical activity (e.g. Garfinkel and Sacks, 1970). Although the term "practice" has been criticized as vague and insufficiently explanatory (Turner, 1994), Rouse emphasizes its philosophical status as normatively constrained performance (Heidegger, 1962; Wittgenstein, 1953), distinguishing it in this way from the looser notions of social regularity, commonality, or custom: "Not all practitioners perform the same actions or have the same presuppositions, but practitioners and other constituents of a practice are accountable for performances or presuppositions that are inappropriate or otherwise incorrect"… (p. 168).

Like "act," "practice" implies a coordination of various dimensions of functioning, and also coordination of dimensions or realms (personal, social, material):

> In the case of scientific inquiry, of course, the relevant practices include ways of encountering, responding to, and being resisted by what scientists are dealing with. The intertwining of language, social practice, and reality cannot be neatly bounded at the points where they run up against the natural world, for encounters with the world, and indeed the very boundaries between self and world, belong to interpretive human practices (Rouse, 2002, p. 85).

Emphasis on the act*ing* 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, including those established by the community of scientists. Similarly, practices are instantiated through the activities of intentional persons. The epistemic standards upheld through the practices of scientific communities are internalized in each scientist according to Polanyi, and provide standard against which the scientist evaluates the merits of a particular discovery. Thus when "actively engaging in the pursuit of discovery and passionately absorbed in solving a problem, he must strive against self-deception and for a true feeling of reality…Before claiming discovery he must listen to his scientific conscience" (p. 55).

Additional Forms of Normativity

The internalization of scientific norms described by Polanyi provides one way of understanding the historical tie of rule following to the distinctive properties of human rationality, as in Descartes' "Rules for the Direction of the Mind (1628; See also Toulmin, 1967). But the origin of these demands remains in question, whether as innate requirements, e.g. for order and coherence, or as values adopted through participation in a community in which they are held in common. Our view is that innate demands and constraints should be considered a kind of cognitive normativity. As Polanyi says of the scientist: "His brain labors to satisfy its own demands according to criteria applied by its own judgment" (1946/1964, p. 38). The specific nature of these demands is not a question to be settled through a priori accounts but by empirical analysis of practicing scientists, historical and contemporary. For example, Neressian's analysis of model-based reasoning practices (2008, 2009) provides room to suggest that these practices reflect cognitive demands for organized representations of the scientist's domain of problem-solving, the affordances of which include both novel conceptions of the material domain and more accurate predictions from observations. Yet that the demand has the label of "cognitive" does not preclude the involvement of the social domain, or an intricate interaction of social and cognitive forms of demand, as has been argued effectively by distributed cognition theorists and cognitive anthropologists (Hutchins, 1995; Lave, 1988).

Finally, any effort to understand the scientist must also face up to forms of demand and constraint imposed by the objects and technology central to that scientist's problem solving. We suggest that the notion of "material normativity" is a more useful concept for psychology of science than is "natural law" to describe these forms of demand, because "material normativity" encompasses the behavior of manufactured or engineered artifacts and devices as well as natural objects. Similarly, the habit of drawing hard lines around "norms" and "laws" artificially divides the social and material domains of scientific inquiry, when scientific accountability is both natural and social in origin. As Rouse explains:

> Kuhn's concern was not whether there "really" are objects, natural kinds, or causal powers independent of scientific practices. Rather, his point was that what scientific practices are normatively accountable to are objects and phenomena manifest through scientists' engaged, meaningfully situated practices of experimentation and theorizing. Muon-detecting is in this respect no less "intersubjectively meaningful" than voting or bargaining, and grasping the significance of an experimental result need be no less affective than recognizing one's own humiliation (Rouse, 2002, p. 84).

We have also found in our study of biomedical engineering laboratories that anthropomorphizing talk, in particular the projection of emotion onto cells, bears important relations to complex problem solving processes, and imposes demands upon researchers of varying levels of expertise. This is a kind of "person-talk" extended to the entities and artifacts of scientific practice, and analysis of its epistemic functions, as we have tried to provide, underscores the complicated nature of demarcating the social domain from the material domain from the cognitive domains of practice.

These fuzzy boundary lines in scientific practice – around cognitive, affective, social, and material dimensions – reflect the level of complexity with which we are dealing when imagining a psychology of science. Our intent in forwarding the "acting person" as the unit of analysis for a psychology of sciences stems from concern with doing justice to the complexity. As we have tried to explain, the "acting person" should not be conflated with "isolated individual." We view "acting person" as an inherently integrated category and reflective of the participatory nature of the scientist with other persons and with the world in producing scientific knowledge.

II. Implications for Method

Despite our interest in promoting an integrated unit of analysis for the psychology of science, we share with Howard Kendler the worry that too much openness on the part of psychologists, instead of doing justice to the subject matter, serves only to obscure it with an "amorphous conception" (2002, p.), for which reason he is critical of unification strategies based on the claim that psychologists "need adhere to no particular set of methods, to no particular field, and to no particular paradigm" (Sternberg & Grigorenko,2001, p. 1078). So here we detail what is required by establishing the acting person as unit of analysis.

It is first important to note that establishing the acting person as unit of analysis is consistent with several influential trends in cognitive science and learning science. As summarized by Anderson (2003), there has been a "rethinking" of the nature of cognition taking place within cognitive science, one that

> focuses attention on the fact that most real-world thinking occurs in very particular (and often very complex) environments, is employed for very practical ends, and exploits the possibility of interaction with and manipulation of external props. It thereby foregrounds the fact that cognition is a highly *embodied* or *situated activity*…and suggests that thinking beings ought therefore be considered first and foremost as acting beings (Anderson, 2003, p. 91).

The situated approach to cognition construes intelligent behavior as arising among interactions within particular settings. The position holds that "problem solving is carried out *in conjunction* *with the environment*" (Brown, Collins and Duguid, 1989, p. 36, emphasis added). It relates closely to frameworks assuming cognition to be *distributed*, meaning that brain and environment are construed as co-constituting a single complex system, in as much as the forms of problem solving that occur would not be possible in isolation from that environment, including the social environment. In so framing our study of the laboratories as situated and distributed we connect it to other investigations of real-world problem solving that implicate the environment in cognition in important ways (e.g. Greeno, 1998; Hutchins, 1995; Resnick, Pontecorvo, and Säljö, 1997).

Consistent with new developments in cognitive science, then, a first methodological implication of the acting person framework is that it requires (1) analysis of real-world thinking in particular, complex environments in which science is practiced. The "acting person" focus shifts the psychologist from studying reasoning under artificial constraints to investigating persons engaged in actual scientific problem solving in settings required for their work (e.g. laboratories). The second implication is that (2) interpretive activities on the part of students of science is required to clarify the nature of the interactions between social, personal, cognitive, and material domains in natural habitats of practice. The interpretation should of course undergo various social checkpoints to insure plausibility and rigor. A third implication is that (3) "the acting person" requires some attention to the particular style, affective expression, learning history, and motivational flux of scientists, aspects that have traditionally been left out of academic accounts of science and that are not fully appropriated by either cognitive or social explanations. Indeed, we believe that even the 'new' cognitive science could do more to integrate personal aspects of scientists into the accounts of science offered, for example as emotion influences styles of interaction and ways of working in the laboratory as a whole, and to hold as an open question, at least, how the idiosyncrasies and unique experiences of scientists may play important roles in the discovery process and possibly beyond it.

We now turn to a description of our own efforts to address these implications through an in vivo investigation of laboratory practices in two biomedical engineering labs. Before providing the details of our study, we first compare it briefly with the in vivo studies of Kevin Dunbar (1995, 2001), which are studies are most obviously aligned in method.

Dunbar, who conducted in vivo investigations of molecular biology and immunology laboratories, includes interviews and analysis of "online creativity" in research meetings. Thus we share Dunbar's conviction that a valid means by which psychologists might study and contribute theoretically to science studies is by entering laboratories and harvesting ecologically grounded data. The qualitative analysis of scientists' practices, including their accounts of what they are doing and why, is vital to a comprehensive psychology of science. Our project differs from that of Dunbar, however, in that our analysis is not limited to cognitive practices as traditionally conceived. We are expanding the range of topics covered to include categories traditionally excluded from analysis of cognition, namely agency, identity, motivation, and sociality, including 'relationships' with the objects and artifacts of practice (the material composition of the laboratory). Because the focus of our work was on the day-to-day activities of researchers, the research meetings assumed less importance than they had for Dunbar's analysis. We needed more to elicit from researchers their understanding and perceived relations to others and to artifacts, and to see how these functioned within the life of the labs – aspects that are better addressed through interviewing and extensive field observations. Thus we differ from Dunbar in theoretical framework, in that we understand cognition to be a phenomenon distributed across a cultural space, in this case a research laboratory. We approach data collection and interpretation with the assumption that cognition, and hence science practice, comprises a complex system of persons, devices, artifacts, instruments, texts, and traditions. The analytic framework for the study also draws from previous work of Nersessian's, including the results of prior research centered on interpreting model-based reasoning practices of scientists and situated reasoning practices in science education (e.g. Nersessian 1995, 2002, 2005).

The Research Context: Biomedical Engineering Laboratories A and D

Nancy Nersessian and Wendy Newstetter directed a 5-year ethnographic investigation of cognitive and learning practices in two research laboratories in the interdisciplinary field of biomedical engineering. These are laboratories populated by "engineering scientists:" a breed of researcher aiming to make fundamental contributions to basic science as well as to create novel artifacts and technologies. The labs are cutting-edge research environments located at a Research I institution.

Lab A is a tissue engineering laboratory, with the overarching research goal to understand mechanical dimensions of cell biology such as gene expression in endothelial cells, and to engineer living substitute blood vessels for implantation in the human cardiovascular system. Examples of intermediate problems that contributed to the daily work during our investigation included: designing and building living tissue – "constructs" – that mimic 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 their mechanical strength. During our study, the main participants included a director, one laboratory manager, one postdoctoral researcher, seven PhD graduate students, two MS graduate students, and four long-term undergraduates (two semesters or more). Additional undergraduates from around the country participated in summer internships and international graduate students and postdocs visited for short periods. All of the researchers came from engineering backgrounds, mainly mechanical or chemical engineering and some were currently students in a biomedical engineering program. The tissue engineering laboratory, as an institution, had been in existence nearly twenty years when we entered. The laboratory director was an established, renowned pioneer in the field of biomedical engineering.

Lab D is a neuroengineering laboratory, with the overarching research problem of understanding the mechanisms through which neurons form connections in the brain and, potentially, to use this knowledge to develop aids for neurological deficits and "to make people smarter" (PI). Examples of intermediate problems that contributed to the daily work included: developing ways to culture, stimulate, control, record, and image neuron arrays; designing and constructing feedback environments (robotic and simulated) in which the "dish" of cultured neurons could "learn;" and using electro-physiology and optical imaging to study "plasticity."

During our study the main participants of Lab D included a director, a laboratory manager, a postdoctoral researcher, four PhD graduate students in residence (one left after two years, three graduated after we concluded formal 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, not pursing a degree (with a BS), who helped out with breeding mice. When we began, the laboratory director was a new tenure-track assistant professor, fresh from a postdoc during which he had developed techniques and technologies for studying cultures of neurons. He already had achieved some recognition as a pioneer. The backgrounds of the researchers in Lab D were more diverse than Lab A and included mechanical engineering, electrical engineering, physics, life sciences, chemistry, microbiology, and some were currently students in a biomedical engineering program. As an institution, the neural engineering laboratory was in existence for a few months and still very much in the process of forming when we entered.

Data Collection

Several members of our interdisciplinary research group became participant observers of the day-to-day practices in each lab, spending what we estimate as over 800 hours in the labs. Researchers took *field notes* on observations, audio taped interviews, and video and audio taped research meetings. *Interviews* were conducted by the by members of the research team involved in collecting data with researchers in both laboratories. For some participants we have interviews occurring at regularly scheduled intervals, usually two weeks, and in other cases the schedule is more sporadic. Interviews are of different types and have different foci. All interviews were unstructured, but most had specific aims. Some are aimed at obtaining the lab members' accounts of their learning and social experiences since entering the laboratory; some are focused on personal history prior to entering the laboratory; some have the aim of understanding the development and current use of technology and equipment specific to the laboratory; some (most) are focused on the current research problems researchers are working on and how these relate to the larger laboratory goals. Many interviews took place within the laboratories, invoking the space with reference to the objects and representations (e.g. MEA recordings of network activity) as projected on a computer screen) relevant to the problem or issue under discussion.

Data Analysis

We do not assume that individual interviews provide us with a telescope into the inner world of participants. Neither do we assume that what researchers say they do when describing their practices necessarily corresponds to what they actually do (the "say-do problem;" (Jordan and

Henderson, 1996). However, the narrative provided by an interview (in some cases a "situated interview" in that it takes place with the environment - the laboratory - in which the cognitive activities of interest occur), is among the best available data available for analyzing these personal contributions of the scientist to the research process, both in discovery and in justifying and 'marketing' discoveries. In his effort toward a "better understanding of the nature and limit of our intellectual tools" in "*The Way Things Are,*" Percy Bridgman adopts a first person singular to give "immediacy" to his account of science and to be a logical implication of his insight that analysis of science should entail analysis of activities, of "doings and happenings" (1959, p. 3). Similarly, our interviews represent first-person accounts of doings and happenings in cutting-edge biomedical engineering research.

Our interest is in both the 1) ways in which personal dimensions of activity impact problem solving in the laboratory as a whole, for example how emotional expression and identity negotiations intertwine with cognitive practices, but also 2) in the specific forms of integration evident in the activities of a single researcher over time. At this point in the analysis the task is to descriptively illustrate the coordination rather than to demonstrate or explain it causally. To date we have coded approximately 16% of the interviews.

1) As an overarching strategy for understanding general effects of "personal dimensions", our process was informed by principles of Grounded Theory (Corbin and Strauss, 2008; Strauss and Corbin, 1998) in that we endeavored to use constant comparison and analytic induction to develop coding schemes and conceptual categories to characterize accounts of practice recorded in the interviews. Selected interviews were analyzed progressively, line by line, with the aim of providing an initial description for most if not all passages in an interview, in an effort to "take the data apart" and begin to characterize it in terms of conceptual or descriptive dimensions

(open coding). For each coded passage, the aim was to formulate a succinct and plausible description given its particular context within the interview and to some extent the larger context of the laboratory's workings and recent history. These descriptions were arrived at through detailed discussion about the possible significance and any alternative interpretations of the passage. A code was only assigned when both researchers were in full agreement about its fit and relevance to the given passage. Theoretical memos were also kept. Codes and memos were then compared across interviews, and were revisited in keeping with new thinking on and discussion of text, as well as new observations from the field work. Many codes were refined, involving further distinctions; some were added, and some eliminated as thinking evolved. Codes were then analyzed for conceptual similarity, overlap, and distinction. Those that did not fit easily into one of the main headings were analyzed further for possible overlooked meanings or fit with other categories. This process was repeated until no further reductions could comfortably be made. A written description of main code categories was also developed and revised, 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.

2) In addition to sampling across texts, another analytic strategy used was to focus coding and analysis on interviews with one particular lab member over time. The interviews conducted at regular two week intervals, enabled a form of case study analysis of a few researchers in particular whereby the trajectory is analyzed chronologically from a point very soon after the researcher first enters the laboratory. For case study analysis a coding system similar to grounded theory was also used in that the codes emerged from careful line by line reading and

interpretation of interview text, and some of the content was assigned to categories guided by questions that specifically concerned learning.

For both forms of analysis, rigor was sought in three phases of accountability: 1) <u>Collaborative Coding</u> between at least two members of the interdisciplinary team to insured that any codes assigned seemed reasonable and important to at least two persons familiar with the laboratories and the aims of the overall project. Some of these coding sessions were tape recorded so the process could be evaluated during external audit; 2) <u>Group Feedback</u>: Updates on coding were presented at regular research meetings of the interdisciplinary research team. Codes were retained, adjusted, or abandoned in line with feedback from these sometimes difficult group meetings; 3) <u>External audit</u>: An outside assessment specialist familiar with qualitative methods but not involved with the project itself evaluated the coding process and the codes assigned. We received a very favorable report on both the process followed and the codes produced through our collaborative efforts.

III. The Acting Person in Biomedical Engineering Laboratories

Overview

In determining our unit of analysis to be *the acting person in normative contexts of practice*, we intend to emphasize the coordination rather than isolation of processes: cognitive, social, cultural, emotional, agentive, and material. Nevertheless, as Anscombe's (1966) philosophical analysis clarifies, people act under different descriptions of the action in question. Anscombe was referring to descriptions people give of their own actions, yet we benefit from the insight that for sake of analysis it is frequently helpful to describe actions with different points of emphasis. We take for granted that the different aspects analyzed coordinate and, indeed, are mobilized in action itself.

Some of the higher order categories that emerged from our grounded coding appeared to us to represent different categories of human action that, intriguingly, bear a relation to some of the traditional categories of a general psychology text. Thus we prepared a book manuscript, currently in production, that analyzes the activity of scientists in the biomedical engineering laboratories described under the following descriptive categories as chapter titles. We concentrate in the text on two broad categories of acting: sense-making and identity negotiating. We use the term sense-making as short-hand for the continual efforts of any person to sort, understand, plan, and evaluate experiences of every kind, thereby giving them meaning. "Sensemaking" is a recognized category in organizational studies, wherein Weick (1995) highlights the irreducibly social *and* cognitive aspects of sensemaking. Weick also emphasizes that no sensemaking occurs without a sensemaker, thereby tying sensemaking in organizations to embodied and intentional actors, and explicitly linking sensemaking with identity formation. Under sense-making we examine the *problem solving* person and the *feeling* person in laboratory practice. Under identity we examine the *positioning* person and the person negotiating culture by means of *enacting race and gender relations* in the laboratory. In the context of discussing positioning we also consider attributions of agency to and cognitive partnering with objects and artifacts that contribute meaningfully to the problem solving act at hand. We then examine how all of these aspects of sense-making and identity formation come together in relation to a single acting person by examining the *learning* person through a case study of one researcher's experience over time, as analyzed in the context of interviews conducted at regular two week intervals after she first joined Lab A. Across chapters there is overlap and cross-reference as is consistent with our theme of integration.

Given current space limitations we provide only brief examples of our analysis of the acting person in the laboratories investigated, one under the general category of emotion and one under identity.

The feeling person

When we began to analyze interview data, we did not approach coding with the explicit goal of identifying “acting person” expressions within the interviews conducted, but rather to “describe cognitive practices” such as modeling. However, as we were seeking to describe and code cognitive practices as evidenced in the interviews, we found striking examples of interview text that did not fit traditional cognitive categories, but seemed rather to have an affective or emotional coloring. Other passages seemed expressive of desires, goals, and aspirations. Although not clearly ‘cognitive,’ passages of these kinds seemed intimately related to and interwoven with accounts of problem formulation and solving, leading us to eventually to code “affect/motivation” as a higher order category of sense-making activity.

Because we have conducted very fine-grain coding on only a small subset of our large collection of interviews, our emphasis is not on counting the presence of emotion words or expressions but on characterizing the variety of functions such expressions play in the context of the interviews analyzed and illustrating with examples from different researchers and across laboratories. Our analysis of emotion is still in comparatively early stages, but we have categorized different forms of what we consider expressions of emotion or motivation: I. overt expressions of emotion (positive and negative); II. figurative or metaphorical expressions; III. anthropomorphizing expressions.

The class of emotional expression we found especially interesting in interview transcripts is the attribution of emotional states to the objects and artifacts central to the sense-making practices in the laboratories. The most striking and theoretically important example of this practice is the attribution of happiness to cells in Lab D:

> D4 *Cell density is important, because for one cells survive more if they, if they're connected to each other.* ***A lone cell by itself is not very happy*** (2003-03-20-D-D4).

> D6 *So we also have microscopes in here that as you can see, they're always closed up in these boxes that we can keep warm in there, and we can modulate the atmosphere* ***so the cells are staying happy*** (2007-02-14-D-D6).

> D24 *This is problem solving on a whole new level because it's like how do we build a device that you put the microscope in that's gonna keep the humidity and the temperature in and we can deliver this to it and* ***keep our cells happy*** (2003-09-16-D-D24).

> D24 *There's neurons in a dish and* ***just count them and keep them happy*** *and hope they don't get sick and die.* (2003-09-16-D-i-D24).

> D28 *Imaging with cells is also non-trivial because there is, you know they're* ***not happy*** *being zapped with laser beams* (2005-03-15-D-D28).

Happiness is occasionally extended to the dish as a collective:

D4 *So yeah there're, there are a couple of noise sources in there. ..So yeah, that's the deal, it's pretty good, works very well;* ***today the dish is happy*** (2003-09-15-D-D4).

And to the cell culture:

D15 *Because the glia in the culture…maybe the neurons too…they're producing factors that* ***keep the culture happy*** (2004-10-28-D-D15).

Researchers across levels of expertise exhibit this practice of attributing happiness to cells, from the PI to D24, an undergraduate who admits little previous experience with or knowledge of MEAs. Therefore the attribution of happiness seems to indicate a local cultural practice (local to Lab D, the field of neuroengineering, or possibly biomedical engineering).

Particularly intriguing is a segment of an interview with D4 that reveals a *normative* component to the concern with happy cells (neurons). This passage reflects *an expectation* for researchers to keep the cells happy, and to *care* about keeping them happy.

I. So tell me about these neurons, because you guys are always talking about neurons in the lab.

D4 *What about neurons?*

I. Well, yesterday for example you killed a whole batch of neurons, you know when you were trying to get the MEA to work? And uh, remember Grad A came in and you guys, and um, what was the girl's name? She was so upset when the neurons were killed.

D4 *Right, Grad B.*

I Yeah, and you guys talk about the neurons with the undergrads all the time because they're culturing the neurons, so, why do you guys care so much about neurons?

D4 *If you don't essentially care about the nodes in the network,…****you gotta care about them!***

I But there's so many of them!

D4 *Right there's so many of them, but that doesn't mean that you want to….* *So they make up the network,* ***each of them has a part to play****, in the network property, so you want to keep as many as you can. You know, because they make up essentially, as I said, they make some basic rules for the way the network works,* ***so you want to keep them happy.*** (2003-04-09-D-D4)

The implication here is that it is not only the happiness of the cells as a collective that is important but even the happiness of individual cells, given that each of them contributes in a particular way to the functioning of the whole. In a later interview with another interviewer, D4 provides more clues as to what is meant by happy cells:

D4 *So what we see is actually summation of the activity of all the cells around it and-uh so, and this flow of activity is basically because of the connectivity of the network…. But then there are some cells which are always active like this guy here, and that guy there.* ***They are just happy firing all over on their own.***

I Which one, on the grid 70?

> D4 *Yeah, on the grid 70. This* ***seems to be happy all the time,*** *you know,* ***it's happy, it's happy.*** *I can't really say. May be they're two cells connected to each other, they go bing-bing-bing all the time, I don't know. So there are some cells which are very active, most of the time. So, this is a* ***pretty active network*** *because I get activity almost like on 80% of recording, recordable channels, which is, which is very good.* (2003-09-15-D-D4)

D4's comments in the context of this interview help to clarify that "happy" cells are not simply alive but that they are actively forming connections, which is essential for the research.

This passage in particular invites analysis in terms of the social dimension of emotion, not merely in terms of the contribution of cultural and linguistic practices to emotional expression, but more strongly to the idea that emotions themselves constitute interactions or *transactions*. Transactional models of emotion emphasize the dynamic interrelations of person and environment or person and situation in the production of an emotional response (Fridlund and Russell, 2006; Lazarus and Folkman, 1987; Parkinson, Fischer, & Manstead (2005) As Lazarus (2006) puts it, "each emotion has a different story to tell about an ongoing relationship with the environment" (2006, p. 34). Philosophers Griffiths and Scarantino (2008) draw from the psychological literature on emotion as transactional in offering what they call a situated theory of emotion, wherein "Behaviors which have traditionally been viewed as involuntary expressions of the organism's psychological state are instead viewed as signals designed to influence the behavior of other organisms, or as strategic 'moves' in an ongoing transaction between organisms" (pp. 439-440).

Contemporary transactional theories of emotion find precedent in Dewey's treatment of emotion in the later period of his work. Within this context, "the situation" is always the focus of analysis; the dynamically integrated nature of the situation is made clear by the participation of the actor's body (including dispositional tendencies) and personal/cultural history with the environment (including the social environment-other people) in constituting and transforming each situation: "Emotion in its ordinary sense is something called out by objects, physical and personal; it is response to an objective situation. ... Emotion is an indication of *intimate participation*, in a more or less excited way in some scene of nature or life" (Dewey, 1925/2008 p. 390).

How the transactional perspective on emotion might inform the practice of *attributing* emotion is the task we face in trying to understand the practice of describing neurons as happy exhibited in Lab D. One aspect emphasized in some transactional theories of emotion is that an emotional expression constitutes *a demand* for a response. This is one way in which it can be understood as shaping or affecting the future of the relationship, by which it might be understood as a transaction. In attributing the ability to feel happiness to cells, the researcher attaches *a demand* to those cells, a demand to tend them and care for them in order to keep them alive and active. This implicates the normative dimension noted in relation to the interview with D4: There is a laboratory mandate to care about and for the cells precisely because they will die if not cared for. Moreover, as D4 notes, happy neurons are active in forming connections with other neurons. The study of these activity patterns is the business of Lab D. In Lab A, we found similar attributions of happiness to cells, but the meaning of "happy" differed in relation to Lab A endotheliel cells. In addition to helping cells to proliferate as in Lab D, in Lab A keeping them happy can also be associated with limiting their number, though this control is conducted in the interests of keeping a subset of cells alive and healthy. For both labs, "happy cells" are vitally important to the problem solving goals of individual researchers, to the work of the laboratory as a whole, and thus to the construction and dissemination of knowledge within the wider field. Of interest here is a statement made by Freud in a lecture to the Vienna Psychoanalytic Society, referenced by Mischel in a paper in an essay for a collection he edited entitled "Understanding Other Persons: "Our understanding reaches as far as our anthropomorphism" (Freud, 1907, quoted in Mischel, 1974).

If we assume that the attribution of emotional states to cells bears some relation to the emotional capacity and thus the embodiment of the researcher, we can say that emotion is implicated in day to day cognitive practices within these laboratories, and further that there is a social, specifically a locally normative aspect to this pattern. The study of emotion, then, is one way of organizing analysis around the acting person of science.

The Positioning Person

Similarly, the topic of identity overlaps loosely with philosophical and psychological categories such as self, subject, ego, consciousness, individuality and agency. The central relation of identity to the very conception of "person" is also emphasized in contemporary discussions of self and subjectivity (Atkins, 2005). Moreover, the topic is critical to our interest in avoiding both social and cognitive reductionism in analyzing the practices of the biomedical engineering laboratory communities. According to Wenger, "the concept of identity serves as a pivot between the social and the individual, so that each can be talked about in terms of the other. It avoids a simplistic individual-social dichotomy without doing away with the distinction…it is the social, the cultural, the historical with a human face" (1998, p. 145). However, even within the general framework that acknowledges the close relation of identity and community or identity and practice, multiple ways of articulating their reciprocity have emerged in recent scholarship. Social identity theory, for example, recognizes that an aspect of self-definition is rooted in group membership, and that emotions and valuations relating to the self accompany self-categorization (Tajfel, 1982; Tajfel & Turner, 1979). In contemporary social theory identity frequently emerges as a form of action, accomplishment, display, or performance situated within networks of meanings and practices; people i*dentify* as or with various options. One prominent approach with several offshoots and varieties is positioning theory.

Positioning is "the discursive process whereby people are *located in conversations* as observably and subjectively coherent participants in jointly produced storylines" (Davies and Harré, 1999, p. 37). Positioning analysis seeks to describe changes in relations (between persons or persons and groups) occurring through discursive strategies. It concerns social effects of speech, a form of activity (Austin, 1955). Our speech affects others and we are affected by what they say. Positioning represents the action though which these effects are accomplished and a position can be understood as an effect. Although the main applications of positioning theory fall within the framework of "social psychology," that positioning theory concerns "cognitive" or "epistemic" dimensions is also emphasized: "Positioning theory is a contribution to the cognitive psychology of social action. It is concerned with revealing the explicit and implicit patterns of reasoning that are realized in the ways that people act toward others (Harré, Moghaddam, Cairnie, Rothbart, & Sabat, 2009, p. 5).

Positioning theory builds on the dramaturgical theory of Goffman (1981) and the analysis of rule-following behavior by persons in social groups (Harré, 1974). However, the concept of position is more fluid than that of the social *role*. Role implies a static location in a social order ("a scientist"); positions, in contrast, are always dynamically shifting and renegotiated, both tacitly and explicitly. Also, different positions can be identified for a single speaker in the context of a single record of interaction, such as an interview passage.

Two features of positions make positioning theory a powerful analytic tool for understanding interactions in science practice. First, positions serve to establish the possibilities of action (i.e. analysis focuses on what actions are "socially possible for any social actor at any moment in the flux of social life") (Harré and Moghaddam, 2003, p. 4-5). A second, related point is that a position "can be looked at as a loose set of *rights and duties* that limit the possibilities of action" (p. 5)… it "may also include prohibitions or denials of access to some of the local repertoire of meaningful acts" (p. 6). Hence different positions are understood to *make possible* different forms of practice and *require* some forms of practice: positions serve to establish the possibilities for action, broadly defined. Positioning is thus closely tied to identity formation *and* emotion in that shifts in position effect changes in the sets of practices in which one can and should participate and feels competent or obligated to do so.

In a forthcoming paper (Osbeck & Nersesssian, in press) we illustrate the usefulness of positioning theory for understanding practices in our biomedical engineering laboratories by analyzing identified positions in terms of rights and duties, cognitive and social --what we call the "epistemic effects" of various positioning strategies. We organize selected examples into three categories to emphasize the qualitatively different ways in which positioning can function in our settings. The categories include (1) statements in which researchers explicitly refer to professional or disciplinary affiliations (e.g. biologist, engineer), which we analyze in terms their implications for epistemic division of labor; (2) statements that relate to ways in which ideas or methods are justified, extended, or modified in relation to the work of other researchers or scholars; and (3) statements that involve positioning in the realm of researchers' relationships with the artifacts and entities central to their practice, in this case living cells. This broad division into three categories of positioning is intended to distinguish three forms, levels, or dimensions of identity negotiation in laboratory practice, for which there are implications concerning what gets done, by whom, and how it gets done. We have space below for only one example, relating to the category of explicit disciplinary identifications:

In this interview passage A10, a graduate student researcher, characterizes engineering in terms of a high degree of experimental control, in part through having well-defined variables:

I So is there a force in vivo that you're trying to simulate with this?

A10 *Right. So the blood vessel is a complex biaxial strain…* ***But, it's important as an engineer to really define your variables or your parameters****, or whatever …If I just stretched the cells…I would be able to say, you know, my cells did x, in my device, but you know, I don't want to make it device dependent. You know, I want to say [to address/inform] the strain pattern.*

I So you think that your engineering background may be motivating your…

A10 Oh yeah!

I … desire to control that?

A10 *Oh yeah! Oh yeah - a lot of people will just want to stretch them, and see what happens when they're stretched… a lot of people will extract that kind of stuff, prematurely*. (2002-07-05-A-i-A10)

In this example, the higher degree of control that A10 associates with engineering also enables more robust inferences and scientifically relevant generalizations, i.e. that relate to the strain pattern of the blood vessel rather than the specific device used to test (stretch) the cells.

In a later interview, A10 echoes the idea that engineers are concerned with high levels of experimental control as the overarching goal of approximating a blood vessel's environment:

I So why don't you briefly tell me what the flow loop is, and what it's for, in your own words.

A10 *So we use the flow loop as um, a first order approximation of a blood vessel environment, is like, in that, um, as the blood flows over the lumen, the endothelial cells experience a shear stress.* ***Well, um, as engineers, we try to emulate that environment*****.** ***But we also try to eliminate as many extraneous variables as possible, so we can focus on the effect of one. Or perhaps two, such that our conclusions can be drawn from change in only one variable****. So, um, we've come up with this flow loop as a way to impose a* ***very well-defined shear stress*** *across a very large population of cells such that their aggregate response will be due to that well-defined shear stress.* (2002-07-09-A-i-A10)

A10 reiterates this emphasis on the engineer's superior control by way of contrast with "the biologist's perspective"

I You just have more control with the flow loop.

A10 *Exactly.* ***It's well defined, you can change one thing and therefore whatever happens it's because of that one thing****. No matter what... Whereas like a biologist's perspective, would be, you know, let's see what's happening, but the problem is the conclusion ….* ***a lot of times biologists will just try to draw conclusions.***

I1 Like a causal model, or

A10 *Right*

I1 And that's problematic…

A10 ***Very*** *problematic*

I2 Why?

A10 *Because you* ***do not have a firm control over all your variables****. And you have so many variables, they all could be changing all at once, there's no way to know, all you have is a snapshot here, a snapshot here*

I And so are—I mean, the biologists are, are content with the kind of systemic cause and effect that they describe?

A10 *They* ***can get away with it in their circles****. Engineers, you know, don't really like that too much.*

I1 So do the biologists make progress with that method?

A10 *Well, I mean, it's kind of like paleontology versus microbiology. Paleontology you see a bigger skull and a little skull and you say this came from that. Microbiology [you say] these genes are totally different…* ***But a biologist would say this is this****. You know, the really,* ***engineering mindset*** *would be let's look at genes, and let's see if can we actually draw this kind of relationship.*

In these passages A10's efforts to align with engineering practices and "mindset' and to distance from those of biology position him as a more rigorous scientist; they establish epistemic rights and duties consistent with that position. Although he does not explicitly criticize biologists, he claims a more rigorous method and solid logical foundation for his own work by identifying with engineers and claiming that engineers are more systematic in drawing experimental conclusions on the basis of eliminative induction.

Disciplinary identifications and their associated rights and duties for laboratory responsibilities take on more importance in the increasingly interdisciplinary landscape of contemporary science. They are assumed by each researcher in ways that reflect historical understandings associated with categories like engineer, as in the A10 example, but also ways of working that are taken up within a given practice context, e.g. a laboratory. In turn, these configurations affect self-understandings, which in turn impact the responsibilities a researcher assumes. For example, A11 characterizes the differences between biology and engineering as entailing a division in both focus and task in Lab A and positions herself as "between these:"

A11 *I guess you could say it's split because we have* ***some people that do mechanical testing of pumps*** *or just looking at micro C-T or figuring out better ways to use micro C-T. That's about half of our lab. The* ***other half does lots of cell culture****.* ***So I consider myself in the middle of these two.*** (2002-03-22-A-A11).

In interdisciplinary science there is no single set of norms to negotiate but rather multiple, sometimes conflicting norms, or conflicting ideas about what constitutes good science. We cannot understand these norms without examining how they are understood and enacted by actual scientists working within these new settings.

IV. Conclusions

We have been able to provide only a sampling of our efforts to understand biomedical engineering through the analytic unit of the "acting person." But the examples provided illustrate not merely the close relation of emotion and identity to the work of our laboratories, but to problem solving and dedication to the work of the laboratory, as they implicate social and cultural dimensions of emotion and identity expression.

As we have also attempted to make clear, our emphasis on the acting person is not meant to convey the impression that science is a 'subjective' enterprise. Neither is the framework "individualistic" in the traditional sense, for at every level the activity of the person is situated in structuring, normative social contexts and inherited conceptual and social categories:

“participation within an ontic circle of individuality and conformity” as is demanded by the very nature of scientific creativity (McGuire and Tuchańska, 2001, p. 150)

Full understanding of this participation and the complementary participation of the scientist with the material and technological domains requires first person accounts from scientists practicing in their natural settings as an essential complement to controlled investigations of scientific reasoning. We by no means suggest that we have achieved “full understanding;” our claim is that the analytic unit of the acting person points us in the right direction. “Attention to activities and the first person” notes Bridgman, “emphasizes the insight that we never get away from ourselves,” though the problem of how to deal with this insight is “perhaps the most important problem before us,” linked to, but “infinitely more complicated than, the problem of the role of the observer to which quantum theory has devoted so much attention and regards as so fundamental” (p. 5-6). The complications Bridgman references implicate complications in data collection and analysis relevant to the psychology of science focused on the acting person. But we do not consider their burden sufficient reason to turn away from the pursuit of adequately integrated accounts of science practice, by which we treat the scientist as a person.

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[i] Sternberg and Grigorenko, 2001 provide brief review of unification efforts.

[ii] Counter-examples include the "whole social act" Mead discusses in the *Philosophy of the Act*, Dewey's analysis of acts in relation to technology, and the more recent incorporation of speech acts into activity theory, drawing especially from Wittgenstein's *Philosophical Investigations* (1953); See Engeström and Miettinen, (1999).