UNIVERSITY OF CALIFORNIA SAN DIEGO

Cognitive Neuroscience and the Experimental Theater of Other Minds

A dissertation submitted in partial satisfaction of the requirements
for the degree Doctor of Philosophy

in

Communication (Science Studies) and Cognitive Science

by

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The dissertation of Yelena Gluzman is approved, and it is acceptable in quality and form for publication on microfilm and electronically.

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DEDICATION

This work is dedicated to Dr. Rae Silver, who taught me to think in the lab, and to Filip Marinovich, who invited me to think through theater.

TABLE OF CONTENTS

LIST OF FIGURES

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Chapter 1, with minor additions, is a reprint of the chapter "Research as Theatre (RaT): Positioning Theatre at the Centre of PAR, and PAR at the Centre of the Academy," in *Performance as Research: Knowledge, Methods, Impact*, edited by Annette Arlander, Bruce Barton, Melanie Dreyer-Lude, and Ben Spatz, published in 2018 by Routledge. The dissertation author is the principle investigator and sole author of that chapter.

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ABSTRACT OF THE DISSERTATION

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Science and technology studies (STS) have long demonstrated how laboratory experiments are shaped by their political, social, and interactional conditions. Only recently, however, has a *collaborative turn* advocated for bringing STS perspectives to directly bear upon scientific practice. I take up these literatures from the perspective of experimental theater, an approach rooted in the historical co-articulation of theater conventions and experimental science. "Going experimental" entails reflexive engagement with laboratory encounters as both staged and lived. Drawing on theater and centering ethnomethods, I embed myself in sites of cognitive neuroscientific practice, working with researchers and research subjects to collaboratively

reconsider how experiments are conceptualized, enacted, and interpreted. More broadly, I argue that engagement with the interpretive practices of both scientists and subjects in the doing of experiments allows for STS to productively reconfigure science's "replication crisis" as a crisis of interpretation, a move that aims to recognizes broader challenges to science's epistemic supremacy from marginalized communities.

I focus primarily on a collaboration with a cognitive neuroscience lab that was interested in designing a novel way to investigate autism and creativity. Our collaboration began with close readings of previous creativity studies. Thinking of these previous studies not as simple tests of a hypothesis but as complex unfolding events shifted our shared attention from experimental data to the ways that data was produced. Taking up lived perspectives of experimental subjects challenged how these previous studies interpreted their data, and contested the claims of autistic deficits that rested upon such interpretations. Incorporating such critiques in the design of a novel experiment was an opportunity to collaboratively grapple with staging an experiment outside of a deficit framework that configures autistic minds as lacking. In response to previous research that used animated geometrical figures based on Heider-Simmel animations (1944) to demonstrate a mentalizing deficit in autistic children, we designed and conducted an experiment that asked autistic and typically developing children to create their own animated films. This experiment serves as an empirical case to consider the challenges and promises of critical collaboration in expanding interpretive possibilities in the cognitive neuroscience lab.

INTRODUCTION

The Interpretation Crisis

The replication crisis

In 2005, Stanford Professor of Medicine John Ioannidis published an article in *PLOS Medicine*, claiming that "There is increasing concern that most current published research findings are false" (Ioannidis 2005, 696). This startling statement, citing a number of factors that contribute to the likelihood of false claims, sounded an alarm for scientists at large and was a jarring beginning to the field Ioannidis identified as "metascience."[1] One of these alarming factors, according to the author, was a lack of data on post-study probability, or how predictive a study's results are of a subsequent study's findings. Lacking published studies that replicate the findings of previous ones, he argued that the odds are strong that seemingly positive and statistically-significant results of a given study are actually not true.

Thus was launched a growing concern among scientists that the trustworthiness and rigor of science was in danger of discredit.[2] Taking Ioannadis' claims even further, papers published later and in extremely high-profile journals made claims that, not only were scientific findings under-investigated in subsequent replication studies, but that an alarming number of studies were, when made subject to replication studies, not replicated (e.g., Baker 2016a, 2016b; Begley 2013; Alberts et al. 2014; Pashler and Wagenmakers 2012). This is what came to be known and publicized as the "replication crisis" or "reproducibility crisis" in behavioral and biomedical

[1] For a sociological consideration of metascience as a "scientific/intellectual movement" (SIM), see Peterson and Panofsky 2020a.

[2] Peterson and Panofsky (2020a) point out that the issues at play in the modern metascience movement are not new, and that "[c]ritiques of null hypothesis significance testing have been around for decades" (7). They do, however, acknowledge that Ioannidis' startling claims "managed to capture attention in ways previous attempts had not" and helped to galvanize conditions for a concern to become a concerted transdisciplinary movement with effects on funding, policy and reform (ibid.).

research. It was understood to be a crisis because the failure to replicate not only threatened public trust in the legitimacy of science, but also because it violated a tenet that, internal to these disciplines, is supposed to demarcate and elevate scientific reasoning in respect to other forms of inquiry, knowledge, and belief (Penders et al. 2019), resulting not in the falsification but rather the "canonization" of inaccuracies (Nissen et al. 2016).

The replication crisis has been a pressing issue first and foremost for scientists themselves, with both underlying causes and their posited solutions largely falling within the bounds of normative standards[3] for scientific inquiry. For example, scientists' responses to the problem of replicability argued for shoring up the rigor of hypotheses through publicly pre-registering both hypotheses and experimental designs before a study is conducted, therefore preventing scientists from "cherry-picking" findings or settling on trivial results that merely reach statistical significance.[4]

Peterson and Panofsky (2020a) further note that metascience-led initiatives to both establish and address a transdisciplinary replication crisis (not local to one scientific discipline but relevant to projects throughout "big-S science") have largely ignored decades of work in the sociology, history and philosophy of science that examined disjunctures between scientific publications and daily practices of scientific research. While metascience highlighted disparities

[3] Problems leading to fraudulent or false research findings were understood to result from violations of normative principles that impose standards on the production and dissemination of scientific claims, like peer review and scrutiny from scientific communities (Merton 1973), and replication studies as mechanisms for falsifiability (Popper 2005 [1959]). Scientists saw these problems as arising from systemic shifts in professional practice and publishing that incentivized novelty and spectacular findings over accuracy, transparency, and community verification through replication (Peterson and Panofsky 2020, Freese and Peterson 2018, Nosek et al. 2012). Solutions proposed reforms in scientific practices like methods training, access to data sets, publication incentives and requirements, and peer review.

[4] Gonzales and Cunningham 2015; Foster and Deardorff 2017; Hillary and Medaglia 2020. Another proposed solution pushed journals and institutions to increase valuation of (and thus incentives for) replication studies—long treated as adding little to the field and disciplinarily devalued in respect to novel studies and "positive" findings (e.g., Alberts et al. 2014).

between the ways that scientific methods were supposed to work and the ways they were done as a revelation of malpractice, sociologists, historians and philosophers of science have long considered the many tacit and ordinary practices that lay outside of (or even contradicted) methodologies of objectivity and verification as necessary to the success of the scientific projects being undertaken. For metascientists, according to Peterson and Panofsky, claims of an overarching replication crisis depended on demonstrating violations of *science-wide* norms and values, and thus on a *unity* of norms and values across scientific fields.[5] Assumptions of such a unity were not upheld by philosophic, historical and sociological studies of science: as the authors put it, "qualitative differences discovered through historical and case studies present a challenge to a field [metascience] with designs on measuring and manipulating capital-S science and whose whole claim to authority rests on the coherence of domain of science" (Peterson and Panofsky 2020a, 22).

One of the repercussions of casting replicability as the threatened gold-standard of scientific knowledge has been a call for replication to be taken up as the standard for epistemic validity writ large, even in humanistic social sciences. Penders et al. reflect on the ways that this delegitimizes particular research programs along familiar lines: "If fields of research exist for which replication is an unreasonable epistemic expectation, then policies for research that universalise the replication drive will perpetrate (some might say perpetuate) an epistemic injustice, ghettoising the humanities and hermeneutic social sciences as either inferior research

[5] In their article "Self-correction in Science" (2020b), Peterson and Panofsky consider the way that "one-size-fits-all" measures to mitigate potential violations of replicability elide and disguise meaningful differences between scientific disciplines, including particular research traditions' different ways of understanding how and why replication can or ought to be done. This results in a flattening out of diverse research practices even among the so-called "hard" laboratory sciences.

or not really research at all" (Penders et al. 2019, 2).[6] In response to this perceived threat to the legitimacy of interpretive social science, Penders et al. (2019) present a well-argued defense of the hermeneutic tradition and its *separate-but-equally-valid* modes of reliability and accountability in humanistic disciplines.

Moreover, replication as an unproblematic mode of verification has long been challenged in the history, philosophy and sociology of science. Freese and Peterson (2017) suggest that when particular research domains undertake replication projects, there are "unavoidable interpretive ambiguities" (149) specific to that domain's questions, practices, and technologies. This observation is not a new one, but builds on decades of previous scholarship that shows how different forms of practice-based ambiguity prevent replication experiments from having the definitive role they are assigned in abstract, generalized accounts of scientific self-correction and falsifiability (e.g., Hacking 1983, Collins 1984).

The replication crisis is a crisis of scientific validity, but it is not the only one. Whereas this crisis is articulated and addressed from *within* scientific communities of practice and through methods endorsed therein (e.g., statistical analyses, the role of hypotheses, bench work, model building), other challenges to scientific legitimacy have come from *outside* specialist scientific communities, having little recourse to accepted scientific tools with which to develop their claims, and resulting in less recognition and legibility as actionable challenges. One of these, targeting the legitimacy and truth-claims of laboratory experimentation, has come from communities who are typically the subjects—not authors—of scientific research.

[6] Peterson and Panofsky (2020b) echo this observation: "Metascientists have suggested that the problems that lead to non-replicable findings tend to mirror the "hierarchy of science" with physical science having the least problems, biological science a middling amount, and the human/social sciences the most" (39).

In recent decades, people diagnosed with Autism Spectrum Disorder (ASD) have argued that ways in which their conditions have been characterized in medical and scientific domains are not only inaccurate, but actively harmful. Damian Milton, a sociologist who has himself been diagnosed with ASD, has pointed out that such critiques from the autism community are the "least attended to," and that therefore, their own claims of expertise in autism studies are infrequently heard or accepted by biomedical domains (Milton, 2014; see also Milton 2012). As he puts it,

> …autistic people have often become distrustful of researchers and their aims, and are frequently frozen out of the processes of knowledge production. Such a context results in a negative feedback spiral with further damage to the growth of interactional expertise between researchers and autistic people, and a breakdown in trust and communication leading to an increase in tension between stakeholder groups. The involvement of autistic scholars in research and improvements in participatory methods can thus be seen as a requirement, if social research in the field of autism is to claim ethical and epistemological integrity. (Milton 2014, 794).[7]

It is my contention in what follows that addressing the various crises of legitimacy in scientific experimentation requires a greater attention to *practices of interpretation*, rather than systems of replication. This suggestion does not contest the "replication crisis" claim that some, perhaps many, scientific experiments are neither replicated nor replicable.[8] It does, however, challenge the notion that "repairing" the replication process alone will restore science's credibility and broad epistemic valuation. Instead, I propose to take up another rhetorical and methodological framework—*interpretation*—that can accommodate legitimacy claims *both* from within and from outside disciplinary science. Thus, a focus on interpretive practices can include concerns about violations of methodological standards. At the same time, it invites consideration

[7] See also Pellicano and Stears 2011, Pellicano et al. 2014.

[8] If they are, as Peterson and Panofsky (2020a) suggest, the problem is best understood not as a sweeping one, but within the radically different methodologies and modes of verification practiced in diverse physical, natural and computational scientific fields.

of interpretive practices *outside* of those standards that are also involved in the loss of public faith in science, the breakdown of science's self-corrective mechanism, and the propagation and canonization of inaccuracies.

In this dissertation project I focus specifically on interpretive practices in cognitive psychology and cognitive neuroscience experiments. Here, I am especially interested in the shift from social psychology to a subfield of cognitive neuroscience that, in the past 20 years, has turned its interest to *social cognition*, looking for the neural bases underlying social behavior. In Chapter Two, to illustrate the ways that, even in the absence of replication trouble, interpretive practices in cognitive neuroscience experiments can lead to delegitimations of lived experience and the canonization of inaccuracies, I turn to the case of mirror neurons. In elaborating the experiments and publications that introduced this class of purportedly specialized brain cells, I focus on the interpretive moves buttressing claims that mirror neurons can account for human intersubjectivity. By a careful analysis of the interpretive practices that shaped scientific and popular understandings of mirror neurons, I show that questions of legitimacy here emerge not from replicability issues, but rather from challenges from the hermeneutic social sciences (particularly ethnomethodological literatures), as well as from autism communities affected by the rippling effects of mirror neuron claims. In doing so, I set the stage for the proposal developed in the rest of the dissertation, arguing for the importance of critical engagement with multiple perspectives *at the site of experiment*, where insights from STS, activist communities, and others can be brought to bear upon situated experimental practice.

In her recent book *Objectivity and Diversity*, Sandra Harding draws upon foundational concerns in feminist STS with her radical suggestion that "sociopolitical commitment to certain kinds of diversity and epistemic-scientific commitment to objectivity need not conflict with each

other" (2015, 150). Following Harding, I propose that multidisciplinary engagement at the site of experiment stresses critical collaboration, in order to make "divergent investigative approaches…visible, actionable, manipulable, and theoretically viable to each other," an endeavor to "materialize and entwine divergent concerns, priorities, subjects and objects" (Klein and Gluzman 2015). This move involves participating in the methodological life of the lab while also introducing methods, histories, and literatures not normally included in lab concerns. In my own case, elaborated in this dissertation, a collaborative, experimental STS is informed by the histories, theories and "methodo-logics" of experimental theater.

Laboratory to theater

My own first-hand experience with laboratory experiments began as a student researcher, working with hamsters in a circadian rhythm lab throughout college. Even in trying to simply describe this work in one sentence, I am torn between the statement that I worked "with" hamsters versus one where I worked "on" hamsters.[9] Procedurally, the protocols of this work involved caring for the hamsters while they were housed in particular conditions of light/dark exposure, sacrificing anesthetized individuals who were part of experimental trials, removing their intact brains, cutting these into extremely thin slices and processing these slices to identify whether, and how, particular proteins of interest were expressed in brain regions suspected to help regulate daily rhythmicity. This was long before I had encountered any of the literatures of STS, and thus had no language through which to express my heightened sense that, while we conducted experiments, held lab meetings, read background research, and managed the daily upkeep of the lab and its hierarchical but friendly relations, there was something interesting and

[9] This distinction, between "objects" to be manipulated and "others" as collaborators, shall be discussed extensively in Chapter 2.

fragile about these very conditions that enabled us to focus on the scientific questions that the lab was organized to address. It was not until my senior year of college, after working in the lab for four years, that I began assembling the resources for thinking more broadly about what an experimental enterprise entailed. These resources did not come from the biopsychology lab, or the philosophy, history or sociology of science, but rather from the shock of making theater.

My first real experience with human experimentation was not in the laboratory, but in the rehearsal room. I was conscripted by a poet friend to direct an off-off-off Broadway production of a Harold Pinter one-act play called *A Kind of Alaska* (1983). It was to be performed in the basement of a Columbia University-owned brownstone on 113th Street, and my friend, Filip, would be both producing and performing in it. I had never directed a play before, nor had any meaningful theater experience other than as a highly attentive and sensitive spectator. I protested that I was inadequate to the task, but Filip convinced me with the argument that, after all, *A Kind of Alaska* was about a woman with a brain disease who awakens out of a 40-year coma, and therefore my university training in the brain sciences made me uniquely suited to direct.

"Something...is happening." This is the first line spoken in *A Kind of Alaska*, by the protagonist Deborah who, after being given a shot of an experimental drug, wakes up from her decades-long sleep. Pinter had written the play after reading neurologist and essayist Oliver Sacks' 1973 book *Awakenings*, and had been inspired by Sacks' account of an entire ward of patients who were given an experimental drug (L-DOPA) and thus roused out of the *encephalitis lethargica*-induced comas in which they had spent the majority of their lifetimes.

Though I had no experience making theater, I did have formative experience reading Oliver Sacks. As a teenager, I had been deeply affected by his evocations of encounters with patients in *The Man Who Mistook His Wife For a Hat* (1985). The book is a collection of short essays, each

one a “case study” of Sacks’ own experience examining and treating an individual patient. Though a collection of case studies is not unusual for a neurologist reporting on neuropathologies, Sacks’ book—oriented to the lay-reader rather than the specialist—not only presented the patients’ symptoms with Sacks’ diagnosis, but also evoked his encounter with the person. As Sacks wrote in the book’s preface, his formal choice to present cases as “stories” was towards a holistic understanding of the person afflicted, the “individual and his history...the experience of the person, as he faces, and struggles to survive, his disease” (viii). He explains,

> There is no ‘subject’ in a narrow case history; modern case histories allude to the subject in a cursory phrase (‘a trisomic albino female of 21’), which could as well apply to a rat as a human being. To restore the human subject at the centre—the suffering, afflicted, fighting, human subject—we must deepen a case history to a narrative or tale; only then do we have a ‘who’ as well as a ‘what’, a real person, a patient, in relation to disease—in relation to the physical. The patient’s essential being is very relevant in the higher reaches of neurology, and in psychology; for here the patient’s personhood is essentially involved, and the study of disease and of identity cannot be disjoined. (ibid.)

Sacks’ accounts of each case were rich portraits, not only of the patient in question and their experience of themselves in the world, but also of Sacks himself as he examined and struggled to understand them, both through direct questions and through simple tests, often using objects at hand: his pocket-watch, a nature magazine, a window. These portraits even included evocative descriptions of the surrounding characters—the weary nurses and attendants in the institutions where some of the patients lived, the daily routine of families and friends. By centering not only the patients but the ways in which they were encountered and known, Sacks portrayed each case, each “disease,” as an attribute not of the patient, but of the encounter, of the particular patient living through a particular personal and social place.

Though Sacks’ diagnostic examinations were, in some sense, experimental (using tasks and observations of responses as empirical tests to gain insights about these individuals and their

impairments), Sacks' encounters were, of course, clinical ones, not laboratory experiments. In this sense, Sacks did not deal with controlling variables across cases nor was he primarily oriented towards the goal of aggregating and abstracting data; indeed, for him, the *process* of producing data (e.g., the interaction around making a request to a patient to draw a pocket-watch) could be just as important as the data produced (e.g., the patient's drawing).[10] This encounter-based perspective on the clinical process was made available to readers through Sacks' evocative prose, his setting of each scene and directing attention not only to the so-called facts of a case, but to their textures and tones. "I feel myself a naturalist and a physician both," wrote Sacks, "and that I am equally interested in diseases and people; perhaps, too, that I am equally, if inadequately, a theorist and dramatist" (vii).

Sacks has written of this dual-tendency as something that persisted from his early years, but also acknowledged the realization that this sensibility could be formalized through a literary register he first encountered in the work of Soviet psychologist Alexander Luria. In a 2002 video recording of an interview with Michael Cole, Sacks explained, "An entirely different sort of excitement came in when I saw [Luria's] *The Mind of a Mnemonist*. I read the first 30 pages thinking it was a novel! And then I realized it wasn't a novel but a wonderful case history with all the accuracy of science but all the sensibility of drama and structure of the novel (...) [S]eeing *The Mind of Mnemonist*, I think, fortified me in my own feeling that I had to attempt some similar sort of portraits of my own patients, which is what I did in *Awakenings*" (Cole, 2002, 16:30-17:27).[11]

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[10] This distinction will be important in subsequent chapters, especially in Chapters 4 and 5, through what Douglas Maynard has called the "concrete competency" needed for ongoing coordination between an experimental subject and researcher in order that the researcher can test the subject's capacity for a particular sort of "abstract competence" targeted by the study (Maynard and Turowetz 2017).

[11] See also "Luria and 'Romantic Science'" (Sacks 2014). In considering how laboratory experiments can draw out situated cognitive phenomena, the relevance of Luria's work, and especially that of his teacher Lev Vygotsky, shall be addressed in Chapter 5.

Could one be a theorist and dramatist while being an experimentalist? Faced with the juxtaposition of the rehearsal and the brain laboratory, I began to think about laboratory experiments through the experience of making theater. Like classic STS literatures on the daily work of laboratory experimentation (Hacking 1983, Collins 1985, Galison 1988), a theater focus grounded my interest in experimental performances, the ways experimental stagings played out in practice. Thinking experiment through theater made it difficult to generalize, to philosophize; it kept bringing me back to the event to look again and ask again about what was happening.

Something is happening

What was it that we[12] were doing when we gathered to rehearse *A Kind of Alaska*? It seemed to me that we were experimenting, a feeling informed by my experiences in both lab and rehearsal room. An experiment in the circadian rhythm lab had been organized around a central pivot—what Hacking, quoting Francis Bacon, called a "crossroads"—in which the *crucial experiment* was designed to unambiguously identify which of the possible paths was empirically supported by the experiment's results.[13] The experimental process we conducted in the rehearsal room, however, was not a fork in the road but a horizon; in order to reach that horizon we had to bring into being a world in which that horizon was reachable.[14] The horizon, in our case, was

[12] "We" were Greta Goetz (playing the part of Deborah, the protagonist), Filip Marinovich (the Doctor), Elizabeth Pierson (Pauline, Deborah's sister), and myself.

[13] Hacking 1983, 249-50. This description of the laboratory may be controversial for Hacking, who claims that experiments must rely on a network of tacit and explicit knowledge in order to serve a heuristic function. With respect to them, in the local practice of the lab where I worked, the "fork in the road" experimental structure was explicit as we were trained in designing experiments, and in the difference between an inadequate experiment full of confounds and an "elegant" experiment that artfully disambiguates between finite explanations.

[14] By invoking "horizon," I gesture to Husserl's phenomenological notion of an "indeterminate actuality" as dynamically informing a natural attitude, taking up Camp's important point that Husserl's horizon derives from the directionality and architectonics of theater (Camp 2007, 626-9). This shall be addressed in Chapter 2.

mapped by the playscript, the text and dialogue written by Pinter. The world in which that horizon was inhabitable, however, was what had to be discovered.

The first line of the play, "Something…is happening," is spoken by the waking Deborah as she emerges from a 40-year coma. What *was* happening? This was the question we consistently asked of the characters, of each other, of the room. Answering the question was an ongoing process of experimentation: conjectures and revelations through provisional, embodied doings. The provisionality of these doings was what made this an experimental process; every doing was an opportunity, an invitation, and an insistence to understand together what was happening. Through doing and redoing we were moved to grasp, in action, the rules of this play-world and the necessary meanings that motivated precisely these scripted utterances and events. This was not a process of crafting an illusion, but rather an iterative practice of reverse-engineering, or what I have called *re-membering*,[15] an experienced and embodied logics of interaction.

Was this, as I strongly felt it to be, an *experimental* process? And if so, how to reconcile the experimentality of the rehearsal room with that of the laboratory? What might it mean to understand laboratory experiment through theater? In the science laboratory, conducting and analyzing experiments necessitated a bracketing out of all activity that was extraneous to the variables being tested. In the rehearsal room and performance, however, all activities mattered to the meanings, events, and insights evoked. In the lab, relationships, roles, hierarchies, and responsibilities constituted the steady, unquestioned background hum of daily practice; in rehearsals, relationships, roles, hierarchies, and responsibilities were available materials to think about, and through. The ethics of the experimental encounters in laboratory research are codified

[15] Gluzman 2017.

in regulatory guidelines for both human and non-human experimental subjects.[16] In the theater, the guidelines for ethical practice (especially when outside of explicit labor frameworks and union contracts) are nebulous. While that can create conditions for rampant abuse (and, to be sure, harm happens—both between theater practitioners and collaborators, and toward audiences), it also foregrounds the practical ethics of theater experimentation as intentional, contingent, effortful and meaningful. This foregrounding enmeshes the ethics of encounter with the epistemics of theater, such that the one cannot be considered apart from the other. What can be known in theater? What sort of knowing sustains activity in theater? These questions are entangled with the relational aspects of theater's production, performance, and ongoing reciprocal reception.

In performance, when Greta spoke Deborah's first line, it was a hiss, ascending from nowhere:

"ssssssssssomet'*ing*….."

It was like sliding through a push, an effort that you didn't choose but you found yourself making, unable to stop. So, too with second "s":

"ssssssssssomet'*ing*…..issss"

But with "happening," it was an exhale, all at once, an arrival:

"ssssssssssomet'*ing*…..issss… hahppening!"

[16] For human participants, these guidelines were articulated in the Belmont Report, which requires informed consent and for experimental methods and protocols to ensure the respect of persons, beneficence and justice (National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research 1979). For non-human animal participants, like the hamsters in my own lab, "3R" guidelines were developed in 1959 to ensure humane techniques in animal research. These guidelines asked that researchers articulate the ways their experimental paradigms would (1) Replace more sentient subjects with less or non-sentient ones, when possible; (2) Reduce the number of animals used in studies, and (3) Refine experimental methods to minimize animals' pain, suffering or distress (Russell and Burch 1959; for questions regarding the adequacy of these guidelines, see Ferdowsian et al. 2020). In the US, any biomedical or social science research must undergo review by specially appointed Institutional Review Boards to ensure that their proposed experiments abide by the relevant guidelines, as I did before undertaking the studies I discuss in Ch. 3 and Ch. 5.

I perceived this utterance to be an experimental result, a matter of fact inscribed indelibly in my memory. We had found our way there, and, finding ourselves there, felt that we'd arrived. Yet it was decidedly unlike a result, since it did not settle the matter, for example, of *what* was happening; it instead thrust us all into the asking.

For the utterance to do this, however, required the participation of intense spectatorship; it was, after all, theater and, as Michael Fried once scolded, a non-autonomous art form in its reliance on an audience for actualization (Fried 1998, 153-4). As the director, I would become that intense spectator in the rehearsal room, standing in for and exemplifying the gazing and breathing of an imagined future audience. It was a weighty privilege. I felt the privilege in the heat of discovery, the elation of thinking and making together, the privilege of both proximity and distance in knowing "a suffering, afflicted, fighting, human subject" (Sacks 1985, viii). But the weight—the ethics of encounter and my own part in it as "the director"—I felt in agonizing responsibility: to performers and collaborators, to friends and spectators, to the text, the playwright, to the characters and their silences. It was a weight reflected back to me through the character of Deborah's Doctor, who trembles upon her awakening but fails to shepherd her into a safe and orderly world.

After *A Kind of Alaska*, I began a performance group called "Science Project," and went on to make dozens of theater works, some based on written plays, some devised with collaborators, and some performed as non-consensual interventions into the lives of unwitting participants. I thought of and described my work as "experimental," but since the term is used rather loosely in the arts (sometimes connoting unusual or unexpected work, often used synonymously with "avant-garde"), it remained up to me to obsess over what "experimentality" might mean in a theater practice that was, in some way, coterminous with the practice of science.

I explored experimentality in the theater through fifteen years of making theater. When I read theater director lin hixson's contention that "I discover performance by making it" (hixson 2009, 444), I recognized in the phrase a collective learning-by-doing process that was the core practice of theater-making as I have experienced it. The "experiential give and take" (Conquergood 1991, 181) was the condition of possibility for theater, meaning that (whether or not you were on stage) the theater event is to be known from inside of it, in encounter, in movement (Sheets-Johnstone 1981) and by *perezhevanie* or sensing/feeling/living through it (Vygotsky 1971). It was never a passive knowing, but an effortful one requiring commitment and intention; it was also never a private, individual knowing, but one conditioned by and conditioning the recognition of others. It is perhaps because of this effortful intersubjectivity that I felt the theater—and later, the lab—as a crisis of responsibility through which to meet a world.

Romantic Science

Alexander Luria, one of Vygotsky's closest collaborators and a later champion of Vygotsky's legacy of the cultural-historical approach to psychology, was himself dedicated to what he called "romantic science": a novelistic approach to describing a particular person in the fullness of their disorder and thus in the fullness of a shared world. As Oliver Sacks explained, Luria's work in this vein influenced his own realization that, to "restore the human subject at the centre—the suffering, afflicted, fighting, human subject—we must deepen a case history to a narrative or tale; only then do we have a 'who' as well as a 'what', a real person…" (Sacks 1985, viii). Sacks felt himself, too, to occupy double roles, of physician and naturalist, and wondered humbly if "perhaps, too, that I am equally, if inadequately, a theorist and dramatist" (vii). Yet, in his essay on Luria's notion of Romantic Science, Sacks recognizes that this doubleness is not a

contradiction, but is indeed the condition of studying mind that must be formally and dynamically reconciled:

> Luria often said that he needed to write "two sorts" of books, but he always saw these two sorts as identical in principle. What then is the principle that unites all his work…? Luria himself spoke of his groping as a young man, of his attempts to reconcile two conflicting viewpoints—and of the "crisis," not only in himself but in the scientific community generally, to reconcile…an explanatory, physiological psychology with a descriptive, phenomenological psychology—to reconcile a "classic" and "romantic" approach to the higher cerebral and psychological functions" (520).

Orienting to the dramaturgies of encounter, as Oliver Sacks oriented to the clinical case study through the form of the novel, is my way to seek singular others as they are dismembered and reconstituted in the doing of experiments, whether these be in theaters or laboratories. Gesturing toward the proscenium-exposing experimental theater of Bertolt Brecht,[17] this move is also an attempt to denaturalize or alienate the interpretive framework—the literal and material frame—of an experiment to enable interpretation by other means. In the chapters that follow, I grapple empirically with attending to the proscenia of experiments, their *mise-en-scène* and dramaturgical unfolding, and in doing so ask what these framings omit and what interpretations are possible. As I once experienced the heavy responsibility of making theater in the rehearsal room, returning to the laboratory invites me to attune to what the stakes are—for researchers,

[17] In his article "On the Experimental Theater," mid-twentieth century theater director Bertolt Brecht proposes a new relationship between the performance and spectator, one that does not rely on sympathy, the "*pity* and *terror*, the twin-yoked classical cause of Aristotle's Catharsis" (Brecht and Mueller 1961, 14, italics in original). This new relationship is based on alienation. "To alienate an event or a character is simply to take what to the event or character is obvious, known, evident and produce surprise and curiosity out of it. Let us consider again the wrath of Lear over the thanklessness of his daughters. Through the technique of sympathetic understanding the actor is able to present this wrath in such a way that the spectator sees it as the most natural thing in the world, so that he cannot imagine how Lear could not become wrathful, so that he is in complete agreement with Lear, sympathizing with him completely, having himself fallen into the same wrath. Through the technique of alienation, on the other hand, the actor presents the wrath of Lear in such a way that the spectator can be surprised at it, so that he can conceive of still other reactions from Lear as well as that of wrath. The attitude of Lear is alienated, that is, it is *presented as belonging specifically to Lear, as something shocking, remarkable, as a social phenomenon which is not self-evident*" (ibid., italics added).

subjects, and others—of the experimental performances that stage and investigate the mind in society.

Contributions, Methods, and Stakes

Responding to calls from social science scholars for more collaborative,[18] interventionist,[19] or constructive[20] engagement between biological sciences and other disciplines, my study of laboratory experiments in social psychology and cognitive neuroscience takes up engaged, reflexive methodologies and concepts from theater scholarship and practice.[21] Thus, drawing on notions of *performativity* in both STS and in performance studies, this project understands itself as intervening in (not simply describing) the laboratory and theater practices it takes as matters of concern.[22] The approach that I take in the work that follows stresses engaged participation in the site of experiment, at once making myself subject to the forces that shape the daily practice of doing experiments, and pursuing a dialogic, collaborative mode in which I make myself accountable to potential interpretive possibilities realized through the doing of experiments.

By embedding myself at the ground level of experimental practice, in continuous conversation with scientists and others, I hope to contribute not only to STS literatures on experiment, but also to the communities directly responsible for and impacted by such experiments. Thus, addressing the disconnect between STS critiques of science and those defined

[18] Fitzgerald and Callard 2015, Tallbear 2014, Harding 2015.
[19] Zuiderent-Jerak 2015.
[20] Law 2004, Latour 2010.
[21] Informed by critical and historical work (Diamond 1997, Jackson 2004, Ridout 2006), my theater training with Anne Bogart (e.g., Bogart 2001) and my 15-year experience making theater in various contexts (e.g., Smalec 2003).
[22] The ways that theater and performativity provide resources for this work shall be addressed in Chapter 1. The concept of a "matter of concern" (itself a response to the concept of a "matter of fact") has been discussed in Latour 2004 and de la Bellacasa 2011, while "intervention" has been theorized by Law 2004, Mol 2002, Barad 2007, and Zuiderent-Jerak 2015.

by scientists' own replication crisis discourse, I attend to interpretive practices in experiments by positioning myself as a co-interpreter alongside scientists. This sort of direct engagement with interpretive practices in the lab, I propose, can materialize more inclusive, impactful and practical possibilities for collaboration toward a more broadly legitimated and valued science.

My methodological approach is three-fold: First, I pursue an exegesis of published scientific reports to draw out relations between experimental design—what I call the staging of these experiments—and their modes of interpreting the data produced. Second, I use concepts and reflexive methodologies from theater and performance studies to involve myself as a *participant-interpreter* in the design, enactment and analysis of experiments, allowing not only description of but also collaborative involvement in interpretive possibilities therein. I understand the term *participant-interpreter* (as opposed to a participant-observer) to mean that, more than gaining a "unique adequacy"[23] or working competency with lab members' methods, my participation in the lab also introduces a new member and perspective into the community. In this sense, my own attempts to coordinate with meaning-making in the lab are part of the study I conduct here. Finally, in order to consider interpretive practices as they play out in moment-to-moment lived experience of various participants, I use an ethnomethodological approach to analyze and describe these experimental interactions as captured in video recordings. An ethnomethodological approach invites attention to the multimodal ways that members reciprocally demonstrate their practical reasoning to maintain ongoing interaction and coordination with each other and their shared settings.[24] In its focused attention to lived perspectives and collaborative sense-making, ethnomethodology is highly suited to the task of noticing interpretive practices from the points of view of participants themselves.

[23] Garfinkel 1985, p. 10.
[24] Garfinkel 1985 [1967], Lynch 1993, Goodwin 2000, 2003, Alač 2011.

Though the stakes of this project will be elaborated through each chapter, the core of these stakes lies in the slippage between *object*, *agent*, and *other* that suffuses the history of scientific experiment, conditioned by ideals of "the greater good" and often along cultural and historical markers of marginalization: race, class, disability, and gender among them. As Cathy Gere points out in discussing the "sordid experiments"[25] that mar the history of human experimentation, the "systematic bias built into utilitarian medicine…permitted *any* vulnerable citizen to be sacrificed on the altar of science. Time and time again, nonconsensual research on terminally ill, marginal, impoverished, incapacitated, undereducated, or institutionalized human beings was rationalized on cost-benefit grounds (…) [A]nyone with little enough to lose was fair game in the cause of medical progress" (Gere 2017, 9). While the experiments discussed in the remaining chapters do not, strictly speaking, deal with issues of informed consent, they follow Alač's proposal (2011) that it is necessary to look at moment-to-moment encounters to respond to the ways in which subjects, objects, researchers, and interpretations are articulated and acknowledged in practice.

In this, the situated dramaturgy of experiments matters. If experiments can be thought of as a script or apparatus (as suggested by Klein 2014), their *enactment* shapes the sorts of bodily actions, identities, and relations that emerge dynamically in their doing[26]. In attending to this dynamic emergence, we see that there is no simple imposition on or "objectification" of the experimental subject; the setting of experiment is itself a social world within which the subject is invited to interpret the scene and to articulate themselves in specific (that is, constrained) terms. Klein's account of her own participation as a subject in a brain imaging experiment highlights

[25] Hacking 1983, 1. This phrase and its context are discussed in Chapter 2.
[26] It is this point of emergence, the meaningful enactment of the experimental script, that I call its dramaturgy. This is elaborated in Chapter 2.

the ways an experimental setting can promote certain kinds of self-articulation, while suppressing others. Klein writes of her experience in an EEG study,

> I sit as still as possible so as not to interfere with the recording of my brainwaves. I am trying not to blink. I am trying not to move my eyes or my face or my body. I am being a good subject, I hope. I am trying to keep my body out of the way so that my brain can be measured. (Klein 2014, 88)

What does it mean to become a good experimental subject?[27] How does one go about taking up one's role as "a child with autism" in the performance space of the laboratory experiment? What—or who—is at stake in the staging of the mirror neuron studies? Asking these questions is not solely a matter of understanding the role of experiments and their legitimacy in the project of scientific reasoning, but is also toward identifying and expanding how experiments can open themselves to competing interpretations and enriching modes of narration. Across its various empirical, historical and theatrical sites, this project considers how the subjects, objects, and relations that emerge in experimental interactions persist, resonate, and form facts, identities, and hierarchies of knowledge.

My engagement with laboratory experiments on other minds in the chapters that follow are grounded in my experience making theater. Rather than a preoccupation with their illusory, role-playing or spectacular elements, I orient to their dramaturgy: their material and procedural conditions (their staging) in respect to the "somethings" that "are happening" in their meaningful unfolding. A grounding in the practice of theater does not place me outside the doing of experiment, with a view privileged by distance, but rather enmeshes me as a character responsive to, and responsible for, the contingent drama that ensues. In what follows, I orient to the staging

[27] See also Emily Martin's (2016) discussion of the training of the experimental subject as a bracketing of subjectivity towards objectivity in the experimental psychology lab.

of experiments by "living through"[28] their performance and attending to their moment-to-moment enactment in order to articulate "the human subject at the centre" (Sacks 1985, viii) *as an experimental achievement* rather than a natural presupposition.

Overview of Chapters

In the first chapter, I lay out my methodological commitments by setting them alongside and against performance-as-research (PaR) as an academic research methodology. Thus, I take up discussions about the validity and legibility of PaR in the academy by challenging its meaning in respect to other methodologies. The term, I argue, posits "performance" as an outsider, an unruly (at best) method alongside other conventional, generalizable and objective methods used elsewhere in the academy. Instead, I suggest that all scholarly methods—even those we take as models of scientific objectivity and progress—are grounded not only in performance and performative sense-making, but also in the materialization process that characterizes the theater. This, I propose, is not Performance-as-Research, but rather Research-as-Theater (RaT). Because our scholarly practices are constructive, not only of knowing but also of what we seek to know, indeed of the "we" that can know, must we not reflect on the material and materializing structures of scholarship? Might it be possible to consider the methods of scholarship as consistent with the conscious and contingent manipulation of boundaries, temporalities, and entities that characterize the theater? To think through these issues empirically, I analyze a conference lecture in which—attempting a RaT approach—I engaged scholarly methods as

[28] "Living through" references Vygotsky's notion of *perezhevanie* (Vygotsky 1994 [1934]), which describes experience as a first-person encounter with an ongoing environment. Vygotsky stresses that this is a personal experience (i.e. the environment will be experienced differently), while also evoking the durational and immersive aspect of "living through" something. Also relevant is the other meaning of "living through," i.e. experiencing life through connection with a surrogate encounter with the world, as one might when reading novels or watching a play, for example.

reflexive theatrical performances, asking what sorts of relations and logics follow particular stagings. Foregrounding (and, in the lecture, superimposing) the relationship between an object of research and a mode of researching is a way to take up theater as a method, pursuing the experimentality of the experimental theater rather than defaulting to the objectivist logics of the laboratory experiment.

In Chapter Two, I focus on the influence of historical and cultural practices of theater on the development of particular "dramaturgies" of scientific experiments. Here, dramaturgy is used as a term that incorporates both the design and situated doing of experiment, and refers to the dynamism and multiple meanings of staging in historically and culturally situated enactment.

Rooted in 1980s STS accounts of laboratory experiments as producing the phenomena they are designed to investigate, I turn to human experimentation and the dramaturgies through which they produce phenomena about the human social mind. While I reject the theatrical metaphor as an approach to understanding experiment, I ground my subsequent analysis in specific histories of the mutual elaboration of theater and scientific experiment. Taking up Tiffany Watt Smith's (2014) notion that theatricality is constitutive of scientific objectivity and Pannill Camp's (2007) suggestion that the architectural theater proscenium conditions contemporary modes of observation, I compare the experimental dramaturgies of social psychology research and mirror neuron studies. I use Camp's example of the vanishing proscenium to describe how an experiment considers or brackets out the conditions that animate and situate its own enactment, and in doing so, I articulate my own commitments toward the experimental practice I undertake in the remaining chapters.

Chapter 3 begins discussing the design of a laboratory experiment on autism and creativity, undertaken in collaboration with cognitive neuroscientist Dr. Jaime Pineda. This

chapter reviews some of this key background literature, first on creativity writ large, and then on creativity and autism. Rather than moving through a great number of research findings and theories of autistic creativity and its deficit, the chapter focuses intensely on a small number of key studies that stood out as particularly relevant among the background literature we read as we deliberated over the design of our own project. For each of these studies, my theatrical orientation involved a close reading of how experiments are staged in order to understand the multiple ways those experiments could be experienced and thus interpreted.

The chapter begins with establishing the background and problematics of laboratory research on creativity writ large, focusing on internal debates over validity and best practices in evoking and measuring creativity in laboratory situations. Then, moving into the specific area of research that looks at creativity in the context of autism, I focus on Craig and Baron-Cohen (1999), a seminal study that established the existence of a "creativity deficit" in autistic children, and related this to an impairment in Theory of Mind, the ability to perceive the mental states of others. By considering the various experiences generated in the doing of the experiment, in relation to its the published analyses, I suggest that the interpretation offered by the study's authors did not inevitably follow from their data, but rather from the experiment's design and performance. The problematic interpretations of the 1999 study are both conditioned by previous research and shape subsequent research. We find similar "deficit-centered" designs and interpretive practices in two related studies. Shaping the questions and methods of the 1999 study was the use of the Sally Anne test, a false belief test using dolls to enact a social situation (Baron-Cohen et al. 1985). Following both of these was a series of autism studies using the Animated Triangles task, a Theory of Mind test based on the Heider-Simmel (1944) animation.

The chapter concludes by relating these three experiments to commitments that we would use to structure our own study, namely: (1) studying children in interaction, rather than in isolation, (2) providing an open-ended and genuinely engaging task that allowed for children's special interests, and (3) qualitatively analyzing the process of creative engagement as a primary heuristic to lead quantitative data analysis.

In Chapters 4 and 5, I present and analyze the interaction of the researcher and experimental subject in the doing of our pilot experiment on autism and creativity. In these experimental sessions, I am myself the researcher interacting with experimental subjects, allowing me to augment the ethnomethodological analysis of our recorded interactions with my first-person experiential account. This participatory position positions the analysis in respect to questions of the requirements of scientific neutrality and ethnomethodological indifference of the analyst, which foregrounds moments of noted non-neutrality in these experimental interactions. In doing so, this experiment was also situated in respect to efforts to understand how the experimenter-subject relationship—what Roepstorff and Frith (2004) called the "joint enterprise" of performing an experimental task—impacted experimental performance, and thus the ways in which the experiment might lend itself to interpretation.

Attending to the situated instances of my own non-neutral exclamations of praise, interest and affirmation suggested that, rather than being a confounding variable to undermine the experiment's ability to elucidate the creative process, these affirmations instead pointed to salient aspects of the distributed quality of creative engagement. This distributed quality of the creative process, articulated across the subject, myself, and the material setting that we engaged, in turn was seen to organize our engagement with the animation task and our variable authorial relationship in respect to it.

Following our moment-to-moment interactions in the doing of the experiment had implications for both the study of creativity writ large, as well as for research on autistic creativity specifically. At the core of these was the observation that the *standards* for creativity—the shared sense of what could be taken as an "appropriate," or more or less creative move—emerged locally, in our interaction. While this supports similar convictions by, for example Sternberg (2019), this study allowed for a dynamic sense of how these standards emerged as sharable in the situated interaction of our collaborative activity.

CHAPTER 1

Research as Theater (RaT)

0. Intentions

While it is no longer outrageous to claim that performance can be a method of investigation, knowledge production and scholarship, the current literature grappling with how to understand and institutionalize performance-as-research shows that work remains to be done in exploring how particular performance practices can be commensurable with the accumulation, verification and dissemination of what counts as academic knowledge.[1] Yet, even in thus articulating the problem, there is a presupposition that performance is just one (unruly) member of a broader category of (otherwise adequate) scholarly methods. In this formulation, performance is the outsider looking in, and thus assumes the burden of justification and assimilation as it attempts to enter the academy. The presumed adequacy of more conventional research methods – always in relation to the purported *objectivity* of the scientific method – still dominates tacit understandings of normative academic knowledge production, and inflects the ways in which *performance as research* (PaR) is theorized as both challenge to and member of the academy. In this chapter, instead of conceptualizing performance as a type of research, I begin with the inverse premise, that scholarship is a type of performance.

There are two senses in which scholarship – the process of reading, collecting data,

[1] For example, Arlander et al. 2017, Nelson 2013, Peccini and Kershaw 2004.

teaching, communicating with disciplinary peers and disseminating research outcomes[2] – can be understood as a kind of performance. One would be the claim that research is *performative*; this is the notion that scholarship, even when it is concerned with simply describing an aspect of the world, not only is acting upon and shaping that world but also is itself constituted as scholarship through a "citational" reiteration of ritualized norms and accepted modes of speech (e.g., Nelson 1999; Dewsbury 2000; Cabantous et al. 2016). This sense of the performativity of scholarship is heavily indebted to Austin (1962), who suggested that, though some utterances seem to be performative (like the assertion, "I promise"), even non-performative speech acts (like the statement "I am writing") impact the situation in which they are uttered, shaping the constraints on meaning for present and future utterances. Also crucial for this sense of scholarship as performative is Judith Butler's (1988, 1990, 1993) specification of *performativity* as a process in which seemingly stable, naturally occurring phenomena (like gender identity or subjecthood) are produced through citations or reiterations of preceding practices. These citational acts, occurring on a massive social scale, shape a nexus of authority that undergirds the logics and form of subsequent acts. Building on these two notions of performativity, then, we might begin to suspect that to acknowledge the *performativity of scholarship* is to call for reflexive attention to the compulsory citations that shape the research process, and to the identities, relations and objects that emerge as facts through these acts of research. While the argument in this chapter relies on this first sense of *scholarship as performative*, it is the second sense of scholarship as performance that I want to develop, a less popular view that considers *scholarship to be a kind of theater performance*.[3] In making a distinction between performativity and theater, I continue

[2] Throughout this chapter, I use "scholarship" and "research" interchangeably to refer to this range of activities.
[3] See, for example, Crease's examination of scientific experimentation as theatrical arrangements (Crease 1993), which will be further discussed in Chapter 2.

to cite both Austin and Butler, who each famously distinguished performative acts from theatrical performances. Austin excluded theatrical speech from his argument about performative speech acts because theatrical speech is *artificial*[4] and thus an "etiolation" (or weakening) of language (Austin 1962, 22). Butler specified performativity as distinct from theatricality because theater and its role-playing presumes an *intentional subject*, one who can realize or "de-realize" (1988, 53) an identity, masking the crucial "compulsory" nature of the citationality through which that subject is formed (1993, 22).[5] Though neither Austin nor Butler is concerned with developing a theory of theater, by specifying performativity against the artifice and intentionality of theater, both Austin and Butler add fuel to the complex logics of a long-standing anti-theatricality that permeates Western philosophy (Barish 1981; Puchner 2002) and inflects the position of theater studies in the US academy (Jackson 2004).[6]

Needless to say, the exclusion of theater from the notion of performativity is a familiar story for many readers of this volume. Lest my dredging up the anti- theatrical prejudice seems gratuitous, I may as well admit my intentions: here, I hope to suggest that theater performance is central to the performativity of scholarship, allowing scholars to engage not with *fact* of ongoing

[4] The status of theatrical speech as artificial is related to the mimetic or representational project that, for Austin, marks the theater as separate from authentic and impactful action in the world.

[5] In Butler's "Critically Queer," she does attempt to re-specify theater performance through drag as resistance to compulsory gender norms through a hyperbolic display of those norms (Butler 1993). However, I will argue for a more central role of theater that is not based in its hyperbolic representation of social norms, but rather in its double-ness: as an intentional crafting of possible experience and simultaneously as an event supremely contingent on situated conditions that always exceed those intended.

[6] Shannon Jackson's influential discussion of the disavowal of theater by Austin and Butler in *Professing Performance* (2004) argues that the evacuation of theater from performativity (and theater studies from performance studies) was more than just another instance of an even longer-standing antitheatrical bias (Barish 1981; Puchner 2002; Ackerman and Puchner 2006). Jackson suggests multiple ways in which the exclusion of theater forms a *constitutive absence* around which the attributes of theater and performativity are defined in opposition to each other. Theater studies becomes associated with traditional cultural formations, craft and manual action, while performance studies is associated with interdisciplinarity (speaking to concerns in anthropology, sociology, cultural criticism and critical studies) concerned with, and articulated through, theory (performativity theory in particular) (2004, 189).

performativity but rather with the concrete, situated *processes by which scholarship is materialized*. So, instead of directly considering this volume's titular issue of "performance as research" (PaR), I hope to suggest that *scholarly research of all stripes* might take seriously commitments to performativity by engaging *experimentally* with their *theatrical underpinnings.* In other words, rather than directly addressing the complexities of PaR (in relation to a normative notion of scholarly methods), I'd like to consider RaT, or *research as theater*: the notion that theater is central to all scholarship, and thus available for experimental inquiry into the relations and knowledges produced through scholarly work. Here, I use "theater" to mean the intentional, contingent, semiotic arrangement of materials, entities and temporalities for an audience; similarly, "theatricality" is used to refer to the particular arrangement of manipulable resources. While this may seem general to a fault, my definition of theater is guided precisely by characterizations of theater in contradistinction to performativity: namely, that it is intentional (addressed to an audience) and artificial (crafted).

To consider the artifice and intentionality inscribed in theater, allow me to get personal (and by doing this, let me also assert the importance of *situated, subjective experience as a basis for knowing* what happens in theater). In my experience of theater, from my earliest memories of sitting among the audience as a child, I was never in doubt that what unfolded in those spaces was (forgive me) real. Yet this is not to say that I doubted the artifice of the event. Even then I understood intuitively that an actor being kissed on stage was receiving a *different* kiss than one received in private, but it was nonetheless a powerful (not *etiolated*) kiss, with consequences not only for that actor's mind and body but also for myself, for everyone in that room, and indeed for what I could only dimly conceive of as a totality of experience that transgressed the closed doors of the theater. As I grew up watching theater, I became more

skeptical of the fuzzy totalities that I'd intuited, yet what never flagged was my conviction that what happened in the theater constituted an exemplar of the inseparability of representation and action. In other words, while recognizing the particular arrangements of space, time, architecture and entities that constrained the operation of a piece of theater, I could also see that such "artificial" constraints could never resist or reduce the complexity of interactions that they made possible. Is this theater artificial? Yes. But importantly, as asserted by a number of theater scholars (Diamond 1997; Jackson 2004; Worthen 1998; Ridout 2006), the representationality and artifice of theater were performative through and through, challenging the idea that theatrical acts were specially "hollow" (Austin 1962, 21) or could be "de-realized" (Butler 1988, 527). As Donna Haraway puts it, "I learned early that the imaginary and the real figure each other in concrete fact, and so I take the actual and figural seriously as constitutive of lived material-semiotic worlds" (Haraway 1997, 2).

Later, when I became interested in theater as a field of engagement, I "made" performances, an activity that was relentlessly laborious, terrifying and rewarding. I qualify the word "made" because a vivid aspect to the experience of "making" theater was in fact the ambiguity of authorship and agency; one both is racked with the weight of responsibility of composition, of introducing a gesture or speech-act into a public space that did not necessarily ask for it, and at the same time, must come to terms, over and over, with the fact that any such occurrence is neither an "introduction"[7] nor in one's control.[8] When taking into account the

[7] "But where and when does a *performed gesture* draw its force . . .?" This is my theater- focused variation on Butler's question from "Critically Queer": "But where and when does such a performative draw its force?" Butler argues that performative acts are a form of "authoritative speech," but whose originating authority is not an individual subject but rather "a reiterated acting that *is* power in its persistence and instability" (Butler 1993, 17). I am suggesting that theatrical compositions are performative in their reiteration of avail- able semiotic precursors, even while they are introduced by an authorial subject.

[8] Texts on theater-making are therefore very concerned with the techniques and technics needed to reproduce a performance night after night. This is an obsession because such repetition is constantly thwarted by any number of

great hubris of theater-making at the same time as honoring its extreme contingency at every moment, theater begins to look more like the sort of material-semiotic interaction that Haraway described, in which "the imaginary and real figure each other in concrete fact" (1997, 2). Is theater intentional? Yes, it is not only intentional but also impossible, requiring enormous coordination of energy and matter to realize. Yet this intentionality is not in opposition to theater's necessary citationality and, as suggested earlier, potent consequentiality in iteration. And, above all, the consequentiality of the theater doesn't presuppose its illocutionary success, since its intentions are never simply realized, but always contingent, thwarted and undone.

This last point is worth unpacking a bit further, since it proposes that the intentional arrangements implicit in theater shape the events, relations and meanings that emerge from it, while simultaneously acknowledging that these effects are indeterminate and contingent not only upon intentions and arrangements but also on a much more complex field of situated encounters. For example, as Nicholas Ridout points out, it is especially in moments when the planned theater event slips, "corpses" and stutters that the smooth flow of representation that is its promise is interrupted, embarrassed and exposed as contingent upon the material and interactive conditions of the event. As Ridout says, "Something fails to take place amid what does take place" (Ridout 2006, 32), and this, like Lucy Suchman's notion of plans as situated actions,[9] is an opportunity to theorize intentionality *alongside* not only its compulsory citationality but also as deeply contingent on its situated material doing. Thus, when looking at theater performance as a performative – that is, re-iterative – event, there is an interesting conundrum between what is

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contingencies, from emergent moods in performers and/or audiences to unruly costumes to slips of the tongue. Likewise, authorship in theater performance, an event often made by multiple collaborators, is vexed (Worthen 1998).

[9] Suchman, a feminist STS scholar who has written about human-machine interaction, stresses that although plans (like intentions) certainly exist, they are better understood as representations of a problem or opportunity than as an explanation of subsequent action. Action, for Suchman, is contingent upon, and emergent from, situated *inter*action (Suchman 2007).

intended and what is produced.

This conundrum can perhaps be explored by taking up the argument of feminist scholar Rebecca Herzig, who critically examined science and technology studies (STS) notions of performativity alongside the performativity articulated by performance studies, and asks why both disciplines assume the compulsory and unproblematic productivity of performance.[10] In her words,

> Here, I wish to draw attention to a second, and largely unacknowledged feature of the vocabulary of motive evident in these studies: namely, that the activity described in such studies is invariably *productive*. Diseases, ships, natural philosophers, and termites appear equally governed by an imperative to produce some rhetorical or material effect. (Herzig 2004, 135)

Herzig suggests that the implicit functionalism of such an "inexorable logic of production" (ibid.) can be avoided by acknowledging the *excess* of performance. Relying on a familiar distinction between theater and performativity (and reminding us of the subtle anti-theatricality of such a move), she pits the "excess" articulated by *performativity* against the functionalism of *theater*:

> Unlike the theatrical sense of the word, the critical feature of the deconstructive performative, Eve Kosofsky Sedgwick points out, is its 'necessarily "aberrant'' relation to its own reference' (. . .) In the hands of writers such as Paul de Man, J. Hillis Miller, Jacques Derrida, Gayatri Chakravorty Spivak, and Judith Butler, the performative tends to maintain this sense of unavoidable excess: slippage, perversion, or occlusion necessarily conditions every inscription. (Herzig 2004, 136)

Herzig makes an important point about performativity that is, I argue, even more characteristic of theater – namely, that its artifice and intentionality is marked by, and undone through, excess. This is precisely Ridout's point in gesturing to the slippages of the highly contingent event of

[10] This productivity has also been referred to as "efficacy" (Bottoms 2003).

theater, as it inadvertently (or, in some cases, experimentally) exposes the methods and conditions of its own production.

1.1 "What are we doing" in science and technology studies (STS)

> *To study technoscience requires an immersion in worldly material-semiotic practices, where the analysts, as well as the humans and non-humans studied, are all at risk – morally, politically, technically, and epistemologically.* (Haraway 1997, 190)

I began with the suggestion that a consideration of PaR can be founded on the premise of RaT: that scholarly research not only is performative but also operates theatrically. I have argued that the theatrical underpinnings of scholarship can be unearthed by appealing to previous anti-theatrical characterizations of theater's *intentionality* and *artifice*, as it is enlivened or undone through the *contingency* and *excess* of their material enactment.

Here, I turn to the literatures of science and performance studies to consider the claim that RaT may have radicalizing potential for academic work writ large. STS literatures are relevant for a number of reasons: they have taken up performativity as a way to describe the production of scientific knowledge, have called for greater reflexivity and methodological experimentation, and, most importantly, STS has long been concerned with the question of *knowledge production*. Even before its disciplinary formation as STS,[11] sociological, historical and philosophical studies of science were already, in one way or another, interested in scientific knowledge: how it is produced, disseminated and applied, how it changes and how it differs from other sorts of knowledges. Whereas such scholarship in the first half of the twentieth century tended to understand science as an exceptional type of knowledge production (Merton 1938,

[11] The first US professional organization for STS, the Society for the Social Studies of Science, was convened in 1975 (http://www.4sonline.org/society).

1942), after the 1960s – as science joined the growing list of institutions of power to be dismantled by an increasingly anti-institutional public – the tenor of social studies of science shifted towards the critical. One of the targets of such critiques was science's purported objectivity. Much of this post-1960s STS work argued that scientific facts do not pre-exist their discovery, but are rather socially constructed.[12] STS literature on objectivity in science (like Daston and Galison 2007) unsettles – if not displaces – the grounds for an assumed primacy of the scientific method among scholarly research methodologies.

By the late 1970s, a small group of STS scholars became interested in shifting from sociocultural accounts that undermined scientific claims of objectivity to looking at the material, daily practices by which scientific knowledge was made. Assuming roles of laboratory participant-observers, these scholars imported the ethnographic methods of anthropology into what had previously been designated a historical, philosophic and/or sociological study of science. This new orientation, taken up by Knorr-Cetina (1981), Latour and Woolgar (1986 [1979]), Lynch (1985), Traweek (1988) and others, was to examine science in daily practice, scientists in their natural habitats, and scientific facts in the making. If, as Knorr-Cetina proposed, an STS concern with the "problem of facticity" could be more profitably seen as a "problem [. . .] of fabrication" (Knorr-Cetina 1981, 3), the attention to practices was a way to characterize *how* this fabrication occurred.[13]

The growing focus on laboratory practices initiated what became known as the *practice turn* in STS. Despite the ephemerality implied by the word "turn," the impact of the practice turn

[12] This is a large body of work across sociology, history and philosophy. For an overview of some of the key players of the 1970s from the perspective of the early 1980s, see Collins (1983). For a critical account of the development of STS, see Zammito (2004).

[13] See Knorr-Cetina's elaboration of this point in the section titled "From the Question Why to the Question How" in *The Manufacture of Knowledge* (1981, 20).

for the emerging STS discipline was formidable. Rather than a short-lived conceptual trend, a focus on daily practice continues in STS (e.g., Schatzki, Knorr-Cetina and von Savigny 2001; Gad and Jensen 2014). Further, the understanding of scientific facts as being contingent on the daily practices of scientists was foundational for subsequent STS approaches; the practice turn undergirds both actor-network theory (ANT, an approach that stresses the co-construction of facts through interactions of networks of human lab members, non-human entities, material artefacts and inscriptions) and feminist STS and new materialism (a view that argues for the inseparability of the material and discursive).[14]

Thus, there is a continuity in stressing the descriptive aims of such work between Knorr-Cetina's 1981 attention to "how" scientific facts are fabricated and, two decades later, a 2002 statement by Annemarie Mol (a scholar indebted to both ANT and feminist STS) that "Methods are not a way of opening a window on the world, but a way of interfering with it. (…) The question to now ask is *how* they mediate and interfere" (Mol 2002, 155). Both Knorr-Cetina and Mol share a commitment to the "how," based on the conviction that scientific facts do not pre-exist their discovery by scientists, but are constituted through situated networks of scientific practices, disciplinary methods and daily interactions. Yet, there is also a subtle *discontinuity* between the argument posed by Knorr-Cetina and that of Mol, most crudely summarized as the difference between the *constructed nature of scientific facts* (Knorr-Cetina's "facticity") and the *construction of the natural world that those facts describe* (Mol's "world"). It is precisely this shift that is amplified, theorized and justified by the uptake of *performativity* into the STS discourse on practices that occurred in the decades between those two publications.[15] Thus performativity

[14] See, for example, Haraway (1997), Coole and Frost (2010) and Barad (2007).
[15] Texts that introduced and developed the notion of performativity in STS include Pickering (1995), Callon (1998), Mol (1998), Law (2004) and Barad (2007).

in STS is oriented to the practical effects of action, and to specifying the ways that scientific descriptions or models of the world themselves act upon and shape that world.

Annemarie Mol's 1998 article ("Missing Links, Making Links: The Performance of Some Artheroscleroses") was an influential text taking up the language of performativity to describe the ways that a disease is not a single, pre-existing entity, but is enacted variously by different members of a hospital. Four years after the publication of this chapter, Mol published *The Body Multiple*, a book- length analysis based on the same study of artherosclerosis (Mol 2002). Though Mol's central argument is quite consistent across these publications, two concerns appear in the book that do not figure in the earlier article. One of these, discussed widely not only in regards to Mol's articulation of it but also across STS since the rise of the practice turn, was a grappling with the reflexive implications of a turn to practice, especially within the framework of performativity. The problem, in a nutshell, was the realization that, just as scientists continually performed the world they sought to describe, so too do STS scholars perform the world *they* sought to describe.

Though the concern with reflexivity preceded the uptake of performativity (e.g., Latour and Woolgar 1986 [1979]), it gained a more urgent tenor when articulated together with performativity, since the world-making stakes of performativity were so high (Law and Singleton 2000; Law 2004; Law and Urry 2004). Thus, through the lens of performativity, a concern with practice spilled over into an anxiety about methods, particularly the methods of STS researchers themselves. In the book, Mol expresses this concern explicitly:

> Self-reflexive desperation about the foundation of our (whose?) knowledge is no longer required. We would be wiser to spend our energy on trying to come to grips with what we are doing when crafting academic knowledge. What are we doing – when we go into fields, observe, make notes, count, recount, cut, paste, color, measure, slice, categorize, and so on. What are we doing when we tame materials, when we publish, give talks, stage stories for various audiences. (Mol

2002, 158)

This sort of imperative appeared in other STS texts of the early 2000s,[16] and continues to inflect STS's interest in how the methods of scholarship may be rethought. Yet, as I will describe later, the calls for re-thinking scholarly methods have vastly outnumbered methodological responses. Before pursuing the question of scholarship, however, there is another seemingly unrelated difference between Mol's chapter and the book published four years later, one that echoes the discussion on performativity and theater in the previous section.

The second, important point in Mol's 2002 monograph that differed from her 1998 chapter is in her use of the word "performance" to refer to performative events. The earlier text used the terms "perform" and "performance" throughout (including in the chapter title), but in the later work Mol writes, "Even if I have been using the term performance elsewhere in the past, I have carefully banned it from the present text" (2002, 41). Mol objects to various resonances of the term performance; she doesn't like that it connotes a stable achievement or effect, since her argument is premised on the ongoing, practical labor that allows objects (as multiplicitous entities) to be held together as singular ontological identities. But also (and perhaps primarily) objectionable is the resonance of "the stage," the theatrical implication of a backstage reality occluded by onstage social performances – a view Mol attributes to Goffman (Mol, 34–6).

The disavowal of theater in STS discourses on performativity is not limited to Mol's

[16] For example, John Law and Vicky Singleton (2000) described how a turn to a performative idiom was not only a rejection of previous STS modes but also necessarily a commitment to reflexively considering how scholarly acts figured into these performances. They argue, "[Previous STS approaches] choose, often knowingly, to ignore the performative consequences of their own descriptions. By contrast, actor-network theory and, to a greater extent, feminist technoscience studies choose to wrestle with the fact that they (and therefore their own accounts) are socially located, *noninnocent*, and therefore political performances. This suggests that they don't offer simple descriptions, but make a difference" (767, italics added).

work; it continues to echo in more recent STS projects that develop feminist new materialism, like that of Karen Barad, a scholar whose uptake of performativity has reinvigorated the use of the concept in STS. Recalling the subtle shift between Knorr-Cetina's concern with the ways in which practices fabricate *facticity* and Mol's attention to methods that interfere with the *world*, Barad (2007) goes further in explicitly arguing that practices do not constitute ways of knowing the world, but *the physical world itself*. So while performativity is a way for Barad to describe how entities are enacted through practices, she insists upon the material contingency of these entities' ontology. Indeed, Barad's argument makes use of quantum physics experiments as an exemplar of a performative interaction that produces difference. Like Haraway's notion of the material-semiotic, Barad suggests that models, measuring devices and theories are themselves *material-discursive*, both manifesting a material world and being in turn manifested within that world. Interestingly, Barad's claim about the material-discursive differentiation of objects of knowledge and ways of knowing is made by a reiteration of performativity specified against theater:

> A performative understanding of scientific practices, for example, takes account of the fact that knowing . . . come[s] from . . . a direct material engagement with the world. Importantly, what is at issue is precisely the nature of these enactments. Not any arbitrary conception of doings or performances qualifies as performative. And humans are not the only ones engaged in performative enactments (*which are not the same as theatrical performances*). (Barad 2007, 49; italics added)

Like in the case of Butler's claim about the performative emergence of subjecthood, Barad's use of theater is understandable as a way to stress that the process of performative enactment does not presuppose the human.[17] Still, that Barad's project – to theorize the

[17] Understandable, but fraught, since in the case of the split-screen experiments that Barad cites, as well as in her own articulation of this argument, human researchers are ever present, both as conditions for and products of such action. This isn't to say that performativity (or theater) presupposes the human, but that human observation or description of a phenomenon will shape and couple both the category of human and the attributes of the

emergence of difference through material interaction – cites the old distinction between performative enactments and theatrical performances is, I argue, a clue to the broad problem of how to track such enactments while taking into account one's own participation as researcher.

Let's imagine that, *pace* Herzig, Butler, Mol, Barad and others, theater can be considered central to the performativity of scholarship. What might it offer that performativity theory doesn't already deal with? Recall the performativity invoked in ANT and feminist STS. Here, in its simplest form, performativity is understood by the axiom that to *act* (to speak, to observe, to behave, to relate) is to contribute to the creation of a world that shapes the ability to act. As we've already seen, some STS scholars have foregrounded *practices* over psychological factors, conceptual frameworks or historical institutions; they have sought to displace ontological categories naturalizing, for example, objecthood, causality and agency; they have attempted to describe ways in which a confluence of practices is not limited to a shift in concepts and inscriptions, but is entangled with shifts of being, calling into question the division between the ontic and epistemic (e.g., Haraway 1997; Barad 2007). It is precisely this understanding of performativity as enacted through practical action that allows a case for theater as a material-discursive practice par excellence.

At the same time, the uptake of performativity in STS foregrounded the imperative for researchers *to take account of their own practices*, resulting in calls for various degrees of reflexivity, and even for measures *beyond reflexivity*. Recall Annemarie Mol's plea for scholars "to come to grips with what we are doing when crafting academic knowledge" (Mol 2002, 158), and, keeping in mind crises of reflexivity in other fields (most notably in anthropology – e.g., Clifford and Marcus 1986), consider the plethora of like-minded frustration from STS:

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phenomenon.

> The argument, then, is that social science is performative. It *produces* realities. But what to do with this claim? (. . .) We have suggested that the issue is one of 'ontological politics'. If methods are not innocent then they are also political. They help to *make* realities. But the question is: which realities? Which do we want to help to make more real, and which less real? How do we want to interfere (because interfere we will, one way or another)? (Law and Urry 2004: 69)

Or this:

> It's no use speaking of "epistemological breaks" anymore. Fleeing from the past while continuing to look at it will not do. Nor will critique be of any help. It is time to compose – in all the meanings of the word. (Latour 2010: 487)

The second quote indexes the movement from Latour's critique of critique (Latour 2004) to his proposal that deconstruction should be abandoned in favor of construction (Latour 2010). Consider this alongside the dedicated attempt by John Law's *After Method: Mess in Social Science Research* (2004) to propose that methods might be reformulated by STS scholars by championing mess and indeterminacy as methodological strategies; we might even include Pickering's account of early cybernetics as a precursor to what such methods might look like (Pickering 2010).

Still, despite so much interest in rethinking methods, attempts at methodological reflexivity or measures beyond have been primarily limited to literary strategies in monographs, as early as Latour and Woolgar's 1979 book *Laboratory Life* and as explicitly as Mol's 2002 *The Body Multiple*. However, these print "performances" have had a difficult time doing more than reflexively acknowledging their performativity while continuing to adopt the problematic distance of participant-observer, shifting scripts as they move between field sites and disciplinary peers. And, despite more than 20 years of growing concern for how to move beyond bare-bones reflexivity in STS, substantive, widespread methodological reconfigurations of research methods outside the monograph did not follow. In fact, foreshadowed perhaps by Mol's rejection of the

term "performance," the popularity of the performance idiom seems to have declined[18] in the STS literature, especially in the US.[19] This might suggest that, in STS at least, the difficulties in methodologically responding to the reflexivity of a performative framework point to the *limits of performativity*. For me, as I will discuss in the following section, this limit of performativity (which, on its other face, delineates the limits of what may count as scholarly method) begs the question of what lies "beyond" performativity. I argue that the resources for *methodologically* addressing the performativity of scholarly methods are to be found in precisely the *artifice* and *intentionality* that have historically marked the theater as outside the borders of performativity, as well as the *contingency* and *excess* that complicate a simple causality of theater's productive effects.

Because, as STS scholars have indicated, our scholarly practices are constructive, not only of our knowing but also of what we seek to know, indeed of the "we" that can know, must we not reflect on the *material staging of scholarship itself*? Might it be possible to consider the methods of scholarship as consistent with the intentional *and* contingent manipulation of boundaries, temporalities and entities that characterize the theater? In this fourth section of the chapter, I talk about how a material-discursive understanding of theater might reframe the possibilities for doing scholarship that takes the co-construction of the world that it reflects upon, and through which it is constituted, as a *matter of concern* (Latour 2004) or a *matter of care* (de la Bellacasa 2011) by attending to the fragile bodies formed through practices of scholarship.

[18] With the important exception of Karen Barad's work, as discussed earlier.
[19] Particularly in STS work coming out of Northern Europe, performativity continues to be invoked, sometimes alongside formal innovations in how scholarship is done (Fitzgerald and Callard 2015; Zuiderent-Jerak 2015; Jensen 2014). It is an open question whether the academic climate for such formal experimentation might be more hospitable *because of* the relatively more integrated place that practice as research has in UK institutions (Nelson 2013).

1.2 RaT at the Podium

So your lecturer is meant to be a performer, but not merely a performer.
(Goffman 1981, 166)

If my use of the term *theater* is broad, so too is my definition of *research*. I consider research to include a range of scholarly activities, including surveying the literature, collecting data, writing, teaching and communicating with disciplinary communities. While all of these can be understood and approached as theater in the sense of intentional yet highly contingent arrangements or stagings, in the following analysis, I will look at two cases of scholarly lectures, both presented at academic conferences. While the first presentation is situated in a more conventional arrangement, and the second presents itself as an intervention, it is important to note that both are acts of theater in non-trivial ways. In other words, I do not wish to suggest that one is more or less theatrical than the other, but that for both, theatrical arrangements are available to and inseparable from the perlocutionary effects of each talk. Thus, in the following examples, I invite you to consider historically theatrical arrangements, including scenarios, divisions of observer/performer, temporary animacies[20] and particular configurations of architecture, light, sound and bodies, as crucially relevant to both the methods of scholarship and the multiple ways this scholarship is taken up.

To begin, I bring your attention to an elegant lecture given by theater scholar Martin Puchner at the 2013 conference of Performance Philosophy,[21] a newly launched scholarly organization bringing together academics with interests spanning performance and philosophy to consider the question, what is performance philosophy? Puchner proposed one interpretation, a

[20] Animacy – or we might rather say, the set of notions characterized by family resemblances – has been described variously as a quality of agency, awareness, mobility, and liveness" (Chen 2012, 2).
[21] Delivered by Martin Puchner on 26 June 2013 at the Sorbonne University. Video recording available at http://labo-laps.com/videos-theater-performance-philosophie- tpp-2014. Accessed 16 December 2015.

genealogical tale in which performance philosophy emerges through the intersection of early "literature" and "scenes of instruction." Literature, for Puchner, begins when the first writing technologies move beyond functions of record keeping and begin to transcribe oral stories. As stories are inscribed, they are sorted and assembled by anonymous scribes into larger texts. These author-less assemblages of text become what Puchner calls "foundational texts," which come to exert great influence on their culture. The existence of these foundational texts makes possible, then, the first scenes of instruction, in which teachers (those who read and interpret foundational texts) surround themselves with a community of students. These master teachers – Puchner means Socrates, Confucius, Buddha, Jesus and Mohammed – do not themselves write, but their teaching circulates through the reports and writings of their students. These students, in turn, constitute prototypically scholarly communities in the sense that they are "held together by devotion to their teacher." These communities of students are "held together by the knowledge that something important is happening in this teaching scene, something world-shattering. A new way of thinking and viewing the world has emerged."[22]

Puchner's account invites us to imagine the origins of scholarly communities in these scenes, where, as students produce texts to communicate and interpret their masters' teachings, the academy, as an institutional assemblage of such students, writers and lecturers, emerges through these students' inscriptions and interactions. So, although other modes of performance – song, storytelling, gesture and so forth – may perhaps have preceded the development of writing, Puchner claims that performance philosophy doesn't emerge until these first scenes of instruction, where what is taught is grounded by, *but always exceeds*, the spoken content of the teaching. What is taught cannot be separated from the scene of teaching itself: "And soon the

[22] These and subsequent quotes were transcribed from the publicly available video of the lecture.

students will go out and proselytize for that, they will transmit their teacher's words. But not just the words! Equally important are the teacher's gestures, the teacher's way of living, dietary habits, everything. Everything matters. Everything in this supercharged teaching scene is connected to this new way of thinking."[23] His most salient claim, as I understood it, was that philosophical knowledge is developed and interpreted through the excess of interpretive performances, where situated, multimodal and seemingly extraneous meanings pervade and shape what might be otherwise mistakenly understood as abstract or conceptual knowledge.

Yet, the import of his account unfolds in its delivery, since Puchner is making a genealogical argument that explicitly extends past Socrates and Confucius to Wittgenstein and de Saussure, and, implicitly, to the scene in which he himself appears when he gives this talk. Although Puchner never comments upon this implicit extension, it raises the question, is Puchner taking the role of a teacher when he delivers this lecture on 26 June 2013? The scene of instruction that Puchner describes is very particular: it features a master teacher, one who reads and interprets, but rejects writing, and draws students' fervent loyalty and interest in their quest for new ways of thinking and living. Yet, as these students (who do write, and debate, and proselytize) become teachers, and mastery multiplies, new networks of peers are formed. The academic lecture, particularly one given at a conference or gathering of peers, is perhaps not a scene of instruction in the foundational way Puchner described it. A scholar embedded in such peer networks (unlike Jesus or Mohammed) must not only succeed at attracting and producing

[23] From an STS perspective, Puchner's lecture can be troubling. It troubles because it is only at the site of the primordial academy that Puchner recognizes a scene of instruction and knowledge-making that counts as performance philosophy. The characters of his stories, both masters and students, are men, "sitting cross-legged, or reclining on chairs." If analogous scenes of instruction occurred among women, slaves and others not authorized to participate in such gatherings, these might well be indispensable for subsequent cultural formations, but are certainly not included in the performance of legitimate and institutionalized knowledge that is understood by Puchner's evocation of philosophy. From an STS perspective – rooted in decades of deconstructing narratives glorifying the lone great men of science – Puchner's story problematically restricts instruction to one sort of scene.

devoted students but also be *constituted as a member* in the now-institutionalized community of his academic peers. In a one- to-many academic lecture, these peers provisionally and theatrically take the role of students in order to give him the floor, as it were, and receive his argument in an extended and thoughtful manner. This obviously theatrical site – the conference lecture – is framing these performances of community. Not surprisingly, then, the conference lecture is a form giving rise to the single author, *solo voce* recitations making possible audibility, attribution and thus, perhaps, accountability; at the same time, it is a form that gives rise to the audience, the community that constitutes the author as member, in which the coherence of membership is declared *a una voce* by the cooperation of those assembled.

In this particular instance, Puchner's talk is one of six plenary lectures, and as a keynote speaker, he is acknowledged by the conference organizers as having achieved mastery among the peers and students assembled there. Held at the historic Sorbonne University, the lecture itself occurred in a large, domed neo-classical auditorium called the Amphitheater du Richelieu. Puchner, a white, male Harvard professor with Clark Kent glasses, a white button-down shirt and a casual grey suit, waited on the side of the stage while he was introduced. After being introduced, praised and linked to various institutions and publications, he was invited to move to the center of the wooden stage and began his talk, facing the many rows and balconies of wooden benches reminiscent of church pews, which the audience filled out nicely. Puchner, a gifted and experienced lecturer, spoke into a hand-held microphone; lacking a podium, he used a music stand for the notes he referred to in the course of his talk. The lights in the auditorium were on, though the stage was even more illuminated, thanks to four theatrical lights positioned, two by two, on the right and left upper walls flanking the stage. These lights created a subtle spotlight and marked the position of the speaker.

I had been among the audience during this presentation, but relied on a video recording available online for the description I offer here. It was by looking at the recording, for example, that I could report the presence of theater lights; in my live experience of the event, these lighting arrangements receded from my attention even while they directed my attention to the speaker. When I consider the theatrical arrangements of ordinary scholarship, it is these sorts of arrangements that seem most salient, ones that are often so conventional and seemingly superfluous as to appear beside the point.

This superfluity[24] recalls sociologist Erving Goffman's account of theatricality, in which theatrical presentations of the self draw upon particular social identities and props in order to articulate a "footing" upon which interlocutors can access intended content or meaning (Goffman 1981, 2012 [1959]). For Goffman, these theatrical machinations are meant to recede, the process of production becoming transparent to allow the product – social meaning – to surface. In the case of Puchner's lecture, this meant that I could focus on the stories and emerging argument of the talk, quite apart from its lighting, the wooden pew in which I sat, and the gestures Puchner used while speaking. But could I? Goffman's claim – one that I agree with – is that all these extraneous events require particular sorts of work to ignore, and that they are nevertheless communicative of a particular frame of interpretation which marks their irrelevance. Goffman discusses this point in his essay "The Lecture," a transcript of a live lecture he delivered in 1976, published as a chapter in his book *Forms of Talk*. He said,

> During lectures, some equipment and encoding faults are inevitable; they imply that a living body is behind the communication and, correspondingly, a self in terms of which the speaker is present and active, *although not relevantly so*. A place is made for this self. It is okay to self-correct a word one has begun to

[24] Though I am unable to explore it further here, this superfluity also recalls notions of everyday rituals, as described by Victor Turner (1982), where "the rules 'frame' the ritual process, but the ritual process transcends its frame" (79). This transcending relates to what I discuss as "excess" a bit further in this chapter. Cf. Goffman's ritualization on the next page.

> mispronounce. It is okay to clear one's throat or even take a drink of water, providing that these side-involvements are performed in speech-segment junctures – except, uniquely, this one [lecture that I am now giving], this being the only juncture when so minor a deflection would not be that, but some overcute theatricality, of merit only as a frame-analytical illustration of *how to go wrong in performances*. In sum, such attention as these various manoeuvres get either from speaker or hearer is meant to be dissociated from the main concern. *The proper place of this self is a very limited one.* (Goffman 1981, 184, italics added).

Here, Goffman considers a central and recurring event in the life of academics: *the lecture*, an activity in which he is himself engaged while making this argument. Goffman identifies the living body of the presenter, along with other distractions, as meant to be dissociated from the content, or "main concern" of the lecture. Yet, despite this dissociation, Goffman thinks of these excesses as a form of what he elsewhere called *ritualization*, in which "the movements, looks, and vocal sounds we make as an unintended by-product of speaking and listening never seem to remain innocent . . . [but] acquire a specialized communicative role in the stream of our behavior" (Goffman 1981, 2). While Goffman finds these non-innocent excesses arising primarily from the behavior of human participants, it is not much of a stretch to claim that such communicating excesses are also to be found in stages, seating arrangements, temporal divisions and lighting instruments, and that these never function in isolation but always together.

Connecting this point back to Puchner's lecture means taking the scene of his instruction seriously; perhaps his lived body, presented to us on the stage of the Sorbonne, does not teach us how to live (as in the case of the original masters he talks about), but it does teach us how to live as scholars. In this sense, Puchner's clothes, way of speaking, his physical elevation and vocal amplification, and the many names and titles associated with his own name are all far more impactful than his literal lecture about what it means to be a member in this particular community.

Goffman was not the only scholar who used theater as frame by which to analyze social

behavior (see, e.g., Peckham 1965), but his was the account most taken up subsequently. For later theorists of performativity (both in performance studies and in STS) Goffman's theatricality had to be disavowed, and the basis for this was usually the fact that his accounts of "on-stage" behavior, role-taking, and other theatrical devices were critiqued for their implicit assumption of a "back-stage" reality behind the artifice of social identity (Wilshire 1982; Butler 1988, 528; Mol 2002, 35–6). While this is an arguably valid critique, at least of Goffman's earlier work, my own dissatisfaction with Goffman's analysis is not with his assumptions about an essential self behind a theatrical one, but with his neglect of what STS scholars might call the performativity of his own account. For Goffman, attending to the theatrical machinations of social presentations of the self is the job of the sociologist; he does not see his description as an intervention, and is careful to separate the two acts. As is clear in the excerpt quoted earlier, Goffman performs a forcefully normative version of the "proper" lecture, in which he is careful not to "go wrong" in his own performance, and thus stresses and propagates the "very limited" place for a lecturing self.[25] Unexamined and unproblematic, all sorts of norms are swept in with the argument; thus, in Goffman's description, a male lecturer will "clean his glasses" while a female one will "finger her pearls" (Goffman 1981, 183). Crucially, for Goffman, culture informs and shapes the speech acts he studies, but it is a silent presence in terms of the *resources for* and *consequences of* his own scholarly work. A very different attitude towards the theatricality of scholarship is to be found in the writings of anthropologist Margaret Mead. Mead did not explicitly use the idiom of theater, but in a modest monograph published eight years before Goffman delivered his lecture, Mead talks about the material arrangements of scholarly communication. She characterizes the

[25] Indeed, Goffman is very concerned with being taken seriously by his audience, and spends much of the first part of the lecture assuring his audience that he is not "yet another self-appointed cut-up, optimistically attempting a podium shuck" (Goffman 1981, 162). To me, Goffman's anxiety reveals how deeply he relies on the norms of the lecture form, even as he points to their social mechanisms. See also Garfinkel on "lecturing's work" (Burns 2012).

one-to-many form of lecture (that is the subject and vehicle of both Goffman's text and Puchner's talk) as one that maintains hierarchy. She wrote,

> The presence of hierarchy has been manifested in the construction of buildings, in the elevated pulpit, the raised lecture platform, the special seat for a cabinet minister, all giving elevated and visible status to those who spoke versus the permanently or temporarily humble positions of the auditors who were to be informed by those who spoke. (Mead and Byers 1968, 4)

Mead's attitude to these observations is profoundly experimental, being at once interested in the way a particular lecture shapes outcomes and at the same time interested in what other outcomes are made possible *had it been arranged differently*. And in fact, her book *The Small Conference: An Innovation in Communication* advocates for another arrangement for scholarly communities, what she calls the "small substantive conference," a face-to-face gathering of a group

> small enough to sit around one large table, called together for a specific purpose, at a specific place, for a limited time. . . . All members of such a conference are accorded participant status; the method of communication is mutual multisensory interchange with speech as the principal medium; attitude, shifts in attentions, gestures and the type of expressiveness that cannot be adequately represented in print, play an important part. (Mead and Byers 1968, 5)

In contrast to Goffman's lecture, then, Mead acknowledges that the "noise" of extra-linguistic gestures, shifts of attention, boredom and fidgeting *can* be in the center of explicit proceedings, and acknowledged as pertinent information in the flow of scholarly communication. For this to occur, however, Mead redraws the procedural and material arrangements of the communication event.

What is relevant about Mead's analysis is that it focuses on how the arrangements of bodies, gazes, voices and time constrain and enable particular sorts of status relations, articulations and interactions. In the vocabulary of the cyberneticists with whom Mead

developed such small conferences,[26] these differences of arrangement *made a difference*; in the parlance of feminist new materialism, where emergent status relations, articulations and interactions are both material and discursive, one might say these differences *matter*. For my own argument here, I am claiming that such arrangements partially constitute what Goffman called the *theatricality* of scholarship, but that, *contra* Goffman, this theatricality is not determined; *it can be staged differently*. Further, to also push gently against Mead's optimism, manipulating the theatrical arrangements of scholarship is not necessarily for the greater good; on the contrary, as making theater has taught me, the meanings and effects of such arrangements always exceed their intent.[27] In this way, intervening in the theater of scholarship is not an answer of how best to do scholarship, but a question about how the doing of scholarship *does* what it does.

If intervening in the theatrics of scholarships is not for the greater good, then what is it good for? Allow me to address this by introducing a second example, a conference paper I myself presented at the same Performance Philosophy conference mentioned earlier.[28] The form of the conference panel is not the same as a plenary lecture format, but it is even less similar to the "small conference" model described by Mead. A conference panel does not have the indeterminate structure of a small conference, where all are authorized to speak; instead, conventionally, three to five speakers are given 10 to 20 minutes each to speak in an

[26] Most famous among these were the Macy Conferences, held yearly between 1946 and 1953. As Mead herself points out, these highly interdisciplinary gatherings of scholars interested in systems, broadly speaking, were also themselves opportunities to experiment with systems. She writes, "In the late 1940s, use of a cybernetic model for cross-disciplinary communication was developed within a series of conferences which themselves exemplified the newly realized principles of feedback and error correction" (1968, 10).

[27] See also Gere (2017) on the greater good and its excesses in the history of scientific experimentation.

[28] This paper, with textual variations, was first presented at a workshop with Karen Barad at the University of California, San Diego, in 2013, and presented again at the annual conference for the Society for Social Studies of Science (4S), in 2015. The score for this latter presentation can be found following all chapters, in Appendix A.

uninterrupted fashion. Like the plenary lecture, speakers are temporarily given the floor, and responses are admissible during a scheduled "question-and-answer" period following each talk, or sometimes following the completion of all talks. At the Performance Philosophy conference, the panel to which I was assigned had three speakers, and was held in a medium-sized neoclassical room, called the Amphitheater du Descartes. Though much smaller than the Amphitheater du Richelieu, where Puchner gave his lecture, the Amphitheater du Descartes was filled with similar wooden benches in the auditorium, descending to a recessed stage with a large table, tabletop microphones and a chalk board.

Here, instead of "giving a talk" or "reading a paper" (an act typical of scholarly communication), I asked the panel's audience to read my paper aloud. My presentation, which sought to intervene in (but not dismantle) the conditions that constituted an authorial, scholarly utterance, began with this uncontroversial phrase: "Hello. My name is Yelena Gluzman." It was spoken by all the people in the room. Three voices spoke the next sentence: "Rather than describe my research, I would like to attend to the material conditions that make possible this panel presentation." In this way, the paper went on to consider the limits of performativity, the historical exclusion of theater from the theory of performativity, and the theatrical arrangements that made possible scholarship as it was being done. Throughout, the talk used the experience of dispersed vocalizing as an experiential lens to ground these claims.

The paper was distributed in the form of prepared booklets, where text to be read was highlighted, and these highlighted portions differed from booklet to booklet.[29] So, certain parts of my paper were spoken by many voices, some parts by a few, some by just one voice. When

[29] In a later version of this paper, given at the 4S conference in 2015, text to be spoken was printed in bold typeface, and all other text was printed in italics.

preparing the talk, I had scored the text according to how many voices spoke each part, and printed unique booklets for each different voice. At the panel, after being introduced by a moderator (by name, but lacking any further affiliations), I asked the audience assembled who was comfortable reading aloud in English; to the people who raised their hands in response, I distributed booklets scored with the most text to be spoken. To the others, I distributed the remainder of the booklets. My instructions, spoken while I moved around the room handing out booklets, asked the participants to read the highlighted parts out loud. The reading began.

Sonically, there was no ideal spot from which to hear the talk, since the dispersal of voices, both when booming *en masse*, as in the first sentence, and stumbling in a trio, as in the second sentence, were hard to hear. The acoustics of the room were constructed for a sonic source to originate at the stage. The Performance Philosophy organizers, who did a wonderful job documenting all the talks either on video or audio, recorded the talk, but because the recording was localized to the tabletop microphone at the front of the room, most of the sound was registered by the digital recorder as noise and fades in and out on the existing recording. In this way, the dispersal of voices caused trouble for a stable point from which either a spectator or a recording device could have an overview of the proceedings; if such a position did exist, it was located at the physical object of the booklet. However, since each booklet (unlike a playscript) contained only one scoring (making it impossible, e.g., to predict how many voices would speak the next line), even this position could not access an overview. And, since the experience of speaking and listening was itself the object of the talk's consideration, a post hoc reading (or worse yet, a description, like I am giving here) of the printed text does provide an overview, but is arguably a view from very far away.

What was the point of such a performance? I respond by posing two questions that

emerged from writing, performing and observing it: (1) What are the relationships between the *form* of scholarly communication and the *object* of scholarship? and (2) What are the conditions that constitute a scholarly utterance as such?

To consider how these questions have been articulated in the literature, I turn to Dwight Conquergood's meditations on ethnographic participant-observation as it relates to the ethnographic texts produced by participant-observers. He writes, "Recognition of the bodily nature of fieldwork privileges the processes of communication that constitute the 'doing' of ethnography: speaking, listening, and acting together" (Conquergood 1991, 181). While he champions the embodied, intersubjective complexity necessary for ethnographic fieldwork, Conquergood mourns the undoing of this complexity in ethnographic monographs:

> Although ethnographic fieldwork privileges the body, published ethnographies typically have repressed bodily experience in favor of abstracted theory and analysis. . . . The *interpersonal contingencies* and *experiential give-and-take* of fieldwork process congeal on the page into authoritative statement, table, and graph. (ibid, italics added)

Though Conquergood is referring specifically to ethnography and the ethnographic monograph, it may be fair to generalize this loss of complexity and intersubjectivity even when talking about other methods of scholarship as they congeal into scholarly communication. This, I argue, is true also for the presentation of papers at scholarly gatherings. It goes almost without saying that paper presentations have a fairly typical structure: the presenter is placed in front of a group and proceeds to either read a previously prepared text verbatim or speak from notes or in tandem with PowerPoint slides. The audience watches and listens and, when appropriate, engages with the speaker by asking questions. Regardless of the topic of the presentation, or the argument advanced by the speaker, certain relations are already activated. So, not only is a great deal of the complexity of the research process necessarily smoothed over in the conference talk,

but also other effects follow. For example, through the conventions of presentation (i.e. one body facing many, one speaking while others listen), the speaker is constituted as an author. The subject of the talk is positioned as a phenomenon that is outside of, and unaffected by, the unfolding presentation and interactions therein. The rules of engagement between participants are constrained, and these constraints are actualized through their enactment. Even time (so precious and enforced in the 20-minute conference talk) is both a condition and product of such proceedings.

Beginning the talk with multiple voices saying, "My name is Yelena Gluzman" was my way to reorient the proceedings, an aporia that was meant to focus on the complexity of authorship. To be clear, I do not consider this choral introduction to undermine authorship; all the voices declaring their name to be Yelena Gluzman were reading from a script, after all. However, the foregrounding of a named author by dispersal through a room-full of audience-speakers was intended to acknowledge that it was the presence of this audience, an audience that is iterative through its very presence, that constituted my authorship and identity as scholar.

The issue of authorship isn't simple, and the subject/object relations, which are built in (and built from) authorship, are multiple. At the Performance Philosophy conference, immediately before the session in which my paper was presented, there was a staged reading of Anne Carson's *Antigonick*, directed by Ben Hjorth. A number of extremely respected performance studies scholars read the parts, including Judith Butler as Creon, and Avital Ronell as Antigone. After the staged reading, there was a large Q&A in which Ronell expressed her discomfort in reading Carson's text, since she disagreed with some of the text's philosophical allegiances. Was her responsibility as an actor to uncritically speak the words given to her by the text, she asked the assembled audience, or was her responsibility to the criticality of her

scholarship?[30] This question echoed in the choral presentation of my talk that followed the staged reading. One of the members of the audience was performance scholar Kélina Gotman, who expressed a similar discomfort: did speaking my text suggest her complicity? This discomfort suggested that the voices in the conference room were harnessed rather than liberated by the performance paradigm of this "distributed" talk; therefore, though authorship itself is foregrounded in its vocal distribution, it is also reinforced by the script that so tightly orchestrated the audience's participation.

To take this a step further, we might say that Avital Ronell's description of the tension between affirmation-through-participation and responsibility-through-criticality speaks to the sort of complexities experienced in ethnographic fieldwork, as described by Conquergood. For Conquergood, the intersubjectivity of the ethnographic encounter is positive; it is embodied, processual and embedded in interactions, and has no recourse to an Archimedean view-from-above. On the other hand, the scholarly monograph, according to Conquergood, smooths out this complexity of experience into a rhetorical singularity in which the author *responds* to the process of research, and *directs* her response to a specialized community that is situated elsewhere. Whereas the messy process of fieldwork (or archival research, or thinking, for that matter) is experienced through multiple encounters in which agency and objecthood are continually re-emerging, the elegant scholarly argument is possible only through situations that pre-empt the instability of subjects and objects.

What then are the repercussions on subject/object relations that emerge during the conference talk? As I see it, the issue of authorship implies the co-constructive relations between

[30] A decidedly Brechtian concern, as one reviewer pointed out.

a singular author/performer and a community of spectator/participants. My (for it is "mine") particular conference performance *reinforces* such a division while at the same time putting its dividing apparatus (constituted primarily by the script as *rubric*) into the literal hands of all those present. However, flowing from (and constituting) authorship there is another aspect of subject/object relations as they exist in typical scholarly communications, and that is the reification of the object of research *as* object. This is the something that Conquergood argues against. I would like to suggest that the distributed conference talk was fundamentally reflexive, asserting its own enactment *as a research encounter*. So, while it reinforced the authorial act, it also made both authorship and voice the moving target of collective consideration. In other words, the distributed conference talk both highlighted authorship through the discomfort of uttering scripted words and at the same time asked speakers to consider this experience of vocalizing as the object of the research being presented.

The experience of vocalizing *en masse* is a funny one, and all three times I have attempted this presentation, people have remarked on their self-awareness of shifts in rhythmicity in response to an emergent rhythm of the group's vocalizing. Here, in attempting to speak out loud, participants found themselves engaged in what Conquergood called an "*experiential give-and-take*" (Conquergood 1991, 181). Though I have no video recording of the talk at the Performance Philosophy conference, there is a video record of this presentation at an STS conference in 2015, held in a large hotel in Denver. The camera was pointed towards the audience, and for the most part, the video shows that people's gazes were oriented downwards, towards the booklet in their hands. One man, smiling, took out his smartphone and filmed the unfolding scene. When voices "entered," some looked up, away from the booklet, to locate the person who was speaking. Most interesting, though, is that when multiple voices spoke the same

text, they almost always ended in unison. Thus, although they were not speaking in strict synchrony, people tended to coordinate their speed with others, and were thus attuned to the emerging rhythmicity of collective speaking. In one part of the written text (pictured in figure 1.1), this rhythmicity is both commented upon and tested, as disintegrating words make coordination more difficult for speakers.

Here, the text in bold was spoken by everyone in the room, while the italicized text was spoken by one voice. Though true for all parts of the text, but particularly in the nonsensical jumble of letters, participants had to attune to an emergent voice, involving themselves in a moment-to-moment give-and-take of rhythm, speed and enunciation. In the video recording of this moment, some people laughed, others did not, but there seemed to be a heightened, effortful attention to each other's sound as voices found their way to the end of the phrase. It was this attunement that was foregrounded as a phenomenon of interest in the attempt to consider the material-discursive conditions that make possible the conference presentation.

What are the relationships between the form of scholarly communication and the object of scholarship?

and

What are the conditions that constitute a scholarly utterance as such?

and

What is my part in this paper presentation?

What have I done to constitute a legitimate author, to make possible the legibility of the talk?

How do the multiple voices that carry, trip, alter, and drop this text negotiate with each other, even when (shout out to Garfinkel) the linguistic social conventions of this negotiation disintegratekbjegbvb
jsbdughbxz
kmdnkjbd
dmnkdsbnkf
dd
khiue
kjhgyyewoihzblioooo
piqnninxx ejijiee
eeeknnk
iiiii

Figure 1.1 Page 10 of the lecture booklet given to the conference audience.[31]

1.3 Situating PaR through RaT

This chapter is not suggesting that theater *should be* levied as a method of scholarship, but that it already is. At its base, my argument assumes that the process of scholarship is partially

[31] Figure 1.1 is from the presentation at the 4S conference in 2015, since that is the one that has video recording available; the full booklet is reproduced in Appendix A. The 2014 booklet used at the Performance Philosophy conference has a similar gesture of disintegrating text.

constituted by its material-discursive arrangements, and that those arrangements tend to reify multiple divisions between subjects and objects that then serve to differentiate scholarship as such. Yet, instead of simply acknowledging this to be the case, as has been done in, for example, STS confessions of methodological performativity, I am interested in how scholarship can actively incorporate these theatrical arrangements as part and parcel of its objects of research: this is the move from theater to *experimental theater*. Thus far, I have given two examples of lectures organized theatrically. In one, although the theatricality of instruction is the topic of the lecture, the theatricality of the lecture itself is not commented upon, and, as is typical in such contexts, allowed to recede in order for the topic of the lecture to enter the spotlight. In the other example, the typical arrangements are redesigned so that attention to *the argument* must necessarily traverse attention to *the form and feeling of the occasion*.

Focusing on the site of the academic lecture has allowed me to consider a particular recurring event in scholarship, one where membership and the community are dynamically creating each other, and has allowed me to consider this in light of historical work on such events, by both Goffman and Mead. However, I do not wish to imply that the conference is the only site of academic theater; on the contrary, I suggest that all stages of scholarship, from reading and discussing the literature to collecting data in the field or the archive, to writing, publishing, teaching and attending conferences, are all theatrical practices whose forms are both conventional and particular. By exploring particular theatrical arrangements in each of these, I propose that meaningful interventions are possible in the spirit of Mol's "grasping our own methods" (2002, 158). By theatrically grasping them, we not only acknowledge our complicity in their constraints and affordances but also, in the spirit of Mead's experimentality, can explore *how* boundaries, relations and meanings shift when methods are done differently.

To return to the question posed at the outset of this chapter, does this proposal for research as theater meaningfully address recent debates around the institutionalization of performance as research, or does it further marginalize art-and performance-based methodologies from legitimate academic work? The issues around performance as research touch on two main areas, one usually framed as the promise of PaR, and the other framed as an obstacle to its institutionalization. As its promise, it has been suggested that PaR, by insisting upon experiential and embodied methods, may contribute to a lineage of expanding what counts as knowledge. In alignment with my own genealogical links between PaR and the practice turn as it occurred in STS, Robin Nelson and others see practice-centered, phenomenological and poststructuralist projects as important precursors to making practice as research (Nelson 2013, 49–57) or performance as research (Riley and Hunter 2009) justifiable and legible. Further, by addressing what constitutes legitimate knowledge-production, PaR also has the potential to influence new policies and visibilities in the university; as Piccini and Kershaw ask, "What kind of academy do we wish to create?" (Piccini and Kershaw 2004, 86). Yet Piccini and Kershaw have more to say about the institutional obstacles to practice as research in the academy, focusing on a long list of questions: how are PaR dissertation projects evaluated? What is required for them to count as "research"? Must they be accompanied by a written work, and if so, what is the status of this work vis-à-vis the performed component? What sorts of resources and infrastructures are needed for PaR? How are the "results" of such projects published, disseminated and critiqued? To consider each of these questions in turn, these authors, and many other members of the conversation, presuppose a normative academy – founded on positivistic ideals – that must be negotiated, coddled and appeased in order for institutional space to be made for PaR.

My proposal, to shift from an idiom of PaR to one of RaT, is not meant to deflect from the particular problems raised by PaR work, nor to ignore the obstacles to its institutionalization. Instead, I hope to shift the conversation from prematurely getting stuck in the impossibility of generalizations (and therefore policies) about what PaR should do, and instead endorse scholars to consider theater methods as a way to *address these obstacles* in their work. Thus the question of audience is crucial for RaT that is well aware that meaning is situated at local sites of encounter (even when these sites and encounters are shaped culturally, historically, grammatically).[32] By foregrounding the always-already theatrical arrangements of scholarship situated in (and situating) particular discursive communities, I question the efficacy of invoking the abstraction of the normative academy in PaR discussions. This attention to situated particularities addresses the problem of how PaR projects can be evaluated and disseminated, how they can enter broader academic discourse, and how they might be cited – textually or performatively. The suggestion of the inversion I propose here (PaR to RaT) is that assessment, legibility and dissemination do not need strict guidelines but rather already emerge as actionable problems through an extended engagement with particular discourse communities. This is not to say that these engagements are necessarily happy or without friction. It is the tension or contradiction sometimes found in such encounters that is the potential for criticality (always in part a self-criticality, if engagement is to continue) and insight. For example, Piccini and

[32] Thus, when considering my distributed conference presentation discussed earlier, it should be noted that the institutional and conventional space of each conference where it was presented was already positioned to read such a text; in all cases, the talk took seriously and addressed particular concerns of that community. And for each presentation, I re-wrote parts of the text to orient it to a site-specific conversation. Thus, I would not endorse a generalization of this particular scholarly act as method; rather, I would propose a keener attention to repercussions of particular theatrical arrangements of particular acts of scholarship in particular disciplinary and discursive communities, including those outside the academy. That said, the method of this distributed conference talk was subsequently taken up and adapted by disability studies scholar Louise Hickman for her conference paper "Distributing Crip Socialities" (Hickman 2016).

Kershaw point out that the multiple locations and forms of practice as research might introduce problems around "authorship, ownership and ethics." They offer this hypothetical problem:

> If a mix of academic and professional practitioners together with students, technicians and designers collaboratively devise a performance that is staged on two nights, one in the university theater, the other in the city's public art space and it is funded by a mix of AHRB and local arts council money and is eventually submitted as an RAE output, how do we deal with those potentially conflicting agendas and systems of value? (Piccini and Kershaw 2004, 90)

Here, I argue that an orientation to RaT, research as theater, allows these complexities described by Piccini and Kershaw to open up as actionable the very practices underlying issues like authorship, ownership and ethics, not to mention issues of collaboration, expertise and publics.

It must be noted, also, that in the literature on PaR, we can see resistance to taking up the complexities of the term *performativity*. For example, Robin Nelson, one of the most vocal participants in PaR discussions, argues for practice as research while explicitly avoiding the idiom of performativity, which for him is too "contested and multi-accented," an objection similar to that expressed by Annemarie Mol in her rejection of the term *performance*. Retaining the idiom of theater as relevant to discussions about PaR (as is the case with the re-orientation of RaT) is, I argue, a way to recuperate theater and theater studies as integral to PaR work across disciplines. For theater departments in the US, which lag significantly in embracing PaR frameworks compared with departments of visual arts (Riley 2013, 175–77),[33] putting theater at the center of PaR might impact concrete institutional policies that support an experimental approach to research methods and, more importantly, facilitate local legibility for such work. Further, inverting the usual understanding of theater as one instance of performance (alongside

[33] As Shannon Rose Riley argues, this fact may be attributed to entrenched characterizations of academic theater as a manual, non-theoretical field, a trend that Shannon Jackson related to the anti-theatrical bias of performance studies (Riley 2013; Jackson 2004).

music, dance, sports, rituals, etc.) and considering performance as research (PaR) as *one instance* of research as theater (RaT) allows for generative and necessary discourse between theater, other forms of performance, practice-oriented social science methods, materialist STS and feminist theory, to name only a few possible intersections. How such conversations might proceed, how they might share risk and what institutional spaces might enable them are open questions that I take up empirically in my own project of examining laboratory experiments on mind through the logics and practices of experimental theater.

1.4 Acknowledgement

Chapter 1, with minor additions, is a reprint of the chapter "Research as Theatre (RaT): Positioning Theatre at the Centre of PAR, and PAR at the Centre of the Academy," in *Performance as Research: Knowledge, Methods, Impact*, edited by Annette Arlander, Bruce Barton, Melanie Dreyer-Lude, and Ben Spatz, published in 2018 by Routledge. The dissertation author is the principle investigator and sole author of that chapter.

CHAPTER 2

Experimental Theaters of Social Minds

In the previous chapter, I argued for the relevance of theater and performativity for a reflexive orientation to scholarly methods writ large, and as a way to make sense of my own reflexive, interdisciplinary methodology in the dissertation project. In this chapter, I focus on the historical and cultural practice of theater as relevant to the development of particular "dramaturgies"[1] of scientific experiments. Here, dramaturgy is used as a term that incorporates both the design and situated doing of experiment, and refers to the dynamism and multiple meanings of staging in historically and culturally situated enactment.

Rooted in 1980s STS accounts of laboratory experiments as producing the phenomena they are designed to investigate, I turn to human experimentation and the dramaturgies through which they produce phenomena about the human social mind. While I reject the theatrical metaphor as an approach to understanding experiment, I ground my subsequent analysis in specific histories of the mutual elaboration of theater and scientific experiment. Taking up Tiffany Watt Smith's notion that theatricality is constitutive of scientific objectivity and Pannill Camp's suggestion that the architectural theater proscenium conditions contemporary modes of observation, I compare the experimental dramaturgies of social psychology experiments on humans and mirror neuron studies of monkey brain cells. By describing how an experiment

[1] Dramaturgy is used here to refer to the composition of a work (Turner and Behrndt 2008) in respect to its coming-to-meaning. It is therefore related to staging, what I have described in Ch. 1 as the intentional arrangement of elements toward meaning-making. Dramaturgy, however, is to be understood as the staging in respect to its enactment. Therefore, dramaturgy is less related to a static form (like, for example, structure) and more related to the dynamic encounter in which form takes shape (as in, for example, a speech act).

attends to or brackets out the conditions that animate and situate its enactment, I articulate my own commitments toward the experimental practice I undertake in the remaining chapters.

2.1 Sordid experiments

In the 1980s, philosophers of science began moving away from early and mid-twentieth century philosophers' attempts to analytically characterize scientific reasoning in respect to other modes of knowing, and began asking what it was that was being done when scientists actually designed, conducted and interpreted experiments.[2] As Ian Hacking described it in his now-canonical 1983 monograph *Representing and Intervening*, "Philosophers of science constantly discuss theories and representation of reality, but say almost nothing about experiment, technology, or the use of knowledge to alter the world" (1983, 149).[3] Hacking proposed to leave aside the question of how scientific rationality *represents* nature, and instead take up the question of how scientific reasoning emerges from specific, historically and culturally situated *acts* of experimental intervention. By focusing on experimental practice, Hacking emphasized scientific knowing as an enculturated activity (e.g., you learn to see through a microscope by doing, not just by looking[4]) that was not "socially constructed" but rather constrained by and co-produced

[2] These included, for example, Ackermann 1985, Franklin 1989, Heelan 1988 and 1989, Ihde 1986, and Rouse 1987. In large part these philosophers of science focused on studies in physics, a point I return to below.

[3] Hacking argues that previous discussions of scientific representation as the pivot between world and theory leads inevitably to one of two conclusions about scientific rationality: either scientists' representations produce a socially constructed image of reality, or their representations are inevitably imperfect depictions of a reality that can never be directly apprehended or adequately described. In both cases, Hacking argues, scientific knowers are confined to observation, trapped in the double-bind of what John Dewey called "the spectator theory of knowledge" (Hacking 1983, especially 130-146). For Hacking and his contemporaries, turning to the practice of experimentation—examining the logics of actually intervening in the world as a form of practical reason—was a way out of this conundrum.

[4] See Hacking 1983, chapter 11 and particularly p.189.

with the material and social world. To experiment, first and foremost, was to negotiate these conditions in order to "create, produce, refine and stabilize phenomena" (230).

Philosophy's turn to the doing of experiments corresponded with a broader movement in a cohering interdisciplinary community of historians, sociologists and philosophers of science called science and technology studies (STS).[5] It is no accident that such projects in the philosophy of science began to converge with the history and sociology of science, since it was in the nexus of this interdisciplinary interest about how experiments were done that each discipline could consider the necessarily enmeshed historical, social and epistemic doing of science. This turn to the doing of experiments allowed for rethinking the touchstones of experimental logics as had been articulated by previous philosophers of science. Important among these, for example, was the issue of replication, posited by Karl Popper as a crucial regulatory feature of scientific reasoning and its relationship to the truth of the scientific matter (Popper 2005 [1959]).

By looking at when and how replication studies were actually done, Hacking and others proposed a more nuanced understanding of how experimental findings challenged or confirmed scientific conjectures, or influenced future action, in practice. Hacking's view of experiments as producing phenomena saw the failure to replicate as relating to the *enactment* of the experiment rather than a conclusive *test* of its hypothesis: "Experiment is the creation of phenomena;

[5] Historians like Peter Galison (1987) sought to understand the local meaning of experimental practice by asking of the historical material, for example, how scientists decided that experiments had yielded findings decisive enough to warrant a conclusion. Among STS sociologists, an interest in experiment coincided with a turn to practice, sometimes pursued through *laboratory studies* (for example, Knorr-Cetina 1981, Latour and Woolgar 1986 [1979], Lynch 1985, Traweek 1988). By taking up the anthropological methods of participant observation, laboratory studies aimed to describe the everyday practices of scientists in their natural habitats; by focusing on scientists' practical reasoning at the site, these studies attempted to directly observe and describe "the *savage meaning* on ongoing events *for and by the scientists*" (Knorr-Cetina 1981, 21). Other types of sociological fieldwork, like Collins' influential *Changing Order* (1985), studied particular experimental cases (e.g., the case of lasers, or of parapsychology) and interviewed the scientists who worked with them to understand how scientific things "come to be seen as true or false" (2).

phenomena must have discernable regularities – so an experiment that is not repeatable has failed to create a phenomenon" (Hacking 1983, 229). Sociologist Harry Collins' description of *experimenters' regress* also challenged the notion that replication could provide definitive falsification of a previous study, since the adequacy of any experiment is a function of the experiment successfully evoking the phenomenon in question. For Collins, this meant that any experiment failing to produce an expected result cannot, by itself, be proof of a failure to replicate, since the failure may lie in either the "real" absence of the expected phenomenon or the inadequacy of the experimenters or experiment itself (Collins 1983). Galison's (1987) historical study of three experiments—measuring the gyromagnetic ratio of electrons, the discovery of the muon, and the experimental detection of weak neutral currents—pointed out that the validities and meanings of these experiments were not governed by general laws of epistemology but wholly dependent on the particular sorts of instruments, practices, and histories at play in each.

These observations suggested that the epistemic status of any particular experiment and its replication—its relation to the production of knowledge—was not accounted for by an abstracted logics of experiment, but inseparable from the ways experiments were done, and the ways their doings were situated within the enmeshed historical and practical competencies, concerns, commitments and instruments of the scientists who designed and interpreted them. In *Representing and Intervening*, Hacking insists that philosophy without history leads to a crisis: "After looking at many of the *sordid incidents* in past scientific research, some philosophers began to worry whether reason has much of a role...is it reason that settles which theory is getting at the truth, or what research to pursue?" (1983, 1, italics added); importantly, for Hacking and the "practice turn" in STS, this required contending with the "history not of what we think, but of what we do" (17).

Hacking's mention of science's "sordid incidents" is fleeting in his monograph, but was—and remains—a concern that motivates and justifies many social studies of science. The sordid experiments Hacking is likely invoking were entrenched in the rationalist logics of utilitarianism and directed toward the "greater good," even as they exploited unwitting, non-consenting and vulnerable individuals (Gere 2017). Though these episodes justified looking behind the rhetoric of scientific values to reveal science in practice, the 1980s turn to experiment largely failed to engage the daily practices of human experimentation. Although these STS scholars encouraged looking behind the grand narratives of scientific reason to attend to the daily work of science, the majority of these studies focused on non-human experiments (often in physics) and the interactions between researchers and laboratory technologies.[6] Of course there was no lack of public and even scholarly condemnation of the abuses of the Tuskegee syphilis experiments, Chester Southam's cancer injections, Milgram's obedience experiments, and other unethical studies on uninformed and non-consenting subjects,[7] a public outrage taken up by the United States Congress as they implemented institutional review boards to ensure compulsory informed consent regulations for human experimentation. But, as Gere argues in *Pain, Pleasure and the Greater Good* (2017), this very condemnation and its seeming resolution effectively marked these cases as anomalies in a dastardly class of their own. Being positioned in this way effectively closed off further consideration of these studies as events that can reveal something

[6] While he did not deal with human experimentation, Michael Lynch's ethnomethodological study of a neuroscience lab looked at the interaction between researchers and experimental rodents, particularly in the transition between orienting to the rodent as animate to rendering it object-like in its capacity to produce data as it was sacrificed (Lynch 1988). While this has echoes with my own account, in the introductory chapter of the dissertation, of sacrificing hamsters in a circadian rhythm lab, it is noteworthy that my own shock of responsibility occurred with human actors, not experimental rodents. Is this a function of my attitude towards humans and rodents? Or is it possible that, if rodents had been in the rehearsal room, and humans in the laboratory, I would have had the opposite response? See the "objects/others" discussion of mirror neurons, later in this chapter, as well as Roepstorff 2002.

[7] Gere 2017, Chapter 1 and, on Milgram, p. 184-6.

more nuanced about the experimental practices that sustain belief in scientific rationality and its objects.

Thus, although the STS turn to experiment in the 1980s yielded wonderfully detailed and rich accounts of science in practice, little of this material dealt with the ways in which phenomena were produced through, for, and by other humans. Indeed, the highly politicized and publicized status of studies like those named above may have contributed to the fact that few of the 1980s STS projects dealt with experimental practices that involved human subjects, and instead found affinity with electrons, microbes, plants, lasers, and other more respectable topics of scientific inquiry.

I am wary of, but resigned to, making categorical distinctions so quickly, setting apart humans from microbes, with all the baggage that comes with this. "How do microbes understand the human diseases they sometimes occasion?," Patrick Anderson asks in *Autobiography of a Disease* (2017, ix), and a particular family of microbes responds at length because, as they say, "...[Patrick] made room for us" to do so (167). These categories (humans, microbes, electrons) are ontologically indeterminate because they emerge in practice, in the room we make for them to speak and in the encounters in which we find our selves. It is precisely this question—how to make rooms in which unexpected voices can arise—that is at the heart of a theater approach to the doing of experiment that I pursue. Rather than an imposition of one domain on another, I argue that the theater of experiment is endemic to scientific laboratory practice, related not through a movement of *metaphor* but through *historically shared* aims and forms. Looking at the ways that both laboratory experiments and theater take apart and reconstitute the human subject, then, is not an insistence on the category of human, but rather an inquiry into its formation.

2.2 The metaphor of theater

In this section, I briefly discuss one approach to elucidating experiment through theater—the use of theater as a metaphor for scientific practice. Though this is perhaps the most frequent notion evoked when juxtaposing theater and experiment, it is one that I resist. Ironically, rendering theater as an applied metaphor misses the crucial aspects that make it desirable as a way to access what Sacks called the "whole person" (Sacks 1985, viii) in interrelation with the world: its specificity, immersion, and contingency.

In philosophy, an interest in comparing laboratory experimentation with making theater emerged a decade after philosophers of science had turned to experiments. This comparison was most thoroughly developed in *The Play of Nature: Experimentation as Performance*, a 1993 monograph by philosopher of science Robert Crease. In it, Crease echoed the frustrations of earlier philosophers, lamenting that philosophy was overly occupied with the rationality of science while neglecting to provide "a comprehensive account of experimental inquiry" (2). Like Hacking, he drew on Dewey's "spectator theory of knowledge" argument and Dewey's insight that "[e]xperimental knowledge is a mode of doing, and like all doing takes place at a time, in a place, and under specifiable conditions" (Dewey 1929, chapter IV). Unlike the *practice-oriented* philosophy of scientific experimentation of the 1980s, however, Crease aimed to develop a "*comprehensive*" theory: an ontological model of scientific experiments that could account epistemically for what experiments did and how they worked (Crease 1993, 6). For Crease, the "theatrical analogy is a tool to create a research program about the nature of experimental science" (101-2). Particularly because experiments must produce (i.e., perform) the phenomena they wish to describe, the "primacy of performance" (ibid.) is an important focal point for

philosophers, argued Crease, who seek to understand the role of experimentation in scientific inquiry.

How did Crease make sense of experiment through theater? An analogy with how scripts are materially and interpretively taken up to present what is scripted allowed Crease to point to the ways that scripts alone (analogous to scientific theories, protocols, etc.) underdetermine the phenomena they are meant to evoke. This, for Crease, foregrounded the role of skill, tacit knowledge, interpretation and even aesthetic commitment necessary for experiment to bring into being what was scripted and could be recognized as the phenomenon of interest. It also highlighted how scientific theories and procedures can change due to the ambiguity between the staging of experiments and the phenomena produced therein: "We are staging what we are trying to recognize…as a result, the very way we are staging it may interfere with our ability to recognize it, and we may have to alter how we stage the experiment before what we are seeking comes into relief" (Crease 2003, 269). Though put in different terms, this notion is similar to, as we encountered above, Collins' description of experimenters' regress and other claims made by philosophers of science who showed the indeterminacy and ongoing improvisation required to move between an experiment and its findings.

Crease's monograph was not received favorably by these philosophers of science, who criticized Crease's overly broad strokes at the expense of considering historically and culturally situated experiments, as the STS community had championed since the 1980s practice turn (Franklin 1994). Andrew Pickering, while appreciating Crease's effort to depict experimentation as "an embodied and material human performance," observed that, "[i]f you don't have a feeling for what doing experiments is like before reading this book, you won't have one afterwards" (Pickering 1995, 200).

If lack of engagement with the situated doing of experiment marred Crease's argument for fellow philosophers, his abstraction of theater practice (introduced as a "secondary structure…used to point out new…features in the principle subject" (Crease 1993, 76) was problematic for me. Crease draws out four features of theater through which to elucidate the doing of experiments (presentation, representation, interpretation and production), yet how and why these features are undertaken (or omitted) in the making of (which?) theater is left a mystery. Interpretation, for example, is depicted as relevant to differences between a script and its enactment, but also in the ways an enactment is understood or felt by an audience. What is the difference? What is it that "calls forth" these different acts of interpretation in their moment-to-moment articulation? Crease brackets these specificities, and in his account, acts of interpretation float across stages, and—rendered groundless—remain both inscrutable and fungible.

The limits of using theater as analogy were argued long before Crease took it up. In "Role Playing and Identity: The Limits of the Theatrical Metaphor," philosopher and actor Bruce Wilshire (1977) warned about the dangers of using theater as a metaphor without engaging it as a practice:

> [I]f theatre is allowed to speak for itself rather than being treated as an instantly exploitable quarry for metaphors, it can speak to us of actual life, e.g., off-stage life, in profound, unforeseen and perhaps unique ways. We must avoid the temptation to think that we already grasp the gist of what can happen on-stage, while it is off-stage life that is dense and opaque and still to be understood. For theatre is possible only for flesh and blood human beings who set up a theatre within the dense and opaque real world. (Wilshire 1977, 203).

While Wilshire's insight may read as trivial—that theater, for all its artifice, acts in the "real world"—it insists on an attention to theater's *doing*, a doing that cannot be generalized *a priori* to have a certain relationship vis-a-vis the real world.[8] Yet to extend Wilshire's argument from

[8] Wilshire, for the most part, is arguing against Erving Goffman's use of the theatrical metaphor in accounting for

theater's "on-stage" to the experiment's laboratory brings us against the same difficulty. If the laboratory experiment makes itself known in its doing, inflected and shaped by the particular "real world" conditions within and through which it is done, then a generalized schema or "comprehensive" account of experiment will always fail to capture what it is that happens in the doing of experiment. Indeed, even a situated account from a particular experiment will always remain partial.[9]

Another reason Crease's theatrical metaphor sits uncomfortably is because, in order to apply the secondary category of theater, he must first posit the primary category as irreducibly different. Crease, in taking up the theater metaphor in respect to science invokes C.P. Snow's "two cultures" distinction as a strawman to set his proposal against; other uses of the theatrical metaphor in respect to science similarly stress ahistorical differences of *kind* between theater and science in order to argue that, despite such differences, science and theater are related in their reliance on craft and artifice.[10] But an abstracted *categorical* difference (a difference in kind) between theater and experiment is not supported by their historical interactions; these "two cultures" worked together and through each other since the rise of European empiricism and founding moves of modern experimental science. The next section will review some of these

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social behavior, a use Wilshire believes to be irredeemably limited. "[T]he metaphor tempts us to think that we persons off-stage are distinct beings in isolation from our 'roles', and then either it is claimed that the real self is unknowable (because we are always 'role playing'), or that it is glimpsed when the role, like a mask, slips" (Wilshire 1977, 201). The experience of making theater belies such a notion, since, as Wilshire points out, off-stage identities, rules, and material conditions are all at play in the experimental composition of the on-stage event, even as constraints to be resisted or violated.

[9] Haraway's classic "Situated Knowledges" (1988) is relevant here.

[10] Crease 1993, see page 30 and page 183. See also Bailey and Ford 1994, who, like Crease, propose a theatrical analogy towards understanding laboratory experiments. Like Crease, Bailey and Ford develop their argument through a point-by-point mapping of a generalized theater process onto an equally generalized process of laboratory experimentation. Richard Schechner, one of the founders of Performance Studies and a theater director himself, responds to Bailey and Ford's proposal not with skepticism that the lab *is* a performance venue, but with dissatisfaction that they have such a narrow view of what culturally and historically specific genres of performance may be relevant to the lab. He asks, "[W]hat *kind* of performance venue is the human subject lab?" (Schechner 1994, 399; italics added).

historical inter-relations between theater and scientific experiment, ways in which each influenced the other at particular historical junctures.

2.3 Historical Entanglements Between Theater and Scientific Experiment

Theater, from the Greek *theatron*, a place for seeing, has long been described as a site that organizes collective vision.[11] Even when, in the seventeenth century, rising cultures of empiricism and experimentation defined themselves in contradistinction to theater, they still maintained active "trading zones" (Galison 1997, 783) with theater practices and technologies. In 1620, Sir Francis Bacon argued that human observation was marred by unavoidable bias, and "declare[d] his intention in the *New Organon* to 'set forth the true way of the interpretation of nature' and to 'perfor[m] expiations and purgings of the mind' of all passions, values, interests and preconceptions" (Coffey 2004, 264). To do this required purging, among other evils, the "Idols of the Theater," Bacon's characterization of philosophical theories as playscripts, in which false but "compact and elegant" stories about the natural world are "plainly impressed and received into the mind" (ibid.). In place of these false stories, Bacon insisted, would be empirical tests and sensible demonstrations that could inductively inform philosophies of nature. Yet, as Coffey convincingly argues, Bacon himself—even in his composition of *New Organon* and its descriptions of experimental methods—relied on techniques and practices of the theater to depict, conceptualize and defend his vision of what would later become scientific objectivity.[12]

[11] In the late 16th century, the development of modern theater buildings coincided with the construction of anatomical theaters for observing medical dissections (Garner 2020) and the notion of *theatrum naturae* undergirded practices of collecting, displaying and looking at the wonders of nature (Coffey 2004; Ait-Touati 2020). These medical dissections, curiosity cabinets and other theatrical scientific practices have a dramaturgy that aims to direct—and is itself directed—by the ways in which they are approached, sensed and understood.

[12] As Coffey (2004) contends, Bacon's use of theatrical scenarios as rhetorical devices in the *New Organon* was informed by his own participation in writing and producing masques, theatrical spectacles presented at court to celebrate the monarch. Whereas the masques were designed to acknowledge and ritualize the power of the

Theater practices and paradigms inflected (or, as Bacon might have suggested, infected) the doing of early experiments and shaped the discursive ways a distinct "culture of science" was performed and publicized.[13] By the late 17th century, Bacon's view of experiments as a more reliable method to establish matters of fact was central to the newly formed Royal Society. As Shapin and Schaffer (1985) point out in their study of Robert Boyle and his air-pump experiments, for experiments to be able to establish facts, they needed the testimonies of public witnesses. Neither the role of the experimental spectator nor the appropriate site for an experiment were yet settled, creating confusion about who could be admitted to witness experiments and how they should behave, as well as the problem of how to conduct the experiment so that a witness would interpret it as intended and produce appropriate testimony. This situation, Steve Shapin explains, "is about what one would expect if new patterns of behavior in one domain were being put together out of patterns current in others" (Shapin 1988, 391). The influence of theater on experimental science was not limited to the staging of the experimental demonstration for witnesses, but also offered itself as a resource for experimentation itself. In the 18th century, for example, Vaucanson's theatrical court displays of automata and mechanical wonders were not only entertainment but, as historian Jessica Riskin argues, experimental systems for research on the mechanics of motion (Riskin 2003).

Theater, in turn, was shaped by scientific theories and practices, particularly through the influence of late eighteenth century mechanistic models on understandings of the actor's craft. Joseph Roach (1993) has argued that 16th and 17th century theories of the craft of acting were directly informed by longstanding medicinal models of passions, humors and essential ether.

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monarchy, similar scenarios appear in the *New Organon* where, instead of the monarch, it is nature, the natural philosopher and the fruits of his invention that take the central place of power.

[13] See also the discussion on Enlightenment spectators and the theater of experiment in Camp 2014.

These models foregrounded movement through and across bodies, evoking the contagion between performers and spectators, but also the linkages between performers' inner passions and their outward gestures and expressions. In the 18th century, mechanistic philosophies of medicine and the body (disseminated in part through the spectacular automata that Riskin (2002) describes) were, in turn, influential models for theater practice. Mechanistic models were influentially disseminated through Denis Diderot's 1773 *Le Paradoxe sur le Commédien*, which insisted that an actor was not to succumb to passions but rather to control and evacuate them in order to galvanize his expressive body toward reliably evoking emotion in others. This radical understanding of acting, built upon mechanistic physiology and Diderot's own keen observations of performers as craftsmen, would become foundational for later 19th century attempts to develop a science of acting in line with concurrent scientific claims about the unity of physiology and psychology (Roach 1993, Chapter 4).

The rise of theatrical Naturalism in the nineteenth century was deeply shaped by cultures of science, as well as by its new technologies of stage lighting (Shepherd-Barr 2020). Audiences in darkened auditoriums observed a specimen of life brilliantly illuminated on stage. Here, it was not the play of passions across bodies but the psychosocial drama of a flesh-and-blood individual that welcomed spectators. Emile Zola, in his 1881 essay "Naturalism in the Theatre" described these shifts in theater genre using the idiom of evolution:

> In fact the great evolution of naturalism, which has occurred from the fifteenth century to our own, correlates totally with the gradual substitution of psychological man for metaphysical man. In tragedy, metaphysical man, man of dogma and logic, held absolute sway. Since the body did not count for anything, since the soul was viewed as the only interesting part of the human machine, every drama took place in the abstract, in the mind alone. (…) And from the beginning natural man, suffocated by rhetoric and dogma, thrashed about silently, wanted to get free…and finally established himself limb by limb. The entire history of our theatre lies in this victory of physiological man who, each century,

> came more into view from behind the mannequin of religious and philosophical idealism. (Zola 2000 [1881], 362)

It is this partial glimpse of physiological man that is envisaged and valued, both on the theater stages and in the scientific experiments that endeavored to bring ever more of this elusive figure into view.

Tiffany Watt Smith (2014) explores the role of theatricality in such attempts to get at psychological and physiological man, and in doing so, argues for the centrality of the theater for the development of the objective scientific gaze. In the following section, by juxtaposing her focus on the theatrical ambiguity of "seeming" and "being" as the central problem of objective looking with Pannill Camp's (2007) description of the disappearing proscenium, I set the stage for thinking about the role of theater in the dramaturgy of twentieth century social science experiments.

2.4 Theater of Scientific Looking

In her study of the overlaps between theater and the sciences of the mind, Watt Smith explores a deeper relationship between theatrical ways of seeing and the scientific gaze:

> In the nineteenth and early twentieth centuries, science did not only inform theatrical innovations, and theatre did not only emerge at points when scientists made spectacles of themselves and their discoveries, but more surprisingly, *theatricality was* deep within the scientific experiment and *a key part of the emergence of objective looking itself.* (2014, 6, italics added)

Watt Smith describes a number of different experimental episodes in studies of mind from this era, including Darwin's staging of his own recoil responses, David Ferrier's exaggerated tiptoeing and jumping to startle his purportedly deafened experimental Monkey F, Henry Head's *mise-en-scène* for his introspective examinations of pain responses to the regeneration of his own purposefully severed nerve. In each, Watt Smith shows how theatrical dramaturgies were integral

to the ability of particular nineteenth and twentieth century experiments to establish matters of fact.

Watt Smith is especially concerned with the late nineteenth century's scientific interest in the relationship between visible gestures and "inner" psychological events. The gestures that she focuses on are flinches—inadvertent or reflexive responses to environmental stimuli—which are variously levied in experiments to explore the physiological basis of mind. One scientific project she explores is that of Charles Darwin's work in researching the expression of emotion, which led to his publication of *The Expression of the Emotions in Man and Animals* (1955 [1872]). To do this research, Darwin used ad hoc experiments and observations coupled with seemingly more "objective" photographic evidence. Yet, for Watt Smith, both of these scientific strategies were mediated by the use of theater in evoking, performing, and watching the expression of emotion. These strategies—including asking friends and family to perform their version of particular emotions, studying photographs of theater actors' expressions, and proprioceptively observing his own muscle movements as Darwin experienced a fright—put the physiology of emotional expression in relief *even as it called the authenticity of these performances into question*.[14] The ambiguity between *seeming* and *being* that constitutes theater's "ontological queasiness" (Watt Smith 2014, 52) became a condition for observation in these early experiments, experiments which themselves establish methodological and theatrical precedents for 20th century objectivist social psychology and social science more broadly.[15]

[14] Darwin is ambiguous in *Expression* as to whether or not these outward expressions were a reliable index of physiological or mental states.

[15] As Watts Smith (2014) points out, Darwin's published descriptions of these theatrics had a performative, iterative function, both for contemporaneous readers and for the future of social psychology. After the publication of *Expression*, readers positioned before mirrors practiced making the angry or surprised faces they read about. In the following century, the typologies it described were taken up as the basis for emotion science in search of universal psychosocial phenomena.

The tension between seeming and being was not limited to the performers—the ones who enact the phenomenon of interest such that it can be observed and analyzed. It was also, as Pannill Camp (2007) suggests, a tension between *seeing* and being—a condition of the necessary *bracketing out* of the self-consciousness of observing "ontologically queasy" phenomena. Camp shows how the construction of neoclassical amphitheater[16] was heavily informed by the study of optics and the anatomy of the eye, an optical model through which the proscenium arch was considered the "'first frame' of vision" (617). Camp contends that theaters of this period did not only present an ocular model of visual consciousness but were also influential in the historical production of particular modes of spectatorship and consciousness *that persist to this day*. These modes of consciousness relied on the "banish[ment of] the frame of representation from spectatorial awareness" (619). Camp connects this way of (not) seeing to a twentieth century experimental method of exploring consciousness: the phenomenological reduction. Alerted by Derrida's insight that "*the phenomenological reduction is a scene, a theater stage*" (627), Camp observes how the spatial, temporal and perspectival qualities of Husserl's phenomenology echo the disappearing frame of the proscenium, as well as the optical paradigm on which it was based:

> …Husserl's phenomenology must open a door in consciousness that will give us access to "the things themselves," according to his motto. The secondary associations—comprised of empirically grounded presuppositions—must be cordoned off in order that the phenomenologist might know that the natural attitude has been left behind. This cordoning, this threshold that will delineate what is bracketed out, is figured by Husserl as a kind of doorway in *Ideas I*…as 'striving toward the entrance gate of phenomenology.' (Camp 2007, 630)

Thus, while Watt Smith relates the development of theories of mind to late nineteenth century theater (and scientific) spectators who both watch and watch themselves watching, Camp notices how the framing needed for maintaining the self-consciousness—the watching oneself

[16] Specifically, the amphitheater of Claude-Nicolas Ledoux in Besancon, completed in 1784.

watching—of the spectator is *designed to recede* in the architectures of eighteenth century proscenium theaters and, later, in the architectonics of twentieth century phenomenology. Whereas Watt Smith repeatedly returns our attention to the problematic double-ness of experimental performance, Camp shows how the very frame that articulates that double-ness is meant to recede from attention, allowing for absorption in the scene on stage.

The ocular paradigm that Camp traces through eighteenth century theater's vanishing proscenium and Husserl's receding entrance gate of phenomenology is, I will argue below, central to the laboratory experiments of twentieth century social psychology. Whereas Watt Smith's focus on theatricality-*qua*-role-playing provides an important critique of objective looking, Camp's vanishing proscenium, meant to give access to "the things themselves," is a persuasive account of how the stagings of social psychology experiments were understood by those who made them. In the next section, we will turn to the receding proscenium of twentieth century social psychology experiments.

2.5 The Things Themselves

This section considers a series of twentieth century experiments that aimed to describe relations between human individuals and human society, beginning with social psychology studies of social behavior to cognitive neuroscience research on mirror neurons. I focus on the dramaturgies of these studies, drawing on Watt Smith's notion that the theatricality of research was necessitated by the project of objective looking, and Camp's proposition that practices of (not) looking—as materially inscribed in theater architectures and models of mind—condition twentieth century attempts to unveil the true elements or mechanisms of social being, the "things themselves."

2.5.1 Milgram and the Experimental Theaters of Social Being

Stanley Milgram's notorious obedience experiment, along with Philip Zimbardo's so-called Stanford Prison Experiment (SPE), are the two most commonly cited examples of theater in the history of social psychology experiments. Indeed, even in texts that don't centrally take up Milgram's experiment *as* theater,[17] its—and the SPE's—relation to theater is invoked in almost every subsequent scholarly account, if only through the use of theatrical terms ("role," "dramaturgy") used to describe these studies (e.g., Baumrind 2015; Gere 2017).

Milgram, beginning in 1961, had recruited experimental subjects for what he described to them as a study on reinforcement learning. Once there, Milgram had instructed subjects (asked to take the role of "teacher") to deliver successively stronger electric shocks to another subject (taking the role of a "learner") in an adjacent room whenever the learner failed to correctly recall a word pair that they had been taught. In Milgram's original experiment—though the unseen "learner" who received electric shocks was actually a confederate of Milgram's and not another subject—the "teachers" instructed to deliver the shocks were lead to believe that the (pre-recorded) cries they could hear through the sound system were that person's real expressions of intense pain.[18] Faced with the choice of obeying the researcher's instructors at the cost of

[17] Taking Milgram's study up *as* theater can be seen in the proliferation of artworks, performances, and films based on this experiment. Milgram's experiment has been reenacted or dramatized by British reenactment artist Rod Dickinson (2002), by the 1976 film *The Tenth Level*, British illusionist Derren Brown (2006), and the French documentary film *Le Jeu de la Mort* (2010), for example. The SPE has likewise gone through a number of reenactments and dramatizations (see Bottoms 2014).

[18] The researcher would ask each "teacher" to increase the voltage of successive shocks, regardless of increasingly alarming cries and exhortations to cease from the "learner." The study's published results reported that 62% of subjects eventually delivered the maximum shock voltage despite their own feelings of discomfort, doubt or distress (Milgram 1963). In this way, Milgram hoped to demonstrate the degree to which experimental subjects displayed an obedience to authority, especially in the dramatic case of being instructed to cause apparent physical harm to others.

inflicting suffering on a person, the majority of participants continued to deliver electric shocks until the maximum voltage has been reached (Milgram 1963).

Milgram's central dramatic pivot was an (unwitting) protagonist faced with a brutal dilemma. In the case of the Stanford Prison Experiment (SPE), where recruited college students participated by acting as either prison guards or inmates to purportedly test the ways that these roles catalyzed abusive behavior, the central dramatic pivot was (unrelenting and almost inescapable) role-play. However, that Milgram's study and the SPE are the most cited uses of theater in experimental social psychology is not because they were theatrical while other human experiments were not, but rather because their dramaturgy spectacularly relied on questionable (at best) and criminal (at worst) casting of non-consenting individuals to inadvertently reveal their *true natures* by observing them living through composed, immersive and traumatic[19] dilemmas in the name of social science.[20] Certainly, the design of these experiments used techniques from theater to create these immersive worlds.[21] But I suggest that their notoriety—more aligned with the tradition of performance art than that of Western drama—relies upon a dramaturgy that reveals "real life" by fearlessly (at best) and ruthlessly (at worst) intervening in it.[22]

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[19] Milgram, for example, reported that some of his subjects suffered nervous fits or uncontrollable seizures (Milgram 1963, as described in Brannigan 2021, 4).

[20] Psychologist and, since the 1960s, one of the most serious critics of the obedience experiment, Diana Baumrind wrote, "Milgram…justified his pervasive use of deception as serving a revelatory function akin to the joint creation by the playwright and the theater-goer for the sake of the viewer's enjoyment. However, Milgram's analogy between theatrical fiction and experimental deception in the benefits they can incur ingenuously overlooks a crucial distinction: whereas theatergoers willingly suspend disbelief in an illusion created for their own entertainment, participants who are lied to are deceived without their informed consent, to serve the experimenter's, not the participant's, interests." (Baumrind 2015, 692).

[21] The theatrical elements centrally deployed by Milgram included play acting, evoking off-stage violence through sound, and the heightened dramatic tension that was the dilemma through which each protagonist was tested. Milgram's experiment was itself a reenactment dramatizing the "following of orders" in Nazi Germany that had been evoked at the Eichmann trials in the same year (Gere 2004, Brannigan 2021).

[22] Here, "Western drama" refers to 19th and 20th century European tradition of a dramatic script in which actors intentionally take on roles (as we did when staging *A Kind of Alaska*). One example of performance art—and its

The intervention into "real life" through inserting human subjects into fictional situations that they experienced as real was one way to deal with the core problem of social psychology, where psychological phenomena had to be produced spontaneously to be valid exemplars. This problem had come to be known as "ecological validity" in late 1950s psychology (Brunswik 1956), echoing the tension between seeming and being that Watt Smith (2014) had identified in Darwin's research on emotional expression. For Milgram, who wanted to ask whether people had an inherent disposition to obey authority, the way to address the question was not to ask volunteers, since their own reports were unreliable, but to put them through an experience where they were forced to choose between obedience to authority and some other imperative and thus inadvertently disclose their psychosocial predispositions. Through the duplicity of convincing experimental subjects that what was transpiring as the experiment was *not* part of the experiment, these human subjects could be observed as agents immersed in an actual social situation instead of actors playing a role.[23] The proscenium frame of the experiment's dramaturgy[24] was designed to recede for experimental subjects, and in doing so, allowed the experimenter access to processes or mechanisms—unknown even to participants themselves—that could explain obedience to authority, the social phenomena of concern.

Milgram's study, conducted in the early 1960s, was not the first of its kind.[25] More than twenty years earlier, in the late 1930s, a social psychology experiment by Kurt Lewin examined

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mode of intervention as revelation—is the work of contemporary Spanish artist Santiago Sierra, who often hires undocumented or illegal laborers to perform menial or degrading tasks to be witnessed, as performances, by gallery audiences (Bishop 2004, 70-74).

[23] Indeed, one of the most damning critiques of Milgram's study contended that, based on subjects' own reflections after the study, many had doubted the scenario they were supposedly immersed within (Hollander and Turowetz 2017).

[24] And to be clear, though this discussion describes a particular sort of dramaturgy exemplified by Milgram's study, it will be the larger contention here that no human experimentation lacks dramaturgy of some sort.

[25] Though Lewin's study precedes Milgram's, its status as the first of this "style" of social psychology experiment is arguable. Some have argues that Norman Triplett's 1897 study of performance when child bicyclists raced alone

the group dynamics of children that emerged under different styles of adult leadership, with each leadership style meant to correspond to a different political formation.[26] Lewin's work in this area, that he would later organize as "field theory," drew on Gestalt studies to elaborate the connection of individual perception and sense-making to the "field" of holistic historical and cultural forces, the "social situation as a whole" (Lewin 1939, quoted in Lezaun and Calvillo 2014, 435). In this project, the import of atmospheres, situations or contexts[27] was emphasized in Lewin's work, and he was interested in identifying theories and tendencies of how these were encountered in the "life space"—the experiential perspective—of an individual (Brannigan 2021, 10-11). Lewin's move to test these societal forces in laboratory circumstances were, on the one hand, driven by his need to make sense of historical trauma[28] and "solve social problems,"[29] and on the other, by the rising laboratory cultures of objective experimentalism.

Like the SPE, Lewin's study involved role-play in the form of graduate student researchers taking on roles of authoritarian, democratic or laisser-faire group leaders, but like Milgram's study, the children in the study were not aware that their group leaders were playing roles. Each session—where three groups of children were assembled, each under the leadership of one graduate student—was observed by a number of other graduate students who were tasked with taking notes on the proceedings, sitting at tables on the periphery of each group's area, next to the curtains that cordoned off one area from another. It is unclear how the children made sense of the note-takers. Lewin's study found that the children thrived under democratic leadership,

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or among other racers (Brannigan 2021, 4).

[26] The fact that the four "group leaders" changed roles (and groups) every six weeks in the 18-week study underscored the necessity for developing a consistent leadership "character" that could be taken up by any of the researchers (Lezaun and Calvillo 2014).

[27] "Contexts" will be discussed further in Chapters 4 and 5.

[28] See for example, Lewin's *The Landscape of War* (2009 [1917]), drawing on his own experiences as a wounded soldier in WWI to analyze elements of the landscape, particularly boundaries and position, from the phenomenological perspective of a soldier in battle.

[29] Lezaun and Calvillo 2014, 452.

became sullen and withdrawn under authoritarian leadership, and surrendered to chaos in the laissez-faire group (Lezaun and Calvillo 2014).

The results of studies like Milgram's, Lewin's and the SPE made for spectacular and easily relatable stories about human nature. Augustine Brannigan identifies this as a problem stemming from social psychology's concern with moral issues coupled with their reliance on reductive laboratory experiments:

> [T]he experimentalist has to conjure up a proxy, or a shorthand artifice or substitute, for the original event. Rather than going to primary sources to study the phenomenon first-hand, the experimentalist has to visualize a way of reducing the process to something that can be studied in a laboratory over a short period of time, whether or not this is the best method of elucidating the phenomenon. The result is not a study of genocide but a metaphor of genocide, a dramatization or allegory that enacts certain key processes that the psychologist feels are critical, though these are frequently researched in a complete empirical vacuum with respect to the original events that characterized the genocide. (…) [N]ot being constrained by any set of "hard facts" that arise from studious observations of the phenomenon in situ – what Fran Cherry called "the stubborn particulars" of everyday life – and not being informed by what is found in the historian's documents, or the clinician's interviews, or the demographer's age–gender tables, the moral substructure of social science inquiry is given free play. [M]oral issues often make "consumers" of experimental social psychological research, students and the public at large… (2021, x).

Looking more closely at how these experiments were enacted introduces ambiguities, lacunae and excesses that challenge the disappearing proscenium of their design. How *did* Milgram's subjects experience and interpret the laboratory space, their roles in respect to the experimenter, and the meaning of their actions within it?[30] What did Lewin's children think the note-takers were doing, and how did that shape the situation to which they responded?[31] Indeed,

[30] Hollander and Maynard (2016) point out the vastly underreported fact that "although two-thirds of [Milgram's] participants across all conditions ended ''obediently,'' virtually all resisted continuation to some extent" (356). They reinterpret Milgram's data using conversation analysis to examine *how* (instead of *why*) subjects in Milgram's study resisted instructions and, importantly, look not at subjects' behavior but rather to the *interactions* between the subjects and experimenter. See also Hollander and Turowetz 2017.

[31] In their research on Lewin's study, Lezaun and Calvillo (2014) point to the tension between this space as a situated, particular gathering with makeshift and adjustable ways of responding to what happens there, and a

it could be argued that it was precisely the experimenters who, in believing or insisting that the staging of the experiment revealed each subject's inherent psychological disposition, were looking past the frames they themselves had built.[32] Although the narratives produced by such studies might recall Francis Bacon's "Idols of the Theatre" with their problematically "compact and elegant" stories, Bacon's remedy of the objective "crucial experiment" is not an adequate one. Indeed, as Watt Smith contended, the particular theater of these psychosocial experiments is not in opposition to, but *emerges from* their requirements of objective looking, the necessity of immersing subjects in a lived dilemma to reveal the social mind (despite) itself.

This is not to say that the results of these studies are false but rather that their dramaturgy suppresses interactions, events and interpretations in order to clear a path to producing and considering their phenomena of interest. These suppressed interpretive lacunae, in tension with a general "consuming" of these experimental stories about social minds, could be glimpsed when situating the experimental results back in the lived interactions through which they were produced (Hollander and Maynard 2016, Hollander and Turowetz 2017). In his recent book, Brannigan (2021) argues that the era of experimental social psychology, inaugurated by Lewin's field theory and its cultivation in the laboratory, and typified by Milgram's experimental morality tale, is ending. As he writes in the introduction to *The Use and Misuse of the Experimental Method in Social Psychology*,

> This book is about the attempts over the past seventy or so years to forge a science of social life based on the systematic use of experiments. Experimental social psychology is unique in the social sciences in that it has committed itself, primarily in North America, almost exclusively to the use of the experimental

"placeless" laboratory that enables the generalization of its events to theorizing about social formations (452-3).

[32] See, for example, Brannigan's (2021, 174 - 186) discussion on the ways that Milgram's published narration and interpretation of the experiments ignored substantive interactions between experimenter and subjects, cherry-picked some studies over others, and otherwise deviated from stated and expected protocols to protect the interpretation that would reveal a human mechanism of obedience in respect to the social force of authority. For Brannigan, Milgram's study is an exemplar of the core *modus operandi* of twentieth century social psychology until the imposition of informed consent.

> method to create new knowledge. In my view, this attempt has wavered, and the rise of the institutional review boards and human ethics boards promises to bring the discipline founded on high-impact experiments based on deception of subjects to an end. The conclusion of this book is that experimental social psychology is, at present, an impossible science with little possibility, in its current configuration, of establishing any credible new knowledge (Brannigan 2021, x).

In line with Brannigan's sentiment, my reason for invoking Milgram's study, the SPE and Lewin's study is not to point to them as exceptions that use theater in the design of experiments, but rather as exceptionally spectacular examples of twentieth century theater-thinking in the *ordinary design of experiments on social minds*. It is, in other words, meant to direct attention to the ways that other experiments (ones unlikely to be singled out as theatric) have also been designed with explicitly theatrical dramaturgies. While Brannigan sounds the death knoll for this sort of experimental social psychology, I suggest that its legacies and ways of looking—its ways of getting to the things themselves—are bequeathed to and refracted through cognitive neuroscience, and particularly their experiments on social minds.

2.5.2 Performing for Mirror Neurons

In the 1980s, Giacomo Rizzolatti's lab at the University of Parma was researching how certain areas of the monkey brain, in particular the motor cortex, became active in response to "goal-directed" movement by the monkey (Rizzolatti n.d.).[33] It had long been shown that corresponding parts of the motor area became active (or "lit up") in response to individual movements (moving a hand, for example). However, Rizzolatti's lab found that, as opposed to simply responding to mechanical movements (moving the joint at a particular angle, for example), well-defined groups of brain cells would light up in response to movements in which

[33] The following description of the lab and their research in the 1980s relies on a detailed account Rizzolatti himself wrote for *The History of Neuroscience in Autobiography*, Vol. 9, published online by the Society for Neuroscience, no date given, especially pages 352-4.

the monkey had a "goal" (e.g., picking up food). In other words, what was being coded in the brain was not the mechanics of a movement, but rather a movement with a particular purpose or meaning. The lab identified four such subgroups of cells in the premotor cortex of the monkey brain, each responding to a different kind of movement: "grasping-with-the-hand-and-mouth neurons," "grasping-with-the-hand neurons," "holding neurons," and "tearing neurons."[34]

This research was indebted to a long lineage of brain mapping, a project that began more than a century earlier with the proposal that different parts of the brain seemed to "control" different bodily and perceptual functions; identifying these areas became a central to the brain sciences, a project known as "brain localization." Brain localization was at stake in neurologist David Ferrier's theatrical crouches and leaps at his experimental monkey in 1881, since Ferrier had removed ("lesioned") an area of the monkey's brain suspected to be involved in hearing, and was trying to demonstrate, to himself and others, that the monkey did not react because he had been thereby rendered functionally deaf (Watt Smith 2014, Chapter 2).

In Rizzolatti's lab a century later, not only had the team localized subgroups of cells that were each associated with a goal-directed movement, they further found that this brain region[35], long known as a motor area, had a surprising number of "visual" cells: neurons that responded to movement *and* to visual stimuli. These neurons didn't seem to have a receptive field (i.e., they did not respond to areas in the monkey's visual field, as photoreceptors do) but instead lit up in the presence of objects of a specific size that corresponded to the type of grip encoded by the neuron. For example, upon being presented with a peanut, the cells that coded for a "grasping-

[34] See, e.g., Jeannerod et al. 1995.

[35] The region in question was the primary motor cortex, the ventral and dorsal premotor cortex, the cortical medial surface, and the rostral part of the agranular cortex. The ventral premotor cortex (called "areas F4 and F5" by the lab) was where the "goal-directed" movement and visual cells were located (Rizzolatti n.d.).

with-the-hands-with-a-precision grip" would become activated. These were highly tuned "visuomotor" cells, neurons that responded to related motor *and* visual stimuli.

This line of research was impactful, since the most established projects to localize function to brain structure stressed the separation between the motor system and the sensory system. Particularly influential in this paradigm was Wilder Penfield's mapping of the motor cortex and sensory cortex in conscious epileptic patients in the 1930s and his resulting famous "homunculus" sketches showing how different body areas took up more or less "real estate" in the brain (hands, lips and tongues dominating both the sensory and motor cortex).[36] The separation between the motor system and sensory system was foundational for (and supported by) research on reflexes and "input-output" models of the brain.[37] The presence of visuomotor neurons in the motor area—especially ones attuned to "goal-oriented" events—challenged this long-accepted division and, it could be argued, the cartographic project of a one-to-one correlation between brain structure and behavioral function.

In the early 1990s, the Rizzolatti lab was continuing their research on monkeys, trying to characterize these visuomotor neurons that coded both for an object as well as the hand movement suitable for that object. They did this by presenting the monkey with objects and, at times, allowing the monkey to pick these up, while recording electrical signals directly from single neurons through an array of electrodes implanted into the monkey's brain. At some point, they noticed unexpected activity that occurred when the monkey was neither moving nor yet presented with an object. They realized that these cells were firing not only when the monkey

[36] Brincker 2012, 163-4. For an account of Penfield's operations on epileptic patients that produced this composite map of the brain cortices, see Winter 2012, Chapter 4. For a further discussion relating Penfield's work to that of Rizzolatti, see Guenther 2016, 271-275.

[37] E.g.,Van Essen et al. 1994. See also Clark 2001, especially Chapter 5, for challenges to input-output or information processing models. Maria Brincker (2012) also questions the mirror neuron interpretations in respect to their seeming return to this notion, in contrast to the challenges posed by the lab's previous "visuomotor" work.

picked up the object, or when the object was visible, but also when the experimenter moved to pick up or place an object. This phenomenon, where cells in the monkey's motor area responded to the "goal-oriented" movement of *someone else*, became of great interest, eventually overshadowing the research they had previously been doing. The lab embarked on a series of experiments to try and specify these newly identified cells that responded to *others* instead of *objects*.

The effect noticed by Rizzolatti's team led to the specification of particular groups of cells highly tuned (i.e. responsive) to the "goal-directed" movement of someone else, a group of cells they named "mirror neurons." Discussions about the discovery of these mirror neurons became a major event, not only in scientific discourse but across popular media as well (for example, Jarrett 2013). The researchers' interpretation suggested mirror neurons could provide a biological mechanism to explain how an individual understood the meaning of another individual's actions (and thereby infer the sensations and intentions of others), and these cells were lauded as a missing link that, as psychologist V.S. Ramachandran put it, "will do for psychology what DNA did for biology: they will provide a unifying framework and help explain a host of mental abilities that have hitherto remained mysterious and inaccessible to experiments" (Ramachandran 2000).

These cells received a very special and distinctive name—mirror neurons—which helped buoy the suggestion that these cells could account for how individual minds used a kind of internalized motor simulation (mirroring) to understand intentions of and form intersubjective bonds to social others. Like Milgram's obedience experiment and many of the social psychology studies discussed in the section above, these claims about mirror neurons were not only relevant to the scientific community, but were broadly publicized and widely taken up (even by theater

researchers and practitioners) as having all sorts of explanatory potential. This popularity extended to the repetition, in all sorts of publications, of the story of the researchers from a "laboratory in Parma" that, one day, while attending to other things, accidentally and to their great "surprise" noticed a strange phenomenon: that these cells responded to the movements of the experimenters (Blakeslee 2006; Rizzolatti and Fabbri-Destro 2010; Falletti 2016, 5-7).

2.5.3 Lingering at the Site of Experiment

The experiment in which this origin story is set (Di Pellegrino et al. 1992) features a cast of characters: a number of researchers, moving through the lab, adjusting and monitoring devices that index brain activity and muscle movement, placing and removing objects to set the stage for each experimental trial, while making sounds and gestures to each other and to the protagonist of the scene. A monkey, at the center of this scene, is the source of the activity that the researchers monitor through their devices, and the visible target of many of the researchers' deliberate placing and removing of objects and their gesticulations. Yet, the monkey in this scene is not the protagonist in the room. Rather, the protagonist—the character who articulates 'themself' through their responses to the unfolding drama—is the monkey's brain.

In keeping with the localization project in the brain sciences, the goal was to map the ways the brain codes the "external environment"—by identifying orderly patterns of neuron groups responsive to particular environmental features. Recall that, in their previous research, this group had demonstrated that the same brain cells became active when the monkey made a particular "goal-oriented" movement (e.g., picking up a peanut with a pinching motion), and when the monkey did not move but simply saw its goal: the peanut or another object that could be picked up in this way. After these researchers noticed that their own movements, as they

picked up and placed the test peanuts, also activated some of these brain cells, they tried to find out more about this "surprising" result using the same protocol.

I do not doubt that this group of researchers was surprised by brain cell activation in response to their own movement. In the experimental cognitive neuroscience laboratory, like in the social psychology laboratories discussed earlier, the experimenter was actively meant to recede from the testing situation itself, such that their presence could not confound the correlation between an environmental event and the subject's mental or neural response. Indeed, the move away from the whole organism (with its complex and excessive affective, attentional and behavioral presence) to a purely physiological and thus seemingly objective entity (the brain and more specifically, its neurons) was a way to minimize the leaky subjectivities and interpretive underdetermination that haunted social psychology studies. Yet, even the purportedly docile protagonists of this study—the neurons that had each been pierced with a recording electrode—had sounded out their recognition of the experimenters in the room. But what exactly were they responding to when some of these cells responded to the movements of the experimenters?

To explore this, the researchers *performed* for the monkey's neurons, their experimental spectators, enacting all sorts of potentially meaningful or nonsensical gestures and movements that they suspected might have an effect on their audience. Watt Smith (2016) discusses these "pantomimes" (19) in a short paper that connects these experiments to the history of social psychology studies of imitation. Watt Smith quotes a description of these pantomimes by cognitive neuroscientist Gregory Hickock[38]:

[38] Hickock, though not challenging the *findings* of these studies, is one of the most vocal critics of the *interpretations* offered by Rizzolatti and his co-authors.

> Some actions involved a food morsel, including putting it on a surface, picking it up, giving it to a second experimenter, and then stealing it away. Other objects were broken, folded or torn. The experimenters waved their hands, lifted their arms, made threatening gestures. They also pantomimed objected-directed actions (such as grasping an invisible raisin) or grasped objects with a tool instead of their hand (Hickock quoted in Watt Smith 2016, 19).

Watt Smith makes the point that the "theatricality of these and similar practices" is worth investigating, since "these experimenter-performers doing their rehearsed skits are part of the complex networks—the bodies, objects, institutions and practices—which help shape new scientific ideas about our brains and our behavior" (19-20). Specifically, she is concerned with the intersections between theater and the experimental mind sciences. Identifying the constitutive theatricality in these experiments allowed her to argue against the hierarchies of knowledge that elevate science while subjugating theater. This point is especially relevant since, as mentioned previously, from the late 1990s onward, a wave of theater scholars took up the scientific evidence for mirror neurons as providing an explanation of the exchange of feeling that is understood to occur between energetic actors and seemingly passive spectators. For example, in a recent anthology titled *Theatre and Cognitive Neuroscience* (2016, 7-11, italics added), Clelia Falletti writes,

> This discovery hypothesizes, explains and scientifically proves something extremely interesting. At the very moment we observe an action being carried out, our motor programme kicks in to carry out that very same action in that self-same moment; furthermore, it also is the self-same moment in which (and also the reason whereby!) we understand that action which is occurring before our eyes. (…) By now it should be clear how interesting this can be to theatre makers. In theatre - which is, par excellence, the locus for beholding - each member of the audience is doing his dance with the actor. The two of them together are creating a dynamic shared space of action; that dynamic space is, at the same time, both a dance and a critical crossroads of intentions. (…) *A more authoritative scientific confirmation for the observations of theatre makers and scholars can hardly be found* (…) It is here that *neuroscience has given us a precious gift, the gift of a shared space of action, a concrete, physical grounding for the actor/spectator connection, which is the same grounding underlying theatre itself - the very stuff, indeed, that theatre is made of.*

Falletti's invocation of mirror neurons as an "authoritative confirmation" of the "concrete, physical grounding for the actor/spectator connection" is not unusual; it is echoed in multiple articles and monographs in Theater and Performance Studies (McConachie 2008, Blair and Lutterbie 2011; Blair and Cook 2016). Watt Smith, on the other hand, is interested in the performances necessary to the experiments producing mirror neuron claims, which point to the constitutive entanglement between theater and scientific experiment. Watt Smith's argument is recuperative; as she puts it, "When theatre practitioners are called on to utilize mirror neuron theory to better understand *their own* work, it should, then, be with the recognition that their labour has already contributed to the conditions which made that theory possible in the first place" (2016, 20).

While Watt Smith thus makes an important intervention in the *hierarchization of knowledge* that subjugates theater in respect to science, I turn back to the scene of the mirror neuron experiment to consider the *hierarchies of acknowledgement* meant to forge a pathway to the things—the mechanisms explaining social minds—themselves. In considering the experimenters' pantomimes as they attempted to understand what exactly these cells were responding to, my point is not that they were "theatrical" but rather that their performance attempted to establish precisely a "shared space of action" in which experimenters and brain cells could reciprocally and sensibly respond to one another. In order to establish communication with these cells, experimenters posited categories of meaning and gesticulated according to each[39]—

[39] "Following the discovery that some experimenter's actions could activate F5 neurons, all recorded neurons were examined by performing a series of motor action front of the animal. These actions were related to grasping food (e.g., presenting the food to the monkey, putting it on a surface, retrieving it, giving it to a second experimenter, taking it away from him), to manipulation of food and other objects (breaking, tearing, folding), or were gestures with or without emotional content (threatening, lifting the arms, waving the hands)" (Di Pellegrino et al. 1992, 177).

limited by their own understandings and interpretations of what, say, hand waving might mean to such a neuron, as well as to their own efficacy as performers. The cells addressed by these performances were themselves constrained in their ability to respond—limited primarily by the receptive field and mode of expression lent by the electrodes that measured and displayed (visually and through sound) the cell's electrical events. Rather than a "shared space of action" being established by the pre-intentional reciprocity of mirror neuron recognition, it was effortfully and dynamically distributed across the signifying bodies, movements, technologies and spaces at play in the moment-to-moment unfolding of these interactions. And, not to be forgotten, between these two performances was (forgive me) the monkey in the middle: at once the proscenium designed to recede to make a path to the mirror neurons themselves and, at the same time, the entity that grounded the meaning of a "goal-directed" movement.

2.5.4 The Goal-directed Subject

One of the first and most stable findings of mirror neuron studies has been that these cells do not respond to just any kind of movement, but specifically to "goal-directed" movement. As I described in my introductory account of the studies the Rizzolatti lab had been doing in the 1980s, the term "goal-directed" was meant to distinguish between purely mechanical movement and purposeful movements that held a particular sense or meaning for the mover. But was a goal-directed event an intentional one? Or one carrying a particular valence of meaning or interest for the mover/observer? Perhaps paradoxically, the notion of a "goal-directed" task was also crucial for early twentieth century psychology that sought to center the lived experience of the person; Vygotsky's cultural-historical approach to psychology, for example, relied on the finding that children acquired skills when in pursuit of an interest or goal, rather than by passively absorbing

information (Cole and Engeström 1993, Wertsch 1984). Understanding children as dynamic (i.e. historical) agents in interested interaction with a cultural world was foundational for Vygotsky's notion of the zone of proximal development, and his own experimental methodology (which we shall consider in a moment).

In cognitive neuroscience experiments like this mirror neuron study, an experimental monkey's "goals" were quite narrowly defined, often referring to the monkey's interest in food items (which itself could be externally maintained by food-depriving monkeys before experimental trials). To refer to the experimenters' movements as more than "goal-directed" (for example, "intentional" or "meaningful") would be to ascribe an anthropomorphic interiority to the monkey that these scientists—and I daresay most scientists today—would wish to avoid. Interestingly, however, the term "goal-directed" persisted even when mirror neuron experiments were later conducted on human subjects and, arguably, have shaped the design of such experiments in terms of the movements and scenarios presented to human subjects as experimental stimuli. What is a "goal-directed" movement for a human being? If, as was once said of pornography, "you know it when you see it," in mirror neuron studies it is not necessarily "you" who needs to recognize or identify the goal, but your mirror neurons who must do the recognizing. This is the dramaturgical proposition of such studies.

Thus, the shift from monkey research to human research in mirror neuron experimentation was not a dramaturgical shift. Largely organized by the spatial and technical devices at play—crucial to getting access to the brain activity itself—both primate and human subjects needed to work with experimenters to "keep [their] body out of the way so [their] brain can be measured" (Klein 2014, 88). If the body, the whole person (or whole primate) is the proscenium to these studies, it is a proscenium whose vanishing is required through concerted

and collaborative effort. As in the case of social psychology experiments, though these collaborative efforts are always necessarily a "joint enterprise"[40] between researchers, subjects, and the experimental setting, they are also necessarily bracketed out of the experiment's analysis, narration, and interpretation.

2.6 Mirror Neuron's Others

Thus far, the focus of this chapter has been the historical and architectonic dramaturgy of the laboratory experiment, especially as it was done in social psychology and cognitive neuroscience. The laboratory experiment, however, is itself part of a larger dramaturgy, where experimental paradigms and their associated stories circulate within scientific communities and more broadly into public discourse about the origins, meanings and pathologies of the social mind. In this final section, I explore the interpretive practices that shaped this circulation in the case of mirror neuron research in order to consider the stakes of these discourses for "others": both for those humans who are most implicated in such stories, and for the humanistic social sciences that could (and as I argue, should) be brought to bear on the production of these discourses. In focusing on the interpretive moves to conceptualize mirror neurons, I notice how *the naming of these cells* in the lab's early publications advanced a persuasive and perhaps hasty framework that cast objects and others as biological "natural kinds."

The initial publication describing these "surprising" cells appeared in 1992.[41] The authors continued to run experiments refining their findings and published a subsequent pair of articles in 1996.[42] It was in these latter articles that the authors introduce the name "mirror

[40] Roepstorff and Frith (2004) pointed out that, "The performance of the [experimental] task is a joint enterprise" (195) between the experimenter and subject. This notion will be explored at length in Chapters 4 and 5.
[41] Di Pellegrino et al. 1992.
[42] Gallese, Fadiga, Fogassi, and Rizzolatti 1996; Rizzolatti, Fadiga, Gallese and Fogassi 1996.

neurons" to distinguish these cells from those they had identified earlier, the visuomotor cells that responded both to a particular grasp and an object that could be picked up with that same grasp. Importantly, it was only *after* mirror neurons were introduced in 1996 that the lab began referring to the neurons that respond to objects (which previously had quite functionally specific and descriptive names, e.g., "grasping-with-the-hands neurons") with the less descriptive term, "canonical neurons," a term whose use lies almost exclusively in differentiating between the neurons responding to objects and the mirror neurons that respond to the actions of others. The term "canonical neurons" traded the functional specificity of previous naming systems for the ability to distinguish and de-emphasize the importance of the canonical neurons in respect to the mirror neurons.[43]

This move, to use names to strongly mark these as two completely distinct classes of cells, was curious. After all, these newly identified mirror neurons could be easily interpreted as a continuation of a "goal-directed," physiologically inscribed set of associations made coherent by the monkey's phenomenological experience of all these things as meaningfully related (their own movement to pick up a peanut, the peanut, and the movement of another toward the peanut as continuous and connected). Instead, however, giving the name "mirror neurons" to the cells that responded to others created a strong conceptual—and biological—division between the peanuts, raisins and other meaningful *objects*, and the *others* who move meaningfully in respect to them.

[43] This differentiation has not gone completely unchallenged. According to Greg Hickok, the categorical distinction between canonical and mirror neurons is misleading, since both are found in the same region and play a similar role in "sensorimotor association." For Hickok, this argues against the action understanding hypothesis proposed by Gallese, Rizzolatti, and others (Hickok 2009; Hickok and Hauser 2010). See also Heyes 2010, Sheets-Johnstone 2012, Brincker 2012.

The naming of mirror neurons had a resounding impact. "The mirror metaphor was a brilliant marketing move," as one neuroscientist put it to me.[44] It was a powerful metaphor to communicate and coordinate an interpretation with a wider community of neuroscientists, cognitive scientists, and philosophers of mind. The evocation of "mirrors in the brain" impacted the design of an enormous surge of subsequent research from the late 1990s until today, foregrounding a centuries-old concern with mimicry as the link between individual consciousness and social formation.

When asked by an interviewer about the origins of the term, one of the researchers in Rizzolatti's lab, Vittorio Gallese responded, "We thought that the metaphor of mirroring captured one basic functional aspect of these neurons: they fire when *I do something* or when *I see you doing the same thing*. That discovery—that naming—marked the beginning of a very important shift in the goals of our research" (Wojciehowski 2011, 4, italics in original). Mirror neurons, in this interpretation, became easily subsumed into a philosophical inquiry on the origins and mechanisms of social behavior. As Gallese explained in the same interview, "After the discovery of mirror neurons, we became progressively more interested in intersubjectivity, in how we understand each other" (ibid.).

The interpretation of mirror neurons as categorically different from so-called "canonical neurons" circulated with little resistance from either specialist or popular communities. As noted above, mirror neurons were thus widely taken up as the biological "missing link" to explaining forms of imitation and learning, mind reading, empathy, and social coordination in humans. In some cases mirror neurons and related neuroscientific claims have been taken up as explanatory accounts by humanistic disciplines outside the biological sciences, including by theater

[44] Private discussion with Dr. Marco Iacoboni, University of California, San Diego, February 28, 2014.

scholars.[45] As noted by Watt Smith (2016), most troubling about these interdisciplinary borrowings was a lack of engagement with the nuances, not only of the neuroscience claims, but also of the situated practices through which such claims were made. Because mirror neurons were staged as nomologically and functionally distinct from the newly named canonical neurons, this "missing link" between the brain and sociality rendered *objects* and *animate others* as natural kinds, hard-wired in human brains.

2.7 Objects and Others in Practice

Is the difference between objects and animate others hardwired in the brain? Heuristic social science that looks at the emergence of seemingly natural categories instead stresses the influence of historical and social forces (e.g., Star 1990, Bowker and Star 2000). In ethnomethodological approaches, which describe the level of lived interaction, the distinction between animate and inanimate interlocutors has been shown to emerge in practice. Alač demonstrates this clearly in her studies of roboticists in interaction with and around their robots (Alač et al. 2011, Alač 2016). Examining the "thing-like" or "agent-like" character of the robots in interaction with roboticists, children, and staff at a pre-school, she shows how the "robot is

[45] For me, this dissertation project began when I encountered the ways that cognitive neuroscience claims had been taken up by theater scholars as *explanations* for the sorts of interaction and meaning-making that happens in theatrical contexts, in some cases even accompanied by suggestions that theater scholarship should take on the scientific mandate for falsifiability (Blair 2007, 2008; McConachie 2008). Compare this with the previously discussed concerns of Penders et al. (2019) that replication as the standard for legitimacy would seep into humanistic fields. For the scholarship and practice of theater, a discipline that has expressed alarm at its increasing devaluation in the academy, the stakes of such a move are great. Importing explanations from neuroscience without a critical engagement with the methods through which those explanations are made puts theater scholarship in an increasingly precarious position in the academy, one in which their own disciplinary methods and knowledges are marginalized while scientific knowledges are legitimated (see Watt Smith 2016 for a related critique). This is discussed further across Chapters 1 and 2.

treated as a living creature while it is handled as a material thing" (Alač 2016, 523). Attending to interaction reveals that, rather than being pre-defined, the status of thing and/or agent is "articulated in moment-to-moment practical encounters" (520). The agential character of these robots is interactively achieved *alongside* their "thing-ness," in the culturally and historically shaped settings that condition their interactions.

Highlighting the ways that settings are culturally and historically shaped in respect to the naturalized differentiation between objects and others recalls how, in fact, the category of human being has been shown to be just as contingent as that of the robot in Alač's case. Experimental composer and music scholar George E. Lewis challenged the purportedly settled sense of "liveness" in performance by relating the portrayal of robot-slaves in Karel Čapek's 1920 play *Rossum's Universal Robots* (*R.U.R.*)[46] with that of Blind Tom, an enslaved American musical prodigy, composer and pianist, who was later thought to have been autistic (1849-1908). Lewis (2018) showed how the very category of liveness was in question in descriptions of Blind Tom's performances; by juxtaposing reports of young Tom's live performances with those of young Beethoven, Lewis showed how journalists exalted the genius and creativity of the latter, while descriptions of the former invariably stressed the agential qualities of the *music* as it moved through the vessel that was Tom.[47]

The dynamic and situated quality of attributions or denials of animacy, agency or personhood, described above, may call into question the usefulness of the mirror neuron

[46] R.U.R. uses the Czech word *robota*, "hard labor" (from roots of "forced labor"), which—through the success of the play—became the origin of the word "robot" in English.

[47] The slippage between objects and others pervades contexts of cultural production. Fernando Dominguez Rubio (2020) has shown the concerted efforts of museums to keep their art objects alive, while Bennetta Jules-Rosette and J.R. Osborn have demonstrated how curators depict living artists as manipulable commodities equivalent to art objects (2020, Chapter 4).

interpretation for describing intersubjectivity.[48] But also important is to notice how locating objects and others as natural kinds, inscribed in the brain, has transitive effects on the lived experiences of human beings. Mirror neuron research bolstered a domain of autism research which, from the mid-1980s, advanced the claim that autistic children lacked a "Theory of Mind" (ToM). Lacking a ToM meant that autistic children did not have the capacity to interpret the thoughts, feelings or intentions of others; thus, they could not recognize emotion in gestures or facial expressions, they could not identify beliefs or thoughts that differed from their own, and therefore perhaps did not experience feelings of empathy or compassion in the conventional sense.[49] These claims (despite being widely challenged by autistic communities and others), continue to be hugely impactful, propagating stigmatizing perceptions of autistic people as robotic, alien, or less-than-human.[50] The ToM experiments and interpretive practices that led to this claim are themselves examined in a later chapter (Chapter 3). For now, suffice it to say that, as excitement about ToM research was at its peak in the late 1990s and early 2000s, mirror neurons offered a biological mechanism through which to ground claims about ToM as a building block of intersubjectivity which autistic people, characterized instead by their "broken mirrors," were purported to lack.[51]

My larger point in this introduction is not primarily to critique the mirror neuron paradigm, but rather to suggest that highly relevant and challenging perspectives like Alač's, Lewis', and those of autistic advocates are too rarely incorporated by scientists to (re)consider

[48] Attempts to recuperate the scientific descriptions of mirror neurons have pushed against the object/other distinction as hard-wired and instead embraced associationist or developmental models to describe mirror (and canonical) neurons as responsive and adaptive to lived environments (Heyes 2010). Maxine Sheets-Johnstone, in particular, argues that mirror neurons are the neurological correlates of our bodies as they are lived in motion and through kinesthetic feeling (Sheets-Johnstone 2012).
[49] Baron-Cohen et al. 1985; Clark et al. 2008.
[50] For example, Gernsbacher 2007; Hacking 2009; Bergenmar et al. 2015; Yergeau and Heubner 2017.
[51] Iacoboni and Dapreto 2006; Oberman and Ramachandran 2007.

(in our example) the role of mirror neurons or visuomotor neurons more broadly in the dynamic, cultural and situated nuances of interacting with, as, or through *objects* and/or *agential others*. It is here that I locate the intervention made by my own project, which does not engage directly with mirror neuron research, but rather with adjacent and related cognitive psychology and social neuroscience studies that share many of the same concerns.

2.8 Conclusion

Taking up the STS interest in the doing of experiments, the aim of this chapter has been to explore the role of theater in understanding, designing and interpreting laboratory experiments, particularly those in social psychology and cognitive neuroscience. Rather than understanding theater as an analogy that can be applied to reveal a comprehensive understanding of experimental scientific practice in general (Crease 1993), I instead draw on scholarship that emphasizes the historical interconnections and mutual influences of theater and scientific experiment (Roach 1993, Coffey 2004, Camp 2007, 2014, Watt Smith 2014, Shepherd-Barr 2020). This, then, allows for considering particular experimental paradigms in social psychology and cognitive neuroscience experiments, while noticing the constitutive role that theatricality and dramaturgy play in their design, doing and interpretation.

I rely heavily on Pannill Camp's (2007, 2014) suggestion that the 18th century architectural development of proscenium theaters has shaped subsequent models of apperception and consciousness. Theater architectures where the proscenium—the "first frame"—was designed to vanish in the perception and experience of an audience has been particularly evocative in foregrounding elements of laboratory experiments that are designed to recede so that the phenomena of interest—"the things themselves"—can be grasped. In experiments on

human sociality and on monkey mirror neurons, while researchers' interest lay in modes of human intersubjectivity, their studies were designed to circumvent the knowing, sense-making subject to reveal the psychological or physiological mechanisms hidden beneath. In this experimental dramaturgy, the "human subject at the centre" (Sacks 1985, viii)—to whom a world appears as meaningful and actionable—is actively bracketed as a disappearing proscenium.

In the case of early mirror neuron experiments, the absenting of a meaning-making subject bolstered the differentiation of brain cells that respond to objects from those that respond to others, categorically severing the lived continuum between objects and others in the dynamic and historically situated sense-making of a "subject at the centre." Rather than an insular procedure relevant only to laboratory work, this move has impacted lay theories of subject- and personhood. In the case of autism research, itself spanning the domains of social psychology and cognitive neuroscience, the expressive constraints on autistic experimental participants have reproduced characterizations of autistic people as lacking the capacity to understand or feel others, while failing to grasp the ways that autistic participants understand, feel, or make sense of experiments. The following chapter explores this "double empathy problem" (Milton 2012) by focusing on the design and doing of a series of experiments directly investigating autistic mind-reading and creativity.

CHAPTER 3

Creativity and Autism:

Foregrounding Background Experiments

3.1 Prologue

This and the following two chapters relate to a collaborative experiment I undertook in the cognitive neuroscience laboratory of Dr. Jaime Pineda. I met Dr. Pineda as a student in a graduate seminar he offered on the Mirror Neuron System, and subsequently invited him to attend a performance installation I had made, being exhibited in a nearby gallery. Our conversation about the performance—which was a participatory audio-theater piece, that involved gallery goers interacting with each other—led to a project where Dr. Pineda and I recorded ourselves analyzing video recordings of the performance, as a way to consider how ongoing, dynamic interaction could be understood through laboratory methods.[1] This project, which I later called *Analyzing the Analyst* (Gluzman 2017), was the first collaboration between myself and Dr. Pineda, and although it did not make claims toward generalizability, it did provide a concrete, shared space of action (Alač and Hutchins 2004, 18) that rooted our mutual engagement with possible methods and questions in the lab. This mutual engagement allowed us to legibly ask each other questions and bring up concerns, even when these did not immediately produce solutions, answers or consensus. Subsequently, after the *Analyzing the Analyst* project

[1] The performance, titled *your position*, was created with poet Jen Hofer. Subsequently, I made and published an experimental data video about the ensuing project with Dr. Pineda (Gluzman 2017).

ended, Dr. Pineda saw a funding opportunity[2] to pursue questions in cognitive neuroscience that were interdisciplinary and potentially relevant to arts practice. He invited me to join that project. The new project allowed me to embed myself in a larger process of experimental design and analysis, and to ask questions of that process, not at a distance but from within.

This second project had its roots in discussions between Dr. Pineda and his colleague, clinical neuroscientist Dr. Erik Viirre about a possible future collaboration investigating the neural and physical markers of creativity. Dr. Pineda had done a good deal of previous work investigating the neural correlates of social cognition in autism,[3] and the question of creativity in autism was one that—despite being relatively little studied—was central to claims and debates about core autism deficits.[4] When I joined the project, Dr. Pineda invited me to read some of the background literature on creativity and autism and, based on my own experience with creative practice in the arts, to contribute to designing a study and co-writing a grant proposal. For me, this second project was a real-world opportunity to explore how laboratory experiments on human cognition were staged, performed and interpreted, and to collaboratively consider whether they could be staged otherwise.

This chapter reviews some of this key background literature, first on creativity writ large, and then on creativity and autism. Rather than moving through a great number of research findings and theories of autistic creativity and its deficit, the chapter focuses intensely on a small

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[2] This grant was the Chancellor's Collaboratory for Interdisciplinary Research Award, a UC San Diego initiative that funded a research project involving at least three faculty members and three students from at least two different university divisions. Divisions included Social Sciences, Arts and Humanities, Biological Sciences, Engineering, Business and Medicine. Supporting observations in Fitzgerald and Callard's 2015 monograph on interdisciplinary, it was the availability of grant funding earmarked for interdisciplinary research that provided the imperative, justification and material support for our second project.

[3] Pineda 2008, Pineda et al. 2009, Oberman et al. 2005, 2006, 2008.

[4] As Uta Frith puts it, "The most important theoretical change in the concept of autism centered around a deceptively minor feature, the lack of creative play, which was found to be as unique and universal a feature in young autistic children as was communication and socialization failure" (2002 [1991], 17).

number of key studies that stood out as particularly relevant among the background literature we read. For each of these studies, my own theatrical orientation involved a close reading of how experiments are staged in order to understand the multiple ways those experiments could be experienced and thus interpreted. At the same time, a theater analysis of this series of experiments is not oriented to critique from outside this mode of production, or to create a seemingly objective representation of what this work entailed; it, instead, heeds Lucy Suchman's (1995) cautioning argument that "Not only do representations of work involve perspectives and interests, but work has a tendency to disappear at a distance, such that the further removed we are from the work of others, the more simplified, often stereotyped, our view of their work becomes" (59). Thus, my close analysis of the experiments described in this chapter was led by our team's situated imperative *to respond in kind*. The mandate to propose a new experimental design required taking seriously the concerns and conditions that disciplined how this scientific laboratory research had been done before.

The chapter begins with establishing the background and problematics of laboratory research on creativity writ large, focusing on internal debates over validity and best practices in evoking and measuring creativity in laboratory situations. Then, moving into the specific area of research that looks at creativity in the context of autism, I focus on a seminal 1999 study that established the existence of a "creativity deficit" in autistic children, and related this to an impairment in Theory of Mind, the ability to perceive the mental states of others. By considering the various experiences generated in the doing of the experiment, in relation to its the published analyses, I suggest that the interpretation offered by the study's authors did not inevitably follow from their data, but rather from the experiment's design and performance.

Regardless, the problematic interpretations of the 1999 study to be discussed here are both conditioned by previous research and shape subsequent research. We find similar "deficit-centered" designs and interpretive practices in two related studies. Shaping the questions and methods of the 1999 study was the use of the Sally Anne test, a false belief test using dolls to enact a social situation (Baron-Cohen et al. 1985). Following both of these was a study using the Animated Triangles task, a Theory of Mind test using the Heider-Simmel (1944) animation. The chapter concludes by relating these three experiments to principals and protocols that we would use to structure our own study, to be described at length in the following chapter.

3.2 The Problem of Studying Creativity in the Lab

Since the 1950s, laboratory experiments on creativity have been debated even as they proliferated.[5] Controversy around creativity research did not focus on whether this research was worth doing—indeed few responded negatively when the President of the American Psychological Association, John Guilford, asserted in his 1950 annual address that creativity was a crucial area calling out for psychological research (Plucker, Makel and Qian 2019, 44). U.S. psychology communities in this era valued the scientific study of creativity as a social good that could enhance production, invention and a distinctly American national character, in contradistinction from the complacency and conformity associated with authoritarianism, and the "rigidity" and "stereotyped thinking" considered to index a less-than-human feeble-mindedness (Cohen-Cole 2009, 227).

[5] Although this literature review is concerned with laboratory methods to study creativity beginning in the post-WWII era during the dominance of behaviorism as an experimental and conceptual paradigm, historical studies of creativity often trace the origins of the concept much earlier. Indeed some make the claim that concepts of creativity are invoked in antiquity and developed in the Romantic Age, while others focus more on the direct lineage between contemporary notions of creativity and the work of Francis Galton (Glaveanu & Kaufman 2019).

Many of these early laboratory experiments on creativity explicitly sought to understand the characteristics of prominent creative individuals and the conditions that facilitated their creativity to apply such findings to identify and nurture creativity in talented individuals who had perhaps been overlooked, and to encourage and facilitate conditions to expand creativity in more average populations (Cohen-Cole 2009). One UC Berkeley study that began in the late 1950s, for example, did exhaustive psychometric tests and interviews with a group of world renown architects to characterize the attributes of truly creative people; the principle researcher of the study, Donald MacKinnon, is said to have been motivated by "the tragedy of talent wasted because it is unrecognized" (Serraino 2016, 17). The debates around these studies from within the psychology community did not so much question the aims or logics of such projects, but rather were concerned about the objectivity and validity of the methods used to assess—or measure—creativity in the laboratory (Treffinger et al. 1971; Hocevar 1981).[6] Before turning to the literature on creativity in autism, this section reviews these controversies about creativity research writ large and the validity of its measures. In particular, it introduces some of the tests and procedures that have been widely adopted to study creativity, since versions of these will re-appear when we turn to studies of autistic creativity in the following section.

The explosion of scientific research on creativity in the 1950s relied heavily on adapting psychometric methods that had been developed to study other psychological phenomena, notably intelligence[7]. Although the use of psychometrics to quantify intelligence, for example, had their critiques, creativity measures were even more contested. From the mid-1960s to the present,

[6] The question of whether assessment measures can in fact be separated from ideological presumptions will be addressed in the later portion of this chapter.

[7] The work of J.P. Guilford himself, who called for a turn to creativity in psychology, is an example of this connection. For Guilford, creativity was one of the six general abilities in the structure of the intellect. His theories and psychometric tools to study creativity were highly influential in the 1950s and 1960s, and were developed to supplement gaps in intelligence tests like the ones used to gauge IQ (Serraino 2016, 111).

critical responses to creativity research programs have pointed to the ambiguity of determining whether what was being measured could be meaningfully considered "creativity." This was not only an issue of agreeing on an adequate definition of creativity, but also of ways to validate that these measures were in fact meaningful in the lived world. Creativity researchers are familiar with this "criterion problem," which asks, "What criteria exist against which the validity of creativity tests may be assessed?" (Treffinger et al. 1971, 106). The scientific literature discussing the intractability of this problem remains eerily similar across the decades.[8] The criterion problem has never been definitively solved, and thus establishing the validity of any one method remains an issue. The lack of a core validity measure has meant that many different methods are used in creativity research. In the literature, they are often grouped into three broad approaches: creativity measures of individual capacities, assessments of created products, and investigations of the creative process.

The vast majority of studies focus on creativity in individual capacities and the products produced by individuals in test situations. One strategy to mitigate the criterion problem for this line of research, from the 1950s to the present day, has been the effort to shore up the objectivity of quantifying creativity. This strategy usually relied on psychometric tests to reliably distinguish, for example, between individuals who are more or less creative, or conditions that correlated with producing more or less creativity in a population. This line of creativity research was (and remains) concerned with developing standardized batteries of tests to measure creativity, with an eye toward real-world application in educational and work settings. A few of these standardized tests have persisted, been updated and are still relevant today, like the widely-used Torrance Test of Creative Thinking (TTCT), which, broadly described, provides incomplete

[8] Compare, for example, Hocevar 1981 and Said-Metwaly et al. 2017.

starting material (e.g., a pear-shape outline or a page of circles) and asks the test subject to complete or augment these in as many ways as possible.[9]

To score these sorts of tests (that is, to analyze how the drawings produced are more or less creative), various studies adopt one or more measures that are understood to index one or more aspects of human creativity. Take, for example, a task where human subjects are given a material (for example, a piece of paper) and asked to make as many different things from that material as possible; here, a greater number of things made is taken to indicate an individual's *flexibility*, thought to be a component of divergent thinking in which the ability to imagine *a greater quantity* of divergent solutions to a problem is understood as revealing greater creativity. *Fluency*, on the other hand, is the ability of a subject to pose solutions *across greater numbers of categories*; thus, a subject given a sheet of paper who creates six humanoid shapes from that paper would be scored as less fluent than a subject that creates three humanoid shapes, a ship, a hat and a veil. A subject's *originality* would be assessed by calculating *how statistically different* their solutions were from the solutions of their cohort or a pre-existing baseline sample (Plucker et al. 2019). Finally, either implicitly or explicitly included in these scoring systems is *appropriateness*, a limit placed on what is considered an admissible response *based on assumed norms and expectations*. The ambiguity in what counts as "appropriate" (Abraham 2018: 12-15) is particularly impactful in the case of research on autistic creativity, as we shall see later.

Critiques of psychometric tests generally point to weak validity when results for individuals are not predictive of their performance on other, correlated tests, or when results don't agree with qualitative measures, such as self-reports and reports from teachers, community, and family members. One strategy for avoiding such validity issues in assessing the products of

[9] Torrance 1969.

creativity tests has been through relying more heavily on qualitative assessments by experts (like teachers) or "inter-rater reliability," the practice of ensuring consistency between multiple assessments by lay people, research assistants, or experts. Here, for example, subjects could be given a more free-form task (like drawing a picture, writing an essay, or coming up with solutions for a real-world problem) and the products of their work would be rated as more or less creative by a number of experts who would then compare their assessments. This seemingly sensible strategy, however, has also met resistance. Sternberg (2019, 397) recounts such a study, where teachers were asked to rate an essay and a drawing produced by a number of experimental subjects. The study found that drawings were rated more creative than essays, across the board. Though there was broad agreement ('reliability') among teachers on how they rated these creative products, a later study found that teachers tended to give lower ratings on the writing samples particularly when these essays expressed views or beliefs that the teachers did not share. As Sternberg observes, such "…measurements of creativity can be no better than their raters" (ibid.).

To deal with validity problems that persist in this approach—based on biases and tacit subjective criteria of raters—further studies have attempted to shore up their experimental design by triangulating between different approaches (standardized tests, experimental tasks, expert-rated free-form tasks and/or qualitative assessments from people familiar with the subjects) (Said-Metwaly et al. 2017, 280). So, while qualitative approaches (like interviews, self-reports, phenomenologically grounded observations and other methods that focus on the *processes* of creative engagement rather than their products) do appear among the literature on creativity, they are in the vast minority of methods used. Most often, analyses of process are used as secondary measures to defend the validity of psychometric measures and experimental tasks, which still

make up the majority of approaches, even in contemporary research on creativity (Long 2014). What, then, is a creativity researcher to do? Dietrich (2007) suggests that these continuing uncertainties about the validity of psychometric and experimental assessments of creativity could be mitigated by turning to cognitive neuroscience. That is, turning to the neural and physiological patterns of activity that may be correlated with particular types of creativity could provide a more objective and defensible characterization of creativity in the lab.

3.3 Our Persistent Problem of Defining Creativity

However, in the situated instance of designing our own experiment in the cognitive neuroscience lab, the turn to physical and neural measures of creativity did not settle questions of how to conduct such assessments. When faced with implementing such a study, we would have to design an activity for participants to do, during which we could identify, for example, moments in which these participants were or were not creative, such that the biological data ("biodata") that was collected could be differentiated as being characteristic specifically of creative behavior. There were certain physical characteristics that had been established in previous literature as relevant for creative engagement, but often these physical markers were associated with more broad events that could not be localized exclusively to creativity. For example, previous cognitive neuroscience literature had shown that greater pupil diameter indicated greater attention, focus and engagement (Konishi et al. 2017; Jerčić et al. 2020); while this was taken to suggest greater creative involvement, it was not sufficient as a marker of creative involvement as different from, for example, relaxation, surprise, or fascination. To correlate patterns of brain activity with particular mental states (in our case, with creativity) requires, as Hacking (1983) suggested, producing the phenomenon. In other words, it requires

evoking those specific mental and behavioral states in a laboratory participant while recording what is concurrently happening in their brain. This, in turn, requires addressing the more fundamental question: "*What is creativity*?"

Our original problem — how to identify creativity as it is happening — was not solved by turning to the biology of creativity, but could perhaps be opened up by attending to it as a process of staging, a theatrical process. Thinking of the problem as one of staging does not simplify the task of designing an experiment, but on the contrary, makes it necessary to ask broader questions to pin down the logics of experimental design. Staging practices in the laboratory or the theater involve manipulating and stabilizing concrete aspects of a situation meant to evoke specific experiences in the interests of producing a meaning or effect. Just as staging in theaters is always directed toward particular sorts of imagined audiences (who are imagined to bring certain interests, expectations and modes of engagement to the occasion), so too are laboratory experiments directed to particular audiences, both the ones who will directly experience these laboratory performances as experimental subjects, and ones beyond the lab, professional communities whose interests and expectations also shape the staging of the event as, for example, a controlled study with a certain sample size. Those audiences are not unrelated, of course, and both are embedded in historical and cultural notions that circumscribe the various meanings and values of creativity—just as the 1950s turn to creativity was embedded in political and social concerns about conformity and authoritarianism.

In our study on creativity, we needed to decide how to evoke *the experience* of creativity. In formulating it in this way, our interest turned to exploring the creative process, rather than its products. To design a suitable experimental task, therefore, we needed to wonder, "What is creative engagement?" and, "What experiences will evoke creative engagement?," but also to

consider the audiences and conversations that our answers were directed towards. Thus, not only were we led to ask, "What is creativity?" but to ask further, "Who is interested in defining creativity, and for what purpose?" Were we aiming to define creativity such that creative individuals be better identified and put to use in projects of nation-building and innovation? Was defining creativity a way to better understand basic processes of human interaction with the social and physical world? Or, do definitions of creativity allow for revealing the ways that biological mechanisms underpin cognitive processes? These "big" questions returned again and again in our practical discussions of designing our experiment in conversation with the literature on autistic creativity elaborated below.

While this section has thus far focused on issues surrounding laboratory research on creativity writ large, the study we were designing was, more specifically, concerned with creativity as a reported deficit in autistic children and adolescents. This specification lent a distinctly human face to questions of audiences and purposes that shaped our experimental design. Namely, did autistic people themselves, or their families, figure as "audiences" for this work? In what ways were questions of creativity meaningful to members of these communities? Could a study on creativity in autism reveal or re-specify societal or biological mechanisms of autism? Could it alleviate experiences of autistic suffering or promote feelings of well-being? Or, was autism—as a documented social disorder—a useful "deficit" case to disambiguate how "normal" creativity works? These questions mark the intersections where we needed to acknowledge our premises and aims, and which would steer the dramaturgical design of the experiment.

We were trying to develop an experimental paradigm through which our team could study creativity in people with autism from the perspective of biodata and brain activity (the

usual mode of the cognitive neuroscience lab) *as well as* from a qualitative perspective centering first- and second-person experience (the mode in which I worked). Faced with the task of designing our own experiment in the face of these longstanding debates about how creativity could be evoked or identified in the lab, we moved to thinking through the autism literature on creativity, with an eye toward what sorts of measures such research used, and thus what meanings of creativity were pursued, and toward what ends.

3.4 Creativity and Autism

The background research on autism, and particularly on autistic creativity, is usually told chronologically, beginning in 1944 with Hans Asperger and Leo Kanner, two researchers who independently published studies describing similar cases of children with unusual developmental impairments.[10] These children were identified with "autism" largely based on their aversion to peer interactions and preference for solitary play. The usual story of autism research continues through 1950s and 60s research in social psychology that sought social explanations for what was understood to be a psychological disorder, including Bettelheim's (1967) suggestion that emotionally withholding mothers were to blame for their children's autism. Autism research of the 1960s and 70s occurred largely in domains of applied psychology and sociology, aiming to develop real-world educational and vocational programs to educate and assimilate autistic children and adolescents (Wing 1981). These four decades of research, then, are usually presented as the foundation for the subsequent proliferation of clinical and laboratory research on autism of the 1980s and beyond. It was, notably, in this post-1980s "contemporary" period of

[10] In his 1944 publication, Asperger wrote this about autistic creativity: "Autistic children *are* able to produce original ideas. Indeed they can *only* be original, and mechanical learning is hard for them." Asperger's attributions of originality were contested decades later, often with the notion that this was not "true" originality but a function of their disconnect from social conventions and norms (e.g., Happé 1991, 216).

laboratory research that many of the now canonical models and theories of autism were developed, including Theory of Mind (ToM), the executive disfunction hypothesis, and the weak central coherence (WCC) model, to be discussed below.[11] This period also saw the growing belief that neuroscientific strategies used to identify brain mechanisms could provide concrete evidence to support one theory over another. Here, rather than re-telling this narrative,[12] and in keeping with my interest in laboratory stagings, I focus on unpacking three significant laboratory experiments that shaped scientific understandings of autism. Again, the aim here is not to critique these—although I include critiques of each from within and outside their field—but rather to look to their dramaturgies and interpretations to help us imagine our own experimental design in respect to the contribution our study could make. The first, and most relevant to our research topic, was a 1999 test of creativity in autistic children.

Autism has long been thought to involve impairments in three domains: *social relations*, *communication* and *imagination*.[13] The former designation of autism as a spectrum disorder is rooted in this "diagnostic triad" of symptom clusters, with individual diagnoses displaying deficits in at least two out of three of these domains. While the majority of research on autism had been (and continues to be) focused on examining and improving capacities for social interaction and verbal communication in autistic people, less investigated has been the described deficit in imagination. Autistic "imagination disorders" have been understood as non-creative behaviors, including "rigid adherence to routine, repetitive activity, and narrowly focused interests" (Roth 2007, 278). Imagination disorders have also been linked to impairments in

[11] Lyons and Fitzgerald 2013.
[12] Berend Verhoeff (2013) examines how the history of the scientific and medical study of autism is narrated in accordance with the values and understandings of contemporary narrators, selectively creating a seemingly linear progression in the understanding of autism.
[13] Wing 1981, Roth 2007, American Psychiatric Association 2013.

"pretend-play" observed in autistic children, for example their avoidance of role-playing games or difficulties projecting animate traits onto inanimate objects, activities understood to be characteristic of typical childhood development. While imagination disorders have been least investigated among the triad of symptomatic domains, they became more interesting after seminal studies in the 1980s dramatically demonstrated that autistic children had trouble identifying beliefs and perspectives different from their own (Baron-Cohen et al. 1985), a key finding in support of the Theory of Mind model of intersubjectivity. If imagination disorders included trouble with pretend-play, it was thought that this could reveal something about how and why perspective-taking was impaired in autism. Further, it was argued that a better understanding of the purported impairment in ToM shown by autistic individuals could reveal something about how ToM functioned in "normal" social cognition. The overall goal in 1980s autism research was to find a single, global explanation—or 'core deficit'—for the spectrum of impairments observed in autism; in line with this, the majority of research focused on characterizing communication and social deficits, often attempting to correlate these to physiological or developmental events (Frith 2012). The relatively few studies that pursued research on autistic creativity and imagination attempted to more precisely specify the particular *kinds* of imaginative thought that were impaired in childhood autism, seeking to connect these to the other symptom clusters to provide empirical support for competing models and theories of autism, including ToM, executive disfunction theory and the weak central coherence model (ibid.).

With Dr. Pineda and the research team, we wrote our initial application for grant funding using precisely this argument: that disorders in autistic creativity and imagination were understudied, and thus our project was warranted. The paper we cited to support this claim was a

seminal 1999 article premised on the same claim, "Creativity and Imagination in Autism and Asperger Syndrome," by Jaime Craig and Simon Baron-Cohen. Though not the only study on autistic creativity, its experimental strategies, interpretations and subsequent influence make it worth examining closely. In my account of the Craig and Baron-Cohen study, I focus on how their experimental design—adapted from tasks in the TTCT—reflected the authors' assumptions about creativity while also dealing with the broader difficulties of studying creativity in the lab. Looking at the ways their experimental design could provide certain types of data over others, I consider how even the limited set of resulting data could have different interpretations and give rise to different conclusions than the ones posed in the published study.

In the introduction of their paper, the authors, unsurprisingly, acknowledged the problems with defining and evaluating creativity, and addressed these by specifying what they understand creativity to be. Citing Crutchfield (1962) and Wilson (1956), they suggest that creativity can be understood "by contrasting it to *conformity*" (Craig and Baron-Cohen 1999: 319; italics added). This definition, then, shaped the authors' choice of methods in measuring creativity in the lab. To reflect the novelty/conformity binary, the authors analyzed what their experimental subjects *produced* and rated these using familiar and standardized TTCT measures like novelty, flexibility, and fluency. To justify the strategies used to stage creativity in their own study, Craig and Baron-Cohen stress that previous literature had established functional connections between *creativity* and *imagination*: "Flowers and Garbin emphasized the role of imagination in the creative process. They suggest that creativity involves the generation, manipulation, and transformation of images to generate novel representations" (ibid.).

Thus, the authors of this canonical paper adopted tasks that asked experimental subjects to generate, manipulate and transform images and objects to generate novel representations.

Further, to specify what aspects of imagination—and thus creativity—are impaired in autism, they conducted three different experiments to disambiguate different forms of imagination necessary for subjects to successfully complete tasks. The tasks they used in all three experiments were derived from sections of the TTCT, but simplified for their experimental populations. Subjects in the experiments were in one of four groups: a group of diagnosed autistic children, a group of children diagnosed with Asperger Syndrome ("AS"), and two control groups: a typically developing ("Normal") group of primary school children, and a group of children diagnosed with moderate learning difficulties (MLD). Autistic participants were matched with MLD participants according to their Verbal Mental Age, as determined by a standardized test. The AS group, who by definition displayed no verbal impairments, were age-matched with children in the autistic group.

In the first experiment, subjects were presented with a sheet of paper with 30 pairs of identical parallel lines (Fig. 3.1a), and told, "I want you to make some pictures by adding to these lines. Try to make lots of different things." They were then presented with a sheet of paper with 10 differing incomplete squiggles (Fig. 3.1b), and instructed in the same way. Both sets of drawings were scored for fluency, flexibility and novelty. In the task with lines, AS and autism groups scored lower overall than the control groups, and in the tasks with squiggles, only the autism group scored lower than the two control groups. Not addressing this variation between autistic and AS groups in the squiggle task, the authors concluded that "*an overall impairment in creativity was found in children with autism and AS.*" (ibid. 321).

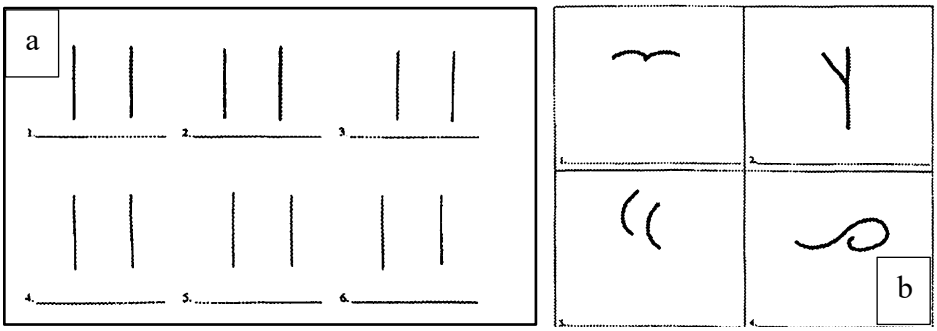

Figure 3.1. Box (a) shows the line task, and box (b) the squiggles task (Craig & Baron-Cohen 1999, 320-1).

Craig and Baron-Cohen's second experiment presented the same subjects with a plush toy elephant, and asked the children to suggest alterations to the elephant, with the spoken prompt, "I want you to tell me lots of ways to make this elephant more fun to play with. What could you change about it to make it different? What could it do?" The authors note that "Lots of encouragement was given" and "the children were prompted for more responses" (ibid. 322). The responses of all four groups were scored on their novelty, flexibility and fluency. The results showed that both AS and autistic groups showed less novelty (statistically rarity) and fluency (category switching) than MLD and Normal groups, with the authors concluding again that AS and autistic groups were less creative. To explain why this may be the case, the authors appeal to two theories. One, the theory of executive dysfunction, suggests that impairments in all types of creativity and novelty stem from disturbances in executive functions of frontal lobe brain areas. The second—the imagination deficit hypothesis—proposes that only imaginative creativity is impaired in autism, not the full range of creativity.

To address these hypotheses, then, the authors also conducted a further content analysis of the toy elephant responses, investigating whether subjects' answers displayed a *reality-based creativity* or an *imaginative creativity*. A reality-based creativity, according to the authors, "entails the production of novel but real-world events," while imaginative creativity entails the production of things that are novel but, in reality, impossible. In the elephant toy test, when

children suggested *additions/alterations* (“Give him a hat”), *manipulations* (“Cuddle him”), or *movement* (“Make his ears flap”), these were all categorized as instances of reality-based creativity. If the response involved some *impossible action* (“He can fly” or “He can read you bed-time stories”), they were categorized as imaginative. Both AS and autistic subjects used fewer imaginative responses than the control groups, prompting the authors to suggest that these results supported hypothesized autistic and AS *impairments in imaginative creativity*.

Table 3.1. Percentage of responses in each category for the toy elephant test (Craig & Baron-Cohen 1999, 323).

Group	Additions or Alterations	Manipulations	Movement	Imaginative
Normal	29.7	4.2	17.7	48.4
Autism	17	45.4	15	22.6
Asperger	70.1	22.7	2	5.2
MLD	20.8	1.1	12.3	65.8

Yet, looking at the published data in a more granular way highlights tendencies that are not explained by impairments in imaginative creativity, and indeed which seem to question conflating the AS group with the autistic group. As shown in Table 3.1, Craig and Baron-Cohen found that almost half of the autistic group’s responses were *manipulations*, while more than two-thirds of AS responses tended to be *additions/alterations*. Table 3.1 also shows that, while AS subjects gave few imaginative responses (5.2%), they gave even fewer responses involving movement (2%). How might this be explained by the suggested deficits in imagination posited by the authors? Further, the finding that autistic subjects had rates of movement responses (15%) similar to those of the Normal group (17.7%) was not interpreted by the authors to be meaningful, and did not prevent them from suggesting that the AS and autistic group showed similar impairments relative to control groups.

It is important to note that, having no access to video recordings of these interactions between the researcher, the elephant, and any of the children being tested, nor to transcripts of the children's responses and the nuances that may or may not have been expressed, my analysis of the toy elephant experiment is limited to the published data like that excerpted above. Thus, my concerns about the toy elephant experiment are based on possible discontinuities between the data produced and the interpretation offered by the authors, namely that autistic and AS response patterns could be accounted for by these groups' shared impairment in imaginative creativity. However, wider skepticism about these suggested impairments also came from educators, advocates and members of autism communities who saw contradictions between this interpretation and the real-world experiences of autistic people. Olga Bogdashina, for example, is an educator, co-founder of the Autism Programme at the International Autism Institute and a parent to an autistic son and a daughter with Asperger Syndrome. Responding to this study, Bogdashina observed, "Though tests like Craig & Baron-Cohen suggest real world creativity and imaginative creativity are impaired, looking at the work of autistic and AS adults shows a great deal of *real world creativity* (e.g., Temple Grandin's designs for animal housing and slaughter facilities) and *imaginative creativity*, especially by autistic artists, poets and musicians" (Bogdashina 2005, 112, italics added). Donna Williams, a prominent author who is both herself autistic and a parent to autistic children, agreed with Bogdashina's view, suggesting, "There has often been the stereotype that people with autism lack imagination. There is, however, a difference between being limited in the capacity to think beyond the self or the here and now, and lacking imagination. Nor is inability to socially invite elaboration the same as lacking imagination" (Williams 2003, 209).

As our team proceeded to imagine a novel experiment on creativity in autism, the disjuncture between the interpretations of Craig and Baron-Cohen's study and insights from the community that comprised their research population needed to be considered seriously. Was it the case that the findings of Craig and Baron-Cohen were illegitimate? Certainly, there was no reason to believe their study was improperly conducted; furthermore, their results and interpretations were in agreement with previous and subsequent research in this area. For example, earlier studies had found that autistic individuals produce less varied patterns of colored stamps or xylophone notes (Frith 1972), less varied drawings (Lewis and Boucher 1991), and fail to understand non-literal or metaphorical language (Happé 1993). Subsequent studies by these researchers seemed to support deficits in imaginative creativity in showing that autistic children had difficulty producing fantasy endings to presented stories (Craig and Baron-Cohen 2000) and in drawing fantastical or impossible creatures (Craig et al. 2001; Allen and Craig 2016). If these studies were capturing a consistent difference between autistic and AS populations and typically developing controls, the question remained whether this difference could meaningfully be considered a true global deficit in autism, or whether it masked other differences that could be more meaningful, both to the study of autism and to the struggles of autistic people. Indeed, of the research cited above, the 2016 study by Melissa Allen and Eleanore Craig reported that, although ASD children were less successful in drawing impossible figures, when planning demands were decreased through the use of templates, ASD subjects performed equally with controls. Allen and Craig concluded that impaired performance in this task seemed to be a function of planning capabilities rather than a "pure deficit" in imagination *per se*. Other lines of research, from the late 2000s onward, also pursued more nuanced accounts of the *conditions* that shaped task performance for people with ASD (Edey et al. 2016; Alkhaldi

et al. 2019), rather than the seemingly clear-cut deficit accounts of earlier work. As seen in the work of Ilona Roth, discussed below, these conditions necessarily include the experimental designs through which autistic children were allowed to narrate their experiences and demonstrate (in)capabilities.

Returning to Craig and Baron-Cohen's 1999 study, it is interesting to examine the third experiment in their series, following the drawing on lines task and the toy elephant task. In the third experiment, to test their "imaginative fluency," the same groups of children were given a sequence of three foam shapes and instructed, "I want you to tell me lots of things that this could be. What does it look like? It can be anything you like." The children's responses were scored according to the number of different responses made (fluency) and further analyzed in regards to whether these were animate or inanimate. The results showed that AS and autistic groups had fewer answers overall (and thus were less fluent) than Normal and MLD groups. Whereas 100% of the Normal and MLD children produced some animate responses, only 33% of autistic and 53% of AS children produced any. The authors concluded that the results suggested, therefore, that both overall fluency and imaginative fluency were reduced in autism and AS.

Whereas in the toy elephant experiment, we could reflect on the experimental design, results and interpretations only through the published data, for the foam shapes experiment, we additionally have a video reenactment from an educational BBC documentary created in 2001 for use in an Open University course on autism. In the documentary reenactment, the foam shape experiment is being administered by Jaime Craig—one of the co-authors of the 1999 paper—to an autistic boy of about eight years old, and a younger neurotypical girl of about 6 years old. Psychologist and Open University faculty Ilona Roth—who both produced the 2001 "teaching video" for her course, and almost two decades later, went back to reconsider this recording—

gives a fascinating re-reading of the experiment. Roth explains that, “in revisiting the footage with a more visual focus, I have observed nuances and considered issues that the experimental tasks were not designed to capture.” (Roth 2019, 72).

The 2001 reenactment of the foam shape experiment features one of the authors (Jaime Craig) administering the test to two children, an eight year old autistic boy and a six year old neurotypical girl. In the video, according to Roth, “Jaime explains that unlike theory of mind and pretend play, which involve social imagination, the tasks we are filming test ‘individual creativity’ – what a child can imagine when not required to attribute intentions or take another person’s point of view” (78). In the scene where Craig actually administers the test, he holds up a long narrow piece of white foam widening into a triangle at one end, and asks the autistic boy what it could be. Roth described the boy as looking “bored and uninterested” as he “tugs at the foam and says ‘glass’” (ibid.). Craig responds, “Good suggestion” and prompts the boy for another response. When Craig holds the foam with the triangle oriented downward, the boy offers his second and last response, “foot,” and mimics walking the foam across the table. The video cuts to Craig administering the same test to the younger neurotypical girl. As Roth describes the scene, “Her eyes light up and she smiles with pleasure. She turns the foam this way and that, modelling and describing her suggestions: extended, the foam is a snake, placed encircling her head, it is a hat, folded over it becomes a measuring rule, then a circle, a leaf and stem, a shoe and leg” (ibid.).

While she does not question that the autistic boy’s responses are fewer and less original than the ones given by the neurotypical girl, Roth’s interpretation focus more on the childrens’ process while responding, “only apparent thanks to the recording.” She observes,

> The little girl engages enthusiastically: each idea is enacted with flair and flows seamlessly into the next. Notwithstanding Jaime’s view that the task evokes

> individual creativity, social imagination infuses her responses. She has to read the intention of the experimenter's instructions as an invitation to perform and acts her part with the experimenter as her audience. Her creativity lies not just in the number and quality of her ideas but in the richness—not scored—of the enactment. By contrast, there is little social engagement and limited enactment in the autistic child's response. (79)

In pointing out the importance of social engagement and enactment, Roth suggests that the differential performance on the foam shape experiment might reflect *low motivation* rather than a *creative incapacity*. Indeed, pointing to the areas of highly specialized interests that many autistic people show—typically seen as evidence for the lack of creativity—Roth suggests that experiments harnessing such areas of pre-existing interest could demonstrate markedly different engagement, and therefore capacities, in autistic children.

From this seminal 1999 study of creativity in autism and Asperger's, as well as from Roth's 2019 reflections and reinterpretations, we could take away a few key points that clarified the study we ourselves would conduct. First, there was no question that the study by Craig and Baron-Cohen demonstrated differences between populations, and showed that autistic and Asperger groups performed worse on the tasks as given and scored. However, questions remained about how these differences were interpreted, whether they reflected real-life experiences of disability, and whether they pointed—as the authors suggested—to a core deficit in autism. For scientific investigations of autism, identifying a core cognitive deficit in should, according to scientists, account for autism's most prominent behavioral symptoms. Moreover, establishing a core deficit should help identify possible neural or biological process correlated with these deficits (Frith 2012, 2081-2). Craig and Baron-Cohen (1999) suggest that impaired performance on measures of imaginative creativity may be related to impairments in pretend-play and perspective-taking, and thus in a deficit of Theory of Mind (ToM). In other words, the

authors' interpretation of their imagination data supports the theory that it is ToM—the ability understanding the intentions, beliefs or mental states of other—that is the core deficit in autism.

It is perhaps unsurprising that Craig and Baron-Cohen link deficits in imagination to those of ToM, since the seminal studies arguing that autistic children lacked Theory of Mind were conducted by Baron-Cohen in the mid 1980s, while he was still a graduate student working with Uta Frith. These ToM studies relied on a different kind of experimental paradigm—the false belief task—to demonstrate a specific ToM impairment. Indeed, the notion that human intersubjectivity emerged through ToM—proposed, paradoxically, in a primate study by Premak and Woodruff (1979)—was largely intractable until the false belief task was developed for humans in 1983 (Wimmer and Perner 1983). Importantly, once the false belief paradigm was accepted as a valid test of ToM,[14] it allowed for scientific research to take up ToM through testable quantifiable and falsifiable experimental hypotheses. A false belief task can be staged a number of different ways, but arguably it is most famously administered through the Sally Anne test. In it, experimental subjects are presented with a situation, usually narrated and acted out by a researcher using two dolls, two boxes, and a marble. One doll (Sally) has a marble, puts it inside her box and closes the lid. The second doll (Anne) sees her do so. Sally leaves the room. Anne moves the marble from Sally's box to her own box while Sally is out of the room. Sally returns. At this point in the narrative, the researcher asks the subject to (1) identify which box the marble is in right now, (2) identify where the marble was at the beginning of the situation, and (3) identify which box Sally will look for her marble. Because Sally should have a "false belief" that the marble remains in her box (false because both Anne and, ostensibly, subjects know it has

[14] ToM, or the ability to understand others as having mental states, presupposes the ability to detect mental states that differ from one's own: therefore, identifying a belief in others that one does not share oneself (a "false belief") was taken to demonstrate a capacity for ToM.

been moved), the correct answer to the final question would indicate that the subject could take on Sally's point of view, even though it differed from one's own.

When Baron-Cohen administered the test to autistic children in 1985, he showed that a significant portion failed the false belief task, while typically developing children and those with mild learning disabilities had no problem passing it. In her reflections on the 1985 experiment, Uta Frith writes, "The results were remarkably clear, showing that the majority of autistic children did not predict Sally's behavior on the basis of her false belief, but instead on the basis of reality…We were amazed" (Frith 2012, 2078). The publication reporting this data (Baron-Cohen et al. 1985) became the landmark study to anchor two different, but mutually related, claims: One was that autistic children had a kind of "mind blindness": they lacked a capacity for ToM, or "mentalizing"[15]. The second claim followed the first: if autistic people could not apprehend the mental states of others, then autistic populations could therefore function as an experimental "deficit" case for validating and elucidating the role of ToM in *normal* human social cognition[16].

An enormous amount of subsequent research would subject both of these claims to experimental tests (reviewed in Gernsbacher and Yergeau 2019). Few of the subsequent studies whose findings supported ToM as a deficit in autism had data as dramatic and clear-cut as Baron-Cohen et al. (1985) [17]; interestingly, a notable proportion of studies failed to replicate

[15] *Theory of Mind* and *mentalizing* are used equivalently here, as they are in the scientific literature more broadly. The term "mind blindness" is a term used by these authors to characterize the inability to mentalize, used chiefly in reference to autism (Baron-Cohen 1997).

[16] "The hijacking of autism by those primarily interested in normal development has added greatly to the intellectual richness of autism research. But just how well does the theory of mind account explain autism ? By the stringent standard that explanatory theories must give a full account of a disorder (…), not that well." (Frith and Happé 1994, 118).

[17] Some studies have shown that autistic participants without verbal impairments could, in fact, succeed at the Sally Anne false belief task (see Frith and Happé 1994). In response, other experimental procedures were developed to capture the ToM deficit, since it was assumed that the Sally Anne task was perhaps too easy and thus had a ceiling

those findings altogether, and challenged the ToM deficit as ubiquitous in autism. Yet, surprisingly, even though these failed replications and challenges are some decades old, and though core principles of the scientific method dictates otherwise, this conflicting data has not resulted in ToM being discredited as a core deficit in autism. As Gernsbacher and Yergeau (2019) point out, the theory continues to appear in textbooks and justify new lines of research. Indeed, having been dramatized by false belief tests like the Sally Anne performance, perhaps the longevity of the ToM account of autism is not so surprising. Because they present simple and coherent stories, supported by neatly delineated results, these false belief tests are powerful narratives taken up as *demonstrations* of a "naturally occurring" phenomenon. However—as we saw with the toy elephants and foam shapes of the creativity tests discussed earlier—attending to the way experiments are *enacted for* and *experienced by* subjects often muddles the clarity of these narratives, challenging the simplicity and "naturalness" of the phenomena they illustrate.

The Sally Anne test, when administered to autistic children, was interpreted and widely understood as demonstrating that these children were unable to take the perspective of another person and thus could not attribute agency, intention or mental states to others (the capacities that constitute a ToM). However, we get a different perspective on this experiment when Ilona Roth revisits her film archive and considers a recording showing Baron-Cohen administering the Sally Anne test to children. Interestingly, she notes that—faced with what is essentially a puppet show, with Baron-Cohen as puppeteer—even the autistic children have no difficulty in attributing agency to the dolls, a fact that is overlooked in the test's interpretations. Meanwhile, she notes,

effect. More difficult false belief tasks were implemented: second-order false belief tasks, for example, used similar Sally Anne paradigms but asked participants to identify what someone else might respond when asked where Sally will look for her marble. In other paradigms, like the Strange Story task, or the Faux Pas task, participants are presented with situations involving characters lying or acting inappropriately, and asked to characterize the motives for these actions and/or their effects on others (see Gernsbacher and Yergeau 2019 for a critical review of these studies).

Baron-Cohen's feedback to each of the children's responses ("Good" or "Well done") is standardized and limited; while this is meant to avoid biasing the results, it also misses the opportunity to seek greater understanding about autistic children's logics by further inquiring *why* they think Sally would believe the marble to be in Anne's box. By never revealing to the children that their response was incorrect, the experimental design privileges the power of an argument illustrated in terms of incontestable data over a more nuanced process of exploring the experimental phenomenon (and the experiment itself) through the perspectives of these children (Roth 2019, 74-77).

Although it remains influential, mind blindness as the core deficit in autism has been challenged or augmented by a number of other theories (e.g., executive function deficiency, monotropism, and weak central coherency). One of these, weak central coherency (WCC), was proposed by Uta Frith to augment the explanatory power of a ToM account, which she noted could not address a wide range of autistic behaviors described in clinical observations and experimental studies, including "narrow interests, savant abilities, and repetitive behaviors" (Frith 2012, 2081; Frith and Happé 1994). Many of these behaviors, we can recall, were also ones suggested by Craig and Baron-Cohen to demonstrate impairments in autistic creative imagination. Although these purported creativity deficits were assumed to be linked to troubles with pretend-play and ToM, Frith proposes WWC as another way of accounting for such behaviors. The WCC theory suggested that autism was characterized by strength in perceiving and manipulating *details* but impairments in *holistic perceptions* that would gather these details into a coherent story or a "meaningful whole,…failing…to interpret stimuli in terms of gist and context" (Klin and Jones 2006, 40). Despite the diminutive "weak" in its name, Frith saw the WWC account as moving away from a deficit paradigm of autism, and toward a model where

autism manifests in particular disabilities but also in unusual or noteworthy abilities (Frith and Happé 1994, Frith 2012). Because it captured both strengths and impairments, WCC was posited as a cognitive *style* rather than a cognitive *deficit* (Frith and Happé 1994).

Theories, including cognitive theories like ToM and WCC, call for specialized experimental paradigms that can test their particular premises and predictions; without appropriate ways to evoke these phenomena in labs, theories remain severely limited in their uptake. Frith supported her initial proposals of WCC with evidence from much earlier studies. Citing Hermelin and O'Connor (1967), she noted that when autistic participants were asked to recall words organized either in a meaningless string or in a meaningful sentence, the coherence of meaning did not make the recall task significantly easier (as it did for neurotypical controls). She also drew upon her own 1983 study using homographs (words with the same spelling, but different meanings and pronunciations, e.g. tear and tear), where autistic subjects asked to read sentences aloud were not better at choosing the correct pronunciation when the sentence provided context (Frith and Snowling 1983). However, whether or not this reflected a "true deficit" was unclear, since these same subjects *could* read for meaning (e.g., could distinguish tear as a rip from tear as part of crying) *when explicitly asked to do so* (Snowling and Frith 1986; Frith and Happé 1994). Frith made sense of this contradiction by parsing the demands of the experimental task. She suggested that, in "closed domain" situations where more explicit instructions were given, coherency impairments might be *masked* by other compensatory behaviors and capacities. To avoid this, she concluded, "open-ended" experimental tasks were needed to elicit the "spontaneous approach or automatic processing preference of people with autism" (Frith and Happé 1994; Happé 2005).

ToM, as discussed above, became a viable subject of research when it could be staged in the laboratory through false belief tasks. Similarly, WCC became testable when "open-ended" experimental tasks could support, challenge or elucidate the predictions of WCC. But this begs the question, what is an open-ended task exactly? Frith specified tasks that are not explicit about what is expected, and are not "closed domains" (like puzzles or calculations). The openness of such tasks do not, however, imply that anything goes or that all engagement is equally valued. Open-ended tasks are ones where some responses and ways of engaging are more "correct" than others, even though the rules governing what is more or less correct are withheld from experimental subjects. So, although WCC seems to be a more agnostic, inclusive and less stigmatizing account of autism than that of ToM, the open-ended experiments used to consider both theories continue to foreground cognitive deficits over capturing cognitive styles. An interesting case in point is the Animated Triangle task.

The Animated Triangle Task, also called the Social Attribution Task (SAT), asks participants to watch a short video clip of animated moving shapes, and then to verbally describe what happened in the animation. The animation they watch is typically the Heider-Simmel animation or a close variant. Created in 1944 by psychologist Fritz Heider and his graduate student Maryann Simmel, the approximately 2 minute Heider-Simmel film features four geometric figures animated to evoke the impression of three characters (a large triangle, small triangle, and small circle) in relation to an enclosure or "house" (a very large rectangle with a movable side); the film still below (Fig. 3.2) illustrates the characters (Heider and Simmel 1944).

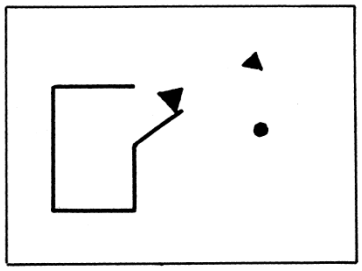

Figure 3.2. Characters in original Heider-Simmel animation:
"Large triangle, small triangle, disc and house." (Heider and Simmel 1944, 244)

When it was first used by Heider and Simmel, the animated silent film presented a sequence of events performed by a large triangle (T), a small triangle (t) and a small circle (c) moving in an out of a large rectangle meant to evoke a house (See fig. 3.3 for the authors' description of the animated events). They asked undergraduate women to verbally describe these after viewing the film sequence presented forwards or backwards. Played both forwards and backwards, the great majority of participants attributed personalities, motives and emotions to the animated shapes as they narrated what they understood to be the events of the film. Decades after this initial study, the Heider-Simmel animation was taken up as an alternative paradigm to study ToM, both in 'normal' development and in autism. Researchers examining the development of mental-state attribution in children (e.g., Thomman 1991; Springer et al. 1996) found that the capacity for such attributions increased with age. When the animation was used to ask similar questions about autistic children, Bowler and Thomman (2000) found that autistic children were able to describe interactions between shapes using mental state language as well as controls. They did note, however, that autistic participants were less likely to involve themselves in the narrative or refer to interactions when objects were not in physical contact.

In the following description of the main features of the picture, the action is, for purposes of reference, divided into scenes. A few 'anthropomorphic' words are used since a description in purely geometrical terms would be too complicated and too difficult to understand. The large triangle is referred to by *T*, the small triangle by *t*, the disc by *c* (circle) and the rectangle by 'house.'

1. *T* moves toward the house, opens door, moves into the house and closes door.
2. *t* and *c* appear and move around near the door
3. *T* moves out of the house toward *t*
4. *T* and *t* fight, *T* wins: during the fight, *c* moves into the house
5. *T* moves into the house and shuts door
6. *T* chases *c* within the house: *t* moves along the outside of the house toward the door
7. *t* opens the door and *c* moves out of the house and *t* and *c* close the door
8. *T* seems to try to get out of the house but does not succeed in opening the door: *t* and *c* move in circles around outside of the house and touch each other several times
9. *T* opens the door and comes out of the house
10. *T* chases *t* and *c* twice around the house
11. *t* and *c* leave the field
12. *T* hits the walls of the house several times: the walls break

Figure 3.3. Heider and Simmel's description of the key events in their animation (1944, 245).

Thus, the findings for autism remained ambiguous, since they showed that autistic children *could* use goal-directed and mentalizing language when describing the animation, but in some ways, they did this differently from neurotypical controls. Subsequent research then made adjustments to the animation in order to tease out these distinctions between the types of attributions that autistic subjects could or could not make. This strategy was also meant to capture impairments in mentalizing for autistic children that showed no such impairments in false belief tasks. In one such study (Abell et al. 2000), researchers made a series of animations based on the Heider-Simmel film, with the new animations falling in one of three movement types. One group of animations ("Random") showed the triangles moving randomly on the screen. Another group showed triangles in interactions where one responded to the movement of another; these were called "Goal-Directed" and were meant to elicit attributions like chasing, following, fighting, or dancing. The third group of animations targeted Theory of Mind

interactions. This group showed one triangle responding to the mental state of another, for example an animation where "one character tried to seduce and persuade the other to set it free," or one showing, "the small triangle mocking the big one behind its back." (Abell et al. 2000, 5). Before each video, subjects were told the triangles represented particular characters to direct them toward appropriate interpretations: in Random interactions, they were "just triangles," in G-D interactions, the triangles were given animal roles (e.g., mother duck and duckling), and in ToM interactions, the characters were human (e.g., grandma and grandson). After each viewing, subjects were asked, "What happened in the cartoon?" Being an open-ended task, there were no explicit directions given about what sorts of answers were expected, and no feedback given other than general encouragement.

Most interesting in this paradigm was the scoring system. Subjects' responses were assessed in two ways. First, the responses were scored for *accuracy*, that is, *how well their response reflected the meanings intended by the animations' designers*. An accuracy score of 2 indicated a "spot-on description of the story or actions represented." If the subject gave a description that was "related to the sequence, but imprecise," they received a score of 1, and 0 accuracy scores were reserved for descriptions that were "bizarre…plainly wrong [or] focus solely on a minor unimportant aspect of the sequence" (Abell et al. 2000, 13). The second way that responses were assessed was by the type of language subjects used, regardless of their accuracy scores; to do this, researchers coded responses for action words (e.g. bouncing), interaction words (e.g. following), or mentalizing words (e.g. pretending). The authors had predicted that, since autistic children had previously been shown to have impairments in mentalizing, they may be expected to perform poorly specifically on the ToM animation. And indeed, even though autistic participants used as many mentalizing terms as controls, the authors

found that, for the ToM animations, the descriptions of the events and the mentalizing terms used were less "accurate" or "appropriate" (i.e., did not match the meanings intended by the designers of the animations).[18]

What do these findings tell us? The authors' interpretation speaks to the hypothesis being tested and the larger conversation that hypothesis is meant to address. In this case, the authors suggest that the findings tell us two things, one about the paradigm, and one about the population. In terms of the paradigm, the authors conclude that these newly developed animated videos are a reliable test to sensitively capture something about the way autistic people perceive mental states differently, even when these are enacted by geometrical shapes. In terms of the population, the study suggests that a specific capacity—ToM—is lacking in this autistic group. These interpretations, in turn, validate a larger conversation investigating ToM as a specific deficit in autism, while avoiding the ambiguity of false belief paradigms.

However, alternate interpretations are possible when the experiment is subjected to other perspectives. There have been instances of autistic people, both within and outside of research professions, commenting on the paradigm. Michelle Dawson, an autism researcher who is herself autistic, is known for her work debunking the evidence and biases behind assertions of autistic intelligence deficits (e.g., Dawson et al. 2007). Though she does not address the Animated Triangles task in her published work, she commented on it in an interview with economist Tyler Cowen:

> Actually, how autistic people perform in social tasks is high variance. It is very hard to pull out specific deficits where you can't point out areas where autistics have performed at *least* as well as typical people on the task. There is an exception, which is the most replicated autistic deficit…This is the task of attributing deep

[18] To arrive at this result, researchers tallied the number of subjects in each group who received perfect accuracy scores on at least two of the four animation sequences viewed for each of the three conditions (Random, G-D, ToM). This seems an odd way to calculate responses (instead of, for example, adding all scores for one type of animation per subject), but the authors did not address the reasoning behind their tabulation method in the publication.

> mental states to geometric shapes…and it goes beyond simply attributing agency to the triangles, which autistics can do. You must attribute mental states to them, profound mental states, like they are jealous or they're flirting. And autistics are very bad at this task. And this has been replicated quite a lot, even at a population-based level, which is not something you necessarily see a lot of. (Dawson 2018)

Dawson acknowledges that, among all the commonly used lab paradigms to study ToM in autism, the Animated Triangle task is one of the only robust laboratory tests consistent in showing impaired ToM performance. Yet, for Dawson and others in the autism community who can draw on their lived experience, the meaning of this difference remains in doubt, as she explains in the quotation below.

> So, is this a social deficit? Autistics are definitely bad at this task. Now, why might that be if you look at things in my own biased way? It may not necessarily be that autistics don't notice that these interactions between geometric shapes, or among geometric shapes, resemble something social that involves mental states. But they are less likely, and they are not going to totally get rid of the accurate information that these are just freaking triangles.

Dawson's view here is echoed in the self-analysis of the author of *Musings of an Aspie*, a blog by Cynthia Kim, a woman with Asperger Syndrome. Kim administered the test to herself and used the research authors' scoring guidelines (included in the appendix of the article) to evaluate her own performance. Though she admits that the test does provide insight into her own thinking style, she, like Williams, questions what it means to perform poorly on the test. "I admit to some difficulty getting past the fact that they're faceless shapes and clearly not people," and summarizes, echoing Williams, "Dude, they're triangles." (April 8, 2014). These "insider accounts" stressing the ambiguity about the real-world implications of the differences captured in the Animated Triangle task are supported in clinical studies; for example, a subsequent study by Salter, Siegal, Claxton, Lawrence and Skuse (2008) that replicated the finding that autistic children used less appropriate mentalizing language to describe the animations also found that performance on this task did *not* correlate with either standardized measures of parent-reported

ToM disability, nor with reports from children's interactions with an observer. Is it possible that the mentalizing impairment identified by the Animated Triangles task is more meaningful in the narrow confines of the experimental configurations and all its assumptions, rather than in the ecological lived worlds of the autistic children who show this impairment?

This problem of the Heider-Simmel animation in autism research is striking because it foregrounds the ambiguities that can haunt the experimental laboratory paradigms designed to provide unambiguous results. Moreover, the lineage of animation experiments shows how interpretations of findings can be limited by the intended precision of these tests and, consequently, an adequate interpretation of the broader meaningfulness of their data. In other words, does the precision of hypothesis-testing lead to conclusions that lack coherence, a problem of failing to see the forest of holistic experience through unwavering focus on the trees of cognitive models? Although the paradigm of Popperian scientific falsification (2005 [1959]) should, in theory, ensure that questionable findings and inaccurate interpretations will work themselves out in subsequent tests, it is equally possible that, instead, problematic interpretive frameworks and unfounded assumptions become tacitly embedded in accepted methodologies that shape the research questions and methods that follow. At the risk of overwhelming the reader with the burden of another close reading of an experimental study, one more example is offered to demonstrate how unexamined assumptions proliferate through experimental paradigms (e.g., false belief tasks, animations) and the data they produce.

The differences found by the study above, among others that used some version of the Heider-Simmel animation, were taken up in a subsequent study conducted by Klin and Jones (2006). This new study extended the Animated Triangles paradigm in order to differentiate between autistic subjects' ability to make *social* attributions and their ability to make *physical*

attributions in narrating the events of an animated film. The authors asserted that making this distinction would allow them to test the predictions of the WCC theory, which proposes that autistic focus on details over coherent narratives might explain a range of symptomatic behaviors. Klin and Jones reason that, because WCC predicts impairments with coherency across all sorts of cognitive tasks, autistic children should have equal difficulty attributing *both* social meaning and physical meaning to narratives; if children have difficulty only in the domain of social attribution, this would indicate a shortcoming of the WCC account. To pursue this question in the lab, Klin and Jones used two different animations. The capacity for social attribution was tested using the original Heider-Simmel animation described above; the authors referred to this as the Social Attribution Task (SAT). The capacity to make physical attributions was tested with a novel animation created by the researchers, which they called the Physical Attribution Task (PAT). They model the PAT on the SAT in several ways, including that it depicts events through the movement of geometrical shapes, and that it was an open-ended task, where the presentation of the animation was "simply followed by the examiner's question 'What happened there?'" The PAT animation (Fig. 3.4), as they describe it, features

> geometric shapes [that] move in a way that depicts the launching of a rocket into space from earth, following the earth's orbit before moving in the direction of the moon. The booster detaches from the rocket, becoming lost in space after briefly rotating around the earth's orbit. The module follows a trajectory to land on the moon, following a wide elliptical orbit, and sometimes disappearing off the screen…The module lands on the moon, and spends a whole (unseen) orbit there…the module is launched from the moon, and moves in the direction of earth. It initially goes around earth, the module booster detaches, and the capsule lands on earth. (Klin and Jones 2006, 44-5)

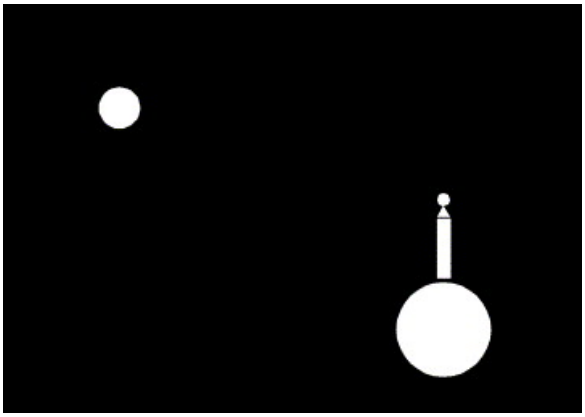

Figure 3.4. The "cast of characters" and opening frame of the PAT animation (Klin and Jones 2006, 44).

Before Klin and Jones could conduct their main experiment, however, they had to first establish the baseline expectations for how people would interpret the PAT, since it was a novel stimulus being used for the first time. So, they did a pilot study to see if age, IQ, gender or other factors affected performance on either animation, if the two animations presented equivalent levels of difficulty, and to log the most typical "physical attributions" made in response to the PAT. The PAT, as the authors point out, required participants to "recognize visual stimuli as physical (*not geometrical*) phenomena and then to extract visual cues from the display to create a physical context (i.e., make physical attributions)" (ibid., italics added). They tested two groups: one group of 35 typically-developing college students (9 males and 26 females) and a group of 40 participants with autism spectrum disorders (38 males and 2 females). The results of this study would surprise to the authors, although, as we shall see below, perhaps even more surprising was the way the authors went on to make sense of the results.

When describing the rocket ship scenario in the PAT, 18 out 26 females (and 2 out of 9 males) were *completely unable* to attribute a physical description of the events and received scores of 0. The authors admit this was "highly surprising" especially given that these same participants performed well on the Heider-Simmel social attribution task. They observe that

typical narratives produced in response to the PAT were "long geometric narratives interspersed with very few social attributions (e.g., "hitting," "hiding underneath") that were used whenever the participant seemed to be struggling to fully articulate a geometric movement" (ibid. 46). They give this excerpt of a typical PAT response and a SAT response from the same person,

> PAT narrative: "There was a big white circle, and attached to it was like a rectangular shape with a triangular shape attached to the end of that and another circle. To the left of it, floating around at first, was a smaller white circle, which then went off the screen to the left. At the same time, the big white circle separated, but the rectangular structure and the triangle and the little circle were all attached as one unit…"
>
> SAT narrative: "It seems like little kids playing to me. The big guy was trying to beat up the little guy. It ran away. It sort of first said "hello," and then the smaller guy realized the bigger guy was being aggressive and was backed into the lower right hand corner of the house. At that point, the circle sort of escaped into the house. As the little triangle escaped from the bigger bully, the bigger guy went inside the house and cornered the little circle. The little circle managed to escape, closing the door behind him and locking the bully inside. The little circle and the little triangle were like 'weeeee!'. They were happy because…" (ibid.)

To make sense of why over half of the typically-developing females (the supposed control group) were not able to extract a narrative from the PAT, the researchers observe that, once told what the plot of the PAT animation was, they could "see" it, but that they showed "no spontaneous tendency to the PAT as a rocketship at all." The authors suggest this may be due to a particular cognitive style in females that had trouble with integrating ambiguous visual displays when they were of a physical nature. The authors suggest this trouble may not be a matter of nature-given skill, but rather a difference in cognitive style shaped by "powerful cultural forces." Incredibly, nowhere do the authors consider that the subjects who were unable to see a rocketship in the animation failed to do so because rocketships, with their modules and boosters, were not of interest to these subjects, and therefore unfamiliar to them. Instead of concluding that the PAT animation was too culturally specific to denote a generalizable event available for

"physical attribution," the authors went on to the main portion of their study retaining both animations exactly as they were, and eliminating the females in both the typically developing and autistic groups.

Here, the point of detailing the logics and interpretations of the Klin and Jones study is not to dispute the use of their PAT animation, but to see the larger problem that inheres even in the seemingly reliable Heider-Simmel animation. Klin and Jones never consider the possibility that their PAT does not actually present a universal narrative but one that requires cultural familiarity and interest to interpret "correctly." While this is itself problematic, it also suggestively calls into question what exactly the failure to attribute mental states in the Heider Simmel animation is a failure of. Is it possible that—like in the PAT animation—the "impairment" of not attributing mental states to the interactions of animated triangles is presupposed by a "deep cultural" difference, rather than a cognitive deficit?

3.5 Considering other experimental paradigms

Lev Vygotsky, a Soviet psychologist who died in 1934, left behind a large body of writing but few detailed accounts of his experimental methodologies. These would be elaborated by the researchers he worked with, and who survived him, like Leonid Sakharov (1994) and Alexander Luria (Vygotsky and Luria 1994). While a more thorough consideration of Vygotsky's approach to experimental methods is not possible here, this section will briefly consider his notion of "double stimulation" experiments, especially in respect the previous discussion of experimental dramaturgies. The question that has organized the discussion thus far has asked how experiments organize a view of their phenomena of interest—what in Chapter 2 I

called “the things themselves.” For Vygotsky’s experimental work, the question remains the same, although his notion of what these “things” were is quite different.

Vygotsky distinguished his approach to experimentation on the basis of its interest in a dynamic (“genetic”) process, as opposed to “the majority of test-based research” which “deals with the result of a previously completed process of concept formation, with a finished product, but does not catch the dynamics of this process, its development, nor its course, beginning and end” (1994, 200). This is precisely what Vygotsky’s double-stimulation experiments aimed to do. Double-stimulation experiments were characterized by having two stimuli presented to subjects (usually children) being “tested.” While the first stimulus (the *stimulus-object*) presented the person with a task to complete, the second stimulus (the *stimulus-means*) presented a sensitive register meant to capture how that person went about completing the task they were presented with the stimulus-object (Edwards 2007, 87). Thus, the focus of double-stimulation studies was to expand possibilities through which subjects could demonstrate their practical reasoning necessary for engaging the experimental task.

Indeed, in contrast to the vanishing experimenter in the previous studies discussed in this section, in Vygotsky’s view, the stimulus-means could in fact be the experimenter. Vygotsky and Luria (1994) describe in a case where a child, “after having completed a number of intelligent and inter-related actions which should help him successfully solve the problem,” encounters an obstacle he cannot overcome and “turns for help to the experimentalist” (117). For Vygotsky and Luria, this is not an unsuccessful task performance, but rather a process in which “[t]he child, consciously introducing another person’s action into his attempts to solve a problem” shows his practical reasoning by “socializing” it, and thereby mobilizing his environment (ibid.). The dramaturgy of double-simulation studies, then, is one that rematerializes a proscenium and asks

instead *how* is it that the proscenium is made to appear or vanish from view. Importantly, this does not necessitate a move out of the laboratory but rather requires expanding the subject of investigation to include considering how the laboratory is managed, lived, and achieved.

This mode of experiment is not limited to Vygotsky's time, but has been influential for subsequent attempts at experimentality under the banner of cultural-historical activity theory or cultural psychology, both in and outside of laboratories (Cole 1996). Engeström (2011, 598) also presents contemporary studies that are based on Vygotsky's double stimulation methodology, proposing "(a) the collective activity system as a unit of analysis, (b) contradictions as a source of change and development, (c) agency as a crucial layer of causality, and (d) transformation of practice as a form of expansive concept formation." Importantly, this "double" dramaturgy of experiment foregrounds the practical intelligence and concrete competencies (Maynard and Turowetz 2017) of participants (a "collective activity system," as Engeström suggests) as a way to orient to the dynamic, moment-to-moment process through which the experimental performance unfolds.

Ironically, although the Heider-Simmel animation has been widely understood and taught as a powerful demonstration of the human tendency to ascribe intention to the world around us and thus adapted to identify mentalizing impairments in autism, it was, in fact, never initially intended to be used as a test of this capacity. It was meant to be a much more radically "open-ended" test for observing *how* people made sense of others, more in line with a Vygotskian double-stimulation approach than the deficit paradigm it came to be used for. In their original 1944 publication, Heider and Simmel explicitly reject an approach based on testing whether experimental subjects can manage "the 'correct' interpretation of [human] expression" or the "correctness of our judgements of others" (243). Taking inspiration from the Gestalt studies on

the apprehension of colors, forms, or movement, where the research questions are instead concerned with "finding out which stimulus conditions are relevant in the production of phenomenal movement and of determining the influences of the surrounding field" (ibid.). Thus, as they clearly state, the animation and narrative task they develop is not aimed to "determine the correctness of the response but instead the dependence of the response on stimulus-configurations" (244).[19] Based on this premise, if two populations consistently differed in their interpretation of the animation, to imply that one was "impaired" would be to miss the point. In this paradigm, all modes of interpretation are not only valid, they present an interpretive process that can be investigated in relationship to its stimulus conditions, a dynamic process of concept formation rather than a static attribution of capacity or impairment.

Sociologist Damian Milton has written about the notion of "double mind-blindness," a "breach in the 'natural attitude' (Garfinkel 1967) that occurs between people of different dispositional outlooks and personal conceptual understandings when attempts are made to communicate meaning" (Milton 2012, 884)[20]. Milton points out that this problem is not one residing in one or the other interlocutor, but rather is experienced by both in their interaction, although he observes that, "the disjuncture [is] more severe for the non-autistic disposition as it is experienced as unusual, while for the 'autistic person' it is a common experience" (ibid.). Milton's model can be taken up here to describe the way that autistic mind-blindness has been marked through experiments and procedures that are themselves "mind-blind," unable to read the mental states of autistic people as anything but lacking. Certainly, the experiments we have

[19] Stimulus-configurations are the configurations of physical stimuli that participants watch, and Heider and Simmel are interested in *how* the participants' various interpretations of the story and characters arise. For example, they point to the aspects of an apparent movement which relate to that movement being interpreted as "fighting" by some participants (Heider and Simmel 1944, 252-3).

[20] While Milton cites Garfinkel to make his point, the argument is relevant also to Goffman's contemporaneous notion of face-work, and the ways "maintaining a line" socially depends on coherency with a "legitimate institutionalized kind" (Goffman 2005 [1967], 7).

examined in this chapter can be held up as examples of such double mind-blindness, a view supported by subsequent studies. For example, Edey et al. (2016) explore the possible influence of double mind-blindness in the Animated Triangles task by inverting the Heider-Simmel animation. In their study, the authors asked their experimental subjects (a group of autistic and a group of typically developing, TD, adults) to *generate* animations by moving two triangles after being instructed to create a particular type of interaction (e.g., a fight, a seduction, etc.). The movement of the shapes was recorded, and these videos were shown to different participants, divided into a TD group or an autistic group. Their results show that, although the TD adults who watched these videos were better at identifying the interactions created by TD adults, they showed a similar level of impairment when identifying the animations created by autistic animators. They were, as the authors describe it, mind-blind to autistic minds and the stories told by their animations. For Edey et al., a possible explanation for why this might be lay in how the animator groups moved; in other words, they suggest that autistic people have atypical body movement which makes them more difficult for TD people to "read" and which, in turn, inflects the movement of their triangles, making the triangles more difficult to read as well. This suggestion calls into question previous theories that claim that autistic troubles in interaction and mind-reading are produced by an underlying deficit in a core cognitive capacity. Further, it supports Milton's proposal that differentially embodied "natural attitudes" are central to dis-alignment between autistic and non-autistic people in interaction.

3.6 Conclusion

Experiments are sites of scientific contestation, and thus they are at the heart of normative scientific reasoning. Normativity advances a perspective on the manner in which

science should be done, based on the theories and values that underlie scientific logics (Popper 2005 [1959]; Merton 1938; 1942). This chapter's review of the experimental literature on autism and creativity has not been led by a normative approach but, instead, by a theater approach. While both approaches call for critical analyses, a theater approach does not appeal to ideological mandates for how science should be done, but rather is concerned with what happens in its doing. A theater approach understands experiments in practice, as *staged, lived events*. Further, a theater-based analysis of the literature both centers the multiplicity of experience within experimental forms of life and takes this concern up *constructively*, in the mandate to itself create a novel experimental form. A theater-based approach, therefore, is not a primarily normative analysis of this body of literature (asking, how should these studies have been done?) but rather a primarily experimental one (asking, what happens when a study is conducted otherwise?).

In designing a study on creativity and autism, the literatures reviewed provided important guidelines for our design. From the studies and critiques elaborated in this chapter, we came to value three principals that would lead our design choices and experimental methods: the open-ended task, the social basis for autism and creativity, and accounting for the lived experience of participants. In taking up the principle of *the open-ended task*—in line with Ilona Roth's analysis of Craig and Baron-Cohen's foam shape experiment—we agreed that, in order to avoid "low motivation," our experimental task needed to be genuinely engaging and flexible enough to allow for subjects' special interests. Further, we would not offer guidelines for how subjects should engage the task, but, unlike Frith and Happe's version of "open-ended tasks" meant to capture deficits that only appeared in the absence of guidelines, neither would we make a priori differentiations between correct/appropriate or incorrect/inappropriate modes of engagement.

Instead, focusing on the creative process (over creativity as an individual attribute or an attribute by which to rank products) meant allowing that all forms of participants' engagement offered important data. Inspired by the history of Heider-Simmel animations as a stimulus in autism research, we decided that the task at the center of our experiment would also involve animation. However, working in the spirit of Heider and Simmel's original analysis and their rejection of deficit-based approaches, our experimental participants would be given the chance to make their own stop motion animations, and our study would be designed to capture how they each went about this. In this way, our study positioned itself as an observational study, not one yet engaged in hypothesis testing.

The second principle guiding our experimental design was that of *the social basis for autism and creativity*. This idea, emerging from the work of autism researchers like Damien Milton (2012; 2014) and Michelle Dawson (Dawson et al. 2007), as well as from the writings of autistic advocates (Williams 2003; Bogdashina 2005), insists that the proper unit of analysis for autism is a social one. This view echoes a broader movement in Disability Studies away from a medical model of disability (where disability is seen as properly belonging to an individual physiology) to a social model of disability (where disability is understood as arising from the interaction between differently-abled individuals and particularly configured social expectations) (e.g., Shakespeare 2013). In autism research, the majority of studies are based on a medical model, and seek to identify a core deficit that characterizes autistic populations. As we have seen in this chapter, such studies have run into trouble when the deficits they characterize do not correlate with the daily lives of people presumably marked by these deficits. At the same time, such studies have difficulties in differentiating between their subjects' "true behaviors" and ones elicited by the specific staging and *mise-en-scène* of their experiment. To incorporate these

concerns into the design of our own experiment, we *focused on the interaction* between participants and a researcher, rather than on participants' actions alone. By defining the unit of analysis as the interaction, we could explore how the conditions of the experiment itself—including the explicit and tacit expectations that participants encountered—impacted the ways they engaged the animation task. The history of creativity research showed how unacknowledged normative expectations (through measures like "appropriateness") served to obscure, rather than reveal, autistic creativity. While our centering of the interaction could not eliminate normative expectations, it was meant instead to make these apparent as they emerged in the shared activity of the researcher and participant working together on an animation project.

The third guiding principle that our literature review highlighted was the importance of *accounting for the lived experience of participants* as they engaged the experiment. This final principle was most complex, especially in the cognitive neuroscience lab that seeks to identify physiological and neural events underlying particular cognitive phenomena. Capturing biological data (for example, EEG, heart rate, or tracking gaze and pupil diameter) normally requires a precise event to reoccur, during which such a measurement is taken. For example, some studies present a series of pictured faces and ask participants to identify the emotion of the face they see; often, biodata is collected in the interval between the presentation of a new face and the button press when a choice of emotion is made. In our open-ended study, however, it was not possible to define a narrow band of events that would trigger the collection of biodata. More importantly, because we were interested in the creative process *as experienced*, it was important to capture creative events from the perspective of participants, instead of imposing pre-defined event categories. To do this, we prioritized a qualitative analysis of the interaction between researcher and participant, using an ethnomethodological approach in analyzing video recordings to identify

events of interest (EOIs) that emerged in interaction. Biodata would be collected throughout the process, but it would be these emergent EOIs that would lead the statistical analysis of quantitative data from brain activity and gaze tracking.

These three commitments defined our approach to studying a creative process in interaction, from the perspective of those involved. In a theater framework, these principles shaped our experimental dramaturgy, i.e., the way we our staging of the experiment could produce or evoke particular insights. The other side of the theater framework, however, understands that staging is just the beginning, and begs the question of how both researchers and participants live these stagings in order to produce data. This question underlies the following chapter, which reports on the study we conducted and discusses its implications for the creativity research and, more broadly, for qualitative, humanistic interventions in the cognitive neuroscience lab.

CHAPTER 4

Animating Experiment

4.1 Prologue

In Chapter 3, a detailed review of some previous autism experiments led to articulating three commitments that would go on to shape our own experimental design. One of these—the commitment to look at creativity *in interaction*—was in response to autistic advocates and disability studies scholars who understood autism through a social model of disability, where disability is taken not as a property of the individual but rather of their interaction with a setting (including others) that fails to accommodate them.[1] Looking back to some of the seminal experiments that described deficits in autistic creativity or mindreading,[2] we further saw how, when taking into account the interactions *through which* these particular experiments were done, their interpretations of subjects' intrinsic deficits were called into question (Roth 2019). Experimental interactions, often bracketed out of consideration by design[3], reappeared as meaningful.

4.1.1. The Performance of the Task is a Joint Enterprise

[1] Shakespeare 2013.
[2] Craig and Baron-Cohen 1999, Baron-Cohen et al. 1985, triangle trick, rocket
[3] This is not to say that interactions and interactional protocol are not considered in the design of human experiments. On the contrary, as I discuss below, these are often tightly scripted to prevent the researcher's conduct from influencing the behavior of the subject. The "bracketing out"—reminiscent of Pannill Camp's disappearing proscenium described in Chapter 2—refers to the attempt to eliminate the interaction as a factor in the experimental data produced.

In this chapter, I consider the interaction of the researcher and experimental subject[4] in the doing of our pilot experiment on autism and creativity. My descriptions draw upon video recordings of these interactions, and rely on an ethnomethodological (EM) analysis of the ways in which the researcher and subject articulate their practical reasoning to each other and in respect to the experimental setting and task.[5] My analysis is therefore in conversation with previous work that centers interaction between subjects and researchers in clinical and laboratory settings. Most directly relevant are studies that examine the "baseline level" of cooperation or agreement required between experimental subjects and researchers. Andreas Roepstorff, in describing an fMRI experiment, describes this as, "a type of contract between the experimenter and the experimental subject where the latter accepts that `when I am tickled under the foot, I will finger tap with the opposite hand'" (Roepstorff 2001, 761). Maynard and Turowetz (2017) refer to this capacity of the subject being tested as their "concrete competency," in other words, their ability to enter into the procedural contract, allowing them to participate in a test or experiment at all.

Looking specifically at testing situations where clinicians administer psychometric tests to autistic children, Maynard and Turowetz (2017) show how these tests, oriented to measure some "abstract competency" indexed by test performance, can obscure the "concrete competencies" and "autistic intelligence" the child brings to the situation. Similar observations have been made when researchers noticed how the apparent impairment of children with learning disabilities changed as a function of the environment where they were assessed: they performed best in their own everyday lives, worse in after-school clubs, still worse in classroom work

[4] In discussing our study, I generally use the term "experimental subject" rather than the preferred "experimental participant" for the sake of clarity, since, in this case, the experimenter is also an "experimental participant."
[5] Garfinkel 1967, Alač 2011. Further explanation shall be presented in the "Qualitative Analysis" description in the section "Data Analysis," in this chapter.

groups, and worst of all in one-to-one testing (McDermott and Varenne 1998). Indeed, ethnographic and ethnomethodological studies of autistic children in their everyday lives are able to describe what, by medical definitions, would appear to be an oxymoron: autistic sociality. This is the claim of linguistic anthropologists Ochs and Solomon (2010), who observed over the course of ten years of ethnographic study that the "orderly social coordination" of autistic children will flourish in particular situations but flounder in others, suggesting that autism is less a stable set of disorders but rather mediated by their "sociocultural ecology" (69).

The import of these observations for laboratory experiments is significant. It suggests, as Roepstorff and Frith (2004) have claimed, that, "[t]he performance of the [experimental] task is a joint enterprise" (195) between the experimenter and subject and in respect to their setting. The adverse consequences of ignoring this co-production often fall more heavily on the person being tested than on the one administering the tests. In his examinations of "how participants make sense of the questions being asked" (Maynard 2005, 500), Maynard's series of studies is able to show how designations of autistic incapacity, deviance or even criminality are occasioned by step-by-step miscoordination in sense-making between clinicians or other authority figures and autistic youths (Maynard 2005; Maynard 2019).

My analysis of the experimental sessions below is inflected by an ethnomethodological attention to participants—both experimental subjects and experimenter—as they together "make sense" of the unfolding event. The difference between my account and most other ethnomethodological or anthropological studies of clinical or laboratory testing situations is that, here, I myself am the experimenter in the episodes that I will analyze. My own participation in these scenes of experiment provides a special case that is particularly relevant to considering

how the experiment itself is figured as "context"[6] for the experimenter-subject interactions and testing activities that occur there, and how that context shapes, and is itself shaped, by those activities. If indeed performing the experimental task is a "joint enterprise" between the experimenter and experimental subject, my analysis inquires into how that enterprise is engaged in the moment-to-moment of an experiment on creativity.

4.1.2 Indifference, Neutrality and Observing Myself

In ethnomethodological (EM) studies, the inclusion of the ethnomethodologist as a constituent participant in the analyzed scene is not unprecedented; however, such studies often focus on how the ethnomethodologist him- or herself goes about acquiring proficiency in the activities of interest.[7] One exception that is particularly relevant here is David Goode's monograph *A World Without Words*, exploring meaning-making in the everyday lives of two "severely disabled" and "non-verbal" adolescent girls, one institutionalized, and one living at home but attending school (Goode 2004).[8] His project is an EM ethnography; he therefore spends significant time with each of the girls: with Christina in the institutional setting where she was a resident, and with Bianca and her family in their home, as well as at Bianca's school among the teachers and staff. While some of the interactions he considers take place between the girls and members at these sites, many of the scenes he describes are interactions between him and one or the other of the children.

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[6] Context, here, refers not to a stable pre-existing setting or set of conditions. Instead, following Cicourel (1987) and Goodwin (2000), context is construed as multiple and actively emergent in respect to the various semiotic resources that are called forth in dynamic interaction.

[7] For example, David Sudnow's classic work on learning how to play a video game, or learning to play jazz piano (Sudnow 1983, 1993), Jacqueline Allen-Collinson's (2006) study of regaining competency with a partner in long-distance running (e.g., Allen-Collinson 2006), Eric Livingston's (2006) first-hand analysis of learning to do mathematical proofs (Livingston 2006), and Sebastian Dahm's (2017) account of learning how to hack.

[8] Some of Goode's other work similarly includes himself as a constituent part of the analyzed scenes, like his monograph-length study of himself playing ball with his dog Katie.

As an increasingly prominent member of both girls' lives, Goode was in a unique position to observe and intervene in the ways in which the girls were constituted as particular sorts of agents (or non-agents) in their daily interactions at these settings, by various teachers, clinicians, staff, family members, and so on. In other words, since he himself was a participant across these settings, and had a relationship (a singular and mutual interaction style) with each girl, Goode was positioned to observe some of the different (and indeed conflicting) ways that the girls were articulated in these different settings and, importantly, to comment on whether some of these articulations were more warranted than others.

Goode's own positioning and participation in these scenes (as, for instance, someone with a vested interest in opining whether describing these girls as being "without language" was warranted or not) caused some troubles for the legitimacy of his EM stance. According to Michael Lynch's (1997) review of the book, Goode's attributions of interiority, intention or *creativity* to the girls' actions (in contradistinction to teacher or staff claims that these actions are without sense or meaning) violates the EM principle of *indifference*. Lynch explains,

> When Garfinkel and Sacks introduced the idea of ethnomethodological indifference, some of their language suggested a strong version of value-free sociology that would endeavor "to describe members' accounts of formal structures wherever and by whomever they are done, while abstaining from all judgements of their adequacy, value, importance, necessity, practicality, success, or consequentiality." They make clear, however, that the "indifference" they recommend pertains "to the whole of practical sociological reasoning" (Garfinkel and Sacks, 1970, p. 345). Far from being an effort to set up a disinterested social science, the policy assigns epistemic privilege to no single version of social affairs, including sociology's own professionally authorized versions. *The policy of "indifference" should be understood not as a principle that sets up a purified vantage point but as a maxim that encourages a unique way of investigating how the social order is constituted*. As such, it is a reminder to keep the constitutive order in view. By reminding us that professionals (social scientists, administrative analysts, and social engineers) do not monopolize the development and use of rules, formulae, algorithms, maps, rules of thumb, *maxims*, instructions and the like, the policy of indifference encourages us to examine how humble accounts, the anonymous uses of such accounts, are no less constitutively embedded in the

> society than official, highly publicized, and professionally authorized versions. (Lynch 1997, 371-2; italics added).

The crucial work done by the doctrine of ethnomethodological indifference is not positioning the analyst as neutral or disinterested, but rather positioning all analytical (which is to say, mundane interactional) activities as having similar features across modes of analysis, such that the practice of professional sociology and that of children playing a game are both equally robust examples of "contexted phenomena" in, through, and for which members' observable and reportable practices emerge. According to Garfinkel and Sacks, this makes "professional sociologists' accounts…not different from any other members' phenomenon" and thus suitable as a "phenomenon for ethnomethodologists' interest." (1970, 345). Lynch, in his critique of *A World Without Words*, extends Garfinkel and Sacks' championing EM indifference as an indifference to the site or subject matter of members' methods. Lynch stresses that Goode, in his concern with the ways that the two girls are differentially constituted or undone as communicative beings at home or school, loses focus on what matters: the differential doing of a constitutive order at these sites. Lynch's critique suggests that ethnomethodological indifference, then, is the expectation that the ethnomethodologist will not privilege her value-laden account of the scene over the ways in which the scene's members demonstrate their own mastery of and participation in constituting the "social order" that gives meaning and order to the scene as a particular sort of occasion. While there is no restriction on whether the EM analyst observes or participates in the scene being analyzed, EM indifference asks the ethnomethodologist to, above all, "keep the constitutive order in view." As Lynch explains, "The policy of "ethnomethodological indifference" offers no guarantees, but it does remind us not to lose sight of the way any account—whether of "autophotic play, rocking, tactile self-stimulation" or "Bianca tells me everything"—is constitutively embedded in a more relentless production." (376).

That said, when the ethnomethodologist is accountable to the constitutive order at the scene as a primary participant rather than as an observer—for example, when her participation has observable, immediate and irreconcilable effects on others at the scene and the emergent integrity of the scene itself—the EM maxim to avoid "monopoliz[ing] the development and use of…maxims" becomes an acutely experienced double-bind. However, I hope to show that performing this sort of multiple-membership in practice, not unlike the Brechtian "alienation" technique of playing yourself playing a character,[9] does offer a double-perspective that is particularly relevant to considering the doing of experiments, and is facilitated by the temporal staggering of roles enabled by video recording.

The interaction between experimenter and subject is an important source of insight for an EM account of the experiment. On the other hand, for the cognitive neuroscientist or lab member who has gone to great lengths to prepare the experiment, this interaction is both a crucial part of the event's staging and a potential source of trouble. Because laboratory experiments are typically meant to test the relation between specified conditions (or independent variables) and some observable phenomenon produced in the presence of those conditions (a dependent variable), variations occasioned by the inconsistent behavior of the experimenter could introduce "confounds," or unwanted confusion, making it impossible to establish any clear relation between independent and dependent variables. This sort of confusion would render the data useless, wasting all the time, cost, resources and trouble spent to design and do the experiment.[10]

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[9] Brecht and Mannheim 1961. See also Chapter 2, footnote 47 for a description of the alienation technique and its relevance to considering the "proscenium framing" of the laboratory experiment.

[10] This is the case not only in cognitive neuroscience but also in experimental psychology. As Bailey and Ford (1994) put it, "Standard experimental practice usually restricts experimenters' delivery to a dispassionate reading, and even in instances where elaborate role-playing is involved, little discretion is allowed. Indeed, a host of safeguards have been developed to control for experimenter bias, including blinding experimenters to hypotheses or conditions, automation, and even removing the human element altogether" (386).

Even in the case of our experiment, which was not testing relations between independent and dependent variables, but was rather an observational and exploratory study of the children's creative process as they worked with a researcher[11] to make their own stop-motion animation, there was concern about the conduct of the researcher who would be interacting with the adolescent subjects of our experiment. After multiple discussions among the lab members in respect to the experiment's protocol, we decided that the researcher—that is, I—should remain reserved in my interactions with the children, not offering praise or encouragement that might unduly influence their creative engagement with the animation task.[12] Instead, I was to remain a supportive, engaged and value-free interlocutor, ready to assist the subjects should they need help or advice, for example, with working the animation set-up, and prompting them to narrate their process.

As the experimenter, and participant in the experiment, I was directed to remain engaged but neutral in interaction, and more broadly, was accountable to the smooth enactment of the experimental procedure. As the ethnomethodologist analyzing the interactions constituting the experiment, I was to remain "indifferent" to the correctness of the unfolding experiment, prioritizing instead how its members demonstrated their own participation and sense-making to each other. This knot of roles, expectations and accountabilities may be assumed to create an unmanageable analytical situation, a collision of incommensurabilities. This was not the case.

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[11] Typically, in this and other university labs, graduate students often conduct the everyday implementation of experiments. I was one of two graduate students on the project. The other graduate student, from the Dept. of Bioengineering, was to be responsible for the technical set up of the study. Moreover, since I was deeply involved in the design of the experiment, I welcomed the opportunity to work with the children who were to be our subjects., and thus became the participating researcher (PR) in our study.

[12] In lab notes from January of 2018 (approximately four months before welcoming our first participant, Matthew), where we were first writing out a protocol for what to do while conducting the experiment, this is rendered as, "Issues around **validation** and **not directing/influencing creative choices** / Don't say: "That's interesting!" or "Maybe you should try this instead of that" / Do say: "So tell me what you're doing." (get the kid to narrate their animation while they're doing it)" (bolded portions in original, forward slash marks indicate line breaks).

First, my participation as experimenter and ethnomethodologist were, to some extent, separated in time; the interactions were recorded on video, and it was only later, watching, re-watching and transcribing these videos that I began to do the bulk of the analytic EM work. Certainly, as a participant in the video recorded scenes I analyzed, I had some "member's insight" into what was happening: events not captured on camera, my own emotional responses in some cases, the feeling in the room at times. Some of these were recalled by watching the videos.[13] That said, my own "first person" access to the scene should not be overstated, and was not nearly as impactful as the repeated viewings of these videos and, above all, the painstaking transcribing of the scenes presented below. Second, my own experience of trying to remain neutral in the role of the experimenter (something, as it turns out, I was bad at) while ensuring that the experimental show would go on enriched my ethnomethodological attunement to elements in the interaction which I might have otherwise not noticed. This second point will be further elaborated through the analysis below.

It is actually here, in the text you are reading, that the knot of roles (experimenter, participant, and ethnomethodologist) becomes more difficult to navigate. The first draft of this chapter was written in the style of a scientific publication, in the third person, mentioning that I myself was the participating researcher only after twenty pages of analysis. In the version you are now reading, I foreground my participation, but in doing so, must also call attention to the fault lines that make this text sit uncomfortably, as the Russian saying goes, with one butt on two chairs. I cannot completely do away with the scientific register, because it itself was a constitutive part of our collective reasoning in the justification and design of the experiment; in some ways, it marks my membership in the lab and my full participation in the study. So, while

[13] See also the discussion in Alač 2020 on rewatching and transcribing video in respect to the lived scenes.

some parts of this chapter adhere to the broad structure and register of scientific writing (e.g., a report organized into introduction, methods, results, and discussion sections, justifying design choices, etc.), other sections depart from the compression of details and bracketing of subjective experience that characterizes much scientific writing. In being able to weave together two seemingly incommensurable registers, I am indebted particularly to feminist science studies works that formally pioneered multiple "voices" within scholarly text, particularly Anna Wynne (1988), Susan Leigh Star (1990), and Annemarie Mol (2002). Whereas especially Wynne and Mol marked shifts between registers more formally (for example, with changes in typeface and spatial cues), here I prefer to let my registers of scientific reporting and qualitative "re-membering" sort themselves out in my writing, and your reading, of them.

4.2 Introduction

Autism Spectrum Disorder (henceforth, "autism") is diagnostically understood to encompass some degree of impairment in at least two of three domains: social relations, communication, and imagination (Wing 1981, American Psychological Association 2013). Least scientifically investigated has been the deficit claim in imagination. The methods used by seminal studies in this area that have described imagination and creativity deficits (Craig and Baron-Cohen 1999) have been challenged by some recent studies,[14] as well as by autistic people themselves[15]. One of these counterarguments suggests that the previous studies are calibrated to normative understandings or interpretations of creativity, and fail to capture atypical styles of creative engagement (Roth 2019). In order to consider how studies in the domain of cognitive neuroscience might be oriented to describe atypical creativity, we conducted an exploratory pilot

[14] Roth 2019.
[15] Milton 2012, Dawson 2018.

study to observe autistic and typically-developing adolescents engaged in interaction around an open-ended creative task. By using a combination of qualitative analysis of engagement and quantitative analysis of neural and gaze activity, we aimed to explore the possibilities of describing emergent creative processes without delimiting beforehand what these might look like.

What kind of study was this? It was, first and foremost, a study in a cognitive neuroscience laboratory, meaning that the study should not only allow for observing how participants went about being creative, but also for investigating what was occurring physiologically while they did that. This was done in two ways: by measuring electrical activity of various parts of the brain through electroencephalography (EEG), and recording eye movement through gaze tracking goggles. While recording this sort of "biodata" with these tools was fairly standard for cognitive neuroscience studies of this scale, other things about the study were unusual.

First, the experimental task was an open-ended, DIY stop-motion animation which, in distinction from most other studies in this domain, did not assume how subjects would, or should, engage the task. For example, experimental subjects were offered materials to use in creating the animations, but also invited to bring in whatever materials they preferred to use. Second, contrary to the accepted practice of testing subjects' individual performance on a task, the study examined adolescent experimental subjects in interaction with a researcher. This meant that, not only was a researcher administering or conducting the experiment, but that they were also included in the collection of brain and gaze data; thus, both the subject and I (as the participating researcher) were to be fitted with EEG caps and gaze tracking goggles. This, presumably, could allow for analysis of interesting moments of physiological coordination

between the researcher and subject in respect to what was happening situationally. Finally, the use of "qualitative" methods referred to my EM analysis of the interactions during the animation task, which focused on participants' own sense-making and practical reasoning during the creative process. In the design of the study, the idea was that this qualitative analysis could identify particular "events of interest," and that the "quantitative" analysis of the biodata would be oriented to physiological processes during those identified events. This would prove problematic, an issue I address in the following chapter.

In line with a broader call to incorporate qualitative analysis into the quantitative cognition lab (McAuliffe and McGann 2016; Rendle-Short 2019; Gallagher 2003), our study was positioned to generate observations and understandings that could shape future hypothesis testing. The aim for the cognitive neuroscience lab, therefore, was to generate brain-and-behavior data that could guide the questions and hypotheses of future experiments to be in greater alignment with the grounded experiences of autistic people and with creative processes as they are lived. My own interests, as a performer within the experiment, and an ethnomethodologist interested in the experimenter-subject interaction, were not focused solely on the generation of data for future use. Rather, in line with both EM and theater approaches, my aims were primarily *to attend to the enactment of the experiment itself*. In this, my concerns were not limited to the experiences of autistic people in creative or testing situations, but also included how participants more broadly (both myself as participating researcher, and the youths as subjects) articulated our roles in respect to each other and toward the experimental setting.

4.3 Methods

4.3.1 Participants

Three adolescents between the ages of 10 and 17 participated in our study, each either typically developing or having previously received an autism diagnosis.[16] While this age range was higher than, for example, that of participants in the Craig and Baron-Cohen (1999) study discussed in the previous chapter, this older range was chosen to gauge the difficulty and interest of the animation task in the pilot study. Subjects were two males and 1 female, and one of the males had previously been clinically diagnosed as autistic with no gross language impairments. Parents of participants consented, and participants assented, to take part in the study after researchers explained the purpose, protocol and risks involved. Each participant attended two sessions.

4.3.2 Animation Task

We asked participants to engage with an open-ended task of creating a stop-motion animated video. There were multiple reasons to use a DIY stop-motion animation task. First, as mentioned, we believed the task would be engaging and thus motivating for participants; at the Exploratorium Museum in San Francisco, where a DIY Animation Station was a favorite activity for a wide range of children, I had observed first-hand the enthusiasm with which children and adolescents created their own animations. Because previous literatures suggested that autistic children may be more able and motivated to fully participate when experimental tasks involved their own special interests (McKenzie 2011, 112; Murray, Lesser and Lawson 2005; Smagorinsky 2016), the stop-motion animation task allowed for children to bring in their preferred toys or objects as materials to use in the making of the animation. To foster participants' sense of authorship and investment in their creations, we made it clear before the

[16] Participants were recruited through flyers and emails directed to local university and grade school communities. Families were remunerated $90 total for their time and costs of transportation.

trials began that they could keep digital copies of the animations they created. We also told them that, at some later point, we might use the animations they created to show to future participants in a subsequent experiment. The possibility of a future reception and narration of their work was indeed something we were considering, but this disclosure also served to ground their task in a real-world act of authorship and communication.

Finally, the choice of an animation task allowed for broad comparisons with autism research that had used animation in deficit paradigms. In these previous paradigms, participants were presented with and asked to describe animated videos of interacting geometrical shapes *to capture and specify deficits* in their abilities to identify goal-directed or theory of mind interactions (Abell et al. 2000; Klin and Jones 2006). The open-ended and child-centered nature of our task, however, allowed us to collect observational data on *the process of engaging creatively* rather than toward identifying particular deficits or ranking the number or quality of products created.

4.3.3 Experimental Protocol

The animation task was done twice with each participant. In a first session, I (as the researcher) joined participants and taught them how to use the animation set-up, which we referred to as the Animation Station. They were given a container of tangrams—colorful plastic shapes—to use as materials to create their first animation. Figure 5.1 shows a broad view of the Animation Station and the tangrams.

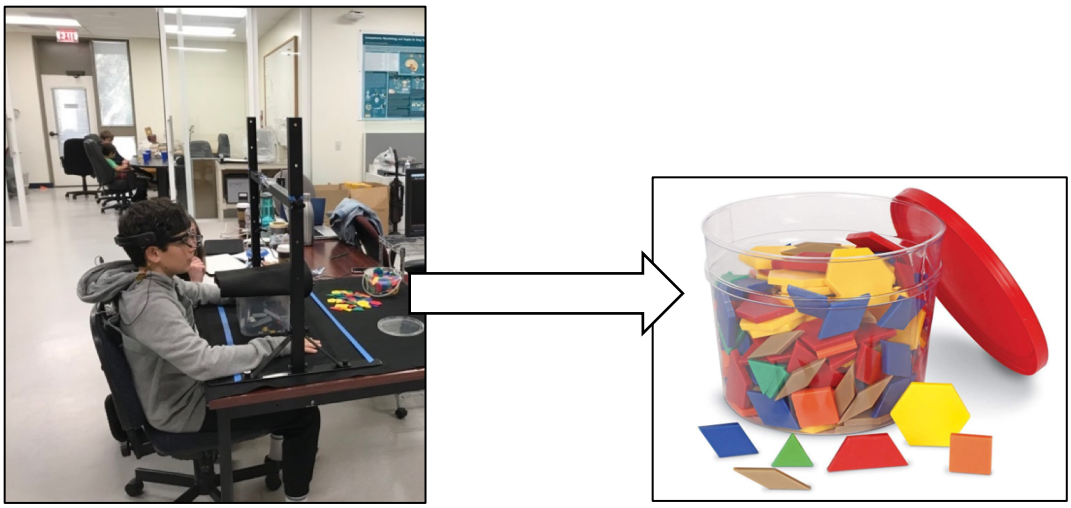

Figure 4.1. Image from a rehearsal of the experimental set-up, showing myself (as participating researcher) sitting next to (and partially obscured by) a stand-in experimental subject at the Animation Station. The expanded image on the right shows the type of colorful plastic tangrams used by participants in their first animation session.

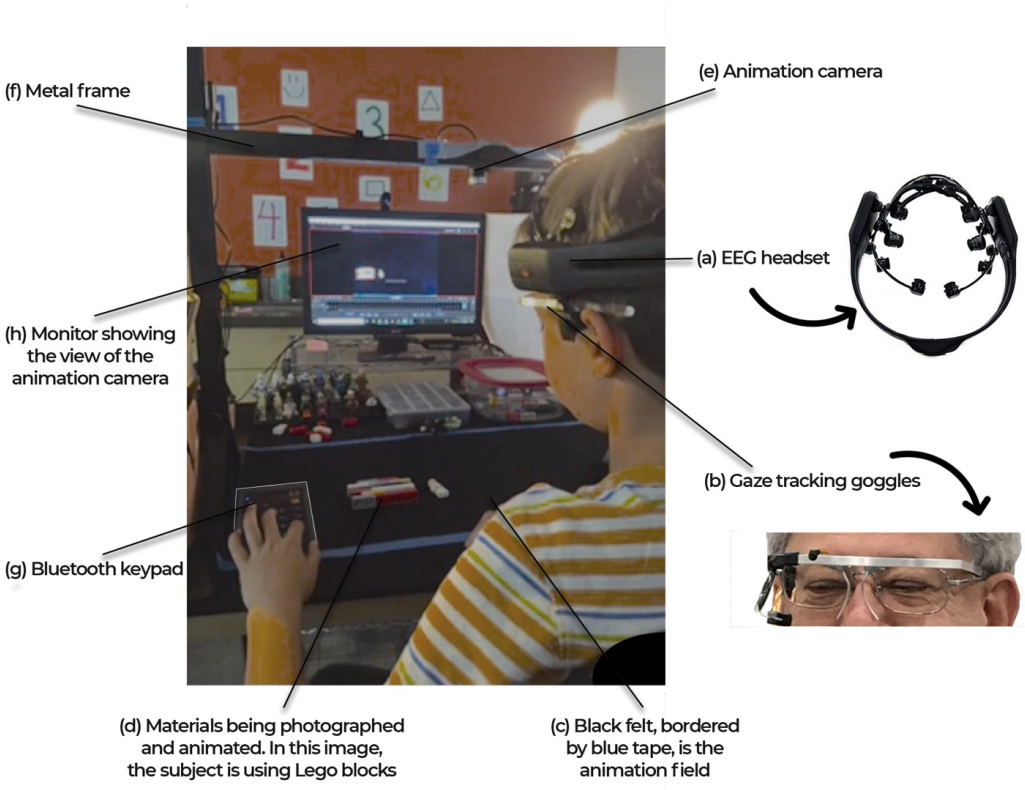

Figure 4.2 The experimental set-up of the Animation Station interface and recording devices. The experimental subject wears a wireless EEG cap (a) and prototyped gaze tracking goggles (b); images to the right show these devices in more detail. The animation field on the table in front of the subject is covered in black felt (c). This is where animation materials (d) are arranged and photographed, using the downward-pointing animation camera (e) that is mounted on the metal frame (f). The subject controls the animation software using a simplified wireless Bluetooth keypad (g), and can continuously see the animation field, as well as play back already-photographed frames, on the monitor they face (h). Though not pictured here, the researcher would sit directly to the left of the participant. Also not pictured is the video camera facing the subject and researcher, which records the interaction.

Before beginning the animation task, both participant and researcher were fitted with Emotiv mobile EEG caps and brain activity data was collected continuously from both.[17] In addition, gaze tracking data was recorded continuously from both participant and researcher, using prototype gaze tracking goggles developed by Dr. Tzyy-Ping Jung's lab (see Fig. 4.2).[18]

With the room lights dimmed, participants sat at a table with the tangrams within arm's reach. As shown in figure 4.2, on the table in front of them was a black felt cloth that constituted the background for their video, mounted by a metal frame that held a suspended, downward pointing camera that would take each still image of their animation. Facing the participants, on the other side of the table, was a monitor that showed the top-down image in the camera's viewfinder, as well as "play-backs" of the animated sequence (see figure 4.2). The camera and monitor were synchronized through Dragonframe, a DIY stop-motion animation software. Participants were able to control taking pictures with the camera, erasing one or more frames of images already taken, and playing back the existing sequence of images by pressing buttons on a wireless handheld Bluetooth keypad, provided by Dragonframe (see detail in figure 4.3).

[17] EEG and gaze data were recorded wirelessly and synchronized into one data stream, bundled with data from the animation (namely, the time at which a participant pressed the "take picture" button at the Animation Station and the photo created) and a web-cam video feed that recorded the interaction between participants and researcher. Though this was the starting configuration of collecting data, some problems with the data flow could not be solved, making it impossible to collect gaze data from the researcher.

[18] Because the gaze tracking goggles were still in prototype form, they needed to be connected by a wire to a transmitter, which in turn had to be worn in a pouch strapped across participants' shoulders. As we shall see in the interactions described below, the goggles, pouch and the EEG cap were the source of some discomfort.

Figure 4.3. The wireless Bluetooth keypad provided by Dragonframe. In our study, subjects only needed to use the five buttons outlined in white to CAPTURE (photograph) a frame, to DELETE one or more frames, to use ARROW KEYS to move around among already-captured frames, and to PLAY, that is, view, all the frames they already captured.

After a participant began making this first animation, they were given no time restrictions but allowed to work until they felt they were finished with what they were making. After they indicated they were finished, I asked semi-structured questions about their animation process, particularly inviting participants to report moments of insight, difficulty or surprise they experienced while working.[19] Finally, participants were told that they would return one week later to make their final animation, this time using whatever materials they wanted. Their second animation, they were told, could be longer, should tell a story, and might be shown for another group of adolescents to interpret in a later experiment.

A second session was held one week after the initial visit, during which participants were given the opportunity to make a longer animation using their chosen materials. Of the three

[19] After this, participants were shown three short Frith-Happé videos, animations inspired by the Heider-Simmel (1944) animated film but further narrowed to show either random, goal-directed, or Theory of Mind interactions (Abell, Happé and Frith 2000). This was done to facilitate comparisons between our study and these previous studies, particularly in considering whether participants' narration of the Frith-Happé videos correlated to the animations they made. Following the method of the original studies using these videos to demonstrate deficits, we asked participants to describe what they saw after watching each of the three videos, and recorded their responses.

participants tested, two chose to bring in novel materials to use in their second animation: Edgar brought a collection of block Legos and Lego figurines, and Hannah chose to use a whiteboard, dry-erase markers and an eraser to create a hand-drawn animation. The third participant, Matthew, chose to use the same plastic tangrams used in session one. In all cases, I was the researcher who worked with participants in both sessions, sitting next to the participant and remaining an engaged and supportive, but neutral, interlocutor throughout the participant-led animation process.[20] When each participant felt their animation was complete, the researcher again asked semi-structured questions about the experience of making the animation. These sessions, including questions and responses, were recorded through the stationary video camera that recorded the interaction during the animation process.

4.3.4 Quantitative Analysis

In relation to the study's interest in the creative process, the quantitative video analysis sought events of interest that indicated members' own expressions and understandings of creativity, and particularly shifts in these. Once the analysis identified events of interest (EOIs), these moments could be located in the time-stamped sequence of biodata. EEG and gaze data in these moments would be analyzed, seeking patterns of activity potentially correlated to certain events and perhaps types of events in an interactive creative process. This analysis was led by other lab members, and will not be addressed in this chapter.

[20] While the study was premised on the notion that both autism and creativity exist as indelibly social, and thus tested participants in interaction, this also created some anxiety about the possibility of undue influence or bias. As mentioned in the "Indifference, Neutrality and Observing Myself" section of the Prologue, during the scripting of the protocol for administering the experiment, the research team decided that the I, the participating researcher, was to avoid praising or in any way influencing the participants' choices or actions, and to remain encouraging but neutral. This restriction aimed to protect the participant's creative process from being overshadowed by the preferences or judgments of the researcher, and ensure that the researcher interacted with each participant in the same way. This shall be discussed at length in what follows.

4.3.5 Qualitative Analysis

Qualitative analysis of video recordings was done by carefully observing and transcribing the recorded interactions. The video analysis did not rely on the sort of video coding typically used to quantify behavioral data, where certain behaviors are pre-defined and each instance of a behavior is identified. Instead, using an EM approach (Garfinkel 1967, Alač 2011), meaningful behaviors, gestures, and actions were understood to be those visibly acknowledged as meaningful by the members of the interaction *while interacting*. Thus, it was not the case that two phenotypically similar behaviors (e.g., leaning forward) were taken to have similar meanings or be grouped together as a type or in a category. Each instance of the gesture was understood through the ways that the interacting pair made sense of it to each other, situated in and contextualized by their particular occasion. I conducted this analysis, and it is the basis of the remaining chapter.

4.4 What happened

4.4.1 Boredom and When *Things* Get Animated

We used a stop-motion animation task because it was assumed to be fun for a broad variety of adolescents, and had the additional advantage of allowing our subjects to choose animation materials that they found particularly interesting. The results of the study did not contradict this assumption: all three participants engaged enthusiastically in creating novel animations, and in all three cases, they spent more time animating in their second session (where participants could bring in their own animation materials) than in the first. However,

participants' enthusiasm for the task was not consistent from the beginning to the end of each animation session, and in most sessions, motivation and engagement were far less when they first began animating, and grew as they continued.

One factor in the low enthusiasm when the time came to start animating was surely the influence of the long, boring and (for myself and other lab members) stressful preparatory process that preceded animation. Participants went through the relatively long process of waiting while EEG and gaze tracking devices were fitted and calibrated on me, then through the process of themselves being fitted with EEG caps and gaze tracking goggles and having each device calibrated, and then waiting again for the research team to troubleshoot,[21] coordinate and initiate the recording of synchronized data streams. This process took an average of 30-40 minutes, but in the charged space of the experiment, felt to me even longer. By the time participants, after these preparations were completed, were told they could begin animating, their energy and enthusiasm were observably low.

As the researcher sitting next to each subject, their low energy and sense of boredom was acutely concerning.[22] Although little could be done about the periods of waiting, I felt some responsibility to somehow mitigate the situation. This was my concern in the beginning of the session with our first subject, a 17-year old autistic teenager whom we will call Matthew. In the excerpt below, I repeatedly try to engage Matthew, with little success. Following the interaction closely, however, foregrounds the mediating and mitigating role of our shared space of action.

The moment transcribed below occurs during Matthew's second session, a week after his initial visit where he was introduced to the Animation Station and made his first stop-motion

[21] The technical coordination between data streams frequently caused problems during the experimental sessions.
[22] My concern and accountability to the experiment's expected modes of engagement shall be explored further in this chapter, and especially in Chapter 5.

animation. Before the transcript begins, we had just finished calibrating Matthew's gaze tracking goggles, after almost 30 minutes of setting up and fitting devices. Though the set-up process should now have been complete, in this instance the animation software was not showing up as a data stream, requiring one of the research team, graduate student Siddharth, to work with three undergraduate RAs to fix the problem. I was the participating researcher (in the transcript, "PR"), sitting directly to Matthew's ("M") left. Like Matthew, I also was fitted with an EEG cap and tracking goggles. Both of us sat silently, facing the table with the Animation Station, looking straight ahead without expression while we waited for the problem to be fixed. A bin of colorful plastic tangrams was on the table in front of me.

In the transcript I use notations adapted from Jefferson (2004) to indicate the volume, emphasis, timing, prosody and pauses of utterances in the interaction.[23]

Excerpt 4.1

1. **PR:** Ok. Soooooo ((*fidgets with tangrams, looking down*)) so whaddaya thinkin?
2. **M:** ((*no response*)) (2)
3. **PR:** ((*looks at M, touches tangrams*)) you wanna use these though today
4. /°right?° >°yeah ok°<
5. **M:** /((*slight nod, still looking ahead*)) (6)
6. **PR:** °Um, do you want me to put this on that side? The clicker? >its up to you (.) how d'you wanna use it its up to you (inaud)<°
7. **M:** ((winces and *shrugs, rubs face*)) (5)

[23] Key to transcription notation:

(( ))	double parenthesis contain non-verbal actions
(.)	single parenthesis with a dot indicate a pause about the length of a beat
(#)	single parenthesis with a number indicate a pause of the length, in seconds, of that number.
° °	small raised bubbles mark the words inside as being spoken quietly
↑ ↑	Upward pointed arrows mark the words inside as being spoken loudly
> <	triangular brackets mark the words inside as being spoken quickly
/	forward slashes show simultaneity of two lines of talk, i.e., people speaking at the same time
(inaud)	indicates that the utterance could not be deciphered from the recording
.	falling intonation
?	rising intonation
!	emphatic intonation
=	indicates no time between utterances, i.e. run-on talk with no pause in between

8. **PR:** ((*dusting off black felt*)) °Do you have ideas this time around°
9. **M:** ((*Shrug, head movement side to side*))
10. **PR:** °>You wanna talk about them or you just wanna start on your own?<°
11. **M:** ((*clears throat*)) uhm I do' know
12. ((*Siddharth and RA set up software off camera, other RAs wait*)) (90)
13. **PR:** ((*rolls chair back, bit away from table*)) So while they're setting up
14. the Dragonframe feel free to start ((*waves hand over tangrams*))
15. planning your stuff out if you wanna try playing around in your field
16. ((*waves hand over black felt*)) your animation field
17. **M:** ((*no response*)) (20)
18. **PR:** ((*turns to M*)) Do you feel like this process would be more helpful for
19. you if these ((touches tangrams)) were separated out ((*M nods slightly*))
20. into by shape or by color, or how would you do it?
21. **M:** ((*looks at box of tangrams from corner of eyes*)) By shape
22. **PR:** By shape? (.) °That's good to know° (5) ((*moves torso forward*)) So let's
23. start doing that (.) why don't we start separating some stuff out by
24. shape so that it'll be easier for you to use ((*Moves bin of tangrams*
25. *toward M*)) I can help, °how bout I take all the hexagons (inaud)°

In Excerpt 4.1, each turn of talk is initiated by me, and with each one, Matthew resists involving himself in the exchange. My first position question in line 1 ("so whaddaya thinkin?") is met with Matthew's second-position refusal to answer (Maynard 2019, 6). This pattern repeats with each question I pose; even when Matthew does respond (line 11), he does so reluctantly and without engaging further. When I invite Matthew to start "playing around in your field" (lines 13-16), this first position invitation is met with twenty seconds of Matthew's silence, implying that he declines the invitation. At no point in this exchange so far does Matthew orient his body or face toward me. From my perspective, he is impassive and immobile, not unlike the boy in the video described by Ilona Roth (2019) in Chapter 3, who seemed unengaged in the experiment and bored, a state of, as Roth suggested, "low motivation."

We have not yet begun animating, and in the twenty seconds of dead silence (line 17) I look off camera, toward Siddharth and the RAs, who continue their troubleshooting. In line 18, I

look back at Matthew, trying a different strategy by appealing to the bin of mixed up tangrams. Instead of beginning this turn with an invitation ("you wanna"), I instead begin by asking for an opinion: "Do you feel like this process would be more helpful for you if these were separated out?" As soon as Matthew nods, I follow immediately with asking for details, in effect eliciting instructions from Matthew that the tangrams should be separated by shape. In lines 22 to 25, I appropriate his decision with the suggestion form, "why don't we…" and myself move to begin this activity. Matthew, though still unenthusiastic, follows suit, and we both begin quietly sorting through and grouping tangrams, directing our attention to them more than to each other. Here, though we share an activity (in the sense that we are both engaged in the same activity), we do not yet work through a shared space of action.

4.4.2 Shared Space of Action

As we saw above, in an attempt to motivate Matthew or draw his interest toward the animating task, I asked him to direct his attention to the tangrams — the materials he had chosen to work with in today's animating session. After about two minutes of quietly sorting tangrams, during which I occasionally asked questions about what shape Matthew would like collected next or where to place a particular pile, Matthew and I have a brief spoken exchange in which, for the first time, Matthew initiates a turn of talk. We can follow this exchange in Excerpt 4.2.

Excerpt 4.2

1.		((*M pauses sorting tangrams while PR continues. Orients his body*
2.		*toward her*))
3.	**M:**	°So :what I'm gonna need is a lot of those little triangles°
4.	**PR:**	((*points*)) these guys?=
5.	**M:**	=Yess=
6.	**PR:**	OK (.) You wan' me to help y' find some? ((tilts bin of tangrams))
7.	**M:**	Yeah.

8. **PR:** OK (.) I'm gonna dump all of these guys out (.) is that OK?=
9. **M:** =°Yeah°
10. **PR:** ((*PR gently dumps out some tangrams from bin*)) °Oh here's one°=
11. **M:** =°Cuz I'm gonna need those for the missiles°
12. ((*M makes eye contact with PR*))
13. **PR:** ↑Oooh↑ good idea! ((*smiling, looks down*)) I like that ((*looks at M*))

Here, Matthew initiates the turn of interaction to make a request for triangles (lines 1-3). The shared and directed activity of finding small triangle-shaped tangrams begins Matthew's larger gesture of sharing an idea he has for his animation. Sharing the underlying idea does not happen immediately, however. After Matthew's directive in line 2, I ask a clarifying question ("these guys?"), establishing Matthew's authority in the matter by confirm my understanding of what he wants. The next two turns (lines 5-9) follow this pattern, with me asking Matthew to confirm what he wants me to do, as our attention aligns on a particular subgroup (the "little triangles") of the many plastic shapes in the container. The back and forth that coordinates us around specific tangrams culminates with Matthew initiating eye contact—for the first time in 40 minutes of interacting. Having gotten my attention, Matthew discloses, also for the first time, something about the animation he wants to make, namely that the triangle-shaped tangrams we are gathering will be used to depict missiles. Immediately, I affirm that something meaningful has been shared by raising my eyebrows, returning his gaze and saying — more loudly than in my previous utterances: "Oooh good idea! I like that." (line 12).

The excitement seen in the interaction above (in distinction to the boredom or reluctance in Excerpt 4.1) is grounded in a mutual involvement with the tangrams as materials for animation. The shared and targeted concern with the tangrams—to their various shapes and colors—is an important condition for the eventual sharing of recognition and excitement about their metamorphosis. It is not the *fact* of depicting missiles that is the event Matthew is

compelled to share, or that I am compelled to affirm. It is, rather, that some of these ubiquitous plastic pieces will be selected and *transformed* into missiles. While traditional approaches to creativity might understand this utterance in regards to concretizing a plan for action (i.e., signaling a movement from divergent to convergent thinking), examining the sequence of interaction as part of a distributed creative process suggests a more complex and dynamic event.

Similar to Alač's observations of scientists using gestures to produce together the meanings of brain scan images, Matthew's utterance does not simply indicate an intention but *itself performs an animation*, incorporating my acknowledgment ("Oooh good idea!") as a distributed resource for this creative transformation (Alač and Hutchins 2004, Alač 2011). In other words, while Matthew has not yet begun to arrange and photograph these triangles-as-missiles in the actual process of making stop-motion animation, he verbally *invites me to see the triangles as missiles* and thereby already begins to *animate* this transformation. Importantly, this transformation is not done only through language, but by its grounding in the materialization of a shared matter of concern, the tangrams.

4.4.3 Breaking Neutrality

The scene in Excerpt 4.2 is the first time Matthew makes eye contact with me and is the beginning of his taking up our interaction as a distributed field for the animation process. However, something else interesting occurs there, also for the first time. That event is in my response, in line 13, to Matthew's announcement that he will use the little triangle tangrams as missiles. "Oooh good idea! I like that" I respond. This comment should presumably be out of bounds for me.

The agreed-upon protocol for our study specified that I take a neutral tone when responding to the way a participant went about creating their animation. In actually executing the experiment, my neutrality turned out to be difficult to achieve. I had not been aware of violating neutrality during my interaction with Matthew, but when we[24] later examined and transcribed the video recordings of his sessions, the transcripts clearly showed that I had regularly used phrases that could suggest praise or preference (e.g., "What a cool idea," "Awesome," "I like that").

While the enthusiasm of my affirmative responses seemed to contribute to a more organic and smooth interaction, the question remained whether they skewed the behavior of participants and had "undue influence" on or even masked their own creative process. While this may seem like a procedural point, peripheral to the aims of the study, it was in fact of central relevance. Recall that, in previous autism research, it was precisely the seemingly neutral affect of the researcher (Jaime Craig, as he administered creativity tests) that was questioned when reviewing video recordings of these experiments (Roth 2019). As Damian Milton (2012) had pointed out, if autistic experimental subjects performed poorly on measures of empathy and mindreading, so too did the experimental interfaces (the conduct of researchers and design of tasks) fail to empathize with or recognize the mental dispositions of autistic subjects. This, indeed, was one of the reasons our study explicitly considered subjects in interaction.

If the concern was that my affirmations unduly influenced the experimental subjects' creative process, the best way to address the concern would in fact be through an EM approach. By looking closely at the particular moments in which such utterances occurred, we could ask

[24] We, here, refers to the undergraduate research assistants (RAs) and myself. We were responsible for initially watching and transcribing video recordings of sessions.

specifically what these phrases were doing in the interaction, how they were taken up by participants, and how that impacted the animation process.

This first affirmation ("Oooh good idea! I like that") occurs at the end of a sequence of interaction in which Matthew takes on an authorial role and I actively signal myself being drawn into his activity. The enthusiasm of my affirmation, punctuated by its volume and accompanying eye contact, presumably does not shape Matthew's creative choice here since his idea to use triangles as missiles motivates this interaction and thus precedes it. Instead, the affirmation, like my previous questions (Excerpt 4.2, lines 4, 6, and 8), makes mutually available my interest, enthusiasm, and involvement in Matthew's authorship. Thus, my second position response "Oooh good idea! I like that" functions less as a directive and more as a way to share Matthew's excitement by signaling my understanding of what it is that Matthew is excited about: inventing a way to depict missiles.

Yet, this moment is not the only one where my conduct can be said to violate my prescribed neutrality. In what follows, we stay in Matthew's session to look at a more complex moment where I "break character" and violate the neutrality principle. Does this introduce bias in visualizing Matthew's creative process? In order to address this question, and the larger issue of the researcher's role in the experimental performance, we will examine how my affirmations are called forth, and how they become part of the ongoing action.

4.4.4 Bias or Recognition?

In the interaction transcribed above, Matthew has not yet begun building the shapes he will photograph and animate, but is preparing for this animation through sorting tangrams by shape and talking with me about how he plans to use them. After preparing neat piles of sorted

tangrams at the edge of the animation table, Matthew begins carefully putting away the remaining shapes that he does not plan to use. As he does this, he tells me that his animation is "gonna be like a little ship fighting a big ship." I respond, "Oooh nice." Here, as in other interactions discussed in the next chapter, I often express affirmations in response to explanations, disclosures or self-articulations of the subjects. Such affirmations seem to invite, and be followed by elaboration by the subjects. This is what happens here.

Siddharth and the RAs, who have been dealing with technical difficulties, tell us that they are now recording, indicating that the animation process can finally begin. Matthew continues to put away extra tangrams and asks me, "Do you know Galaga? That old game…." Galaga, Matthew explains, is an arcade video game that he used to play with his father. The game features fighting spaceships. I ask if Galaga is inspiring Matthew's animation idea. Matthew confirms that it is, and, prompted by my further questions, that the blockiness of the tangrams reminded him of the blockiness of the game's graphics. Eventually, Matthew is ready to begin animating, and I follow along as he begins to place tangrams and photograph them.

The tangrams are a shared semiotic field of interaction (Alač 2009; Goodwin 2000) in the aforementioned early moments of the animation session, grounding the meanings of the exchange between Matthew and myself. However, as the session continues and Matthew assembles tangrams to become recognizable figures, it is no longer the tangrams that constitute the shared field for interaction between us, but instead it is the meaningful *composition* of tangrams—gradually resembling a large spaceship, then joined by a smaller spaceship—being made to move in relation to each other. Whereas, early in the session, the focus on individual tangrams drew our attention to the table-top area in front of us, the growing animation later in the session required a split attention: downward to the figures being composed on the table-top and

also straight ahead, to the monitor showing the composition from above, as seen by the animation camera. While these dynamics of sharing attention change as Matthew begins animating in earnest, the positive valence of my comments do not diminish. Indeed, as I follow Matthew's activity in animating his spaceship, my relationship to the animation, if anything, becomes even less neutral. The following excerpt shows such a moment, in which I am so enthusiastic about one of Matthew's ideas that I continues to discuss it even after Matthew himself decides against it. Here, as above, we are interested in how my enthusiasms participate in Matthew's authorship within the creative process. In the following scene, however, our bodily movements and gestures in respect to each other and the animation space play a prominent role in this process.

The following exchange (Excerpt 4.3) occurs while Matthew is building the larger ship in his animation, on the left side (my side) of the table-top. As with the smaller ship that he built first, he takes a photograph after laying down each piece, so that the animation will show each ship coming into existence, piece by piece, one after the other. For approximately two minutes, I sit in silence as Matthew animates; my chair is further away from the table than Matthew's and I lean to the side of my chair, chin in hand. I follow Matthew's actions by looking consistently at the monitor screen ahead. Matthew's motions—placing a tangram, taking a photo, placing another tangram, taking a photo—are regular, and their rhythm is underscored by the imitation shutter sound (an electronic *shwoop-click*) that marks the taking of each photograph. Matthew's gaze is primarily directed downward toward the table-top, shifting from the large ship he is building to the Bluetooth controller to take the photo and back again. Every so often he casts a quick glance upward at the monitor screen, which allows him to see what the image will look like when photographed. As the big ship nears completion, Matthew pauses and spends about 5

seconds silently looking at the monitor, and then shifts his gaze downward and places the final tangram to complete the ship. At this, I lean back in my chair with my arms crossed and gaze even more intently at the monitor.

In the excerpt that transcribes this interaction, the broken out "b" and "g" lines for each speaker allow for more nuanced descriptions of concurrent ***b***ody movement and ***g***aze, respectively.[25]

Excerpt 4.3

```
1.  PR:                    °I have to say it's really cool looking right now°
     b:   leans way forward, face near table
     g:   at screen

2.   M:   (1.2) mm [I wanted there to be like [a health bar for it
     b:   adjusts battery pack              one hand over table, palm down
     g:   at table                          at PR

3.  PR:
     b:                                   [smiles, opens mouth in silent wow
     g:          [at M

4.   M:   like how much health is has while      /you shoot it
     b:   both hands in front as if holding string /right hand points at left hand
     g:   …at PR

5.  PR:
     b:   …open mouth silent wow with smile
     g:   at screen, at table and back at M

6.   M:   [and every time it would /shoot] (.) and every time it would /shoot (.)
     b:                          /right middle finger touches table [/finger tap
     g:   at table               /at screen then at finger

7.  PR:   [that is :such a good] ide:a!
     b:   …smiling
     g:   at M's finger on table                                    [at M

8.   M:   and you /hit it (.) /it would go down like [/
     b:         /finger tap                         [/9 finger taps to the left
     g:   …at finger        /at PR…

9:  PR:                                              [/that's a great idea! (.)
     b:   …smiling
     g:   …at M                                       /turns toward screen

10. PR:   Why donchu=you could do that /
     b:   …smiling…
```

[25] Here, while the forward slash (/) indicates overlapping speech, gesture, and gaze for one speaker, square brackets ([ ]) are used to indicate overlapping talk or action *between* speakers. Ellipsis (…) indicates that an action is continuing from a previous line.

g: *…at screen…* */at table*

11. **M:** Well I won't have any room to put it
b:
g: *at screen*

12. **PR:** **You c:ould put it :maybe in the** (.) [**upper**?=**upper corner**?] °**orsmthng**?°
b: *sits back in chair with arms crossed* [*rolls up, points, retracts hand*]
g: *at screen* */at upper right corner of table*

13. **M:** [**Because I was I was I was**] **gonna put it like right here**
b: [*traces top blue tape on table*]
g: *…at screen* *at table*

14. **PR:** [**well what if you put it a little bit /<u>down</u>. on <u>that</u> side**]
b: *arms crossed, small pointing gesture* */pointing across M to far corner*
g: *at screen* [*/at far corner*

15. **M:** [**b:ut** (2) **I wanted this**]
b: *moves hand and hovers it over big ship on left of table*
g: *glances at screen and back to hand* [*/follows PR's pointing*

16. **PR:** **of the of the ship.**
b: *…pointing* *retracts pointing hand*
g: *at screen…*

17. **M:**
b: *moves hand to hover over right side of table*
g: *watching own hand on the screen*

18. **PR:** **uh h-how much how much farther can you go up**? (1.5) **hmmm**
b: *leaning forward*
g: *…watching M's hand moving on screen…*

19. **M:**
b: *moves hand horizontally left, right, forward and down to touch blue tape*
g: *watching own hand on the screen*

20. **PR:** °**like what if you put it up there?**°
b: /leans back arms crossed
g: *…at screen, glances at M's hand under the blue tape, back to screen*

21. **M:** **/n-uh it's fine**
b: *fingers on table below blue tape* */retracts hand*
g: *at table then screen*

22. **PR:** °**uh-eh-no**?°
b: *touches same spot on table*
g: *…at screen, glances at M's hand under the blue tape, back to screen*

23. **PR:** **I love that** **/idea though, [that's such a cool idea]**
b: *retracts hand from table* */smiling turns towards M*
g: *…at screen* */at M*

24. **M:** **[if this was higher th:en]**
b: *gestures palm up toward camera*
g: *at the frame where camera is mounted* *toward PR*

25. **PR:** **mmhm (.) yeah right /if-fi this camera was /higher °more room yeah yeah°**

```
     b:                          left hand points at camera     scratches nose
     g:   at screen          /at frame and camera   /at screen

26.  M:                                                 /yeah
     g:                          /at camera frame                at table
```

I initiate this interaction after Matthew has signaled that he finished composing the big ship, and my remark in line 1 that "it's really cool looking" is in a very quiet voice, almost under my breath. Matthew does not respond to this directly, but instead treats it as an invitation to share an idea for embellishing the (already "cool looking") depiction of the large ship: a health bar. The health bar, a feature typical of video games, would show the damage inflicted by one ship on the other once they are animated in battle. Sharing the idea, however, does not mean Matthew believes it will be done: his choice of words in line 2 ("I wanted…" instead of "I want…") already hints that he is sharing an idea that he has decided against. First with my facial expression (lines 3 and 5), and then with words ("That is such a good idea! That's a great idea!" in lines 7 and 9), I immediately indicate my encouragement ("you could do that" in line 10) for what I treat as a proposal. Matthew responds reluctantly, explaining in line 11 that there is not enough space in the animation field for a health bar.

Instead of dispassionately accepting this explanation in the manner of a neutral observer, I pursue the notion. In the subsequent turns of talk, I offer ideas about how a health bar could be incorporated within the limited space available, each of which Matthew entertains but declines. This exchange (lines 12-20) will be examined in more detail below. His final rejection is indicated in line 21 by his statement "n-uh it's fine," meaning that Matthew thinks the animation is fine as it is, without the embellishment of a health bar. This refusal is acknowledged by my "uh-eh- no?" in line 22. This disagreement about the health bar has the potential to precipitate a conflict in the interaction, and I repair this misalignment with my following complimentary

statement ("I love that idea though, that's such a cool idea"). Matthew participates in this sequence of repair by introducing a conditional in line 24, explaining again that his rejection of the idea is predicated on the limited space in the animation field, presumably not due to an irreconcilable belief between us. I confirm and accept this point in line 25.

4.4.5 Animating With Hands, Through Others

In this excerpt, my praise and suggestions do not function as commands, as they do not sway Matthew to include a health bar in his animation. They do, however, get taken up in the interaction by motivating a mutual exploration of the material environment of animation. As shown in Figure 4.4 below, early in this exchange (line 2), Matthew uses his outstretched left hand to enact the large spaceship that would receive a health bar, and then in line 4, he uses both hands to show the health bar itself as an imagined horizontal line ending at each hand.

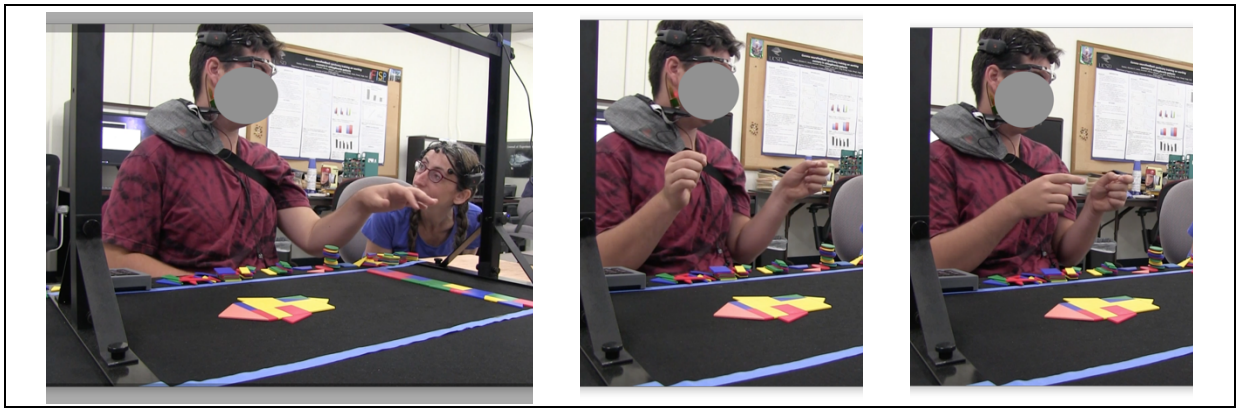

Figure 4.4. Matthew's enactment of the "health bar" using his hands, as described in Excerpt 4.3. The image on the far left happens during the underlined portion of the speech in line 2 ("I wanted there to be like a health bar for it"). The middle image occurs during the first half of line 4 ("like how much health it has…") and the image on the right occurs when Matthew completes the phrase in line 4 ("…when you shoot it").

He then twice enacts the changing level that the health bar would show when its corresponding ship is damaged, once by keeping this imaginary horizontal line aloft and pointing with one hand toward the other (line 4), and a second time (lines 6 and 8) when he uses his finger to tap out the

changing health bar level on the table. In these gestures, Matthew uses his hands to perform and animate a health bar that does not yet exist, thereby making it intelligible to me. Here, as we saw in Excerpt 4.2, Matthew is already engaged in animation, moving his hands and relying on my recognition of his hands as the animated health bar.

At the same time, these gestures highlight the spatial and physical aspects of the animation space, and initiate the table-top space as a site for shared embodied imagining, creating an opening for me to join Matthew in exploring material possibilities. When Matthew initially declines the health bar by explaining that "Well I won't have any room to put it," I engage the particular spatial features of the animation space in line 12, by reaching past Matthew's body to point to "the upper corner," a spot I suggest Matthew could potentially place a health bar. This begins a sequence where we together physically explore the animation space to consider together the ways that the space may or may not accommodate this addition. Interestingly, we do this by looking away from our hands and the table, and looking up at the monitor screen which shows the top-down camera image of the animation field. We are rapt spectators as we intently follow the movements of Matthew's hands on the shared screen. And, as I discuss below, our shared exploration of the animation-in-progress is unevenly distributed across the verbal, physical, spatial and spectatorial modes that must be actively held together.

4.4.6 Collaboratively Doing Authorship

The gestures in lines 12-20 occur over the table, in the animation field. In lines 15, 17 and 19, Matthew uses his hand, held aloft palm-down, as a stand-in for hypothetical elements in the animation, while in lines 13, 19, and 21, he moves his hand parallel to the table to find the boundaries of the field as they are defined by the camera's viewfinder. The viewfinder POV is

available on the screen, to which we are both oriented, and Matthew's hand organizes the material conditions for their shared imagining. Indeed, by engaging the monitor to watch Matthew's hands explore the animation space, we both take on the perspective of the animation's spectator. Sharing this spectator perspective, as it is organized through the spatiality of the screen that faces us, may therefore act to enhance the collaborative character of this exploratory activity.

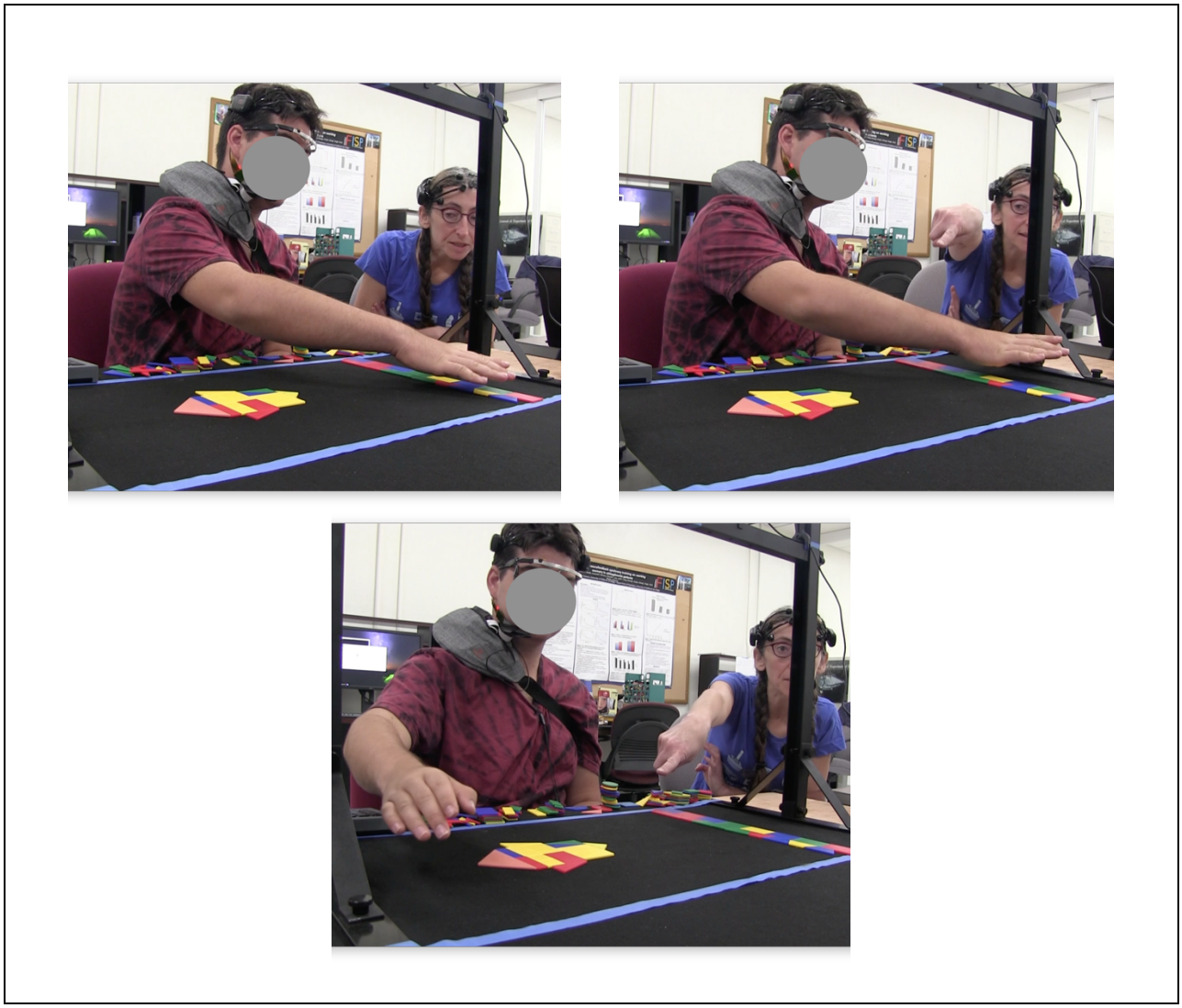

Figure 4.5. Exploring the space for a health bar. The image on the upper left shows Matthew simulating the moving ship by floating his hand over the partially-constructed ship on the left, as described in Excerpt 4.3, line 15 ("but I wanted this"). The image on the upper left shows me pointing to the other side of the animation field, as described in line 16. Notice my reach is well above Matthew's extended hand. The bottom image shows Matthew's response, floating his hand to the area I pointed out; here, we are both oriented to his hand on the screen in front of us (line 17).

However, despite the fact that we collaborate to imagine a move together through observing these targeted body movements, we are not equals. When I gesture in line 14, I do not use my hand as a stand-in for the health bar, but instead I quickly point and retract my hand, as I did in line 12. Only in line 15 do I extend my pointing gesture across Matthew (see Figure 4.5); even here, my motion curves above his hand to avoid directly disturbing his gesture. While the differences between Matthew's miming gestures and my pointing gestures are subtle, they reinforce Matthew's position as the one who populates this animation and mine as one who calls Matthew's attention to possibilities by intervening in, but not physically occupying, the animation field. Instead, my suggestions are taken up by Matthew when he tries them out by moving his own hand under the camera. His movements, whether his own or when suggested by me, allow us to observe together what 'it would look like' (as shown in the bottom image in Figure 4.5). This, in turn, allows Matthew to physically demonstrate the cramped conditions justifying his rejection of the health bar. In this way, Matthew accommodates, manages and resists my enthusiasm for an idea he does not want to implement while including me as an interested party in exploring the addition of a health bar. While, as mentioned already, this indicates that my comments do not dictate Matthew's creative choices, it further suggests that Matthew is thereby demonstrating his accountability to my role as a personally and structurally "interested party." In other words, Matthew accommodates my interest in line with his accountability to the experimental setting.

My engaged responses, in turn, are situated within, and work to articulate, a setting in which Matthew is meant to take on the role of author in the creation of an animation. My lively responses, including exclamations, like "Cool!" and "I love that idea," are far more complex than simple directives to overshadow or undermine Matthew's own creativity. As seen in these

episodes, they alert us to the efforts taken by both Matthew and myself to make space for each other's participation, and articulate each other's role in respect to both the animation activity and the setting of experiment.

4.5 Discussion

In examining the scenes between Matthew and myself, I looked at the effects of my—as researcher—breaches of neutrality in my interaction with an experimental subject. In keeping with the study's protocol and the conventions for human experimentation in the laboratory, I questioned whether such utterances obscured or unduly influenced the creative process we were interested in observing. This did not seem to be the case. Affirmative utterances did not have a prescriptive influence on the activity of the animator, as we saw in the case of Michael's resistance to adding an animated health bar to his Galaga-inspired animation.

This is not to say, however, that such utterances were inconsequential in these interactions. By considering how such utterances were called forth, for example, I noticed that, rather than act as directives, these affirmations more often served to coordinate our roles in respect to the animation and the experimental setting. In the scene depicted in Excerpt 4.2, when Matthew first makes eye contact and tells me he will be using the small triangle tangrams "for the missiles," my genuinely effusive response reciprocates his enthusiasm and acknowledges his engagement with the experimental task. When I verbally recognized that transforming a triangle into a missile is "cool," Matthew's process of animation was already in play through the medium of my recognition. Just as Beth Ferholt (2009) has suggested that adults entering children's "playworlds" can mutually ratify and materialize these worlds for *both* adults and children, my

recognition of Matthew's plans and visions also drew me into the increasingly lively world of the materializing animation.

Indeed, instead of asking whether breaching neutrality can ever be a legitimate move for the experimenter, one can just as productively ask whether, even in the absence of overzealous enthusiasms, an experimenter working with a human subject can ever be "neutral." Even if the researcher was able to strictly control their script, inflections, and gestures to be identical with every experimental subject, the experimental subjects—in their irreducibly situated singularity—would receive, interpret and understand these identical communications differently. Thus, when speech act theory (Austin 1962) pervades the meaning-space of the experimental laboratory, this complicates our inquiry into how "the" experiment is done, since the experiment is not one context but multiple, emerging from the dynamic encounter of the researcher, the experimental subject, and who/whatever else appears in the setting as relevant (Cicourel 1987, Goodwin 2000, Maynard 2006).

Relating to previous EM and ethnographic work on the impact of experimenter-subject interactions on evaluations of subjects' capacities, we asked: What is the context for subjects' participation in an experiment, and how does this inflect how their performance is to be interpreted? Although my own performance as experimenter did not adhere to the usual conventions of neutrality, an EM analysis of my interaction with an experimental subject was able to follow how my performance was woven into the fabric of the animation task and the "abstract competency" (in our case, the creative process) this task was meant to elucidate. Rather than undermining our attempts to capture something about the creative process, then, the affectively charged interaction between myself and Matthew helped us notice how creative

action engaged the emergent multiple and nested contexts of the experimental task and the setting of experiment together.

By staging an experimental inquiry based on interaction (instead of an individual capacity for creativity), it was then possible to begin to notice the distributed quality of the creative process. In my session with Matthew, for example, we saw how transformation—arguably foundational for any creative action—began not with the animation software but with our shared attention to the tangrams as materials for transformation. Matthew's suggestion, and my reciprocal recognition, of turning the triangles into missiles began the animation process as a relational achievement situated in shared material concerns.

As Matthew began using the animation software, we could see how the animation field of the table top could, when called upon, take on the role of a space for collaboration enacted through our bodily gestures and positions. The monitor we both watched to see "what [the animation] would look like" further organized our collaborative activity by positioning us spatially and attentively as focused spectators, with the "happening" in the animation field as a matter to be settled through a shared looking and a moving, de-emphasizing a distinct subject position toward a dynamic shared subjectivity in respect to action. At the same time, our collaborative activities were directed not only to what should happen in the animation, but also managed our relational roles in respect to the multiple extant "contexts" that were produced through our accountable activities. On the one hand, this entailed managing our gestures to maintain Matthew's authorship of the animation, and my authority in the ongoing experimental that gave sense to, and was made sensical by, the making of a stop-motion animation.

Yet some aspects of the creative process, especially in relation to previous research on creativity and autism, remain elusive in the described interactions with Matthew. Primarily, these

are marked by the question of "appropriateness," a consideration relevant in the majority of creativity studies that circumscribes what can be counted as a creative response and what is out of bounds. As discussed at length in Chapter 3, these standards for assessing creativity are generally withheld from subjects, and are made explicit only for researchers (if at all). As a result, experimental subjects' "creativity" is a partially a measure of how well they are able to guess at these standards in situations where (because of researchers' mandates for neutrality) "contexts" are opaque and interactional exploration is not possible. While the problem in these cases seems straightforward, the larger question remains. Are there standards for evaluating or even identifying creativity? And if so, what (and whose) are they? From a lived perspective, the question becomes, how do standards for creativity become known, articulated, or meaningful in the course of action and interaction? This is the question I address in the following chapter. There, staying within the doing of this experiment on autism and creativity, I turn to moments of interaction with the two other experimental subjects to consider how the standards for creativity emerge within the creative process.

Chapter 5

Animating Creativity

5.1 Introduction

The previous chapter introduced our exploratory pilot study on autistic creativity, where I, as researcher, worked in interaction with adolescents who were invited to create stop-motion animation in the lab. In that chapter, I attended to moments where I seemingly violated the mandate for neutrality of the experimenter. This allowed for noticing how the creative process was distributed across my and an experimental subject's gestures, spatial positions, conversation, the material environment and competing contexts of our activity. In this chapter, I stay with the experiment to further inquire about the creative process. Can an ethnomethodologically (EM)-inflected attention to creative interactions reveal aspects of the creative process that are opaque in the psychometric tests widely used to evaluate the creativity of isolated participants[1] and their products? And in particular, can attending to our interactions offer insights into how the *standards* for creativity—an understanding of and commitment to what can be considered "appropriately" creative—are known or shared in this experimental setting? We will consider this question as we turn to the second participant in our animation experiment, a typically developing 12-year-old girl whom we shall call Hannah.

[1] Although I refer to "isolated participants," in fact participants are not isolated in psychometric tests like the TTCT or the Fantastical Creatures task, since these tests are usually administered by a co-present researcher (see Chapter 3). Participants are, however, isolated in the sense that their interaction with the researcher is analytically relevant only in the narrow terms of subjects' responses to the tests they are presented with. Other aspects of the interaction are not considered relevant for analysis, and typically are bracketed out of what count as experimental results. See also Maynard and Turowetz 2017.

5.2 Acknowledgment and Elaboration

As with Matthew, Hannah visited the lab two times. During her first visit, I taught her how to use the animation station and supported her as she made a first animation using the tangrams provided. At the end of this first session, I told Hannah that she would have the opportunity to make another, longer animation that "tells a story" and that might be shown to other people her age in a later experiment. I asked her to consider what sort of second animation she might want to make during the week-long break between the first session and the second, with the understanding that she could choose whatever animation materials she wanted for her second animation. Hannah immediately responded that she might choose "drawing things, because [she] really like[s] drawing."

In Hannah's second session, we provided her with a whiteboard and a bag of dry-erase markers of different colors and thickness, which she began trying out on the whiteboard as the RAs fitted her with the EEG cap[2]. When the RAs began working on launching the animation software and coordinating the streams of data, Hannah and I waited, sitting adjacent to each other at the animation table. Hannah was reticent to engage me, though she immediately started using the whiteboard and markers. We had not yet begun the animation process, but Hannah was already drawing a wolf, and using the dry erase markers to sketch circles and ovals with which she approximated the wolf's proportions. She erased portions of these with her fingers (even though a whiteboard eraser lay nearby) and replaced them with more detailed shapes: the wolf's head, its snout, its jaw. Hannah's formal strategy—using sketchy circles and ovals for proportion—suggested that Hannah was not a novice at drawing. The RAs were still working on

[2] See Chapter 4, Section 4.3 Methods.

getting the system running, and Hannah did not look up from her drawing at all. I watched her closely as she drew. After some time, I asked her about drawing, as the following excerpt describes.[3]

Excerpt 5.1

13. **PR:** ((*looking at H*)) You do a lot of drawing?=
14. **H:** ((*looking down at drawing*)) =Mhmm
15. **PR:** Is that your thing?=
16. **H:** =Yeah, kind of yeah. Drawing and reading
17. **PR:** Drawing and reading? That's pretty cool(.) And would you say that when
18. you draw, the thing that you draw most often is(.) uh(.) like a wolf?=
19. **H:** =Yeah, I usually only draw wolves
20. **PR:** Uh huh. When'd you start?(.) drawing wolves?
21. **H:** Umm ((*looks up, ahead*)), well kind of this year, like last year was
22. cats and the year before that it was dogs.

In Excerpt 5.1, Hannah reveals that she is serious about drawing, and identifies it as one of two activities that are her "thing." Her seriousness is indicated also by her self-reported long-term focus on a subject for her drawing practice: she spends "a year" learning, practicing and developing her drawings of one kind of animal. Whereas, of course, Hannah's self-description may be exaggerating or misrepresenting her drawing practice, her intense focus on the wolf she is drawing before, during and after this moment of interaction supports her description.

Hannah's approach to the animation task, therefore, differs in some way from that of Matthew, whom we met in Chapter 4. While Matthew's work was itself animated by his affinity for playing Galaga with his dad and video game graphics and narratives in general, Hannah's animation process is motivated not only by her love of wolves, but also by her ongoing engagement with drawing. Despite their differences, interacting with Hannah and Matthew had a

[3] Here, as in Chapter 4, I am the Participating Researcher (PR) and Hannah is (H) in the transcript. The transcript notations used are defined in Chapter 4, footnote 21.

number of similarities, not least the various ways that my affirmative statements marked our efforts toward coordination.

In the course of this exchange, I make one complementary comment of the type examined in the sessions with Matthew; this is in line 17, when I say, "That's pretty cool." In attending to what this utterance refers to in the interaction, notice that the phrase is not commenting on Hannah's current drawing activity but rather on the way she identifies herself. After the two turns in which I inquire about Hannah's drawing practice, and Hannah confirms and elaborates that "her thing" (line 15) is both "drawing and reading" (line 16), I repeat the phrase "drawing and reading" with a rising pitch, as a question. Immediately thereafter, I de-emphasize the questioning tone by adding, "That's pretty cool." While taken by itself, the phrase could indicate an act of evaluation (as has been described in teacher-student or test administration interactions, Andiliou and Murphy 2010), it seems to have a different valence when invoked here.

Here, looking closely at the role of praise and affirmation in the flow of situated interaction suggests that in cases like this, seemingly complementary statements ("That's pretty cool") can also act as *acknowledgements* of the child's own self-narration, where the researcher recognizes and affirms the value of what the participant shows or describes themselves to be doing. In our interaction, "that's pretty cool" seems to coordinate us around a *shared valuation* of the activities (drawing and reading) that Hannah describes as being important to her. Indeed, as I continue to ask questions about her drawing practice, Hannah not only provides a response but, uncharacteristically, breaks from her drawing activity (to look up), and elaborates, adding details about other animals she has drawn. Acknowledgement here, and as we saw in Chapter 4 when Matthew told me about Galaga, seems to invite the child's expressive elaboration that

follows.[4] Rather than *affirmations as problematic experimental confounds*, this exchange with Hannah shows *affirmations as interactional conditions that invite self-expression.*

Though our exchange in Hannah's session is much less lively than in Matthew's—with minimal conversation—following the interaction closely can reveal our enactment of contexts relevant to creative engagement. Indeed, tracking emerging contexts—which are multiple and dynamically nested—*requires* an ongoing attention to how these appear, change, or recede in the course of participants' sense-making during "an" experimental procedure.[5] Because what counts as creativity depends on a social context[6], implicitly or explicitly undergirding the evaluative requirements for "appropriateness," understanding contexts as multiply and dynamically enacted raises the question: how are social standards for creativity produced and how do they come to bear upon the potentially creative activities at play?

While this sort of processual analysis can cause trouble for traditional hypothesis testing (where a dependent variable is carefully altered in order to understand its effect on the independent variable measured by the test), it builds upon situated approaches, like those of cultural-historical activity theory (Vygotsky 2004). Vygotsky's understanding of children's creativity was as a socially situated process, not an intrinsic individual capacity; thus, he stressed the necessity of testing children in interaction, within their social-cultural environment. Indeed, he drew a clear distinction between these sorts of investigations and laboratory experiments, in which laboratory tests "experimentally induce a creative response in children…suited only to the

[4] A similar relationship between affirmation and elaboration is seen in the interaction with Matthew, p.177-178.
[5] This notion of contexts as actively produced and maintained by the accountable actions of participants is a feature both of the ethnomethodological literature, as described in Chapter 4, and of the "enactive" model of cognition, which posits that participants and their "environments" mutually determine each other (Varela, Thompson and Rosch 1991).
[6] "Creativity depends on a social context in at least two ways: ontologically, it is the consensus of a critical segment of society that defines what is or is not creative; empirically, the realization of creative ideas relies on the support of the social milieu" (Csikszentmihalyi 1990, 164).

goal for which they were developed, that is, to evoke creative writing in children in order to study it" (Vygotsky 2004, 66). Vygotsky's contention, however, is that the calling forth of creative activity by so-called "artificial" means misses the crucial connection between the activity and its sense in what Kurt Lewin (1943) later called the "life space" of the child.[7] For Vygotsky, then, little could be said about a child's capacity for creativity by analyzing their individual performance on a standardized test, but much could be gleaned about the creative process by looking at how a child's approach to a historically and culturally situated creative problem developed as a function of interacting with others.[8] The child's own situated interest and motivation in such a process could therefore not be underestimated: "The best stimulus of creativity in children is to organize their life so that it leads to a need and ability to create" (Vygotsky 2004, 66).

It is precisely the calling forth of "a need…to create"—the emergence of mutually recognized values for creativity in relation to "interaction with others"—that is investigated here. Particularly important is the emergence of "others" along with the "contexts" that render these interactants as participants in creative action. Therefore, in what follows, I attend not only to the exchange between Hannah and myself, but also to the *others* that emerge from the seemingly non-agential participants in these scenes: the drawings and animation materials.[9]

So far, in Matthew's and Hannah's sessions, as we attended to my phrases of affirmation, we found that these served different roles in the flow of interaction, serving to coordinate us around shared spaces for action, to animate imagined moves, and to negotiate our roles in respect

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[7] A life space referred not to the "environment" or "context" as a pre-existing setting with stable meanings and demands, but rather the "psychological environment" as it exists for the person in question (Lewin 1943). For a discussion of the relationship between Lewin's notion of life space and Brunswick's notion of an experiment's "ecological validity," see Cole, Hood and McDermott 1997, 49-56.

[8] As formalized in his Zone of Proximal Development (Vygotsky 1978).

[9] Referring to emergent interactants as animate "others" is meant invoke the discussion of mirror neurons in Chapter 2.

to authorship. Excerpt 5.1, above, shows how the phrase "That's pretty cool" acts as an acknowledgement and valuation of Hannah's self-narration. In the following, Excerpt 5.2 (divided into four sections, a-d) examines how other kinds of acknowledgement animated shared standards for, and commitments towards, creativity. Here, we shall see that it is not only verbal affirmations from the researcher to the subject that are at issue, nor is it just the situated acts of *mutual* affirmation between Hannah and myself that articulate shared standards for what counts as creative. Following the scene below demonstrates how the very "stakeholders" or participants in the creative process themselves emerge in lively interaction toward a shared recognition of creative action. Thus, in the following scene, through mutual and effortful moment-to-moment recognition, a fluctuating "we" (that includes Hannah and myself, but also the drawn scenarios and animation materials that emerge as interlocutors) produce and articulate intersubjective values for creativity, grounded in our shared, ongoing animation activity.

5.3 Calling (Forth) Creativity

In the next excerpt, Hannah has begun the animation process, and is drawing each frame on the whiteboard before taking the photo, erasing parts of the drawing with the white board eraser or her fingers, and drawing the next frame. As with Matthew, her gaze is largely directed downwards, at her drawing. Occasionally—especially just before and after capturing a frame by taking a photograph—she looks up at the monitor, to see what the frame looks like on screen. The room is otherwise quiet. I sit to Hannah's left, with my gaze largely directed ahead, at her activity on the monitor screen, with occasional glances down at the whiteboard where Hannah draws and erases. The Bluetooth controller lays to Hannah's right. Unlike the sessions with Matthew or the third experimental subject, Edgar, there is very little overt interaction between

us: in the 10 minutes preceding this excerpt, Hannah has been steadily working on her animation and we have not exchanged a single word or made eye contact.

Hannah is drawing a large wolf in the lower center of the whiteboard and a smaller rabbit to its right. The whiteboard eraser lays in the middle of the whiteboard, above the wolf. As Hannah finishes the drawing, her right hand moves to the Bluetooth controller and she looks up at the monitor. At the start of Excerpt 5.2a, she examines what the frame looks like on the screen for a few seconds and then presses the capture button on the controller to take a photo. Still keeping her gaze on the screen, she immediately then pushes the playback button on the controller to watch the whole animation from the first frame to the most recent one. I have been following Hannah's activity on the screen.

Excerpt 5.2 (a)

1. **H:** ((*H takes photo*)) (3) ((*PR looks from eraser to H to screen*))
2. **PR:** **You you wanted that eraser/ in there?** ((*leans forward toward screen*))
3. **H:** /((*purses lips while looking at screen and*
4. *moves eraser out of frame. Finger-erases and redraws a line on the*
5. *rabbit, looks at screen*)) (3) ((*Presses button to erase last frame,*
6. *presses button to take photo, presses button for playback*))
7. **PR:** ((*at screen*)) (2.4) **For a second I thought** ((*at H*)) **you were going to**
8. **have /a flying eraser, like, hit him on the head or something** (.)
9. **H:** /((*looks at PR, smiles*))
10. **PR:** /**hhe-hhee-hhee I was like Oh wow! Hh-hh-hh-huh** ((*looks at screen*))
11. **H:** /((*still smiling, looks back at screen*))

Before line 1, I notice that the eraser is still on the whiteboard, and I wait for a few seconds to allow Hannah to notice this herself. When Hannah takes the photograph in line 1, I still don't immediately call her attention to the eraser in the frame but first look to Hannah to see if she herself notices this. I look at Hannah, back at the screen, and at Hannah again. When

Hannah looks back down at the whiteboard as if to begin drawing again, I lean forward and interrupt to tell her that the whiteboard eraser was in the last frame she captured (line 2).

In fact, before this exchange, in the previous frame Hannah photographed, the corner of the whiteboard eraser had also been slightly intruding (Fig. 5.1f). Although I had noticed this too, I had not commented on it, and allowed Hannah to continue animating in silence.

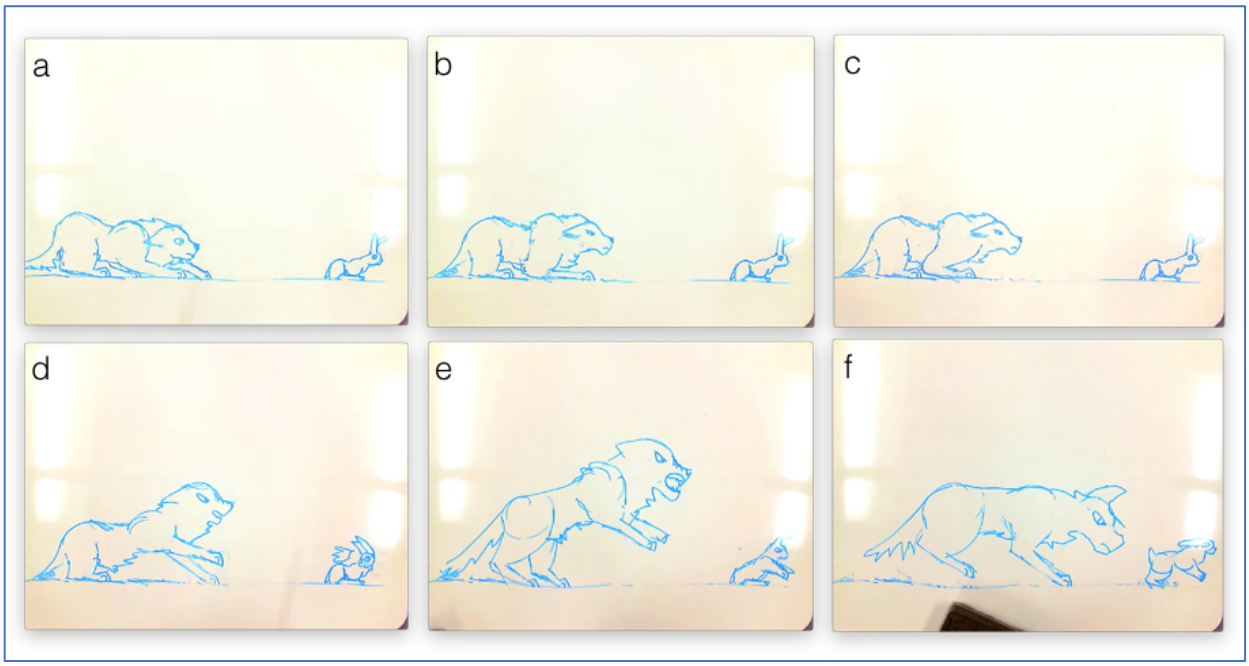


Figure 5.1. Frames 2 – 7 of Hannah's animation. In the seventh frame (f), the whiteboard eraser intrudes slightly from below, unremarked upon by Hannah or by me.

As the experimenter, I was skittish after being called out for being so enthusiastic in Matthew's sessions, and was torn between intervening and letting Hannah work on in silence, with the possibility she would be upset when she realizes her error after she has drawn and captured more frames. This time, with the eraser squarely in the middle of the photographed frame, and receiving no indication that Hannah herself has noticed this, I intervene cautiously, with a question: "You you wanted that eraser in there?" (line 2), which I follow by leaning forward toward the animation table and into Hannah's peripheral vision.

Hannah does not utter a reply, nor does she look at me, but instead looks up at the screen and purses (or tightens) her lips when she sees the eraser in the frame she just photographed. She moves the eraser out of the frame, looks down and alters a small portion of the drawing. Moving her right hand to the Bluetooth controller, she looks at the screen as she deletes the frame that has the offending whiteboard eraser in it, retakes the photo without the eraser (Fig. 5.2), and hits playback to see the animation thus far. I am still leaning forward.

Figure 5.2. The eighth frame of Hannah's animation, after she removed the whiteboard eraser and re-photographed the frame.

Perhaps to clarify why I brought the issue up as a question, or maybe to repair any negative feelings Hannah might be experiencing as a result of being "called out" for a "mistake," I look at Hannah while saying, "For a second I thought you were going to have a flying eraser, like, hit him on the head or something," and, laughing, continue, "I was like Oh wow!" (lines 6-8). In line 9, Hannah acknowledges this by momentarily meeting my gaze and smiling, then looking back at the screen while continuing to smile. Seemingly, with this, the exchange comes to an end, and Hannah (lines 10 – 15, below) resumes her previous activity of silently drawing to prepare the next frame of the animation.

Although the subsequent sequence of action (Excerpt 5.5b, lines 10-19) unfolds in silence (notice the number of seconds that pass in parentheses), it features a complex interaction between Hannah and myself, as mediated by the animation Hannah is intently constructing. After our brief exchange about the whiteboard eraser, Hannah does not speak to nor orient toward me at all.

Excerpt 5.2 (b)

```
10. H:      ((shifts in chair, erases rabbit with eraser, resumes drawing/)) (33)
11. PR:     ((/leans back in chair, still following along by watching screen))
12. H:      ((shifts in chair, presses playback and watches screen)) (6) ((picks
13.         up eraser, erases wolf's head and redraws)) (17) ((presses playback
14.         and watches screen, resumes drawing)) (60) ((presses playback /and
15.         watches screen. The eraser is in the frame, above wolf's head. H
16.         resumes drawing)) (30) ((looks at screen, takes photo, playback))
17. PR:     /((looking at screen, raises eyebrows and smiles slightly, looks back
18.         and forth from screen to H to her hands drawing, quick looks))
19. PR:     ((looks back to screen, watches playback)) (.) ((tilts head))
```

In line 10, she turns to focus on drawing the next animation frame, and I (line 11, as notated by the forward slash "/") respond to this shift by leaning back in my chair and watching the screen to follow Hannah's activity there. In lines 12-16, Hannah alternates between long stretches of drawing-erasing-drawing and playing back the animation to see how the current drawing fits with the previous frames. When she erases parts of the drawing, she sometimes uses her fingers and sometimes uses the whiteboard eraser. The whiteboard eraser, therefore, is in use and, by line 14, is once again laying on the whiteboard above the newly re-drawn head of the wolf. Hannah continues her activity, checking out her work in the playback and resuming drawing, without moving the eraser out of the frame. On the screen, I see the playback (when Hannah initiates it) and can see the current frame as it is being drawn. After Hannah fails to

remove the eraser following the second playback (line 14), I begin to show some surprise (line 17), quickly shifting my gaze from the screen to Hannah's face to the whiteboard and back. The eraser is visible in the frame on screen, and it is unclear whether this is intentional on Hannah's part, or whether she once again simply does not notice its presence.

Because I am unsure, but also because I am trying to not "interfere" with Hannah's creative process, I do not inquire about it or alert Hannah, but continue to follow Hannah's activity closely, looking from her to the screen and back. After another 30 seconds of silent drawing, Hannah looks at the screen, plays back the animation, and finally takes the photo of the frame she has been working on (line 16). The frame includes the whiteboard eraser (Fig 5.3). I make no comment to Hannah, but show a heightened state of alertness, as I quickly look from the screen to Hannah drawing and back again (lines 17-18). Was Hannah's inclusion of the eraser in the frame intentional? Does it warrant another comment from me to draw her attention to it? I do not inquire but tilt my head as I watch the screen closely while Hannah continues animating (line 19).

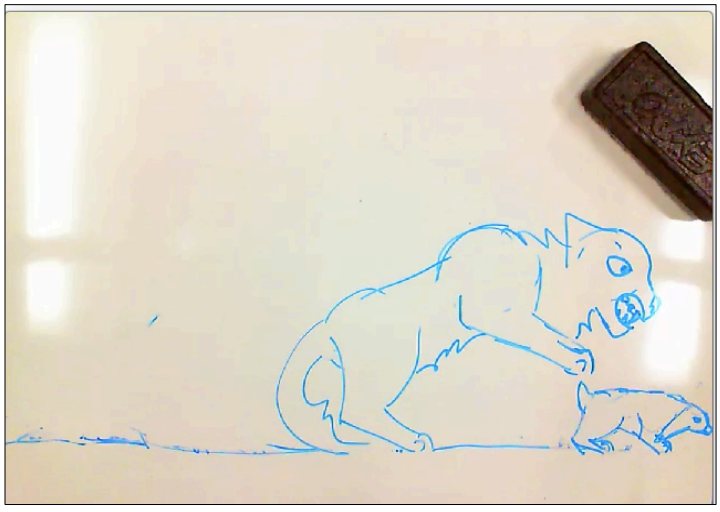


Figure 5.3. The ninth frame of Hannah's animation, in which it is unclear to me whether the eraser's inclusion is intentional or inadvertent.

This tension between the inclusion of the eraser as *intentional* or *unintended* is a crucial hinge to my own understanding of Hannah's activities as *creative* or *improper* (and thus potentially needing correction). In this, while I am seemingly not imposing a standard expectation of what counts as "more" or "less" creative, my alertness and increasingly active scanning of the scene enact a standard that circumscribes what is a valid move in the animation activity (as understood though its being demonstrated as intentional), and a move that is (by its appearance as error or omission) to be considered out of bounds and pointed out. The silence that characterizes my ongoing interaction with Hannah shapes this uncertainty: lacking recourse to verbal explanations—and being reluctant to draw these out in the interests of not interrupting her authorial flow—I must look entirely to what Hannah is doing to find clues.

What happens next is presented in Excerpt 5.2c. Throughout this sequence, Hannah again does not direct any action toward me, but instead continues to divide her focus between drawing, erasing, and playing back the existing animation. I follow Hannah's activity mainly by watching it unfold on the screen in front of us.

Excerpt 5.2 (c)

20. **H:** ((*erases*)) (8) ((*draws*)) (4)/ (52)
21. **PR:** ((*/tilts head left* (2), *then right* (5) *cranes neck toward screen* (2)
22. *leans back with head tilted right*))
23. **H:** ((*moves eraser out of frame looks at screen and playback* (4) *smiles*
24. *at screen and moves eraser above wolf's head* (4) *draws* (6) *looks up*
25. *at screen and playback* (5) /))
26. **PR:** / °**hh**° ((*smiles, still looking at screen with head tilted*))

As Hannah erases part of the wolf and draws new lines to render it in a different pose (line 20), I indirectly involve myself in Hannah's activity through orienting and reorienting my body toward the screen (line 21). This sequence of active movement ends with me leaning back

in my chair with my "head tilted," but my gaze is still directed unwaveringly at the screen. I maintain this attentive position for the silent duration of Hannah's drawing activity in line 20, lasting almost a full minute. I continue in the same position as Hannah (in line 23) moves the eraser off the whiteboard, looks up to check what the new drawing looks like on the screen, and plays back the whole animation thus far. I still do not understand whether the eraser in the last frame was intentional or an error.

Hannah smiles at the screen (line 24) as she then carefully *places the eraser back* on the whiteboard, just above the wolf. Her action of placing the eraser back in the same spot is the hint I have been waiting for. Without moving the eraser, she adds a few details to her drawing, looks at the screen and plays back the animation thus far, ending with the current frame that has not yet been photographed (lines 24-25). It is as I watch this playback alongside Hannah that I come out of my alert state and laugh. Without opening my mouth, my laugh is conveyed as a smile and a burst of air expelled through my nostrils. This quiet non-verbal sound (line 26: "hh") is the only utterance in Excerpt 5.2(c). Despite its simplicity and modest volume, my resounding "hh" demonstrates my acquired understanding of what Hannah has just done as we watch it unfold on the playback together. The eraser, as it starts to become apparent through her careful placing of it above the wolf in line 24, and confirmed in seeing the eraser's movement animated in the playback, is not an oversight but seems to be an intentional act of composition on Hannah's part.

What do we make of this? Recall the earlier moment from Excerpt 5.2(a), when I alerted Hannah to the fact that she'd inadvertently included the eraser in a frame. At that time, I had jokingly invoked the possibility of the three-dimensional eraser becoming part of the two-dimensional animation by hitting the drawn wolf on the head. Does Hannah's taking up the image I invoked imply undue influence on Hannah's creative process, justifying precisely the

danger that neutral researcher-subject interactions are meant to avoid? Or, does a close look at *how* this idea is taken up and realized by Hannah, and how it shapes the ongoing interaction between us, allow for insight into a mutually acknowledged (and distinctly Vygotskian) creative process unfolding here?

When Hannah takes up the idea of the eraser as material to work with, this decision is not the completion of an action, but rather a beginning: Hannah *animates* this idea by bringing it into the lived world of the scene she is creating. In other words, each subsequent frame that Hannah creates is an opportunity to make sense of this premise in the pictorial world constituted by the wolf and rabbit she has already drawn. Looking at the sequence of frames that follow (Fig. 5.4), we see how the event of the 3-D eraser intruding on the 2-D characters is elaborated through these characters and their situation. The eraser falls upon the wolf as the wolf attacks the rabbit in Fig. 5.4(b-d). In Fig. 5.4(e-f), the wolf's response to this unexpected event is depicted: the wolf turns its head to follow the direction of the eraser's movement. The wolf's confusion is rendered as question marks floating above its head (Fig. 5.4e). The next frame (Fig. 5.4f) continues this event, but this time the wolf's confusion gains a level of abstraction, as it is depicted imagining itself being confused about the object that has struck it. The situation resolves in such a way as to emphasize the physical reality of the eraser through its mass and momentum in the depicted world, as the eraser is shown falling across the wolf's back and (perhaps nudged by the wolf in Fig. 5.4g) onto the ground behind it (Fig. 5.4h and 5.4i).

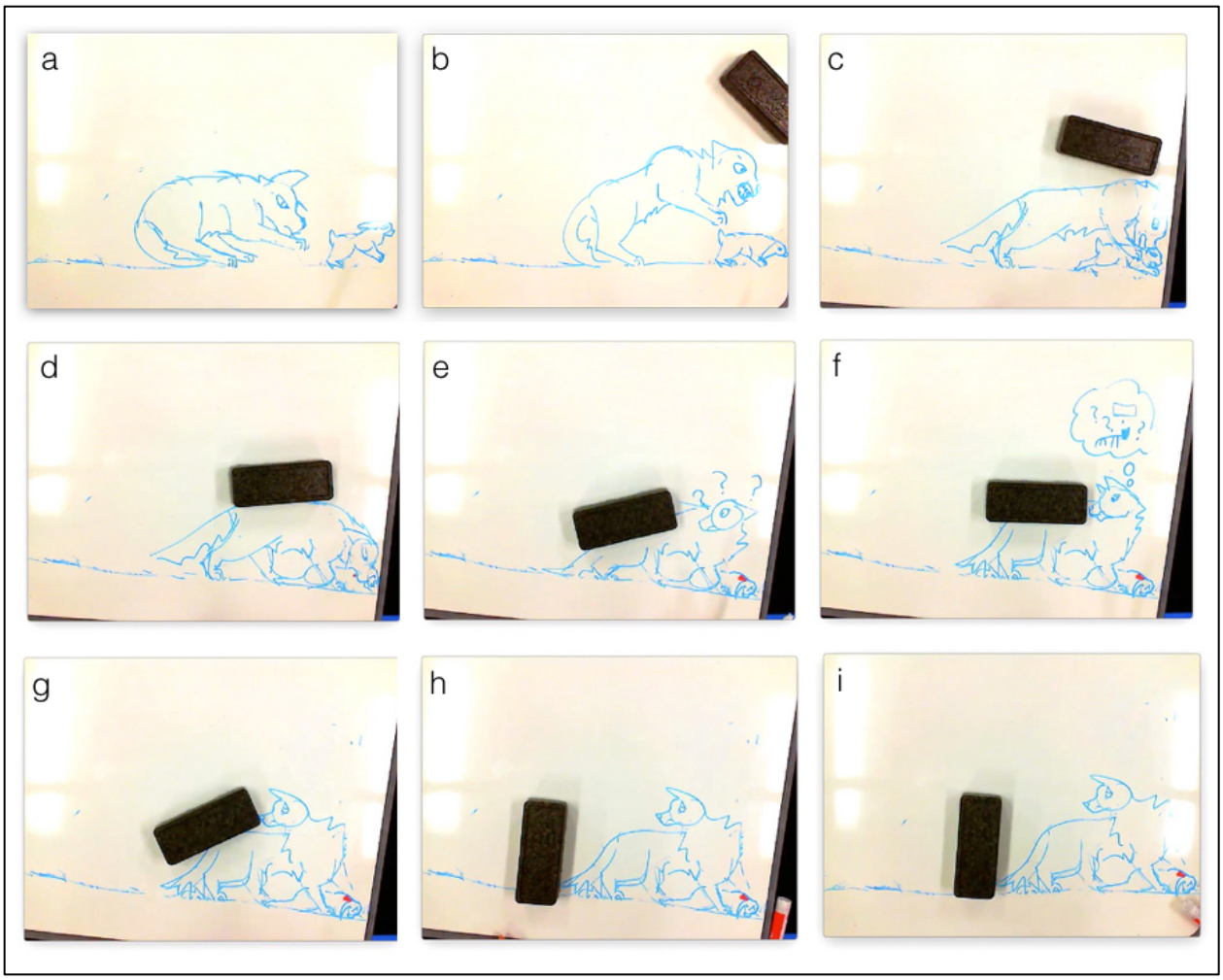


Figure 5.4. Frames 8 to 16 of Hannah's animation. Frame (a) was re-taken after deleting a version of the same image with a whiteboard eraser intruding in the frame (same image in Fig.5.2). Frame (b) emerges from the activity presented in Excerpt 5.2(a) and 5.2(b), in which I was unsure whether the intrusion of the eraser in the frame was intentional.

Hannah's animation of the eraser, while certainly influenced by my comment, shows more than a "simple" instance of influence. Just as my own comment arose from my ongoing, situated interaction with Hannah and accountability to my role as an engaged interlocutor who helps her make the animation she wants to make, Hannah's use of that comment was similarly oriented to her accountability to the drawn situation at hand. In other words, the eraser's action in Hannah's scene was elaborated by her in relation to both my suggestion and to the lively situation that was progressing in her animation.

Looking back at our interaction in the next moment, following my realization that Hannah has decided to use the eraser in the animation, we see that my physical movement comes into synchrony with Hannah's own movements (Excerpt 5.2d).

Excerpt 5.2 (d)[10]

```
27. H:      ((shifts in chair and sweeps hair out of eyes (2) draws rabbit in
28.         lower left corner (2) / moves whiteboard slightly // draws)) (28)
29. PR:     (( / glances at H's hands then back to screen // at hands and back
30.         (6) tilts head to left, slight smile (13) cranes neck tilts head to
31.         right))
32. H:      ((brings head up, erases with finger, back to drawing /)) (5)
33. PR:     (( / brings head up ))
34. H:      ((tilts head to left / (2) looks at screen (1) draws a line (2) looks
35.         at screen and playback (4) // ))
36. PR:     (( / tilts head to left still looking at screen // straightens head))
37: H:      ((erases part of wolf with her finger and redraws it (7) looks at
38.         screen / (1) takes photo //))
39. PR:     (( / tilts head to the right while looking at screen // glances at
40.         Hannah's hand and back to the screen))
```

In Excerpt 5.2(d), which begins just after my laugh ending the previous excerpt, Hannah shifts her position and resumes drawing, her gaze directed at her drawing (lines 27-28). I am once again quite active in following Hannah's work, looking back and forth from Hannah's hands to the screen and reorienting my position to get a better look at the activity on screen (lines 29-31). I retain a slight smile in lines 30-31. Although my smile indicates my understanding of, and perhaps sense of complicity in the creative choice Hannah seems to be making by incorporating the eraser, Hannah has not yet taken the photo to capture the eraser in the frame she has been working on throughout Excerpts 5.2b and 5.2c.

[10] Here, while forward slashes indicate temporal matching between actions, double slashes (//) are used to avoid confusion about which actions for one participant are coordinated with which actions of the other participant.

Hannah, in line 32, raises her head to look at the drawing on screen. I shadow this movement, bringing my own head up from its tilted position (line 33). Hannah then tilts her own head to the left as she looks at the screen (line 34) and I again echo Hannah's movement in tilting my head in a similar direction (line 36). Hannah's gaze as she looks at the image on screen is engaged, intense even. Hannah's tilting of her head while she looks at the screen is similar to what we saw in my active engagement as I looked back and forth from the screen to Hannah's hands, and repositioned my head as I intently observed the scene on screen. In both cases, we move like viewers at a museum, actively repositioning our bodies as called forth by the art object's invitation to be seen in a particular way. In this way, the object *in the museum* and the animation *on screen* perhaps function less as objects (to be manipulated) and more like others (inviting, even insisting upon, a response).[11]

The active gazing that we both do in respect to the rabbit-wolf-and-eraser scene displayed on screen articulates the agency and liveliness of that scene. Hannah's, especially, is a gaze that actively inquires of the depicted scene what it wants to be, and she responds to this (in line 34) by augmenting the drawing. After drawing, in lines 35-36, Hannah plays back the whole animation sequence, ending with the newly adjusted scene, and I watch this and straighten my head upon the playback's completion. Hannah again responds to what the scene "wants to be" by making one final adjustment in her drawing. She then quickly looks up at the screen (lines 37-38), and I follow Hannah's attention by tilting my head to the right immediately after (line 39). In lines 38-40, Hannah finally takes the photo of the frame she has been drawing for the previous few minutes, and I look to Hannah's hands before turning back to the screen.

[11] My use of "objects" as distinct from "others" is meant to recall the discussion of mirror neurons in Chapter 2, in respect to the question of whether these categories are natural/biological kinds, or if they emerge in interaction.

Following this sequence of interaction visually, as I watched and re-watched the video recording, underscores the dance-like coordination of these subtle movements. This improvised choreography of tilting and raising heads, notated in the transcripts above, challenges the notion that Hannah's activity is subordinate to mine. The temporal coupling of our moving heads suggests, on the contrary, that it is Hannah who leads the dance, and I interestedly follow. At the same time, I am not passive in the interaction. Though Hannah leads, I actively—through movement—attend to how the animation unfolds. I attempt to orient to the animation not only as a pictorial image, but as an available extension of Hannah's own perception and experience, and I do this through my embodied involvement with Hannah's body as it organizes and responds to what she is making.

Yet this dance between us would be unintelligible if its description was not grounded in the screen image, and the screen itself, that organizes our looking. The image—a provisional scene of a wolf, a rabbit, and an intruding projectile eraser—makes its own demands on Hannah, and through Hannah, on me. In this sense, the animation is not a *passive* shared space of attention, a container waiting to be occupied by Hannah and myself, but is itself an interactant in this distributed process of creation, not least of all distributed through the facing screen in organizing our directional looking. This agential role is not reducible to the animation as an object with stable affordances (Gibson 2014 [1979]; Norman 1999) that draws our relational engagement; on the contrary, the potential affordances of the image is what Hannah is focused on discovering and co-articulating when she gazes at the screen and augments the drawing. At the same time, understanding the screen image as agential makes no claim toward reconfiguring the ontological status of the animation to be on par with that of the two human beings engaging with it. Instead, paying attention to our situated interaction reveals the specificity and

contingency of relations through which the animation "wants" to move a certain way, through which Hannah's responsiveness to this desire grounds her role as an author or artist, and through which I, in my attentiveness to Hannah and the demands of the experiment, participate as a particular kind of interlocutor and active spectator, an experimenter.

5.4 Response-able to Images

The inquiry in this chapter is directed to both the creative process and to the experiment through which a creative process can be interrogated. When Hannah looked at the screen to see what the wolf-rabbit-eraser composition will look like, she did so as the one accountable to the image. She was the one who could, based on what she saw, change, augment or edit its composition. Her gaze did not have the quality of a confirmational glance, in which she checked that the actual appearance matched her intention as an integral *fait accompli*. Instead, her gaze was intense: it inquired rather than confirmed, and therefore oriented her as responsive to what the image "wanted." The image was agential, therefore, not universally or ontologically but in its situated relation with Hannah.[12] The standards for creativity—for what makes any given move better than another in Hannah's composition—were distributed across Hannah's inquiring process, grounded in the scene as it was and toward what it may want to become. That said, Hannah's moves had more to do with my presence than her bodily orientation and silence let on. The other two experimental subjects tended to verbally narrate their plans and ideas to me;[13] with Hannah, however, our interaction was mediated almost completely through the drawing she

[12] The image's agential qualities cannot be disengaged from its socially situated "thing-like" or manipulable qualities (Alač 2016; Alač, Movellan and Tanaka 2011), nor can "aesthetic experience" be considered apart from from its dynamic social, interactional and contextual enactment (Brincker 2015).

[13] For example, in one moment in my second session with Edgar, who we will meet in a moment, he made something happen in the animation, watched the playback and asked me, "You see what I did there?"

created, using the interface of the monitor screen and the suggestion of the whiteboard eraser to acknowledge my presence, spectatorship and participation.

In turn, I played a part in acknowledging and facilitating Hannah's authorial orientation to the animation through my active attention to both the image being made and to Hannah's activity in the making of that image. I, however, was responsive and accountable ("response-able") to the experimental situation itself, rather than to the image Hannah scrutinized on screen. Thus, I worked to share standards for creativity with Hannah by making her intentions a matter of primary concern. Moreover, I demonstrated this to Hannah by laughing and smiling (in a kind of "I get what you're doing" gesture) that was similar to the valence of my more explicit affirmations ("That's pretty cool") in other moments. This orientation conditioned my interest in Hannah's work, but also conditioned my caution in performing my role as an "engaged participant" while actively maintaining the creative sovereignty of the animator.

5.5 Second-person neuroscience

Analyzing scenes from the moment-to-moment doing of Hannah and Matthew's animation sessions has thus far allowed for considering the relationship of the experimenter and experimental subject in respect to the ways that the creative process was dynamically distributed across these participants and the animation materials (tangrams, whiteboard, eraser, Animation Station, and screen) they worked through. But what of the experimental setting itself? This question—how the doing of the experimental setting relates to the creative process, and how the creative process is produced in a laboratory experiment—is central to the often-drawn distinction between the controlled laboratory experiment and "naturally occurring," "ecologically valid" processes that take place "in the wild" (Brunswick 1956, Hutchins 1995). This distinction

between the real and artificial has troubled experimental science alongside the rise of experimental objectivity.[14] Since the 1990s, the rise of "4E" cognitive models in which cognition is irreducibly *embodied*, *embedded* (or situated), *extended* (or distributed), and *enacted* has brought the question of ecological validity to the forefront again, since pursuing these 4E models empirically required grappling with existing cognitive neuroscience models, methods, and technologies tuned toward monitoring episodic activity in relatively immobile individuals (Froese 2018, Parada 2018).

Among the senior scientists who were the co-PIs of our study on autism and creativity, this tension between the artificiality of the lab and "ecological" cognitive phenomena was a matter of practical concern, guiding their efforts towards integrating the two. Neurologist Dr. Viirre did clinical work with autistic patients, and wanted to explore ways that autistic children used their bodies in creative work. Dr. Pineda, whose lab hosted our study and who was most involved in the experiment's design and analysis, was interested in exploring how processual, embodied, and experiential phenomena could be evoked and analyzed through the methods and technologies of cognitive neuroscience (Pineda 2018). Similarly oriented, co-PI Dr. Tzyy Ping Jung's lab had previously developed Mobile Brain/Body Imaging (MoBI): wireless EEG and physiological tracking systems for monitoring brain and behavior activity in people who could move their bodies relatively freely (e.g., Makeig et al. 2009), a major step towards being able to consider the ways that cognitive phenomenon are embodied, and allowing for a greater range of bodily activities that could be studied. These developments and interests inflected the design of

[14] In other words, an inquiry in a controlled setting where causal relations could be drawn between distinct conditions and changes to the phenomenon of interest is understood to be in tension with the idea that, outside of its naturally-occurring, everyday "ecology," this phenomenon is already a distorted version of itself. While this became widely understood as a problem in experimental science when Brunswick coined the term "ecological validity" (citation), the issue arises much earlier, as argued in Tiffany Watt Smith's analysis of Darwin's use of theatrical photographs and ad hoc staged actions in his struggle to study spontaneous facial expressions (citation).

our study, allowing us to use mobile EEG and gaze tracking to monitor two people in interaction (instead of relatively immobile individuals) around an open-ended task that required substantial bodily movement.

The gap between design and implementation is a steep one in laboratory practice, however, and research teams are always confronted with technologies, systems, software and conditions that resist the best laid plans. So it was in our attempt to record both brain activity and gaze—along with the animation activity—from two participants simultaneously. This meant the coordination of multiple large streams of data, some of which (from the animation camera) worked in a different time scale from the others (seconds vs milliseconds). Indeed, the plan for recording gaze activity from both researcher and subject was unsuccessful and abandoned after our first subject, Matthew—leaving only gaze data for the subject but not the experimenter. Similarly, although the EEG data was successfully recorded, linking the EEG to its precise occurrence in the animation activity caused troubles as well. While, in principle, these problems are not insurmountable in future trials, they pointed to the fact that these particular imaging technologies were designed and optimized to be used on an individual subject, not an interacting pair. Thus, despite calls from within the cognitive neuroscience and cognitive psychology communities for turning toward a "second person neuroscience" to explore humans in interaction (Shilbach et al. 2013), studying the neuroscience of interaction was still a difficult task, requiring work-arounds, hacks and much tinkering.

Perhaps a more skeptical STS approach would look at these measuring devices with suspicion, and question their ability to speak at all to the creative process we were interested in exploring. This was not my orientation; I find nothing illegitimate in studying the brain, which is, as Vygotsky pointed out, itself responsive to and hierarchically organized by the cultural-

historical life world (Vygotsky 1936). For me, concerned always with the conditions of the experiment in respect to the phenomena produced in its doing, a more salient question was about how these devices became a part of the lived world of the experiment, and how they themselves participated in the organization of our activity. Observable aspects of this influence emerged in moments when these measuring devices became mutually present to us as obstacles or discomforts in the experimental interaction. This is explored in the following section, where a pair of EEG caps reveal themselves as something that must be tolerated in the active maintenance of the experimental setting itself.

5.6 Performing the Experiment is a Joint Enterprise

Inasmuch as I worked to actively maintain the authorial role of each animator in the interests of enacting the experiment itself, it is also the case that each animator participated in this effortful work to produce and maintain the experiment. Thus, the activity of myself, the animator, and the animation emerged as accountable to the experiment that produced, and was produced by, their participation. This relationship is evoked poignantly in a brief moment of interaction with the third subject who participated in the study, a twelve-year-old typically developing boy who we shall call Edgar. Edgar was a lively pre-teen with a cheerful, outgoing manner. Like Matthew and Hannah, Edgar was invited to bring his own animation materials to his second session, and he chose Legos, which he had prepared and arranged neatly in multiple cases and boxes. He was eager to begin animating, using pre-assembled Lego characters in conjunction with Lego blocks to stage the scene, and frequently speaking in the voices of the characters or voicing the sounds (e.g., “zzzzz” for a character’s lightsaber) of their action as he worked.

Throughout all sessions, all participants (myself included) had expressed some discomfort resulting from the worn devices: two participants complained that the EEG cap made their head hurt, and all three youths showed their discomfort with the pouch they wore on their shoulder or back, that held a wireless receiver connected to their gaze tracking goggles. This discomfort could be seen on video in periodic fidgeting, shifting the pouch and/or goggles, and asking research assistants to make these adjustments for them. During his second session, Edgar expressed some discomfort with the biodata measuring instruments he was wearing, including tugging at the shoulder pack that help the battery and wireless transmitter for the goggles, and complaining that the EEG cap was uncomfortable. In the moment transcribed below (Excerpt 5.6), Edgar and I have just watched a playback of the sequence Edgar has put together so far, after about 18 minutes of animating and capturing frames. I take this moment to ask Edgar about the sequence we just watched on the screen in front of us.

Excerpt 5.7

1. **PR:** ((*torso turned slightly toward E but looking at screen*)) Whaddaya think?
2. **E:** I think it's pretty good
3. **PR:** °I think it's pretty good too°
4. **E:** (2) I'm not done yet though=
5. **PR:** =No no I know
6. **E:** Yoda has a part in this.=
7. **PR:** =Mmmm (.) so what happens now?
8. (3)
9. **E:** Auhh (2) °I don' know: I can't really think°/ this ki- ((*touches head*))
10. this headset thing is kinda
11. **PR:** /((*looks at E*)) The headset
12. is tight?
13. **E:** ((*torso oriented toward PR but not making eye contact*)) ah yeah
14. ((*smiles*))'ts kinda annoying now so I can't really think
15. **PR:** ((*sharply turns head 180 degrees to the left, towards RA off camera*))
16. **E:** uh >↑its fine though↑</ it's not bad it's just uh=

17. **PR:** /((*turns head back to look at E*)) =distracting?
18. **E:** yeah=
19. **PR:** =yeah I hear you
20. **E:** and it gives me a slight headache
21. **PR:** Yeah (.) yeah me too ((/ *leans forward*))
22. **E:** huaaa:= (2) / =I just have to see that again
23. ((*E and PR orient toward screen*))

After watching the playback, I ask Edgar what he thinks about the animation sequence thus far, and Edgar responds positively (line 2: "I think it's pretty good"), which I, under my breath, echo in agreement. Edgar begins a new turn by offering a clarifying statement to inform me that the animation is not finished (line 3: "I'm not done yet though"), and after my acknowledgement in line 4, adds to his statement to hint at what will happen next, "Yoda has a part in this." I acknowledge this and, taking it as an invitation to ask for elaboration, ask, "so what happens now?" It is at this juncture that Edgar discloses that he "can't think" because his EEG headset is uncomfortable (lines 9-10), causing me to immediately react. I quickly turn to look directly at Edgar and confirm what he is expressing by finishing his sentence: "The headset is tight?" Edgar repeats that the headset is making it hard for him to think. I immediately respond to this, leaning back in my chair and swiveling my face toward the RA, who is positioned off camera, on the far left side of the room. Presumably, I am about to ask the RA for help with this situation. EEG headsets can be partially adjusted, and trying this may or may not resolve Edgar's discomfort. Before I can say anything to the RA, however, Edgar says loudly "Its OK though," to interrupt my move toward initiating an intervention. To further support this, in line 16, Edgar minimizes the problem by adding "it's not bad it's just uh," and I, having turned my head back to look at Edgar, once again complete his sentence with a question: "distracting?" Edgar accepts this suggestion in line 18, and I demonstrates empathy toward this trouble "Yeah I hear you" (line 19), a sequence of talk that Edgar keeps in motion by adding, "and it gives me a slight

headache." (line 20). This time, in response to this much more specific articulation of Edgar's discomfort, I reply in sympathy, indicating for the first time that my headset is also inducing a slight headache. Yet, despite Edgar's discomfort, which I understand and, it turns out, share, I "lean forward" (line 21) back into the animation as Edgar states "I just have to see that again." This turn back to our ongoing activity is led by Edgar's tired exhalation "huaaa" as he once again initiates the playback of the existing sequences (line 22).

In the scene above, both Edgar and I acknowledge the troubles that make our current activity difficult to maintain, yet, we go on nevertheless. In this case, disambiguating the force of the uncompleted animation from the in-progress experiment is not easy to do. The uncomfortable EEG headsets are not, strictly speaking, required to do animation; they are necessary for this experiment to be a particular sort of experiment, namely one that considers biological activity to understand the creative process. Both Edgar and I make ourselves accountable to the demands of the experimental occasion. Edgar refuses the potential intervention, which could pause or stop this entangled activity of the experiment/animation session. I also maintain the session: upon learning that Edgar's headset is uncomfortable, I do not stand up or deal with it myself, constrained by my own EEG headset that, although meant to incorporate movement, can lose its connection through too much movement. We both work to maintain the balance between the fragile and uncomfortable measurement devices (the EEG cap, the goggles, the shoulder pack) and the emerging animation which calls forth our shared engagement.

In this episode with Edgar, myself and our uncomfortable headsets, a more subtle but crucial part of this creative process was oriented toward maintaining the experimental occasion itself. We might ask: why did we not take off the EEG headsets and continue the animation, if the headsets caused pain? Yet neither Edgar nor I moved to separate the animation activity from

the measuring devices we wore: they were conjoined in our doing. The animation task—even as an open-ended, engaging activity—was necessary for, and necessitated by, the experimental setting marked by the EEG headsets. In negotiating this dilemma, therefore, both Edgar and I made ourselves accountable to the animation and the experiment, as one entangled activity that we effortfully maintained. Thus, although the experimental setting may be designed, like Pannill Camp's disappearing proscenium, to recede so that a participant can become immersed in the experimental task, the scene with Edgar suggests that the frame does not simply vanish but must be actively *managed* in the moment-to-moment enactment of the experiment. We can visualize this through Hannah's animation, where experimenters are the wolf, chasing our phenomenon, only to be interrupted by extruding intrusions, projectiles, uncomfortable caps. Just as Hannah's wolf reflected on and incorporated this intrusion, so did we tolerate and *incorporate* our bodily response to the measuring devices.

In these animation sessions, apprehending the experimental context as it was maintained by its participants was possible in moments when the occasion and the creative process were at odds, as in the episode of the uncomfortable headsets, or in the mystery of the whiteboard eraser. The creative process, therefore, did not occur "within" an experimental setting but was at all times a distributed effort to maintain it. In this sense, the experimental occasion is not so much a matter of the experimental participant subjugated to the researcher, as is often the concern, but that all participants and all activity must traverse the opacity of standards and expectations to understand *how* to make their conduct and interaction accountable to a laboratory experiment. What is appropriate in this room? What are the rules for interaction, or the meaning of discomfort, or the materials to be engaged? This is perhaps a concrete creativity similar to the "concrete competency" that Maynard and Turowetz identified as required for participating in the

experimental task at all; riffing on (or against) Vygotsky, here a laboratory experiment can be thing that "organizes [a child's] life so that it leads to a need and ability to create." The play must go on, and indeed, so must the experiment.

5.7 Discussion

In the study of creativity, is the value of "appropriateness" appropriate?

Involving myself step by step in the animation process required accountable efforts to apprehend, articulate and enter the children's "playworlds" (Lindquist 1996, Ferholt 2009), even before these worlds were fully materialized. While I did some of this work silently, with my bodily movement (e.g., leaning forward towards the screen in response to a move made by the animator), much of it was also done through verbalized affirmations (e.g., "that's a great idea") that acknowledged the mutual visibility of the animators' work. This and the previous chapter questioned whether such utterances obscured or unduly influenced the creative process that the research team was interested in observing. This did not seem to be the case. Affirmative utterances did not have a direct influence on the activity of the animator, as seen in the case of Matthew's resistance to adding an animated health bar to his Galaga-inspired animation. This is not to say, however, that such utterances were inconsequential in these interactions.

Indeed, in respect to the question of whether a creative action should be evaluated by its "appropriateness" to a particular context, tracking my affirmative phrases within their situated interactions revealed moments in which standards for creativity were being locally invoked through mutually available appreciations. This is in contrast to canonical experiments on creativity as an individual attribute, where researchers applied standards of appropriateness in scoring participant performance but withheld these standards from participants. In our study, the "non-neutral" attitude of the researcher as she made sense of participants' creative choices

allowed for the discovery and apprehension of shared standards for what was creative (i.e., what was "cool" or worth pointing out) and made these moves also available for later analysis. This suggests that it may be impossible to completely eliminate the notion of "appropriateness"—in terms of a situated valuation of certain moves over others—as relevant to attributing or investigating creativity. Instead, the interactions described here show that such standards emerge locally, through "consensual validation" (Sternberg 2019) in situated interaction. While I am not the first to suggest that the evaluation of creativity is necessarily reflexive to the local scene of activity (Amabile 1996, Glaveneau 2011, Sternberg 2019), this study presents scenes that demonstrate this dynamic process grounded in particular interactions taking place in the cognitive neuroscience lab.

Implications for Studying Creativity

The local emergence of evaluative standards for creativity (especially when conceptualized through the heuristic of "appropriateness") has concrete implications for future research on creativity as well as future studies of autistic creativity. First off, as Sternberg (2019) points out, limiting the evaluation of creativity to its local emergence does not preclude the ability to study or evaluate creativity. He writes, "We can evaluate it just fine if we agree that our evaluations of what usually is viewed as constituting creativity—novel, surprising, and compelling ideas or products—represent local norms. This, of course, is disappointing to those who believe in creativity as a trait" (398-9). Taking seriously the contingencies of local norms on identifying or evaluating creativity, then, means that local norms are themselves a relevant part of the phenomenon being investigated in research on creativity. According to this view, it makes little sense for future research to treat creativity as a stable, universally valid individual capacity

that can be reliably measured by seemingly de-contextualized and standardized tests, like the TTCT. This perspective is echoed in approaches that take up Vygotskian notions of creativity and imagination as situated acts of world-making in ongoing human development, like those described by Pelaprat and Cole (2011, 416), who write,

> What is crucial here is that creativity and imagination, though both involved in the production of novelty, cannot be understood by the criteria of their products. What is primary and irreducible to creativity is *social and cultural change* in the world of reciprocity, norms, laws, knowledge and institutions—that is, in a "culturally defined domain of action in which innovation is possible." Imagination, for its part, references not simply the criteria of novelty in an 'image,' but the development of an individual into a social, cultural form of life.[15]

In agreement with Pelaprat and Cole, our study suggests that the "local norms" evoked by Sternberg are not reducible to stable, implicit cultural beliefs, but are themselves produced (and/or co- and re-produced) in dynamic interaction as participants encounter researchers, techniques and technical systems in laboratory experiments. Thus, our study has implications for experimental paradigms whose implementation calls for neutrality and consistency toward the comparability of findings across participants, and their subsequent claims of generalizability. Our study, despite being limited to only three animators, suggests that the consistency and neutrality of implementation is *undone* in the differential meanings those measures take on in situated interaction. In other words, whether a researcher withholds their comments or offers affirmations, this will not "land" the same way across participants, or even across moments within the trial of one participant. The interpretability of participants' activities during experimental trials necessitates qualitative engagement with locally emergent and mutually

[15] Following Vygotsky, Pelaprat and Cole distinguish imagination from creativity, with imagination describing the "individual-temporal-spatial" development of an individual, and creativity describing the "socio-historical" process that informs, and is informed by, imaginative acts. Our study did not take up this central distinction between creativity and imagination, but rather used the term "creativity" to consider individual-becomings as co-productions with socio-historical or ecological (Hutchins 2010) environments.

performed meaning-making at the site of distributed interaction. Moreover, in line with Pelaprat and Cole's description of this process as one of development, wherein an individual is themselves produced within a particular form of life, our animation task called forth authorial animators and responsive researchers in their processual production of a shared, provisional and experimental form of life.

Implications for Autism Research

While these findings do not identify alternative styles of autistic creativity in comparison with creative strategies of typically developing populations, they do have implications for subsequent research on autistic creativity. First, these scenes suggest that research on autistic creativity cannot assume that seemingly identical experimental stimuli remain identical across participants or even throughout one participant's ongoing activity. In other words, research on autistic creativity must be accountable to the different and dynamic ways that various participants experience the experimental conditions that are meant to evoke creative response or engagement. This is especially relevant in autism research, since studies have shown that atypical interaction at the site of the experimental trial impacts the ways in which autistic participants' performance is interpreted and scored, even when task performance is meant to demonstrate a capacity *outside* of this interaction (Milton 2012, Edey et al. 2016, Maynard and Turowetz 2017, Heasman and Gillespie 2018, Alkhaldi et al. 2019, Sheppard et al. 2020). It also follows empirical observations of differential engagement with experimental tasks (Roth 2019) that confound assessments of capacity or ability with those of motivation or interest.

The study of autistic creativity, despite being a relatively small portion of the otherwise large body of scientific autism research, has tended to follow the deficit models embedded in

much of the autism literature (Happé, Briskman, and Frith 2001), a situation which results in much being said "…about what autism *is not* rather than *what it is*, as a form of being in the world" (Maynard 2005, 500, italics added). Addressing this issue involves attending to "the world(s)" or, as we considered them, "contexts" that situate autistic being and becoming. Recent literature has suggested that this question necessitates autism research to be done in "ecologically valid" environments, like school, family and social spaces, the everyday "life space" (Lewin 1943) of autistic individuals (Ochs and Solomon 2010). This project instead asked how autistic being and becoming can become visible in laboratory experiments, themselves actual and lived sites that are marked as artificial and reductive. In laboratory experiments and tests, this requires consideration of how participants make sense of the experimental procedures they are presented with, and an account of how all participants "live" such settings. In our study, I used an ethnomethodological approach to apprehend experimental sessions through the enacted sense-making of participants, following similar strategies used in studies of clinical interactions between medical professionals and autistic patients (Maynard 2005, Maynard and Turowetz 2017, Maynard 2019), experimenters and subjects (Alač 2020a; 2020b), experimental researchers training novice researchers (Alač 2011), and ethnomethodologists and "non-verbal" children (Goode 1994). Doing so revealed the dynamic context-dependencies of creative engagements as they emerged in the improvisational choreography between animators, researcher, animation materials, and the experimental occasion.

Chapter 6

Conclusion: What is an Experiment?

6.1 Performative methods

In 2002, British artist Rod Dickinson staged a meticulous reenactment of Stanley Milgram's notorious obedience experiment as a live performance witnessed by an audience through a one-way mirror. Dickinson did not take a 'properly' scientific event and theatricalize it; Milgram's experiment was already an explicit piece of theater. The theatricality of the Milgram experiment itself is not limited to its spectacular scenario and core deception, nor even to the fact that the experiment was conceived as a reenactment of the bureaucratic Holocaust scenarios dramatized by the contemporaneous Eichmann trials. The theater of Milgram's study, like that of other, less notorious social psychology experiments of the twentieth century, was a necessary strategy to resolve the problems of seeming and being that obscured objective psychological truths within the subjective comportment of experimental subjects. Social psychology experiments relied on theatrical strategies to circumvent people and their messy differences to get to "the things themselves." In Milgram's case, the staging of the study was a way to "get to" the obedience drive. As discussed in Chapter 2, the proscenium frame of the experiment's dramaturgy was designed to recede for experimental subjects, asking them to live what was staged so experimenters could harvest what was assumed to be otherwise not accessible: the inherent obedience that subjects themselves could not perceive.

Dickinson's grueling[1] reenactment of Milgram's experiment asks an audience to live through, moment by moment, the felt events that are staged by the dramaturgy of an experiment in which a person is instructed to inflict pain on another person at a distance. By re-theatricalizing the experiment that, in its initial staging, was exclusively focused on thethe experimental subject that was its protagonist, Dickinson made the experimental situation *itself* — in its specificity, materiality and multiplicity of encounter—the protagonist of the drama. Thus the audience of Dickinson's piece was invited to experientially consider not only the experimental subjects' dilemma, but also this dilemma's emergence relative to the technical mise-en-scene, the researcher giving instructions, the off-stage actor, and the broader communities that grounded and validated the experiment as a necessary and legitimate inquiry into human sociality. For Dickinson, the layer of reenactment is meant to catalyze viewers' experience of mediation itself. Referring not only to his Milgram reenactment but also to his other reenactment-based artworks, Dickinson explains,

> My hope with these pieces is that the audience's direct experience of the live performance is constantly undercut by their knowledge of the layers of mediation that are at play in both the original historical event and my double of it. I hope with pieces such as these that rather than making "history" "real" (often the declared aim of reenactments found in other cultural spaces, such as TV or hobbyist recreations), history is actually experienced by the audience as deferred and displaced, but through the apparently immediate and direct lens of live performance. (quoted in Arns 2020, 201)

Dickinson's dramaturgy of reenactment does not elide the disappearing proscenium but makes this disappearance ever-present. Consistent with strategies of experimental theater, the Milgram

[1] Grueling, not least of all, for the audience who attended Dickinson's four-hour performance, as one reviewer from *The Guardian* explained. "By the halfway point I'm with the guy wailing to be set free, as I contemplate a Lady Macbeth-style fainting episode and wish I had a camera with flashbulb in my bag. Rather than a sign of the project's failure, however, the creeping enervation of being here is central to its dark power. Dickinson is testing us, seeing if we'll stave off boredom, nicotine cravings, and the lure of the busy bar just beyond the gallery door, just because we've been told to stay where we are" (Mahoney 2002, quoted on https://www.roddickinson.net/pages/milgram/project-synopsis.php).

reenactment asks audiences to live through the event while reflexively attending to the layers of mediation that condition the experience of "living through."

Reenactment is just one formal strategy of the experimental theater, a broad genre of historical and cultural production that seeks to expose the means of its own production towards a relational reflexivity about processes of sense-making. Interestingly, Dickinson stresses that reenactment (and perhaps, by extension, experimental theater) is a properly artistic method, outside the bounds of scholarly or scientific inquiry. See, for example, this exchange between historian of science Charlie Gere and Dickinson from a 2004 interview:

> Charlie Gere: None of your work resembles any other artwork that I can think of, in that it is not self-consciously art, it is self-consciously something else. The Milgram reenactment, the work for which you are most well known so far, could equally well be a very serious sociological or social science experiment. It is presented absolutely deadpan with no attempt to impose any notion of the artist or the art world.
>
> Rod Dickinson: Yes, exactly. But the reason that it couldn't be a social science experiment is that it uses an amateur genre of representation, the reenactment, which is something that nobody, certainly nobody with any kind of academic backing, would do. It's too discredited, too dubious, and too nebulous in terms of what it might mean and what it might be. (Gere 2004, 338)

Dickinson posits reenactment as outside the bounds of acceptable academic methods, assuming adequate methodologies to be, as I wrote in Ch. 1 (p. 36), always "in relation to the purported *objectivity* of the scientific method." In that chapter, as in the rest of the dissertation, I have contested this presumed distinction between performance practice and data collection, arguing instead that interpretive acts of staging, performing, and intervening *inhere* in even the most seemingly objective modes of experimental measuring, computing and classifying.

In fact, performative strategies of reenactment, reconstruction, replication and "re-working" are increasingly taken up as ways to explore histories of science and technology, cultural practice and interaction, and the processual aspects of artifacts, technologies and

infrastructures (e.g., De Jaeger et al. 2017, Dupré et al. 2020). The move toward performative methods in social studies of laboratory experiments and in STS more broadly is a feature of the field's turn to intervention and collaborative experimentation (Roepstorff and Frith 2012, Downey and Zuiderent-Jerak 2017, Lezaun et al. 2017). In conversation with these literatures, and despite Dickinson's doubt that "nobody with any kind of academic backing" would use reenactment and other performative methods that allow ambiguity as to what they "might mean," that is precisely what I have attempted to do.

For theater studies, as I discussed in Ch. 1, the wider academic turn to performativity theory had the disconcerting effect of disavowing and further marginalizing theater practice as a theoretically potent and epistemically relevant method of inquiry in respect to hierarchies of knowledge and prominence in the U.S. academy. In my insistence on theater, staging, and dramaturgy as an intentional (or, as Pannill Camp might put it, directional), contingent, and iterative mode of knowing and being, I locate theater as the moving center of performativity, a theory-in-practice informing experimental inquiry of all kinds. I suggest that theatrical modes of collective knowing offer radical ways of engaging inquiry across disciplines, domains, and communities, particularly in conversation with recent turns to embodiment, movement and change as conditions for knowing. By leveraging theater as hermeneutic and method, this project presented one domain—scientific experimentation—where a reflexive, immersed view made available novel insights and entryways.

The sort of critical collaboration I advocate for in the doing of this project refuses a respectful distance where one domain explicates, justifies, or subjects itself to the other.[2] Instead, I build on the work of historians who have unearthed the important inter-dependence of

[2] McConachie 2008, Blair and Lutterbie 2011, Blair and Cook 2016, Faletti 2016.

theater and experimental science.[3] The experimental theater I pursue in this dissertation suggests that theater and experiment can and should continue to elucidate and shape one another to approach many-layered and questions crucial to epistemic inquiry: What is happening here? How is being done? How does it condition ways of knowing? How can knowing be done otherwise?

It is by focusing on and participating in the production and resolutions of experimental 'ambiguities' that I have been able to describe possibilities and obstacles for expanding interpretive practices in the design, implementation and analysis of cognitive neuroscience experiments. My method of reflexive engagement—working from within the phenomenon of interest as an engaged and accountable collaborator—acknowledges the insufficiency of critique at a distance (Latour 2004), and takes up de la Bellacasa's (2010) suggestion that participatory transformation of research objects "is a way of relating to them, of inevitably being affected by them, and of modifying their potential to affect others" (99). In working this way, one of the contributions of the present project is in its description of this transformation, showing how various research objects (whiteboards, autistic youths, cognitive neuroscience experiments) are locally animated alongside the researchers they animate in turn.

6.2 The interpretation crisis and going experimental

In the introductory chapter, I argued that the current concept of science's "replication crisis" fails to capture the range of challenges to the legitimacy and trustworthiness of scientific experimentation, and I proposed to think of these challenges as instead part of a wider "crisis of interpretation," a description that can include a broader group of troubles that plague even the most controlled, elegantly designed, and reproducible of experiments. While not denying that

[3] Roach 1993, Coffey 2004, Camp 2007, 2014, Watt Smith 2014, Shepherd-Barr 2020.

failures to replicate experiments are of concern, I suggest that hermeneutic scientific practices outside of replication troubles can equally canonize inaccuracies through, for example, their naming strategies (as in the case of mirror neurons, Ch. 2), or the design and staging of the experiments themselves (as in many of the autism studies discussed in Ch. 3 that lacked the capacity to grasp the subjectivities and concrete competencies of their autistic research subjects).

The notion of an interpretation crisis is not meant to suggest that the very presence of interpretive practices in experimental science is problematic; on the contrary, STS literatures have thoroughly demonstrated the centrality of intuition, affect, sociality, bodily engagement and interaction in the unproblematic doing of "objective" science.[4] Rather, an interpretation crisis refers to the relative lack of attention to the ways that experimental constraints on interpretation delimit and articulate the sorts of phenomena registered by an experiment, and, correspondingly, the forms of knowledge produced. A focus on interpretive practices also highlights hierarchies in experimental relations: how researchers, research subjects, and research technologies have differential roles and opportunities in responding to the doing of experiment and in articulating themselves and their interpretation of the scene. Attending to the emergence of these differentials in the moment-to-moment enactment of experiment, rather than as a post hoc critique of experimental data or research claims, seeks to put STS in grounded and generative conversation with science-in-the-making. As Roepstorff and Frith (2012) suggest,

> If instead of outlining abstract theories one would attempt the dangerous process of constructing one potential bridge [between cognitive neuroscience experiment and anthropology], what would it, then, look like? Our suggestion is to start out with joint research projects, do things together, and then be sensitive both to the type of facts and the types of contexts produced by going experimental (108).

[4] For example, Knorr-Cetina 1981, Mol 2002, Alač 2011, Myers 2015.

By "going experimental," I have been attuned particularly to the design and performance of a pilot experiment on autism and creativity in the cognitive neuroscience lab. Here, "interpretation troubles" appeared not only in the form of longstanding critiques about the deficit paradigms and exclusions of substantive autistic participation that have shaped experimental autism research (Ch.3), but also, as discussed in Ch. 5, in methodological obstacles to experimentally pursuing the sorts of embodied, distributed, dynamic and experiential cognitive processes that scientists themselves were increasingly interested in considering (Pineda 2018). While much of the existing effort to overcome such obstacles has been through developing more flexible technologies for collecting neural and physiological data (e.g., Makeig et al., 2009), my focus was to consider, intervene in, and analyze the design and performance of experimental protocols involving interactions between researchers and experimental participants. As Roepstorff and Frith describe above, "going experimental" in respect to the STS of laboratory experiment is a way to "understand experiments as a very peculiar human practice," to explore and intervene in the ways that "imaging experiments transform subjects into objectivity" by taking seriously the experiences of experimental subjects (Roepstorff and Frith 2012, 105).

Drawing on my understanding of theater as both staged and lived, my approach resisted a presupposition of the laboratory experiment as controlled and artificial in distinction to the ecologically valid "real world." Instead of advocating for an escape from the lab into the wild, this project demonstrated that attending to the situated ways that experiments are lived, in respect to how they are staged, can itself contribute to a more "ecological" understanding of the experiment's phenomenon of interest. Here, this was done both as an orientation to a scientific literature review (Ch. 3) and as a mode of active experimentation in the lab (Chs. 4 and 5). In Ch. 3, centering the experiential aspects of classical autism experiments allowed for reinterpretation

of their demonstrations of autistic impairment, and suggested that these studies missed crucial aspects of autistic subjects' "concrete competencies" (Maynard and Turowetz 2017, Maynard 2019, Roth 2019) called forth by the demands of the experiment. In Chapters 4 and 5, exploring the creative process of autistic and typically developing youths was done through a careful analysis of the moment-to-moment situated interaction between a researcher, the experimental participants and the experimental task. This allowed for an ecological view of each experimental session, tracing how local standards for creativity emerged dynamically, rooted in situated mutual involvement in shared spaces of action.

6.3 Expanding interpretive practices in the cognitive neuroscience lab

The overarching argument of this project advocates for opening the laboratory to more heterogenous participation with or alongside scientists. My own project is one possible version of this opening, one attempt grounded in the particularities of my own interest and background, shaped by those of my collaborators (both scientific and adolescent), and the constraints of our process. As radical as it may seem to suggest that the experimental laboratory can be more porous to disciplinary and methodological difference, it is also a deeply conservative—that is, conservationist— impulse that recognizes the vast excess of potential "data" or "material" that suffuses laboratory happenings, most of which is either suppressed or discarded as noise. For my scientific collaborators in the experiment on autism and creativity, the technical problems that prevented the reliable collection of two streams of EEG and gaze tracking data made it difficult to analyze the data that remained. For me, as an embodied, embedded, enactive and extended ethnomethodologist, the "data" was the process itself as it unfolded dynamically in moment-to-

moment interaction. What else was lost in those sessions, discarded or unregistered because there was no experimental composer, poet, historian, or dancer present to save the phenomena?

Indeed, the most radical inclusion into the design, performance and analysis of cognitive neuroscience experiment may be that of people who most often constitute the cognitive populations being tested. Even in the face of our tiny sample size, technological troubles, and non-generalizable findings (items that would ordinarily appear in the laundry list of limitations in a standard scientific report), this is what I identify as the greatest limit of the experimental process we designed and conducted. The participation of the adolescent subjects of our study was strictly circumscribed by their two sessions at the Animation Station. There was no mechanism for their further participation, either in the analysis of the video recordings, the interpretation of the stop-motion animations made across the study, or in discussions about how this work may be meaningful, how it could be extended, or how it should be talked about. The limitation here is not on how the experiment *could be* interpreted, but in how its design limited how it *was* interpreted, and by whom. Compare this to exceedingly clear directives from autistic researchers and advocates:

> Specific manifestations of participatory research might include leadership by autistic researchers, partnership with autistic people or allies as co-creators of knowledge, engagement with the community…and consultation with relevant individuals or community organisations (…) *Put bluntly, the traditional autism research culture – in common with many fields of scientific enquiry – is inadequate regarding the extent to which autistic people have been able to shape the research agenda, its implementation and dissemination of its findings*. (Fletcher-Watson et al. 2019, 944; italics added)

and

> AASPIRE evolved from an informal "journal club" composed of autistic self-advocates and parents of autistic children. The more research studies the group reviewed, the more frustrated members became with problematic issues in the autism literature: A misalignment between researchers' priorities and those of the autistic community; *a lack of inclusion of autistic individuals in the research*

> *process*; use of demeaning or derogatory language and concepts; threats to study validity derived from miscommunication between researchers and participants; and the use of findings to advance agendas that opposed community values. (Nicolaidis et al. 2011, 143; italics added)

and

> The only way to respond to these principles [guide ethically-informed autism biomarker research] is *for scientists to work immediately to create an open and participatory research process*, as exemplified by community-based participatory research. (Pellicano et al. 2011, 769; italics added)

It is possible to speculate on future directions for our initial work on autism and creativity, shifting away from questions about the researcher-subject relationship and instead asking young people (either autistic or typically developing) to work together to support each other's animation work. But what might a more radical participatory laboratory environment look like? How might a different participatory framework mitigate participants' experiences of intimidation and alienation from the experimental task and broader experimental context?

One way to expand the available involvement (and investment) of autistic young people is through the creation of a long-term "Animation Studium." Functioning as both an animation studio and an ongoing research group on distributed creativity, the Animation Studium would welcome autistic and other participants to join the group if they were interested, and engage in *ongoing* artistic and animation projects while considering how to experiment with creativity. This core group of young people (with their parents' consent) would not only participate in making animations within researcher-designed experimental studies, but would themselves participate in the design, doing and analysis of experiments, suggesting follow-up research questions, co-designing subsequent experiments, and co-creating reports and multimedia projects to present findings to diverse audiences.

Although this sort of participatory approach is unusual, it is neither unprecedented nor unwarranted. For example, UCSD autism researchers Jeanne Townsend and Leanne Chukoskie, who investigate visual processing anomalies in autistic teens and design video games to mitigate these anomalies, have long included their autistic experimental subjects as lab members and video game designers, to the benefit of the work.[5] In the Japanese practice of *tojisha kenkyu*, autistic people are themselves researchers, naming, studying, experimenting with and reporting on their own unique condition, among a community of researcher-peers (Nakamura 2013, Kumagaya 2015). Finally, the British group Playing A/Part is an interdisciplinary project, exploring the identities and experiences of autistic girls and adolescents through creative and participatory research,[6] a collaboration involving academics in drama, media arts and psychology between the Universities of Kent and Surrey. Among these projects and practices, the Animation Studium is another attempt to meaningfully welcome autistic people as collaborators in research that makes conjectures and claims about their communities. In respect to expanding interpretive practices in the cognitive neuroscience lab more broadly, it is particularly autism-led participatory initiatives that may yield the most insight on the possibilities for diverse sensitivities to shape research questions, practical methods and modes of deliberation in the daily life of the lab.

Establishing something like an Animation Studium would make possible long-term engagement by and with autistic young people in the service of continuing to design, conduct and interpret cognition research in dialogue with their own experiences and priorities. At the same time, by grounding this work in stop-motion animation, Studium members could continue

[5] DeWeerdt, Sarah. "Can Science-based Video Games Help Kids with Autism?" *Science* 22 June 2018, online. DOI:10.1126/science.aau5369.

[6] https://playingapartautisticgirls.org.

to explore animation as an expressive, collaborative and experimental medium. Thus, in contrast to short-lived laboratory interactions that can feel alienating or intimidating, the Animation Studium is imagined as a long-term, inclusive research space, both like a lab and like a rehearsal room, where members come together to think, collaborate, and experiment.

6.4 What is an experiment?

I have little interest in defining, identifying or circumscribing what an experiment is or should be. Like all the activities that I have considered in this project, the definition of an experiment—what makes it adequate as such—is always in respect to what is being done, by whom, where and toward what purpose. It is for this reason that I was stuck for so long when, for years after beginning to think through theater, I kept trying to understand and articulate the relationship of the experimentality of the theater to the experimentality of the laboratory. My mistake was in putting the two side by side and attempting a comparison of kinds, when in fact the way forward was in attending to what it was I was doing when I felt myself to be experimenting. What does it feel like to get experimental? Poet Lyn Hejinian offers a description of poetry as a form of inquiry that expresses something of how I felt it:

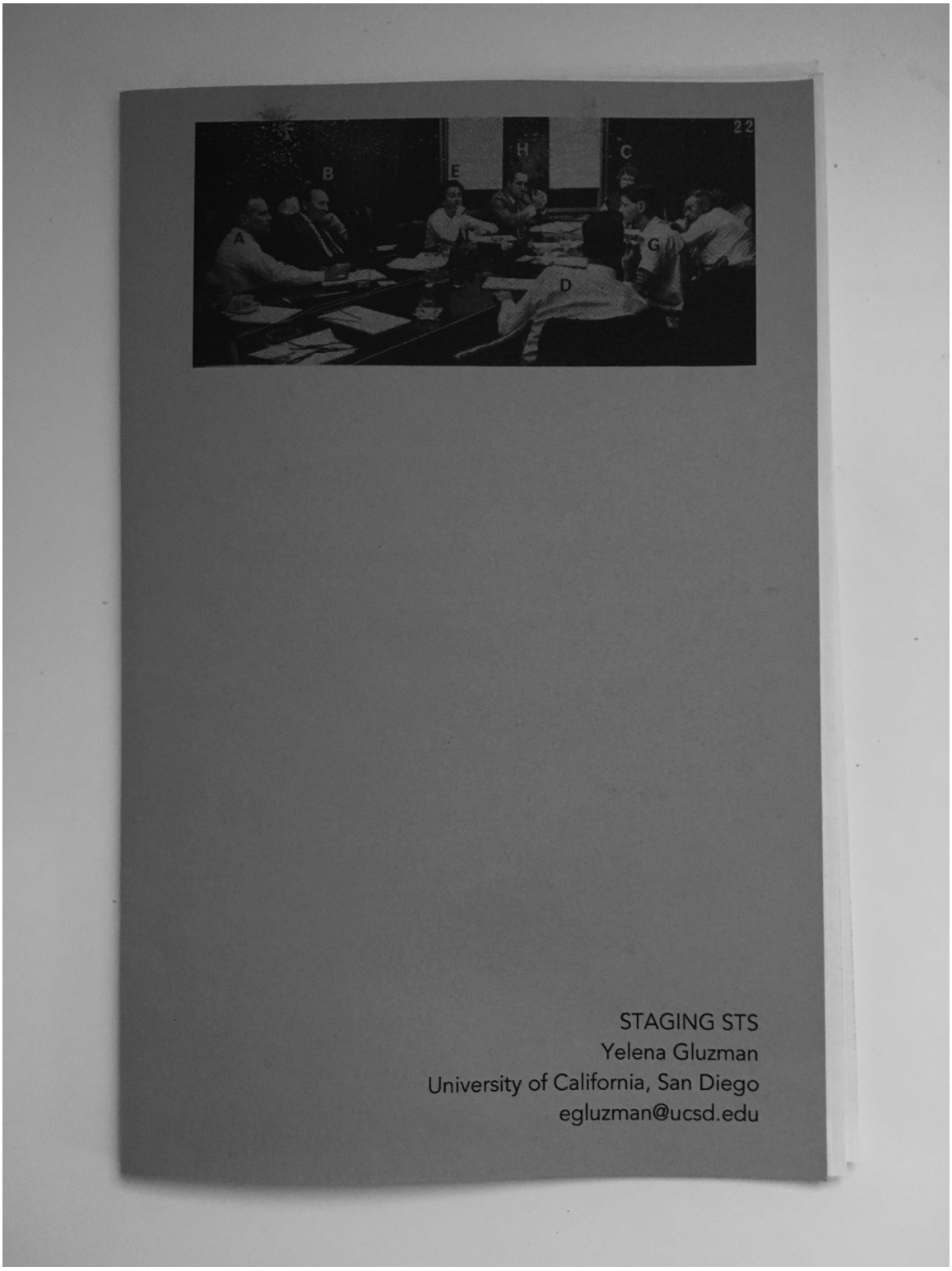
> Poetry comes to know that things are. But this is not knowledge in the strictest sense; it is rather acknowledgment—and that constitutes a sort of unknowing. To know *that* things are is not to know *what* they are, and to know *tha*t without *what* is to know otherness (i.e. the unknown and perhaps unknowable)…This acknowledgment is a process, not a definitive act; it is an inquiry, a thinking on. (2000, 2).

The way I experience experiment is irreducibly social; it is a way of actively being and participating in the world with others, in the lab, in the rehearsal room, and in the classroom. In that sense, experiment has been both a way toward (to use Hejinian's formulation) *acknowledging* a world, and a way of being acknowledged within it.

In this project, I considered the ways experiments are staged in respect to how they contingently materialize phenomena as interpretable. My invocation of theater in respect to laboratory has not been as a frame or lens, meant to vanish so we see past or through it. Nor was it a mirror, a self-exemplifying reflection of the laboratory experiment. Instead of putting laboratory and theater side by side, I made both and walked inside. I took theater and lab experiment to be co-constitutive aspects of each other, not only because the two are historically and paradigmatically enmeshed, but also because their entanglement has come to materialize my own histories and paradigms for experimental knowing and not-knowing, for an inquiry that is a thinking on.

Appendix 1.1

Staging STS Booklet

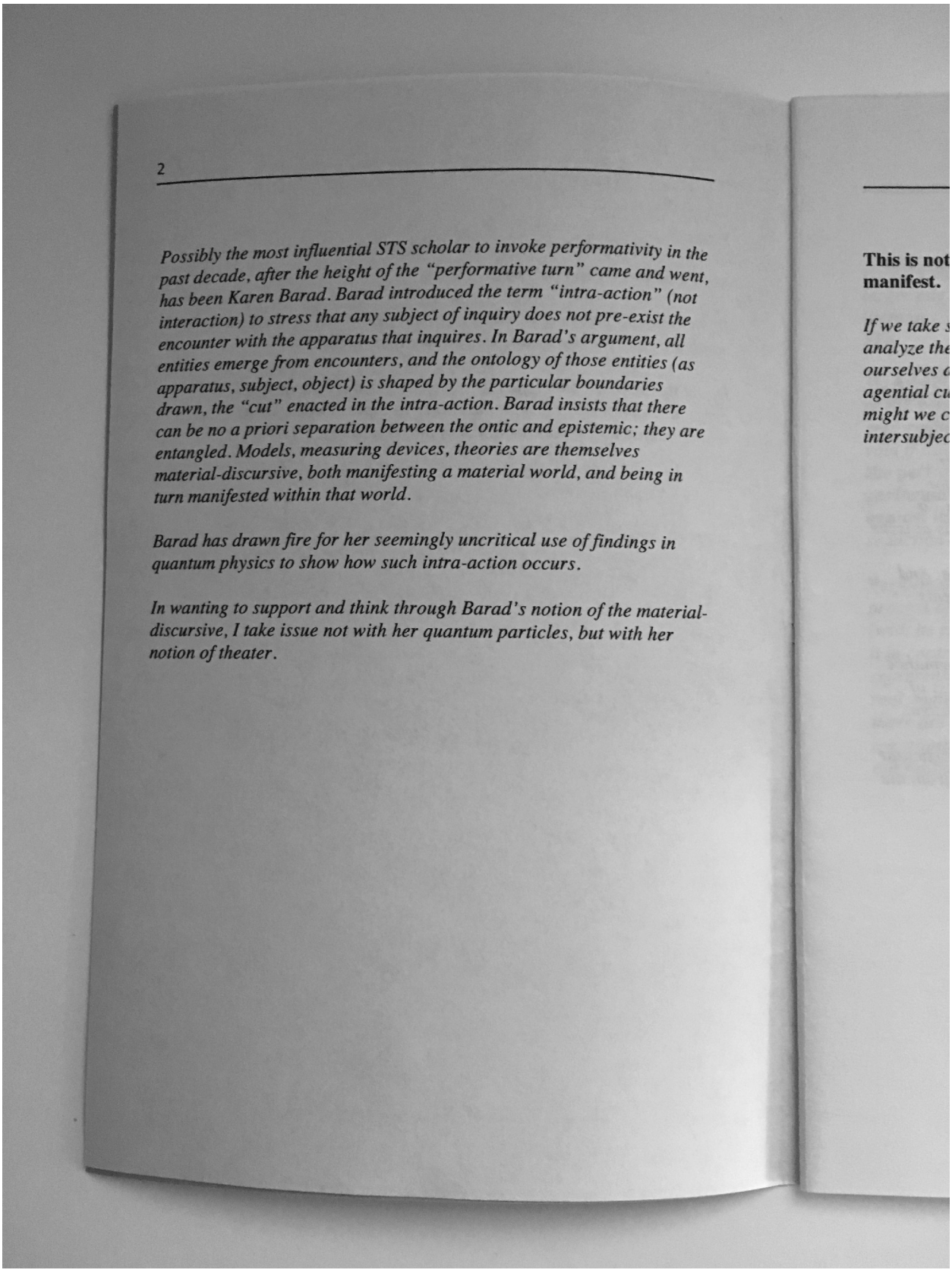


Hello.

My name is Yelena Gluzman.

Rather than describe my research, I would like to attend to the material conditions that make possible this panel presentation.

Doing this sort of "panel gazing" is not to demonstrate the performativity of scholarship, but rather to explore what, for STS scholars, may lie beyond the limits of performativity.



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Possibly the most influential STS scholar to invoke performativity in the past decade, after the height of the "performative turn" came and went, has been Karen Barad. Barad introduced the term "intra-action" (not interaction) to stress that any subject of inquiry does not pre-exist the encounter with the apparatus that inquires. In Barad's argument, all entities emerge from encounters, and the ontology of those entities (as apparatus, subject, object) is shaped by the particular boundaries drawn, the "cut" enacted in the intra-action. Barad insists that there can be no a priori separation between the ontic and epistemic; they are entangled. Models, measuring devices, theories are themselves material-discursive, both manifesting a material world, and being in turn manifested within that world.

Barad has drawn fire for her seemingly uncritical use of findings in quantum physics to show how such intra-action occurs.

In wanting to support and think through Barad's notion of the material-discursive, I take issue not with her quantum particles, but with her notion of theater.

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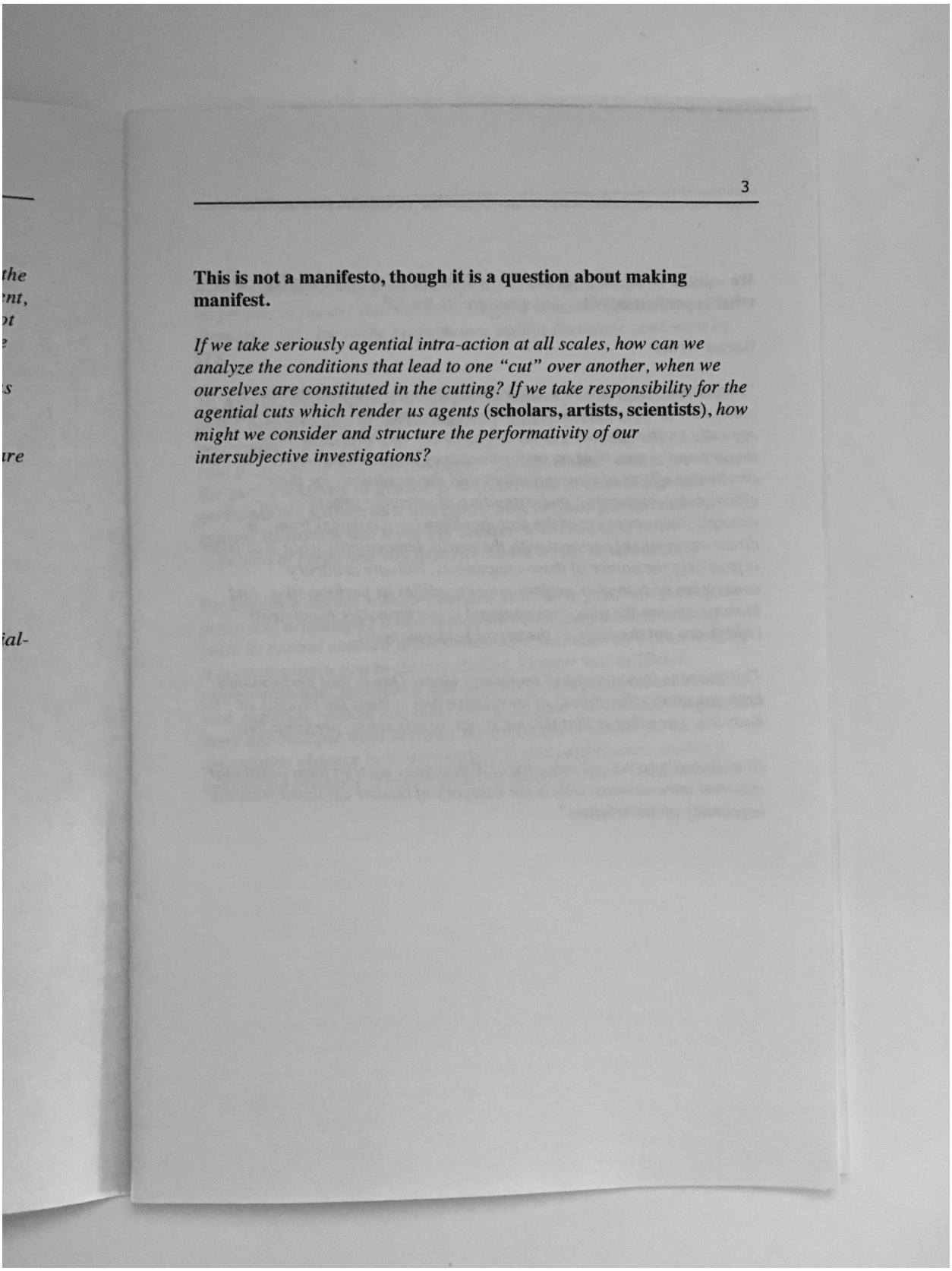
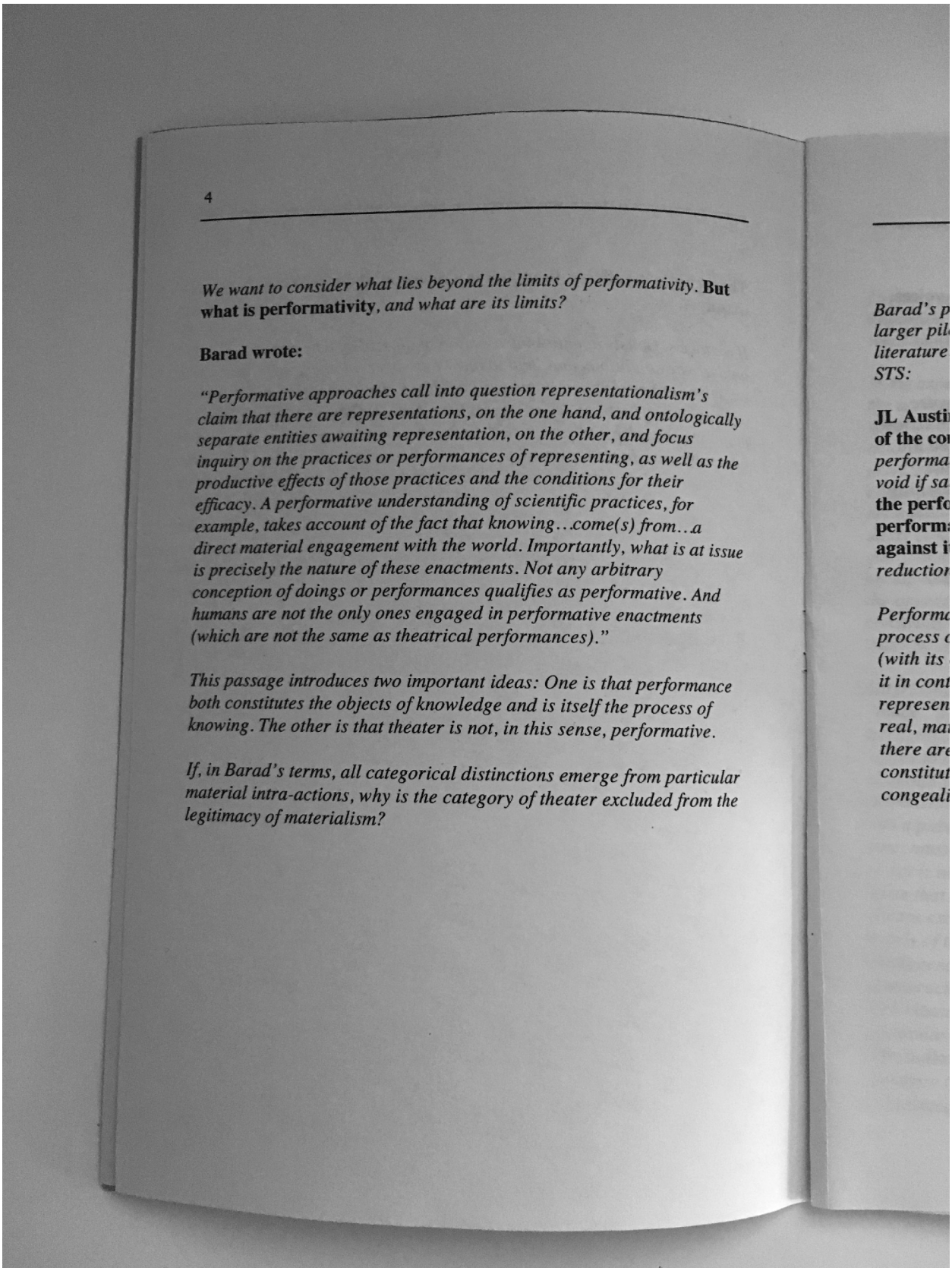


3

This is not a manifesto, though it is a question about making manifest.

If we take seriously agential intra-action at all scales, how can we analyze the conditions that lead to one "cut" over another, when we ourselves are constituted in the cutting? If we take responsibility for the agential cuts which render us agents **(scholars, artists, scientists)**, *how might we consider and structure the performativity of our intersubjective investigations?*



4

We want to consider what lies beyond the limits of performativity. **But what is performativity**, *and what are its limits?*

Barad wrote:

"Performative approaches call into question representationalism's claim that there are representations, on the one hand, and ontologically separate entities awaiting representation, on the other, and focus inquiry on the practices or performances of representing, as well as the productive effects of those practices and the conditions for their efficacy. A performative understanding of scientific practices, for example, takes account of the fact that knowing…come(s) from…a direct material engagement with the world. Importantly, what is at issue is precisely the nature of these enactments. Not any arbitrary conception of doings or performances qualifies as performative. And humans are not the only ones engaged in performative enactments (which are not the same as theatrical performances)."

This passage introduces two important ideas: One is that performance both constitutes the objects of knowledge and is itself the process of knowing. The other is that theater is not, in this sense, performative.

If, in Barad's terms, all categorical distinctions emerge from particular material intra-actions, why is the category of theater excluded from the legitimacy of materialism?

Barad's parenthetical disavowal of theater is only the tip of a much larger pile of theater that has been shuffled to the side, not only in STS literature, but also in the performance studies literature cited most by STS:

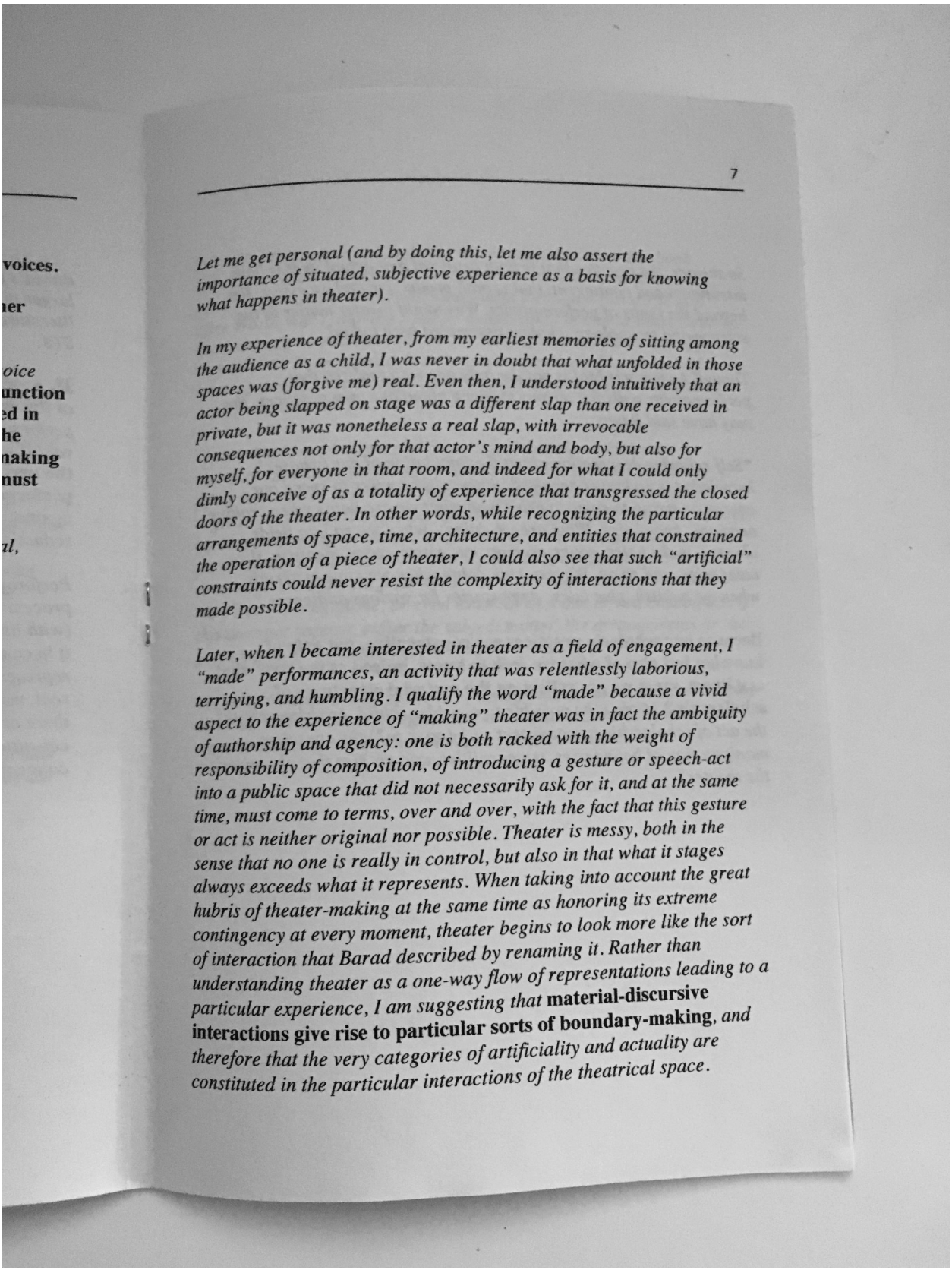
JL Austin, whose notion of the performativity of speech acts is one of the cornerstones of performativity theory, wrote that *"a performative utterance will, for example, be in a peculiar way hollow or void if said by an actor on the stage."* **Judith Butler, whose work on the performativity of gender is foundational to the emergence of performance studies as a discipline, also defined performativity against its divergence from the theater when she said that** *"the reduction of performativity to performance would be a mistake."*

Performance studies scholar Shannon Jackson argued that part of the process of articulating a new discipline called performance studies (with its central concern of performativity) was done by characterizing it in contradistinction to theater studies. Theater was artificial, representational, traditional and manual so that performance could be real, material, radical and theoretical. Jackson convincingly argues that there are multiple ways in which the exclusion of theater forms a constitutive absence in performativity's (and performance studies') congealing presence. Theater delimits, and delineates, performativity.

As I write this, I imagine these words diffracted by multiple voices. I imagine the instability of the word "I". I choose my words carefully, trying them on to a new "I," seeing if they fit another "my." I am writing differently.

A room full of simultaneous talking is difficult to hear. *One voice rings with clarity*. **This is not only a legacy of liberalism or a function of the legal structures that insist on individuals, it is embedded in the material-discursive systems through which we listen. If the clarity of one voice dominates, and yet we are committed to making a case for distributed cognition, for shifting subjectivity, we must attend to tuning, the structures through which we listen.**

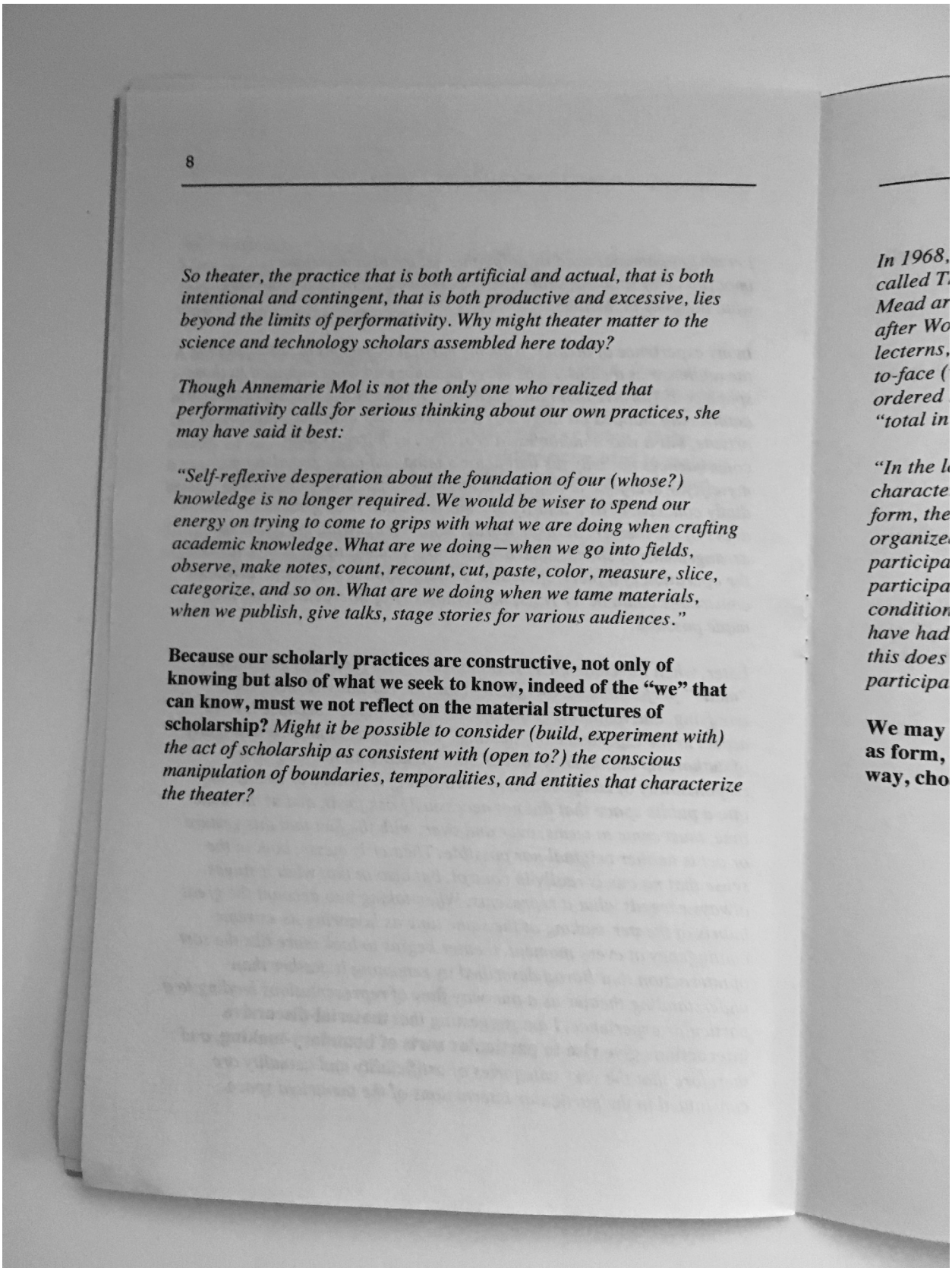
To say this in another way: what is this artificial, representational, traditional and manual practice that has been called theater?



Let me get personal (and by doing this, let me also assert the importance of situated, subjective experience as a basis for knowing what happens in theater).

In my experience of theater, from my earliest memories of sitting among the audience as a child, I was never in doubt that what unfolded in those spaces was (forgive me) real. Even then, I understood intuitively that an actor being slapped on stage was a different slap than one received in private, but it was nonetheless a real slap, with irrevocable consequences not only for that actor's mind and body, but also for myself, for everyone in that room, and indeed for what I could only dimly conceive of as a totality of experience that transgressed the closed doors of the theater. In other words, while recognizing the particular arrangements of space, time, architecture, and entities that constrained the operation of a piece of theater, I could also see that such "artificial" constraints could never resist the complexity of interactions that they made possible.

Later, when I became interested in theater as a field of engagement, I "made" performances, an activity that was relentlessly laborious, terrifying, and humbling. I qualify the word "made" because a vivid aspect to the experience of "making" theater was in fact the ambiguity of authorship and agency: one is both racked with the weight of responsibility of composition, of introducing a gesture or speech-act into a public space that did not necessarily ask for it, and at the same time, must come to terms, over and over, with the fact that this gesture or act is neither original nor possible. Theater is messy, both in the sense that no one is really in control, but also in that what it stages always exceeds what it represents. When taking into account the great hubris of theater-making at the same time as honoring its extreme contingency at every moment, theater begins to look more like the sort of interaction that Barad described by renaming it. Rather than understanding theater as a one-way flow of representations leading to a particular experience, I am suggesting that **material-discursive interactions give rise to particular sorts of boundary-making**, *and therefore that the very categories of artificiality and actuality are constituted in the particular interactions of the theatrical space.*



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So theater, the practice that is both artificial and actual, that is both intentional and contingent, that is both productive and excessive, lies beyond the limits of performativity. Why might theater matter to the science and technology scholars assembled here today?

Though Annemarie Mol is not the only one who realized that performativity calls for serious thinking about our own practices, she may have said it best:

"Self-reflexive desperation about the foundation of our (whose?) knowledge is no longer required. We would be wiser to spend our energy on trying to come to grips with what we are doing when crafting academic knowledge. What are we doing—when we go into fields, observe, make notes, count, recount, cut, paste, color, measure, slice, categorize, and so on. What are we doing when we tame materials, when we publish, give talks, stage stories for various audiences."

Because our scholarly practices are constructive, not only of knowing but also of what we seek to know, indeed of the "we" that can know, must we not reflect on the material structures of scholarship? *Might it be possible to consider (build, experiment with) the act of scholarship as consistent with (open to?) the conscious manipulation of boundaries, temporalities, and entities that characterize the theater?*

In 1968, Margaret Mead and Paul Byers published a modest book called The Small Conference: An Innovation in Communication. In it, Mead argues that scholarly meetings went through a historical shift after World War I, moving from a configuration of one-to-many (with its lecterns, stages, microphones, linear scripts) to a configuration of face-to-face (with its organization around a conference table, its lack of ordered speech, and its ongoing multimodal engagement as a form of "total involvement"). She wrote:

"In the long presentation and counter-presentation which may characterize a live symposium or panel, but is essentially a written form, the weaving together of thought appears only in sequentially organized statements by single speakers. In the [small] conference, each participant, by expressed disagreement, dissent, even boredom, is participating continuously... . The ideas that grow under such conditions are different from the ideas which any participant would have had working alone, or even working with one or two colleagues. If this does not happen, either the subject matter, the arrangements or the participants of the conference were poorly chosen."

We may disapprove of Mead's optimism about the small conference as form, but we cannot argue that the form was, in fact, in some way, chosen. *And that some choices are better than others.*

What are the relationships between the <u>form</u> of scholarly communication and the <u>object</u> of scholarship?

and

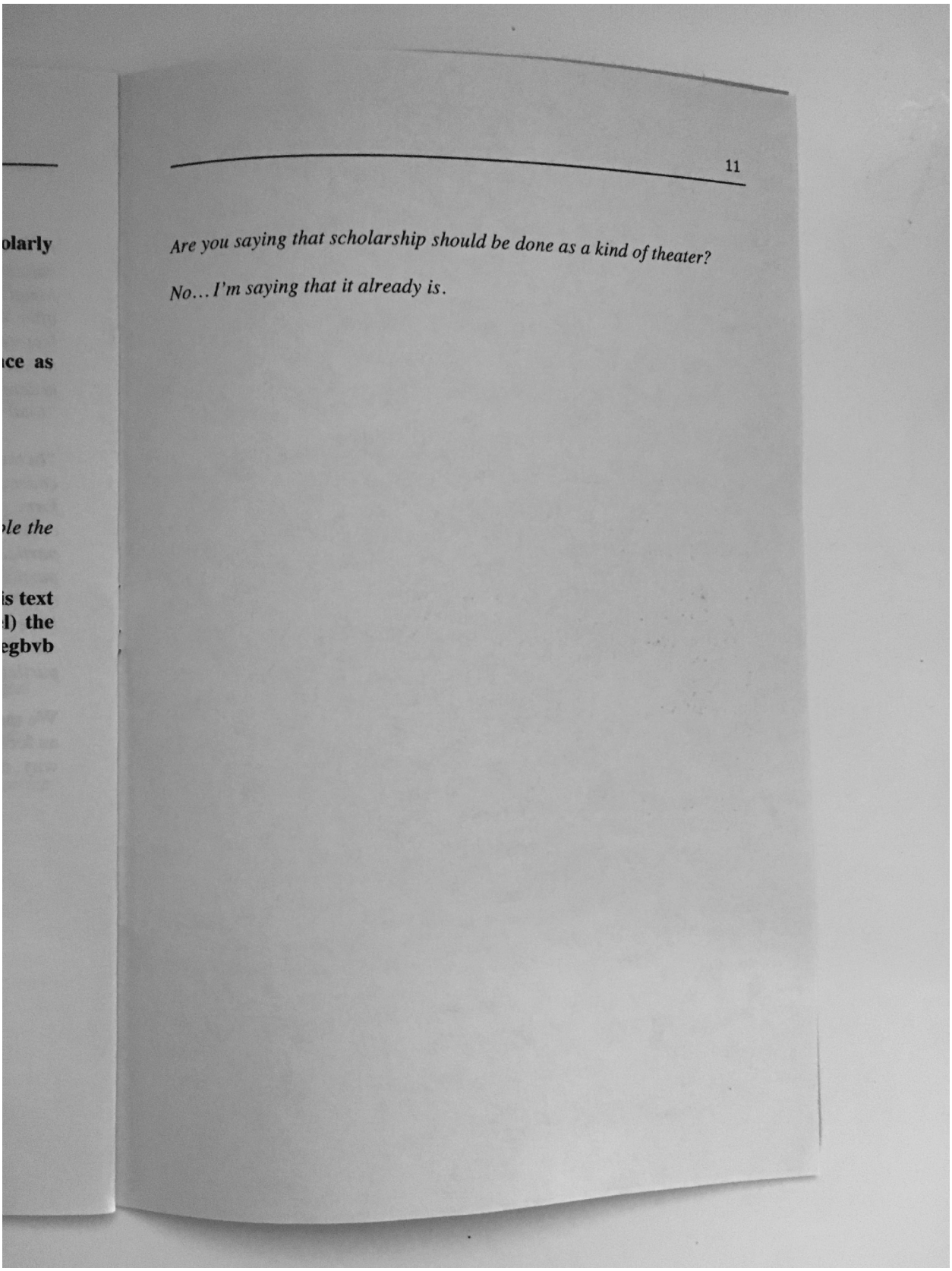
What are the conditions that constitute a scholarly utterance as such?

and

What is my part in this paper presentation?

What have I done to constitute a legitimate author, to make possible the legibility of the talk?

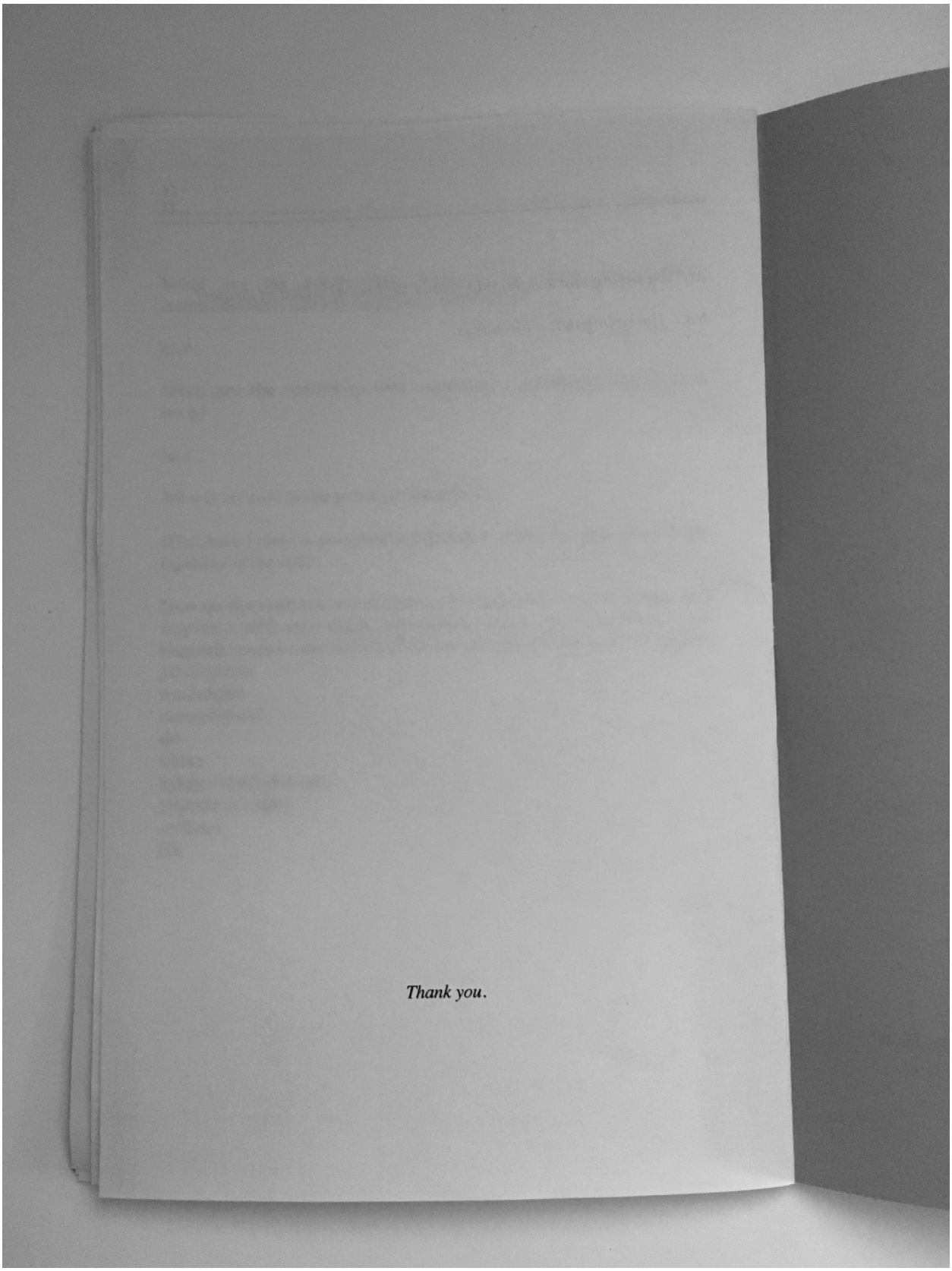
How do the multiple voices that carry, trip, alter, and drop this text negotiate with each other, even when (shout out to Garfinkel) the linguistic social conventions of this negotiation disintegratekbjegbvb jsbdughbxz kmdnkjbd dmnkdsbnkf dd khiue kjhgyyewoihzblioooo piqnninxx ejijiee eeeknnk iiiii



11

Are you saying that scholarship should be done as a kind of theater?

No… I'm saying that it already is.


Thank you.

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