MATERIALITY AND THE DISCOURSE OF SCIENCE

by

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ABSTRACT

This dissertation examines the relations between scientific work as it is performed at the benchtop and scientific work as it is represented in scientific prose, where "scientific prose" is defined broadly as instances of communication which point to the methods and objects of scientific research. The idea I expand upon is that scientists have historically been embodied knowers, moving through a physical context involving research objects, and that some forms of scientific communication have evolved to convey this material relation of scientists and their objects of study. Using concepts from phenomenology, such as intentionality, and Kenneth Burke's theory of Dramatism, I develop and apply an "action-attention-language" triad model for examining how references to actions on research objects are represented. With this triad model as a heuristic tool, I examine various cases of representation in science. I look at experimental papers which attempt to introduce an apparatus into the community; the use of mice as research apparatus in neurobehavioral genetics; advertisements for research products placed in the journal *Science*; and two instances of what can be called "cyberscience": websites selling research mice and services, and research involving "virtual" mice.

The chapters of this dissertation progressively problematize the relation of physical and linguistic practices. In experimental papers describing an apparatus, the style of reporting tends to portray a direct correspondence between physical and linguistic research practices; when mice become research apparatus, language is both constrained by the mouse body and also used to surpass material limitations; advertisements for research products point to the automation of apparatus and the division of research work between those performing

the experiments and those writing about experimentation. Furthermore, I observe that the digitization of research allows researchers to access mice and research services over the internet, while virtual biosimulations ask researchers to infer and describe material processes from digital representations. The contribution of this dissertation to knowledge in Science and Technology Studies is primarily evidential, as I attempt to show the relationships and transformations in research work which others have pointed at, but often in broad terms which call for further substantiation through specific textual examples.

PREFACE

A version of Chapter Two has been published. "How Do Mice Mean: The Rhetoric of Measurement in the Medical Laboratory?" *Rhetorical Questions in Health and Medicine*. Eds. J. Leach and D. Dysart-Gale, Lanham, USA: Rowman & Littlefield, 2011. This publication was edited by Joan Leach and Deborah Dysart-Gale, and also features independent work by the following authors: Carol Berkenkotter, Colleen Derkatch, Deborah Dysart-Gale, Lisa Keränen, Joan Leach, Judy Z. Segal, and Philippa Spoel.

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LIST OF ACRONYMS

CMC	Computer-Mediated Communication
CRO	Contract Research Organization
DIO	Diet-Induced Obesity
ISP	Internet Service Provider
MAOA KO	Monoamine Oxidase A Knock-Out
MRI	Magnetic Resonance Imagining
NOD	Non-obese Diabetic
PCR	Polymerase Chain Reaction
PET	Positron Emission Topography
PLN	Pancreatic Lymph Node
STS	Science and Technology Studies
TJL	The Jackson Laboratory

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INTRODUCTION

Parallel Scientific Worlds of Materiality and Discourse

In this dissertation I develop and implement a way of reading scientific prose in which I focus on the relationship between physical and linguistic research practices. I am interested in the relations between how scientists do their work at the laboratory benchtop, and how they treat and portray this work in various kinds of scientific prose. I define "scientific prose" broadly as instances of communication which point to the objects and methods of scientific research. These objects may be material, and my interest lies primarily in such material objects (the cells, reagents, mice, tools, and machines found in the laboratory), but they can also be models, theories, and computer programs. "Scientific prose" includes reports on experiments but is not limited to these; this broad, inclusive definition also accommodates such genres as advertisements for research products (which I take up), instruction manuals, email correspondences concerning laboratory practices, and other communications which point to actions on the objects of research in experimental contexts.

The central concern of my project is one familiar to Science and Technology Studies (STS) scholars: what do we do, as critics of language, with the objects and methods of scientific research, given their (often) simultaneous instantiations as material and symbolic entities, on which researchers act in both physical and linguistic ways? How do ways of acting on material research objects, by which I mean engagement with research objects on a physical level (meaning that there is at least some piece of the natural world, even if it is only a protein fragment, being acted upon at the physical level, which would include separating cells, injecting mice, making aliquots, adding reagents, incubating samples, etc.), relate to symbolic ways of acting on these objects through language-related practices such as writing? Can we observe the relations between these parallel worlds of scientific research practices in the prose? How do we develop analytical tools which might reveal the relations between scientists' actions on research objects and the discourse that surrounds these actions?

The issue of how the material world of research relates to scientific discourse and epistemology, in terms of how research practices affect the kind of knowledge scientists put out into the world, has been a long-standing topic of STS studies. Indeed, the Situating Science project initiated in 2007, a seven-year collaborative project to promote and coordinate STS research in Canada, funded by the Social Sciences and Humanities Research Council of Canada Strategic Knowledge Cluster, states on its website that one of its four organizing themes is "Material Culture and Scientific/Technological Practices," and another is "Scientific Communication and its Publics." According to the website, the latter theme refers to "how scientists communicate with one another, with their objects, and with the wider world," and the former theme of "Material Culture" refers to the "practices" of science, "in the lab, the field, and the community." Scholarship investigating scientific practices thus tends to focus, as stated on the website, "on instruments, experiments, observation, methods of interventions, . . . the interaction of embodied practices, skills (which include scientific personnel beyond the researcher), concepts and theories and machines/technology." The ways in which scientific communication relates to research practices becomes especially important in understanding the impact of science on wider social structures. In the introduction to their edited book *Science Observed* on the social study of science, Karin Knorr-Cetina and Michael Mulkay state: "Since the production of discourse which purports to have a systematic relationship to scientific work is such a crucial part of the scientific enterprise, its study promises to be particularly critical to the study of science as a social phenomenon" (11).

Philosopher and social theorist Theodore R. Schatzki, in his introduction *The Practice Turn in Contemporary Theory* (co-edited with Karin Knorr-Cetina and Eike von Savigny), thus argues that language and "signifying systems" can only be analyzed "via the field of practices" in which they occur, which includes a field's material practices (3). In the same collection, sociologist of science Andrew Pickering also calls for conceptions of communal practices which can take the "struggles with the material world" into account, and can link these back to conceptual schema ("Practice" 164). Most recently, sociologist (and director of The Centre for the Study of Knowledge, Expertise and Science [KES] at Cardiff University) Harry Collins, inquiring into the "relative contributions of language and physical practice to practical understanding," argues that physical practices are central to cultural institutions, but their influence happens on the collective level, where language functions to organize individuals into common practices ("Language" 271). The discourse of a domain somehow "contains" its material practices according to the author (271). Collins thus refers to "domains of practice/language [which] are embedded within one another in fractal-like relationships" (271). Similarly, rhetoric and genre studies scholar Charles Bazerman proposes that language is a practical activity anchored to the material world, and that utterances cannot be separated from the practices of science. According to Bazerman, the material world serves as a reference point for linguistic activities, "constrains the language negotiation," and allows audiences to associate the symbols of language "with concrete operations on concrete objects" (305).

Despite such acknowledgements that material and linguistic practices in science "work in tandem," as Schatzki summarizes (3), STS scholars have found it challenging to develop analytical frameworks which relate these domains of science to one another. Questions remain about what it means for "linguistic practices" in science to be constrained by the "physical practices" of researchers, to use Collins' terminology, which the author invokes to deal with the fact that language is also a practice ("Language" 273). What might such constraint look like in different genres of scientific prose? When concrete objects and actions on those objects are referred to, *how* are they referred to? Where does the bodily agency of the researcher performing these "concrete operations" figure in such references? How are linguistic practices linked back to physical research practices in the prose itself? How might linguistic practices influence communal ways of acting on research objects in a physical sense? I will attempt to answer these questions here using an interdisciplinary methodology centered primarily on theories of phenomenology that deal with embodied ways of knowing, Kenneth Burke's theory of Dramatism, and theories that deal with how language is used in science.

In a recent call for papers by the interdisciplinary STS journal *Spontaneous Generations* (for a volume dedicated exclusively to the topic of instruments and techniques in science), for example, volume editor Isaac Record states that STS scholarship still "struggles to do justice to material things" in a broad sense (1). In *Thing Knowledge*, philosopher Davis Baird refers to the "text bias" that has dominated thinking about science, in which knowledge entails an ascension "from the material world to the 'Platonic world' of thought" (5). Offering a possible explanation for why the material world of scientific practices might be problematic for some STS critics, Bazerman points to Saussure's formulation of language as a linguistic code separable from context, and the concomitant difficulty in conceiving "how language talks about anything other than itself" (295). J. E. Meguire and Trevor Melia propose the following sequence on the road to the globalization of text and exclusion of materiality in some STS analyses: "Everything is text (Derrida's '*Il n'y a pas dehors text*') – "All science is therefore text'" (305). This notion that science is text was popularized by the observation by sociologists Bruno Latour and Steve Woolgar, in their influential ethnographic study *Laboratory Life*, that eventually all material activities in the lab are turned into text through inscription devices, obscuring all traces of what was actually done in the laboratory.

At the other end of what is sometimes treated as a material practice-linguistic practice divide in STS approaches to science, Collins argues that recent focus in STS on practices has resulted in language being "entirely ignored and [physical] practice alone has been taken to be what makes it possible to understand practice" ("Language" 272). Pointing to the work of philosophers such as Maurice Merleau-Ponty, Martin Heidegger, and, especially, Hubert Dreyfus, Collins states that emphasis on the role of the body in generating understanding about the world has relegated language "to the domain of symbols" and marginalized its importance in understanding science in STS analyses (272). Responding to his opening question concerning the relative contributions of linguistic and physical practices to practical understanding, Collins claims that, at the individual level, "language dominates practice (nearly) everywhere," whereas at the collective level practices drive language (274). Collins calls this latter formulation "the social embodiment thesis," which refers to how collective physical experience determines what words we invent and the stories we tell about the world (if everyone were blind and wheelchair bound, Collins explains, then there would be no talk about tennis, for example) (274). What Collins is asking, then, is "what can come to be known through language and what can come to be known through [physical] practice"? (247). This question relates broadly to how human beings come to know things about the world, and it is beyond the purview of this dissertation; I am interested specifically in how physical and linguistic ways of acting in a scientific research context are represented in scientific prose.

Prioritizing scientists' discursive productions can mean many different things, however, and studies of scientific texts as a means of getting at the various practices of science can pose many challenges for scholars. One of the things that textual analysis can mean is rhetorical analysis. Rhetoric of science and medicine scholar Judy Z. Segal states that Kuhn opened science to rhetorical (and sociological) analysis, and Kenneth Burke opened rhetorical criticism to science (*Health* 10). According to Segal, Burke "sees all relations as discursive, all discourse as action, all action as motivated" (*Health* 11). This inclusive view of human communication means that science, which has sometimes been conceived as outside rhetoric's purview,[1] becomes open to analysis focused on persuasion. If rhetoric is defined, as it is by Burke, as "the use of words by human agents to form attitudes or to induce actions in other human agents" (qtd. in Segal 12), then the ways in which scientists use words to do their work becomes rhetorical, and can be analyzed as such.

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[1] Joseph Gusfield observes that the Aristotelian definition of rhetoric as "the faculty of observing in any given case the available means of persuasion" has traditionally made it the concern of those interested in moving others to thought and action, such as politicians, journalists, and artists (16), a point made by many others (Bazerman, *Shaping* 6; Finocchiaro 188; Gross, *Rhetoric* 3; Harris xi; Prelli 1). Not surprisingly, then, Steve Fuller states that "[t]he very idea of rhetoric of science has a grating quality," resulting in much friction between scientific and humanistic disciplinary communities (279). The "toe-stubbingly hard subject matter" of scientific communications, as rhetorician of science Randy Allen Harris calls it, and the fact that such communications do result in toaster ovens, nuclear energy, and planes that stay in the air, furthers this impression of analytical incompatibility, that there is nothing rhetorical going on here (xi). Gusfield points out, however, that such "a neutralized use of language is an impossibility," arguing that if science uses words for reporting, then an analysis of how science leads to practical actions cannot ignore the literary style of the reporting (17).

Burke's definition of rhetoric is expansive, and, accordingly, so are rhetorical approaches to scientific communication. For Segal, "[r]hetorical criticism identifies the persuasive element in the discourse of health and medicine and asks, 'Who is persuading whom of what?' and 'What are the means of persuasion?'" (2). In the context of medicine, these broad guiding questions morph into more specific ones related to patient-physician interactions, for example. For others, rhetorical analysis means looking at writings by the giants of science, such as Newton (Bazerman; Gross *Rhetoric*), Darwin (Campbell), and Watson and Crick (Halloran; Bazerman; Prelli; Gross *Rhetoric*; Miller), for the ways in which their texts attempt to influence readers. Other analyses of scientific texts (and communication more broadly) can mean the study of metaphor use (Condit "Recipes"; Reeves "Language"; Ceccarelli); how different audiences process scientific information (Condit "How the Public"; Segal "Internet Health"); and the grammatical and organizational features that scientists use in their communications (Reeves "Language"; Swales; Myers *Writing*).

Although the subjects for analyses of scientific discourse need not necessarily be texts, textual analysis is still, broadly speaking, the most common type of analysis of scientific discourse. In *Starring the Text*, rhetorical scholar of science Alan G. Gross notes that rhetorical analyses of scientific communication share the common, broadly defined, aim of centralizing the "hermeneutical unraveling" of texts (ix). A criticism of such textual analyses, Gross recounts (see Gaonkar), is that they miss something because they cannot access the (physical) practices of science (21). Gross responds to this charge by positing that it is not problematic to limit the scope of scholarship to specific aspects of a domain (21). One could also add to Gross's justification for such an approach the fact that scientific texts often do claim some relationship to material research practices, and that a close reading of *how* texts portray their relationship to research practices is not outside the scope of a linguistic analysis. I would also add that scientific texts can be analyzed outside of the laboratories from which they originated because the vast majority of their readers will also encounter them in this way; as Callon, Law, and Rip state, scientific texts are intended to influence social practices at a distance. The language analyst's task thus becomes, as Mulkay, Potter, and Yearly propose, to identify the "recurrent structural features of participants' discourse" (200), and to describe how these features might function within the wider sphere of practices in which they occur.

Although all of these varied approaches to scientific texts elucidate important features of scientists' discursive productions and their possible impacts on different audiences, many of the questions concerning how scientists' linguistic practices relate to physical research practices remain, as evidenced by Record's statement that STS scholars are still struggling "to do justice to material things" (1). In particular, an analytical framework for studying references to the physical practices of research within scientific texts themselves could be mapped further. In this dissertation, I propose and enact one way of reading scientific prose which focuses on the relationship between physical and linguistic research practices as depicted in scientific texts.

Methodology

My contribution to the discussion of how material and linguistic research practices relate to one another is an analytical approach centered on embodiment. Using concepts from phenomenology (namely, intentionality), Burke's Dramatism (in particular, his Pentad), and studies of how language is used in scientific communication, I develop what I call an “action-attention-language” triad model for looking at how references to physical research practices function in different genres of scientific prose, and how they are portrayed as relating to linguistic practices. The idea that I begin with and expand upon is that scientists are essentially embodied knowers, moving through a physical context involving the objects of research on which they act, and that many of their communications have evolved to convey, and possibly influence, this embodied knowing. The “action” component of the above triad refers to the enactment of agency on research objects, to what agents do to things in research settings (and where Burke’s Pentad comes in). The “attention” component is related to consciousness, to what is noticed and marked as salient in a given research situation; to the way in which bodily movement and language become directed towards certain things and not others. The “language” component of my triad refers to how words are used to represent this sense of limited and directed attention as linked to actions on research objects, and to how language is used in general to refer to research practices. I imagine a triangle to represent the relations among these three components, rather than a linear string where one component always follows another in a specific order, although I have written my description this way for simplicity; all three components are constantly influencing and restricting what happens with the others in a research setting. When agents come into an experimental scene, they will possess the language component from others’ accounts of coping with the same object, and thus action and attention are already narrowed. Similarly, when agents come into an experimental situation with a set of practical skills, their language and attention will already be constrained. It is the way in which these components are portrayed as relating to one another in different scientific communications that I will be exploring in the chapters that follow.

Other scholars interested in the relations of material and linguistic practices in science have proposed similar models in attempts to link the world of materiality with the social world of language. Bazerman, in particular, has described various permutations of a model which also attempts to link researchers' actions in the laboratory with the wider social world in which objects and actions on them take on meaning. In order for scientific cooperation to work, Bazerman states, "we must already share much," referring not just to the meaning of words, but also to "how those generalized words apply in this situation and how they are to be realized in action" (303). Bazerman thus proposes that a shared background denotes shared "conceptual, practical, and social worlds" (303). Elsewhere, when he describes how a child learns to associate specific actions with specific phrases, he refers to how a neophyte becomes socialized into a communal "semiotic-behavioral-perceptual system" (307). Two paragraphs later, Bazerman rephrases this formulation slightly to "semiotic-cognitive-behavioral" (307), and then to a shared communal "activity/language/perceptual matrix" (310), followed by a shared "social/empirical/cognitive" context of activity (313), and finally to how Newton created a "perceptual/behavioral/empirical complex" (317). Schatzki similarly proposes that a "body-activity-society complex" makes practices intelligible (3); Barnes points to shared "practical-symbolic" world views; and Pickering refers to a need to understand how "struggles with the material world" can be linked to conceptual schema ("Practice" 164). All of these formulations thus point to the need to understand science through an analytical system that unites the parallel worlds of material and linguistic practices. My action-attention-language triad model gives me a shorthand way of approaching scientific texts with these delineations in mind.

My approach is twofold. It relies on Burke's Pentad for a way to conceptualize the "action" component of my triad, and it relies on phenomenology for a way to conceptualize the relations among the components of the triad, and in how I situate an embodied agent at the center of the triad.

In *A Grammar of Motives*, Burke outlines his five key terms of "Dramatism," now commonly known as his Pentad, which he develops to answer the question, "What is involved, when we say what people are doing and why they are doing it?" (xv). The five terms are: act, scene, agent, agency, and purpose. "Act" refers to what took place; "scene" describes the background context of the act; "agent" identifies the person(s) who performed the act; "agency" refers to how this act was performed, with what means; and "purpose" identifies why this act was undertaken (xv). Burke gives the following passage as a brief example: "The hero (agent) with the help of a friend (co-agent) outwits the villain (counter-agent) by using a file (agency) that enables him to break his bonds (act) in order to escape (purpose) from the room where he has been confined (scene)" (xx).

I use the term "action" in my triad model to similarly refer to what was done, by whom, to what, in a scientific research setting. Initially, I imagine all of these components pointing to a material reality: the agent is an embodied researcher, who acts on material objects in an embodied way, by directly manipulating them somehow, in the context of a physical space called a laboratory. I say "initially," because when I consider, in my fourth chapter, the use of virtual mice in research, the object, the setting, and actions upon the object are several orders removed from this material notion of research. For now, however, I begin with this material definition of "action." I also require an additional term, which Burke does not supply, to refer specifically to the objects of research being acted upon, as opposed to all of the objects found in the scene and the objects (agency) with which the agent is acting upon these research objects. For this, I will use the term "recipient objects" to denote those objects receiving the agent's actions in a research situation.

In searching for a way to better understand the relationship between actions in a research laboratory and the linguistic practices that surround them, I turned to phenomenology. Specifically, I turned to interpretations of phenomenology that deal with the body as the unifying center of both physical and cognitive acts in the world. The core doctrine of phenomenology, known as "intentionality," proposes that all of our acts of consciousness are intentional, meaning that consciousness is always "consciousness of" something (Sokolowski 8). According to this view, awareness is always directed towards something else. If we see, explains philosopher Robert Sokolowski, we see some object; if we imagine, our imagination presents us with imaginary objects; if we are remembering, we remember past objects (8). Thus, consciousness and experience are always correlated with an intended object (whether that object is tangible, like an apple, or conceptual, like the image of an apple that probably just appeared in the mind) and intentionality entails a conscious relationship with the object. I use this idea of intentionality-as-object-focused to reinterpret Bazerman's claim that the language of science is constrained by "concrete operations on concrete objects." Language may be constrained in the same way that our experience of the world is constrained through consciousness when attention is object-intended and necessarily selective; language directs and follows our movements through the world.

At the same time as consciousness is about something, the perceptual capacities of the body shape the nature of our thoughts about objects. Maurice Merleau-Ponty incorporates the role of the body into this idea of intentionality. In the newly discovered series of 1948 radio lectures titled *The World of Perception*, Merleau-Ponty attempts to integrate the phenomenon of consciousness with the phenomenon of the body, rendering each of us a body that constitutes the world through interaction. This viewpoint positions the body as the subject of perception which offers to consciousness its experience of objects in space and time: "Here, for the first time," writes Merleau-Ponty, "we come across the idea that rather than a mind *and* a body, man is a mind *with* a body, a being who can only get to the truth of things because its body is, as it were, embedded in those things" (43, emphasis in original).

Expanding on Merleau-Ponty's ideas of embodied intentionality, Samuel Todes, a philosopher who writes on the philosophy of mind, explores in a more focused way the body's contribution to our knowledge of objects. In *Body and World*, Todes begins with the premise that the body is "the material subject of the world" at the center of all experience, and that "our knowledge of objects is body-centered" (1). From these premises, Todes proposes that "we experience all objects as circumstantially related to our active body in the midst of circumstances" (1). The body becomes more than just a material thing among other material things; the body becomes a material thing that can move itself to generate its experiences of the world. To say that language directs and follows our movements through the world, then, would be to say that language accompanies our experiences of objects as related to our active body in the midst of material circumstances. I reinterpret Bazerman's statement, that linguistic practices in science (Bazerman, like Collins, uses the term "linguistic," referring, for example, to "linguistic agency" and the "linguistic work" that goes into scientific communication [14]) are constrained by "concrete actions on concrete objects," to mean that the way scientists use language is constrained by this bodily being-in-the-world. Can we therefore see this constraint in scientific prose? What does it look like?

A philosophy based around "the living animate body," as philosopher Dermot Moran explains, means that our specific sensory and motor capacities embed us physically and psychically in the world in a particular way (423). Our body embeds us in a spatial world; "I discover things as left and right," states Moran, "tall and small, etc., all on the basis of my orientation wherein my body occupies the 'zero point'" (424). The "functional asymmetry of our forward-directed body," according to Todes, thus defines all our human perceptual experience (3). Once we make the objects around us "determinate" through "practical perception," philosopher Hubert Dreyfus, who has written extensively on phenomenology, states, elaborating on Todes' theory, we can then make "practical judgments about these objects" ("Samuel" 392). Such judgments are "non-conceptual," Dreyfus states, in that they are "a way of coping with an actual object in this situation, from this point of view, in this light, in this orientation, and so forth" (392). These non-conceptual judgments are then retained in our "conceptual imagination" when we withhold actual action, and can be recalled in our mind's eye when needed (Dreyfus 392). Cognitive linguistics scholars George Lakoff and Mark Johnson importantly argue that the structure and perceptual capabilities of our forward-directed bodies also shape the way we use language. Accordingly, if scientific prose is constrained by actions on objects, and purports to have a systematic relationship to physical research practices, then we should see this constrained bodily capacity to act on the world in the prose.

My triad model thus makes explicit that action, attention, and language flow through bodies that are similarly constrained in their relations with the material world. Burke's term "act" in his Pentad can thus be reinterpreted in this embodied sense when applied to what scientific researchers do to things in the lab. The term "attention" in my triad refers to the movement of consciousness and thought as tied to bodily being-in-the-world, and to the idea that thought and language will be tied to what an agent (in the midst of material circumstances in an experimental scene) notes as salient. Just as Dreyfus and Todes argue that the structure of our language is shaped by the experience of bodily being-in-the-world, linguistics scholar Wallace Chafe proposes that language is also shaped by the experience of consciousness as tied to bodily being. In *Discourse, Consciousness, and Time*, Chafe introduces the importance of attention by observing that language mirrors the constant motion of our consciousness. While in the midst of interaction with the material world, or in the midst of imaginative contemplation, we can only pay attention to one restricted segment of all possible candidates for our attention. According to Chafe, consciousness entails "an active focusing on a small part of the conscious being's self-centered model of the surrounding world" (28). Such active focusing is essential, Chafe continues, in order to restrict incoming information to only that which is relevant in the moment (29). What is noticed, then, in an experimental scene, becomes etched in the "mind's eye," as Dreyfus would say, and will, presumably, feature in the scientific narrative of what was done in the laboratory. If an agent's attention, for example, is never drawn to the apple core on the lab benchtop (because it is not salient to the experimental situation), then this object will likely not feature in the linguistic account of what happened. Linguistic activities are dependent, according to this view, on what is noticed when an agent moves around in an experimental scene. When Bazerman speaks of a "perceptual" element in how language is constrained by actions on objects, this notion of constrained consciousness is what I imagine.

My "action-attention-language" triad model thus describes what would be happening in an experimental scene from an embodied, language-using agent's perspective, and how a material scene involving bodies moving around, doing things to the objects found there, is rendered meaningful. With this triad, I attempt to map how linguistic practices might figure in such a scene, and how conceptualizations of the scene etched in the mind's eye might be used to construct scientific prose which further manipulates the experience through language. In the chapters that follow, I adopt a case studies approach to trace how this tripartite process of making meaning out of research events is portrayed in scientific prose. My contribution is primarily evidential and descriptive, rather than theoretical, as I attempt to pin down in specific scientific texts the relationships and transformations in research practices which others have pointed at, often in theoretical terms which call for further substantiation via specific examples.

Outline of Chapters

The chapters of my dissertation are organized around specific research objects as depicted in specific genres of scientific prose. Many STS scholars use the case studies approach in studying different aspects of scientific practices. Ethnographic studies, by definition, have tended to mean a focus on the microscopic, as John Law observes, and the grounding of analysis in "the specificity of the case study" ("On STS" 636), describing the enactment of intersubjective realities in specific, situated contexts. In many instances, case studies also historicize language-based analysis, delving into how language is used by a particular person or group, participating in a particular social act, at a specific cultural moment (Harris xxix; Halloran 39; Bazerman16).[2] Different genres of scientific prose will portray the research situation in different ways, and propose variations in how action and language relate to one another; they will feature different kinds of acts, on different kinds of recipient objects, by different kinds of agents, in different kinds of research scenes. The order in which I discuss these case studies progressively complicates the relationship between physical and linguistic research practices, and the degree to which they are reliant on one another.

In Chapter One, I apply my action-attention-language triad model towards a reading of scientific papers dealing specifically with research apparatus as the recipient object. I begin by looking at how early experimental philosophers, such as Boyle, Newton, and Huygens, refer to research practices, and what kind of correspondence their scientific papers depict between what agents do in research settings and how those actions become represented in language. Using my action-attention-language triad model, I explore how authors refer to concrete actions on objects; how the body and embodied actions figure in the prose; how agents are portrayed; and the ways in which authors' descriptions of experimental events might speak to a collectivity of potential users. Scientific papers dealing with apparatus are especially conducive to an exploration of how physical and linguistic practices relate, because authors are attempting to introduce a new object into the research community; the prose therefore refers extensively to the embodied research context.

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[2] The tension between "vertical integration" and "horizontal dispersal," between case studies and at least some generalization in discourse analysis, is captured by Dilip Gaonkar's critique of the rhetoric of science (38). Gaonkar points, in particular, to the "palpable anxiety" about reframing specifics into generalities, "lest such redescribing turn into a Sisyphean task" (41). The alternative is not made explicit, however. At the time that Harris's *Landmark Essays* was published, some, such as Halloran, were still calling for case studies to balance overly theoretical analyses (39). This tension may simply be an unavoidable aspect of discourse analyses dealing with such a large corpus of work as science.

Next in this first chapter, I follow with an analysis of a 1955 *Biochemical Journal* paper by Oliver Smithies as my case study, in which the author describes an apparatus known as gel electrophoresis for the first time. Smithies' paper follows a prose style similar to that of early experimental philosophers, wherein experimental events are portrayed from the central spatio-temporal perspective of an agent embedded in the scene. I characterize this prose style further, proposing that Smithies' experimental prose is able to recreate through words the sense of embodied skillful coping with the objects of research as linked to constrained attention. The prose focuses on the bodily movements associated with a specific recipient object, where attention should be directed when handling this object, and what aspects of this material situation have been translated into language. I propose that scientific papers that deploy this particular style of reporting portray a direct correspondence between physical and linguistic research practices, between the action and language components of my triad; the author is also portrayed as the agent in the scene, directly witnessing the described events. I conclude this first chapter with a discussion of how this prose style might function to give the impression of replicability when it places the reader vicariously in the agent's role, and how it might be conducive towards the standardization of many potential agents separated from one another in space and time.

In Chapter Two, I look at a recipient object, the mouse, whose treatment in prose throws into question the idea of a direct correspondence between physical and linguistic research practices, between the action and language components of my triad. Specifically, I look at the use of mice to model a human psychiatric disorder supposedly marked by enhanced aggression. Hans G. Brunner et al. first characterized, in 1993, a family of Dutch men exhibiting aggressive behavior; the authors link this behavior to a single gene, the monoamine oxidase A gene (MAOA). Shortly after, in 1995, Oliver Cases et al. create a line of genetically-altered mice also lacking the MAOA gene, and claim to witness aggressive tendencies in the mice. Although this MAOA mouse model is adopted by others, it remains problematic on many levels, as critics note (Longino; Rose). The fit between physical and linguistic research practices is not an easy one in the instance of mouse modeling of human psychiatry; the material side of the equation, the mouse body, seems unequal to the complex psychosocial questions it is being used to answer. Researchers are thus forced into some fascinating linguistic moves to deal with this lack of fit between what mouse bodies can do and the psychosocial concepts being investigated.

Animal models thus complicate ideas about how ways of doing science relate to ways of talking and writing about it, about how the action and language components of my triad can relate to one another. The language in experimental papers on the MAOA mice does something extra, I claim, something more than just describing actions on objects, something more than orienting bodies with respect to common objects (although it does this too, and I discuss how): it surpasses the constraints of physical research practices, the action component, at the same time as it adheres to a prose style of immediate material experience. Historian Theodore M. Porter, writing on the history of quantification, notes that researchers are constrained by nature's capacities and what can be done in the laboratory, "though not absolutely" he adds (*Trust* 11). This qualifier, "though not absolutely," becomes especially salient in the case of the MAOA mice, and has implications for epistemic activities. Using Eleanor Rosch's theory of prototypes, where things are compared against typical examples, allowing even dubious cases to fit the category, I propose that the language component of my triad can be highly "pleotropic," a term I invent to account for how one word, such as aggression, can describe both mouse and human behavior, and allow for animal models. The case of the MAOA mouse model also highlights the importance of the attention component of my triad. An instance of controversy around the use of mice in neurobehavioral research shows how much of the physical research context is normally excluded from attention, from language, and from communal knowledge.

The genre of scientific prose that I look at in my third chapter is not the experimental paper, as in the previous two chapters, but rather advertisements for research tools, primarily for gel electrophoresis, placed in the prestigious experimental journal *Science* from 1956 until 2008. Ads also tell stories about scientific research practices, about who is doing it, with what, where, and for what reason, giving important clues about how science is being conducted on a physical and linguistic level. Over several decades, ads for commercialized research apparatus tell a story of the steady re-engineering of research work towards automated agency and towards automated agents whose embodied roles have been greatly simplified; many once tacit skills are packaged directly into the machine, implying the minimization of the action component of my action-attention-language triad. Smithies' paper thus now serves as a reference point for how this particular recipient object can be handled physically and described linguistically.

Compared to Smithies' description of gel electrophoresis in a scientific paper, the ads describe a (non-problematic) separation of physical and linguistic research practices; linguistic activities are portrayed as easily divorced from benchtop research work in recent ads. This separation presents a different kind of agent from the one seen in the scientific papers described in Chapter One. The agent/author embedded in the research scene who directly handles recipient objects and composes scientific prose becomes replaced by (at least) two different kinds of agents: those who engage in the physical practices of research (which have become simplified down to the push of a single button in some instances), and those who engage in the linguistic practices of science, who write the scientific papers, attend conferences, and discuss science with other scientists. These "linguistic agents" (which Collins and Evans would call "interactional experts") are depicted as removed from the physical experimental scene where recipient objects are acted upon in physical ways. Calling on Collins and Evans' "Periodic Table of Expertises," I look at how ads portray expertise in relation to physical and linguistic research practices, and explore the claim put forward in the ads that the "real work" of science means linguistic, rather than physical, work.

In the fourth chapter of my dissertation, I focus on the phenomenon of "cyberscience" as depicted in selected websites selling research mice and services (Charles River, Harlan, The Jackson Laboratory, and Sigma), and in a scientific paper describing the use of "virtual mice" to study diabetes (Zheng et al.). On websites selling research mice and services, researchers are urged to hand the physical work of handling mice over to a commercial mouse supplier; custom-ordered mice and research services can be ordered directly over the internet and data can be accessed remotely. Physical and linguistic research practices can therefore be split among distinct groups of agents. In Yanan Zheng. et al.'s experimental paper on "virtual non-obese diabetic (NOD) mice," a computer "biosimulation" is used to model experimentation on mice. There are no real mice anywhere on this experimental scene, and yet the authors frame their prose in material terms, as though embodied agents had acted upon real mouse bodies, and as though real mice responded to physical interventions (such as injections under the skin) in physical ways (such as alternations in immune cell activity). In this instance, the authors skip discursively ahead to the physical acts that the computer biosimulation is said to represent; there is little correspondence here between what agents did to research objects in a given research setting and the ways in which language is used in scientific prose, raising many questions about how knowledge about the natural world is being created through such novel technologies.

Delving into these instances of what may be called "cyberscience," I argue that through the online negotiation over mouse bodies, the mice start to unravel *as mice* and become partial "epistemic objects," to use Knorr-Cetina's terminology ("Objectual"). For the researcher accessing mouse bodies exclusively online, the words entered on the website (the new immaterial "scene" of research), rather than physical actions on "concrete" material objects, come to largely define the object. Linguistic experts composing accounts of experimentation, distanced from the materiality of the work (from mouse bodies, syringes, test tubes, machines), thus come to act on mouse bodies mainly through language. The language work on mouse bodies can be seen as cleaning this epistemic object of the messiness of situated materiality, with epistemological implications.

With "virtual" research "mice," there appears to be a breakdown of correspondence between what agents do to recipient objects (and, indeed, confusion as to what the recipient object even is; "where" these acts occurred; and who, or even what, performed these acts) and how language is used to describe these actions. To make sense of what is happening when experiments on computer biosimulations are described in material terms, I propose that there are several orders of recipient objects, and that the prose in Zheng et al.'s paper is referring to third order objects such as "mice" and "cells," instead of first order objects such as computers. There are potentially several orders of agents as well, then: the computer program itself as agent; the computer programmer as agent; and the researcher writing the account of the experiment as ("linguistic") agent. Scientific research mediated through the internet and the use of biosimulation programs are thus forcing reconceptualizations of what it means to do scientific research, to talk about that research, and the nature of grounds for knowledge claims.

Scientists themselves, and not just their critics, are thus also struggling with how to reconcile physical and linguistic research practices in their accounts. Some of their solutions for dealing with the physical world of laboratory experimentation (with what Pickering calls the "mangle" of research), however, imply that a radically new way of making knowledge about the world is emerging. The effort to do justice to the materiality of science, of which my project is a part, becomes especially important when the ways in which research is being conducted are changing beyond anything previously known, through the mass commercialization of research tools and the relocation of science into cyberspace. Understanding how physical research practices contribute to the discourse of science may therefore help us better predict how such changes to research practices might affect wider social relations and how we may wish to respond.

CHAPTER ONE

Materiality and the Discourse of Science

In this chapter, I look at one discursive genre in science designed for researchers to share with others what happened in an experimental scene: the experimental paper. The experimental paper is broad as a category of scientific prose, however, so I will be focusing specifically on papers dealing with experimental apparatus as the recipient object. I define "apparatus" as a grouping of tools, materials, and machines gathered under a defined protocol, denoting specific embodied actions that agents need to take in order to use the apparatus for its designed purpose. Although apparatus can be used to refer, in its broadest sense, to any system of organized activities, I am interested in the kind of apparatus that takes up space on a laboratory benchtop, and which must be manipulated physically by an agent. The kind of experimental paper that features an apparatus as the recipient object might therefore be called a methodological paper, whose predominant aim is to demonstrate a new object and a new way of doing research into a scientific community. The central claim, or problem, that such a paper would purport to solve is thus a methodological one and the solution is the apparatus itself.

I begin my exploration of how physical and linguistic research practices might relate to one another here because experimental papers featuring an apparatus, attempting to persuade others to do their research in a certain way with specific tools, tend to refer extensively to the physical actions taken on the object. The prose style in such papers can therefore present a rich example of reference to embodied actions on objects in an experimental scene. Such papers tend to highlight the "action" component of my action-attention-language triad, and tend to frame the relationship between physical and linguistic practices in science as a direct one. For these reasons, such methodological papers are an appropriate place for me begin an exploration of what it means for experimental prose to refer to actions on research objects.

I also chose to begin my analysis by emphasizing the treatment of recipient objects in scientific prose because the objects of research are central to scientific practices and hence to scientific communication. To speak of embodied practices in research often means speaking of the nonhuman entities in the laboratory with which agents interact—all of the reagents, beakers, pipettes, machines, and research animals towards which researchers' bodily movements are directed. These nonhuman elements of research constrain and shape human embodied experience, as both Pickering ("Mangle") and Knorr-Cetina ("Objectual") propose, and thus also the discourse of science. Nonhuman entities in the experimental laboratory form the meeting point that brings individual bodies, communal practices, and language together; non-human objects are central to scientific communication and to the communalism of science more generally, and experimental narratives are commonly about human-nonhuman (agent-recipient object) interactions. As Record states, however, a heuristic framework for reading scientific texts that refer to these agent-object interactions still needs to be further refined. Here, I show how my action-attention-language model can be used to do this kind of reading and analysis.

In this chapter I perform three broad moves. In the first part, I perform both a literature review of what has been said about the experimental paper, and in particular about papers dealing with apparatus, and simultaneously frame scholars' observations in terms of my action-attention-language triad. In the second part, I look at a specific methodological paper as my case study. I analyze Oliver Smithies' 1955 *Biochemical Journal* paper that outlines an apparatus known as gel electrophoresis for the first time; using my triad as a heuristic tool, I illustrate how principles discussed by others concerning the experimental paper, and, in particular, how agents' actions are described, play out in this specific example. In the third part, I look at how references to "concrete operations on concrete actions," as Bazerman states (305), might help align communal research practices. I thus apply my triad model to the important issue of practice standardization.

I chose gel electrophoresis for my case study because it is a crucial tool in molecular biology. As early as 1964 (in a special issue of *Annals of the New York Academy of Sciences* dedicated exclusively to the subject of gel electrophoresis) Jerome Fredrick declares that gel electrophoresis is something special, "something that . . . makes possible a potentially limitless application of methods for the solution of our biochemical enigmas" (307). Milan Bier similarly notes in his preface to *Electrophoresis: Theory, Methods, and Applications*: "[t]he contribution of electrophoresis to our knowledge of protein is second to no other method. Its impact is felt in biochemistry, physiology, and medicine" (x). The importance of this commonplace apparatus to our understanding of genetics thus cannot be overstated; the majority of experiments in molecular biology now begin with electrophoresis.[3] As an instance of scientific prose which refers to the physical practices of science, Smithies' paper represents an attempt to introduce a new way of separating biological molecules, of doing

[3] The apparatus has, however, received very little attention from STS scholars. The only other study that features gel electrophoresis is an ethnographic exploration by K. Amann and K. Knorr-Cetina, in which the authors look at how electrophoresis autogradiographs are used in the situated, social process of "evidence fixation," which the authors frame as a physical process involving much object handling (135). I will be referring to Amann and Knorr-Cetina's paper in the third chapter, when I discuss the importance of interactions with the non-human objects of research in shaping discursive expertise. Although Amann and Knorr-Cetina's study brings this important apparatus to the attention of STS critics, the authors do not refer to Smithies' seminal paper.

molecular biology, into the community, and refers extensively to physical agent-object relations. The emphasis in Smithies' prose is on the action component of my triad; on what an embodied agent does to a specific, physical, recipient object.

Gel electrophoresis also represents an example of an extremely successful campaign to secure practical agreement among researchers. I am interested in how descriptions of experimental actions in research papers might encourage communal practical agreement. Although some scholars (notably Collins) claim that the tacit knowledge embodied in practical skills cannot be adequately captured in words, they also do not deny that language can affect the development and maintenance of such skills at a communal level. Although on its own the experimental paper does not guarantee the acquisition of the practical (and often "tacit")[4] skills needed to perform an experiment, it nonetheless remains the main venue for reporting situated, material experiences of laboratory events. If scientists want to "extend their worlds beyond the physically immediate," as sociologists Michael Callon, John Law and philosopher of science Arie Rip note, "then the means have to be found to act at a distance" (*Mapping* 10). Experimental papers, the authors propose, are one means for scientists to act at a distance; texts are "dispatched" as concentrated and packaged representations of what was witnessed in the laboratory "to exert influence at a distance," Callon, Law, and Rip state ("Putting" 223). I explore the idea that scientific texts can be

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[4] The relationship between tacit and explicit knowledge remains an enduring topic for STS scholars. In his latest treatment of the subject, Collins defines tacit knowledge as "knowledge that is not explicated" (*Tacit* 1). The typical example (popularized by Polanyi) is bicycle riding: "we can know how to ride a bicycle," Collins writes, "without being able to tell anyone the rules for riding, and we seem to learn to ride without being given any of the rules in an explicit way" (2). Collins argues that this particular example has led to a lot of confusion about the meaning of the term "tacit." Sometimes tacit knowledge is defined as that which *is not* explicated, and sometimes it is knowledge that *cannot* be explicated, and confusion about the two has shaped the wider discussion (4). He thus proposes a further subdivision of the term "tacit" into "relational tacit knowledge" (to do with social relations), "somatic tacit knowledge" (to do with bodily knowledge), and "collective tacit knowledge" (to do with human society) (x). Importantly, Collins states that "all explicit knowledge rests on tacit knowledge, [but] we would have no concept of the tacit without the explicit" (x).

used to orient others (located in similarly constructed material settings, surrounded by similar objects) on how to physically approach and communicate about their experiences, and show what this looks like using Smithies' paper as an example.

The analysis that I undertake in this first chapter will thus show what it looks like when agents' physical actions on research objects are emphasized in experimental prose. The examples of experimental prose that I take up here represent a high water-mark, in terms of references to concrete actions on concrete objects, allowing me to compare in later chapters what happens to scientific prose when the recipient object resists a standard action-attention-language sequence; when there is less for the agent to do on a physical level; and when the recipient object loses much of its physicality by moving into cyberspace. I start, however, with a discussion of how the experimental paper does, or does not, reflect the actual work undertaken by scientists.

The Experimental Paper as a Reflection of Laboratory Work

The idea that experimental papers depict the work of science has been contested by STS scholars, particularly by ethnomethodologists, who have done much to illuminate the kinds of daily activities associated with scientific research (Gilbert and Mulkay; Knorr-Cetina "Manufacture"; Latour and Woolgar; Lynch "Ethnomethodology"). Latour and Woolgar define ethnomethodology of science as the "*in situ* monitoring of scientists' activity in one setting," or—"the soft underbelly of science," what happens on a moment-by-moment basis in a tangible context occupied by specific actors (27).[5] The literal meaning of the term "ethnomethdology," according to Lynch, is "folk investigations of the principles or

[5] The term ethnomethodology is generally attributed to Garfinkel's studies of situated social scenes and the kinds of work that happen there in his *Studies in Ethnomethodology*.

procedures of a practice" ("Ethnomethodology" 1). Latour and Woolgar focus on the routine minutiae of scientific activity, which they contend forms the constant background hum of science, only occasionally punctuated by breakthroughs and revolutions. Latour and Woolgar's fictional anthropologist thus observes science as it happens and notes the following:

> *5 mins*. John enters and goes into his office. He says something very quickly about having made a bad mistake. He had sent the review of a paper…. The rest of the sentence is inaudible.
>
> *5 mins. 35 secs*. Jane comes in and asks Spencer: "When you prepare for I.V. with morphine, is it in saline or in water?" Spencer, apparently writing at his desk, answers from his office. Jane leaves.
>
> *6 mins. 15 secs*. Wilson enters and looks into a number of offices, trying to gather people together for a staff meeting. He receives vague promises. "It's a question of four thousand bucks which has to be resolved in the next two minutes, at most." He leaves for the lobby.
>
> *6 mins. 20 secs*. Bill comes from the chemistry section and gives Spencer a thin vial: "Here are your two hundred micrograms, remember to put this code number on the book," and he points to the label. He leaves the room. (15)

Latour and Woolgar go on to famously note in *Laboratory Life* that the scientific manuscript is the end product of all the activity in the laboratory, and that the minutiae of observed activity do not make it into the experimental paper.

Included in Latour and Woolgar's conceptualization of science, then, are the minutiae of social interactions, in addition to the manipulation of material objects. Law observes that it is impossible to understand what makes laboratory work so special unless one grasps this "extraordinary heterogeneity of the tasks of the scientist" ("Laboratories and Texts" 38). Yet it is precisely this heterogeneity of tasks, according to Latour and Woolgar and other ethnomethodologists, that is deliberately erased from the published account. The closer one gets to the stabilization of a fact, the less reference there will be to the specific material and social conditions of fact construction. Eventually, then, "all traces of production are made extremely difficult to detect" (Latour and Woolgar 176). In terms of the relation of action to language in science, Latour and Woolgar's study would imply a lack of correspondence between physical practices (what agents do to recipient objects in an experimental scene), and how researchers represent these actions on objects in the final experimental paper. If the work of science is erased from the final experimental narrative, however, then what is the experimental paper about, if not about the work?

When ethnomethodologists follow scientists around and compare their daily tasks against the published paper—their physical practices against their language-based, literary practices—they immediately discern that the published account is a creative retelling of what happened. Experimental papers are thus "retrospective formulations" of laboratory work that market an image of laboratory work "that fits an ideology rather than actual practice" (Holmes 220). In composing an experimental paper, scientists rearrange, reinterpret, omit, connect, substitute, highlight, downplay, and generally edit the draft until they have transformed an "ongoing, open-ended" process of inquiry, as historian of science and medicine Frederic L. Holmes observes, into "discrete investigative units" depicting a rational sequence of events (234). Sociologist Susan Leigh Star describes how the "complicated turbulence" of the natural world, as well as the turbulence of the work of research itself, becomes rendered in "orderly, predictable, and bounded" terms (205). This process of deliberate simplification, Star proposes, helps to reify scientific facts as their production histories are erased. Others have studied more explicitly the extensive editing that goes into the construction of an experimental paper, noting how authors haggle over content and stylistic details in an attempt to maximize the impact of their findings.[6] Latour and Woolgar thus state that scientists "systematically conceal" the nature of their practices in the experimental paper (28). John Swales, a linguist who writes about the genre of the experimental paper, also observes that the idea of scientists fiddling around in the laboratory and then dashing away to scribble what they did "is decidedly at odds with reality" (117), leading him to reiterate Donald M. Murray's celebrated saying that "process cannot be inferred from product any more than a pig can be inferred from a sausage"[7] (qtd. in Swales 127).

The experimental paper may be an abridged re-construction of research work, but, at the same time, the subject matter is still the (physical) work completed. The paradox of the experimental paper, as Star observers, is that we cannot fully grasp the varied work of science through its discursive productions, but neither can we understand it *without* discursive references (210). Swales likewise observes that in the end scientists' discursive productions must be "tethered, however tenuously and obliquely, to an experiential world of

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[6] Greg Myers, for example, studies successive drafts of two biology articles as they are prepared for publication ("Texts "). Law and Williams ("Putting Facts Together") look at the way scientists deliberate over such discursive features of the experimental paper as the title, the introduction, the citation of colleagues, the display of data, and issues of syntax in an attempt to "maximize the attractiveness" of their reports. Nigel Gilbert, on the other hand, considers the discursive techniques used by authors when trying to persuade others of the correctness of their findings, as well as others' responses to such knowledge claims. See also sociologists Nigel Gilbert and Michael Mulkay for how scientists link their data to "accounts of social action and collective belief" ("Contexts" 270).

[7] By which Murray means that the process of writing cannot be learned by simply studying a finished text. Here, the reference also seems to imply that one cannot grasp the nature of research work by reading the textual account.

substance" (123).[8] In other words, physical practices do, somehow, still form the basis for language-based practices. It is this "somehow" that remains in question. It thus becomes important to understand how physical research work is reorganized into scientific prose which refers to this physical work. Readers do routinely assume, according to Holmes, that the experiments described were actually performed at some point in time. Holmes states that even Hans Krebs' highly theoretical paper on the synthesis of urea still bears "telltale traces" of the experimental events described in Krebs' rough laboratory notes (234). Holmes thus concludes that the modern experimental paper still represents, as it has for centuries, both a carefully crafted argument as well as "a description of research that the author has carried on through a prior period of time" (234). Meaning that researchers' linguistic productions do bear— again, "somehow"—traces of what agents did to recipient objects.

In summary, then, experimental prose appears to be neither fabrication nor a straightforward account of everything that was done in the laboratory. Scientific discourse remains tethered to the material context of research, then, but selectively so. It is the selectivity of references to embodied agents, actions, objects, and physical scene that I am interested in, rather than the minutiae of social interactions that are omitted from the published account. So when actions on objects are referred to, *how* are they referred to?

Assertions that the experimental paper is a misleading account of scientific work therefore need to be qualified further. Latour and Woolgar do suggest, at one point, that there exists in experimental prose a continuum of descriptive detail. In some cases, references to the "conditions of [data] construction" are "necessary for purposes of

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[8] One of the criticisms of *Laboratory Life* is that it is extreme in its subjectivist position. "The documentary world of Latour and Woolgar rather conveniently ignores the real substances (and animals) left behind," Swales charges (123).

persuasion," the authors state (176). In other words, when a researcher is first trying to establish a phenomenon or procedure, it may be advantageous to include the material details of the work, to highlight the "action" component in the prose itself. Once a fact or a procedure is "stabilized," however, references to the conditions of construction (to actions on objects) may not only be unnecessary, but might even undermine the strength of the author's claim (176). The process of data construction becomes difficult to detect at this stage. Latour terms statements that steer the reader away from the conditions of fact construction "positive modalities," and those that steer the reader back towards the conditions of construction (that explain why the statement is solid or weak), "negative modalities" (23). Positive modalities are generally "devoid of any trace of ownership, construction, time and place," Latour states, whereas negative modalities are uttered by specific, situated agents (23; see also Gieryn). Importantly, negative modalities point towards the "complicated work situation" from which statements arise (23), Latour proposes, thereby implying that sometimes scientists do include a lot of material details in their writing, and do attempt to point to embodied actions on objects.

Contrary to Latour and Woolgar's assertion that the experimental paper ends up concealing the details of the work, Swales states that in context the careful and selective construction of the experimental paper is "neither deceitful nor misrepresentative," although it might be judged so if compared to the laboratory process (118). The experimental paper is ultimately a different genre than the laboratory record, Swales proposes, with its own unique stylistic conventions and processes of reasoning. Sociologist and historian of science Steven Shapin furthermore asserts that no linguistic version of laboratory work will ever describe all of the intervening social and material activity of scientific work ("Pump" 494). Nor should it. Does the audience really need to know, for example, that Bill hands Spencer a vial? Rather than focusing on what is missing from the published account (a theoretically infinite list), I turn instead towards the aspects of the material context of the work that do become included in the text. It is not a question of whether the reported events actually occurred (because we assume that they did), but rather why certain details of the work have been retained over others, and why they might be described in a particular way. In order to answer these questions, we need to understand how the experimental paper developed, what it was designed to do, and the kind of relationship between physical and linguistic practices that it was designed to portray. Next, I reframe scholars' observation that early experimental prose was designed to give readers a sense of "vicarious witnessing" in terms of my action-attention-language triad model. I then apply my triad model to a specific case study.

Experimental Prose and Vicarious Witnessing

The experimental report was born in 1665 when the Royal Society of London began circulating the *Philosophic Transactions of the Royal Society of London*, the first scientific journal in English.[9] It was launched privately by Henry Oldenburg in March 1665 to report on technical observations from near and abroad; most of the contributions were sent to Oldenburg via letters (Gross, Harmon, and Reidy). If discursive genre is defined as "a socially recognized, repeated strategy for achieving similar goals in situations socially perceived as being similar" (Bazerman 62, paraphrasing Carolyn Miller), then the experimental report was developed to solve a problem of social interaction. The

[9] The French equivalent to the Royal Society was the Académie Royale, and it was more like our modern version of a government-funded research institute; its *Journal des Sçavans* (also started privately in 1665, by Denis de Sallo) was the French answer to *Philosophic Transactions* (Gross, Harmon, and Reidy).

experimental paper was the solution to a *social* problem, because, unlike in the early period of the Royal Society, when experiments were performed before an assembly of members, experiments became increasingly private affairs. A means of sharing individual experimental events with others who could not witness them directly became necessary. Several scholars have thus proposed that the experimental paper was designed to provide others with a vicarious sense of laboratory work, rather than to erase as much of it as possible (Atkinson; Bazerman; Dear; Gross, Harmon, and Reidy; Holmes; Shapin "Pump").

Historian Peter Dear describes how a new philosophy of experiment came to trump the old system of ancient authority, thereby changing the style of experimental writing. During the Middle Ages and Renaissance, emphasis was placed on the form of an argument, and less on the premises for that argument. Quoting an authority—"above all, Aristotle"—typically served to establish the premise for the argument, Dear states (148). Although sensory impressions were important for the establishment of facts, such experiences quickly became statements "of how things are, or how they behave" (Dear 148). Importantly, Dear argues that one did not need to have these direct sensory experiences oneself to argue for their truth, as long as one could draw on "a weighty authority" to suffuse the argument with influence (149). When the experimental philosophers eventually rejected the use of ancient authorities in scientific argument, they were not rejecting the importance of ancient texts, but rather the way the texts were being used in scientific inquiry (Dear 150). Thus one system of authority, based on ancient philosophy, became replaced with one grounded in personal material experience. Importantly, this means that the prose became focused on the immediate material context as experienced by an agent embedded in the scene.

The style of argumentation accordingly changed from ontological generalizations about how something is, into how, in one particular instance, something was. English studies scholar Dwight Atkinson, who looks at the evolution of research writing, notes that in contrast to the "scholastic approach" that relied on ancient authority to bolster an argument, the experimental philosophers, most notably Robert Boyle, campaigned for "a rhetoric of immediate experience" (335). This new system of authority centered on experimental events "as directly experienced by the author" (335). Critically, this new experimental philosophy placed the agent in a central role; "[t]hese things, we seem to be told," Atkinson writes, "had happened by the action of, or in the presence of a particular person, at a particular time and place" (154). This shift meant that suddenly the veracity of the described event became a central rhetorical concern (Atkinson 153). In other words, authority now came from the report of the experience itself, and from the material details supplied about the work.

When this new system of authority in science became established, the language of science also became focused on the objects and physical conditions of experimentation. Researchers isolated in private laboratories needed to convince others that what they were describing had really happened, and that it had happened in the way described, as Shapin notes ("Pump" 491). Experimental reports became a way of symbolically re-creating the events witnessed by the solitary researcher, and this new imperative changed the style of the writing. In particular, this shift towards a description of events as experienced by an agent, I will argue shortly, renders the writing corporeal in focus, indeed tethered to the embodied experience of actions on objects—to what was done to recipient objects on a physical level (action); to what was noticed while the agent was in the midst of practical coping (attention); and to how language is portrayed as directly related to this corporeal experience.

It is implied in experimental prose that the agent depicted in the experimental account could be the author, but this need not necessarily be the case. It is perhaps easier to think of the agent as the carefully constructed central position of action and perception in the scene. Although this central position of action and perception is surely crafted from tangible experiences in the laboratory—and the prose must necessarily convey this impression to be credible and persuasive—the agent in the scene is nonetheless not the original doer, but the author's *rendition* of an idealized agent, just as the protagonist in an autobiographical memoir is also a character crafted by the author. Later, I will argue that such a perspective allows for the universalization of the experience, where each reader becomes a vicarious doer through the act of reading, and thereby a potential doer in actuality.

Scholars have argued that when experimental prose is framed from an embodied agent's perspective, an individual experience of experimentation can become shared among many. Prose rich in circumstantial details helps to establish the verisimilitude of the actions and objects described because it enables the reader to imagine the event. The circumstantial details included in early experimental writing could often be "excruciating" in their specificity, as Dear notes (153). Robert Boyle, for example, writes the following:

> We took a slender and very curiously blown cylinder of glass, of nearly three foot in length, and whose bore had in diameter a quarter of an inch, wanting a hair's breadth: this pipe, being hermetically sealed at one end was, at the other, filled with quicksilver, care being taken in the filling, that as a few bubbles as was possible should be left in the mercury. Then the tube being stopt with the finger and inverted, was opened, according to the manner of the experiment, into a somewhat long and slender cylindrical box (instead of which we are now wont to use a glass of the same

> form) half filled with quicksilver: and so, the liquid metal being suffered to subside, and a piece of paper being pasted on level with its upper surface, the box and tube all were by strings carefully let down into the receiver. (qtd. in Dear 153)

Philosopher of science David Gooding likewise shows how Faraday, Biot, and Davy all separately employ the same descriptive style devoted to circumstantial, procedural details. Explaining the principle behind his experiments, Faraday writes of "placing the wire perpendicularly, and bringing a needle towards it," and of "allowing the needle to take its natural position across the wire . . . and then drawing the support away from the wire slowly, so as to bring the north pole, for instance, nearer to it" (qtd. in Gooding 170). Alan Gross, Joseph Harmon, and Michael Reidy, writing about argumentative practices in early experimental articles, also observe the same descriptive style in Huygens' *Journal des Sçavans* series of articles: "I removed the vial C from underneath the vessel," Huygens relates, "and after having forced in a small air bubble, I replaced it as before; having worked the pump, I saw that in the end all the water descended very nearly up to the level of that of glass D" (qtd. in Gross et al. 381). Newton, forced by controversy to account for his procedures in ever more detail, similarly describes his optical experiments in the same kind of circumstantial, procedural details (Bazerman 117). This particular style of experimental narration, focused on embodied actions on objects, is therefore not idiosyncratic to Boyle; it appears to be the norm among the early experimental philosophers, and we hear its echo in experimental papers three centuries later. This style of narration, especially in methodological papers dealing with a novel apparatus, thus forefronts the physical benchtop practices of scientific research.

Early experimentalists describe not only what was seen during the experiment, but also the experience of directly handling the object of interest—of rotating the prism, as it were. Bazerman suggests that this "narrative intensity" allows the reader to imaginatively reconstruct the event (121). The kinds of circumstantial details provided, Bazerman furthermore suggests, lead the reader to a vicarious "tactile" experience of the material world: "Newton has vicariously given us that same concrete feeling of holding the phenomenon in our hands and turning it over and over again" (123). Shapin likewise states that Boyle's circumstantial language is a way "of bringing readers into the experimental scene, indeed of making the reader an actor in that scene" ("Pump" 511). The elaborate sentence structure of appositive clauses stacked on top of one another works to mimic the effect of "immediacy and simultaneity of experience," according to Shapin, leading to an impression of "verisimilitude" (493). Here, the reader is shown not only the end products of research work, but, critically, also "their mode of construction and the contingencies affecting their performance, *as if he were present*" (Shapin 511, emphasis in original). Our imagination thus goes where the author has led it, "in both the concrete and cognitive senses of the word" (Bazerman 124).

In terms of addressing a problem of social interaction, passages such as these also extend the witnessing public from a few, or even one, to countless others through what Shapin calls "virtual witnessing." Boyle's contentious experiments in pneumatics in the 1650s and 1660s forced the issue of how knowledge was to be authenticated. Matters of fact needed to be secured, and separated from opinion, by a "multiplication of the witnessing experience" (Shapin, "Pump" 483). Circumstantial details could help readers create mental representations of an event they had not actually witnessed, Shapin notes (481). This new literary technology of science therefore had the power to produce unlimited numbers of witnesses; the experiment could be realized "in the laboratory of the mind and the mind's eye" (491).

The reason why circumstantial prose is able to talk readers through a material situation they have not experienced, I want to propose, is that it is written from the central spatio-temporal perspective of a human body in the midst of physical circumstances. The described experience becomes shared because readers share the same basic motor and perceptual capabilities. Although scholars writing on the development of the experimental paper have observed that the prose is conducive to vicarious witnessing, they have not examined the role of shared embodiment in making such vicariousness possible. I am interested in the linguistic means by which authors of experimental prose put the body and its actions on research objects into the text. In the remainder of this chapter, I unpack what it means, in terms of specific discursive maneuvers, to make experimental prose focused on embodied agents and their actions on objects in this immediate way.

The Sense of a Body in the Midst of Circumstances

In my introduction, I state that according to phenomenological philosophers, such as Merleau-Ponty, Todes, and Dreyfus, the body is positioned as the subject of perception which offers to consciousness its experience of objects in space and time. This idea of embodied intentionality means that the body becomes the center of all experience from which we know the world. Looking at the circumstantial prose adopted by Boyle, Newton, Biot, Davy, and Huygens with this idea of intentionality in mind, we can see that what Shapin, Bazerman, Gooding, and Dear call "vicarious witnessing" relates to the author's ability to recreate in words the sense of a body in the midst of physical circumstances. More specifically, the circumstantial details provided appear to be related to those objects *towards which movement is directed*. If we conceive of conscious attention as tethered to the forward-directed asymmetry of the human body, then circumstantial prose can also convey the sense of attention as tied to actions on objects. The way language is used by early experimental philosophers taps into practical, situated judgment as non-conceptual—a way of coping with an actual object, from this point of view, in this orientation, etc., according to the specific physical and perceptual capacities of an agent's body. Experimental prose can therefore be body-centered, but *object-intended*. The methods section of an experimental paper can be saturated with description that orients the reader physically and psychically in the scene, leading to a vicarious experience of the described events.

Importantly, the body is not merely a material thing among other material things in space; it is that material thing whose capacity for internally generated movement defines its perception of space and the objects found there, which become partly defined *through* the movements we associate with them. The sense of movement in experimental prose saturated with circumstantial details thus becomes part of the definition of the object.

The importance of movement is reflected in phenomenology's concern with "motor intentionality." Todes observes that, through movement, subjects not only notice the objects in their spatio-temporal field, but also produce the circumstantial field in which things appear (49). The functional asymmetry of the body means that bodily movement alters our perception of space and objects. Because the body is always in some form of contact with the objects around it, Todes explains, being poised in one's circumstances means knowing something about the objects to which one is doing something with one's body (65). In other words, we come to know objects partly through the sequence of actions we associate with them. "I perceptually identify the chair I'm about to sit on as my office chair," explains Dreyfus, "simply by being set to sit in it in the way I usually sit in my office chair" ("Samuel" 403). The concept of motor intentionality also, significantly, implies that when we change our movements with respect to an object, we change the nature of that object. In other words, by altering our movements we change what we know about an object, and hence the very definition of that object.

Thus, apart from being an object in a laboratory, an apparatus is, crucially, a sequence of specific actions, and knowledge of the apparatus entails the development of motor intentionality with respect to it. The prose style adopted by early experimental philosophers implies that language can help in the development of motor intentionality with respect to specific recipient objects. We can therefore think of vicarious witnessing through experimental prose as helping to define the motor intentionality associated with an object. The kinds of circumstantial details provided in Newton and Boyle's experimental papers can be said to relate specifically to motor intentionality; the details serve to orient the reader—as a self-moved body—within the experimental scene. The aim of experimental prose would therefore seem to be partly the practical orientation of embodied subjects in an experimental scene involving objects common to a group of practitioners.

Scientific prose may also be said to contain the materiality of the work because we put our body into language more generally. In *Metaphors We Live By*, Lakoff and Johnson explain how the constraints of embodiment affect our linguistic choices. "We are physical beings," the authors state, "bounded and set off from the rest of the world by the surface of our skins, and we experience the rest of the world as outside us" (29). Our embodied experiences of the world and the objects found in it, the authors argue via numerous examples, give rise to spatial and orientational metaphors, and generally affect how we view things such as activities, events, and ideas. Thus, for example, because we experience our bodies as "containers" that we imagine as having an inside and an outside, we tend to project this orientational idea onto other objects, as well as onto our activities; we speak of moving *into* or *out of* a room; we speak of being *in* a marathon, or having gotten *into* our chosen profession (Lakoff and Johnson 29-32). Our bodies impose boundaries on our experiences, and we describe our world in terms of those boundaries. In experimental prose, when the body is put into the language, agents separated from one another in space and time can become oriented with respect to specific objects in specific kinds of scenes.

Experimental prose rich in circumstantial details can give readers the sense of moving around in the tangible apparatus world, even if they never actually do it. The prose is essentially realist, and works hard to reinforce the belief that the words in an experimental report are tied to real objects and to real actions on those objects. The more that an author can inspire a vicarious experience of moving around in the experimental scene for the reader, the more the reader will trust the author's description, and, critically, the more possible the experience will seem. Establishing the material possibility of an experimental event becomes especially important the first time that a new way of doing something is introduced into the scientific community; the coping sequence begins to seem highly doable to readers imaging the actions described.

Smithies' *Biochemical Journal* paper on gel electrophoresis represents such an instance of a new type of apparatus, with its attendant practical coping sequence, being introduced into the scientific community. Next, I apply my action-attention-language triad model towards this specific case study, with the aim of refining further what it means for agents' actions on objects to figure in the text.

Actions on Objects in Experimental Prose

Smithies' paper on gel electrophoresis, like those of the early experimental philosophers, represents an instance where the physical work of scientific research has been retained in considerable detail. It is a methodological paper, whose predominant aim, as Smithies explains in his introduction, is to introduce a new apparatus into the scientific community. According to Latour and Woolgar's continuum of procedural detail, Smithies' paper thus stands as an instance of the early phase of phenomenon stabilization, when the conditions of construction must be retained for persuasive purposes. The paper adheres to the modern, sectioned, format now commonly referred to as IMRD, for "Introduction," "Methods," "Results," and "Discussion," a format that was relatively stable by 1950.[10] These distinct sections of the paper evolved to serve different rhetorical functions, Swales posits, and mobilize different linguistic resources (136).[11] Smithies' paper is thirteen pages long, featuring a brief, three-paragraph introduction, a four-page (smaller font) experimental

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[10] I say "relatively" because variation on this standard IMRD is also the norm in experimental papers. Lilita Rodman notes that most of the variation on this structure occurs in the Results, Discussion, and Conclusion sections, which are often grouped or one is selectively omitted (311). Joseph E. Harmon also finds in his survey of biochemistry articles that only about 25% contain these three sections.

[11] Richard P. Fulkerson proposes, for example, that the modern experimental paper adheres to the classical oration pattern, where "The survey of past research constitutes the narration. The hypotheses constitute the proposition. The research design and data constitute the confirmation. The defense of the design against critics and those who have reached contrary conclusions is the refutation. And suggestions about the importance of the results and about future research constitute a peroration" (56). Swales is partial to Hill's "hour-glass" theory of the macrostructure of experimental narratives, which moves from the general context of experimentation in the Introduction, to the particular in the Methods and Results, then back again to the general in the discussion and conclusion sections (qtd. in Swales 133). Kathryn Riley looks at changes in "authorial role," noting that in the introduction the researcher's role is that of "a chronicler and critic of previous research"; in the methods and results the author's role changes to "presenter of new data"; finally, in the discussion, the author's role changes to "that of an interpreter of data and advocate of a new thesis" (247).

methods section,[12] a seven-page results and discussion section, and finally a one-paragraph summary section. Most of my focus in the analysis that follows will be directed towards the Methods section of the paper, as it is here that situated actions on objects are described in detail.

Broadly defined, gel electrophoresis is the separation of molecules through an electric charge. "Electrophoresis" refers to the electromotive force that pulls charged molecules through a gel, typically a crosslinked polymer whose porosity is carefully controlled. Solid, yet porous, the gel provides a mesh through which molecules move, at rates related to their size and charge, when an electric charge is applied. As one would predict, the smallest molecules with the biggest charge move through the gel fastest. If a stain is applied to the gel, the molecules can be visualized as small bands in separate columns, their location in the matrix corresponding to their charge and size, and each band theoretically containing one type of molecule (see fig. 1). Gel electrophoresis makes it possible to separate molecules from a heterogeneous mixture and study (and further manipulate) them in isolation; electrophoresis is often a key step in experiments in molecular biology.[13]

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[12] The Methods section is further subdivided into a "General" section, "Composition and preparation of the gels," "Introduction of the sample," "Protection and cooling of the gels," "Electrical connexions," "Power supply and voltage control," "Protein detection," "Preparative experiments," "Dimensions of apparatus," and "Conditions for starch gel electrophoresis of serum proteins."

[13] For more detailed information on the apparatus see David Hames and David B. Rickwood; Anthony T. Andrews; and Reiner Westermeier, who all write about the apparatus for a scientific research audience.

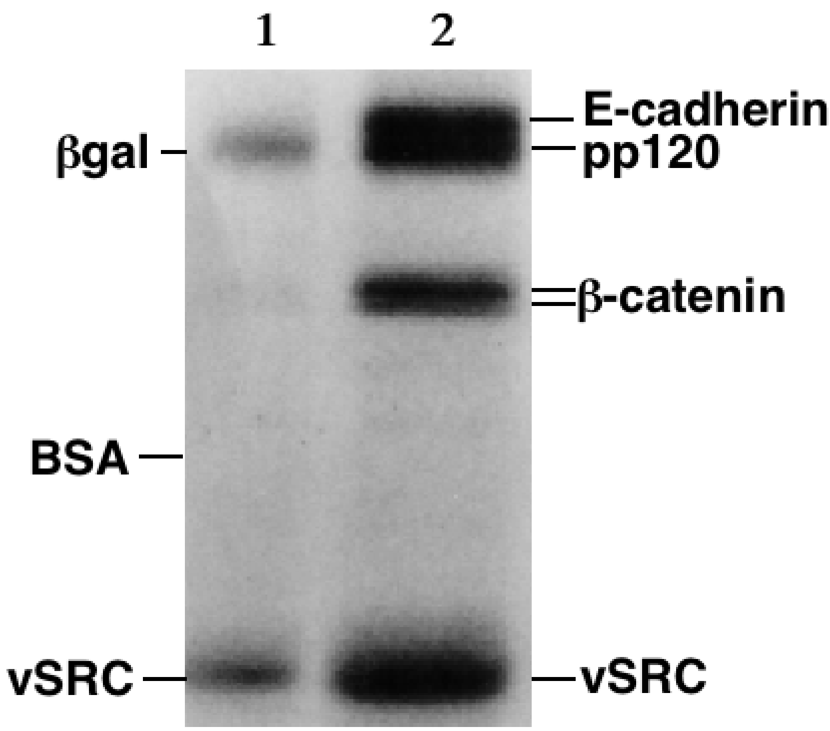

Fig. 1. Gel Electrophoresis Autoradiograph (with two lanes on the gel).

The electrochemical theories behind the apparatus, as described above, do not inform the main discursive maneuvers in Smithies' description, however, which relate more to what an embodied agent might experience in an experimental scene involving this recipient object:

> After electrophoresis has proceeded sufficiently long the seal of mineral oil or petroleum jelly is removed by blotting the top of the gel with adsorbent paper, and the gel is then turned out on a suitable cutting block (see Fig. 2). To do this the gel is freed from the edges of the tray with a spatula, when it can then be caused to fall out on the cutting block by inverting it over the block and starting movement from one end of the gel with the spatula. In order to observe the protein distribution in regions of the gel where no edge-effects are present the gel is then split along its length in a horizontal plane before staining, by means of a thin rigid cutting blade (a single-

> edged razor blade removed from its holder) and making the cut parallel to the bottom of the gel. The cut should be made in a single movement with the blade supported on either side of the gel by the flat guiding strips of the cutting block. The two gel slices so obtained are rapidly separated (to avoid interdiffusion of the proteins in case of relative displacement) and transferred to a dish for staining with the cut surfaces uppermost. The protein detecting dye is then poured over them, allowed to remain in contact with the gels for the requisite time and decanted for re-use. The gels are then washed with solvent until the background is colourless. (632)

Although such a description may not be enough for speedy (or any) replication, it includes much more circumstantial detail than is needed to acquire a general grasp of the apparatus. I therefore disagree with Alberto Cambrosio and Peter Keating when they state that the materials and methods section of a scientific paper is necessarily sparse in its description and "of little use to researchers unfamiliar with the procedure" (250). Despite the richness of descriptive detail, however, Smithies' passage is still, as it must be, a highly selective account of events, focused on some aspects of the material work but not others. What might account for this selectivity, then? I propose that the way language is used in Smithies' paper also, like early experimental prose, taps into how an agent might physically cope with this recipient object, from this point of view, in this orientation, according to the physical capacities and constraints of a human body embedded in space. To give this sense of a human body embedded in a given spatio-temporal scene, the prose must reflect the movement of consciousness (attention) as linked to the physical movements of an agent acting on recipient objects (action).

There are, for example, phrases in Smithies' description which serve to orient the reader physically with respect to the apparatus. The author describes how the mineral oil is "removed by blotting the top of the gel"; how the gel is caused to "fall out on the cutting block by inverting it over the block and starting movement from one end of the gel"; how "the gel is then split along its length in a horizontal plane"; how the cut should be made "parallel to the bottom of the gel"; and how the gels should be transferred to a dish "with the cut surfaces uppermost" (632). The reader thus becomes practically oriented towards the apparatus in the same way as the implied, but invisible, agent in the passage, poised to cope non-conceptually with the apparatus. As a result, readers can picture the object from this point of view, in this light, in this orientation as though it were in their immediate spatio-temporal field. These kinds of statements are thus related to the organization of space around the body of the agent in the scene.

Merleau-Ponty notes how in classical science there was a clear distinction between space and the material objects within it. In this classical view, "space is the uniform medium in which things are arranged in three dimensions and in which they remain the same regardless of the position they occupy" (38). Everything changes, Merleau-Ponty argues, with non-Euclidean geometry. Suddenly, the world lacks the rigid framework of uniform space and the distinction between space and the objects within it seems to dissolve. We cannot even draw a distinction between the "pure idea of space and the concrete spectacle it presents to our senses," Merleau-Ponty states (39).

Modern art captures this shift to non-Euclidean geometry. Prior to painters such as Cezanne, the painter would establish perspective by arranging the landscape relative to a vanishing point on the horizon, towards which the viewer's gaze would be directed (Merleau-

Ponty 40). Such a perspective, according to Merleau-Ponty, does not involve the viewer, nor does it capture how we actually perceive the world. Modern painters, by contrast, disobeyed the laws of geometrical perspective, and "sought to recapture and reproduce before our very eyes the birth of the landscape" by trying to capture the subjective experience of spatial perception (Merleau-Ponty 41). Thus, in Cezanne's paintings, different areas of the painting speak to different points of view:

> . . . those who look closely will get the feel of a world in which no two objects are seen simultaneously, a world in which regions of space are separated by the time it takes to move our gaze from one to the other, a world in which being is not given but rather emerges over time. (Merleau-Ponty 41)

In Smithies' paper, the depiction of space is closer to this modernist, non-Euclidean rendition; his perspective does not adhere to the idea of space as a uniform medium in which objects remain the same regardless of bodily position. Instead, the description seems to follow the perspective of the agent who is embedded in the scene, rather than hovering somewhere with a view over the entire laboratory. In other words, the prose reflects the shifts in attention that agents might experience as they move around in the experimental scene; if they are doing something to the gel, their attention is on the gel, on the blade, on the cutting block, etc., rather than simultaneously on the entire scene and every object in it. The author only describes those objects being immediately handled. Objects also change with the agent's position. When the gel is inverted over a cutting block, or cut, or repositioned, the reference point for the description likewise changes to account for how the object now appears to the agent as s/he becomes poised to handle the object in this new orientation. Smithies' description therefore captures not only the experience of space, but also the experience of space in time as the agent shifts her or his spatio-temporal position through movement.

In order to recreate a vicarious embodied experience in words, then, experimental prose must also recreate the experience of constrained consciousness (attention) as tethered to a body. Smithies' paper conveys this kind of embodied, constrained attention by describing only those aspects of the apparatus towards which an agent's gaze and movements need to be applied on a moment-by-moment basis. Accordingly, only those aspects of the situation being paid active attention to *through movement* are described; readers vicariously experience only the gel, the edges of the tray, a spatula, a cutting block, the gel falling out onto it, etc. In other words, readers are urged to notice, firstly, only those aspects of the experimental scene directly in an agent's forward-directed, spatio-temporal field, and, secondly, only those aspects of the apparatus on which motor intentionality is being exercised. In terms of practical persuasion, what this perspective does is show where to direct attention while moving around in the experimental scene. The author makes some things stand out as opposed to others, thereby defining the experiential boundaries of specific research objects. The edges of the gel are worthy of notice, for example, but the ticking noise in the ceiling fan is not because the agent is not exerting motor intentionality on it. This selectivity may account for why the social interactions that are part of research work are omitted from the experimental paper, as they do not relate directly to the motor-intentional definition of the recipient object which the author is attempting to disseminate among a scientific community.

I stated before that apart from being an object in a laboratory, an apparatus is a sequence of specific actions, and knowledge of the apparatus entails the development of motor intentionality with respect to it. Language can be used to disseminate this motor-intentional definition of the object among others. The reader of Smithies' paper thus comes to know gel electrophoresis by being led vicariously through the critical movements that define this object. Critical movements, in this case, include the following: "[t]he cut should be made in a single movement, with the blade supported on either side of the gel by the flat guiding strips of the cutting block" (632); "the mixture is heated over a naked flame in a conical flask with constant and vigorous swirling" (630); "[t]he razor blade is then used to compress the gel to the left away from the cut to form a slot about 5 mm. wide, and the blade is withdrawn" (631); "[e]xcess of gel in the tray is then squeezed out by pressing on the polyethylene with a rigid plastic sheet" (631); "[t]he displaced section of the gel is then caused to close up to the other surface of the paper by returning it to its original position with a spatula introduced between the edge of the gel and the tray" (631); "[t]he gel is not displaced in the longitudinal direction in any way during the cutting out of the slot" (631), are but a few examples of how circumstantial prose can reflect the motor intentionality associated with a specific research object.

Critical movements here, then, do not include the fact that Bill gives Spencer a thin vial; the action component of my triad as depicted in experimental prose does not appear to mean all possible actions that happen in a laboratory scene. The critical movements that do make it into the experimental account are therefore those that are object-intended. This selectivity makes sense if we ask, "What is the element of this experimental scene that will potentially act a distance"? The answer is the apparatus itself, and hence why only those actions associated with making it fulfill its intended purpose are included in the text.

When the prose is action-oriented, but object-intended, something intriguing also happens with attribution and location of agency in the text: an embodied agent is simultaneously present and absent. This observation raises questions about features of experimental prose, such as use of the passive voice, which have long been commented upon by scholars. In particular, I am intrigued by the possibility that experimental prose framed in the passive voice can serve the important purpose of aiding practical alignment among a research community. I thus want to examine this important feature of experimental prose through the lens of my triad model.

Embodied Agency in Experimental Prose

Through prose rich in circumstantial detail, described from an agent-centered, but object-intended perspective, the reader can also, importantly, become implicated as an agent at the center of the described activities. Readers can have a sense of vicarious embodied involvement because the prose is, paradoxically, object-focused rather than subject-focused; there is bodily movement in the description; there is an object towards which movement is intended, but oddly no identifiable agent. Interestingly, the expected subject-noun whose actions are captured with an active verb is notably elusive in such passages. There are action verbs (all directed towards the object)—material entities are cut, heated, swirled, compressed, and squeezed—but no coherent, distinct agent responsible for these actions, as there often is in literary prose; there is literally *no body* in these passages that is responsible for the cutting, heating, and swirling, at least no identifiable one with distinct features, and yet bodily movement is nonetheless conveyed.

In English, there are three main ways of stating that x did something to y, technical-writing authority Lilita Rodman states, writing about voice construction in scientific articles. In the active, the sentence would read as "I calculated the standard deviation" (312). In the full passive, the sentence would read as "The standard deviation was calculated by me" (312). Finally, in the truncated passive (the style typically used in experimental prose) the sentence would read as "The standard deviation was calculated" (or, in Smithies' paper, "the gel is freed" [632]). Rodman suggests that the choice of grammatical style alters the "end-focus," the final element emphasized, and in the truncated passive the emphasis is on the verb ("was calculated," or "is freed") (312). Critically, Rodman states the frequency with which the truncated passive is used in scientific articles makes it "obvious that the possibility of *omitting the agent* must be an important motivation for using the passive" (312, emphasis added). Although I agree with Rodman on her characterization of the passive voice, and that it emphasizes the verb rather than the agent, I want to explore further her view (generally shared by others) of the possible motivations driving the use of the passive voice in experimental prose.

One effect of the use of passive voice in experimental prose, Gusfield notes, is the impression that the data and subsequent conclusions emerged almost spontaneously "from an external world of data or tables" (20). Swales observes that in passive descriptions, the method itself, rather than a human agent, seems responsible for what happens (120). Daniel Ding, an English Studies scholar who writes about the construction of agency in experimental communications, likewise notes that we do not see who did it, but rather "*what* was done," echoing Rodman's observation that the truncated passive emphasizes the action ("Object-centered" 304, emphasis in original). Although it is recognized that data depend on the actions of embodied agents, the impersonal style of the writing "denies that its author's actions are relevant to its content," according to Gilbert and Mulkay (56). When observations are rendered in the passive form of "X was observed in order to" it therefore seems as though "observers have eyes, minds and theories but never hands in the matter," Gooding similarly states (3).

From Smithies' description of gel electrophoresis, however, it does appear as though the author/observer did have "hands in the matter." As Rodman and Ding observe, it is the verb (the sign of the action component) that is emphasized in the prose, although the specific owner of the body performing the actions is not identified. It does not appear entirely accurate, then, to say, as Swales does, that the method alone is responsible for the actions depicted. An embodied agent is implied somehow. Again, it is this "somehow" that I am interested in.

Although Rodman portrays the passive voice as motivated by a desire to erase the agent from the prose, it is also important to note that individual authors have only minimal choice when handling a genre as entrenched as the experimental paper. Bazerman, for example, takes issue with critiques that posit the passive voice as the result of free literary control for the conscious purposes of effacing human agency. By framing scientific writing as "unbounded free play of competing interests," Bazerman argues, critics ignore the social and historical structure of scientific communications that constrain scientific writing (156). Segal likewise states that certain stylistic elements of the experimental paper, such as the passive voice, have become conventions and are often explicit prerequisites to publication, rather than the result of individual decisions by authors to suppress agency ("Strategies" 526). The issue of authorial intent is a complicated one; some intentions may be unconscious; some effects may be unintended; authors may recast their intentions in light of effects; intentions are culturally and historically contingent, and details concerning a given historical moment may be difficult to recover; intentions are also partly (or sometimes largely) defined by the groups shaping a particular genre, as Bazerman and Segal point out. In analyses of scientific prose, it may therefore be more productive to explore the writing on this level of the group, and to keep the focus on possible effects.

When vicarious witnessing and the (possibly concomitant) communal alignment of practices are taken into account, an alternate possibility emerges for why the passive voice became a consistent feature of experimental prose, and in particular in the methods section of experimental papers:[14] it can serve to show agency from a central spatio-temporal perspective, thus putting the reader directly in the scene as the centre of the action. This take on the passive voice thus relates to the notion of the experimental paper as a solution to a problem of social interaction, and the need to extend the witnessing public. The lack of an observable agent in Smithies' paper constructs the impression that the *reader* is the one looking out onto the scene as it happens, in the central position of the one moving and noticing aspects of the material world as they come to attention (reminding us, once again, of a non-Euclidean perspective on the world).

This alternate view of the effect of the passive voice on the reader is still congruent with the critical scholarship. Ding brings up the important point that "this thing-centered approach to scientific writing conforms to thing-centered scientific work" ("Passive Voice" 145). Atkinson also observes that "the agentless passive" is the norm for describing

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[14] Kathryn Riley asks, for example, about the rhetorical conditions under which it is preferable to use the passive voice, and suggests that changes in grammatical style correlate with how the experimental paper is divided (241). In sections that are expository (rather than argumentative), such as methods and results, the passive voice is most frequently used (247).

phenomena "in a basically object-centered way" (340). In other words, because scientific work is focused on the objects receiving agents' actions, the prose is similarly focused on the action and attention components of my triad. The phrase "object-centered" can still imply an embodied agent moving towards the object, handling the object, rather than an object being objectively described from a distance. Segal notes that the *American Medical Association Manual of Style* states that the passive voice is preferred "in instances in which the interest concerns what is acted on," and that since in scientific writing interest invariably lies in "what is acted on" the passive voice thereby dominates (527). We therefore tend to see the passive voice used more consistently and frequently in the methods section of experimental reports, since this section is, by definition, concerned with actions on objects, rather than the elaboration of theory.[15] Gusfield notes that the drama of the experimental paper thus comes from "the unfolding of the procedures of method" (21). When an author presents an experimental narrative in the passive voice it furthermore *shows* this unfolding of procedures, rather than telling the audience about it; the author thereby becomes "a camera," according to Gusfield, "with its shutter open, quite passive, recording, not thinking" (21).

The predominant view among scholars, however, is that the embodied agent is altogether absent from scientific prose. This view is seen in Gusfield's critique, when he states that the agent is absent "as a significant mover of events" (21). Ding comes to a similar conclusion when he states that the passive voice "de-emphasizes . . . human agents as action performers in this world of things" ("Passive Voice" 147). I disagree with the view that the passive voice necessarily de-emphasizes human agents as action performers in a

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[15] Indeed, Rodman finds that of the various possible types of noun phrases found in experimental prose, the methods section contains the largest percentage of "real world" referents (that point to concrete objects such as blades, cutting blocks, and spatulas), as well as "research process" referents (observation, calculation, measurement, etc.) (315).

world of things. Placing the drama of the narrative in the externality of methods and objects does not have to be incompatible with the presence of an active, but invisible, agent—the implied agent *behind* the camera, pushing the button, that we infer from the fact of the picture's existence, even though this agent is nowhere to be seen in the frame. Far from being "peripheral" to things, as Ding suggests, the human agent remains the spatio-temporal center of the experience, which can lead to the impression that the agent has disappeared completely. Embodied human agency does not disappear, I propose, but instead shifts from the author/agent to the reader, who is ideally already embedded in a research scene like the one described, and can rally the practical means to enact the experience. Language then potentially becomes part of the means of standardizing the motor intentionality associated with an object for a community of practitioners.

This viewpoint concerning the possible effects of the passive voice in experimental prose brings up questions related to science as an ultimately communal experience, and about language as a communal activity linked to physical practices. For example, how might a prose style framed from the central spatio-temporal perspective of an agent embedded in the scene be conducive to the standardization of motor intentionality among practitioners separated from one another in space and time? Next, I apply the triad model to this issue of practical and linguistic standardization.

The Embodied Agent as Experimental Variable

An important implication of the shift to an agent-centered perspective focused on the physical research context is that subjective, situated events experienced by specific agents can become extended over a larger group precisely *because* the embodied agent in the narrative remains unspecified. Passive prose conveys the idea that any *body* in the same position, performing the same actions, observing the same scene would achieve the same result. Sociologist Thomas F. Gieryn writes about the "presumption of equivalence" in scientific practice and discourse, where it is assumed that "the circumstances of production" are essentially the same at one lab as at another (127): "presumably true everywhere, supposedly from nowhere in particular" (113). Gieryn posits that claims move from "place-saturated" discourse to "place-less transcendent truths," where "place" incorporates natural geography and "built architecture" (113).

Extending Gieryn's thesis, I propose that equivalence is also achieved when the standardized body becomes the place of knowledge production; the standardization of motor intentionality detaches research objects from specific places and attaches them onto generalized bodies instead. Readers cannot help but arrive at the same conclusions as the author/agent when they vicariously retrace the same action-attention-language sequence.

The presumption of equivalence can therefore also mean the presumption of embodied coping equivalence. Historian of science Lorraine Daston describes "aperspectival objectivity" as the impression of interchangeability, and therefore featurelessness, of the observer (609). Aperspectival objectivity is about eliminating individual idiosyncrasy and making many I's/eyes into one (Daston 599). The agent loses the markings of nationality, education, "sensory dullness or acuity," any personal tacit knowledge, distinctive writing style, or any other idiosyncrasy that might interfere with the communicability of the phenomenon, Daston states (609). I would also add that anything which might interfere with the experience being potentially extended to the group—which might shift a reader's focus from actions on objects to a specific agent acting on objects—appears to be generally omitted. Whatever the author/agent in the passage was able to do is therefore, theoretically, doable by all—the "we" of science. I contend that the "we" often seen in experimental prose is still an embodied "we," and the perspective is still from the center of a human body. The body featured in experimental prose has, however, been averaged over the group, in terms of having all embodied idiosyncrasies and markings of individuality removed.[16]

When the situated idiosyncrasies of experimental work are excluded from the written account the narrative also becomes an idealized account of how things should happen, and not a complete account of what did happen. The experimental paper thus occupies a kind of liminal space between real past events and idealized future events (which are also based in the real world of objects and embodied actions). What actually did happen to the author/agent and what might/should happen to the universal reader/agent exist simultaneously in the text. Context-specific contingencies (especially those related to situated social relations) that are unlikely to arise again in a different context are therefore excluded from the methods section of the paper.

The kind of objectivity seen in experimental prose is not inevitable: it takes work. Ethnographic studies of science are certainly correct when they highlight the selectivity of experimental accounts, in terms of the types of actions performed in a research context become included in the text, and the way in which these actions are described. Daston explains that the kind of objectivity that allows "many scattered observers to compare notes on universal phenomena" must be actively constructed and maintained through communal

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[16] Experimental prose would never contain, for example, reference to the author/agent's arthritic hands, unable to handle delicate research tools, and that an experiment took several tries as a result. The universalized body of experimental prose is therefore much like the body of medical literature, with its diagrams of the prototypical man or woman, who has had his or her facial features erased and organs smoothed out into indistinctness so that every patient (gazing around the walls of the waiting room) can imagine themselves into the picture.

practices, especially those dedicated to the standardization of instruments, methods, and writing style (608). Accordingly, the kinds of contingent details that do make it into Smithies' paper are those related to helping the reader imagine, and potentially act out, the ideal scenario of experimentation promoted in the paper.

One way of encouraging ideal handling of an experimental scene—so that the reader arrives at the same practical destination as the agent in the written account—is to describe the concrete consequences of deviating from the proposed protocol. In Smithies' paper, the reader is warned, for example, that the boundaries of the protein zones must be vertical, "otherwise cutting out any required zone free from contamination with other zones will not be possible" (633). Elsewhere, Smithies writes of the anodic end of the gel swelling or shrinking if the bridge solution is not made properly, but that "under the conditions finally selected" this problem is mitigated (632). There are also statements that outline the practical consequences of deviating from the ideal protocol that are prefaced by how alternate methods have, in actuality, already been tried: "[a] number of Plexiglass (Perspex) trays were used in which to cast the gels," Smithies informs the reader, before revealing the dimensions "found most convenient" (633). Similarly, he writes how "[v]arious supporting substances were tried," but "[f]or the most precise work starch grains of a narrow size distribution were used, so that the packing throughout the vertical extent of the slot should be uniform" (631).

Such descriptions hybridize real past events and hypothetical future events into one narrative entity. The reader assumes that these events must have happened to the author/agent, but this is never explicitly stated. An event such as the contamination of zones in a gel thus floats between past and future, between the real and the possible. Interestingly, even these liminal, real/imagined experimental descriptions show the reader in a tangible way (that is still focused on recipient objects, on movement, and on the concomitant shifts in attention) what it would be like to physically encounter such a problem scenario and how to practically respond to it. The author thus guides the reader towards accepting the depicted practical coping sequence as inevitable and as the most efficient one for consistently making a specific object behave as desired.

Shapin proposes that accounts of failed conditions perform two important functions: they reassure the novice that success takes time and tinkering,[17] and they reassure the reader that the author/agent is not suppressing "inconvenient evidence" ("Pump" 494). As early as volume 80 of the *Transactions* (when articles became more argumentative and authors began to detail the care they took with the experiments to stave off possible objections) authors began demonstrating how other methodological alternatives have already been attempted (Bazerman 70). By relating these kinds of practical failures early experimental philosophers such as Boyle were able to portray, according to Shapin, the recounted experience as a truthful mirror on the world, "in which a wide range of contingencies might influence outcomes" ("Pump" 494). This concern with variables—"differences of conditions or execution of the experiment that might affect results"—became an explicit feature of experimental design and the experimental paper (Bazerman 71).

What still remains implicit is the embodied human element as an experimental variable (the most important one, in fact) to be controlled. The language used in Smithies' paper betrays this concern for the variability of the embodied human element. The author prefaces many sentences in the methods section with some variation on the phrase "it is essential that," or "for the most precise work," or "it proves necessary to." For example,

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[17] On the importance of "tinkering" in experimentation see Knorr-Cetina's ethnomethodological study, "Tinkering Towards Success: Prelude to a Theory of Scientific Practice."

Smithies writes: "[s]tandard conditions are essential at this point if the gels are to be reproducible, and are more easily obtained if . . ." (631). This cautionary tone is not about getting the inert machine to do anything; it is meant for the reader, an attempt to persuade the human variable to act less variably, to follow a prescribed action-attention-language sequence associated with a given object.

The embodied human thus becomes an inextricable part of the experimental system, and thus another experimental variable, rather than a detached agent whose bodily participation remains outside the boundaries of the experiment. The challenge for communities of practitioners, then, is the establishment and maintenance of communally patrolled experiential boundaries that constrain how others act with respect to common research objects—in Smithies' case, a gel electrophoresis apparatus. As Latour and Woolgar state, sometimes the work must remain in the text for persuasive purposes. The kind of persuasion being discussed is thus of a practical nature, aimed at standardizing many separate bodies into the type of uniformity that makes science communicable and, importantly, communal.

Standardizing the Embodied Agent Variable

If we can understand how situated, embodied experiences become translated into words and then into narratives of experimentation so that others, embedded in similar laboratory settings, can understand these experiences and adjust their own practices, we begin to understand the unparalleled success of science as a social movement; science is surely one of humanity's most successful examples of practical agreement. The mechanisms by which this feat of social organization is accomplished—materially, socially, and linguistically—are still a source of much discussion among scholars.

Scholars in various STS disciplines have debated the issue of how practices become communally aligned in science. In Amann and Knorr-Cetina's study of gel electrophoresis, practical standardization with respect to specific research objects comes up as "consensual ways of seeing" and "modes of relating to an object" that are maintained through shared standards of practice (134). Similarly, Cambrosio and Keating conceive of scientific technique as a situated system of practices where movement is standardized (249). The authors speak of the difficulty of transmitting the "visual and motor aspects of the procedure which contributes to the learning of a gestalt" (249). Cambrosio and Keating also talk of instructions as "embodied in a series of gestures," where the written instructions evoke nonpropositional, imaginative associations related to things like "shapes, colors, time spans, and gestures" (249). Boelie Elzen similarly notes that researchers try to stabilize artifacts by influencing situated practices (655). Sociologists Kathleen Jordan and Michael Lynch, writing about the communal standardization of plasmid prep technology in molecular biology, refer to "deliberate attempts" to standardize laboratory procedure and observe that procedures become situated rituals based on mimicry and "arise from a 'purely' social process of following what others have taught one to do" (93). Shapin and Schaffer propose that controversies in science are often over methods, and over "different patterns of doing things and of organizing men [sic] to practical ends" (15).

The common thread is thus the importance of communally patrolled experiential boundaries in science, and the need to constrain how others act with respect to specific objects; I link this theme of communalism specifically to shared embodiment, and to a shared bodily means of perceiving and responding to the material world.

I propose the term "practical persuasion" to refer to how experimental events are rendered communicable in an embodied way. The term "practical persuasion" thus relates to how specific embodied experiences can be translated into words and shared among many. From the subjective and situational the universal-objective (as in apersectival and interchangeable) is formed, from which the reader can imagine and perhaps begin to organize the situational and subjective, from which a new universal-objective is formed again, and so on. Establishing the kinds of standard conditions of behavior that result in consistent experimental phenomena, however, necessitates more than just the standardization of motor intentionality; attention, as well as the way that language is used to represent a complex experimental situation, must also be standardized. According to this view, then, when agents come into an experimental scene with a specific vocabulary, gleaned from others' accounts of their experiences with the same object, action and attention become narrowed and purposeful. This is one way in which scientific texts can act at a distance.

It is important to note that the experimental paper evolved to give the *impression* of replicability.[18] As Gross proposes, "in contemporary papers the possibility has replaced the

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[18] Boyle admitted that, despite the richness of his circumstantial detail only one or two of his engines had been made to work; "[f]or though they may be easily read . . . yet he, that shall really go about to repeat them, will find it no easy task," he finally laments (qtd. in Shapin, "Pump" 490). The difficulty of this early experimental apparatus may have had much to do with these issues of replication. On the issue of replication of experimental conditions in science, see Collins and Pinch; Travis; Swales (in particular page 121, where Swales speaks against the idea that the purpose of the methods section of an experimental paper is to permit replication); and Knorr-Cetina (*Manufacture*).

In an article that looks at the practical relationship between text and actions on objects, Frances Willmoth attempts a reconstruction of a seventeenth-century plane table (a survey instrument), based on Arthur Hopton's 1611 description. Not surprisingly, despite Hopton's circumstantial details, Willmoth finds the reconstruction "not . . . entirely straightforward" (352). Although Willmoth does manage to make the table (a feat that I think actually does show how instructive such circumstantial details can be in physically organizing a reader), she concludes that the text serves, importantly, more "to convince the reader that the author had seen all fact of replication" ("Form" 18). Through agent-centered, object-intended prose, readers potentially take away with them new visions of themselves acting on objects in an experimental setting. Dear points out that the unique combination of circumstantial specificity, along with a recipe-like format, conveys the impression that although the event described is a discrete one, it could nonetheless be replicated (153). When readers are helped to imagine specific actions on specific objects through an account rich in such circumstantial details, the event and object begin to take on a material reality. At the least, experimental prose rich in circumstantial details establishes in the reader's mind of the *possibility* of this kind of material situation, of specific actions on specific objects, and a means of representing this physical experience in language. What the author is doing, in a sense, is imaginatively walking the reader through a research scene that already presupposes the real-world existence of this kind of material situation involving this type of object.[19]

The reason why circumstantial prose is able to walk readers through a material situation they have not experienced, I want to stress, is that it is written from the perspective of a human body in the midst of material circumstances. The described experience becomes

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these instruments in use and knew what he was talking about" (358). See also Sibum, where the author makes a replica of Joule's apparatus based on texts.

[19] There is now evidence from cognitive science that certain uses of language rich in spatio-temporal details may also literally affect a reader's ability to act by stimulating parts of the brain associated with movement. Some cognitive researchers are arguing that cognition is intimately tied to sensory-motor experience, and that reading descriptive passages centered on movement may lead to a kind of mental simulation of the event. In support of such motor resonance (or "simulationist") theories of language, neuroimaging studies show that reading or listening to action words activates the motor and premotor cortex regions of the brain associated with the actual performance of those actions (Buccino et al.; Hauk, Johnsrude, and Pulvermüller; Rumiati and Caramazza; Tettamanti et al.; Wilson). In other words, "[i]magining and doing use a shared neural substrate," as Vittorio Gallese and George Lakoff propose (456). In a different set of reading experiments, Rolf Zwaan and Lawrence Taylor show that observing the rotation of objects influences the processing of sentences about rotation. Daniel Richardson et al. similarly provide evidence that the comprehension of verbs involves the activation of mental spatial representations. Although such studies are limited in terms of what they reveal about the hidden processes of the mind, they nonetheless point to a connection between embodied experience and how we process language. Arthur Glenberg and Michael Kaschak propose that potentially all language comprehension is grounded in embodiment, a view reiterated by Gallese and Lakoff.

shared because readers share the same basic motor and perceptual capabilities. If we put the body into scientific prose, then language can also act to constrain attention in a physical research situation.

Experimental narratives full of circumstantial details describing proper movement and where to direct attention can thus be said to disambiguate the experimental scene for the reader. Prose such as Smithies' can show how action, attention, and language must be narrowed in order to cope with a specific research object, in no specific order, but rather simultaneously. Through others' experimental narratives, the world is served to us already full of names for things and processes, and this naming can order our experience of the world. The description shows the reader what to pay attention to while performing a given action and what this segment of his or her spatio-temporal field is called. Our attention and our movements will then be directed, in a less ambiguous manner, towards those aspects of our spatio-temporal field that have been given names. On the other hand, we cut the world up into words when we exert motor intentionality on it.

Without the communal alignment of these three elements—the ability to move in the required way, to notice the required things, and to represent the experience in the right words—the collective project of science could perhaps not succeed. Language is instrumental in the scientific process, and it remains as pressing as ever to understand exactly how language fits into a larger picture involving many bodies acting in physical ways on the varied objects of research.

My triad model makes explicit that all action, perception, and language flow through bodies that are very similarly constrained in their relations with the material world. We understand one another's descriptions of laboratory events because we share the same basic structure and perceptual capacities of a human body.[20] Our bodily understanding of the world, then, makes the alignment of practices possible because the body is put into our descriptions, as Lakoff and Johnson show, as a way of orienting and understanding one another. In this chapter, I have endeavored to show what it means to analyze scientific texts with this idea of shared embodiment in mind, and have shown one way of linking the physical and linguistic practices of research into one heuristic model. My action-attention-language triad model will serve to guide my analysis in the chapters that follow, where I take the model into scientific communications dealing with animal experimentation, advertisements for research apparatus, and cyberscience. Having characterized here what it means for scientific prose to refer extensively to actions on objects ("a rhetoric of immediate experience"), I turn to instances of scientific communication which increasingly problematize the correspondence between physical and linguistic practices, between what is done in an experimental scene and how those actions are described.

[20] The question of minimal embodiment immediately comes up. Would Stephen Hawking, for example, with his different body, experience and construct different accounts of experimentation? If his account was a personal one that read more like a laboratory diary, then perhaps yes. But scientific accounts are communal constructions, as I have been arguing, and this communalism would erase the embodied idiosyncrasies from Hawking's account. Secondly, Hawking's body, although wheelchair bound, is still organized according to the same basic asymmetrical structure as other human bodies, rather than, say, that of a spider. If spiders did science, their accounts would presumably sound very different from ours, but would be comprehensible to other spiders. The question is a fascinating one to contemplate.

CHAPTER TWO

The Reciprocal Tuning of Agents and Objects

In the previous chapter, I explored the idea that experimental prose can be focused on the nonhuman objects towards which actions, attention, and linguistic choices are directed. I showed how experimental prose dealing with apparatus, in particular, can be about human-nonhuman interactions, and about ways of standardizing motor intentionality among a group of practitioners. Embodied negotiations between agents and objects are not unidirectional, however, and each has the ability to affect, in different ways, what happens with the other. Even with an inanimate experimental object such as a gel electrophoresis apparatus, the agent is continually asked to be ready to respond to the material contingencies posed by the object. In Smithies' paper, the gel was hard to handle, the protein bands were not always clear, and the whole procedure was timing sensitive. Pickering's extension of Karl Marx's aphorism, "production not only creates an object for the subject but also a subject for the object" (qtd. in Pickering, "Practice" 163), to the laboratory situation thus aptly portrays experimentation as the "reciprocal tuning of people and things" (163). "Reciprocal tuning" importantly implies that objects also act, in a sense, on subjects, and that agency can be extended beyond Bazerman's formulation of "concrete operations on concrete objects"; objects can also act on those that act on them, and this clash of material agencies makes its way into the discourse of science.

With this idea of reciprocal tuning of objects and agents in mind, I focus on a different kind of research object in this second chapter: the mouse—or, more broadly, the animal model. Unlike an inert apparatus, the mouse-object constitutes a self-moved center of experience in its own right. As such, it is an inherently unstable research object, in terms of the construction of predictable action-attention-language sequences that agents separated from one another in space and time can follow in order to arrive at the phenomena of interest (such as specific mouse behaviors); practical concessions must constantly be made to accommodate the natural capabilities of mouse bodies—this is what I mean when I say that objects “act” on the human agents that would act on them. Even once stabilized, the mouse-object retains the ability to surprise with unexpected responses. We thus witness the same kinds of attempts to disambiguate the mouse-object that I described in the previous chapter with respect to gel electrophoresis. The mouse-object, however, resists such attempts at stabilization in ways an inert apparatus does not. In researchers’ discursive productions dealing with the mouse-object, then, the reciprocal nature of material tuning is foregrounded.

In this chapter, I am interested in what happens when attempts to standardize motor intentionality, and hence communal definitions of a research object, break down, and the process is revealed as partly ad hoc and reactive to the constraints of mouse embodiment. Here, I study one particular community of practitioners as they attempt to standardize the mouse-object, both in a physical and discursive sense. When this particular research object pushes back with its unpredictable embodied responses and evades stable (material) definition, the community is forced to return to the “negative modalities” of data construction that Latour describes. The discussion that ensues in scientific journals, which I will detail, points to some of the ways in which a complex research situation is normally simplified through selective attention and language-related omissions. The discourse around the mouse-object also shows how the relations between action and language can be much more flexible than Bazerman’s formulation would initially indicate. There can be a substantial gap in the case of animal modeling between situated actions and how those actions are represented in experimental prose. An analysis of the mouse-object can therefore potentially offer important insights about how the materiality of research work is translated into scientific knowledge.

I chose to study the mouse in this chapter as an example of a more complicated recipient object because of its widespread use as a model organism in biomedical research. Indeed, in 2007, the Nobel Prize in Physiology or Medicine was awarded to Mario R. Capecchi, Martin J. Evans, and Oliver Smithies for discovering a method of introducing specific gene modifications into mice via embryonic stem cells. The technology known as *gene targeting*, involving the inactivation of single genes in mouse embryos to generate "knockout" mouse strains (meaning that a specific gene has literally been knocked out of the mouse's genetic, and hence biochemical, profile), is being used to probe the roles of specific genes in nearly all areas of biomedicine. This toolbox of experimental methods involving genetically-altered mouse bodies is said by the Nobel Assembly to have revolutionized biomedical research and the development of medical therapy. Mouse knockout technology has so pervaded biomedical research, in fact, that the Nobel Assembly declares it is "difficult to imagine contemporary medical research without the use of gene targeted models" (n.p.). The pace of research involving genetically-altered mice is also slated to intensify, as researchers are currently trying to make knockout mice available for all of the genes in the mouse's genome (where each strain of mice will be missing a specific gene, and hence the molecular products of that gene) (n.p.).

This "mousification of medical science," as science and medicine writer Greg Critser calls it, means that our current conceptions of human health and disease have become mediated by one of the most vilified pests known to humankind. Critser states that the widespread use of mouse models, where the mouse functions as the official proxy for the human body, has led to the expectation in biomedical research that every NIH grant application "have some kind of mouse narrative – x into mouse equals y out of mouse" (68). Fujimura might therefore call this mouse technology highly "doable," where doability is "conceptualized as the alignment of several levels of work organization," making one line of research literally more doable at a given historical moment than another ("Constructing" 258). Mice are often used to model human biology not because they are the most appropriate experimental system for the study, but because they have become a highly doable experimental system. The physical and linguistic tuning of this particular research object, and how actions on this recipient object are depicted in scientific prose, are thus much more problematic than in the papers dealing with inert apparatus described in the first chapter.

The historical forces that have led to the unparalleled prominence of mouse models in biomedical research have been thoroughly explored elsewhere. In particular, both Karen Rader and Jean-Paul Gaudillère give detailed accounts of how The Jackson Laboratory became a key supplier and promoter of research mice. More explicitly social critiques of the focus on genetics in biomedicine can be found in Celeste Condit (*Meanings*); Evelyn Fox Keller; Daniel Kevles and Leroy Hood; Dorothy Nelkin and Susan Lindee. The main concern in these critiques is the social applications of the research findings discovered by molecular biologists, this concern often stemming from the deterministic language used to describe the role of genes in human biology and social relations.

While the possible social implications of genetic research have been explored, the same critical attention has not been given to the questions concerning the process whereby "x into mouse equals y out of mouse," whereby the mouse body becomes translated into data, and from there into what Critser calls "the scientific mouse narrative." Here, I follow one specific mouse narrative, the monoamine oxidase A knockout mouse (MAOA KO), where a targeted disruption in the gene that codes for the enzyme MAOA becomes linked to a behavioral syndrome, in both mice and men (specifically), supposedly marked by enhanced aggression.

The community of practitioners that forms around the MAOA KO mouse must patrol the material and discursive boundaries associated with this specific research object, and set limits on reciprocal tuning, on what aspects of the embodied mouse response count as data, how best to elicit the desired responses, and how to talk about it. The community tries to define material and discursive boundaries through practical persuasion, by defining the motor intentionality associated with the object, by specifying where agents should direct their attention in the midst of practical coping, by showing how to represent the experience in scientific prose, and how to use these material experiences with mice to make scientific claims. Unlike an inert apparatus, however, the mouse-object refuses to enter into permanent (embodied or symbolic) relationships with agents; its responses change unexpectedly. The mouse-object thus continually points back to the conditions of its construction as an object of knowledge, pushing the community of practitioners clustered around it into controversy. This particular research object makes agents continually realign their practical coping and their language; I use my triad model to show what this constant physical (re)alignment and discursive selectivity looks like in a series of specific textual examples, and the ways in which experimental prose about mouse bodies attempts to conform to the conventions of embedded witnessing.

In the sections that follow, I explore how the use of animals in research complicates the relationship between ways of doing science and ways of talking about it. I begin by discussing how the constraints of mouse embodiment shape agents' actions, and how the reciprocal tuning of agents and objects is framed in experimental prose. The mouse body is a difficult research object with which to study human biology, psychology, and social behavior; complicated discursive maneuvers therefore become necessary in the experimental paper to make the mouse body speak about these phenomena. I use neurobiologist Steven Rose's criticism, in his 1995 *Nature* paper titled "The Rise of Neurogenetic Determinism," that mice as research objects often lead to a "faulty reductive sequence" (380) of explanation to explore what happens when actions on this recipient object and the discourse that results do not easily fit together. I argue that the use of mice to model human biology encourages such reductive maneuvers when researchers attempt to align their actions, attention, and language. I posit that in order for animal bodies to say something about human biology, certain terms, such as "aggression" must be able to float between different material situations and different kinds of bodies. I call this phenomenon "discursive pleotropism," a play on the term "pleiotropism," which in genetics denotes how one gene can lead to several outward traits. Such discursive pleotropism may be one way in which language can overcome the physical constraints of specific research objects. I end with a case of controversy involving the use of mice in neurobehavioral genetics research, which shows some of the ways in which the mouse body is normally made stable as an epistemic object through a series of practical and discursive omissions.

Constrained Objects, Constrained Agents

I begin by looking at a paper that initially links the enzyme MAOA to behavioral abnormalities in the males of a Dutch family. This landmark human study sets the practical-discursive constraints on the animal studies that follow, as is often the case in biomedical research involving an animal model. Hans Brunner et al.'s 1993 *American Journal of Human Genetics* paper reports observations on eight males from the same family who exhibited "behavioral problems" marked by "repeated episodes of aggressive, sometimes violent, behavior" (1035). This aggressive behavior "was usually triggered by anger and was often out of proportion to the provocation" (1035). In one instance, a twenty-three-year-old male was convicted for the rape of his sister. This same male, at the age of thirty-five, stabbed a warden of an institution "for psychopaths" in the chest with a pitchfork, after having been told to get on with his work (1035). Similarly, another male from this Dutch family attempted to run over his boss with a car after being criticized for his work (1035). A third male "would enter his sisters' bedrooms at night, armed with a knife, and force them to undress" (1035). Two males were known arsonists. Several males in the family would also "suddenly grasp or hold female relatives" (1035).

Despite the varied behaviors exhibited by the men, Brunner et al. map this "very characteristic abnormal behavior" to a place on the X chromosome, "the monoamine oxidase type A locus in Xp11.23-11.4," to a gene that codes for the sequence of amino acids that make up the MAOA molecule, using a technique called linkage analysis (1032). The enzyme MAOA catalyzes a chemical reaction known as oxidative deamination in a number of important brain neurotransmitter molecules, such as serotonin, norepinephrine, and dopamine. These molecules are generally believed to be important in the regulation of mood and behavior.

Genetic mapping itself is an abstract and assumption-laden enterprise, and many of Brunner et al.'s conclusions are full of the requisite hedging, where the strength of claims is down-graded (on hedging in experimental papers, see Horn; Hyland; Myers "Pragmatics"; and Thompson). For example, the authors state that "the locus for this disorder *could be* assigned to the Xp11-21 interval"; "*these data are compatible with* a primary defect in the structural gene for MAOA"; "a mutation involving either or both of the MAOA and MAOB genes *could explain* the clinical picture in the family reported here," thus leading the authors "to conclude that the characteristic behavioral phenotype in this family *is probably due to* isolated MAOA deficiency" (1037, emphasis added).

Notwithstanding such hedging, the computer linkage data become the starting point for a narrative linking aggression among the males in this Dutch family to the MAOA gene. In the discussion section of their report, Brunner et al. state with less hesitation: "we here describe a family with X-linked nondysmorphic borderline mental retardation *that is caused by* a genetic defect in Xp11-21" (1036, emphasis added). According to Latour's classification of statement types as either leading the reader back to the conditions of data and statement production ("negative modalities"), or as leading the reader away from the fact that the statement was constructed "from a complicated work situation" ("positive modalities") (23), Brunner et al. thus move from negative modalities to positive modalities that remove traces of ownership and solidify their claims.

In their second paper on this Dutch family, published in the prestigious journal *Science*, Brunner et al. encourage further studies on MAOA using other families or even animal models (579). "Animal models," Brunner et al. propose, "could help to determine the various neurochemical alterations that are induced by selective MAOA deficiency and their secondary effects on the organism" (580). The authors' explicitly-stated hope is that animal studies might elucidate treatment options for "the metabolic disturbance caused by the MAOA deficiency state" (580). Here, all hedging has been removed from Brunner et al.'s language, and aggression has been definitively linked to the MAOA gene. On June 23, 1995, Oliver Cases et al. publish a paper in *Science* describing a line of genetically-engineered mice lacking a functional MAOA gene (a MAOA KO strain, for short). Their findings support Brunner et al.'s: without a functional MAOA gene, the authors conclude, the male mice "manifested a distinct behavioral syndrome, including enhanced aggression in males" (1763).

So what does it mean for a mouse to be excessively aggressive? How does mouse aggression compare to human aggression? How, exactly, does one use a mouse in a controlled research setting to model stabbing someone in the chest with a pitchfork, or forcing a woman to undress at knifepoint? How, in other words, is the mouse body being used, both practically and discursively, to model human aggression? Put differently, how does a research community interested in the biology of aggression (or other kinds of behavior) decide what aspects of mouse embodiment, of mouse behavior, to count as data? What does it mean for agents in this kind of research scene to act on this recipient object? It may seem outlandish to talk of mice wielding pitchforks, but I think such questions are critical in experimentation involving animal modeling, especially in those experiments dealing with complex human behavior. Since it is impossible to model the exact human behavior, what is close enough, and how can agents reliably elicit these behaviors from mice (and then portray the reliability of these behaviors in scientific prose)?

The discursive convention that experimental prose must deal with situated actions on objects witnessed in the laboratory setting ("a rhetoric of immediate experience," to call up Atkinson's phrase once more) forces both practical coping and the prose to be focused on the mouse body, on what was done to the mouse body, and on what the mouse body did in return. The mouse body, however, is highly constrained, in terms of its range of behaviors (none of which resemble those described in Brunner et al.'s papers); there are simply things it cannot do, like force a female mouse to undress at knifepoint. Bazerman's observation that a researcher "cannot use impossible machines, nor can he make machines do what they cannot do" (207) becomes especially salient here. Historian of science Hans-Jörg Rheinberger likewise explains how the kinds of questions researchers can ask and the kinds of answers they can expect are all highly constrained by the choice of experimental "system" (306). The mouse body, as the experimental system in this case, exerts its material agency on experimentation by refusing to wield a pitchfork.

Despite such pressing material constraints, however, the genetically altered mouse is currently, as Critser points out, the most popular biomedical research tool around. Even in instances where the mouse body might not be the most appropriate model for the study, its ubiquitous availability often makes it the one that researchers choose. The author must then use situated experiences involving this problematic research object to construct a scientific narrative, from which clear conclusions about aggression and the MAOA gene can be deduced. In order to make the mouse body do the epistemic work that researchers need it to do, then, the language component must do extra work to make the mouse body answer the complex theoretical questions related to human social behavior being asked.

In the specific situation of the MAOA KO mice, a limited repertoire of possible mouse behaviors must be manipulated in the laboratory setting to model aggression, preferably in a way that is easy to characterize, consistent, and, above all, quantifiable. As mice cannot wield knives or pitchforks, force a female mouse to undress, or run someone over with a car, the situations described by Brunner et al. cannot be replicated with mice. Of course, even in experimentation involving human objects, such behaviors would never be replicated, but neither can mice fill out psychological questionnaires regarding their behavior. What the mice can do is bite, squeak, rear up on their hind legs and sniff the air, mount one another, poke their noses into things, walk around, and swim. From this available repertoire of possible murine behaviors, an experiment on aggression must be designed. The specific action-attention-language sequences that are developed to transform a mouse into a scientific mouse-apparatus are thus necessarily shaped by what the mouse body can naturally do, and from these constrained experiments conclusions about genes and complex social behaviors follow. In Cases et al.'s 1995 *Science* paper, the mice are run through what are called the Porsolt forced swim test, the open field test, the beam walking test, the resident intruder test, and tests for mating behavior.[21]

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[21] The Porsolt Forced Swim is named after its inventor, Roger D. Porsolt, in a 1977 *Nature* paper. It is supposedly a measure of depression in rodent models. After being placed in a tank of water with no possibility of escape, if the animal spends the majority of its time floating, instead of trying to escape, it is described as despairing, and hence depressed. Although initially controversial, the forced swim has become the standard test for "behavioral despair." The history and rhetoric of this test are ripe for analysis. For a scientific critique, see Franco Borsini and Alberto Meli. In Cases et al.'s paper, the MAOA KO mice "instead of mostly floating, made persistent attempts to escape" (1764). The open field test is said to measure anxiety; a rodent is placed in an open field and monitored for anxious behavior. Cases et al.'s mutant mice "stayed a longer time in the center, with much hesitation as to which direction to take" (1764). The beam-walking test is exactly that: the animal is made to walk a beam. It is used to evaluate motor impairment. In Cases et al.'s paper the MAOA KO

The study of aggression more specifically in Cases et al.'s paper begins with the observation that males housed in groups showed "signs of offensive aggressive behavior," characterized by "bite wounds on genitals and rump" that were detected by missing fur and scabbing, as well as by palpation (1764). The most common laboratory behavioral test to measure aggression among mice is usually a variation on the resident-intruder scenario, which capitalizes on the fact that male mice, when isolated for prolonged periods, become aggressive towards those of the same sex introduced into their territory (the intruder). In his 1975 paper detailing the test, John B. Thurmond cites its advantages in the following terms:

> The advantages of the technique include the use of a naturally occurring aggressive behavior (as opposed to techniques employing long periods of isolation, shock, or drugs), the highly reliable occurrence of aggression, . . . the clear and valid measures of aggression produced, the simple and sturdy apparatus, and the convenience and economy of data collection. (879)

In Cases et al.'s paper, in one version of the test, six-month-old residents were given a single ten-minute encounter in their home cage with a two-month-old C3H intruder, a variant mouse strain accepted as docile. In another version of the test, the residents were housed with a female from the age of two months and the intruders were housed in groups of ten. In yet another version, MAOA KO mice and the "docile" C3H control males were housed in isolation for five weeks, beginning at weaning, and were then given a single three-minute encounter in their home cage with two-month-old C3H intruder. Latency to "the first

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mice "grasped the edge of the beam with hindlimbs while walking, whereas C3H adults were sure-footed" (1764). It should be noted, however, that MAOA KO mice have retinal degeneration and are also blind, a point which is revealed in the paper, but not factored into the analysis of the data. This aspect of the mouse body is thus selectively deemed not a relevant part of the experimental system, and neither is the fact that mice exhibited "intense head nodding," "trembling upon locomotion," "prolonged righting," "frantic running," and "sleep accompanied by violent shaking" (1764).

appearance of biting attack" was noted (1764). One month later, the same residents were given another ten-minute encounter in their home cage with a two-month-old BALB/c (another "docile" strain) intruder.[22] In both sets of tests, Cases et al. observe that the MAOA KO residents were quicker to attack the intruder than the C3H control strain residents were—a difference of 163 seconds between strains in the one test, and 62 seconds in the other (1764).[23] The way in which the resident-intruder experiments are set up therefore capitalizes on naturally occurring mouse behaviors, but in such a way as to generate numerical data that can be used for statistical analysis and to make graphs, which can then be compared among research groups to ensure standardization. Experiment design, in terms of the actions that agents can take on this recipient object, must factor in not only the constraints of mouse embodiment, but also acceptable forms of data.

The modeling of sexual aggression in the MAOA KO male mice is especially interesting. Without explicitly invoking the idea of rape, which is part of the behavioral inventory in Brunner et al.'s paper, Cases et al. note "alterations . . . in mating behavior" (1764). When "virgin" MAOA KO males were given a thirty-minute encounter with a nonreceptive virgin C3H female, "the courtship" was punctuated by "episodes of grasping" (12 $\pm$ 2 grasping events, mean $\pm$ SEM, n=5) that were accompanied by "increased frequency and intensity of female squeaking" (113 $\pm$ 20 versus 28 $\pm$ 8 squeaks) (1764). Based on such

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[22] It would be interesting to track down the history of the resident-intruder behavioral test and the way in which it became communally legitimated. Cases et al. make no reference to the original authors. The earliest mention of the test that I found is a 1975 article on territorial behavior in roaches, although John B. Thurmond's paper is probably closer.

[23] The average attack latency for the MAOA KO mice is 74 $\pm$ 10 seconds (just over one minute from the moment of introduction of the intruder to signs of fighting); the C3H control mice begin attacking in 237 $\pm$ 18 seconds (1764). In the second version of the test, where the residents were isolated, the MAOA KO mice attacked on average of 110 $\pm$ 12 seconds from latency; the C3H control mice attacked 172 $\pm$ 4 seconds from latency. When these same residents were introduced to two month old BALB/c intruders one month later, the MAOA KO mice attacked 27 $\pm$ 6 seconds from latency and the control mice an average of 252 $\pm$ 35 seconds from latency (1764). All of these numbers are reported as statistically significant in the paper.

data, Cases et al. conclude that MAOA deficient male mice “display enhanced aggression under standard rearing conditions,” supporting the idea “that the particularly aggressive behavior of the few known human males lacking MAOA . . . is a more direct consequence of MAOA deficiency” (1766).

Taking a step back to look at the process whereby “x into mouse equals y out of mouse” in light of the embodied constraints of this recipient object, however, evokes the seminal question unexamined by Cases et al.: are these behaviors the mouse-equivalent of the behaviors exhibited by Brunner et al.’s Dutch men? More importantly, are these naturally occurring behaviors instances of aggression at all? The resident-intruder test may be a “sturdy apparatus,” in terms of being able to lead the agent—through deliberate action, attention, and language sequences—to the phenomena of interest, but does it measure “aggression”? In other words, how might the language component be compensating for the material limitations of the mouse body, and forcing connections between what was done in the laboratory and what the data mean?

Researchers in neurobehavioral genetics face at least two practical problems, according to Helen E. Longino, a feminist philosopher of science: how to determine the strength of the connection between a suspected causal factor and a given behavior, and, less commonly but perhaps more importantly, defining behavior more broadly and then figuring out how to measure it (688). The problem of defining behavior becomes even more vexing for researchers working with an animal such as a mouse which exhibits a very narrow range of responses. Wim Crusio, a behavioral neurogeneticist, identifies the problem as “the definition of the phenotype,” in terms of what counts as the outward manifestation of specific genes, referring to whether the outward behavior displayed by the men in Brunner et al.’s

Dutch kindred should even be termed aggression, or, more simply, whether aggression can properly be called a trait and linked to genetics (459). Philosopher of science William Bechtel also notes that questions about what counts as data are often paramount for scientists. This concern recurs, according to Bechtel, because “the generation of data is typically the result of procedures designed to transform radically the phenomenon under investigation so as to generate the data” (167).

A radical transformation of another sort not entertained by Bechtel must also happen in order to generate data from animal models; a radical *linguistic* transformation is often required as well to compensate for the material constraints imposed on experimentation by animal bodies. If doability constrains the kinds of objects researchers can work with, and physical constraints related to embodiment limit how they can work with them, then various linguistic maneuvers become necessary in order to transcend these constraints to construct narratives of biological causality. In short, the data cannot appear constrained by the phenomenology of the mouse body, meaning that the data cannot appear specific to the immediate mouse situation. In experimental prose, the mouse body is thus simultaneously referred to, as it must be according to convention, and left behind as the place of knowledge construction, which leads to issues about how mouse data are interpreted.

Constrained Objects, Constrained Experimental Prose

When the mouse body is used as a source of answers for human behavioral phenomena, the pathway from situated laboratory experience, to data, to a narrative of causality is likely to be a complex one. Biologist Steven Rose claims that the core contention between neurogenetists and their critics is over how and where to seek the causal determinants of behavior (380). Animal models in neurogenetic research, Rose argues, are likely to lead to a "faulty reductive sequence" of reasoning "whose steps include the following: reification, arbitrary agglomeration, improper quantification, belief in statistical 'normality', spurious localization, misplaced causality, and dichotomous partitioning between genetic and environmental causes" (380). I take Rose's reductive sequence as a starting point and build upon it using my action-attention-language model to look at how the material constraints of animal experimental systems might encourage precisely the kinds of discursive maneuvers that Rose identifies. In other words, I show this reductive sequence at work in the prose itself in a specific instance.

According to Rose, reification converts a dynamic, interactive process into a static linguistic phenomenon (380). Violence thus becomes a characteristic, rather than a type of interaction, and the human or mouse is said to be aggressive. It also becomes something that can be abstracted from the total context in which it happens, Rose notes (380). Rose furthermore argues that if the various activities captured by the term, "aggression," can only be grasped in terms of an interaction between individuals, then "to reify the process is to lose its meaning" (380). "Arbitrary agglomeration" then takes reification a step further, Rose states, "lumping together many different reified interactions as all exemplars of the one thing" (380). In research, these dynamic interactions are then bracketed into discrete units of observed activity and "given numerical value" in some manner, a process that Rose calls "improper quantification" (381). Once quantified, these observed units of behavior can be reframed in statistical terms, and depending on where the data fall on a mathematical (typically "bell") curve, they can be labeled as "normal" or "abnormal" (381). Abnormal behaviors, Rose states, can then be disentangled from the whole organism and assigned to a specific molecule, or, increasingly, a single gene (381). This is "spurious localization," according to Rose, leading researchers to misplace the causes of dynamic, inter-individual behaviors in "isolated and localized lumps of biology," which can furthermore be separated from the total environment in which they occur (381).

Rose's openly critical assessment of the neurogenetics field does not take up, however, how the "bandwagon" of a specific research protocol (here, experimentation on mice), to use Fujimura's terminology ("Molecular"), is contributing to this reductive sequence. Rose identifies the problematic discursive maneuvers employed by researchers working in the neurogenetics field, but not how the various material and social forces constraining experimental choices may be encouraging these discursive maneuvers. He does not take up, for example, how laboratory practices, the nature of the data, and argument choices might be constrained by available experimental resources and the inherent material capabilities of the recipient object, and what important rhetorical work such steps as reification and agglomeration may be doing. Rose's analysis tends to treat the discourse of the field as somewhat divorced from its social and material context. Although Rose mentions the highly selective nature of data sampling, where some animal interactions count as data but others do not (381), he does not examine how the limited choice of experimental system, dictated by doability, affects everything that follows.

If the word "reification" means to make something abstract "more concrete or real" (OED) by naming it, to make it into a thing, then my action-attention-language model implicitly acknowledges that dynamic situations experienced in an experimental context (physical actions on objects) will be cut up into discrete things by being assigned specific words. This process of reification is especially problematic in the case of the mouse-object, however. In Brunner et al.'s *American Journal of Human Genetics* paper, dynamic, contextual interactions as varied as arson, rape, exhibitionism, and physical assault are all grouped as examples of the same one thing—aggression. In Cases et al.'s *Science* paper, the time it takes a mouse in its home cage to attack an intruder under different experimental circumstances, bite marks, fighting among cage mates, and attempts to "grasp" a nonreceptive female are all similarly grouped as instances of aggression. The ability to linguistically represent the situation as something other than what is directly being witnessed is what makes an animal model of aggression possible at all. Rose does not analyze how reification may be compensating for the material limitations of the mouse body. Without the capacity of conceptually abstracting a re-identifiable property such as aggression from different, dynamic, interactive systems, there would no be data, and certainly no way of making sense of *what* it is that is being observed when a female mouse squeaks 113 times.

Without the capacity of making a dynamic interaction between mice into a thing, no experiment would ever be comparable to any other; the very concept of modeling in experimentation implies this kind of linguistic abstraction from a dynamic material situation—a mismatch between agents' actions in a research scene and the words used to describe the event. The problem becomes a phenomenological one grounded in materiality when a researcher must determine how to make this specific mouse-apparatus *embody* the word aggression (or anxiety, depression, phobia, etc.). What can the mouse (or fruit fly, or lobster) do that can be called "aggression"? How can this behavior be elicited in the lab? What aspects of the situated event can researchers consistently quantify? How can situated actions on this research object be framed to say something meaningful about human aggression?

In phenomenology, the dilemma around materiality and language surfaces as "the reidentification criterion," which states that "for a subject to possess a concept of an object or property 'x,' the subject must be able consistently to re-identify a given object or property as falling under that concept" (Dreyfus, "Samuel" 402). On no two experimental occasions will the agent's sequence of practical coping, and the embodied response of the mouse-object, be exactly the same. Researchers must be able, in the midst of coping with a live mouse, to consistently re-identify the phenomenon of interest; otherwise the dynamic situation becomes uninterpretable in its situational uniqueness. Linguistic abstraction thus disambiguates a dynamic experimental scene; it shows where to direct attention and what to call what one is observing.

Prior papers dealing with the mouse-object will therefore show not only how to practically cope with the object, but also how to disambiguate a complex, dynamic experimental situation with language. Recall that the resident-intruder scenario was specifically developed to test the reified concept of "aggression." The language component of the experimental sequence is thus often built into the practicality of the experiment. Language does the important work of focusing attention during the dynamic interaction; it allows the agent to focus on some features of the situation and to ignore others. The reification and agglomeration of data might also not be as "arbitrary" as Rose implies; although a wide range of interactions, both between mice and between humans, can be reified as aggression, not *every* interaction will conceptually count as such. A word such as "aggression" is specific enough to constrain attention during an experiment to certain kinds of behavior, yet also flexible enough to accommodate imaginative roaming. This will be developed further when I discuss prototype language theory.

What linguistic abstraction also means, though, is that suddenly slightly different experimental outcomes—thirty seconds quicker to bite, two bruised cage mates instead of three, five extra episodes of grasping, ten squeaks instead of twenty from a nonreceptive female—can still be identified as the same one thing, aggression: one word for different physical events. If another research group attempts to repeat Cases et al.'s findings, their data will be situationally idiosyncratic, but then nonetheless identified as instances of aggression, as the same kind of experiment, comparable across different laboratories. Thus, for example, in more recent research on aggression using the MAOA KO mice, carried out by Galina Borisnovna Vishnivetskaya et al. at the Russian Academy of Sciences, the categories of behavior measured include new behaviors such as "sniffing the partner's head (nosing), sniffing the body, genitals and tail (sniffing and following), grooming and exploratory rearing (when the mouse stood on its hind legs and sniffed the air or walls)" (2). Aggression more specifically in this lab is measured as the accumulated time spent fighting after the onset of attack and the number of attacks within a two-minute period. If the mice do not attack the intruder within ten minutes, they are "considered to be non-aggressive" (2). Once again, we see how researchers working with mice observe, and attempt to measure, the kinds of embodied behaviors that mice are capable of, and then compare and liken them to the similarly constrained observations made by other research groups using the same vocabulary.

It is not such a large leap from re-identifying different experimental trials carried out by different laboratories as the same one thing, to what Rose calls "arbitrary agglomeration" (380). Thus, we find studies of "aggressive" fruit flies (Edwards et al.; Kravitz and Huber), crustaceans (Huber et al.), bees (Bernasconi et al.), ants (Roulston et al.), spiders (Clark et al.), crickets (Murakami and Itoh), and just about any other kind of animal whose behavior can be stereotyped, characterized, and evoked in a laboratory situation. Fruit fly aggression, for example, is measured—according to the material constraints posed by fruit fly bodies—as instances of "wing threat," where the wings are raised; "fencing," when the flies "push off with one of their legs in a sideward or forward direction"; "boxing," in which the opponents "rise on their hind legs and thrash at each other with their forelegs"[24]; and "chase," in which the winner of an encounter pursues the loser until the latter moves away from the fought-over food (Kravitz and Huber 738; see also Edwards et al. 1393). The results are then compared to those obtained using other kinds of animal recipient objects, even though the practical constraints posed by species-specific embodiment mean different sequences of skillful coping for researchers.

The language component therefore appears highly flexible, with words such as "aggression" able to float between different action-attention sequences. I propose to call terms that can move between different material systems this way "pleotropic," and the phenomenon "discursive pleotropism." The term comes from genetics, where "pleiotropic" refers to a gene which affects several outward traits (OED). I use "discursive pleotropism" to refer specifically to the phenomenon of one word or term being used to mean the same thing in different material systems in scientific experimentation. The various animal and insect interactions thus become conflated, through discursive pleotropism, into the more general phenomenon—aggression—being studied; the word "aggression" is able to float between

[24] Perhaps not surprisingly, there is a YouTube video, titled "Aggression in Drosophila (fruit flies)," that documents all of these behaviors. It can be found at: http://www.youtube.com/watch?v=uJaDoTigvEI (accessed August 23, 2010). Even from this brief video, one gets a sense of the practical difficulties involved in translating this dynamic fruit fly encounter into scientific data, into discrete, quantifiable units of behavior.

different material situations and retain relatively stable meaning.[25] Without such discursive pleotropism, animals would perhaps be meaningless as objects of knowledge-making in research. Through discursive pleotropism, researchers working with different species are able to participate in a conversation supposedly about a common object of interest, such as aggression; agents' actions can be varied, but the words they use to describe experimental scenes remain stable.

Once a generalized term such as "aggression" is applied to a dynamic action-attention sequence, creature-specific embodied constraints can be erased, which is perhaps the point. The various animal and insect bodies used to do the research become conflated through specific terms into a kind of amorphous, shape-shifting, experimental body that loses all traces of species-specific, embodied idiosyncrasy. The immediate laboratory experience, and certain portions of experimental prose (most notably the methods section), may relate to specific animals or insects (giving the impression, as discussed in the first chapter, that these events happened to a specific agent, at a specific time and place), but the real work being done is actually on all bodies everywhere as *represented* by these model bodies. This idea that the research is about something other than the immediate mouse or insect situation being witnessed is exemplified by neurobiologists Edward Kravitz and Robert Huber when they state the following in their experimental paper on fruit fly aggression:

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[25] In some ways, discursive pleotropism shares features with Susan Leigh Star and James R. Griesemer's "boundary objects," which "inhabit several intersecting social worlds . . . *and* satisfy the informational requirements of each of them" (393). Star and Griesemer state that boundary objects "have different meanings in different social worlds" (393). Words that become pleotropic, however, have a relatively stable meaning in different experimental worlds; the stable meaning appears to transcend situational, material specificity. Star and Griesemer also state that in collective work researchers often have the sense that they are addressing an object that means different things for those using it (412). The way that the term "aggression" appears to be functioning in the communal scientific conversation, however, gives the sense that the word means the *same thing* for those using it, even though on the material level of situated experimentation the term will mean very different things (like mice barbering one another, versus fruit flies "boxing").

> [t]he presence of highly structured, easily evoked behavioral systems offer unique opportunities to quantify the aggressive state of individuals, to explore the mechanisms underlying the formation and maintenance of dominance relationships, to investigate the dynamic properties of hierarchy formation, and to explore the significance of neural, neurochemical and genetic mechanisms in these behavioral phenomena. (736)

Hence fruit flies become "individuals"; fruit fly bodies become "behavioral systems"; interactions between fruit flies become "dominance relationships" and "behavioral phenomena"; and dynamic, embodied interactions between flies become significant only in terms of the general, non-species specific categories of "neural, neurochemical and genetic mechanisms." It is these more general terms that become pleotropic and move between different bodies and between different scientific studies, linking all of the various animal and insect bodies into something singular, highly malleable, still embodied, but only vaguely so.

Terms can therefore remain stable while the material conditions surrounding their use do not. Knorr-Cetina's general observation that a stable name does not imply "stable thinghood" ("Objectual" 193) becomes especially relevant in the instance of animal experimentation. Writing about the differences between everyday material objects and "epistemic objects" used in knowledge-making practices such as science, Knorr-Cetina proposes that naming "is a way to punctuate the flux, to bracket and ignore differences" (193). "Epistemic objects," according to Knorr-Cetina, are continually in the process of "unfolding," of evading stable thinghood, and naming is "a way of translating between different . . . institutional time zones" (193). Knorr-Cetina cites computer programs as an example of epistemic objects constantly in the process of unfolding but able to retain a stable name (193). In neurobehavioral genetics, stable terms for varied experimental situations and objects also become a way of translating across different *material* “time zones,” I want to stress—a way for different research groups working with an array of animal bodies to partake in a wider conversation supposedly about the same thing.

Two other ways for the various communities of practitioners clustered around different animal-objects to trade in generalities is to speak in the language of numbers, rather than the language of specific bodies, and to speak in the language of molecules, which also take on the ability to move around between different bodies in experimental discourse. Language can overcome the constraints of animal embodiment when the focus of attention shifts to statistics and to molecules isolated out of whole bodies.

Quantifying Embodiment

The quantification of dynamic animal and insect behaviors is an explicitly stated goal of many research studies. Kravitz and Huber, for example, declare that stereotyped, easily evoked behaviors “offer unique opportunities to quantify the aggressive state of individuals” (736). Alexis C. Edwards et al. tellingly name their paper on fruit flies “Quantitative Genomics of Aggressive Behavior in *Drosophila melanogaster*,” and announce that they have “developed an assay to rapidly quantify aggressive behavior” in their experimental system (1386). Bernasconi et al. propose that their study of staged fighting between queen bee pairs “provides quantitative evidence” of the role of “spraying” in aggressive interactions (21). Likewise, Cases et al. quantify, perform statistical analysis, and graph the number of wounded cagemates, the time it takes the mice to “right” themselves if they are placed on their backs, and the time it takes the mice to attack another mouse in various staged scenarios. In Vishnivetskaya et al.'s more recent paper on MAOA KO mice, a data table shows that the mice exhibited 1.4 ± 0.5 instances of "grooming," which lasted for 2.6 ± 0.9 seconds, as well as 9.0 ± 2.2 instances of "sniffing and following," which lasted on average 24.1 ± 6.7 seconds (3).

Rose would likely state that such data demonstrate "improper quantification." Improper quantification allows for the assignment of a numerical value to reified and agglomerated characteristics—thus 12 ± 2 grasping events, 113 ± 20 squeaks, and the time from latency until the first sign of aggressive mouse behavior. "Place an unfamiliar mouse into a cage occupied by a rat," Rose explains, "and often the rat will eventually kill the mouse. The time taken for the rat to perform this act is taken as a surrogate for the rat's aggression" (381). Rose further suggests that such quantification is "arbitrary" (381), meaning that it did not have to be these behaviors that become quantified as data; there is nothing inherently obvious about "grasping events" as a unit of measurement for aggression. The important point that Rose's criticism hints at, which I develop further, is that data from animal bodies are constructed through a process of omission; some aspects of the immediate material situation are noticed, while others are not; some are reified and used in communal discourse and knowledge-making, while others are not.

Once again, however, I want to examine what important persuasive work quantification may be doing from the perspective of experimentation constrained by animal embodiment. Once the experimental system (such as the mouse) has been chosen, there are only certain, easily recognizable, behaviors that will qualify as instances of aggression, and, furthermore, only a subset of those behaviors will be quantifiable. As Longino points out, "[f]or animal models there are standardized procedures or protocols of observation, and, with few exceptions, the behaviors under study are stereotyped and easily identifiable, such as biting or nipping at the flank of another male" (694). Quantification seems less "arbitrary" when one takes the constraints of object embodiment into account. The problem, yet again, becomes primarily a practical one: what aspects of the mouse body *can* be turned into data? What features of the interaction can be quantified so that the mouse-apparatus becomes a reliably *transportable* experimental apparatus, where others can follow the same action-attention-language sequence to arrive at the same phenomena, which can then be labeled as instances of the same one thing?

If mice are territorial and will eventually attack an intruder—a constraint of the mouse apparatus—then the most obvious parameter that researchers studying aggression can quantify in a dynamic interaction is the time that it takes a mouse to do this. But is this interaction between instinctively territorial mice "aggression" in the way that a man stabbing another in the chest with a pitchfork is aggression? When researchers must work with mice because they are the most doable research apparatus available, then yes, in their discourse this becomes the same type of aggression. Discursive pleotropism becomes necessary when mice, fruit flies, and lobsters are being made to speak for human biology and social behavior.

Numbers become another means of translating across material time zones, of standardizing action-attention-language sequences among different research groups. Theodore M. Porter, a historian who specializes in the history of quantification in research, proposes that the form of knowledge that results from "rigid quantitative protocol[s]" is predominantly "public in character" ("Quantification" 640). Rigid, numerically-based knowledge is especially useful, Porter states, when the actors are separated from one another in space and time; numbers and ways of manipulating them become primarily "strategies of communication," a way of coordinating activities at a distance (*Trust* viii). Quantification also appeals to a "mechanical" form of objectivity, Porter explains, implying "personal restraint," and rules that put a check on subjectivity (*Trust* 4-7). In the unpredictable world of animal experimentation, aiming for common numbers is thus like making sure that one person's red is the same as another person's red. As Porter states, "Experimental truth claims depend above all on the ability of researchers in other laboratories to produce results sufficiently similar, and to be convinced that the similarity is indeed sufficient" (*Trust* 12).

Once again, the challenge of re-identification in scientific experimentation means finding a means of ensuring that others cope with an object in a way that is "similar to the way I have coped with it on other occasions," as Dreyfus states (402). Todes similarly remarks that if our experience of the world is bodily-mediated, then how do we know that our knowledge of objects is the same as someone else's (2)? Numbers "lend credibility to forms of belief and action when personal trust is in short supply" Porter proposes ("Quantification" 640). Numbers in research act like geographic coordinates, offering something more tangible for comparison across subjective laboratory experience; a community held together by a common animal body must set limits on how it is to be used, and numbers help. The conversation can then revolve around numbers and statistics rather than specific agent-object interactions. Common numbers can also erase the idiosyncratic material differences of research situations. Numbers also imply the standardization of embodied experience; the kilometer, the gram, the second, and the liter imposed equally on all bodies everywhere.

Thus, like Fujimura's idea of transportable apparatus-theory packages ("Constructing"), numbers also become transportable action-meaning units, common denominators that anchor different labs to similar phenomenological experience. Like the word "aggression," numbers similarly constrain action and attention in an experimental situation. When one lab reports that its female mice squeaked an average of 113 times, it sets the stage for the sequence of skillful coping necessary to elicit the same behavior from a different set of mice in a different laboratory. Numbers can help standardize skillful coping because they imply proper procedure. If the number ends up wildly different than expected, it implies that something went wrong with how the mouse body was handled. Conversely, if a researcher ends up with a number that is similar to what others have found, there is the sense, perhaps false, that a coordination of embodied coping was achieved. Porter notes that a scientific claim "has little standing until it becomes a collective product" (*Trust* 12). The achievement of communal numerical data therefore lends further trust in the accuracy—versus just the precision—of the data; the more times that a number is achieved through a given coping sequence, the more true its interpretation will seem.

As a kind of literary technology, however, numbers achieve their communicability in animal experimentation through the same symbolic channels as words. Numbers alone, even if highly consistent among many different actors in different places, do not explain their own significance. The reason we accept that a 163 second difference in the time it takes one strain of mice to attack another is a measure of aggression (note that eventually both the MAOA KO *and* the strains labeled docile attack the intruder) is that conceptually "out of proportion to the provocation" means faster and more by definition. The conclusion that the mutant mice are more aggressive comes from being able to contemplate in an abstract way this one quality of the situation. The time from latency to attack can be redefined as something other than what is directly being witnessed, as the more complex concept called "aggression." Aggression is understood as an excess of response. Aggression here is also a function of comparison. Even if the MAOA KO mice attacked *only two minutes faster* than the docile control mice, that is still twice as fast, therefore twice as much, therefore *more than*. It is our symbolic understanding of "more than" that allows two minutes to make all the difference between aggressive and non-aggressive. It is our language system that does much of the work of making one group of mice "aggressive" and the other "docile." The language is still tied to something in the material world, and some mice do attack faster than others, but the interpretation that one group of mice is more "aggressive" than another is far from inevitable, even under these material circumstances. The immediate laboratory scene is always mute about its own significance; researchers make the situation meaningful through language.

From Bodies to Molecules

Another way for researchers separated in space and time to trade in generalities, and be able to claim commonality in their benchtop practices, is to move their focus from whole bodies to molecules, although Rose simply calls this turn "spurious localization." Rose proposes that once dynamic interactions have been recast as stable things, as stable features of bodies, somehow residing *in* bodies, these things can become divorced from whole bodies altogether; a stable thing such as aggression can then be attributed to isolated molecules as well (381). Suddenly, aggression has a physical location within the body, a specific gene. The "mythopoeic genome," as Rose calls it, now becomes the ultimate cause of behavior along "a linear view of living processes" (382). This tendency to seek a single causative agent for complex social phenomena both reflects and endorses a way of thinking that Rose charges is highly deterministic (381). This “neurogenetic determinism,” he argues, is an inevitable consequence of the reification and agglomeration of dynamic interactions; “for if there is one single thing called alcoholism,” Rose states, “then it becomes appropriate to seek a single causative agent; complexity is hard to deal with within the neurogenetic agenda” (381).

In terms of the practical constraints of experimentation, however, there is something reassuringly static and constant about molecules, as opposed to dynamic interactions. The quantification of molecules in particular can seem a truer measure of the state of things because data appear less dependent on the subjective judgment of the observer/agent and on problematic animal bodies. Invariably, this new set of numbers, generated through molecular biology techniques, comes from highly standardized and increasingly automated assays that reduce tacit embodied coping as much as possible (as I discuss in the third chapter), thereby making the data seem more certain because specific agents are rendered interchangeable through machines.

All of the studies on the MAOA KO mice done after Cases et al.’s paper point towards a molecular cause (common, of course, to both mice and men) for the behavioral data, most commonly the MAO A gene. Interestingly, but perhaps not surprisingly, papers citing Brunner et al.’s original, hedged claim (that the locus for this aggression disorder “could be” linked to the MAO A gene) often remove the hedging and make stronger claims such as “due to,” as witnessed in Kim’s paper. Communications scholar Kelly Horn observes that researchers must consider carefully what to do with the hedges in a work they wish to cite (1086): do they keep the more cautious form of the claims, or do they upgrade the claims to make them appear more certain? Claims that seem more certain also tend to steer the reader away from the immediate material situations from which the data came

(Latour; Gieryn). If researchers paraphrase the original, hedged, statement, they might be able to change "the degree of certainty of the statement," as Horn notes (1088). In a fascinating conceptual turn, then, when the hedges are removed from the Brunner citation, a sequence of nucleotides also comes to represent the thing it supposedly causes. The gene itself becomes "mean," as the title of René Hen's *Neuron* paper, "Mean Genes," proposes, or "selfish," as the title of Richard Dawkins famous 1976 book on evolution *The Selfish Gene* states, and behavioral responses are portrayed as abstractly embodied in genes, as Rose charges (382).

This shift from reified interactions to reified lumps of biology now requires a new action-attention-language sequence. The action component shifts from problematic observations of live creatures to assays on isolated molecules extracted from the now-dead mice. Molecular biology is easier to standardize among many researchers separated from one another in space and time (Fujimura "Constructing"), which affects the discourse around mouse bodies. Cases et al., for example, remove and isolate brain and liver cells for chemical reaction analysis. "Using serotonin as a substrate," the authors report, "we found that MAOA activity was abolished in the brain and liver" (1764). A table of the data is titled "MAOA activity in the brain and liver" where "each value corresponds to one mouse" in units of "nanomoles per 20 min per milligram of protein" (1764). Shih et al. show data featuring the "[l]evels of monoamine oxidase A and B in rodent brain measured by immunohistochemistry, enzyme autoradiography, and in situ hybridization" (199). Kim et al. describe a procedure where the frontal cortex, hippocampus, and cerebellum are "rapidly dissected" from the brains of research mice, frozen in liquid nitrogen, homogenized, sonicated, centrifuged, and, finally, "divided into aliquots for HPLC analysis" to determine protein concentrations (5930).

Researchers are thus dealing with completely different recipient objects in experiments on isolated cells, molecules, genes, and fragments of genetic material from those constituted by behavioral studies. Accordingly, the nature of agenthood must change as well in response, if we believe Pickering's maxim that agents and objects take on a particular shape in relation to the other ("Practice" 163). Importantly, when the object becomes an isolated bit of biology, the data appear more objective, in one sense, because they are less dependent on the idiosyncratic judgment of the agent. Observing and characterizing interactions between live mice creates a different sort of subject than running a Southern blot (a type of gel electrophoresis) or a polymerase chain reaction assay, which have become highly automated and black-boxed[26] procedures. Comparison of these types of data is easier, especially if everyone obtains their reagents and machines from the same commercial supplier. Molecular data give a stronger impression of "aperspectival objectivity," to use Daston's terminology, than behavioral data: anyone could have run the Southern blot or pressed a button on the PCR machine.

The mouse body thus closes as an epistemic object to a greater extent and becomes a black box when attention shifts from whole, moving, reacting mice to the buttons on an automated machine; the mouse body becomes more certain as it loses the ability to react in unexpected ways. The mouse-object data must still, however, speak back to complex social

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[26] Although the term "black box" has so permeated the STS lexicon that it is now often used without attribution, it was originally used by Latour in *Science in Action* to describe something whose inner workings no longer need to be understood (and often are not due to layers of packaging and complexity) in order to use it. I do not need to understand computer code or electrical mechanics to type these words. As Sismondo sums up, objects or procedures can become "taken for granted as completed projects, not as messy constellations" (*Introduction* 85); they become "predictable input-output" devices (120).

interactions, which once again forces the language of the experimental report to do extra work to make the material world fit the discourse of aggression.

The problem with studying single molecules, or single genes more specifically, in order to explain dynamic behavior, according to Rose, is that "social phenomena are of their essence historically contingent and framed by meanings that the reductive process loses as surely as the information content of a page of *Nature* is absent from a chemical study of the paper and ink that comprise it" (380). This dependence of social phenomena on the context in which they occur is even evident with mice: the resident-intruder test takes advantage of the fact that mice, like most animals, are territorial—the total behavior being reified is dependent on breeding and housing strategies and then the context of the encounter (mice housed together from weaning typically do not attack one another).

If current molecular biology technology is able to selectively "knock out" one gene and still produce a viable embryo, then any observable, quantifiable alteration in behavior will be linked to that one gene, and then that gene will be framed as the causative agent. As behavioral geneticist Boris Tabakoff observes, Cases et al.'s findings are "intriguing" scientifically because one has "a specific genetic mutation that inactivates a single enzyme involved in monoamine neurotransmitter metabolism," and then "one is confronted with a series of behavioral manifestations in animals carrying this mutation" (196). Clearly, then, it is challenging for neurobiologists to move beyond Beadle and Tatum's one gene-one enzyme hypothesis, where one gene is responsible for one molecule; or, in this instance, one gene is responsible for one behavior.

Since both mice and men have this one gene in common, it becomes the fixed biological entity that explains the many different kinds of interactions as the same one thing, traceable to the "same" gene. Genes thus also become pleotropic, able to move between different material contexts and retain discursive "stable thinghood," as Knorr-Cetina would say ("Objectual"); the MAOA gene is treated as the same thing, regardless of whether it came from a mouse or human body. If researchers did not have the means of genetically altering mice this way, of knocking out select genes from animal bodies, then perhaps biology would be less genetically deterministic in its language. Genetic knockout technology encourages the question of how much ways of doing science affect the discourse that results.

The problem with ascribing aggression to a single gene is that we "mistake correlation or even consequence for cause," according to Rose, and I agree (381). It is highly plausible, Rose explains, that during certain kinds of encounters, both people and animals show physiological and molecular changes, and that a lifetime of such changes may show lasting changes in genetic expression (381). Changes in genetic expression, Rose states, do not necessarily point to a genetic cause; at best, such changes show correlation. Once again, however, I want to raise the possibility that doability, in terms of the technologies available to researchers, among other factors (such as the complex infrastructure of funding, which often necessitates strong claims about causation to be made), may encourage such reductive discursive maneuvers that turn correlation into causation.

The research dilemma faced by neurobehavioral geneticists is similar to Heisenberg's uncertainty principle in physics, which states that one cannot know precisely both the position and momentum of a particle at the same time. The more one narrows in on one parameter, the less certain the other becomes. Narrowing in on the precise biological properties of the least reducible units of an organism, such as genes, necessitates moving attention away from dynamic situations. It is (currently) impossible to know the molecular details of an interaction in *the midst of the interaction*. What happens then in terms of data analysis, is that researchers must conduct many separate experiments, each one focused on a different aspect of the system. It is only through narrative that they can weave the data together into a tale of momentum, of dynamic mouse or human interaction. Scientific knowledge generated using animal models is therefore critically dependent on the narrative process to make these research objects say important things about human biology. Narrative becomes especially important when animal data is compared with the original human data.

From Men to Mice and Back Again

At some point, researchers working with animal models must take the agglomerated animal data and compare them to the agglomerated human data. The comparison necessitates corralling mouse and human data as examples of the same one thing. Comparing data across species is not a practical laboratory exercise; it is not a matter of *doing* more experiments to prove the agreement, such as putting a mouse into a resident human's space and measuring the time it takes the human to attack the mouse, by whatever means; it is a persuasive exercise that requires narrative. What makes the agglomeration of animal and human data possible is therefore more about what language can do than any inherent experimental logic. What happens, then, when animal data are brought into a larger scientific conversation that includes human studies—where 113 squeaks, and the sequence of skillful coping it embodies, cannot easily (if at all) be transplanted between experimental contexts?

One way in which agreement between animal and human studies is conceptually constructed is through direct reference back to Brunner et al.'s original paper linking MAOA to aggressive behavior in human males. In their 1996 *Neuron* paper, for example, Cases et al. state that their findings “could be relevant to several human states in which the MAO genes were shown to be altered,” and that “[c]loser to the deficiency found in [MAOA KO] mutants, Brunner et al. (1993) described a point mutation on the MAOA gene that is associated with mild mental retardation and increased aggressiveness in male patients” (304). Cases et al. then propose that “the mechanisms implicated . . . could be similar” and that similar biological mechanisms cause the aggressive behavior in both mice and men (304). Likewise, Vishnivetskaya et al. conclude that the aggressiveness found in the mutant mice is “consistent with abnormal behavior in men of the Dutch family with heritable lack of MAOA, showing disproportionate to the provocation impulsive aggression” (5). Kim et al. repeat this phrasing almost word for word when they also state that “[t]he enhanced aggressive behavior exhibited by the MAO mutant mice is consistent with the impulsive aggression reported in men from a Dutch family with a complete MAOA deficiency” (5929). Referring back to both Cases and Brunner’s papers, Shih et al. state that MAOA KO mice show “a distinct behavioral syndrome, including enhanced aggression . . . manifested by adult males (Cases et al 1995),” and that this “is consistent with the abnormal aggression reported in males from a Dutch family with a complete MAOA deficiency” (210). Similarly, René Hen et al., referring to the “enhanced intermale aggressive behavior [and] increased attempts to mount nonreceptive females” seen in the mutant mice, also reiterate that “[t]he enhanced aggressive behavior displayed by mice lacking MAOA is consistent with the violent behavior displayed by the few men lacking this enzyme” (18).

The uniformity in the way that later animal studies using the MAOA KO mice refer back to Brunner et al.’s paper implies that the phrasing “consistent with” is doing important persuasive work. The case of the MAOA KO mice demonstrates the power of “discursive momentum," as English Studies scholar Carol Reeves would say, to influence communal agreement in science. Writing about how Stanley B. Prusiner generated popular momentum for his new prion lexicon, Reeves proposes that repetitive usage of a term, declarative statements, and appeals to external consistency all helped to popularize Prusiner's language and way of thinking. Remarking that "[i]ncreasing usage helped promote universal usage and gradual agreement," ("An Orthodox" 113), Reeves compares the way that a lexicon can come to dominate a community to the spread of an infectious disease (114). We can see the infectiousness of language, and its power in constructing knowledge, in statements that link MAOA to aggression. One of the linguistic strategies that Prusiner employed, according to Reeves, was to use terminology "as if it referred to an entity whose characterization had already been ratified and documented by collective experience" (107). The "declarative passive" statement in particular, Reeves states, allowed authors to promote the new lexicon by making it seem as though material reality itself came up with it (107). Statements that the behavior of MAOA KO mice is "consistent with" Brunner et al.'s paper do the same thing: they make it seem as though the data themselves are aligning with Brunner et al.'s paper, obscuring the fact that human actors must actively construct this impression of consistency between mice and men. Statements seemingly about material reality, Reeves posits, thus become tools "in the materialization of an idea" (108), and we can see a similar "materialization" happening in the case of MAOA and "aggression."

Jumping from mice to men, however, requires additional discursive work in order to transcend the embodied constraints of the experimental system and make the data relevant across varied material situations. Discursive pleotropism becomes essential here. I thus return to the question of what it is about language that enables the application of a word like "aggression" towards so many different kinds of interactions in different species.

In "Human Categorization" cognitive psychologist Eleanor Rosch addresses how an ever-changing context becomes classified into seemingly stable concepts. Rosch proposes that we choose a word for a segment of our context by comparison to a typical instance of that category. This is known as Rosch's "prototype theory." The notion of a typical instance means that even instances that deviate from this yardstick (which itself is not fixed) still count as examples of the prototype. In a study of the logic behind fuzzy linguistic concepts, Lakoff summarizes the import of Rosch's prototype theory by stating that category membership becomes an issue of degree (458). Thus, for example, Lakoff states that birds become ranked by their degree of "birdiness," rather than simply as birds or nonbirds, and vegetables are ranked by their degree of "vegetableness" (robins turn out to be more bird-like than bats and cows not at all; carrots are more vegetable-like than pickles) (459). This means that utterances will often be neither entirely true or false, "but rather true to a certain extent and false to a certain extent, true in certain respects and false in other respects," Lakoff notes (458). Linguist Guy Cook observers, in a mobilization of Rosch's theory to analyze the language of advertising, that the less like a prototypical instance something is, the less likely we are to put it in that category (13). Importantly, Cook notes that "we are still willing to tolerate fuzzy, borderline, and dubious cases" (13). This ability to tolerate fuzzy cases of categorization becomes obviously important in research involving mice to model human social behavior, where many different kinds of mouse interactions are still classified as aggression.

The linguistic categories of aggression, anxiety, and depression most typically investigated by neurobehavioral researchers also lend themselves particularly well to tolerance of fuzziness. Rosch et al. would probably suggest that these categories have a higher degree of inclusivity and thus can accommodate a higher level of abstraction ("Basic Objects" 383). Inclusivity within a category, I want to stress, is also not endlessly fuzzy; at some point, an instance clearly does not qualify. Perhaps, then, reification and agglomeration of instances of aggression are not exactly arbitrary, as Rose charges, but rather minimally determinate. Categorizing stimuli, Rosch et al. propose, entails making them "not only equivalent to other stimuli in the same category but also different from stimuli not in that category" ("Basic Objects" 383). Mice biting an intruder is thus more like our prototype of aggression than mice merely sniffing or grooming one another; mice biting one another is more like a man stabbing someone in the chest with a pitchfork than playing a game of chess.

Such nuances of category assignment are often quickly lost in scientific discourse, however, when the discussion needs to move back and forth between animal and human data. Lakoff further suggests that once distinctions have been made (that, for example, "[a] robin is more of a bird than anything else," and that "a chicken is more of a bird than anything else") we tend to collapse the cases into the prototype and cease to distinguish differences in degrees of belonging (461). That is, speakers turn relative judgments into absolute ones by assigning something to the category in which it has the highest degree of membership (461). We can see this kind of conflation in the experimental papers that use animals or insects to model aggression: mice nipping at one another's flanks is most like aggression; fruit flies thrashing at one another with their forelegs is most like aggression. The discourse around animal models can overcome the limitations imposed by the material world. Where animal bodies come up short, in terms of their embodied capabilities and relevance to what is being investigated, language compensates by forcing relevance.

Embodiment is critical in the application of a given prototype category towards an instance in question, however. The instance in question is judged to be like, or unlike, a prototypical instance of something based on criteria inherent to the material constraints of the object involved. That is, some types of instances would never occur in a given material situation. Thus, we would never ask, for example, whether a mouse forcing another mouse to undress at knifepoint is an instance of aggression; or whether two men licking one another's fur is an instance of non-aggressive behavior. The borders of categories can be fuzzy, and allow for discursive pleotropism, but materiality still limits the scope of possible abstractions.

Interestingly, once we decide that varied instances fit a prototype, distinctions related to embodiment seem to vanish. The nature of the object shifts once again. This time, the object becomes an animal-human hybrid creature of experimentation, partly embodied, partly theoretical. Scientific knowledge generated from animal models thus comes together in acts of discourse (as when the words "consistent with" link animal and human data), rather than strictly through the act of experimentation, where the nature of the data is often unclear and amenable to multiple interpretations and categorizations.

Occasionally, the fact that language does important epistemic work in animal research becomes explicit in the scientific literature, often through controversial findings that throw into question the strength of connections between what was done on a physical, lab benchtop level and what happens on the level of language in scientific prose.

Questioning the Boundaries of the Object

Normally, the fact that language is doing important work in making animal data relevant to the human situation is obscured by the momentum of research, by collective agreement about the nature of the problem, the models that should be used, how agents should act on these models in experimental scenes, and allowable interpretive strategies. Few challenge these interpretive strategies, at least not publicly, when everyone is similarly busy working out the details of the same story. Debates over interpretation and the physical-linguistic boundaries of the object, however, enter the forefront of a scientific discipline when there is disagreement, which, in animal experimentation, often concerns the nature of the experimental variables, or controls, and what counts as data. For neurobehavioral genetics, disagreement entered the public discursive arena when John C. Crabbe, Douglas Wahlsten, and Bruce C. Dudek tested the predictability (or, rather, the predicted genetic correlation) of six mouse behaviors simultaneously in three different laboratories (Albany, New York; Edmonton, Alberta; and Portland, Oregon). What they found sent shock waves through the field. The debate that ensued elucidates how experimental systems involving animal bodies are normally carefully controlled through a process of communal omissions, allowing for some material details of the experimental situation to enter attention and the communal conversation, but not others.

Crabbe et al. introduce their 1999 *Science* paper by acknowledging that genetically altered mice are "valuable tools in biomedical research," particularly in neurobehavioral studies (1670). Although the authors accept that genetic background affects physiology and behavior, they also admit that little is known about "the effects of environmental background" on mouse behaviors (1670). In other words, little is known about the influence of other elements found in the total experimental scene which are largely excluded from the final experimental narrative. The mice in Crabbe et al.'s study were shipped from the same supplier, at the same age (77 days), and experimentation was started at the same time (0830 to 0900 hours local time on 20 April 1998) in all three labs (1670). The aim was to test for standardization in six common behavioral tests: open field, elevated plus maze, rotating rod, swim to visible platform, locomotor activation after cocaine injection, and alcohol preference (1670). Despite these efforts to "equate laboratory environments," the authors nonetheless discovered "significant and, in some cases, *large effects of site*" on mouse behavioral outcomes (1670, emphasis added).

In other words, when three different laboratories conducted the same set of behavioral tests on mice from the same supplier, beginning at the same time, on the same date, standardized more than would ever be expected in the everyday reality of research, variations in the data were substantial; the genetic background of a mouse did *not* guarantee predictable behavior, putting into question the expected causal pathways that lead from (single) genes to behavior. Commenting on these observations, molecular psychiatric researcher Marina Picciotto and biologist David Self state that Crabbe's paper "demonstrates clearly what is widely known in the neuroscience field: behavior is a complex phenomenon that is strongly affected by both genetics and environment" (2067). Importantly, Crabbe et al.'s findings put into question, publicly, the reliability of the action-attention-language sequences created by researchers, and the ability of the animal research object to remain stable at a distance and between different agents acting on them.

What I find most fascinating about Crabbe et al.'s research is that suddenly the experimenter is also implicated as one of the variables to be controlled, as an important and inextricable part of the experimental system.[27] Crabbe et al. make it a point to question the possible influence of technician handling on mouse behavior: "specific experimenters performing the testing were unique to each laboratory and could have influenced behavior of the mice" (1672). Research psychologist Larissa Pohorecky also brings up the likelihood that differences in how the mice are handled by specific experimenters might affect the data, and, furthermore, that "it is difficult to control for idiosyncratic differences in picking up and in handling rodents during behavioral testing" (2067). Caretakers are sometimes substituted, for example, and Pohorecky states that this "can introduce further unexpected, and most likely unknown, variability in the behavioral response of experimental animals" (2067). Similarly, G. R. Dawson, J. Flint, and L. S. Wilkinson also question the uncontrolled influence of the human on the animal in their *Science* commentary on Crabbe et al.'s findings, pointing to the technician who wore an Airstream helmet when handling mice (due to allergies) as an important experimental variable. "Not surprisingly," the authors state, "experimenters who have allergies handle animals in very different ways from those who do not" (2068). They also explore the possibility that the fan motors in these helmets (which emit ultrasound) "can profoundly affect rodent behavior" (2068).

Importantly, in this discussion about Airstream helmets and specific handlers, the conversation shifts once again from generalities of mouse research practices, presumably true in standardized labs everywhere, to situated material specificity. The discussion in the wake

[27] In fact, when Elissa J. Chesler et al. used a computational approach to identify and rank the factors affecting outcomes in mouse biobehavioral experiments, they found that "a factor even more important than mouse genotype was *the experimenter performing the test*" (907, emphasis added). The authors have no ready answer for why this might be the case, since the experimenter's age, sex, and level of experience failed to correlate with the data (917). The authors thus conclude that "differential animal handling, perhaps inducing different levels of stress in the subjects, is responsible for the 'experimenter effect'" (918).

of Crabbe et al.'s findings provides an example of Gieryn's observation that too much physical detail about scenes of experimentation risks converting the lab "from a truth-spot to an epistemically 'stigmatized place'," as happened when physicists Pons and Fleischmann suggested that humidity levels in their lab might account for why others could not replicate cold fusion (128). Such details throw into question the whole notion of communal practices, where much about the material context is normally taken for granted as equivalent across scenes and agents, as Gieryn proposes. "To foreground the particulars of a place of scientific inquiry," Gieryn states, "invites suspicion that the research is going on in non-standard circumstances that scientists elsewhere cannot know because it is no longer presumed to be equivalent to their own. Doubt creeps in" (128).

Although an Airstream helmet is a part of the total research scene, it is implicitly, communally recognized as the wrong kind of physical detail to include in the formal scientific conversation, as it points the interpretation back to a specific place and specific bodies. We can thus see the kinds of discursive maneuvers that normally maintain the status quo of material equivalence by deliberately excluding physical idiosyncrasies specific to place and bodies from reaching experimental prose.

In the wake of Crabbe et al.'s study, the boundaries of the experimental system expanded to include parts of the experimental scene not originally included data. Previously unimportant variables, such as the possible effects on the mice of ultrasound from a technician's helmet and even the way the mice are held, which were originally omitted from the communal conversation, become part of the system. All human action, then, potentially becomes part of the experimental system, in addition to those variables being specifically quantified, studied, and actively controlled. As Dawson et al. observe, "in unconditioned tests (elevated plus maze, open field) the controlling variables are often obscure" (2068). It is only when the research is publicly questioned that suddenly there are no limits to the physical aspects of the experimental scene that *could have* been translated into words and included in the paper; it becomes obvious how much of the total experience had to be excluded from attention and language, thus exposing the selective and, ultimately, creative nature of researchers' reports and interpretations.

The public discussion concerning the import of Crabbe et al.'s findings also reveals the power of communal action-attention-language sequences in silencing the problematic epistemic nature of some research objects. In her discussion of how epistemic objects are used in various meaning-making practices, Knorr-Cetina points out that when the object becomes problematic, its "lack of completeness of being" is revealed; it suddenly acquires "the capacity to unfold indefinitely," never attaining a final material definition ("Objectual" 181). Instead, it keeps acquiring new properties and those once thought solid change yet again; the object continually explodes, evades fixation, and "mutate[s] into something else" (182). The mouse as a research object appears to have this potential to explode indefinitely and evade fixation, constantly mutating into something else due to its embodied ability to react in new ways to an ever-changing context. Crabbe et al.'s study highlights in a very public way that the mouse can be problematic as an epistemic object, and, simultaneously, how it must be continually made less problematic through language. Paradoxically, the more that researchers attempt to control this object from exploding into new epistemic definitions, the more the constructive, selective nature of the scientific mouse narrative, whereby "x into mouse equals y out of mouse," is revealed.

Despite it being, supposedly, widely known in the field of neurobehavioral genetics that behavior is a complex phenomenon equally affected by environment as by genetics, the fallout of Crabbe et al.'s paper highlights researchers' attempts to create idealized narratives of genetic causality. In his response paper to Crabbe et al.'s findings, aptly titled "Precision in Mouse Behavior Genetics," neurobiologist Donald Pfaff acknowledges the desire for "a universal lawfulness, expressed quantitatively, in the demonstration of stimulus-response connections" (5957). Although Pfaff states that the complexity of some behavioral responses makes the attainment of a "universal lawfulness" of causal relations between genes and behaviors difficult, he nonetheless believes this is achievable and defendable in the face of Crabbe et al.'s findings. If the experimenter focuses on "well-controlled stimuli" and "biologically important responses of enough simplicity," Pfaff proposes, then "reliable" information can be gained from mouse studies (5957).

One of the unstated implications of Pfaff's statement is that studies that fail to show such clear causal relations were insufficiently controlled. Pfaff does not address the possibility that it may be impossible to fully control an experimental system with a live animal at its center. Pfaff's response to Crabbe et al.'s paper also hints at a longing for "aperspectival objectivity" (Daston), where interchangeable agents easily achieve the same results. The "universal lawfulness" that researchers are aiming for might therefore refer primarily to the stability of the action-attention-language sequences designed to hold research groups to one another through common practices on common objects.

Crabbe et al.'s research also highlights the importance of attention in research practice and discourse. In any research situation, there are limitless possibilities for where an embodied, language-using consciousness can go. The focusing of attention on one aspect of an experience at a time lets agents attend to the situation, skillfully cope with it, and make sense of it, but it also obscures what else could have been noticed.[28] By shifting attention elsewhere, an agent thereby alters the data, and so alters the discursive trajectory of the research.

The most important, and problematic, implication of Crabbe et al.'s findings, however, is that "experiments characterizing mutants may yield results that are idiosyncratic to a particular laboratory,"[29] as the authors themselves acknowledge (1670). Idiosyncratic results are especially troubling in the case of transgenic mice, Crabbe et al. note, since the mice are often in limited supply and tend to be behaviorally characterized in one laboratory using the problematic tests described earlier (1670). Thus, the authors conclude, "for behaviors with smaller genetic effects (such as those likely to characterize most effects of a gene knockout) . . . specific behavioral effects should not be uncritically attributed to genetic manipulations such as targeted gene deletions" (1672).

The danger with such uncritical attribution of behavior to genetic manipulation, and with a biomedical research program more generally that relies so heavily on animal mutants as models of human biology, is in its direct application to social policy. The applicability of the research to the human situation is often part of the expected justificatory rhetoric of animal studies. Although this chapter is an exploration of laboratory knowledge-making when the epistemic object is a mouse, and not about the social applications of the research, I

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[28] A fitting example is Daniel J. Simons and Christopher F. Chabris's study on the psychology of "inattentional blindess," wherein participants watching a video of a staged basketball game are told to count the passes made; most participants then fail to notice the woman in a gorilla costume walking through the action (1066). Simons and Chabris thus observe that "we are surprisingly unaware of the details of our environment," and that we "perceive only those objects and details that receive focused attention" (1059).

[29] Although mouse knockout models are currently the most doable technology for studying the effects of genes on behavior in a general sense, some individual knockout strains, especially when first created, may be in scarce supply (which changes when they become commercialized).

want to briefly point to some of the problematic ways in which animal research might be used. Edwards et al. begin their paper on fruit flies, for example, by stating that "[a]ggressive behavior is important for animal survival and reproduction, and excessive aggression is an enormous social and economic burden for human society" (1386). Edwards et al. then go on to say that "[u]nderstanding the genetic, neurobiological, and environmental bases of aggressive behavior is of great importance to human health and society, as this could lead to more effective treatments for the increased levels of aggression observed among patients suffering from many behavioral disorders" (1386). Again, this paper is on fruit flies, yet discursive maneuvering makes the data applicable to the human body and social sphere. Data obtained using genetically-altered animals in scarce supply, characterized by one laboratory using a questionable methodology (see Crabbe et al.), could therefore be used, due to discursive pleotropism that makes data applicable across species, to justify the marginalization of certain vulnerable human beings.

Although studies such as Brunner et al.'s have not been reproduced in humans, in the early 1990s the US National Institute of Mental Health set up a Federal Violence Initiative aimed at identifying inner city youth at risk of future criminal behavior through genotype testing (Rose 382). Widespread critical outrage at such initiatives did not prevent parts of the programme from being instituted in Chicago and other US cities (Rose 382). The fact that researchers treat seriously the localization of social behaviors in molecular causes gives legitimacy to such social programs, according to Longino (687). More explicitly, Longino worries that locating the causes of violence within the genetics of the individual is a way of diverting attention away from contributing social factors, thus weakening support for initiatives that address racism, poverty, education, and social support systems[30] (687).

The need for reflection and caution concerning how animal bodies are being used to make knowledge in science became even more urgent in 2002, when the mouse became an even more "doable" technology with the completion of the mouse genome project. Chris Gunter and Ritu Dhand, the editors of *Nature,* stated at the time that the mouse genome "holds more promise for our future *than even the human genome itself*" by promising to "give us detailed insights into many aspects of human disease as well as basic human biology"[31] (509, emphasis added). Analyses that address how animal bodies are used in research to construct scientific prose and scientific knowledge can thus potentially point towards the limitations of such research, thereby encouraging more debate about downstream social and medical applications.

In this chapter, I have applied my action-attention-language triad model towards the case of a more problematic recipient object. I have pointed at some of the discursive maneuvers which attempt to render the mouse less problematic as an epistemic object. What emerges from my analysis of the discourse emanating from one particular group of researchers clustered around the mouse-object is the extent to which researchers themselves,

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[30] Interestingly, Edwards et al. cite (but do not adequately address) a study by Avshalom Caspi et al. that found that reduced MAOA enzyme activity in human males is only associated with an increase in violent behavior if the individual has been abused as a child. "Maltreatment has lasting neurochemical correlates in human children" Caspi et al. state, highlighting the importance of environment on genetic expression (851). Commenting on these findings, Erik Stokstad calls the study "a wonderful class example of how social factors can play an enormous role in expression of behavioral traits," showing "that antisocial behavior might depend on social situations, not just genes" (752).

[31] Although I do not address the ethical issues of animal experimentation here, I do want to note that aggression towards mouse bodies is another material detail that is selectively omitted from the communal research conversation. No formal research account, for example, describes how a "retro-orbital blood sample" was obtained (a small capillary tube is inserted into the inner corner of a mouse's eye). Such material details are invariably framed in technical language that obscures what is actually done. It is perhaps the ultimate taboo in research accounts to frame practices on animal bodies in such immediate, accessible terms.

and not just STS critics, are struggling to do justice to the material world. Next, I consider what happens to the discourse of science when material engagements with research objects are simplified and reduced in attempts to make research more predictable.

CHAPTER THREE

Agents, Objects, and Scenes in Scientific Advertisements

In order to understand more about the relation of agents, the discourse they produce, and the material objects they use to make scientific claims, I turn my attention in this third chapter to the instruments themselves. I look at a genre of scientific prose not typically associated with scientific communication: advertisements for research apparatus featured in scientific journals. Ads also point to the objects and methods of scientific research, often describing specific relations between agents and recipient objects and how the work of science relates to linguistic activities. I am interested in ads because they communicate with their audience in ways different from the formal experimental literature, and thus present new opportunities in STS criticism for examining the relationship between physical and linguistic research practices. This chapter is therefore less about what the ads imply about the discourse of advertising, and more about what they imply about the ways in which science is being conducted on a material and linguistic level. In keeping with my focus on embodiment and on the benchtop, material basis for scientific prose, I will be asking what kinds of stories ads for research apparatus are telling, either explicitly or implicitly, about the relations between agents and the objects they act on in research scenes—who is doing research, with what, where, and for what reason. Unlike experimental papers, ads tend to be more direct about their depiction of agents, in terms of the nature of agents' involvement in research practices (both physical and linguistic); about the nature of the agent-recipient object relationship; about the nature of the agent's relationship to the scenes of scientific research; and about the very nature of expertise itself in relation to physical and linguistic research practices.

Despite increasing attention in STS to the social and epistemic consequences of the commercialization of research (see, for example, Sismondo "Ghosts"), a discursive analysis which concentrates specifically on ads for scientific research apparatus has not, to my knowledge, been undertaken. In her discussion of how the tools of molecular biology become standardized among different actors, Fujimura briefly notes that ads have become important in science because science has become increasingly commercialized. In many instances, she notes, the most well-designed object is not necessarily the one that becomes the most widely used; "the 'technically best' mousetrap does not get to trap the mice in buildings across the nation" (*Crafting* 114). The winning product, Fujimura goes on to state, is more often the one backed by resourceful manufacturers, suppliers, and marketers "who have good advertising" and can make the product easily available (114). Discourse critics have long been stating that in consumption-based societies advertising has become an important means of shaping our personal and social lives (Beasley and Danesi; Cook; Dyer; Williamson). Advertising, "the bluntest quest of advantage," as Burke calls this overt form of persuasion (xiv), has also become a powerful means of influencing the practices of science, of bringing agents and objects together and influencing the nature of the relationship.

Good advertising therefore also becomes important for scientific research products, Fujimura posits, when the winning apparatus must increasingly secure the commitment of commercial companies with marketing resources. We are clearly no longer talking about the experimental world of Boyle's scarce air pump, or even Smithies' rather involved version of gel electrophoresis, when the co-founder of Bethesda Research Laboratory (BRL) remarks that his company's mission is to become "the Sears and Roebuck of molecular biology" (qtd.

in Fujimura, *Crafting* 99). "We are part of the flow of information and materials . . . ," he declares. "Our mission is to supply the tools and techniques of molecular biology, wherever it may lead." If commercial companies selling research products are part of the "flow of information and materials," then ads represent an important form that this flow of information takes, influencing and reflecting how scientific work is done through their portrayals of agents, objects, and research scenes.

In this chapter, I analyze ads featured in the journal *Science* from 1956, when ads first appear in the journal, until the year 2008. I chose *Science* because it caters to a very broad audience, with diverse scientific interests, and thus features a similarly broad range of ads. I limited myself mostly to ads for gel electrophoresis, advertised over this whole time span, but have also included ads for products invented more recently, such as automated PCR (polyermase chain reaction, a method of making many copies of DNA). I will be sampling ads in five-year intervals. *Science* is a weekly publication, and it would have taken an unreasonable amount of time to go through each issue. That being said, when I noticed a large change in advertising style (as in the interval between the years 1995 and 2000, for example), I also went back and sampled the relevant years on a year-by-year basis. In total, my sample size consisted of ninety-five ads.

For the ads dealing with gel electrophoresis, Smithies' paper will now serve as a reference point for tracking the evolution of the apparatus over five decades and the attendant changes to embodied coping (physical handling of the object) that successive versions imply. The nature of the agent-object relationship, as depicted in the ads, changes dramatically over this time span, in terms of the motor-intentional definition of the object, and how much (and in what way) the agent must interact with the object. I track these changes using my action-attention-language triad model, looking at changes to physical coping. I am especially interested in what seems to be the successive subtraction of the agent's body from the practical definition of the object. Unlike Smithies' scientific paper on gel electrophoresis and papers dealing with MAOA KO mice, which are concerned with apparatus-in-the-making, ads for apparatus necessarily feature objects that have been stabilized and rendered predictable enough for mass marketing. With mass-marketed apparatus, the embodied human variable has less opportunity to act variably; the action-attention-language sequences associated with commercialized apparatus are often tightly prescribed. Many of the ads that I will discuss promote the fact that agents' roles as embodied subjects have been significantly reduced. I look at how ads promote this altered role—from the physically-intensive, craft-skill dependent version of the apparatus in Smithies' paper, to the push of a button in recent (commercialized) versions of the apparatus—and how this altered role is portrayed by the advertisers as relating to scientific expertise.

I begin by looking at how ads for research products portray the material benchtop work of science. I note, for example, an overall trend towards the subtraction of the embodied agent from successive versions of gel electrophoresis. When agents' bodies are coaxed into new action-attention-language sequences involving simplified and automated research tools, the threshold for embodied competence is reduced so that nearly anyone can do the work, as the ads promise. Automation thus creates a ready-made embodied competence in the ads. Scientific research has entailed a division of labour for some time, where those who do the technical benchtop work do not necessarily participate in the linguistic work of writing about experimental events and vice versa, and the ads reflect and promote this. In many ads, further division of labour becomes an important selling feature, causing me to raise questions about the link between physical and linguistic research practices and who participates in what. Collins and Evans call those who participate solely in the linguistic practices of a domain "interactional" experts and propose that these kinds of experts can talk competently about material practices they have not engaged in. I use Collins and Evans' "periodic table of expertises" to look at portrayals of agents in ads for research products and how those agents are framed as experts in relation to physical and linguistic research practices.

Throughout this chapter, I work towards complicating Collins and Evans'categories of expertise to account for how scientific work is becoming increasingly subdivided among new micro-experts, who are involved in smaller and more specialized pieces of the scientific puzzle. I end by exploring what the interactional experts depicted in ads might be missing, in terms of linguistic abilities, by not having engaged in the benchtop work of scientific research. Scholars such as K. Amann and K. Knorr-Cetina, who use an ethnographic approach to look at data analyzing practices involving a gel electrophoresis autoradiograph, propose that knowledge is tied to embodied experience. I extrapolate the implications of their study to the issue of interactional expertise. I also call on several other ethnographic studies (Holt and Beilock; Schilhad et al; Ribeiro) which examine real-world examples of interactional expertise and its relation to linguistic abilities. Psychologists Lauren Holt and Sian Beilock look at embodied experience and linguistic ability in the case of athletes and non-athletes; learning researchers Theresa Schilhad, Gudlaug Fridgeirsdottir, and Peter Allerup examine the difference embodied experience makes to linguistic abilities in the case of midwife mothers and midwife non-mothers; and engineering specialist Rodrigo Ribeiro looks at the case of interpreters working in the steel industry who must talk about the trade but who do not participate directly in the work. I apply these studies to my discussion of interactional expertise and linguistic abilities in science, and to my reading of agents and expertise in ads for research apparatus, to suggest that the disaggregation of material and linguistic research practices requires further collective consideration.

Subtracting the Embodied Agent

In the time since Oliver Smithies first described gel electrophoresis in his 1955 *Biochemical Journal* paper, the apparatus has become common in molecular biology laboratories. The technique described in Smithies' paper, however, is time-consuming, error-prone, and requires a great deal of embodied craft skill, as described in the methods section. Recall from the first chapter the extensive tinkering described by Smithies in order to arrive at an apparatus that works. To produce a gel that works as intended, one has to suspend potato starch in acetone-HCl at exactly 38.5 degrees, incubate it for 45 minutes, add aqueous sodium acetate, filtrate on a Büchner funnel, wash thoroughly in distilled water, resuspend in distilled water, incubate overnight, wash again with distilled water, dehydrate with acetone, and finally dry thoroughly at 45-50 degrees (Smithies 630). Every step in Smithies' procedure implies extensive physical trial and error on the agent's part to arrive at workable conditions. Smithies' paper introduced gel electrophoresis to the scientific community, but before it could spread into many laboratories it needed to be standardized as an easily transportable technology, into a simpler, more predictable action-attention sequence.

One way of binding agents separated from one another in space and time is, as Latour observes, to make a machine that will keep the various material and human forces from splintering off (128). Unlike a tool, which Latour observes terminates at the end of the hand, a machine severs the link between the tool and a *specific* body (129). As an example, a pestle is a tool directly in a user's hand, but if one attaches the grinder to a wooden frame and attaches this to the sails of a mill, one has a windmill, a machine representing "an assembly of forces no human could ever match" (129). Once a machine is built, the trick is to persuade users to let go of the various idiosyncratically adapted tools in favor of the machine that brings them together. Ads for research apparatus invariably do just this: they portray the new apparatus as a stabilized assembly of forces that improves in some way upon the tools wielded before.

In the way that ads for research apparatus argue for the minimization of the subject's embodied role, they also become distinctly reminiscent of the scientific management movement pioneered by Frederick Winslow Taylor at the turn of the twentieth century. The breadth of recent scholarship on Taylorism attests to its continued relevance and the seemingly limitless applicability of the principles of scientific management to human affairs, [32] including science. Many of Taylor's arguments regarding the benefits of standardization and automation—from the emphasis on efficiency, to the superiority of machines over craft skills, to the benefits of the extra time that automation generates—can be seen mirrored in ads for research products. Research on Taylor and Taylorism suggests that changes in the phenomenology of scientific research work is like other changes in the organization of work

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[32] Indeed, Taylor envisioned the scientific management of work extending far beyond the factory, imagining his principles being applied to many types of human activity, "from our simplest individual acts to the work of our great corporations" (7). The same principles, he goes on to elaborate, can be applied "to the management of our homes . . . our farms . . . the business of our tradesmen, large and small . . . our churches, our philanthropic institutions, our universities, and our governmental departments" (8), and now science as well. Taylorism has been linked by scholars to everything from modern call centre management (Bain); to Japanese manufacturing systems (Conti); to the organization of modern ballet "away from focus on the ballerinas and onto the 'creative' choreographer and composer" (Van Delinder 1439); to "The Taylorization of Vladimir Ilich Lenin" (Scoville); to the design aesthetic of modern architecture (Duina). For a more general historical study, see Daniel Nelson.

in modern times; when applied to ads for research apparatus, this research helps explain portrayals of agents and how they act on objects.

Especially relevant to my analysis of how ads for research products portray the action component of my triad (how embodied agents act on recipient objects) is Taylor's central theory of scientific management, or "task management," based on the unifying idea of efficiency. One of the main purposes of Taylor's *The Principles of Scientific Management*, published in 1911, is to convince workers to abandon their traditional, craft-intensive practices in favor of standardized movements involving standardized implements. Taylor argues that the largest wastes of human effort are due to the "ill-directed movements of men" (5). Scientific management is about developing each worker to the maximum state of efficiency, which can be further qualified as the biggest output, at the fastest pace, with the least expenditure of effort (11). Taylor's aim is to ultimately eliminate all "false," "slow," and "useless" movements (117). Altering workers' movements towards simplification often entails replacing "rule-of-thumb" methods—those learned by word of mouth, or unconsciously through direct observation and trial (31)—with formulas that replace individual judgment (37). The most efficient movements and implements then replace the "inferior" ones formerly in use (Taylor 117). Taylor thus seems to be writing against the sort of tacit knowledge one gains through direct observation. Although Taylor admits that, under scientific management, workers have little choice in tools and methods (128), he imagines the advantages of "absolute uniformity" ultimately making work faster and easier (118). Ads for scientific research products are ultimately designed with the same purpose in mind: getting individuals to let go of idiosyncratic, locally developed tools, in favor of mass-marketed ones that standardize, and typically simplify, action and attention.

As the tools of research are redesigned, however, so are users. Recall Marx's aphorism from the previous chapter that "production not only creates an object for the subject but also a subject for the object" (qtd. in Pickering, "Practice" 163). Whenever a machine is built, the body is repositioned and coaxed into a new action-attention sequence. There are new things for the user to do, new things to notice, new words for it all. The commercialization of research objects can substantially alter agent-object relations, which implies that the ways in which researchers can talk about what they did in the lab potentially becomes reconfigured as well.

Before we arrive at a fully automated version of gel electrophoresis in more recent ads, there are successive changes to the design of the machine along the way that point towards the packaging of various (often tacit) bodily skills directly into the machine. A sampling of ads shows the steady re-engineering of the apparatus towards automation, and thus towards the minimization of the action component of my action-attention-language triad.

Even as early as 1965, for example, E-C Apparatus Corp. is advertising a Vertical Gel Electrophoresis Apparatus where the "cell locks itself together—no further manipulation" and the user is encouraged to be "lazy." In 1975, Pharmacia Fine Chemicals is advertising "[c]onvenient ready-to-use gradient gel slabs," where "[s]elf-limiting migration concentrates the zones after separation by molecular size," containing a "built-in cooling system," "[p]ush-button selected constant voltages," and "[t]imer controls." By 1985, LKB Bromma is advertising a Horizontal Electrophoresis System where "[t]he unique LKB Immobiline System is significantly different to traditional gels in that its pH gradient is an integral part of the gel." At no point does it appear as though the apparatus cannot be packaged some more; there are always additional production tasks that could be streamlined, minimized, or outright eliminated through automation, and newer versions of a research product are frequently framed as superior to previous ones based on these considerations. Even already highly packaged versions claim an improved, more efficient apparatus design:

> Since its introduction in 1984, the original PROTEAN®II cell has enjoyed unparalleled success with more than 10 000 in use. However, good designs are not static, they evolve. Thanks to your input, we have made significant design and performance improvements. (Bio-Rad 1990)

In addition to "[n]ew state-of-the-art plastic construction" and new materials, Bio-Rad's new design also boasts "six other new features to improve ease-of-use, convenience, and versatility!" As I will discuss shortly, the phrase "ease-of-use," invariably means a reduction in the motor intentionality associated with the object.

When the drive to package further is taken to its extreme endpoint, the machine arrives with hardly any embodied production tasks left for the user. Compare Smithies' description with that of a 1997 ad for Stratagene's "Cast Away" precast acrylamide gel, which represents gel electrophoresis as a tightly packaged machine for standardized action. The ad shows only a shiny, thin, sealed packet. The copy (text) claims the pre-cast gel inside has "So Many Applications," such as DNA Sequencing, Differential Display, and DNA Fingerprinting. All of the artful, bodily-involved tasks associated with casting the gel described in Smithies' paper have been packaged for the user. The gel in this ad arrives already cast and the tasks still required of the user have been greatly simplified. If the copy accompanying the image is posing partly as operating instructions, or, at least, invoking this association, then the relay between the minimalist image and copy[33] suggests altogether fewer tasks and the further simplification of those that remain.

An Invitrogen ad from 1999 for "Ready-Made Electrophoresis" likewise captures the reduction of embodied coping, and an altered agent-object relationship. The copy in the ad states:

> E-Gels™ are unique precast agarose gels that provide everything you need for high-resolution electrophoresis. The convenient UV-transparent E-Gel™ cassette not only supplies the gel, but also electrodes, ethidium bromide, and an innovative dry buffer. You don't have to pour hot agarose, wait for the gel to solidify, prepare liquid buffer, or stain the gel. And you don't have to use a bulky gel box either because E-Gels™ run in a space-saving E-Gel™ Base that connects directly to your power supply. In just 3 easy steps, you're running the gel.

Packaged into one machine are now all of the skill-intensive tasks initially described by Smithies. The copy at the top of this ad declares that "[i]n 30 minutes you could be loading your samples. Or you could have your results." Thus even the previous design of the apparatus, which let the agent load samples in as quickly as thirty minutes, can still be packaged further. Here, scientific research appears on its way to yielding instantaneous results with the bare minimum of bodily involvement. A 1997 ad from Pharmacia Biotech finally promises that the agent "can push a few buttons" and "walk away."

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[33] The notion of relay between image and text, as well as "anchorage," comes from Roland Barthes, describing how image and text work together to construct meaning. Greg Myers also notes that the relationship between image and text is always more complex than mere illustration, and that the message is greater than the sum of image and text alone (*Words* 136). This will become especially important in more recent ads that feature images of people rather than the products.

The repeated promotion of simplified agent-object relations potentially reflects, and affects, how researchers expect to interact with the objects of research, meaning with a minimum of craft-intensive knowledge.[34] Mass-marketed research products importantly imply more uniformity across diverse bodies and material contexts, helping to explain Gieryn's observation that scientific claims seem to "shed the contingent circumstances of their making," becoming "transcendent" and true of all bodies everywhere (113), allowing for the presumption that research in other labs is happening in exactly the same way as it is in one's own (128). When it can be assumed that everyone is buying their products from the same few companies (a point that Gieryn does not bring up in his discussion of how claims shed the contingent details of place), many of the material details of the work can be omitted from research accounts. The ubiquity of mass-marketed research products potentially shapes discursive options, in terms of what those writing research accounts believe they need to say in order to be understood.

Ads for automated, mass-marketed research apparatus also tend to frame embodied coping as having been made more certain, often as a result of having been reduced as much as possible. The standardization of work seminal to Taylor's theory of scientific management was a means of ensuring predictability in how interchangeable agents handled a given object. In scientific research, Fujimura observes that the standardization of embodied

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[34] Although the association of science with the prototypical "hard sell," defined by Cook as "a man in a suit, standing in front of a pile of carpets, talking loudly and directly to the camera about low cost, limited availability and guaranteed reliability" (15), might seem antithetical to the idea of research as a disinterested, rational process, ads for research products nonetheless can and do influence the practices of science. Fujimura notes, however, in her discussion of the kinds of considerations that shape the ways researchers assemble their work spaces, that science is not distinct from other kinds of labour (Crafting 104). She says that when science is considered work, preoccupations with things like ease, speed, cost, and efficiency make sense (104). The researchers interviewed by Fujimura admit to being swayed by these more pragmatic considerations when deciding on their research program and the material set up of their laboratories (104). Indeed, Fujimura notes seeing a "dog-eared" photocopy of an ad for Oncor (supplying oncogene research tools), taken from the journal *Science*, taped to the wall in one laboratory (98).

coping leads to the impression of increased certainty, because it links research objects and embodied skills into “relatively stable relationships” (*Crafting* 105). The simplification of research objects through automation thus helps ensure the practical alignment of many dispersed agents because commercialized research objects imply the repetition of the same action-attention-language sequences.

In ads for research products, references to increased certainty and decreased uncertainty are frequently part of the copy, and often associated with the reduction of tacit-intensive tasks. Some of the recurrent key words linked to claims of increased certainty include “reliable,” “precise,” “identical,” “uniform,” “guaranteed,” “systematic,” and “reproducible.” Many of the ads invoke several of these key words at the same time. An ad from LKB Bromma from 1985, for example, states that their apparatus allows the researcher to avoid the “trouble” of casting their own gels while also “guaranteeing both high resolution and excellent reproducibility.” Apart from being “easy to use,” the “UltroMould” is advertised as “producing perfectly accurate and uniform gels and ensuring a high standard of reproducibility.” Next to an image of a resolved autoradiograph featuring distinct, clear protein bands (the desired outcome) the copy further explains that “LKB UltroMould casts bubble-free ultrathin gels in only 30 seconds, making gel casting quicker, easier and more certain.” In these ads, then, the idea of certainty seems to be tied to the idea of predictability which appears tied to the idea of easiness.

Some ads circulate the idea that the uncertainty involved in scientific research has been decreased by a reduction in tricky, embodied tasks. A BRL Bethesda ad from 1980 claims gel transfer technology that “is easier, convenient, and systematic,” and, importantly, that “there’s less chance of damaging the gel” using the advertised product. An LKB ad from 1985 likewise promises "a whole new scenario of problem-free chromatography." This same ad also interestingly refers to the "built-in magic of matrix engineering," implying that the "magical" element, which Alberto Cambrosio and Peter Keating note in their ethnographic study of molecular biology is often associated with tacit craft labour, has been reliably built right into the product.[35] A Novex ad from 2000 is even more explicit in its reference to the uncertainty that researchers face in their work. It features an image of a puppy. The accompanying copy underneath states the following: "Reduce your separation anxiety with NuPAGE™ Pre-Cast Gels." The fine print underneath asks, "Worried that your pre-cast gels aren't telling the truth? Not performing consistently?" The copy in the right hand corner, the last thing the viewer will read,[36] then promises, "Accurate results. Time after time." In terms of the kind of certainty that is associated with "identical conditions," "exceptional uniformity," and reliable results "every time, without lot-to-lot variability," as a Bio-Rad ad promises (2000), then even the most diligent human is no match for a fully automated machine. The kind of certainty that these ads are promising therefore has to do with the agent's embodied performance in a research setting; the more automated a scientific apparatus becomes, the more certain an agent can feel that his or her physical interactions with the object will lead to the desired outcomes.

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[35] In this study, Cambrosio and Keating are interested in the tacit dimension of scientific work. They look at the transmission of hybridoma technology, and note that, far from being ignored by scientists, the local, tacit, and "magic" elements of scientific work are often consciously part of researchers' practices and situated dialogue.
[36] The Gestalt principle of Prägnanz states that we do not see the separate components of the ad, but rather, eventually, the ad as a whole (Gross "Verbal and Visual"). The layout becomes critical to our reading of the ad as a whole because it gives the sense of "temporal progress" through the ad, guiding our movement through the ad by manipulating the arrangement of an ad in space (Myers, *Words* 140). Like the progression through a written page, ads have a spatiality to them, a top and a bottom, a left and a right that is exploited by advertisers for best effect. This spatiality is also why, typically, the logo appears in the lower right corner of the layout, as the last thing the audience reads, and "the answer to what is posed above" (Myers 139). For more on the rhetoric of images, see Gross, who blends Gestalt theory, linguistics, logic, and narrative theory to look at how images are made meaningful (Gross does not address ads per se, however).

The ability of the embodied agent to handle a given research object is thus implicitly questioned in such ads. The implication is that the agent's body is potentially a source of uncertainty, and that removing this problematic part of the material process leads to more reliable scientific knowledge. This notion that scientists themselves might be standing in the way of information has been traced back by Daston and Galison to the latter part of the nineteenth century, when mechanical image-making became popularized by French physiologist E. J. Marey and his followers. Daston and Galison explain how a new brand of objectivity came to dominate science at this time, one based on mechanically generated images, and its slogan was, "Let nature speak for itself" (81). At its center, Daston and Galison state, was the idea that scientists should impose as little of themselves on their work as possible:

> . . . the all-too-human scientists must, as a matter of duty, restrain themselves from imposing their hopes, expectations, generalizations, aesthetics, even ordinary language on the image of nature. Where human self-discipline flagged, the machine would take over. Wary of human intervention between nature and representation, Marey and his contemporaries turned to mechanically produced images to eliminate suspect mediation. (81)

Ads for research products appear to be echoing the argument put forward by Marey, as interpreted by Daston and Galison, that embodied subjects can be a source of "suspect mediation," and that machines can do the job better. In particular, the ads imply that science goes better when scientists have fewer chances to impose their bodies, values, and interpretive perspectives on proceedings. Latour likewise observes that non-human elements in science can appear more reliable because they are directly interested in the operation of the machine. Why leave the operation of a valve, for example, Latour states, to an "unreliable worker" when the machine itself can be engineered to do the job (130)?

Some of the more recent ads for research products attempt to sell the product by listing the tasks that have been taken away from (the potentially unreliable) agent. Earlier ads tend to list the features that have been added to the design, showcasing what the apparatus now does that it did not do before. A Gilson ad from 1976, for example, describes, "[s]everal important design features such as incremental advance of the gel, a rotating cutting wire and a pulsing pump." An interesting feature of the Invitrogen ad from 1999 for "Ready-Made Electrophoresis," by contrast, is that it frames the new design in terms of what is no longer needed from the agent in terms of embodied, skillful coping. The Invitrogen ad lists the production tasks that have been swallowed by the machine: "You *don't* have to pour hot agarose, wait for the gel to solidify, prepare liquid buffer, or stain the gel," and "you *don't* have to use a bulky gel box either" (emphasis added). The language used is that of *subtraction* rather than addition. More specifically, it is the (unreliable) user's bodily contribution that is being subtracted from successive versions of the apparatus

The Stratagene ad from 1997 discussed previously similarly lists the production tasks no longer associated with the "Cast Away" precast gels. Listed are the tasks that were once associated with the apparatus that have since been packaged directly into the machine:

NO Glass Plate Cleaning

NO Solution Mixing

NO Acrylamide De-Gassing

NO Gel Pouring

NO Waiting for Polymerization

NO Mess to Clean Up

“Save up to 6 hours!” the ad finishes by stating. Importantly, both the Stratagene and Invitrogen ads propose to subtract tasks, but do not go on to describe what *is* still needed from the user in terms of embodied coping, implying that procedural steps have simply been eliminated. In other words, there is less for the user to do. Imagine, for example, an agent handling Smithies' version of the apparatus, and then imagine the kinds of bodily tasks associated with Invitrogen and Stratagene's version of the apparatus. The motor-intentionality associated with these automated versions of the apparatus has been greatly simplified, if the ads are to be believed.

The subtraction of embodied tasks is finally taken to its logical conclusion in a Qiagen ad from 2008. “Pure excellence, pure efficiency,” the copy states at the top of the page. Taking up nearly the entire page is a picture of two nondescript, smooth, plastic machines: the QIAxcel and the QIAcube. The picture is an extreme close-up of the machines. It is impossible to tell without the copy what it is that these machines do. “Say goodbye to manual spin-column preps and gel electrophoresis!” the copy states at the very bottom of the page. This is the sole line of text in the ad. The only component on the machines that implies *any* bodily coping is a round power button. When the copy tells the viewer to say goodbye to the manual way of doing electrophoresis, the ad implies that *every* production task previously associated with the apparatus has at last been subtracted, and all that the agent is required to do is push a button. This minimization of embodied participation is reflected in the minimal design of the ad itself. Although the machine appears simplified, it has actually grown more complicated through layers of redesign and the packaging of production tasks. The machine itself is not doing less. It is only the agent who is doing less, at least in terms of time spent physically interacting with this recipient object, which has become "black-boxed," as Latour would say, in that the inner workings of the machine have become opaque to the user. In this ad, the machines literally look like opaque boxes, and agents have little choice in how they use them. It would seem, then, that when a scientific apparatus becomes "black-boxed," it potentially also implies less physical handling overall.

If time has been freed up through the use of automated apparatus, which calls only for the push of a button, then what kinds of tasks is the agent now occupied with in a research scene? My action-attention-language triad implicitly links the notion of an agent with embodied actions on recipient objects in a research scene; automated research tools, with their concomitant reduction of embodied coping, therefore call for a re-examination of the term "agent."

The Ready-Made Competent Expert

Ads for research products point towards some of the possible effects of automation on the scientific agent. Highly automated research tools appear to lower the threshold for embodied competence by simplifying the motor-intentionality associated with a recipient object. When embodied tasks are simplified, the need for active, intense attention on one's movements is also lowered. Hence the stereotypical movie image of an assembly-line worker lost in thought, mentally elsewhere, while repeating the same, simple movement all day long. The simplification, standardization, and automation of embodied tasks in science thus appear to create ready-made competence.

In the introduction to *Scientific Management*, Taylor makes the observation that everyone is looking for "the ready-made, competent man" who can step into a task with minimal training (6). This ready-made competence to which Taylor is referring, echoed in the ads, is a kind of guaranteed embodied competence (everyone can push a button), and one of its effects is to render parts of the work of research so easy that nearly anyone can do it, thereby allowing less skilled personnel access to it. Daston notes that one of the main casualties of the pursuit of "aperspectival objectivity" in the latter part of the nineteenth century was "not trust but skill" (611). Skill, especially tacit skill, Daston notes, did not fit with an enlarged, collective version of science; it was rare and expensive, could not be expected of all workers, and could not always be communicated (611). Thus, as science expanded, Daston explains, so did the need for inexpensive labourers who were easy to train (611). According to Daston, Charles Babbage, a nineteenth-century experimental philosopher, even suggested that scientists should follow the example of factory manufacturing and divide tasks into the smallest, simplest element in order to minimize the necessary qualifications (611). An interesting contradiction potentially emerges when automation allows a wider range of people, with a wide range of skills, to participate in scientific research: science becomes both more and less inclusive at the same time. More people are allowed in, but potentially fewer of them count as scientists. I return to this idea shortly.

As the technology becomes increasingly packaged and automated, ads begin to commonly use some variation of the phrase "easy to use" as a persuasive factor. An Ortec ad from 1975, for example, promises, "The step-by-step instruction manual that accompanies each ORTEC PAGE System is written so that anyone trained in basic chemical laboratory procedures can use our equipment." An LKB Instruments ad from 1980 likewise promises, "[g]els which are so easy to use you're ready to apply samples in less than one minute." A

Protein Databases ad from 1990 similarly states that the product is "as easy to use as a rule and pencil—only *faster* and more *accurate*" (emphasis in original). Now "anyone" with the basic training, as the Ortec ad promises, can do the work, as opposed to only those with tacit skills developed through situated tinkering. It is interesting that we are seeing the same rhetorical move in electrophoresis ads from different decades. The phrase "easy to use" appears to be relative to the nature of benchtop practices that came before, but the reiteration of this phrase over several decades implies a wider and wider net, in terms of who is allowed to participate in research practices as the work gets easier.

Eliminating the requirement for tacit skills through standardization and automation thus makes it possible to treat research personnel as Taylor had factory workers, as replaceable, ready-competent elements of an assembly-line system. Many of the biologists interviewed by Fujimura in her ethnographic, sociological study of the daily practices of molecular biology admitted to including recombinant technology in their research programs because the work had become relatively easy. Researchers did not need to be experts in enzymology, chemistry, or physics to use the commercialized technology, and even those untrained in molecular biology could quickly learn the technology (*Crafting* 105). Fujimara quotes an industry analyst as remarking that "any technician can be trained to operate an automated synthesizer" when the apparatus comes preprogrammed and all that the user has to do is add the reagents, enter a DNA sequence, and purify the solution (*Crafting* 108). What Fujimura observes happening in molecular biology labs, in terms of who is participating in benchtop practices and the basis for their participation, thus mirrors what the ads themselves are promising.

Notwithstanding some consistency in the rhetorical appeals used in the ads regarding ease-of-use and predictability, we can also see an interesting progression in the ads. First, the difficult, craft skill-intensive aspects of the work are eliminated through automation, and then the resultant tedious tasks that remain follow suit. Phrases related to tedium initially tend to refer to time-consuming and tricky production tasks. An LKB Instruments ad from 1975, for example, promises "[n]o tedious preparation": "[w]hen you use an Ampholine PAGplate you avoid the tedious preparation and exposure to toxic chemicals involved in do-it-yourself gel plates. No pipettes or complicated sample applications either." Similarly, a Kodak Laboratory and Specialty Chemicals ad from 1985 states that their product "eliminates the tedious staining-destaining steps that tie up your lab facilities and people for days." We can thus still see Smithies' labour-intensive version of the apparatus in these ads, which are primarily concerned with reducing time, complexity, and embodied, tacit skill labour. A Beckman ad from 1990 similarly refers to the process of electrophoresis as "a tedious, error-prone process." Reducing this kind of tricky tedium then becomes the justification for giving the task to an automated machine (or to a contract research organization): "give the job to Biomek and each step is performed with predictable precision."

In more recent ads, however, the language shifts towards the implication that the work has become tedious in the repetitive and monotonous sense, while still remaining time-consuming. Instead of words such as "trouble" and "error-prone," we start to see phrases such as, "Are you tired of . . .? Are you annoyed by . . .?" (Sigma, 1997). The tiresome and annoying nature of the work then becomes the new justification for handing it to someone else; "What if someone could do the tedious part of research for you?" a 2000 advertisement for LabOnWeb.com finally proposes.

Having less skilled persons do the embodied work of science thus sets up the conditions for the separation of benchtop and linguistic research activities. Once the practical part of research work is simplified, standardized, and automated so that someone without any theoretical knowledge of the field can do it, then it becomes *too simple*, or "tedious," for those *with* theoretical knowledge to do it. The tedious can then be passed off to someone else and the material work of the body can be fully separated from the work of making knowledge claims and propagating the discourse of science. In the ads, it is the technological shift to automation that appears to be the engine of separation for different kinds of research work. In reality, the "tedious" part of the job has been passed off to technicians and graduate students for some time, but the skill level required of such agents appears to be reaching a new minimum when all that is left to do is mix some reagents and push a few buttons. It is this level of automation in life sciences research that is a fairly new phenomenon; in the grand scheme of science, we have not seen this widespread level of commercialization and automation before.

Particularly salient to my discussion of the relation between physical and linguistic research practices, and a way of making sense of the depiction of agents in ads for research products, is the story of one of Fujimura's own sociology graduate students, hired by a laboratory at Harvard to sequence genetic material.

> He was specifically hired for a short term of two years (no more) and *for his lack of biological knowledge*. The scientists heading the lab preferred workers who were not interested in biology and therefore not likely to be 'distracted' by the biology itself. In other words, they wanted smart assembly line workers. (*Crafting* 105, emphasis added)

The phrase “for his lack of biological knowledge” in the above passage is especially significant because it points to the possible separation of benchtop and linguistic activities. This student would clearly not be participating at any point in the writing of research accounts.

I stated earlier that a contradiction potentially arises when automation allows many agents with minimal skills to participate in research, in terms of making science both more and less inclusive. Assembly-line workers generally repeat a given motion, but are not expected, and indeed rarely have the opportunity, to contribute to the formal discourse of their domain. Fujimura’s student did not have the opportunity (nor, as the story is told, did he have the necessary linguistic skills) to be able to contribute to the writing of the experimental paper, but he could do the physical work that would become the basis for language work. The student would be able to describe in immediate physical terms what happened during the experiment, but the description would entail the kinds of material details that would point the claims back towards situated contingency, potentially back towards a specific body acting on objects at a specific time and place. If those doing the physical work of research (handling samples, using the machines, pushing the buttons, etc.) are not participating in the writing, then what becomes the basis for scientific expertise, in terms of these different kinds of activities? How is expertise portrayed in the ads? What kinds of activities are scientific experts portrayed as engaged in? How do ads depict those who are doing the language work?

Action-Attention-Language and Expertise

In *Rethinking Expertise*, Collins and Evans outline a model of expertise that suggests some ways to think about the relationship between material and linguistic participation in a scientific domain. Their resulting "periodic table of expertises" classifies the different kinds of knowledge that one might possess. I am especially interested in the wider category Collins and Evans term "specialist expertises," which they further subdivide into "ubiquitous tacit knowledge" and "specialist tacit knowledge." Under "ubiquitous tacit knowledge," the authors place what they call "beer-mat knowledge," "popular understanding," and "primary source knowledge"; under "specialist tacit knowledge," they place "interactional expertise" and "contributory expertise." It is these latter two categories of specialist tacit knowledge that are the most relevant to my discussion. "Contributory expertise," then, is the most comprehensive expertise, in terms of engagement in both physical and linguistic research activities (14). As the term implies, it allows individuals to contribute to their domain of specialist knowledge because they have internalized certain physical, craft skills (24). The term "expert" is typically associated with contributory expertise.

The concept of "interactional expertise," on the other hand, is a novel category of expertise proposed by Collins and Evans, defined as the possession of linguistic skills in the absence of "practical competence," which I take to mean embodied expertise in research work (14). It is expertise in the insider language appropriate to the reporting of science. Anyone who can participate in scientific discussions without being able to do the actual benchtop experimental work would fit in this category. Interactional expertise is still immersion in a culture of participants, but not in a practical sense. As Collins and Evans explain, the interactional expert "may be able to *understand* scientific things, and to *discuss* scientific things, but is still not able to *do* scientific things" (35, emphasis in original). This category would therefore include, according to the authors: sociologists who do ethnographic work, social anthropologists, specialist journalists, salespersons, managers, and those who adopt false identities in internet chat rooms, to give some examples (31). The authors' "strong interactional hypothesis" is that interactional experts can, theoretically, attain the same fluency in the language of a domain as those who are also fully immersed in the practices of the domain (31).

My interest in Collins and Evans' discussion relates to the interplay between being able to do scientific things, as the authors put it, and being able to talk about those things. Collins and Evans' begin with the overarching question: "Who should contribute to which aspects of technological debate in the public domain?" (113). Their aim is to explore how boundaries are set around public input on technical and scientific matters, and their "Periodic Table of Expertises" was devised to help us decide who counts as an expert in such disputes so that contribution is balanced (133). I am less concerned with the issue of boundaries, and more concerned with how doing science relates to talking about science. In this chapter, I rely on Collins and Evans' terminology to explore portrayals of expertise in ads for research apparatus. I find Collins and Evans' distinction between contributory and interactional expertise a fruitful model for looking at a similar kind of distinction being made in ads for research apparatus. To my knowledge, no one has applied their "Periodic Table of Expertises" to scientific ads.

I would, however, like to propose an additional category to Collins and Evans' periodic table of expertises to accommodate those who have practical, embodied, expertise in the absence of discursive competence in the domain. This kind of expertise would be the exact opposite of interactional expertise as described by Collins and Evans, as it is the ability to do the benchtop experimental work *only*. I call it "practical expertise," although I add the caveat that "expert" may be a problematic term here as well, if all that there is to do on an embodied level is to push a few buttons. Nonetheless, I want to use "practical expertise" here to refer to those that *are* still pushing the buttons on the machines, and to those that do much more than this on an embodied, practical level, but who do not participate in the formal language work of a scientific domain.

Ads for research apparatus do portray a division of labour among distinct groups of agents, and posit the new work of science as "thinking about science" (versus handling recipient objects) and as engaging primarily in various linguistic activities. Limited time becomes a crucial feature of arguments for the division of practical and linguistic work.[37] Many of the more recent ads appeal to the idea of time as a limited resource, proposing that researchers can now spend their time doing x instead of y, with "x" invariably being some form of linguistic work and "y" the embodied benchtop work of research. A Chemdex ad from 2000 (whose slogan is "Accelerating Science") captures especially well the rhetorical trajectory of the limited time appeal, although the product in this case is a website for ordering supplies, rather than an apparatus. "Are You Spending Your Time Focusing on Science or Ordering Supplies?" the ad begins. "The hours you spend ordering lab supplies cut into your day, distracting you from your research. You have more important things to do." The ad does not specify, however, what these "more important things to do" might be, nor what "focusing on science" might entail. In an LKB Bromma ad from 1985, the

[37] On the limited time appeal in advertising, see Barbara L. Gross and Jagdish N. Sheth. In particular, Gross and Sheth argue that time concern is correlated with, and possibly stems from, industrialization and urbanization (81).

apparatus “enabl[es] users with high workloads to spend their time screening samples instead of casting gels.” A SciQuest ad from 2000 likewise captures the essence of this limited time argument, when it states that their product “offers you something money can’t buy – more time to do what you do best.”

Interestingly, the nature of the x and the y in above formula—what it is exactly that “you do best”—keeps shifting in the ads. Where the LKB Bromma ad from above (1985) urges the viewer to spend more time *analyzing data* instead of doing the preparatory work of experimentation, a Protein Databases ad from 1990 proclaims, “Produce Results; Don’t Just Analyze Data.” This ad promises that its automatic gel analyzer “performs thousands of tedious analysis tasks and calculations” so that “[t]he routine decisions are automatically done for you; the difficult ones are brought to your attention so that you can make the right decision, quickly. *You are thinking about the results, not just analyzing data*” (emphasis added). Since the product supposedly “scans the film or gel, analyzes the data, and gives you results,” the x that researchers could now be spending their time doing becomes “thinking about the results.” “Just Picture it…Time to Think,” the ad concludes.

A recent TransnetYX advertisement from 2005 (whose slogan is “Serving Research. Saving Time.”) is even more specific about what focusing on science might entail. This ad explicitly captures the emerging division of labour between practical and interactional experts, where the latter are portrayed as engaged in the work of science proper, which is itself becoming harder to define. Unlike earlier ads for research products, which typically feature images of the product,[38] the TransnetYX ad shows a half-page photograph of a male

[38] Around the year 2000 there is a noticeable change in the images used in ads for research products, with significantly more images of people instead of objects. Images of people become the norm in the most recent

researcher, wearing a white lab coat and plastic eye goggles. He looks inconvenienced to the point of apathy. The copy to his left states:

> I have real work to do.
>
> Getting published.
>
> Writing my grant.
>
> Putting together my presentation.
>
> Discovering something meaningful.
>
> I need time to do it.
>
> But here I am, genotyping mice tails.
>
> Not exactly what I had in mind when
>
> I went into research.

The small print informs the viewer that TransnetYX is a genotype testing service, where "[a]ll you have to do is clip and ship the [mouse] tails, then click for your results." Testing is automated and reportedly "error-free," and researchers can have their results (available over the internet) in as little as twenty-four hours. "Most importantly," the copy concludes, "it frees up your time for research, *or anything else you'd rather be doing*" (emphasis added). The white lab coat and goggles in this ad thus symbolize wasted time, rather than expertise and credibility, as in ads for household[39] or beauty products.[40] The scientist in this ad is portrayed as engaged in the wrong kind of work for someone with theoretical and linguistic skills, who could be thinking and writing about science, rather than actually engaging in its

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ads (2005-2008), where images of the products are minimized, if not eliminated altogether, which in itself is telling of the disappearance of the embodied work of research.

[39] On the use of science in ads for household products see Bonnie Fox (31).

[40] Scientist Roger Highfield's article on science in beauty advertising is especially illuminating, as even this Nobel Prize winner finds himself "baffled" by the pseudoscientific language used in such ads.

physical nitty-gritty. The blank, drone-like expression on his face furthers this message that the embodied benchtop work he is involved in is physical but mindless. TransnetYX also has a website, which elaborates even further on the message of this print ad by telling the viewer "You're losing time by doing it yourself"; "You're making it harder than it needs to be"; and, "You're risking less-than-accurate results." Interestingly, other than the oblique reference to "discovering something meaningful," none of the activities listed in the ad have traditionally been associated with science. In this ad, then, the basis for scientific authority becomes the ability to engage in the listed linguistic activities. This ad raises the important question, though, of how one comes to possess this linguistic scientific authority, and whether some kind of previous experience with benchtop practices is necessary. The ad seems to imply that it is not necessary.

The message that those who have not directly engaged in experimental practices can competently speak for science is not incongruent with emergent trends in the commercial management of scientific work. The extreme example of interactional expertise might be the independent scientific (or "ghost") writer. Reporting on a meeting of the International Society of Medical Planning Professionals (ISMPP), which represents professionals involved in the "publication planning" industry, Sismondo states that even the notion of a single ghostwriter doing all the writing for which one author will take credit is outdated ("Ghosts" 187). Biomedical writing, Sismondo explains, has become "part of a larger process of the corporate production of knowledge," in which experimental papers are written by a team of people, none of whom may individually meet the requirements of authorship as outlined by many peer-reviewed journals (187). The criteria for authorship adopted by most journals is set by the International Committee of Medical Journal Editors (ICMJE), and it states the following:

> Authorship credit should be based on 1) substantial contributions to conception and design, or acquisition of data, or analysis and interpretation of data; 2) drafting the paper or revising it critically for important intellectual content; and 3) final approval of the version to be published. Authors should meet conditions 1, 2, and 3. (qtd. in Sismondo, "Ghosts" 184).

Sismondo comments that these criteria appear to be an attempt to preserve traditional ideas of scientific authorship (184), where some familiarity with benchtop practices is still expected of those writing about research. The emergence of a publication planning industry, however, points to the increasingly complicated nature of writing in science, and a further complication of this category of interactional expertise.

The TransnetYX ad seems to be both invoking traditional ideas of scientific authorship and also speaking against them. There is an acknowledgement of the expectation of involvement in both benchtop and discursive practices, but then this requirement is dismissed with the phrase, "Not exactly what I had in mind when I went into research." According to Collins and Evans' periodic table of expertises, the agent in this TransnetYX ad would be a contributory expert, able to engage in both the physical benchtop work and the linguistic work of science. This ad thus seems to be speaking to graduate students, postdoctoral fellows, and possibly new researchers (since the agent is described as writing grants) who are still doing work in the lab, as opposed to senior researchers who are removed from the material work to various degrees (which I discuss shortly when I bring up Ribeiro's concept of "physical contiguity"). The TransnetYX ad is urging these researchers to *stop* doing the lab benchtop work and concentrate solely on the language work. The “real work” of science, as the ad states, becomes language-based activities: publication, grant writing, giving a presentation, and thinking. It is difficult to tell, however, if the ad is implying that at some point those doing the benchtop work, on the path towards a senior research position, can stop doing it because they have acquired the necessary experience and can now write about science, or if the ad is implying that this type of benchtop work need *never* be done by those destined for interactional expertise. My impression is that the ad is implying the latter. Indeed, this ad seems to be promoting the idea that contributory experts *should not* be engaged in laboratory work, that it is a misuse of their time and skills. At the same time, the kind of interactional expertise promoted in the ad is still not the kind that Sismondo describes happening in the publication planning industry; the agent in the ad, even if he were to stop doing the lab work, would still be engaged in all of the important language activities of science in a rather holistic manner. There is the impression that this agent could become a full contributory expert again at any moment.

This subtraction of the contributory expert’s body from the material work of experimentation is not unique to the TransnetYX ad. In a Pharmacia Biotech ad from 1997, introduced earlier, viewers are first asked whether they are “spending hours at the bench staining gels?” The answer is, presumably, “yes.” The copy then states: “You don’t have to anymore. Now you can push a few buttons, *walk away*, and return to reproducible staining results” (emphasis added). Researchers are thus being told they can physically remove themselves from the experimental scene while a machine generates the data. In this ad, the dominant image is also a close-up of a man’s face, literally in a thinking-man pose, who is quoted as follows: "'Manual staining is an obsolete concept. It exists no more,' Doug Burtrum, Immunologist/Research Scientist, Haiku writer, New York, NY."

What is more surprising than this professional endorsement for an automated gel stainer, is that the makers of the ad included the fact that Doug writes Haiku poems. Why include this? This information is not incidental. It shows that Doug is a person with extra time to devote to activities not tied to scientific research. Interestingly, these other activities, haiku writing, are also thinking, language-based activities. As in the TransnetYX ad above, there is the implication that once unfettered from lab benchtop work, researchers become unfettered in a more encompassing sense. In a 2005 ad from Stratagene for automated QPCR (quantitative polymerase chain reaction), a handsome, smiling young man, dressed in a shirt and tie (note, no white lab coat and goggles), is shown sitting behind a desk which has nothing on it but a computer and a small, non-descript machine. He is wearing headphones, holding a sandwich in one hand, and snapping his fingers to the music. This researcher is also depicted as doing things unrelated to the embodied production tasks of scientific research while an automated machine does the work.

In all three of the latter ads, agents are indeed free to focus on anything else they would rather be doing. The relay between image and copy serves to highlight the changed nature of the agent-object relationship, as compared to Smithies' labour-intensive apparatus. It is significant that in the TransnetYX, Pharmacia Biotech, and Stratagene ads, the dominant image, as noted, is not of the product but of a person. The impression is that there is nothing tangible, no recipient object, left to interact with. This disappearance of research objects is perhaps part of the intended message or merely coincidental—a feature of the modern "soft sell" in advertising, which Cook defines in general terms as selling a lifestyle and persona rather than a thing *per se* (as opposed to the "hard sell" that lists all of the superior features of the product) (Cook 15). When we take away the object towards which bodily movement and attention would normally be directed, we are left, strangely, with just the agent. The irony is that ads for research products begin to feature images of the agent, to focus on the "you," rather than the "it," precisely as the work becomes disembodied, as the embodied "you" component is made more certain by being subtracted from the situation. A possible meta-reading of these ads points to the gulf between the public face of science and its benchtop reality, where handsome, smiling spokesmodels represent the last link in a chain, the face that appears on the news, or, as Sismondo shows ("Ghosts"), the name that appears on the scientific paper to lend it academic credibility.

More recent ads also tend to espouse what has come to be called the "You sell" in advertising, as coined by Steve Maich and Lianne George. If the "hard sell" is about the object—described by Cook as "a man in a suit, standing in front of a pile of carpets, talking loudly and directly to the camera about low cost, limited availability and guaranteed reliability" (15)—and the "soft sell" is about a lifestyle to aspire to, then the "You sell" is about the subject (here, the agent). The essence of the "You sell," George and Maich explain, is that it attempts to sell all things mass-produced by making them seem highly personalized, and a celebration of the subject's uniqueness (*Ego Boom*). Dell Computers states in its "Purely You" campaign: "We don't make technology for just anyone. We make it for only one. You"; Burger King states that you can "Have it your way"; Ford states that "Everything we do is driven by you"; and Nescafé states that "It's all about you" ("It's All About"). The Stratagene ad similarly features the word "personal" or "personally" ("Welcome to your own personal world of QPCR," for example, and "Personally priced at

$24, 995") no less than four times. It also repeats the phrase "designed just for you" in at least three different ways. The irony of the "You sell" generally, according to Maich and George, is that a celebration of uniqueness is to be arrived at through the mass-produced. The irony of the "You sell" in ads for scientific research products is that mass-produced tools can be "designed just for you," and that novel science is to be arrived at through the mass-produced (and that presence is best achieved through absence).

The agent pictured in recent ads is also a very different kind of agent from the one implied by the copy in ads that feature only images of the apparatus. In ads that feature images of the apparatus, rather than a person, the agent is often portrayed as anxious about his or her ability to perform physically in an experimental scene. The copy in such ads, as described earlier, tends to revolve around reducing the agent's anxiety and making the experimental process more certain. When agents are pictured—a more recent phenomenon—they are invariably smiling and relaxed. These smiling, relaxed agents also tend to be pictured in a kind of contextual vacuum; the background scene is minimal, as in the Stratagene ad, or completely blank, as in the Pharmacia Biotech ad campaign. The impression is that these agents are smiling and relaxed because they are no longer involved with the messiness of physical benchtop work. In the TransnetYX ad, there are many reagent containers in the background scene; interestingly, the agent pictured in this realistic laboratory scene appears quite miserable. Taken as a whole, these ads link happy agents with reduced physical involvement in the work of scientific research.

There appear to be two messages in the ads, then: firstly, that the embodied work is so simplified that all *you* do is press a button, like the researcher in the Stratagene ad, listening to music and eating his lunch while science happens; and secondly, that the embodied work is so simplified that you can hire a button-pusher. Both options allow the agents in the ads to focus on the work of "science" proper: the linguistic work of writing papers, presenting at conferences, writing grants, and discussing the data with other scientists, and generally thinking about science. Whether scientists themselves would agree that this *is* the real work of science is another question, and one that I do not have the means of answering here. The separation in the ads between benchtop, button-pushing work and language work that enables researchers to focus on science is also a somewhat artificial one, as grant and experimental report writing is done in the aid of real science. The other problem with the way science is portrayed in the ads is that none of them acknowledge that often the language part of the work is handed over to others as well, in which case the writing itself is not the real work of science either. The notion of real science, and real scientists, thus begins to seem elusive.

If we return to the agents that are portrayed in the ads, however—who are doing the writing themselves, but who are not doing any of the benchtop work—then questions also come up concerning their abilities to speak for science if they have not directly experienced the experimental events they are writing about. What might this kind of agent depicted in the ads be missing by not having engaged in the physical practices of research, by not having handled the cell cultures, looked in a microscope, measured out the reagents, held the test tube up to the light, seen the bands on the gel? What might this type of agent be missing by having pushed a few buttons and walked away, or by having given the research to a company like LabOnWeb.com? An exploration of this question will also help me further refine the nature of the relationship between action and language in my triad.

Action and Language

The agents in more recent ads are portrayed as "interactional" experts, according to Collins and Evans' terminology, steeped only in the linguistic practices of a domain, divorced as much as possible from physical benchtop practices. The way in which Collins and Evans conceptualize interactional expertise implies that embodied experience of research objects need not constrain linguistic research practices. The idea of having someone who has never participated in, or even witnessed, a given experimental process having linguistic competence in the domain (an implied message in the ads as well) is reflected in a thought experiment discussed by Collins and Evans, which the authors use to argue that transfer of scientific knowledge can happen through language immersion alone. It involves a "spy" who pretends to be an inhabitant of a town that s/he has never visited. This spy is thus equipped with only the language component of my action-attention-language triad. Would this spy be caught out in a conversation with a native inhabitant of the town? Collins and Evans claim that the spy could potentially pass as an inhabitant if s/he has been trained to pass via linguistic immersion for a year among exiled expatriates (43). The authors imagine that the exiles would be able to impart to the spy nuances of the local vernacular and all of the information about the town that the spy would need in order to pass. Collins and Evans thus propose that "physical experience of the town would not be important since everything that could be said as a result of physical immersion could be said by the expatriates" (43).

The main objection to Collins and Evans' argument, which the authors themselves bring up, would be that genuine inhabitants would possess knowledge unavailable to the spy, gained through physical immersion in the town, and that they would be able to have different kinds of conversations as a result. In other words, agents involved in both physical benchtop and linguistic research practices, as exemplified by the agent in the TransnetYX ad, would possess knowledge (and, presumably, linguistic skills) unavailable to agents who push a button or pass the work off to someone else. The authors do address the possibility that the spy might fail a kind of "Turing test"[41] compared to a native inhabitant if the interrogator was likewise an inhabitant (43). They dismiss this possibility, however, by claiming that immersion in the language alone would be enough to pass the imitation game.

Although I can readily accept that a spy trained for a year by expatriates would be able to hold a conversation with a native inhabitant[42] (especially compared with someone who has not spent any time among the expatriates), I wonder where the spy's linguistic abilities might fall short compared to those of the inhabitants. That is, I wonder what the agents depicted in the ads might be missing, in terms of linguistic abilities, by being several orders removed from physical interaction with the objects of scientific investigation. Dreyfus and Todes would probably hypothesize that someone equipped with only the terminology of a scientific domain would be missing critical information that comes from experiencing in a bodily way what various objects can and cannot do in an experimental scene. The same could potentially also be said of the technician whose work has been reduced to the push of a button, who is now also alienated from the space/time details of an unfolding and often unpredictable material process, which is no longer being experienced by

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[41] The Turing Test is a kind of imitation game proposed by Alan Turing. It is typically played with three people, a man, a woman, and an interrogator of either sex. The three players are separated into different rooms and converse only through text (computer). The object of the game is for the interrogator is to determine which player is the woman and which the man (Turing 433). Turing's variation on this parlor game is to make one of the players a human and the other a computer.

[42] Carol Cohn demonstrates that a year spent among nuclear defense intellectuals, immersed in the language, can indeed result in the ability to hold a sustained conversation with experts in the field. The question that I am interested in, however, would have required Cohn to produce communications aimed at military experts, where, presumably, Cohn's discursive abilities would have fallen short due to lack of any physical immersion in the domain.

any subject under the new regime of standardized and automated research. And then there are the professional science writers who have perhaps never been scientists at all, but just good technical writers.

Passing an imitation game is also different from shaping a given domain's discourse, which is more what is at stake when those divorced from the material world of research circulate its ideas and language, and where differences in linguistic abilities might become critical. Importantly, Collins and Evans describe the discourse of a domain as emerging from the activities of full contributory experts (from the agent depicted in the TransnetYX ad, who is being urged in the ad to become an interactional expert, rather than a contributory expert), written *by* those contributory experts, whereas interactional experts are depicted as participating in only a secondary kind of discourse with other experts. The authors do not address the possibility that interactional experts might, in fact, be the ones who write the formal accounts of scientific research. The ads that I discuss, and Sismondo's work on the commercial management of scientific writing, seem to be promising exactly this scenario of divided labour, however.

Direct physical experience of research objects becomes especially important in the acquisition of linguistic capabilities in science when we consider that the language component of the triad has traditionally been worked out in the presence of research objects. Amann and Knorr-Cetina's ethnographic study of how a group of molecular biologists makes sense of a gel electrophoresis autoradiograph through direct physical manipulation of the object shows the close relationship among action, recipient object, attention, and language. In this study, Amann and Knorr-Cetina are concerned with "the fixation of belief": the process of consensus negotiation in science involving "sense data" as evidence (133). The authors show how data gathered during experimentation may be problematic as evidence, and that seeing as a mode of relating to a research object may not necessarily bestow an "accent of truth" on that object; sometimes, researchers themselves do not know what they are looking at, and must negotiate such information (134). Amann and Knorr-Cetina's aim is to investigate this process. I use their findings to explore how direct interaction with the objects of research can accompany and perhaps aid the language-work of science, and to argue that interactional experts might be missing something by not engaging in this kind of materially-dependent process.

Observing molecular biologists at work, Amann and Knorr-Cetina importantly note that evidence talk is tied to objects (134). What the authors call "evidence fixation" is the social and embodied process of fixing in language the meaning of a given data artifact, and this process is centered on the material details of objects and agents' actions on these objects. Importantly, Amann and Knorr-Cetina show how data artifacts (data as physical objects which somehow bear the traces of experimental events) such as an autoradiograph in gel electrophoresis, become the basis for a sequence of practices that turn the artifact into a kind of workplace, enabling a conversation about the details of how the artifact came to be produced (138). Evidence fixation is thus an interactive process "attached to objects"; the exchanges that Amann and Knorr-Cetina witness in their ethnographic work are, critically, not just about the object, but rather "with" the object, where the object is manipulated further during the exchange as participants attempt to find their "way" on the autoradiograph film and make sense of what happened during the experiment (140). Typically, there ensues a sequence of questions related to "the geography" of the data artifact (note that 'He' is the inquirer and 'Er' is the agent who ran the electrophoresis and made the gel autoradiograph):

He	and, what is this?

Er	ha, over night ((exposed)), exactly like last time. And what do you see ((holds up film))? Nothing! ((Pause))

Where is the probe anyway?

Ni	simple enough, there is nothing on it

He	these are the markers, aren't they? Left and right. This is the probe?

Er	((remains silent))

He	this is the probe?

Er	((remains silent))

He	which ((film)) does it compare to?

Er	((annoyed)) what do you mean, which does it compare to?

(Amann and Knorr-Cetina 142)

Importantly, making sense of the data implicates the agent's past embodied experiences with the recipient object, in terms of the actions performed on it and the effects these actions had. Amann and Knorr-Cetina note that, during such exchanges, participants attempt to glean "non-obvious conclusions" from the data via an imaginative reconstruction of "the procedures through which these outcomes have come about" (146). Amann and Knorr-Cetina call this kind of discursive sequence "the procedural implicature device" (146). The aim is to make explicit eyewitnesses' experience of an experimental process, where the author of the artifact is questioned "as a living archive" of the space-time details of the process (146). The interrogation thus clarifies and re-produces the physical history of a phenomenon (148), the physical history of the agent-object relationship. The purpose of this kind of exchange, the authors go on to propose, is not so much agreement as it is to produce "not previously obvious" observations about the phenomenon (152). What Amann and Knorr-Cetina call "the history of the phenomenon" can therefore also be conceptualized as the history of embodied coping associated with a specific object (the motor-intentionality of the object). Critically, the interactional experts depicted in recent ads would not be able to act as a "living archive" of the experiment, and hence would not be able to extrapolate non-obvious features of the phenomenon, features inextricably tied to the history of embodied coping.

Several studies of real-world interactional expertise also suggest that direct physical experiences might affect the ability to talk and write about science. I want to point to some of these studies as a way of thinking about the relationship between benchtop and language activities in science. Although these studies are not directly about science, the observations made by the authors can be used to explore issues around interactional expertise. Psychologists Lauren Holt and Sian Beilock, for example, argue for the importance of bodily experience in shaping linguistic abilities. They are interested in language comprehension and how our responses to written texts are related to our embodied experiences of the world. In their study, expert athletes, novices, and non-athletes were asked to respond to sentences describing sport-specific situations. When asked whether a pictured item occurred in the preceding sentence, the athletes with embodied knowledge of the item responded quicker than members of the other two groups (694). The authors thus hypothesize that representation of objects and events appears to be linked to sensorimotor information regarding possibilities for action (696).[43] Holt and Beilock argue that, if language processing were strictly amodal and propositional, as opposed to tied to embodied experience, there would be no difference in the reactions of those who have embodied knowledge of the described situation and those who do not (694). Importantly, these findings imply that individuals who have not had the opportunity to acquire embodied experience might have less differentiated conceptual-linguistic representations (699).

In a study based on the imitation game described earlier, learning researchers Theresa Schilhad, Gudlaug Fridgeirsdottir, and Peter Allerup explore the relationship between bodily experience and linguistic capabilities, likewise finding that "personal experiences make a linguistic difference" (1). The authors wanted to complement grounded cognition studies—which suggest that learning is dependent on embodied experiences and that language comprehension activates motor programs in the brain—with a study of complex real-life verbal exchanges. Schilhad et al. thus set out to investigate whether midwives who have not given birth (as the interactional experts) could pass as mothers when questioned about embodied experiences such as pregnancy and delivery. Despite the fact that both midwife mothers and midwife non-mothers have contributory expertise in the practice of midwifery, the midwives who have not given birth themselves describe the birthing experience in such a way that midwife mothers, acting as judges, can tell the difference. This difference exists despite the fact that the midwife non-mothers have the same birthing terminology at their disposal. The authors thus importantly conclude that bodily experiences are somehow transmitted to language and to the written output (12). This study speaks against the idea, which philosophers of science Evan Selinger and John Mix attribute to Collins

[43] See R. A. Zwaan et al. for a cognitive linguistics-based discussion of embodiment and the comprehension of language.

("Interactional"), that someone who has not had any embodied experience with the practice of surgery, but who has been socialized in its terminology, would respond appropriately when a surgeon refers to an aspect of a delicate operation (Selinger and Mix 147). Like the previous study, then, this one also suggests that the agents depicted in science ads, urged to walk away from laboratory work in a variety of ways, might be missing something when it comes to talking and writing about science.

A last example of interactional expertise involves interpreters working in the steel trade. Production engineering specialist Rodrigo Ribeiro analyses the case of three Japanese-Portuguese interpreters. Ribeiro is interested in the transfer of technological information, and is attempting to address what he states is a lack of empirical evidence concerning real-world interactional expertise. The nature of the interpreters' job requires proficiency in the language of the steel trade without having to physically do the job. The interpreters do work on site, however, and have access to the practices they communicate about. To distinguish between this type of expert and one who is completely dissociated in space and time from the practices of a domain, Ribeiro invents the term "physical contiguity" to describe proximity to the work without bodily involvement (713).

I like the term "physical contiguity" because it allows for a continuum of second-hand involvement with the material side of research work. I find this term is especially useful when applied to the tricky issue of multiple authors in scientific publications and their varied levels of benchtop involvement. Often, the senior researcher, who appears last on the author list and who leads the laboratory, does not do the work itself, but will still have frequent contact with personnel immersed in the work. Typically, the lead researcher's office is close to the laboratory, and s/he will frequently walk through to talk with students, postdoctoral fellows, and technicians who are doing the work. This kind of setup gives the lead researcher a lot of physical contiguity with the objects of research and with those performing embodied actions on them.[44] This additional qualification, physical proximity to practices without direct participation, is an important aspect of scientific work, and it calls for a further nuancing of interactional expertise. Not all interactional experts are alike, then, as those who hand the benchtop work over to others can still have physical contiguity with it. The agent who lets LabOnWeb.com do the "tedious" part of research has minimal physical contiguity, but the researcher employing Fujimura's grad student to do molecular biology might have a lot. Researchers might also have minimal physical contiguity with some aspects of the work, and be more physically involved with others. In the least, this additional term supplied by Ribeiro urges for more caution around how interactional expertise is being defined in specific instances.

Based on extensive interviews with the interpreters Ribeiro concludes that physical contiguity does facilitate the development of language skills (713). According to the interpreters interviewed, being able to see the machinery in use is indispensable in acquiring fluency in the domain. One interpreter says the following:

> . . . when someone says 'Look, you have to align with tolerance of zero point something' . . . I can see in my mind's eye the image of how this alignment should be performed, which tools are going to be used, the importance of the job, why he has to do it, the difficulties [of doing it] . . . If you only know [about alignment] in theory,

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[44] This was the set up at McMaster University, where I worked for five years in the immunology department as a research assistant, and remains the typical set up in many biomedical research laboratories. The lead researcher, Dr. Manel Jordana, would walk through the lab on an almost daily basis, and would frequently be present when a new piece of equipment was being used for the first time. Dr. Jordana therefore had a high degree of "physical contiguity" with the material part of research work.

> [if you have] never seen it done . . . [For instance] the person says 'alignment,' but what kind of alignment? . . . There are many types of alignment. To align a railway track, to align a wall (laughs). It is very different. So, when you *know the process*, have *seen and know* [the phenomena], [the] 'alignment' already comes to your mind. It is *that* alignment . . . [You] *know what it is about*. So, you are not, as I always say, lost in the conversation . . . (qtd. in Ribeiro 718, all formatting in original)

When prodded further by Ribeiro to elaborate on how, exactly, seeing the process affects linguistic abilities, the interpreter replies that, without the physical contiguity, "[It would be] simply a word, in certain ways, even a vague word," (719, formatting in original). When Ribeiro asks whether having someone else explain the process using drawings would accomplish the same thing (the scenario Collins and Evans describe when someone spends time among exiled expatriates), the interpreter states that this kind of second-hand explanation would "not give an idea" of the material process (719). Like Amann and Knorr-Cetina, Ribeiro also notes that physical contiguity allows the interpreters "to retrieve information about past events" (719). Critically, Ribeiro states that even with physical contiguity interpreters' linguistic skills did not match those of contributory experts (713). Not surprisingly, the interpreters also found it more challenging to interpret in areas "where there was nothing to see," in domains such as informatics (720). Applied to the case of science, Ribeiro's study indicates that when agents are separated from benchtop practices involving concrete objects their abilities to contribute to the discourse of their domain (especially with information that is new) might be affected.

Ads for research products seem to be promising a scenario where there is increasingly nothing to see for the interactional experts participating in the public discourse of science.[45] At the same time as ads for scientific research products point to the split of full contributory expertise into practical expertise on the one side and interactional expertise on the other, they also potentially point to some of the linguistic effects of this split. In the Pharmacia Biotech advertisement from 1997 that features the endorsement of immunologist Doug Burtrum, Haiku writer from New York, and urges the viewer to "push a few buttons and walk away," the copy states, "Doug now stains electrophoresis gels with the push of a button." The font and the coloring of the copy are not uniform, however; "Doug," "stains," "gels," "with," and "button" are all enlarged and highlighted in red. The effect is that the viewer focuses immediately on the enlarged red words. Put together into one sentence, the copy thus reads more like: "Doug . . . stains . . . gels . . . with . . . button." The copy has thus been reduced even further, down to the minimum words necessary to convey meaning, reflecting the subject matter of the ad. The automated gel electrophoresis staining machine being advertised has reduced physical coping down to the push of a few buttons; accordingly, the copy has also been simplified to the minimum amount of words that still capture what was done.

This ad thus potentially points to the reciprocity among action, attention, and language. If all Doug is doing is pushing a few buttons and then taking his body and attention elsewhere, what will he be able to say about the experiment? Put simply, if Doug

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[45] In an example of how in some scientific domains there may be less to see, Fujimura recounts how embryologists used to work closely with sea urchins in order to "get their eyes used to" their materials in order to observe any significant changes (Crafting 115). Now developmental biologists work with clones of organisms. Fujimura thus notes that "[t]he messiness and randomness of the organism are gone, but so is the 'object' of study" (115).

does not *do* anything (or very little), will he be able to *say* anything? Studies that look at the effects of physical experience on linguistic abilities indicate that making sense of a complex spatio-temporal situation may mobilize a kind of motor program in the mind for dealing with a given object, and that this will affect how language is used. By extension, then, the reverse might hold true as well, that if there is less of the material world to engage with during experimentation, there might be less to say about it. Further study is needed, however, to explore whether ads for research products are reflecting a changing pattern of research work, or whether these ads are merely promising such a scenario of divided labour. Sismondo's study on the commercialization of research work and the ghost-writing phenomenon indicates that some division of labour is happening ("Ghosts"). Further study is also needed to explore whether interactional agents' linguistic abilities might be affected through not having much physical contiguity with the scenes of experimentation.

To conclude, I return to Collins and Evans' and propose that their use of H. G. Wells' story "The Country of the Blind" becomes salient here for reasons other than those intended by the authors. In the story, a climber falls down a mountain and discovers an isolated civilization whose members have all gone blind. When he attempts to converse, he finds that the inhabitants cannot understand him; "the names for all the things of sight had faded and changed" (qtd. in Collins and Evans 80). Collins and Evans observe that as the people adapted to their blindness their whole way of life changed, including their language (81). The authors' point is that the climber could have acquired enough interactional expertise through immersion in the culture to be able to converse (81). My interest in the story lies in its suggestion that changes in bodily-being-in-the-world, in how people habitually interact with the material world, might, over time, alter vocabulary and how the world is described.

An important feature of Collins and Evans' theory of interactional expertise is that it can only be acquired "*to the level that the expatriates still posses[s] it*" (43, emphasis added). The authors surmise that in the absence of contributory expertise over a long period of time, interactional experts' linguistic abilities would become "distorted," as in a game of telephone (35). The nature of a specialist language, the authors go on to elaborate, is thus a function of the total material and social environment. If one removes the material activity seminal to the development of a domain's discourse, then "the language will change," the authors themselves conclude (35). It is therefore "those doing the thing," Collins and Evans state, the full contributory experts (the agent in the TransnetYX ad), who are responsible for the content of a domain's discourse (39). Ads for research apparatus, however, seem to be reflecting a shift in how science is done on a material level. There is increasingly less need for those with discursive skills to turn their body and attention towards the concrete objects of research. If this division of labour becomes pervasive, with interactional experts relating mostly with other interactional experts, will the vocabulary used to describe experimental events, and how those events are described, change as a result of experts' altered embodied status in the world, as in "The Country of the Blind"? My prediction is that over time the language of research will change, where the wording related to actions on objects will increasingly point to the names of companies supplying the simplified research objects, and to the names of CROs doing the work.[46] The critical question then becomes whether this is a

[46] A preliminary glance through the *Journal of Immunology* shows a steady increase in references to commercialized products. In 1950 (volume 64.1), for example, there were no references to commercialized products in the methods sections of the four papers I looked at. Not that commercialized products were never used in the course of experimentation, but they were not, it would seem, the kind of black-boxed kits so available today (the earliest ads were typically for glassware, cover slips, chemical reagents, balances, and the occasional spectrometer). By 1975 (volume 114.3), there are more references to products. A paper by Yoshida et al., for example, refers to how "serum was concentrated to 2 ml and applied on a Sephadex G-100 column (2.5 × 50 cm)" (916); a paper by Gelfand et al. refers to how "Cytochalasin A (CA) and cytochalasin B (CB)

potential problem and from what perspective. It will be important for STS critics to explore the reality of the depiction of scientific work in ads for research products, and, especially, to explore how minimized physical contiguity with the objects and scenes of experimentation might be affecting experts' abilities to propagate the discourse of science. Indeed, it will be important for critics to continually revisit the question of what it means to be a scientific expert in relation to both the physical and linguistic practices of a domain.

In the fourth chapter I address another element of modern research that promises to alter the spatio-temporal experience of science beyond anything previously experienced: the digitization of science through personal computers and the internet, where the (interactional) agent composing research accounts is not only completely dissociated from the embodied benchtop work of science, but also where the materiality of research objects, the nature of experimental scenes, and the nature of actions upon objects are radically reconceptualized. The relations among the components of my action-attention-language triad become further complicated when the recipient object is digital, but when actions upon it are framed in material terms.

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were obtained from the Aldrich Chemical Company" (919); and a paper by Ponzio et al. refers to how "[c]ell counts were performed with a Coulter Counter Model ZB, (Coulter Electronics, Hialeah, Fla.)" (971). In this volume from 1975, there was an average of seven references to commercialized products per paper (averaged, rather uniformly, over five papers). In a 2002 paper by Wiley et al. (that resulted from the work I did while a research technician at McMaster University in immunology research, for which authorship went to someone who had been involved with very little of the benchtop work, and only some of the work of writing the report—perhaps an early signal of my interest in the division of labour that I discuss here) there are thirty references to commercialized products and the companies that make them in a methods section just a page and a half long. The thirty references in this paper (which is not an anomaly, as a look at other papers in this volume yielded a similar number of references) thus point to changing patterns in how the work of science is being reported. Instead of describing how slides and samples were handled from a spatio-temporal perspective of the agent in the scene as in Smithies' paper, the author need simply write, "Intracellular cytokine staining was executed according to the protocol detailed in the Cytofix/Cytoperm GolgiPlug kit (BD Biosciences)" (Wiley 4996).

CHAPTER FOUR

Research on Digital Objects

In this fourth chapter of my dissertation, I explore scientific prose relating to research practices carried out through digital technologies, as this represents a further step in the removal of the researcher from the embodied experience of benchtop experimentation. As with many areas of twenty-first century life, scientific research is also moving into the digital realm. Although a significant portion of the scientific research taking place at the beginning of the twenty-first century is still in the context of situated laboratories and physical actions on concrete objects, much of the work is also rapidly moving into high-speed computers in a variety of ways. The terms for this shift vary—cyberscience, e-science, internet science, virtual science, digital science, and cyberinfrastructure—as do their precise definitions. Charting this shift in scientific research towards digitization is an interdisciplinary effort, engaging scholars across many academic fields, and this is reflected in the myriad notions of what the phenomenon means. Nicholas W. Jankowski, a new-media scholar and Fellow at the Virtual Knowledge Studio for the Humanities and Social Sciences in The Netherlands, acting as editor for a special volume of the *Journal of Computer-Mediated Communication* on the subject in 2007, predicted much future debate about terminology and parameters of inquiry (552). At the very least, the phenomenon in question involves the coupling of science and computers in ways that challenge previous conceptions of scientific work.

The above formulation of cyberscience is broad, and definitions accordingly vary. Self proclaimed "cyberscientist" Michael Nentwich, director of the Institute of Technology Assessment of the Austrian Academy of Sciences, and the first to publish a book-length study of the subject, gives an admittedly encompassing definition of cyberscience as all scholarly and experimental activities that are carried out in virtual spaces using networked computers (22). At the same time, Nentwich states that cyberscience is about more than the involvement of a computer at some stage in the research process, and most would agree; for Nentwich, the "networked" part of his definition is essential (22). Jankowski, on the other hand, defines "e-science" more specifically as including the following features: international collaboration; the use of high-speed computers interconnected by grid architecture; the use of computers in the visualization of data; the use of Internet-based tools; organizational structures based around the internet; and the electronic publication of scientific scholarship (552). According to the National e-Science Centre in the UK, established in 2001, the term "e-Science" likewise refers to "the large scale science that will increasingly be carried out through distributed global collaborations enabled by the Internet," and will include large scale data collection, computing resources, visualization technology, and the development of internet-based tools to facilitate scientific research (qtd. in Jankowski 551). For communications scholars David Ribes and Charlotte P. Lee, the terms "cyberinfrastructure" and "eInfrastructure" similarly point to "the networked information technologies" that enable scientific collaboration, data sharing, and publication (231).

My interests in cyberscience lie in scholars' suggestions that both the benchtop and language-work of science are radically reconfigured when scientific research becomes mediated through a computer interface. The shift to a knowledge infrastructure built in the intangible world of cyberspace can mean an altered role for agents handling recipient objects. Cyberspace offers, as sociologists Mike Featherstone and Roger Burrows state in the introduction to their edited text *Cyberspace/Cyberbodies/Cyberpunk* on cyber technologies and embodiment, “the ultimate possibility of the displacement of the material body from the confines of its immediate lived space” (2). Cyberscience thus raises questions about the relations between physical and language-related practices not merely because data become accessible and manipulable through networked computers, but because digital research tools are also altering the very nature of the recipient objects being acted upon, what it means to act on them, and the ways in which actions on objects are depicted in scientific prose. I thus tend towards definitions of cyberscience that also encompass the ways in which computer-mediated research practices affect the epistemic functions of research objects. I especially like Anne Beaulieu’s definition of cyberscience as “a novel, technologically-supported organization of knowledge and knowledge-production, in which the digital format and electronic transmission of data figure prominently” (636).

Scholars from a range of disciplines are beginning to take interest in social and technical phenomena associated cyberscience. As noted above, many of the observations being made have to do with the sociological aspects of science conducted digitally and how actors dispersed through networked computers carry out their work. In their study of how high-speed computer networks are changing their disciplinary field, for example, psychologists Thomas Finholt and Gary Olson argue that traditionally laboratories “as social organizations” have been equated with laboratories “as physical settings” (28). Citing Wilhelm Wundt’s laboratory at Leipzig, Humphrey Davy’s research for the Royal Institution, and B. F. Skinner’s Harvard lab, Finholt and Olson contrast this kind of “strong association between physical location and schools of thought” (linking individuals, historic periods, and specific places) with the social, technical, and epistemic dispersion made possible by computer networks, which allow researchers to conduct their inquiries “without regard to geographical location" (28). In this respect, cyberscience appears to bring researchers closer to shedding the contingent complexities of specific research scenes on the road towards what Gieryn describes as location-transcendent truths. Yet another term that tries to capture this transition to cyberspace is "collaboratory." Coined by William Wulf while at the US National Science Foundation, the term obviously merges "collaboration" with "laboratory," as collaborative computing scholar Richard T. Kouzes et al. note, defining a "center without walls" in which researchers can interact with colleagues, and access data, instrumentation, and even research objects in digital information repositories (40). Jankowski observes that in many ways the transition between a situated version of science and cyberscience is well underway, "and often transpiring without awareness or particular concern" (553).

Other scholars are noting more explicitly that cyberscience tends to change not only how researchers interact with one another, but also how they interact with the objects of research, which can now be accessed and manipulated digitally. Mary S. Morgan, a historian of economics who focuses on the epistemic functions of economic models, for example, notes that modern science is increasingly relying on a "technological interface" to mediate between researchers and the objects of their inquiries, both in terms of the interventions on objects and how those interventions are assessed (2). Anne Beaulieu, who studies the relations between knowledge and technology in neuroscience when it becomes "neuroinformatics," similarly observes that computers are being used in science not just to support existing research practices, but to radically reconfigure those practices; computers become much more than technical support for existing research protocols when the tools of informatics are used on digital research objects (636). Cyberscience thus appears to be transpiring in the realm of informatics, where there is increasingly nothing to touch, nothing to define in spatiotemporal terms through motor intentionality, and no concrete (material) object at the center of researchers' linguistic activities.

I use my action-attention-language triad in this chapter to help to make visible the ways that cyberscience represents the realm of the language component, where action means typing on a computer keyboard, and attention is focused on a screen whose content changes at the speed of thought. How can agents act on research objects which can be partly, or sometimes totally, accessed in the digital realm, or when objects only exist in a digital form? The idea that I will be exploring in this chapter is that agents separated from cyber-generated recipient objects come to act upon such objects increasingly through linguistic activities themselves, and that these linguistic activities can have a rather tenuous link to benchtop research practices. I therefore apply what others have said about cyberscience towards specific examples of science conducted in the digital realm. My analysis serves to further pin down what cyberscience means from the point of view of agents, their actions, the nature of recipient objects, and the altered scenes of experimentation. My particular emphasis, however, is on the link between how research objects are acted upon and how language is used to facilitate and describe these experimental practices.

This chapter thus concerns the shift from materially-situated research to cyberscience, the prefix "cyber-"[47] denoting that something (which typically predates the cyber revolution) has become computer-mediated, networked, and invariably dematerialized to some extent. Here, I look at how the prefix "cyber-" is changing research practices involving, in particular, mice, which I propose become "cybermice" when their bodies become mediated through

[47] The term is thought to have originated with Norbert Weiner's 1948 book *Cybernetics or Control and Communication in the Animal and the Machine*, where it was used to refer to the self-regulation of complex systems. The cyber- prefix trend, however, as I mention below, was supposedly triggered by William Gibson's novel *Neuromancer* in the 1980s (Straubhaar and LaRose).

networked computers. I look at two examples of how networked computers are changing research involving mice: websites that sell research mice and services, and a mathematical biosimulation of the non-obese diabetic (NOD) mouse, otherwise known as the "virtual NOD mouse," as discussed in an experimental paper by Yanan Zheng et al. I chose to look at websites for companies selling research mice because these represent the most common way in which the mouse body has become (at least partly) digitized[48]; I chose to look at the virtual NOD mouse because it is the first biosimulation of the mouse to be used in a research context and to be discussed in an experimental paper.

Corporate websites selling research mice and services mediate agent-object interactions, to various degrees of material distancing, through layers of digitization, where some (or even all) of the actions carried out on real mice are done in the company's lab by company technicians, and the data are delivered to the researcher over the internet. Mathematical modeling (using nonlinear ordinary differential and algebraic equations to model the behavior of immune system cells and molecules) of the NOD mouse, on the other hand, involves non-material (computer) interventions on a non-material (computer) object. My study is the first, to my knowledge, to look at such treatments of research mice in cyberscience. I argue, broadly, that in both instances computer-mediation changes how mice function as epistemic research objects. Something fascinating happens when I attempt to apply Burke's key terms of Dramatism, as defined in my Introduction, to the realm of cyberscience; it is no longer clear who, or what, the agent in the scene is; it is no longer clear where the primary research scene is taking place; it is no longer clear how agents are acting

[48] I also considered looking at the mouse genome project, which was completed in 2002 (Gunter and Dhand), for this chapter, but decided against it, as I am less familiar with large-scale databases than with the uses of mice in immunology research.

upon objects (the method of agency); and it is no longer even clear what it is that the agent did to the recipient object (act). This chapter thus problematizes the association between the action and language components of the conduct of science. What does it mean to act upon objects partially situated (and possibly entirely constituted) in cyberspace? What does it mean for digital objects to react in cyberspace? How can this reaction be witnessed? Can one still have a "rhetoric of immediate experience," which highlights experimental events as directly experienced by a particular person at a particular time and place, if the actions and objects of research cease to be concrete? What might such a paradoxical account of experimentation look like? How is the empirical nature of the evidence to be established?

I begin by looking at websites selling research mice and services. Suppliers of research mice are increasingly functioning like contract research organizations, and it is now possible to order research mice and services over the internet. Websites therefore serve as important points of contact between agents and mice. I propose that when the mouse body can be ordered online, agents act on mouse bodies partly through language, through the words that are entered on the online order form. The "dialectic of resistance and accommodation"(Pickering "Mangle")—the situated, material negotiation between what agents want recipient objects to do and what those objects actually do—which would normally associated with the mouse-object, becomes neutralized to some extent, depending on how much of the material interaction with the mouse object is done off-site by company technicians. For the agent interacting with only the website, the mouse body does not offer the same material resistance that it would in a situated experimental scene, where mouse flesh does not always yield to physical manipulation. The messiness of research is gone for the agent interacting with a computer, but so is the object to some extent—or, rather, the experience, and hence definition, of the object becomes significantly altered. I explore depictions of actions upon mouse bodies on selected websites, and look at how the language around mouse bodies defines the epistemic nature of the object.

With the virtual NOD mouse that I bring up in the second section of the chapter, the object becomes entirely non-material, and acting on it means acting on a computer program. Nonetheless, the authors frame their experimental paper in materialist terms, retaining many of the conventions of experimental prose that I describe in Chapter One. Although virtual experimental mice are a very new phenomenon, there is some theory in place, as exemplified by the work of Morgan, Monteiro, Beaulieu, and others, to talk about material versus virtual experimentation. My close reading of Zheng et al.'s paper serves as an illustration, then, of how this shift away from concrete actions on material objects actually works in the language of the experimental report, of what happens when action becomes language. I examine the specific linguistic maneuvers that the authors perform in their paper to transform a computer program into a dynamic, reactive mouse immune system. Importantly, with the virtual NOD mouse, what is actually done to the object no longer seems to limit what can be said about the object and what has been done to it; the language in Zheng et al.'s paper appears to revolve around what the computer model represents.

A potential problem with the virtual mouse as an epistemic object is that its ability to react in truly unexpected ways in an experiment has been largely minimized. Although traditional experimentation on real mice (involving real mouse bodies that are handled directly) always involves some removal of the potential for surprise, through the selection of experimental controls that constrain attention, as I showed in the second chapter, the object nonetheless retains at least some of its independence and mystery (as witnessed by Crabbe et al.'s paper); when constituted through a computer program, however, the virtual mouse arguably loses a certain degree of the mystery which accompanies real mouse flesh. What kind of information can therefore be gleaned from virtual mice? Using information theory, I conclude this chapter by examining this question.

Materiality in Cyberspace

I begin with the idea that research conducted in cyberspace will no longer entail the concrete actions on concrete objects that have critically shaped scientific prose (especially in the life sciences) for centuries. The term "cyberspace" is generally attributed to William Gibson's 1984 science fiction novel *Neurmancer*, where the author refers to the "consensual hallucination" of "unthinkable complexity" experienced by billions of people, a kind of "nonspace of the mind," with its "clusters and constellations of data" (qtd. in Nentwich 21). Cyberspace in this sense represents, in Featherstone and Burrows' words, "a Borgesian library of vast databases containing all a culture's deposited wealth, where every document is available, every recording playable and every picture viewable" (6). The distance between different locations in this cyberspace is measured in the time it takes one to select a destination. More generally, "cyberspace" has come to refer to a communal space of information in which the user holds an "illusion of control, movement and access to information" (Featherstone and Burrows 2). Importantly, cyberspace is both nowhere and somewhere at the same time; we can get to it, we can move around in it, but it lacks location, as Kevin Robins, cultural geography scholar and contributor to Featherstone and Burrow's collection, notes (135).

In the early days of Western society's mass migration into cyberspace, which I am delineating as the early 1990s, following the commercialization of internet services,[49] scholars made many predictions about the possible impacts of computer-mediated communication (CMC) on human life. Many of these predictions had to do with overcoming the constraints of material existence. At the time of Michael Benedikt's edited collection *Cyberspace: First Steps*, published in 1992, Nicole Stenger, a computer animation artist and research fellow at MIT's Center for Advanced Visual Studies, refers to cyberspace as "the new bomb, a pacific blaze that will project the imprint of our disembodied selves on the walls of eternity" (51). In this version, cyberspace opens up "an infinity of space" for our normally limited minds and bodies to play with (51). A large part of the early excitement around cyberspace came from the feeling of escape from the constraints of materiality, in particular from the constraints of embodiment. Featherstone and Burrows likewise note the tendency in many of the essays in their collection (Balsamo; Heim; Holland; Lupton; McCarron; Tomas) to imagine "embodied subjectivity" in cyberspace as "more varied and flexible, surpassing the 'horizons of the flesh' and constraints of the 'physical' body" (12). Early feminist critics of cyberspace were especially prone to focusing on the pleasure, and potential power, associated with escape from materiality, a position popularized by Donna Haraway in "A Cyborg Manifesto," wherein the author describes, and celebrates, perpetually unstable categories and relationships (181). Inspired by Haraway, Claudia Springer, similarly writes about the "thrill of escape from the confines of the [sexualized] body" (306). For scientists doing research in cyberspace, and on digital objects, the experimental scene would thus (according to the above descriptions of cyberspace) be very different from the

[49] Commercial internet service providers (ISPs) emerged in the late 1980s and early 1990s, and the Internet became fully commercialized in 1995 (Moschovitis et al.).

one I discuss in my first chapter; in this new experimental scene, the agent potentially achieves a kind of disembodied omnipotence over digital research objects.

One might therefore imagine that the allure of such disembodied omnipotence would be especially compelling in science, where experimentation on material research objects has often proven recalcitrant, unpredictable, and frustrating. Recall Crabbe et al.'s findings, which demonstrate the difficulty of standardizing mouse bodies as scientific apparatus among many dispersed agents. In cyberscience, research objects can potentially occupy "worlds ambiguously natural and crafted," to use Haraway's wording (149), and can escape the materiality that seems to often stand in the way of knowledge, rather than contributing to it. So what have been the effects of CMC on scientific practices? Has cyberscience been able to overcome the material constraints of situated research on concrete objects? Yes and no; and therein lie many of the challenges regarding linguistic research practices dealing with cyberscience, in particular the writing of experimental prose, which must often reconcile the simultaneous materiality and non-materiality of practices and objects.

Cybermice

Website storefronts for companies selling scientific research mice and services are currently the most common way in which research on mice has become mediated through networked computers. In 2008, approximately 2.5 million JAX® Mice were distributed to 16 000 investigators in at least 60 countries, according to The Jackson Laboratory (TJL) website, representing the largest supplier of reseach mice, and millions more were distributed by Charles River, Harlan, and SAGE. With the ubiquity of internet-ready computers in research labs, most researchers will acquire their mice and information through these websites. It is not simply the reliance on networked computers and a complex cyberinfrastructure that makes these websites an instance of cyberscience, however: the mouse as an epistemic object becomes transformed through the online interaction, and research interventions become quasi-material, in the sense that online linguistic activities can make things happen to mouse bodies which are located elsewhere.

Before I delve into how mice are transformed through the digital interaction, here is what can be done through the websites: inbred (genetically standardized) mice can be ordered off the shelf, as can genetically engineered mice with a variety of conditions; custom-made genetically engineered "knockout" mice with selected missing genes can be designed and ordered; "preconditioning services" can be ordered (Charles River; TJL; Harlan), where mice are fed special diets and monitored up to a specific age before being shipped to the researcher; specific surgical manipulations can be ordered, to be performed on the mice by the company's technicians, before the animals are shipped; imaging services, such as magnetic resonance imaging (MRI), positron emission tomography (PET), and digital x-rays, can be ordered done on mice (Charles River); select mouse tissues, cells, extracted fluids, DNA samples, as well as mouse embryonic stem cells and embryos can be ordered, where "[e]ach shipment contains enough embryos to recover at least two pair of mice and includes detailed recovery protocols" (TJL).

In addition, researchers can order various actions to be carried out on mouse bodies at the company's lab by company technicians, further removing those engaged in linguistic research practices, such as experimental report writing, from the situated contingency of mouse materiality. On the moderate end of the contract research spectrum are the various "study-ready," "preconditioned" mouse models of human diseases. These are mice which have been transformed through various interventions into particular sorts of diseased bodies, at which point they tend to be called “models,” with the word “mouse” often being dropped from the nomenclature. The marketing strategy used on the websites is that when preconditioning (the making of these custom-diseased mice) is done by the company researchers can save “space, time, and labor costs” (Charles River). Some examples of these study-ready induced models include a diet-induced obesity model (DIO), an “aging service,” an Alopecia Areata mouse model, and a VCD-induced model of menopause, just to name a few. The DIO model means that “rodents are fed diets specified by the client for defined periods of time prior to shipment” (Harlan). Jackson states, for example, that it can “create custom DIO mice to order based upon your unique specifications (strain, gender, diet, length of feeding, etc.).” For the aging model, the company “can hold and care for these animals and deliver them to you at the point in time that you determine” (Charles River). In Jackson’s Alopecia model, mice receive a diseased skin graft and are then aged for 10-20 weeks before either being shipped to the researcher, or evaluated by company histopathologists. In Jackson’s menopause model, mice are treated with the industrial chemical “4-vinylcyclohexene diepoxide (VCD),” which accelerates the loss of ovarian follicles; the researcher receives the mice in the perimenopausal or menopausal state. Finally, on the more extreme end of the contract-research spectrum, the researcher can order only those fluids, extracted tissues and cells, and even DNA portions of interest. The Jackson website specifically markets this type of research object as designed “for those of you who don't need a whole mouse.”[50]

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[50] On the standard Jackson order form, there is space where the user can specify additional “special instructions” or “surgical modifications.” There is an extensive list of the possible modifications that can be ordered, including the following: “adrenalectomy,” where the adrenal glands are removed (for $23.50 per

In the online negotiation, the desired actions upon mice can simply be ordered and done by others. For those doing the ordering, who will have no physical contiguity with this part of the work on mouse bodies, the mouse-object potentially comes to seem composable (and controllable) through language itself. On the SAGE website, for example, on the "SAGEspeed™ Custom Services" page, a cartoon panel depicts the process of constructing a custom-made transgenic mouse strain. The subheadings "Design," "Develop," "Establish," and "Deliver" describe this process (there is a similar image panel on TJL website, featuring the words "Find," "Order," and "Support," where finding means searching the Jax database "for the mice you need," which can then be ordered online, along with any support that you may need for your research). The first step involves designing your mouse. "Call or click to let us know what you need," the website states. If the user cannot find the desired mouse in the company's "off-the-shelf portfolio," the website states that "a SAGE specialist will work with you to design a model perfectly suited to your research." Development entails making a "founder animal"; establishment means that founder animals are "carefully fostered to maturity"; and delivery means that "final models" (note, that the object is no longer a "mouse" at this point) are delivered via "stress-free" travel to the researcher.

When website users click on the "Request Quote" button, they are taken to an order form that first asks them to specify the "project type" (gene "knocked out," "knocked in," or "other, please specify") and the type of organism (rats, mice, or other). Next, the order form asks for the "target gene information" for the gene that the researcher wants added or

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mouse); the insertion of a catheter in the carotid artery ($102.40/mouse); castration; hysterectomy; intrathymic injections; microchip insertion; olfactory bulbectomy (where the olfactory bulb responsible for smell, located in the forebrain, is removed); and a subdiaphragmatic vagotomy (surgical resection of the vagus nerve), to name but a few. The user can also order specific fluids, tissues, and cells. There are over fifty different types of mouse fluids and tissues that can be ordered, including bile (at $15/mouse), the brain stem, placenta, fat, feces, skin, and the pooled serum of several mice, again, to name just a few.

subtracted from the mouse's genome (this is done at the embryonic stage). "If genomic sequence is not publicly available," the order form asks the researcher to provide the sequence of the gene that is to be added or subtracted from the mouse. After users enter company affiliation and contact information, they click on "submit" for a price quote. If everything checks out, they wait for confirmation that the animal—or, rather, the "model"—has been made and either request shipment or order additional research services online. The additional research services that can also be ordered online, to be done at the company's lab by company technicians, are vast, and in effect mean that, if they choose, researchers need never handle any part of the mouse other than final data. When the company takes over the work of handling the mice, it is functioning more like a contract research organization (CRO) than a supplier of products. Historian and philosopher of science Philip Mirowski and economics scholar Robert Van Horn, who look at how CROs are "re-engineering the goals of research" and definitions of authorship, predict that considerations related to "improving the bottom line" will change the nature of scientific research, "extending commercialization into the process of research and not just its products" (524). The authors predict that CROs will appear wherever research processes can be outsourced (537). Websites selling research mice and services represent one concrete example of the outsourcing of research processes. I discuss shortly how researchers are urged on the websites to participate in such outsourcing, and how the language on the websites mirrors that of the ads discussed in Chapter Three.

The online ordering process can thus be conceptualized as a kind of digital-genesis instigated through a linguistic cyber interaction, an order form, from which embodiment that would not otherwise have happened comes into the material world. But this embodiment remains attached to the order form. The object is, as the text states, "a model" first and foremost, custom designed, rather than a mouse per se. The nature of the object becomes symbolically constituted through the order form, and the precise words that were filled in on the order form. The process by which mice come into the world and are acted upon is not solely a linguistic process: actions are performed on real mouse bodies, just not by those who will be writing about these actions. Websites can thus represent important encounters between agents and objects, and, in instances where research services are also being ordered (and not just "preconditioned" mice which are later shipped), these websites may be the *only* points of such contact. If agents are physically removed from the experimental scene where these interventions on mouse bodies happen, then their experience of the object becomes the online interaction itself. The researcher therefore need never have any embodied contact with the mice at all in order to partcipate in linguistic research practices which make sense of what happened during experimentation. This is where the term "agent" becomes tricky.

Cybermice as Constructs of Language

It seems, then, that when the object is constituted through an online interaction, the constellation of words circulated comes to define the nature of the object because the object itself, from the perspective of the researcher doing the ordering, can remain entirely absent throughout the research process. It is the researcher's cyber-mediated experience of research mice, I propose, that turns the mice into hybrid creatures that are partly material and partly digital. The portrayal of mouse bodies on the website can thus greatly affect the epistemic status of mice as research objects. In that the mice encountered online are always incomplete, in a sense, they represent an excellent example of what Knorr-Cetina means when she states that some epistemic objects can unfold indefinitely and never come to resemble themselves. Building upon historian of science Hans-Jörg Rheinberger's notion of "epistemic things" as constantly in the process of definition, Knorr-Cetina attempts to characterize, in a broad, conceptual way, how objects function in knowledge practices. She proposes that "epistemic objects" can be characterized by their ability to keep unfolding into ever-new versions which highlight previously unseen aspects, as I explained in Chapter Two; they seem to perpetually lack the solidity and "completeness of being" of everyday objects and are constantly changing and acquiring new characteristics, displaying "an unfolding ontology" ("Objectual" 182). One could therefore say that the research mouse is never just "a mouse"; for one research group, it might be mainly a missing MAOA gene; for another lab, it might be a non-obese model of diabetes; for another, it might be a female with no ovaries. The possibilities for what the mouse can mean in any given lab are limitless because the mouse body can constantly be redefined based on the parts that are made salient through research activities, to the exclusion of everything else. On the websites, through the order forms in particular, this kind of selectivity is amplified. When Knorr-Cetina defined epistemic objects, she did not specifically have the cyber conditions of current science in mind; I thus take her terminology into a new kind of research scene and scientific text.

Mice ordered online are also "partial" epistemic objects in that, for the researcher, part (or even all, depending on what is being ordered online) of their embodiment has been constructed through a networked computer; the networked computer part of this research equation has the tendency to render the mouse-object even more "partial." Even if an actual mouse body arrives, it still becomes conceptually linked to material interventions that happened elsewhere, that were certainly real for the company technicians carrying them out, but not for the agent receiving the mice, at least not in an embodied sense. These offsite interventions thus acquire their reality mainly through cyberspace. The assumption of a previous embodied mouse existence, where things were done to its body that may reveal no traces when it arrives, is critical in constituting the mouse as an experimental apparatus, and this assumption comes from negotiations done in cyberspace; the object only achieves its status *as an epistemic object* when the sum of the agent's cyber and situated experiences are taken into account. The way in which the computer-mediated nature of the negotiation allows the researcher to talk about, and relate to, the mouse body (as composable through computer commands) renders it less material, less mouse-like, and more digital, more *cyber*mouse-like. Simulated online worlds (The Sims, Second Life) allow users to construct a digital materiality through computer commands; websites selling research mice appear to allow the user the same freedom from the constraints of the material world. At the same time, things happen in real life as a result of things that happen in these digital simulated worlds: users, for example, spend real money to buy virtual things.[51] The boundary between real life and simulated life is frequently breached in various ways. There is a similar interplay between the real and the virtual on websites dealing with research mice; there is some blurring, in particular, of where actions on mice take place, and by what means.

When part (or, again, potentially all) of the agent-object interaction transpires online, then, the words associated with mouse bodies, traded back and forth between the agent and the company through the online order forms, come to stand in for "the mangle" (Pickering), the give and take of material research work, except the mouse ordered online this way comes to seem less mangle-like, more amenable to control. As in virtual online worlds, where

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[51] For example, David A. Bray and Benn R. Konsynski note that in 2006 an average of $1,500,000 (U.S.) was traded daily among Second Life's virtual characters; Entropia Universe, an interactive virtual world popular in Europe, saw more than $160,000,000 (U.S.) traded among its 500 000 participants in 2005 alone (2).

typing fingers make things happen to characters, the online order makes things happen to mouse bodies.

Through the online order forms, and how they allow researchers to think about the mouse body—as a set of fields to be entered in the appropriate slots—the object becomes a constellation of words, such as gene for p53, MAOA, adrenal glands, carotid blood sample, etc., which keep themselves exploding into alternate meanings. Knorr-Cetina proposes that epistemic objects "structure desire," by pointing towards their own incompleteness, allowing researchers to constantly chase them through yet more incomplete representations ("Objectual" 185). The online interactions with research mice allow agents to act out their desires in ways that make (cyber) materiality seemingly complicit with agents' desires. For example, the Jackson website states that it can supply the user who does not need "a whole mouse" with "virtually any mouse tissue you need—meticulously prepared according to your specifications." These custom-ordered mouse bits are "harvested by expert surgeons with a 'can do' attitude," according to the website. The impression created through the phrase "can do attitude" is that the online interaction can make things happen to mouse bodies which would normally require considerably more tacit skill and delicate material negotiation.

Agents' desires are thus literally made (mouse) flesh, the embodiment of desire. The order form allows the precise constellation of words and mouse parts that make up this always-partial mouse-object to keep changing indefinitely as the agent's desires regarding the object change. The order forms are thus one way in which acting on the object now comes to mean acting through linguistic activities, rather than directly on the material world; the interactional expert discussed in Chapter Three, with no physical contiguity with the mice, thus acts on them through language. Critically, the research object constituted and acted upon in linguistic ways provides little resistance.

For the interactional expert, then, the mouse body as encountered through these online order forms becomes even more real, from the perspective of epistemic research goals, than any actual creature. The online-ordered mouse thus never resembles itself as a mouse. The lack of completeness and shifting epistemic nature of research animals is captured especially well by an image on the SAGE website, which depicts a rat standing on top of a tower of terms (from top to bottom: Bcrp, Mrp2, Mrp1, Mdr1a, KNOCKOUTRAT). The first four terms refer to specific genes. The wider possible reading of this image is that the animal as epistemic object has become defined by this constellation of terms; the rat travels with these terms to sites of research, and situated actions on its body are critically related to understandings of these terms.

Once commercial companies have made situated, material interactions with certain recipient objects unnecessary, they can serve up stylized images of the objects and experimental scenes with which subjects have lost physical contiguity. On the Jackson website (on the "Mice and Services" page), an image shows a young woman in a white lab coat and blue surgical gloves reaching for a lone mouse on a clear, plexiglass surface. The user is seemingly observing the physical work of research as it is being carried out by others (except the computer screen is not actually a window onto the scene, but a static simulacrum of a window onto a research scene; the scene depicted, *as a research scene*, never happened). The caption underneath this image reads, "Focus on your research, not your mice," and explains how the researcher can save "time, labor and space" by letting Jackson's "experienced and highly skilled" technicians perform the work in their "state-of-the-art facilities." Especially salient is the phrase "Focus on your research, not your mice," reminiscent of the ads I discuss in the third chapter that urge researchers to walk away from the laboratory benchtop to focus on linguistic research activities such as writing of the experimental paper.

Commercial websites dealing with research mice therefore appear to be extensions of print ads selling research products, and the language used is similar. Recall once again the TransnetYX ad from Chapter Three, where the pictured researcher, clad in protective lab goggles and white coat, states that genotyping mouse tails is not what he had in mind when he went into research. On the Jackson website, then, research also means something other than dealing directly with experimental objects (and interestingly, here too the white lab coat becomes synonymous with the physical benchtop work of science that can be passed off to someone else). On the Jackson website, the researcher is also being urged to *not* focus on the mice, to focus attention elsewhere.

Versions of this argument are made on all three of the other websites as well. The SAGE website states, "Leave the research model care to SAGE™ Labs"; Harlan touts the "Reallocation of labor and other institutional rescourses" when you let the company precondition the animals; and Charles River claims it "understand[s] that the quality of our research models has a direct impact on what matters most to you—your research," and that if you hand (at least some of) the benchtop work over to them, "you can focus on your work with the assurance that you are receiving high-quality, reliable models for your research." On the extreme end of the contract-research spectrum, commercial companies will carry out all elements of the research: "JAX® Services *can manage the entire process*, allowing you to focus on your research" (TJL, emphasis added). It is interesting that "the entire process" has become divorced from "research." This phrasing encourages the question, Process of *what*, exactly? Where does the "process" end and "research" begin? The impression is that "process" means the physical benchtop work of handling the mice, and "research" here might mean (although it is never made explicit) the linguistic work described in the TransnetYX ad from Chapter Three: writing an experimental paper, writing a grant, making a presentation, discovering something meaningful. The "action" that the implied researcher on the Jackson website will be engaging in will most likely be linked to language-work, but the language work will be about the actions done by others in a different research scene; a research scene which, importantly, this interactional expert has no physical contiguity with. Such agents can therefore be said to act on mice essentially in the process of writing itself.

The mouse body thus begins to matter only as data, as information, as numbers, and as a final graph, likely delivered over the internet, rather than as a dynamic exchange between agent and object. The mouse-object can thus come to seem closed as an epistemic object, and can be taken for granted as complete, rather than as an object that can react in unexpected ways.[52]

Once research involving animal models has been separated from the benchtop materiality of the work, and begins to mean language and thought-based actions upon

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[52] It is interesting to contemplate whether mice encountered partially, or completely, online do offer any resistance. The interactional expert would experience this resistance linked to mouse bodies through linguistic activities, however, and not through situated material resistance (where the mice might wake up mid-way into a procedure, or their lymph nodes are too small to remove, or a blood vessel collapses when a needle is inserted, etc.). What would it be like, then, to experience this material resistance second-hand through language only? What might such communications between the company and the researcher look like? The researcher might receive complicated data, or the news that the mouse ordered cannot be made in reality, or is not phenotypically viable and died shortly after birth. There might be communications between the company and the researcher around services which are difficult to perform on mice, where the "can do attitude" advertised on the website is revealed as predominantly a marketing strategy. These are perhaps some of the ways in which mouse flesh can offer resistance which will be experienced through a linguistic negotiation between different agents, rather than directly through motor-intentionality.

objects, the next step is to move the animal body entirely into cyberspace. Enter virtual experimental animals, with some fascinating consequences for a genre of communication, the experimental paper, which has traditionally been about situated material interactions, especially in the life sciences.

The Virtual NOD Mouse

In 2007, Yanan Zheng et al. published a paper in the *Annals of the New York Academy of Sciences*, titled "The Virtual NOD Mouse: Applying Predictive Biosimulation to Research in Type 1 Diabetes," in which the authors introduce "a large-scale dynamic mathematical model of the female non-obese diabetic (NOD) mouse" (45). The model, which is called "The Type 1 Diabetes PhysioLab Plaform," was developed collaboratively between the companies PhysioLab, Entelos, and The University of Florida, and is a dynamic mathematical representation, consisting of nonlinear ordinary differential and algebraic equations, of the cellular, biochemical, and tissue functions of a female NOD mouse. The model simulates what happens, on a cellular and biochemical level, in two main tissues associated with the pathology of diabetes: pancreatic lymph nodes (PLNs) and pancreatic islets. The model is a mathematical representation of cell trafficking between these two tissues, and the cell types represented include various types of T cells, B cells, NK ("natural killer") cells, dendritic cells, and macrophages. For each cell type in the model, processes such as cell proliferation, apoptosis, differentiation, activation, and immune-modulating molecule secretion are also represented mathematically. The molecules released by immune cells represented in the model include insulin, glucose, autoantibodies, and immune cell mediators such as IL-1, IL-

4, IL-10, IL-12, TNF-α, IFN-γ, and TGF-β, as well as various cell-surface molecules (Zheng et al. 49-50).

If experimentation done on animals is called "*in vivo*," and that done on isolated biological components "*in vitro*," then the virtual NOD mouse represents, as Zheng et al. term it, the next generation of experimentation, "*in silico*." (47). Interestingly, the authors initially call the model a "virtual" NOD mouse, with quotation marks around virtual (48), whereas I would shift the quotations to the word "mouse" instead.

The virtual NOD mouse would seem, then, to represent the escape from the constraints of materiality that early cyberspace scholars (Benedikt; Featherstone and Burrows; Robins; Stenger), writing in the early 1990s, predicted. In a 2006 interview for the *Medical Post*, Celia Milne reports the chief medical officer for the American Diabetes Association remarking that the virtual NOD mouse model "shortcuts the whole experimental process," that there is now a way "to do experiments in a day that might otherwise take months." Zheng et al. likewise introduce the model by saying that it was designed to "address challenges associated with translational research [from mice to humans]," and that it allows for a "speed and transparency" not possible in real mice (47-48). In their experimental paper, however, Zheng et al. retain all of the linguistic elements associated with situated material research, including the expected temporal constraints; the object is non-material, experimentation on it is non-material, but the larger discussion the authors attempt to join (concerning diabetes as modeled in real NOD mice) is phrased in materialist terms.

In language, the virtual NOD mouse functions like a real NOD mouse. We can see this in statements such as the following: "Early in the life of a virtual NOD mouse, various lymphocyte subtypes generated in the thymus and bone marrow populate the PLN" (51); "A reduction in β cell mass leads to a decrease in insulin secretion, impairment of glucose control, and eventually hyperglycemia, resulting in death of the virtual animal" (51); "The resulting early-onset virtual mouse develops diabetes at approximately 15 weeks of age, 4 to 5 weeks earlier than the average-onset virtual mouse" (51). This last statement is especially interesting because, earlier, Zheng et al. promote "the speed of *in silico* experiments," in which the whole disease modeling process takes only "minutes to hours" of program running time, allowing for "the rapid investigation of multiple hypotheses" (49). Presumably, time points in the virtual biosimulation are matched with time points in real mice, yet the authors do not state this. The data could have been phrased in non-material terms relating to the computer program itself: the program registers a switch corresponding to the disease state in 40 minutes for the parameter corresponding to early-onset NOD mice, as opposed to 2 hours for the parameter corresponding with the average-onset NOD mice, for example. Instead, the authors skip immediately in their discussion to what the computer program parameters *represent*, and speak in terms of disease progression taking the weeks it would in real mice. What makes it possible for the authors to reframe their experiments in materialist terms? One way to think about the use of materialist terminology in instances where actions and objects are actually non-material is as the strategic deployment of what sociologist Nigel Clark calls "the recursive gaze."

Drawing on Jean Baudrillard's notion of the recursive moment, where new and disorienting cultural productions draw their stability by retaining elements of the old, and Walter Benjamin's idea of society's retrospective gaze in the age of rapid industrial innovation[53] (the "'angel of history' perpetually backing his way into the future"), Clark proposes that cycles of digital innovation look to preceding practices to ground their productions (115). Even as innovations in digital media destabilize our sense of reality, they also conjure up the objects, bodies, and scenes which they are making obsolete (Clark 115). Thus, digital media can also be used to contain, subjugate, and reorder "a universe of refractory messages," where new forms of subjectivity and embodiment are often countered by those that preserve traditional demarcations (115). I use Clark's notion of the recursive gaze to explore how the concrete materiality associated with actions on objects is *linguistically* restored to this particular digitally-mediated object in Zheng et al.'s experimental paper. Here too, then, acting on the objects of research comes to mean language play; the mouse *body*, specifically, is acted upon only once the author begins writing, when suddenly manipulations of a computer program become "injections" on a female NOD mouse.

Zheng et al.'s paper encourages many questions, then, about the relationship between experimental actions on recipient objects in a research scene and the use of language to describe these actions. In the case of the virtual NOD mouse, action and attention during the research phase are turned towards a computer program, yet the language used in the experimental paper is about physical actions on tangible mouse bodies. Something odd happens with in such depictions; in the writing phase of research work, actions on virtual mice are suddenly reconceptualized in terms of what the computer program represents, rather than where a situated agent's attention would actually go during the simulation, towards a

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[53] The examples given include the first electric lightbulb, which was made to resemble a gas flame; mass produced utensils that were modeled on "organic forms"; and iron that was cast to resemble wood or leaves (Clark 114).

computer screen; nothing is stated about working with a computer program. The dilemma faced by interactional experts who need to communicate about experimental events but who have minimal physical contiguity with the material world of experimentation is amplified to an extreme degree in the instance of virtual experimental models. Along a continuum of physical contiguity with the objects of research, computer simulations would occupy the extreme position, which puts machines with a few buttons to push, in comparison, closer to the material end of the spectrum. The virtual modeling of experimental animal bodies may be so completely new that researchers do not yet have linguistic rules for it, and thus turn to traditional discursive forms to communicate about it, even at the risk of wild linguistic contradictions, as I discuss shortly.

Virtually Mouse-Like or Virtual Mouse

At issue in cyberscience involving virtual research objects and virtual interventions is how researchers frame the evidence generated with these objects *as empirical* in their experimental communications. Writing on models, simulations, and the kinds of epistemic objects they produce in science, Sismondo states that simulations occupy a problematic position as both theory and objects of research to test theories: “they are objects, as well as ideas” (“Models” 247). Science-and-technology-policy scholar Marko Monteiro, in an ethnographic study of scientists working with computer models of heat transfer in human prostate tissue, proposes the term “digital object” to denote the uniqueness of this kind of object in experimentation (336). Monteiro invents this term to try to conceptually locate the “materiality” of digitally-constituted research objects, which he proposes resides in how researchers handle the computer-generated data as part of the empirical process of evidence analysis (335). Monteiro suggests that digital objects acquire materiality when they are manipulated and handled in the course of experimentation (although not in physical ways), whereas I propose that virtual NOD mice acquire their materiality through language, much as mice experienced through a commercial website acquire their materiality (for the interactional expert, anyway) through linguistic activities about mouse bodies.

An important feature of virtual experiments that allows researchers to frame computer work as material benchtop work is that the language of computer programming is minimized or omitted altogether and the focus remains on the idea of mouse bodies being acted upon. Mary S. Morgan analyzes how "hybrid" experiments that use both material and computer-based interventions on model objects deal with controls, modes of demonstration, and material inputs and outputs—and how such hybrid models refer to things in the world. Morgan then characterizes scientific experiments based on how and where materiality figures in them. There are four types of experiments according to Morgan: ideal laboratory experiments on one end of the spectrum, where inputs, interventions, and outputs are all material (corresponding to my initial definition of the action component of my triad in Chapter One); mathematical model experiments on the other end, which feature mathematical inputs, interventions, and outputs; and two types of hybrid experiments in between, that blend material and non-material elements.

Experiments that have "quasi-material" inputs, "non-material" interventions, and non-material outputs are called "virtually" experiments (meaning almost material) (29). These, according to Morgan, are experiments that "use non-material resources to experiment on quasi-material objects" (10). An example would be a computer-manipulable 3-D photographic image of a biological structure (Morgan cites an experiment that cuts real bones into thin slices to generate such images, which can then be handled digitally to simulate what happens when bones fracture). In "virtual" experiments (as opposed to "virtually," or almost), the inputs are non-material, the intervention is non-material, and the outputs are non- or quasi-material (29). The example Morgan gives is the creation *denovo* of a computerized 3-D image of a "stylized" bone that looks like a 3-D stack of small squares; this image can also be manipulated digitally to simulate bone fracture, but the input for the image was computer-generated. Although both types of experiments focus on a method of demonstration that is similar to real-world experimentation, Morgan states that "the agent of intervention" has become mathematical rather than mechanical (12). Importantly, Morgan posits that such hybrid experiments *use* mathematical models, but are not *on* mathematical models (12). The fact that virtual experiments are not on mathematical models allows the language in Zheng et al.'s paper to remain focused on the idea that it is a *mouse* body that is being acted upon.

When I attempt to use Morgan's framework to characterize what type of object the virtual NOD mouse is (based on Zheng et al.'s description of the biosimulation), however, I encounter some difficulties in terms of where materiality ends or begins. According to the examples that Morgan gives, the virtual NOD mouse would qualify as a "virtual" model because the inputs, interventions, and outputs are all non-material. The data used to generate the model, however, are all from published papers that deal with real mice; the interventions are based on protocols developed on real mice; and the outputs are mathematical, but when graphed resemble those in papers on real mice. Furthermore, Zheng et al. propose that such "*in silico*" experiments be used to guide the development of protocols for *in vivo* and *in vitro* experiments on real mice (59). This is another instance, then, of the boundary between the real and the virtual, and what happens where, blending and blurring. The issue with Morgan's framework is how and where to draw the line between "non-material" and "quasi-material" elements in a hybrid model. Morgan also does not explicitly take into account linguistic treatments of these hybrid models, in which authors appear to have considerable latitude in how they represent their interventions and outputs, as more material rather than mathematical. I propose an extension of Morgan's framework for a category dedicated to linguistic treatment, which would acknowledge that modeling does not stop at the computer interface, but includes language activities as well, which can selectively emphasize the material, non-material, or quasi-material elements of the models.

The tendency to skip discursively ahead to the materiality (the mouse body) that the mathematical computer model represents becomes especially pronounced when Zheng et al. discuss the "interventions" carried out on the virtual mice. The purpose of the model is to see how virtual NOD mice respond to various treatment protocols. A common treatment protocol in animal models of immunological disorders, such as diabetes, is to administer exogenous immunomodulatory molecules, often molecules called "cytokines," which are released by the body's own immune cells to modulate disease progression. A cytokine that has received much attention in NOD mouse models of diabetes is one called interleukin-10 (IL-10). Zheng et al. mathematically model the addition of "exogenous IL-10" to the system, following a protocol developed by Pennline et al. in real NOD mice. When the authors discuss this intervention, however, they phrase it in materialist terms:

> IL-10 therapy was evaluated in virtual NOD mice using the protocol reported by Pennline et al. Based on this protocol, 1 μg recombinant human IL-10 was

administered daily to virtual NOD mice for 15 weeks, starting either at 9 weeks of age or 14 weeks of age via subcutaneous [under the skin] injection. (55)

The authors thus describe the computer intervention on a computer program as an injection under the skin of a female NOD mouse; the object, the intervention, and the output are all phrased in the language of real mouse bodies. Although Zheng et al. do refer to the "virtual" nature of the mice, they also simultaneously infer living animals through their language. The situated experience of dealing with a computer is thus selectively omitted from the experimental account. The recipient object acquires its materiality, then, when the researcher writes the mouse body into existence, and composes a narrative of situated embodied actions on mice and embodied reactions by mice.

The materiality of the mouse-object is further solidified in the experimental paper when the authors then graph "[t]he response of the average-onset virtual mouse to IL-10 therapy," and conclude that "the virtual mouse is protected by 1 μg/day IL-10 treatment administered from 9 to 24 weeks of age, but not by treatment from 14 to 29 weeks of age," and that the virtual NOD mouse shows "reduced insulitis . . . which preserves β cell mass" (55). Finally, the authors discuss the implications of their results solely in terms of what the model represents, dropping all terminology associated with the non-material, digital elements of the model: "The accelerated progression of type 1 diabetes in some animals within a colony may account for the failure of a fraction of NOD mice to respond to this generally efficacious IL-10 therapy" (57). The "animals within a colony" and "NOD mice" that the authors are referring to are actually of the virtual variety, yet there is no indication of this; for all intents and purposes, the discussion is about real interventions on real mice. How, exactly, the computer program can model "subcutaneous" injections of "recombinant human

IL-10" is never discussed. These are indeed experiments *with* computers, but not *on* computers, and Zheng et al.'s language choices demonstrate this fact. Noting how simulations do not easily fit as either pure theories or pure experimental phenomena, Sismondo importantly asks how we should understand objects "that appear to depend upon human agency, but have the solidity that we attribute to independently real structures" ("Models" 253), to which there is no simple answer.

Actions on Third-Order Objects

In terms of my action-attention-language triad model, the action-attention components can seemingly be reconceptualized and linguistically reframed in terms of the third-order objects being handled though the biosimulation. The first-order of actions on objects would be those associated with the materiality of the computer itself, fingers on computer keys and what the agent sees on the computer screen; the second-order of actions on objects would be the conceptual world of the computer program running the simulation and the commands that make it run; the third-order of actions on objects would relate to subcutaneous injections of recombinant cytokines in female mice with diabetes. The what-we-did part of the experiment, the action-attention component, therefore has several possible versions in biosimulations. The third-order representational version of what was done therefore takes precedence in the experimental paper over the first-order version that would have the authors discussing actions *on* a computer. The agent's experience of interacting *with* a computer would seem to be the wrong kind of material detail to include in an experimental account dealing with mice and diabetes, as it would point back towards the (wrong kinds of) conditions of experimentation.

For researchers working with digital recipient objects, the third-order of actions, the imagined natural systems being represented, are the ones around which their language revolves. Sismondo notes that in experimentation on simulations, the "ostensible object of representation is absent," which means that those working on and with simulations are trading in symbols ("Models" 255). In Zheng et al.'s paper, we see in more concrete terms what it means to trade in symbols. When the object (the diabetic female "mouse" itself, in Zheng et al.'s case) is absent, researchers seem to gain some flexibility in the symbols that are traded, allowing them to refer to third-order actions and objects. At the same time, the symbols take on a kind of material reality because they are constituted *through* experimentation, and can therefore seem, as Sismondo states, as more real than the theories to which they relate ("Models" 250). Symbols can also come to seem more real than the objects they represent. Monteiro's observation, after watching researchers handling computer-generated prostate tissue, that manipulations of digital objects on a computer screen become "just as powerful as those enabled by the direct observation of nature," becomes especially relevant in the case of Zheng et al.'s biosimulation (340). Which order of the action-attention component is discussed appears to depend on the audience being addressed.

In the instance of Zheng et al.'s *Annals of the New York Academy of Sciences* paper, the audience being addressed is clearly those working on animal models of diabetes, and more specifically those working on mouse bodies. This can be seen in the citations Zheng et al. list, which relate predominantly to experimentation on NOD mice. The audience is not those interested in the specifics of the programming code used to design the virtual model, as there is nothing in Zheng et al.'s paper that would let such an audience engage with the content. The audience is also not those interested in the electrochemical hardware components of the computer itself, as there is also nothing in the paper to engage this type of audience. Accordingly, then, Zheng et al.'s paper follows very closely the form, content, and prose style of experimental papers dealing with real NOD mice, adhering to a rhetoric of immediate experience.

The intended audience is made especially apparent in the data graphs that Zheng et al. display. There are three data figures, featuring twelve graphs in total in the paper. These graphs plot such parameters as "blood glucose," in "mg/dl," against the age of the mice in weeks; the number of cells in the PLN versus age; fraction of "CD4+ T cell[s] in islet" versus age; "β cell mass" in "mg" versus age; and "IL-10 dose" in "μg/day" versus "age at initiation of treatment" in weeks. The graphs also plot the results of published data from real NOD mouse experiments alongside the graphs generated from the biosimulation. In the plotting of biosimulation data alongside that generated from real mouse bodies, we can see what allows the authors to call the computer biosimulation a "female NOD mouse" at all: the level of congruence between graphed data points. Sismondo notes that for models to be useful, they must behave like the natural systems they represent ("Models" 249). But there are many ways in which the behavior of models can be judged to be like or unlike the systems they represent because behavior itself is not fixed. Thus, in Zheng et al.'s paper, the simulation behaves like a "mouse" because it produces graphs that look like those made from data on real mice. In effect, if the graph looks the same, then object can be called a virtual female NOD mouse.

Virtual Mice and Discursive Pleotropism

The way in which Zheng et al. refer to non-existent experimental events and mice is not an entirely new phenomenon; we do this all the time when we communicate, and in this sense there is nothing surprising about Zheng et al.'s descriptions of interventions on virtual mice. The novelty comes with context, in that we have not seen this specific usage of language in experimental papers dealing with animal research. The language of immediate material experience has been entrenched in scientific communication for centuries, and researchers dealing with novel phenomena (which resist easy classification as either material or digital) may have few options for how to handle such research in an experimental paper if they wish to partake in wider scientific conversations. The notion of concrete actions on concrete objects therefore continues to have a hold on scientific prose, even though some aspects of experimentation in the life sciences are moving into the digital realm.

Virtual experimental animals become especially challenging for researchers writing in an idiom, the experimental paper, which has been based on a rhetoric of immediate experience; those writing experimental accounts must now find ways to make virtual research seem relevant to a wider scientific conversation involving real mice and humans. I thus return to the idea of discursive pleotropism introduced in Chapter Two, to the idea that one term can accommodate many different material manifestations. Because the virtual NOD mouse model is meant, ultimately, to point to novel ways of approaching diabetes in humans, biological terms must now float between digital biosimulations, real mice, as well as between mice and humans generally.

Animal models in a general sense must attempt both situated material specificity (the rhetoric of immediate experience) and translational relevance to human biology. Virtual animals are used in the same way. According to Knorr-Cetina's formulation of epistemic objects, naming allows one to "bracket and ignore differences" and concentrate on how the name captures instead the sense of "identity-for-a-particular-purpose," rather than capturing stable thinghood ("Objectual" 184). In Zheng et al.'s paper, the name "NOD mouse" captures the sense of "identity-for-a-particular-purpose," which in this case is modeling of human diabetes; the thinghood of both real and virtual mice is defined by their use as models of human diabetes in a research context; aspects of these very different recipient objects that do not pertain directly to this (common) sense of "identity-for-a-particular-purpose" are ignored.

Thus, in Zheng et al.'s paper, there are references to "*the* immune system," "*the* disease," "*the* PLN and pancreatic islets," "*the* pancreas," etc. (emphasis added), as though there is only one immune system, one disease, or one pancreas that floats among virtual and real mice, and among mice and humans. Consider the following passage from the paper: "In this model, termed the Entelos® Type 1 Diabetes PhysioLab® platform, virtual NOD mice are constructed by mathematically representing components of the immune system and islet β cell physiology important for the pathogenesis of type 1 diabetes" (45). Here, "the immune system" and "type 1 diabetes" point simultaneously to the platform, implicitly to real mice, and also to humans. The authors are attempting both material specificity, when referring to female NOD *mice* (of either the real or virtual variety), and translational relevance between species when referring to "the" pancreatic islets that mice share with humans. Because pancreatic islets share a commonly defined "identity-for-a-particular-purpose" in real NOD mice, the virtual biosimulation, and in human beings, the term "pancreatic islet" can float, simultaneously pointing to something common to all these different recipient objects as well as to situational specificity. In the writing process, attention and language also become fixed on “identity-for-a-particular-purpose,” and the experiment becomes reframed in these terms. All of the ways in which virtual mice are not like real mice which in turn are not like humans are then ignored through omission.

A stable name—such as “diabetes,” or “NOD mouse,” or “T cell,” or “cellular infiltration”—can therefore enfold multiple versions of an unfolding, always partial, epistemic object. For Knorr-Cetina, computer programs aptly represent a typical example of an unfolding epistemic object, generally speaking (“Objectual” 184). Different versions of the program are made available under the same name, such as “Windows,” for example. In successive versions of a computer program, Knorr-Cetina explains, the precise constellation of elements gathered together under the same name will vary; some elements will be retained, some changed, and others dropped completely, but the name will remain constant throughout such epistemic unfolding (184). If we think of virtual experimental animals as updatable computer programs, then they also enable such indefinite unfolding, as new and different elements are gathered under the name “virtual NOD mouse.” Indeed, Zheng et al. refer to the “creation of alternate virtual NOD mice,” such as the early- and average-onset of diabetes versions (49).

In both the online order forms discussed earlier and in the virtual biosimulation, then, the agent ends up relating to the experimental mouse body as a selection of parts brought together through text. Importantly, in both instances, mouse materiality is rendered manageable through omission; the parts not named do not factor in epistemic activities. The unpredictability and mystery of mouse bodies, which sometimes unexpectedly react to things like the buzzing of Airstream helmets, can thus be ignored; it is difficult to imagine that all possibilities for embodied mouse reactivity can be programmed into the biosimulation. Each successive version of the virtual mouse literally becomes defined by the elements selected to be included in the mouse body—which cells and molecules are represented in the simulation. The virtual mouse, then, is controlled in a way that no real animal experimental system can ever be; the controlling variables are not obscure in such experiments, unlike in those involving real mice. As Sismondo states, simulations are, by definition, "manageable systems," standing in for unmanageable, "opaque" ones ("248). In Zheng et al.'s paper, the means by which mice are made more manageable, through careful selection of what becomes represented in the simulation, what is excluded, and how it is all described, tend to stand out. Zheng et al. explicitly state at one point that the virtual NOD mouse allows for a "transparency" not possible in real mice (48). When I discuss information theory, I claim that it is this level of control which potentially affects the kind of information that can be extracted from virtual experimental systems.

When scientists omit mention of experimental actions having to do with computers and computer programs, they may, at the same time, bracket and ignore differences in practice fields that do not easily fit together. The kind of discursive pleotropism discussed in my second chapter dealt with translations involving bodies and groups of individuals. In Zheng et al.'s paper, however, translations must also cross the material/non-material, or body/computer, divide. The concepts associated with the world of embodied animal research in immunology and those associated with the world of computer programming share very little cross-over, in terms of the objects and practices they imply. Zheng et al. deal with this mismatch between practice fields by almost completely ignoring the terminology and concepts associated with computer programming, even though it is a critical part of their research. For the most part, the language associated with digital practices can be ignored in discussions of the virtual NOD mouse. Researchers will always selectively leave out aspects of an experimental system from their writing which a community has agreed to take for granted and assume are equivalent (or irrelevant) between situated contexts, except that here Zheng et al.'s paper is the *first* description of the virtual NOD biosimulation, and the computer program is not a minor component of the system. Left out, then, are not just mouse parts, but also references to research practices that are not immediately relevant to mouse immunology. In the few instances that the worlds of computers and immunology do commingle, the result is invariably what I term "dystropic couplings," which defy the rules of the linguistic-practical domains that they join. Such dystropic couplings point to the difficulties associated with non-material interventions on digital objects and the multiple orders of actions that researchers must now incorporate into experimental prose (which is still based on a rhetoric of immediate, material experience).

Dystropic Couplings

Dystropic couplings attempt to make each term in the union do something that it cannot do in the linguistic-practical world in which it normally circulates. If the suffix "-tropic," from the Greek word *tropos*, means direction and turning towards or affinity for (OED), and the term "isotropic" in physics means uniformity of properties in all directions (OED), then dystropic couplings imply the opposite, that the melded terms somehow repel one another in different directions: they are oxymoronic. In the instance of biosimulation specifically, I use "dystropic couplings" to refer to the way the combined terms repel one another on a material level.

An example of a dystropic coupling is the title of a paper by scientific communications scholar Anne Beaulieu, "Voxels in the Brain." Beaulieu looks at how digital tools in neuroscience are changing the practices of the discipline and, by extension, the ontological, epistemic, and pragmatic notions of objectivity. "Voxels" refers to computer terminology associated with pixels, picture elements. When neuroscience becomes digital neuroinformatics, Beaulieu explains, digital tools (voxels) hold numerous types of data about the brain, stored in varied representational formats, such as drawings, measurements, and brain scans (643). When all of this data is compiled in digital information repositories, physiological and anatomical information can also be retrieved from these voxels. Thus, authors now write about how "databases will develop in which N-dimensional attribute lists will exist for each voxel in the human brain"[54] (qtd. in Beaulieu 643). To which Beaulieu comments: "Voxels . . . are then to be found in the brain? In a neuroinformatics context, yes . . ." (643). Although Beaulieu identifies that something interesting is going on when voxels are said to reside in brains, she does not offer a way of looking for similar meldings in the scientific literature, however. If virtual animals are to be used more in scientific experimentation, it may be useful to have a way of identifying such dystropic couplings.

The coupling of "voxels" and "brains" points to the melding of incommensurable (to invoke this tricky term from Kuhn) practice fields, which have different action-attention-language sequences for dealing with domain-specific epistemic objects. The phrase "voxels in the brain" makes each term do something that it cannot. Brains cannot contain voxels, and voxels cannot reside in brains. We see this attempted melding of incommensurable practice fields in Zheng et al.'s paper as well. For example, the authors write that "an average-onset

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[54] This quote comes from John Mazziotta and Arthur Toga's *Human Brain Mapping:The Methods*.

virtual mouse was calibrated to reflect the high level behaviors of the untreated female NOD mouse and its response to a set of published interventions" (51). Normally, one would not speak of "calibrating" mice; the concept of calibration comes from the linguistic-practical world of computer technology. Likewise, Zheng et al. state, "The validity of these [alternate] candidate mice was checked . . ." (52). Again, one would normally not speak of checking the "validity" of mice; the concept of validation is part of programming terminology, and refers to how closely the biosimulation program matches real world data and published interventions (Zheng et al. 48; Monteiro 349). Similarly, in order to represent pancreatic insulitis in the virtual mouse, "the pancreas is divided into 10 'islet bins'" (49). This division of a pancreas into "islet bins" only makes sense in the world of biosimulation. The following phrase likewise only makes sense in this specific context: "To implement exogenous IL-10 therapy in the platform" (53). One cannot administer exogenous IL-10, a biological molecule found in bodies, via an injection under the skin to a PhysioLab platform; yet in this context mixing the strictly biological and the technological into impossible relationships is allowed.

What I find especially fascinating about such phrasings is that computer terminology is applied to mice *that do not exist*. The action being referred to (checking validity) remains tethered to the linguistic-practical world of computer technology, but *what* it is that is being checked becomes reframed in terms of the third order object, a mouse, that the program represents. Such dystropic couplings will likely become more common with the increased use of biosimulation technology in life sciences research. A researcher interviewed by Milne remarks: "Imagine taking the human immune system and overlapping it over the NOD mouse, which we know so much about." Indeed, the chief scientific officer for Entelos (one of the companies that developed the virtual NOD mouse), Dr. Mikhail Gishizky, states they "want to open this [biosimulation] up to the academic scientific community. Hopefully this will lead to better understanding in human beings. State your hypothesis and test it," he proposes (qtd. in Milne).[55] As biosimulations spread, they will challenge traditional notions of what it means to do science, and how one demonstrates discursively that one has done science. Beaulieu observes, in her discussion of how neuroscientists are struggling with the collapse of terminology that happens when different knowledge systems come together, that there are currently no communal rules for how to handle the quasi-material objects of cyberscience (643). This confusion around what was done to what at times becomes evident in the discourse itself.

Materiality, Unpredictability, and Information

I now return to the observation I made earlier, that researchers working with virtual mice achieve a level of control that perpetually evades researchers working with real mice. Researchers achieve this control, however, at the possible cost of the complexity which

[55] The exact manner in which *in silico* research on virtual mice will help humans with diabetes remains vague, however, in many of the online communications about the platform, although the implicit message is that virtual mice represent a more direct line to the human body, presumably due to the transparency of the model. An online article in diabetes.org, for example, states that biosimulation "will help researchers translate the knowledge they obtain through the mouse model into information they can apply to humans with diabetes," because researchers "will be able to use the *in silico* model to study the onset of type 1 diabetes as well as how the disease advances. They can use that data to predict how people will respond to different treatments aimed at preventing or reversing type 1 diabetes." ("Going High-Tech" 20). The question that remains unanswered is how virtual mice are more translational to the human situation than real mice.

A blogger named Ellen raises this issue on the islet.org message forum, asking, "Can someone please explain how this will bridge the gap between animal research and patients with type 1 diabetes??" ("in silico"). To which a blogger named Klausen responds, "Thank you, Entelos, for a $10,000,000 approach to solving the problem of making rat research relevant to human diabetes." This is followed by a response from Al: "What an utter waste of time! This kind of garbage plays right into the hands of the animal rights goombas who say that there is no need for animal trials because it is possible to fully simulate animals in software. This kind of rubbish is what happens when unscientific people with no knowledge of the limitations of digital technology make funding decisions." Ellen reiterates her confusion by asking if researchers will "provide insulin to the 'virtual' mouse." According to the wording in Zheng et al.'s paper concerning interventions performed on the PhysioLab platform, the answer to this question would appear to be "yes."

comes with material experimental systems, and at the possible cost of certain kinds of information. Mathematical renditions of the natural world in experimentation work, Morgan notes, in her discussion of how experimental systems are deliberately simplified, because of the assumption that causes deemed less significant can be neglected altogether (9). Beaulieu likewise notes, referring to the Visible Human Project and to visualization technologies in science more generally, that science has always attempted to "evacuate[e] messy bodies," so that when "high-tech wizardry . . . floats in and out and through bodies" concerns about the subjectivity of research on some kinds of objects (live mice, in my discussion) are more easily dismissed (637). Critically, when the messiness and mystery of bodies can be omitted from biosimulations (in favour of the transparency that makes experimental events easier to understand), much of the opportunity for virtual animal bodies to do something truly unexpected has also been programmed out of the experiment. In my third chapter, I proposed that reduced physical contiguity with the objects of research and the further simplification of those objects might affect the nature of scientific prose; here, I explore one possibility for why the language of science may depend on continued contact with the objects of research: because these objects supply the unpredictability and surprise that may be important for certain kinds of information, namely novel, unexpected information.

Some scholars have suggested that information (understood as the pattern extracted out of a situation) can only be understood against the background of unpredictability, and that predictability and surprise are in constant, productive, interplay. For example, literary scholar of science and technology Katherine N. Hayles summarizes the relationship between information and uncertainty in these terms: "the more unlikely the event, the more information it conveys" (32). Accordingly, then, Hayles states that we gain more information from a situation that is unexpected than from one that is entirely predictable (32). Information may thus depend on a balance of both predictability and unpredictability, and uncertainty becomes "both antagonistic and intrinsic to information" (33). Literary scholar Gary Saul Morson proposes, along similar lines of argument, that unpredictability becomes essential to narrative-making more generally, as we tend to want to impose a sense of process onto the world (61). If one has total predictability, Morson asserts, one no longer has suspense, and if one no longer has suspense, one no longer needs to order events (to impose meaningful connections) through narrative (68). Hayles goes on to explore how this interplay between pattern and unpredictability—the desire for pattern and fear of randomness—shapes various cinematic and literary productions;[56] applied to scientific research involving involving virtual mice, however, this formulation would mean that researchers potentially learn more when things are unpredictable and there is the possibility of surprise. The unpredictability that holds high production potential for information can also be conceptualized as implying a high need for narrative as well in science; we can think of experimental papers as accounts of what happened, as an ordering of events which were not given in advance.

Attempts to render experimental animal bodies transparent and predictable in cyberspace, by turning them into data (and then back into digitized flesh), may therefore reduce the potential for information in cyberscience.[57] If we think of cyberspace as a kind of

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[56] Hayles' analysis features, for example, Cronenberg's film *The Fly*, Gibson's *Neuromancer*, Hans Moravec's *Mind Children*, and Italo Calvino's digital text *If on a winter's night a traveler*, which Hayles states shows an acute fear that randomness will swallow pattern in cyberspace, and the text will be "reduced to a body of meaningless data" (43)

[57] My analysis does not consider the ethics of animal experimentation, only the implications of the dematerializtion of scientific research on scientists' linguistic productions. Of course, it would be desirable to reduce or eliminate the use of animals in experimentation, but whether virtual animals are the solution deserves much more discussion.

orchestrated wish fulfillment, then researchers dealing with digitized mouse bodies which are "well-defined," as stated on the Jackson website,[58] may get less information from such experimental systems. Morgan notes that in mathematical simulations, over a range of values, solutions may be surprising, but the ways in which researchers will *expect* to be surprised are to a large extent known in advance (8). The elements that remain unpredictable in a biosimulation are known in advance; their unpredictability is a controlled part of the experiment. The inherent unpredictability of material systems is acknowledged on the Jackson Laboratory website, when the "conditions of use" disclaimer states that animals arrive "AS IS," with "no warranties of any kind, either express, implied, or statutory." In science, unpredictable material resistance may be important in the pursuit of knowledge, and something might be lost if we attempt to completely neutralize this antagonism.[59] Left out might be the real mice from Crabbe et al.'s study that made researchers acknowledge that the controlling variables in animal experimentation are often obscure, and that alternate explanations cannot be ruled out.

The unpredictable spatio-temporal unfolding of material existence has traditionally been important to scientific prose; among other things, "a rhetoric of immediate experience" implies a retelling of witnessed events whose precise unfolding sequence was surprising in some way to the person(s) witnessing/orchestrating those events; the authors would then need to account for the sequence of events. Information and the potential (and need) for narrative

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[58] Jackson's "Genetic Quality Monitoring" page states that "[t]he valid interpretation of experimental results depends upon the assurance that the mouse models used are genetically stable and well defined," and that Jackson represents "the gold standard for genetic integrity."

[59] Media and communications scholar Jenny Sundén, writing about forms of online embodiment and the interface between subjects and online material representations, explains especially well what may be lost if material antagonism is neutralized: "Left out are heterogeneities, variations, different and maybe marginalized versions. What can never be included in the discourse of a global web of information, where bodies paradoxically are made both invisible and equal, is the particular, local, and concrete" (14).

in science may therefore both increase as a function of the entropy of the system, defined in terms of the number and strength of unknown variables and the potential for unexpected outcomes. The higher the potential for the agent to be surprised by events (because entropy is measured from the agent's perspective), the higher the information and narrative-potential of the situation. This idea can be stated quasi-mathematically, as IN=E, where I stands for information, N for the potential for narrative, E for the entropy of the system. The entropy of the system E can further be defined as E=S/V, where V stands for the extent to which variables and possible outcomes are known in advance to the agent, and S for the potential of the agent to be surprised. Hence, the higher the value for V (the more known the variables), the lower the S value (surprise factor), and the lower the entropy of the system as a whole.

When this equation is applied to an experimental scene, it potentially implies that experiments on mouse bodies which are "well-defined" in advance may require less narrative after the fact to impose connections and order events. In Zheng et al.'s paper, the experimental system is admittedly "transparent," in that possible outcomes are well known in advance because the input data for the biosimulation were published data. As Zheng et al state, "[t]he platform was developed based on published experimental data and represents the key components known to contribute to [diabetes]" (58). A set of "parameter values" define a particular virtual NOD mouse in the biosimulation, but these parameters were "derived from published quantitative data" (51). In other words, the biosimulation has been informed by studies which have already ordered a sequence of experimental events into a narrative that imposes meaningful connections. These previously constructed connections, then, become the basis for "cell" movement and behavior in the biosimulation. When the simulation is run, the output data will have to be variations on these same (already established) connections.

Researchers will then take the (virtual) data and order them into narratives of meaningful connections based on the same published narratives which served as input data (and now the responses of virtual mice are found to be "consistent with the range of responses reported in the literature" (51). The pathways between inquiry and interpretation begin to seem circular. The data do become easier to decipher after the fact (a supposed benefit of using virtual mice), but because the surprise factor (S) will be relatively low (as compared to experiments in real mice overall), and the value for V (known variables) will be high, entropy (E) will also be low, which implies less information (I) from the experiment (if information is linked to novelty which was unpredictable in advance), and less need to order the data into a narrative (N) which makes sense of a surprising sequence of events. In an attempt to control the flux of control versus unpredictability, researchers using virtual biosimulations may therefore be achieving control at the expense of novel, serendipitous information.

This discussion of how situated research on unpredictable recipient objects relates to information and narrative-making in science also reveals another way of looking at the relationship among action-attention-language. The language component of my triad can now also be imagined as representing the organizing force of order and pattern against the unpredictability supplied by the situated action component. The triad can thus be imagined as representing the dynamic flux of control (through linguistic activities that order the world) against the unpredictability of being a self-moved agent in the material world acting on the (often mysterious) objects found in a material scene. In the instances of cyberscience that I feature in this chapter, we see the attempts to skip the disorder and seeming randomness of research on complex recipient objects such as mice to arrive directly at the ordering stability of the language component.

Although scientific researchers have always attempted to control research scenes in order to limit variables and possible outcomes, researchers also appear to be in the midst of grappling with new technologies that defy all previous conceptions of scientific work and what this control looks like. As Monteiro proclaims, cyberinfrastructures are changing the "parameters of adequacy" in research, what counts as witnessing, and how data are handled (341). I have argued here that researchers appear to be employing the recursive gaze of familiar discursive forms to anchor these new digital technologies to a materiality that may be disappearing from research on the natural world, as those deciphering research and disseminating scientific knowledge are increasingly being asked to act as informatics experts.

What place, then, should virtual research have in knowledge-making practices? Zheng et al. envision that *in silico* studies will be used to "complement and help guide *in vivo* and *in vitro* experiments" (59), and end their paper by stating that "[u]ltimately, the goal is to apply this technology to improve the prospects of successfully curing type 1 diabetes" (60). The Entelos Inc. website states that biosimulations give researchers "the ability to quickly predict the effects of different therapies proposed for treatment of Type 1 diabetes," presumably with the aim of deciding which therapies to pursue further. The diabetes.org website ascribes even more predictive power to biosimulations, stating that "[t]hanks to computers, scientists now have the opportunity to study type 1 diabetes without having to enter a laboratory or enroll people into a clinical trial," and that with such technology "people with type 2 diabetes can determine their chances of having serious diabetes complications, such as a heart attack or stroke, just by spending a little time on their personal computer" ("Going High-Tech" 19). The prospect of such digital mediation in scientific research and healthcare underscores the importance of understanding more fully what situated, physical experiences in the research context bring to knowledge-making practices involving language.

CONCLUSION

In analyses of communication, the methodological orientations held by critics shape the problems that are identified, the kinds of questions that are asked, how answers to these questions are sought, and the nature of resolutions. In order to explore the possible relations between physical and linguistic research practices, I have adopted a methodological orientation influenced by phenomenology and Burke's theory of Dramatism. This orientation is centered on the idea that scientists are embodied knowers, embedded in a physical context, surrounded by the objects of research on which they act, and that their use of language in various forms of scientific prose is tied to these laboratory benchtop experiences. I began by framing the agent's body as the epicenter of epistemic activities. The kinds of questions that I asked in this dissertation therefore had to do with how ways of doing science on the level of material interactions with the objects of research might relate to how the work is framed in scientific prose. An important implication of my action-attention-language triad model is some form of reciprocal relationship between the action and language components, between what is done to research objects and how those actions are described. My aim was to progressively push on the limits of this relationship and to explore the instances when language does not adequately capture what was done in an experimental context. Below, I summarize my main analytical moves and findings before I consider the possible limitations of my dissertation and avenues for future exploration.

I went into my first chapter with Bazerman's assertion that the "language negotiation" in science is tied to "concrete operations on concrete objects" as my organizing theme, hoping to define further what this might mean, what such an association might look like in the prose itself, and how this association between what Collins calls “physical” and “linguistic” research practices (“Language”) can be analyzed. Thus, I set out to investigate what it might mean for the physical work of scientific research to make its way into the text. In the process of trying to answer these questions, I was also testing the usefulness of my action-attention-language triad model as a heuristic tool.

All four of my chapters revolved around specific case studies of scientific prose, defined broadly as prose that refers to actions on research objects. I was especially interested in the experimental paper. In Chapter One, I looked at an example of scientific prose, Oliver Smithies’ paper on gel electrophoresis, that refers extensively to situated actions on objects. I also looked at what language and STS critics have said about scientific prose which deals specifically with apparatus and experimental methods. Using my triad as a shorthand for thinking about the relationship between benchtop and language-related research practices allowed me to see how Smithies’ prose is framed from the central spatio-temporal perspective of an agent embedded physically in an experimental scene. The attention component of the triad let me see how agents’ actions on recipient objects are framed from the perspective of an agent who is constrained in the way that s/he notices the material world, and how scientific prose which describes an experimental method can capture this sense of constrained attention as linked to a human body moving around in a scene. I noted that this is closer to the non-Euclidean perspective on the world that we see mirrored in modern art. In such scientific prose, the perspective is therefore on the objects being acted upon, and the way in which objects change over time as a result of interventions upon them, rather than on the agent.

Several scholars have noted that experimental prose can convey the impression of vicarious physical experience when authors use "a rhetoric of immediate experience" (Atkinson); I reframed this observation in terms of my triad model, and explained why the prose can lead to vicarious experience of the research scene. When I focused on representations of embodied experience in the prose, I noticed that an embodied agent is implied (rather than erased altogether, as many have proposed) but invisible to the reader. This notion of an implied but invisible agent let me formulate an alternate reading of the purpose of the passive voice in scientific prose. The passive voice now becomes a means of letting the reader experience the scene as if he or she were the agent acting on research objects. I showed that the agent is still very much present in such depictions, but in a different way than in non-scientific prose. This particular way of framing actions on research objects might serve the purposes of scientific communication better than an author-omniscient style of writing, as it would help to align many different potential agents separated from one another in space and time on how to physically cope with a common object. In this first chapter, I therefore found that there can be a very close, correspondent, relationship between physical and linguistic research practices; that the language negotiation in science can indeed be constrained by "concrete operations on concrete objects," or, at least, that researchers can order their words to give this impression.

In my second chapter, I looked at a very different kind of research object, the mouse, and how a community of practitioners, held together by this common experimental tool, attempts to set material and linguistic boundaries on how it is used. I used Bazerman's notion of possible and impossible machines, in terms of what research objects can and cannot do, and proposed that the natural limitations of the mouse body dictate the ways in which experiments are set up and how the data are woven into narratives of biological causation. I drew on biologist Steven Rose's critique of neurobehavioral geneticists' linguistic maneuvers in experimental papers dealing with mice to posit that by exerting material agency (by reacting in unexpected ways in the research scene), the mouse-object also influences how researchers construct their arguments. The close association between physical and linguistic research practices that I describe in my first chapter, between the action and language, begins to break down in experimental accounts dealing with this more unpredictable research object.

My triad model applied to papers on animal experimentation allowed me to notice and explore how language can be used to compensate for the material constraints imposed by the object; it allowed me to see that the "language negotiation" in this case is a bit less constrained, in some respects, by "concrete operations on concrete objects," even as researchers attempt to anchor their linguistic activities to "a rhetoric of immediate experience" in adherence to convention. Language makes sense of what happened in the lab and supplies relevance by linking data across different species; I called this phenomenon "discursive pleotropism." My triad model also let me see the importance of the attention component in the making of meaning from a dynamic material situation. An instance of controversy involving the use of the mouse body in neurobehavioral genetics research showed how much of the total material context is normally excluded from attention and language, and, later, possibly knowledge. I thus proposed action and language work in a circular manner, where prior experimental descriptions show others what to pay attention to because they name certain features of the situation and not others; aspects of the situation named by others then influence how subsequent experimental events will be described. In this second chapter, it became more evident that there is considerable flexibility and choice in how physical laboratory experiences are framed in the scientific paper. Many STS scholars have noted, as I summarize in Chapter One, that the final experimental paper is a selective (and creative) reconstruction of events; the papers dealing with MAOA KO mice that I looked at in Chapter Two further demonstrate this selectivity at play; they importantly show the ways in which language in experimental prose is *not* tied to "concrete operations" on animal bodies when these bodies act as models of human disease.

My third chapter focused on a genre of scientific communication not typically considered by critics who look at the language of science: advertisements, aimed at researchers, for research products. Using ads for gel electrophoresis spanning nearly fifty years, I explored portrayals of agents and their actions on increasingly automated versions of electrophoresis. Ads for research products are pointing to the simplification and automation of apparatus so that embodied coping in some instances becomes reduced to the push of a button. I asked how agents who push a few buttons and walk away, as one ad explicitly suggested, will be able to speak for science as experts. Collins and Evans propose that "interactional experts" unfamiliar with the materiality of benchtop research work can hold the same linguistic abilities as "contributory experts" who also do the work. The agents portrayed in more recent ads have very little to do with the physical, benchtop practices of science, but are portrayed as still engaged in the linguistic activities associated with science, such as writing experimental papers and grants and giving talks at conferences. The agents portrayed in the ads would therefore be interactional experts. I explored what such agents might be missing, in terms of linguistic abilities, by not having participated in the research itself.

I pointed to several studies which show that reduced "physical contiguity" with the materiality of a domain, to use Rodrigo Ribeiro's term, affects what can be said about that domain. Collins and Evans do state that interactional expertise is dependent on contributory experts to propagate the language of a domain (in science, such experts may be graduate students, postdoctoral fellows, and possibly newly hired researchers who still do some of the work themselves), but the ads that I surveyed imply that those who do the linguistic work may increasingly have less to do with the benchtop part of research work. A rhetorical style of scientific reporting based on an impression of directly witnessed events as described in the first chapter has tended to mean an agent who has participated in *both* physical, benchtop work and the linguistic work of recounting personal laboratory experiences; the verisimilitude of the account depends on this impression. Some of the ads I looked at paint a very different picture of the relationship between action and language, with various aspects of research work becoming increasingly divided among different kinds of micro-experts. Such representations of scientific practices therefore seem incompatible with the action/attention coupling of rhetorical consciousness which has been foundational to the genre of the experimental article.

In my fourth chapter, I looked at two ways in which research involving mice is becoming mediated by high-speed, networked computers: websites selling research mice and virtual biosimulations. In both instances, those writing experimental accounts are distanced from the physical part of working with mouse bodies, either because some, or all, of the work is done off-site by others (who cannot easily be accessed afterwards about the details of the experimental work), or because the object of experimental study is itself computer-generated and non-material. In the case of the websites, I used my triad model to look at what actions on mice come to mean when the interaction happens over the internet; what the experimental scene becomes for the agent ordering mice and research services online; and how the recipient object is experienced in this new digital experimental scene. I proposed that action comes to mean acting on recipient objects through language itself via the online order forms, and that the recipient object appears to emerge directly from the words entered online, where word is literally made flesh (and often flesh which is unlike anything that would emerge spontaneously in nature, with select genes missing). For the agent encountering these mice online, animal flesh appears to provide little resistance to wish fulfillment. For the agent encountering mice online this way, rather than physically in an experimental scene, a different sense of the experimental scene perhaps emerges, yet "concrete operations on concrete objects" must still be represented in experimental prose. What this interactional expert divorced from real mouse bodies might not be able to include in the experimental account are situational contingencies that were experienced by the various technicians handling real mouse flesh.

In the case of the virtual NOD mouse, as represented in a paper by Zheng et al., I used my triad model to look at the kinds of linguistic maneuvers that Zheng et al. employ in their paper to describe actions on this very novel type of research object in the life sciences. The object that is being acted upon is a non-material entity, a computer program, yet the authors describe the object and their actions upon it in material terms, as though physical interventions had been carried out on real mouse bodies. In experimental prose, virtual NOD mice function like real mice, even if physically they do not. This novel research object therefore challenges the conventions of scientific prose; the rhetorical style of "immediate experience" is retained to describe actions that never took place on objects that do not exist.

The attention component of my triad model allowed me to notice that the language in the paper is framed in terms of the third-order objects being handled when commands are entered into a computer. As with the websites, researchers working with virtual mice seem to act on mice with language itself; it can be argued that "agency" in the Burkean sense of instrument used for the action becomes language itself in virtual biosimulations when researchers write material mouse bodies into existence. Action and language thus conflate: the action carried out by the agent is the act of using language. I concluded by proposing that the kind of circular experimental situation that virtual biosimulations represent may lead to less serendipitous discovery and less novel information. At the moment, researchers wanting to participate in a wider scientific conversation may have little choice in how they frame their experiments on virtual objects, but this uneasy coupling of non-material experimentation and material language potentially points to an important shift in a style of scientific reporting that has dominated for over three hundred years. It will be important for STS scholars to follow what happens with such novel research objects and the ways they affect scientific reporting.

The observations and hypotheses that I made in this dissertation are a beginning to further questioning, and part of the usefulness of my action-attention-language triad model is that it has pointed along the way to new areas of inquiry concerning how science is being carried out at the levels of benchtop and language. My dissertation has, however, roamed far and wide in its subject matter, while at the same time narrowing in on a limited selection of case studies. My exploration thus excludes much more than it includes, but, as Bazerman states, one could always write "an anti-contents" of the topics not investigated (17). My inquiry is thus also, to borrow Bazerman's apt phrase in describing his work, "a spotty affair" (8), which likewise explores some implications and sidesteps others; does not even begin to map the range of variation in scientific texts; and can only conjecture about moments of emergence and transformation in scientific writing. This roaming was deliberate, however: a way of putting a series of landmarks on the map from which to organize more detailed cartographic exploration. Responding to the suggestion that analyses should refrain from pulling in case studies that show such breadth in their subject matter, Alan Gross states that variety can be used to demonstrate the profitability of a given approach (*Starring* ix). Bazerman likewise writes that such "spotty" beginnings can give important insights into fundamental processes of language use in science, and allow critics to "[see] what kinds of things [can] be said" (9). Here too, I have used breadth of subject matter to attempt to demonstrate some of the possible uses of my particular approach. This breadth has also made it easier to see the limitations of my approach.

One such limitation, brought to the fore by the wide range of scientific texts that I looked at, concerns terminology. I wanted to use Collins' "physical" and "linguistic" terminology because it seems to capture the distinction I was after between different kinds of research practices, and it enabled me to link my discussion to an ongoing scholarly conversation. At times, however, both terms struggled to do the job I needed them to do. For my purposes, the term "linguistic" needed to capture the use of language to refer to experimental actions. In usage, however, the term "linguistic" had the tendency to become quite global, casting a rather wide net that I needed to continually explain and try to limit. This global feeling in the way I use the term likely comes from the diverse ways in which scientists use language to refer to actions on objects, both in talk and in text. This raises the following question: Where, then, does the "language" part of the triad stop, and, indeed, start? Scientific investigation begins in language as well, rather than simply ending there, and endings (in the form of experimental reports, for example) form the basis for new beginnings (in the form of physical practices). The "language" part of the triad can also be said to infuse physical, benchtop practices, and vice versa. I have tried to capture this reciprocity among practices by framing my triad as a triangle, rather than a linear string, but at times the triangle tended to unhinge in my usage into something more linear. This unhinging may have been made easier by the attempt to divide "physical" and "linguistic" research practices to begin with. Also, how far beyond the immediate experimental situation do the linguistic practices associated with that situation extend? The language part of my triad clearly does not stop when the experiment is over and everyone leaves the lab. How much distance, then, can there be between a situated experimental event and what is written or said about it for the language work to be part of the triad? The perimeter that I drew around the term necessitates that language be used to refer to a specific experimental event, but it would be interesting to pursue further the questions raised above.

The term "physical" at times became similarly problematic, as it is easy to associate it with agent embodiment and embodied actions. Agents are always embodied, however, and so all of their actions become "physical," even when typing on a computer keyboard, so I found it necessary to keep adding the qualifier "benchtop" to the description to make the term refer to a specific kind of experimental scene, full of machines, various tools, and material research objects that can be physically handled in some way. The qualifier "benchtop" becomes problematic, however, when research practices move into the digital realm and the computer becomes the benchtop. This issue of the place of an experimental scene, in terms of where it is that an experimental scene takes place, is an interesting one, and I would also like to pursue it further. I am not sure how else it would have been possible to frame this distinction between different kinds of research practices; as I discuss in my introduction, STS scholars have been struggling for some time to figure out how scientists use both language and the material world to make knowledge, and how the two realms relate to one another, and, indeed, whether it even makes sense (or is productive) to think of these as different realms. I began with a division for the purposes of my analysis, and the division has been productive in that sense, but my analysis has also led me to ask whether positing of an opposition between "action" and "language" presents problems in understanding scientific epistemology in the first place. In this sense, the limitations of certain terms become as equally instructive as successful usage: enforced divisions reveal underlying unities.

In the course of my analysis, the (potential) limitations of Burke's theory of Dramatism and his Pentad as applied to science also came to the fore. Although Burke's Pentad was a tool, rather than a central focus, of my analysis, some interesting observations about this part of my approach have emerged through its application in a wide-ranging selection of case studies. The term "scene," for example, becomes tricky when part of the work of acting on mice happens online, through order forms, and part of it happens in a distant commercial laboratory. The term "scene" also becomes tricky in cybercollaborations, where many dispersed agents come together in an intangible online space to act on virtual research objects in that space, while their bodies and typing fingers are situated elsewhere. The term "act" becomes knotty when mice and research services are ordered online. When does action on mouse bodies begin? Does it begin when a researcher accesses the online order form and types in the specific genetic sequence s/he would like "knocked out" of a mouse's genome, or when a company technician handles real mouse bodies? Who bears responsibility for the act in this latter instance? In other words, who is the agent here, the technician, or the person ordering the action, or a combination of the two? As I discussed in Chapter Four, the nature of the act itself becomes hard to define when virtual mice are administered immunological molecules. The act becomes both running the biosimulation and injecting mice under the skin. Once again, "action" and "language" conflate in fascinating ways. Finally, the term "agent" becomes awkward in the case of science because the work is invariably divided in increasingly complex ways. In the case of cyberscience and virtual experimentation, the term "agent" becomes especially hard to pin down.

For Burke, such shifts and slippages in how his terms of Dramatism are applied, however, form part of their intended usefulness. The idea of the pentad is to generate possibilities for thinking about, for example, scenes or acts, as multiple, with each identification of a scene or act changing what is noticed. The central task, then, is not to disambiguate these terms, but rather, as Burke states, "to study and clarify the *resources* of ambiguity"; what we want "is *not terms that avoid ambiguity,* but *terms that clearly reveal the strategic spots at which ambiguities necessarily arise*" (xviii, emphasis in original). In applying the Pentad towards my case studies, I have inadvertently identified some of these "strategic spots" of ambiguity and demonstrated (notably because I was attempting to avoid ambiguity) the ways in which they play out in specific scientific texts, circulated within specific scientific communities, who use specific kinds of (digital) research technologies that push these ambiguities even further. A more deliberate and focused analysis of Burke's terms of Dramatism in scientific prose on virtual biosimulations may therefore yield other interesting results.

Another possible area of further study involves linguistic regularities in experimental prose as shaped by ways of doing research. Since Boyle's experiments in pneumatics, experimental prose has been used to establish the sense of events "as directly experienced by the author," as Atkinson states, which has rendered the writing corporeal and focused on actions on objects. What new types of linguistic regularities might emerge if this convention of experimental prose falls out of use because the nature of the work has changed so dramatically? What would happen, for example, to linguistic regularities that have evolved to convey the experience of vicarious witnessing if those writing research accounts, themselves completely divorced from the material work, need no longer create this kind of vicarious experience for readers? A publisher attending the ISMPP (The International Society for Medical Publication Professionals) conference is quoted by Sismondo as remarking that journal editors need to change their attitude about ghost-managed research, that it would be better to be transparent about changing research practices and "get this out into the open" ("Ghosts" 181). If scientific research in general goes towards the path of ghost-management,[60] where no one meets the traditional requirements of authorship, in terms of having been involved in both benchtop work and the composition of the experimental report, and it becomes widely accepted that scientific knowledge is being made this way, how might experimental prose change as a result? Put differently, if scientific communication has evolved in response to problems of social interaction, as explained in my first chapter, and if the social interactions involved in science are changing, how will scientific communication change to mirror these developments? Sismondo predicts that this new corporate science will become steadily normalized, given the high commercial stakes involved (195). How might the ways in which scientists know things about the world change as a result of changes in how experiments are reported?

[60] Sismondo notes, for example, that in 1990, academic collaborations received 70% of available industry research funding, whereas in 2000 CROs received 70% of the funding (176).

In particular, I have in mind an ethnographic study that would follow researchers who acquired (at least some of) their data from a CRO through the process of data analysis and the writing of the experimental report. What might the process of writing scientific prose look like when there is no one to interrogate as a living archive of the experiment? The methodology I imagine for such a study would be similar to Amann and Knorr-Cetina's in their paper on evidence fixation (where the authors follow researchers to see how they reason their way to evidence through the handling of material data artifacts), except with an emphasis on how language is used in the *absence* of material data artifacts, and in the absence of personal experiences, to create an account of experimentation.

At the end of my third chapter, I briefly showed one possible way in which scientific prose changes when the work becomes increasingly mediated by commercialized tools; references to commercialized products and the companies that sell them, and perhaps eventually to contract organizations themselves, may come to displace descriptions of situated, embodied coping. A more systematic examination of the scientific literature would be needed to characterize what appears to be an emergent trend in experimental prose. Have regularities developed in how such referencing is done? What sorts of material details might such referencing allow researchers to omit from the prose? It would perhaps also be interesting to study the impact of increased references to commercial products and companies on reader uptake of scientific information. How might various readers' experience of experimental prose shift, for example, when full of references to commercialized products, stating the name of the kit used, but not what it means to use it? Would references to products affect readers' abilities to understand how data were interpreted, and to judge the validity of authors' interpretations? Would references to products affect readers' abilities to imagine new experimental possibilities from the information presented? In Chapter One, I discussed how experimental prose rich in circumstantial details related to embodied coping lets readers imagine in "the laboratory of the mind's eye" what was done. If readers can no longer picture what was done, is their ability to engage with the information affected? Does it become unnecessary to describe how a commercial kit was used because it is assumed that everyone has the same kit in their laboratory? The widespread dissemination of commercial products would lead to the impression that the material conditions of knowledge production are indeed equivalent from one lab to another, as Gieryn might state, but what might be lost through such standardization?

Technology generally does not go backwards, and those writing accounts of experimentation will likely be increasingly removed from the laboratory benchtop work of research in a variety of ways. As scientists and their critics struggle to do justice to material things in science, both groups may now also have to take into account the dematerialization of research practices as well, as science begins to move into the digital realm in a variety of ways.

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