MAKING LIFE IS EASY:
NOVELTY AND UNCERTAINTY
IN SYNTHETIC BIOLOGY

Talia Dan-Cohen

A DISSERTATION
PRESENTED TO THE FACULTY
OF PRINCETON UNIVERSITY
IN CANDIDACY FOR THE DEGREE
OF DOCTOR OF PHILOSOPHY

RECOMMENDED FOR ACCEPTANCE
BY THE DEPARTMENT OF
ANTHROPOLOGY
Adviser: Abdellah Hammoudi

January 2012
Abstract

This dissertation, based on roughly eighteen months of fieldwork in two laboratories at Princeton University, examines how practitioners of synthetic biology orient themselves vis-à-vis organic systems, by both engaging with and displacing biological knowledge. It also explores experimental methods in a highly uncertain field.

Synthetic biology is a new field of study in which practitioners are attempting to build novel organic systems from materials in the lab, drawing on knowledge and practices from engineering as well as biology and chemistry. Many synthetic biologists share the long-range aim of designing organic systems that will perform predictable functions. They seek to use cells for technological ends: a material among other materials. The newness of synthetic biology, I argue, has methodological value for anthropology in that it transposes difference—a hallmark of classical anthropological knowledge production—to the register of time. Novelty is a tool for generating difference and discontinuity, a tool that allows scientists and engineers, but also anthropologists, to unsettle entrenched understandings.

There are two major themes that emerge from my research. The first theme has to do with the denaturalization of biological knowledge. I examine the transformations and mythical alterations by means of which synthetic biologists question deeply held beliefs about living beings. The second theme involves the ability to take a conceptual framework or hypothesis and use it to produce results in the lab. The dissertation thus addresses such questions as: How are concepts reinforced and embedded in the research
process? How do researchers conceive of their projects and their findings in such a way that the results of their work fit into the broader world in which it is set? And how do their projects and the answers they generate build on or displace other ways of approaching living beings?
Table of Contents

Abstract .............................................................................................................................. iii

List of Images ................................................................................................................. vii

Acknowledgments .......................................................................................................... viii

Introduction ..................................................................................................................... 1
  What is Synthetic Biology? ............................................................................................ 7
  Locating Synthetic Biology .......................................................................................... 11
  Notes on Methods: Objectivities-in-Progress ............................................................... 16
  Anthropology, Novelty and Difference ....................................................................... 23

1. Two Labs at Princeton ............................................................................................... 34
  Weiss’ Lab .................................................................................................................... 35
    Discovering Synthetic Biology ................................................................................. 44
  Hecht’s lab ................................................................................................................. 48
    Falling Into Synthetic Biology ............................................................................... 52

2. Denaturing A Discipline:

  Knowledge, Complexity and Synthetic Biology ....................................................... 58
    Step One .................................................................................................................... 62
    Step Two ................................................................................................................... 65
    Step Three ............................................................................................................... 66
    Biochemical Magic .................................................................................................. 68
A Synthetic Biologist’s Encounters with Biologists.......................................................... 73
Biological Knowledge as Troubleshooting................................................................. 77
Complexity..................................................................................................................... 91

3. Changing Teleology ........................................................................................................... 99
Making Life is Easy.......................................................................................................... 107
Biology Bricolage .......................................................................................................... 112
Constructivism in Action ............................................................................................... 127
A Mythical Ontology ...................................................................................................... 134
Old Circles, New Circles ............................................................................................... 136

4. Between Concepts and Conventions .................................................................................. 140
Indeterminacy and Experiments ...................................................................................... 140
Hecht’s Lab: “How do we define growth?” .................................................................... 146
   Lab Precedent and Inherited Practice ........................................................................... 150
Weiss’ Lab: “What is a pattern?” ...................................................................................... 154
A Major Concept in Flux ................................................................................................. 160

5. In The Shadow of Method ................................................................................................ 163
The Move......................................................................................................................... 163
   Mobility Redux ............................................................................................................. 168
In the Shadow of Method............................................................................................... 174
Back on Track .................................................................................................................. 186
Conclusion: Anthropology and Emergence Revisited .................................................. 188

Bibliography ..................................................................................................................... 193
List of Images

1. Biological Toggle Switch.................................................................40
2. Neuronal Cells Differentiated From Mouse Stem Cells..................42
3. Roachbot.........................................................................................78
4. Synthetic Turing Patterns..............................................................157
5. Models of Synthetic Turing Patterns.............................................159
Acknowledgments

This dissertation was written in the Anthropology Department at Princeton University. I have accumulated a great debt to this wonderful department, and especially my committee. Jim Boon offered a wealth of associations and connections that proved fruitful beyond measure. Rena Lederman was a constant source of insight and clarity. My advisor, Abdellah Hammoudi, through unwavering seriousness, commitment and care, teaches by example. Outside of the department, Paul Rabinow provided intellectual inspiration and personal support, helping me move forward at every step of the way.

João Biehl, John Borneman, Isabel Clark-Decès, Lisa Davis, Carol Greenhouse, Alan Mann and Carolyn Rouse offered invaluable advice and guidance. Larry Rosen’s wisdom and generosity kept me on track and in good spirits. Carol Zanca, Mo Lin Yee, and Gabriela Drinovan helped me get through graduate school almost unscathed.

For camaraderie, as well as scholarly input, I would like to thank Leo Coleman, Ronnie Halevy, Nathan Ha, Janet Hine, Peter Kurie, George Laufenberg, Peter Locke, Ricky Martin, Mark Robinson and Erica Weiss. Claire Nicholas has been a consummate friend and colleague. Nicolas Langlitz offered encouragement and inspiration. I am grateful to Anthony Petro for his unwavering honesty and daily dose of buoyancy.

Additionally, this thesis would not have been written without the collaboration and cooperation of Ron Weiss and Michael Hecht, who tolerated my presence and questions, and shared with me pieces of their works and lives. I owe a debt of gratitude to all the members of Weiss and Hecht labs, especially Cil Purnick and Betsy Smith.

This research has been financially supported by the graduate school at Princeton and the Fellowship of the Woodrow Wilson Society. My fellow Fellows for the years 2009-2011 provided helpful comments and interesting interdisciplinary discussions.

Throughout this project, Paulo Natenson provided home, love, laughter, and the occasional much needed pep talk. Finally, I dedicate this thesis to my brother, Ishai Dan-Cohen, and my parents, Meir and Hana Dan-Cohen. Best. Family. Ever.
Introduction

Imagine a kit that allows you to grow a house out of cells. The kit contains some Petri dishes, solutions, buffers, cells, and instructions on how to grow and feed your dwelling. Maybe you can design the house on your computer. We know that our cells can make us: human beings with hands and feet and heads and brains and lungs and thoughts and feelings. Humans are comprised of all sorts of different architectural wonders, with different functions and requirements, and the instructions for that engineering feat are contained in cells from the get-go. Shouldn’t it be pretty easy for cells, given the right instructions, to make a house?

The living house is an oft-used example of the long-range plan for the field of synthetic biology. It’s gripping. Sure, humans build transistors and rockets and plasma TVs, but before that, they build shelter. And they use materials at hand that are suited to the task and are available to them whether through foraging or through markets. Today’s markets include biological life. Synthetic biology seeks to use this life for technological ends: a material among other materials that contains special properties and capabilities. Maybe these houses aren’t really on the horizon, but as parts of an imaginary world, they do help us think about cells—about biological life—as a material like other materials, with exploitable qualities. A group of European synthetic biologists attempting to build an artificial cell described such material as “living technology.”

---

The house made of cells ties a technological future to present-day problems. It makes the notion of a living technology intelligible: an answer to questions and problems that have already been posed. Yet such technologies are also transformative, producing practices or shifts in relations that help delineate problems in unanticipated ways. This dissertation examines the conceptual and practical struggles that accompany the continued transformation of life into technology, and of technology through life. My project follows synthetic biologists as they attempt to mobilize organic systems, despite, and even through, gaps in knowledge. I argue that this move towards framing organic systems in technological terms destabilizes much of what biological formulations have made commonplace.

Uncertainty is a fundamental component of experimental work, but it is amplified in the field of synthetic biology, and operates on numerous levels. Not only are experiments and projects riddled with uncertainty, but the very productivity and stability of synthetic biology as an experimental field is still unknown. Thus, my project, based on eighteen months of fieldwork, tracks how people engage with a world in which they do not know if their tools, strategies and practices can have intelligible effects. In some fields, over time, the reach of knowledge practices—symbolic, analogic, metonymic, mimetic, etc.—becomes established and entrenched, such that researchers can proceed as if knowledge and nature are one. In others, languages and metaphors are still in the process of being tested for their traction, for their ability to do things and do them predictably. This latter kind of endeavor, still in its early phases, still in its test period, constitutes the general object of this study.
In this dissertation I pay special attention to synthetic biology as a nascent field, and I do this for methodological reasons. I argue that newness has methodological value for anthropology in that it transposes difference—a hallmark of classical anthropological knowledge production—to the register of time. I show that the hopes and wagers of synthetic biologists trouble our understanding of what counts as “nature,” and what qualifies as our knowledge of it. Whether synthetic biologists ultimately succeed in bringing their ambitious projects to fruition, their work points to the destabilization of the relationship between biological knowledge and organic systems. Such a destabilization allows us to peek beneath settled understandings to their foundation, and gives the anthropologist a glimpse of the cultural repertoires at work in producing knowledge.

These cultural repertoires were what I set out to study. I wanted to find out about the “mopping-up” procedures that contributed to and maintained some standard of what counts as good science in a particular field. This initial agenda was very much built on an interest in the work of Thomas Kuhn, whose influence in philosophy and in the social sciences is deeply felt. I was particularly interested in how normal science related to a field in formation. That is, I wanted to find out about normative practices in relation to the business-as-usual of operating within an institutional and research agenda that was anything but business-as-usual. How can institutionalized “normal science” and protodisciplinary instability carve out a communal space of inquiry? How do norms and uncertainty interact? It seems clear that experimentation couldn’t proceed without some set of norms, nor could tenure cases, theses, publications and grant applications march along, even if the field to which they contributed was itself in formation. Synthetic biology offered solid footing for such an exploration because of its multiple scales of
uncertainty, which included its infrastructural ambition and its disciplinary hybridity.

Thomas Kuhn’s ideas have been stretched beyond the Natural Sciences. Notions such as “paradigms” and “normal science” have been applied to diverse fields, and these applications have been, in turn, questioned and critiqued. But what they point to are the resonances between knowledge forms kept institutionally distinct, and the sensed need for an account of change in areas of knowledge outside the “hard sciences.” Hidden in this concern, perhaps, is some longing. How do we change our understandings? See things differently? Start anew? These are, at heart, anthropological questions. This thesis explores some of the ways that synthetic biologists are reframing knowledge and its aims, and invites the reader to think across knowledge forms that tinker, disassemble, and reassemble.

My dissertation project continues where my undergraduate experience left off. As an undergraduate at Berkeley, I had the rare opportunity to co-author a book with my teacher, Professor Paul Rabinow. The book, *A Machine to Make a Future*, examined a genetic diagnostics company in the San Francisco Bay Area as it attempted to turn genetic sequences into diagnostic tools, and hopefully financial profit. Synthetic biology came to prominence around the same time, and represented an entirely different, and much more radical approach to the post-genomics era, one that, despite being application driven, starts out with a conceptual overhaul. Rabinow and his collaborator Gaymon Bennett have been involved in their own project on synthetic biology, trying to develop analytic tools for understanding contemporary ethical practice in this developing field. By

---

contrast, my project focuses on the knowledge practices of synthetic biologists and what these practices might mean for beings made of cells.

I do share with Rabinow, Bennett, and others a distaste for construing anthropology’s value in terms of the “social consequences” or “social implications” of science and technology. As Margaret Locke and Sarah Franklin have written, following Donna Haraway, “...[T]he phrase ‘social implications’ is often annexed to ‘the new genetics’ to bookmark a space for dealing with the consequences of technology—an equation that places society and sociality after the fact of technological innovation. This flies in the face of everything practitioners of sociology, anthropology, science studies, and feminism have argued about how the social precedes and is embedded within any kind of technoscientific project, initiative, or discovery.”3 In the wake of the science wars, the notion of “social consequences” has acted to sequester scientists from their “anti-scientific” colleagues. Rena Lederman’s caricature of anthropology and science studies through scientists’ eyes lends some force to why such a barrier has seemed necessary. For die-hard scientist-believers, “in questioning historically particular constructions of science’s autonomy and authority, cultural constructivists and their legions—Unabombers all—aim to ‘end’ science, destroy Civilization, and plunge us all into a stereotype... of rural ‘premodern’ misery, fear, and superstition.”4 Ironically, this was really the fear of postmodern solipsism and meaninglessness, grafted onto poor “premoderns” who were in many respects, if we accept the premise of “the original

affluent society,” less miserable than we are.\(^5\) I therefore join a group of anthropologists and Science and Technology Studies scholars who reject the notion that what lies between the “premodern” and the “postmodern” is the staunchly and universally “objective.” Asocial science is a social fact, with normative force and extra-individual powers of coercion.

This thesis is located at the intersection of different traditions of thought and methods of engagement. At one end, it revisits questions that have concerned epistemologists and philosophers of science for a long time. At another, it engages the history of science, and specifically the school of thought commonly referred to as historical epistemology. It also rests on and pulls from classical anthropological texts, as well as contemporary ethnographies of biotech and synthetic worlds. There are two major themes that emerge from my research. The first theme has to do with the denaturalization of biological knowledge. I examine the cultural projections and entrenchments with relation to organic systems which are both revealed and displaced by synthetic biologists’ ambitions, and I do this through notions of myth and transformation. The second theme involves the ability to make a conceptual hypothesis or framework produce results in the lab. How are concepts reinforced and embedded in research processes? How do researchers constitute projects, and how do they delineate a world in which those projects fit and can elicit answers? What do those answers look like for an emergent field?

What is Synthetic Biology?

Synthetic biology can be divided roughly into three dominant approaches or aims, with fuzzy boundaries, and even fuzzier logics of membership. These are ideal types which help make sense of who does what and under what circumstances. There are commonalities to these three groupings, which can be summoned for larger group coherence or dissolved in favor of smaller group allegiances.

The first approach to synthetic biology inherits its conceptual arsenal from computer science and engineering, and grafts the model of circuitry onto the design and construction of living systems. The tendency to describe biology in terms of circuits dates back to the 1970s, which saw the first attempts to formalize genetic interactions in Boolean terms. The following couple of decades territorialized circuits in the brain, in interactions of neurons. The circuit-oriented synthetic biology builds logic gates out of diverse kinds of cells. This kind of synthetic biology is perhaps the most infrastructurally robust owing to the organizational legacy of engineering disciplines with their emphasis on standardization. Practitioners of this kind of synthetic biology envision a hierarchy of functionality for biological engineering. They seek to design, build and characterize

---

6 Rabinow and Bennett divide synthetic biology into four approaches. They make an additional subdivision in the first approach between the Berkeley synthetic biologists, and the MIT group. The former are less interested in and committed to the development of an engineering discipline. Their observation of this division is based on their fieldwork at SynBERC.

7 Boolean algebra, which appeared in the 19th century, was applied to the construction of electronic logic gates in the 1930s. Logic gates take one or more inputs and produce predictable outputs. The application of Boolean logic to genetics therefore signals the bringing together of electronics and genetics.
“parts” made of cells. Parts are finite sequences of DNA that perform specific functions and can be integrated into “devices.” Devices can be further assembled into “modules.” Synthetic biologists would then be able to choose components “off the shelf” and build systems of ever-higher complexity (meaning more biochemical and cellular interactions). In other words, these synthetic biologists seek to “extract from living systems interchangeable parts that might be tested, validated as construction units, and reassembled to create devices that might (or might not) have analogues in living systems.”

The language of this kind of synthetic biology mimics the terms of electrical engineering and computer science. It imagines groups of cells as “switches,” “oscillators,” “latches” etc., all devices or modules, and all functional circuit components. The name of the game is functionality. Ron Weiss, head of one of the labs in which I conducted my fieldwork, is a committed practitioner of this sort of synthetic biology, though he also critiques it from the direction of systems biology. On an esthetic and discursive level, this group is the least likely to make grand pronouncements about the meaning of life, and the most likely to tie their research to concrete (albeit far off) ends.

Another group of synthetic biologists has been feverishly constructing synthetic genomes. These researchers take nucleotides (the building blocks of DNA) and string them together into synthetic constructs, which nonetheless follow the blueprint of existing life forms. They further utilize existing cellular processes to animate their synthetic genomes. That is, they insert their constructed genetic sequences into cells of a different kind so that they, in turn, produce the proteins coded in their new DNA. Within a generation or two these cells, with their new DNA, will change phenotype drastically.

---

Unlike the circuit-based synthetic biology, the synthetic genomes work is not (yet) function focused. Practitioners have been primarily concerned with the construction of organisms which mimic existing life forms. They are far from being able to design an entire organism with intentional behavioral or functional characteristics. And while these practitioners may refer to genomes as the cell’s “software” and attempt to use synthetic genomes to “boot-up” cells, their use of these terms inherits more from the inexorable language of genes-as-code, which morphed from linguistics to cybernetics in the recent past, than from any engineering discipline. In Foucault’s terms, both languages explore “a single theme, but based on two types of discourse.”\(^9\) Michael Hecht, head of the second lab I observed, ran a lab that was closest in its ambitions to this group, though his lab’s work was in many respects quite distinctive. Hecht’s lab did build genes and insert them into bacteria to try and make synthetic life, but they did not do this on a whole-genome scale, and they did aim to answer specific questions about functionality. Nonetheless, this group is concerned with the notion of synthetic life as a philosophical and theological idea, and therefore exudes a certain grandiosity.

Finally, and perhaps most radically, a third group of practitioners is attempting to construct synthetic cells, with capsules to contain some proxy for DNA, and the capacity to replicate. This group is the most radically oriented towards the creation of synthetic life. Their main goal is to use all human-made components in their constructs, rather than exploit the malleability of existing organisms, lest some critic should claim that synthetic

life made out of any organic materials is not really synthetic life.\textsuperscript{10}

As mentioned, two features of synthetic biology give special dynamism to norm building in this fledgling field: infrastructural ambition and disciplinary hybridity. The infrastructural ambition of synthetic biologists can be observed in the massively funded centers devoted to the field: institutions which exist in some array of spatialized and bureaucratized networks of actors. At the same time, synthetic biology inherits from engineering practices of standardization aimed at allowing practitioners to share inventions and knowledge across domains and constructs. Standardization, therefore, is not just a matter of gate-keeping. It is a constitutive part of attracting funding and of turning cells into “parts” that can circulate through different labs and into new inventions. Standardization, however, has proven difficult. As one postdoc in Weiss’ lab put it, “Well the problem I had with synthetic biology was that it was like being in the forest and trying to build an empire. You’re trying to figure out how fire works, because there’s no set of standards for synthetic biology yet. People are trying to apply different ones but there’s nothing sort of like agreed upon over the whole community or that holds water.”

Here, standards matter in two distinct spheres: one is the sphere of disciplining knowledge and its products (what counts as a good paper, experiment, project? How are experiments validated? Etc.), and the other concerns the engineering norm of building and validating parts. This second line of standardization was far more overt, and represented as pressing by engineering oriented synthetic biologists. But both were still in question.

The disciplinary hybridity has to do with the diverse educational backgrounds

from which synthetic biologists emerge. The labs I observed were peopled with biologists (molecular biologists, biochemists, embryologists, neuroscientists), organic chemists and engineers (electrical and chemical engineers, computer scientists). Each discipline and subdiscipline brings with it different expectations, requirements and assumptions. Furthermore, this diversity is institutionally inscribed, with synthetic biology labs popping up in bioengineering schools and genomics centers, but also in engineering, biology and chemistry departments, not to mention industrial labs, private institutes and start-ups. Students and postdocs in these labs, more than their often already tenured advisors, must meet disciplinary requirements, whether in course work and exams, or the scope of an acceptable project. Thus, far from being able to invent standards and norms from scratch, synthetic biology relies on taking and tweaking the norms of their immediate and proximate kin.

**Locating Synthetic Biology**

“Living technology” in itself, is not a radically new idea. It is continuous with, and reliant upon transformations in the practice of biology that go as far back as the late 19\textsuperscript{th} and early 20\textsuperscript{th} century German-American biologist Jacques Loeb. In 1890, Loeb wrote: “the idea is now hovering before me that man himself can act as a creator, even in living Nature, forming it eventually according to his will. Man can at least succeed in a
technology of living substance.”¹¹ Loeb’s control-centered biology in turn changed the significance of experiments. Historian of science Philip Pauly writes: “Experimentation gained significance beyond its ordinary function of providing determinate answers to definite problems within a hypothetical-deductive schema. The activity of experimentation took on value in itself, and experiments became demonstrations of the manipulative power of biologists.”¹²

Hannah Landecker’s insightful Culturing Life tracks the way in which cells, over the course of the 20th century, became technological objects.¹³ Cells themselves came to be seen as standardized manufactured goods. At the same time, cells were made to produce other goods, providing a technological platform for molecular manufacturing. The biology of the 20th century is therefore replete with tinkering practices, interventions and forms of biological engineering. This is perhaps why many practitioners and observers of synthetic biology tack back and forth between emphasizing continuity and discontinuity with previous endeavors, as we shall see. Synthetic biology is sometimes referred to as “genetic engineering on steroids,” a term that was frequently used by Ron Weiss, one of my principal interlocutors. Nicolas Wade’s announcement of synthetic biology’s arrival in the New York Times also pointed to the intensification of existing practices, though he framed this intensification as surpassing previous techniques. “Forget genetic engineering,” wrote Wade. “The new idea is synthetic biology, an effort by engineers to rewire the genetic circuitry of living organisms. The ambitious

¹² Ibid., p. 5.
undertaking includes genetic engineering, the now routine insertion of one or two genes into a bacterium or crop plant. But synthetic biologists aim to rearrange genes on a much wider scale, that of a genome, or an organism’s entire genetic code. Their plans include microbes modified to generate cheap petroleum out of plant waste, and, further down the line, designing whole organisms from scratch.”14 Beyond the “genetic engineering on steroids” view, synthetic biology has precursors in 20th century discourses and practices. It is hard to calibrate, but easy to sense the influence of cybernetics on molecular biology and its offshoots.

The emergence of synthetic biology as a bona fide field of research should also be seen against the background of the last decade’s developments in genetics. One of the major outputs of the Human Genome Project was a staggering amount of information. The burgeoning field of bioinformatics was the most readily marketable subset of genomics. And as sequencing technologies have become cheaper, information has become easier to amass. Scientists hardly publish peer reviewed papers on sequenced genes anymore. They barely publish papers on sequenced genomes.

Along with a sharp growth in the quantity of genetic information to be digested, some have argued there has been an equally dramatic reassessment of the “complexity” of biological systems.15 The sequencing of the human genome thus produced somewhat paradoxical results. On the one hand, it created a mountain of data on gene sequences and genomes, and on the other, it revealed knowledge gaps which introduced new questions

in the struggle to understand biological processes.\textsuperscript{16} For example, at the beginning of the Human Genome Project the number of genes in the human genome was generally thought to be around 100,000. With the sequencing of the genome the number shrank to around 21,000, a figure roughly equivalent to the number of genes found in the genome of \textit{Drosophila melanogaster}, the fruit fly. So how do we account for the difference between a person and a fly?\textsuperscript{17} The answer is not yet fully known, but biologists are turning to non-protein coding regions for answers.

Systems biology was one outgrowth of the difficulty with the ever-growing number of components in feedback loops of interactions. Erika Check Hayden wrote in Nature, “A new discipline — systems biology — was supposed to help scientists make sense of the complexity. The hope was that by cataloguing all the interactions in the p53 network, or in a cell, or between a group of cells, then plugging them into a computational model, biologists would glean insights about how biological systems behaved. In the heady post-genome years, systems biologists started a long list of projects built on this strategy… So far, all these attempts have run up against the same roadblock: there is no way to gather all the relevant data about each interaction included in the model.” Besides, the complexity of models in systems biology is frequently such as to foreclose any possibility of insight, producing, instead, lists of interactions, writes Hayden.

\footnotesize
\begin{enumerate}
\end{enumerate}
Given this state of biological knowledge, it may seem strange that a prominent group of biologists and engineers is attempting to rewire organisms and produce predictable biological parts at the moment when the complexity of biology seems to outstrip our understanding. In fact, synthetic biology can be seen as a response to the disappointing aftermath of the Human Genome Project. In an attempt to shift gears, many synthetic biologists are consciously adhering to engineering principles which specifically address gaps in knowledge: “You focus on parts of the science that you do understand and clean out the parts that you don’t understand. The paradigm shift is that you treat biology like engineering [and] work with the parts that you understand.”

Working with parts that you understand and cleaning out the rest means assembling, fitting things together, then checking the results. It is a creative activity which pulls together parts of organic systems, along with parts of disciplinary paradigms and ways of knowing. In other words, synthetic biology, in its current form, is engaged in tinkering at every level.

---

Notes on Methods: Objectivities-in-Progress

This dissertation is based on roughly eighteen months of fieldwork in two labs at Princeton University, both of which do research in synthetic biology, albeit different versions of synthetic biology. The fieldwork was conducted between June 2008 and June 2009, and again intermittently between February 2010 and May 2011. My fieldwork involved attending lab meetings, spending time at the lab bench with students and postdocs, and conducting interviews with lab members. On a number of occasions I also provided editorial help with articles in order to facilitate what felt to me like a more equitable exchange, and hopefully learn something about the scientific writing process.

In studying two labs, my hope is to capture both breadth and specificity in the kinds of activities that constitute an ambiguously bounded, semi-structured field of research. I had originally set out to study only one lab. But my original research site along with the persistent buzz of media coverage made it clear that there were competing accounts of what synthetic biology is and how it should be done. When I subsequently got the chance to study another lab doing synthetic biology in another department, I jumped at the opportunity.

The methodological peculiarity of this project stems from its location, in relation to my location, that is. As a graduate student at Princeton, I was never more than a couple of blocks away from the labs I was studying. Once I established connections, this was in many senses easy-access fieldwork. This allowed for three years of highly recursive work, in which the distinction between fieldwork and writing-up was blurred, not by
writing “in the field” but by continuing conversations through the writing process. If I had a question, or was unsure of a certain formulation, I would fire off an email, or chat with a lab member (online, on the phone or in person). In the age of such easy communication with so many field sites, my hunch is that the intertwinement of fieldwork and writing is an increasingly big piece of anthropological method that changes the conditions of production quite radically. Many of my graduate student colleagues skyped and chatted with their interlocutors after completing formal research on formal research grants, in some of the most “traditional” sites of ethnographic inquiry.

The ability to check in, to vet, ask questions, test-drive formulations and delve deeper over time makes writing a dissertation (or a book or article or blog) a more collaborative activity than ever that stretches well into “writing-up.” It is very, very different from the set of concerns which animated the volume, When they Read What We Write. Following the Writing Culture critiques, the reflexive fever in anthropology produced a set of behind the scenes, self-doubt laden, but ultimately almost always triumphant (in the sense of learning from our failures) narratives of fieldwork and ethnography. This particular volume was concerned with the response of literate groups to completed academic work. What is striking about a number of the pieces in the volume is the way they fold the responses of their informants into the ethnographic scene, thereby retaining complete analytic control of their informants’ critical responses to their work. Perhaps, had the conversations that occurred after publication for some of the contributors occurred before publication, the texts which were at the center of controversy would read differently in places. There is something to be gained from knowing what your interlocutors think about what you’re doing as you’re doing it.
Two notions help us identify current modes of anthropological knowledge production: Collaboration, which describes the constitution of knowledge, and objectivities-in-progress, which describes its critical positioning. Rabinow and Bennett make a useful distinction between cooperation and collaboration. Cooperation involves multiple parties contributing to a predetermined enterprise through a division of labor. Collaboration, on the other hand, involves an endeavor in which responsibility is shared and the outcome is not known in advance. Collaborations have conditions of existence. They require some terrain of overlap that draws on the synergic potentials of different paradigms, languages, discourses, viewpoints. They therefore call on the creativity of practitioners, who mold overlapping space.

Collaboration can be further differentiated according to the actors involved and the mode of collaboration they engage in. One mode of collaboration involves anthropologists joining among groups of experts to produce knowledge or outcomes. Such collaborations are collaborations among colleagues. This is the mode that perhaps best describes Rabinow and Bennett’s own engagement in SynBERC, a synthetic biology center at Berkeley. There, Rabinow and Bennett were meant to be full, institutional partners.

Another kind of collaboration is a variation on a more traditional theme. This kind of collaboration expands on the model of interlocutor-based fieldwork, but takes the forging and shaping of the problem space to be a collaborative activity. This is the kind of collaboration that is perhaps more common today in ethnographic practice than in the past. It is heavily based on the making of a shared space for thought and action, and continues to question the lines drawn between anthropologist and informant. In other
words, where anthropologists used to exclude their informants from the knowledge produced (“they don’t understand what I’m doing”) this kind of collaboration seeks domains of overlap. Thus, my project is built on collaboration in reference to specific problems. But these problems are parts of what is overall a cooperative work in which everyone involved knows that I’m writing a thesis (that’s one known outcome) about synthetic biologists (that’s another known outcome) that will be a contribution to (I hope) anthropology (and that’s my responsibility in a division of labor).

Furthermore, such collaborative work still rests on objectifications which would be unilateral were it not for the fact that they often come prepackaged. My interlocutors’ sense, therefore, that “they” can be studied, is part of a menu of accepted objectification that we all agree to bracket at the start, hoping that, at the end, our work will somehow help fill the brackets. When I said, “I want to study synthetic biologists,” no eyebrows were raised, no shrugs elicited. The synthetic biologists I worked with experienced their bracketed field as a pressing problem, a clear object of interest. What was less clear was what anthropologists have to offer the study of anything contemporary. Thus, the question, “Why are you studying us?” was replaced with the same question entirely transformed through emphasis: “Why are you studying us?” For many of our colleagues in the greater university, anthropology is still the study of geographically and culturally isolated groups. Thus the mutual strangeness between anthropologist and informant which accompanied fieldwork abroad in remote places, and frequently disappeared with the saturation of certain field sites by anthropologists, resurfaces with a vengeance at

---

home. Strangeness is therefore a component of work in contemporary field sites precisely because the expertise of anthropologists is believed to lie elsewhere. Building a collaborative space then involves mutual curiosity and interest, and since the starting point is one of estrangement, this requires work.

Collaboration is seemingly very different from Malinowski’s paradigmatic form of participant-observation, in which “only such ethnographic sources are of unquestionable scientific value, in which we can clearly draw the line between, on the one hand, the results of direct observation and of native statements and interpretations, and on the other, the inferences of the author, based on his common sense and psychological insight.” 20 There are implications to Malinowski’s formulation. Observation is objective. The contents of one’s perceptions are separable from one’s perspective, and are reliably “scientific.” But this doesn’t mean that one’s own insights and common sense are subjective in any dismissive or derogatory sense. Rather, such interpretation can be “insightful” and “common sense,” so assessable. So there are perhaps two forms of validity here, one more thorough, one more suspect, and the two are potentially distinguishable and in fact must be distinguishable for the ethnographic account to count as scientific.

The sources of objectivity in ethnography have been thoroughly critiqued. Observation has been the subject of decades of unpacking, as the authorial voice and the positioning of anthropologists, their modes of engagement and esoteric cultural yardsticks made observation itself a form of cultural domination. Yet Malinowski’s distinctions still hold today, and we still (perhaps inadvertently) have some residual faith

in the idea that anthropologists have something to contribute, some kind of truth-telling that runs along side of whatever it is that we get from the field. This despite “a quiet renunciation of any rigorous notion of validity or comparability of fieldwork discoveries.”\textsuperscript{21} We see this most clearly in the frequently posed argument that anthropologists shouldn’t hinge their interpretation on the agreement of their informants. The entire trajectory of American anthropology is premised on the notion that there is something to be gained knowledge-wise from being an outsider. Sure, anthropologists can uncover to their interlocutors dynamics or processes that had been previously concealed, eliciting agreement. But many anthropologists feel that ethnographic work circumscribed by such agreement would effectively be hostage to the self-understandings of the people in question. Thus we hold steady a notion of validity in interpretations at a critical distance from our field sites.

More gutsily, in their edited volume \textit{Being There}, John Borneman and Abdellah Hammoudi systematically dismiss the bugaboos of the writing culture debates, and deliberately endorse a return to critical distance in ethnography, albeit with some nuance. Borneman and Hammoudi’s notion of encounter involves double-edged critique, predicated on critical distance. They write, “Double-edged critiques would require the anthropologist to integrate a more dialectical understanding of historical encounters…that might lead to mutual, intersubjective questioning rather than smug assertions of identity

\textsuperscript{21} John Borneman and Abdellah Hammoudi, Eds, \textit{Being There} (Berkeley: UC Press, 2009) p. 3. See also Carol Greenhouse, \textit{The Paradox of Relevance} (Philadelphia: University of Pennsylvania Press, 2011) p. 38. Greenhouse identifies a paradox in the positionality of the anthropology which took up and responded to the critiques of the late 80s and 90s. This paradox consists in “the rejection of the privileged distinction between observer and observed, yet still alongside a defense of anthropology’s value primarily in terms of “the Other.”
That is, encounter, for Borneman and Hammoudi, involves a certain critical symmetry and distance, and is ideally constitutive of what they call “objectivities-in-progress.” The term is compelling. The authors don’t define it, but I take it to mean something like progressively steadying perspectives which have footing in more than one normative horizon or set of practices. In which case, recursive fieldwork may be one path to such objectivities, alongside of more traditional but perhaps neglected long-term emersion. It is a way of dismantling the “retreat from knowing” which accompanies the end of encounter and the return to ones books, ones thoughts, and ones own knowledge horizons. Recursivity allows anthropologists to experiment with the boundaries of their own knowledge and their interlocutors’, to agree or disagree, or to come to new understandings, to know where and how divergence has happened, and to stand by it. It is not a blurring of perspectives, but a transformation and progressive understanding of their implications. It is therefore part of a measured reinstatement of critical distance, not as the inevitable accompaniment of fieldwork, but as a self-conscious mode of knowledge production.

What does this mean in practice? It means that my sometimes collaborators sometimes interlocutors sometimes informants have engaged with ideas in this thesis. And it means that frequently I avoid treating synthetic biologists “as natives to be explained and [am] more interested in arguing with them,” as Chris Kelty asserted in his ethnography of Free Software. But it also means that in our cooperative mode, I know where they

---

22 Ibid., p. 20.
disagree with me for the most part, and sometimes I know why. It means that their input at various stages was taken as grounds for further work, clarifying what is my contribution, and where I think there is something worthwhile to be said as an outsider. Email, chat, facebook, skype and the like highly facilitate such recursive work and may constitute grounds for kick-starting some reflection on methods in many kinds of field sites. Rather than focusing on the conditions of knowledge in a globalizing world, such reflection should take on the actual temporalities of fieldwork in which asking a question of an interlocutor is a matter of a few clicks, and in which we have open to us written modes of communication—instantaneous like chat, or delayed like email—as more fodder and mediums for thinking through problems. If fieldwork involves a certain kind of disorientation (you really shouldn’t come out of it with exactly what you went into it), writing necessitates some reflection and re-orientation. With the intensification of “post-fieldwork” engagement, re-orientation is a changed enterprise, much more based on exchange, or a conscious decision to abstain from it.

**Anthropology, Novelty and Difference**

Anthropology of contemporary forms, especially scientific and technological ones, frequently focuses on new developments, assemblages and technologies. In this dissertation, synthetic biology is taken up as a source of novelty. Newness for anthropology has been consistently cast as a problem of truth: Is this or that *really* new?
Haven’t we seen this before? We can eschew the problem by taking novelty as a tool aimed at allowing certain juxtapositions and comparisons to come to the surface. For example, in his ethnography of the Icelandic company DeCode genetics (a company which has since filed chapter eleven), Gisli Palsson explains that his use of the term “new genetics” follows a pragmatic motive, aimed at “the task at hand.”  

Palsson uses the definitional flexibility of the concept of the gene as an analogy: “If the vagueness and multiple meaning of the ‘gene’ concept is acceptable, if not essential to the practices and successes of geneticists and molecular biologists (Rheinberger 2000), as central as the concept has been for more than a century of bioscience, why would one want to stabilize or to abandon a term such as the ‘new genetics’?” This is ultimately a pragmatic view of concepts, and one that crosses spheres of knowledge making. As I argue in Chapter Three, Derrida’s reading of Lévi-Strauss highlights the tensions inherent in inevitably ethnocentric concepts. Their role is doubled as they reveal their own limits in their very use, and are always on the verge of being discarded. Such concepts are tools. For anthropology, novelty is perhaps best understood as this kind of tool which brings into alignment anthropological methods and contemporary field sites.

According to the current disciplinary mythology, much of anthropology suffers from a serious case of methodological malaise. Our methods are out of step with many of our modern field sites, whose requirements are distinct from those that defined some of our key tools and techniques. While the objects of anthropological analysis have shifted, the tropes have been less eager to budge. Such is the premise for a recent book of

---

26 Ibid., p. 13.
discussions between Paul Rabinow, George Marcus, James Faubion and Tobias Rees, titled *Designs for an Anthropology of the Contemporary*. An engaging conversation about the state of anthropology today, the book presents an easy anchoring point for observing, tentatively, a powerful isomorphism between traditional anthropological methods and the tendency of anthropologists of contemporary stuff to study novelty. An isomorphism here refers to a formal overlap, a relationship of correspondence. One common critical response to such work has been to focus on the pitfalls of studying new technologies or medicines as they may replace, skew or foreclose understandings of enduring problems or phenomena. Another has been to challenge novelty on existential grounds (is anything ever really new?). Instead, we may ask, what does the emergent or the novel do for us? Said otherwise, what is the methodological value of novelty? This formulation takes advantage of the paradoxical nature of discontinuity or disjuncture, observed by Foucault in *The Archaeology of Knowledge*. Writing of the historian’s task, Foucault points out the dual role of discontinuity as research object and tool for research. Discontinuity organizes a field of observations but also constitutes that field. For Foucault, the hallmark of the historical discipline of his (our?) time has been the attribution of discontinuity as an active concept in the historical enterprise. Foucault writes,

> The notion of discontinuity is a paradoxical one: because it is both an instrument and an object of research; because it divides up the field of

---


which it is the effect; because it enables the historian to individualize
different domain but can be established only by comparing those domains.
And because, in the final analysis, perhaps, it is not simply a concept
present in the discourse of the historian, but something that the historian
secretly supposes to be present: on what basis, in fact, could he speak
without this discontinuity that offers him history—and his own history—
as an object? ²⁹

For anthropology, discontinuity has played a quite different role for most of its
disciplinary history. Its constitution has been largely geographical and ethnological,
though the bounding of such units and the method of rendering them discontinuous also
did something in the domain of knowledge. For the anthropology of contemporary things,
and especially when we study those things that exist right under our noses in our own
proverbial backyards, Foucault’s diagnosis of the discontinuity paradox remains
pertinent. Novelty, as a form of discontinuity, can be construed as both object and
method, and the two are co-constituted. We can therefore emphasize the object side, or
the method side. And if we emphasize the method side, which is what I propose to do
here, we ever so subtly de-reify or de-naturalize its mode of attribution in the
anthropology of the contemporary.

Marcus and Rabinow’s discussions elucidate their respective approaches to the
changing objects of research in anthropology. For some time now, Rabinow has been
putting together guiding concepts with which to pursue inquiry on emergent phenomena

²⁹ Ibid., p. 9.
as part of the anthropology of the contemporary. Marcus’ concern arises more directly out of the *Writing Culture* critiques of the late 1980s.

One agenda item which Marcus brings to the table involves the difficulty of pedagogy in anthropology today.\(^{30}\) He pushes Rabinow on this point: How do categories such as the emergent, apparatus, and problematization connect to the traditional tropes and methods of anthropological fieldwork and knowledge making?\(^{31}\) Marcus’ concern is that without a bridge between the expectations of anthropological knowledge and the conceptual arsenal for contemporary sites, Rabinow’s distinctive tool kit lacks justification as anthropology, and therefore it will also lack disciplinary reach. In response, Rees and Rabinow formulate an implicit place for the emergent as itself part of that tool kit, and provide a powerful analogy between this tool, and its other more common incarnation in traditional anthropology. It is this formulation of the emergent as method which offers a frame for this dissertation. In Rabinow’s lexicon the emergent subsumes novelty, containing in itself some old and some new elements. Since the emergent implicates the novel, I use the two terms interchangeably here.

Marcus presents his concern with the distinctly anthropological through an example of what the anthropology of the contemporary is not. It’s not Habermas. Habermas is attentive to the contemporary, notes Marcus, paraphrasing Rabinow, but only as a means of subsuming it into an existing conceptual framework.\(^{32}\) Habermas’ analytics, such as “reproduction” or “human dignity” swallow up novel developments and questions without being transformed by them. So what then would be the distinctly

\(^{30}\) Rabinow et al., p. 47-53.
\(^{31}\) Ibid., p. 49.
\(^{32}\) Ibid., pp. 66-67.
anthropological approach to the emergent? Rees comments, “It is not the exotic and it should not be the moralistic. Would “foreign gaze” do? I.e., a kind of “foreignness” detached from the spatial connotation of the term?”33 Yet this foreignness seems to be the crux of the problem. As Geertz observed, “Those people with pierced noses or body tattoos, or who buried their dead in trees, may never have been the solitaries we took them to be, but we were.”34 It was the anthropologist who was truly isolated in the field, the real stranger. The problem with the foreign gaze therefore extends far beyond the anthropology of the contemporary, as the truly strange, the truly exotic element in so many fieldwork situations is and has been the anthropologist. What’s more, there may be gradations of estrangement but that external gaze which was generated by distance and difference in the traditional model of ethnographic fieldwork isn’t so easy to generate when mining in our own backyard.

Annelise Riles has been attentive to this situation in her ethnographic work on knowledge forms which replicate our own. The problem that Riles seeks to address is the trickiness of overlapping knowledge forms and aesthetics between anthropologists and the practitioners they seek to understand. The difficulty lies in our own imbrications with the contemporary, which brings us back to the narrative of mismatched methods and objects of study. We are then at risk of producing, on the one hand, lay translations of expert knowledge belatedly (meaning something akin to bad journalism). On the other hand, we may try to analyze the familiar by exoticizing it. Riles writes, “We might understand the impulse of anthropologists of globalization to exoticize through notions of

33 Ibid., p. 67.
community, identity, or tradition, then, as a methodological device, an effort to render the familiar strange so that it may be apprehended as ethnography.\textsuperscript{35} The anthropologist, rather than trading her own strangeness for that of the Native’s, produces strangeness through superficial distance.

This otherness requirement still pervades ethnography, Marcus argues, “Even though the traditional way of constructing the exotic and foreign is gone in anthropology, unless you can produce something like that, and its effects, then you’re not doing proper anthropology.”\textsuperscript{36} Maybe, ventures Marcus, the contemporary yields some of the aspects of otherness, though he quickly notes that this would be simply a remedial move for an underlying problem, concurring with Riles: devices for producing the effect of difference.\textsuperscript{37} Nonetheless, the question remains, how does the contemporary yield otherness? Rabinow suggests that “anthropology of the contemporary achieves this by asking what difference does today make with respect to yesterday—and tomorrow.”\textsuperscript{38} It is in the relationship between difference and time that the contemporary offers up some version of otherness. Rees clarifies and questions, “Could one say that the distinctly anthropological evolves around a difference but this difference is neither spatial nor cultural but rather temporal?”\textsuperscript{39} Rabinow agrees, though he hedges. Still, the insight, if speculative, seems nonetheless generative. If novelty is understood as the unfolding of difference over time, we can see why it would reproduce a desired effect for fieldwork with practitioners who share our norms. As such, it could be construed as one of an

\textsuperscript{36} Rabinow, et al., 67.
\textsuperscript{37} Ibid., p. 67.
\textsuperscript{38} Ibid., p. 67.
\textsuperscript{39} Ibid., p. 68.
arsenal of techniques for wedging oneself back into and between concepts that are otherwise too close to home. Very tentatively, if we were to map out such techniques, novelty and history might be very close to each other. However each carries with it separate risks as to which frames of reference are stretched and in what temporal direction: history introduces the risk of anachronism, novelty is susceptible to projection. From another angle, novelty and history are complementary. Novelty can be understood as that which exceeds history; that which historicization fails to account for. The value of novelty for anthropology is thus construed as a method for revising assumptions, through a comparative exercise.

To put this in the language of post-positivist philosophy of science, stranded within the horizons of theory laden observations, we move forward by posing alternatives that allow us to see things differently, pick out facts and features that other theories suppress. How much access we have to the world through our senses, what kinds of contexts count as normal circumstances of observation, what kinds of impressions may be trusted, these are the sorts of problems we operate through and with and which carry a load of assumptions that accompany our observations wherever we go. Furthermore, languages, cultures, groups, breed interpretations which are not divorceable from some primary moment of perception. We maneuver the world through observational statements which contain within their core certain interpretations. The philosopher of science and methodological anarchist Paul Feyerabend argues that prejudices are unearthed through contrast rather than analysis: Analysis is bound by the very same interpretations and assumptions that we are trying to discover, while contrast may offer a point of reference

---

outside of our limited practices and habituations from which to question a prevailing framework. He writes,

> We need an *external* standard of criticism, we need a set of alternative assumptions or, as these assumptions will be quite general, constituting, as it were, an entire alternative world, *we need a dreamworld in order to discover the features of the real world we think we inhabit* (and which may actually be just another dream-world.)\(^{41}\)

Such dream worlds may be found anywhere, Feyerabend contends. The full diversity of alternate cosmologies is fair game when trying to uncover basic assumptions in scientific work. Such alternative cosmologies are possible worlds that may hang around the corner, never fully realized, but which help us color the otherwise transparent categories and sensual apparatuses through which we see the world. They may also be the most concrete of concrete worlds next door. So why is this anthropology? Because anthropology has long been attentive to the denaturing effects of other cosmologies and other ways of life, at the same time pulling them into frames of intelligibility (what else can it do given that it is always positioned?). Hence Feyerabend delivers a short declamation of the efficacy of voodoo—the standard non-science of philosophers of science who find a view that posits the potential equal merit of conflicting cosmologies unconvincing—with citations to two of Lévi-Strauss’ works: Chapter Nine of *Structural Anthropology* and *The Savage Mind*.

\(^{41}\) Ibid., p. 22.
Synthetic biologists hope to supplement natural systems with their own constructs. As such, their work constitutes a particular stance vis-à-vis nature. Through their imaginative projects and ambitions, “nature” becomes a particular case, one instantiation out of an infinite set of possibilities. Thus, synthetic biologists, through practices of tinkering and assembling, compose possible worlds. In the process, they bring the way we understand nature a step closer to art and to *bricolage*. The anthropology of synthetic biology is thus the anthropology of tinkering with and assembling cells, discourses and knowledges, which simultaneously allows us to approach cells, discourses and knowledges from a different vantage point.

Since I treat novelty as a kind of *dreamworld*, it may make sense to ask, why do we need it at all? Jim Boon has observed, “No science fiction, after all, quite matches the actual variation in human death rites. No utopian dream has outstripped tribal ideals of the intermarriage of clans, perfect reciprocity-plus-difference.” Why then take ones own cultural imaginings as sources of alterity? Because to see biology recomposed at the seams is not the same as seeing it refracted in alternative ways of making sense of living beings. And the great profundity of anthropological thinking, the truism that others are different, sometimes works to stabilize “us” and “ours,” even if we pay for that stability with contingency. Through novelty we catch a glimpse of a social exercise of differentiation, critique, de-ontologization and re-inscription as processes which have to be lived through and with, and are ever-present.

---

This dissertation is organized as follows: In Chapter One, I introduce the labs and individuals who constitute the focus of this investigation. I present both lab heads’ personal narratives which track how each became involved in synthetic biology. Chapters Two and Three focus on the particular relationship between nature and culture from which synthetic biology emerges, and which it may perhaps displace. Chapter Two looks at the relationship between biological knowledge, engineering and synthetic biology. I open this discussion by presenting a set of responses to the work of the famed synthetic biologist, J. Craig Venter. These responses tease out the different orientations of engineers and biologists. In this chapter, I further argue, following Hirokazu Miyazaki and Annelise Riles, that the notion of complexity which biologists appeal to and many synthetic biologists inherit is an effect of the failure of the Central Dogma—DNA makes RNA makes proteins. Chapter Three explores evolutionary theory with its attendant teleological and eschatological concerns, in relation to synthetic biology. In Chapter Three, I argue that synthetic biologists are producing a new myth, one that borrows from evolutionary theory and biology, but also empties these domains of some of their meanings. Chapters Four and Five move into the conceptual work of synthetic biology as an experimental enterprise, specifically addressing uncertainty in concepts and methods. In chapter Four, I argue that experimental concepts are constantly in flux. Experiments, therefore, have conceptual effects, which allow practitioners the wiggle room to interpret, and choose paths for action. Finally, in Chapter Five I examine “the shadow of method:” practices of reproduction and replication which lie just outside of formal method but produce doubt, belief, confidence, anxiety, etc. I develop the role of such practices through religious and legal analogies.
Chapter One

Two Labs at Princeton

There is no single path to synthetic biology. Nor is there a single institutional site in which to locate it. As a roughly defined domain of inquiry which surfaced in the late 90s, age and trajectory play an important role in individual practitioners’ relationships with the field. This chapter introduces the two labs which furnish much of the material for this study. It provides an overview of specific projects, and tracks how both labs’ heads got involved in the research that they pursue.

One common feature to both of the labs was their relative intellectual and geographical isolation from the centers of their specific areas of expertise. Princeton is not a hotbed of research in synthetic biology, nor is it particularly strong in the more general area of biological engineering. The result is that both of the labs described in this thesis were inventing the game as they went along, whereas some of their colleagues were embedding (and embedded in) disciplinary norm-building at the center. For example, MIT, one of the most prominent centers of synthetic biology, offers undergraduate and graduate curricula tailored to bioengineering. While the power centers will most likely lay the groundwork for (economically and intellectually) sustainable research in synthetic biology, they provide a very different view and a very different set of constraints than the ones given by peripheral research labs, plodding along in relative isolation. Furthermore, these centers are being studied by many, many others: sociologists, legal scholars, anthropologists, policy makers, ethicists, etc. Synthetic
biology now has a small coterie of commentators and observers who tend to stay focused on the centers. The centers are the center. They draw money, people, projects, power, students and entrepreneurs.

But the center and the periphery are related. There are collaborations, peer reviews and journals. There are meetings, competitions and keynote addresses. In other words, there is traffic, material, symbolic, ideological and epistemological. And one of the things trafficked, is the excitement of a particular moment, generated by research and ideas, but also by funds and institutional inertia. This inertia was not to be found at Princeton. It was being absorbed and circulated through a wider community and through the more steady investment of institutions like MIT and UC Berkeley.

Weiss’ Lab

I pitched my project to Ron Weiss in May of 2008. Rabinow and Bennett had met Weiss at some conference and had described him as a likely willing collaborator, affable, an interesting guy, and an important figure in the burgeoning synthetic biology scene. Ron invited me to join the lab’s busy summer itinerary without hesitation. Tall, dark-haired, in his late 30s at the time, Ron had a giddiness about research that permeated his activities, and his style of lab management. He was devoted to his work, which he found truly exciting. He once reflected that there was nothing he’d rather be doing. He got up in the morning excited to work, and left his work reluctantly at night. Often, he would return to
his office after dinner, and stay there until well past midnight. The next day, he was at his desk before nine.

Weiss’ lab highlighted some of the distinctive features of Princeton’s foray into synthetic biology. In the eight years that Weiss had been at Princeton, a few major hires in synthetic biology—real coups—fell through. At the same time, motion towards the creation of a bioengineering school, or center, or program, proceeded at academic (glacial) pace. This produced a situation in which Weiss was one of few practitioners in the fledgling field to have both feet firmly planted in an engineering department. Weiss had been trained at MIT in computer science and electrical engineering. His lab at Princeton stood as the lone wet lab in the Electrical Engineering department for a good part of the time he was there, until he and his lab picked up and moved to MIT, abandoning their anthropologist for greener pastures.

Weiss’ lab was comprised of three postdocs, and four graduate students. Ernesto and Cil were the two postdocs in the lab upon my arrival. Ana, a polish protein modeler, joined the lab midway through the year. Claire was a graduate student from Molecular Biology who left for another lab after the summer of 2008. Ting, Saurabh, and Josh were all engineering students in various stages of PhD work. All of the lab members were bench scientists (meaning they ran experiments and got their hands dirty). Ron’s lab occupied two discrete rooms in the electrical engineering department, both of which were impeccably neat, owing, I surmised, to the gruff but supportive presence of the lab manager, Mike. Mike’s fulltime job was to oversee lab nitty-gritty, restock, and make sure that everything was in working order. For the most part, under Mike’s watchful eye, Ron’s lab members followed lab rules (no open toed shoes, no food or drink in the lab,
I wasn’t the only visitor in Ron’s lab in the summer of ’08. For about ten weeks of summer this second lab also contained a gaggle of undergrads. Ron led a team in the International Genetically Engineered Machine Competition (iGEM), a remarkable, bizarre synthetic biology fete where institutionally affiliated teams of undergraduates tackled synthetic biology projects under the guidance of faculty, postdocs and graduate students. The competition involved months of preparation, with students first learning how to build plasmids, the circular pieces of DNA that would allow them to modify the genomes of cells, while also deciding what project to pursue. Weiss led the Princeton iGEM team, along with some of his graduate students and postdocs. The team consisted of eight undergrads and two high-school students (the precocious daughter of a post-doc and her precocious best friend), and their summer schedule was grueling. The competition culminates in a “jamboree” that takes place at MIT in mid-November, where students present posters and give talks, are judged, ranked, and fed ice-cream. Meanwhile, faculty advisors judge projects, network, and engage in some internal politics.

The competition and its build up deserve an anthropologist all of their own (and I’m pretty sure they have one). It is an astonishing event in scale, in experimental scope,
in economic and sociological terms. iGEM was launched in 2004 and gained speed in the following years. In that first year, iGEM consisted of five teams from five universities: Boston University, Caltech, MIT, Princeton University, and The University of Texas at Austin. By 2009, the competition had grown to over 110 teams, comprised of some 1100 students. The funding for individual teams ranges from universities to corporate sponsors, whose logos are displayed on team t-shirts.

The competition goes hand in hand with The Registry of Biological Parts. In 2003, Tom Knight, Weiss’ thesis advisor at MIT, laid the foundations for the registry after frustration with the irregularity of lab practice and components in biological experimentation. The registry is modeled after the extensive TTL Data Book, the electrical engineer’s tool kit for building TTL (transistor-transistor logic) circuits. The biological registry is made up of BioBricks, combinations of genes with known function and DNA segments that allow genes to be easily integrated into plasmids. At the beginning of the summer, each team receives a parts kit from the registry. They also contribute any new parts they produce for their project to the registry. In fact, the iGEM students are a parts-factory. Building plasmids is a tricky, detailed, multistep process, well-suited to division of labor among a team of enthused youths.

It just so happened that Weiss’ team didn’t use the registry, because A) it was infamously unreliable and B) their projects involved mammalian cells, and therefore parts that worked in mammalian cells. This was always a sore point for Weiss when his team was penalized for neither using nor contributing to the registry.

After the iGEM jamboree, some of the undergraduates stuck around to continue work on junior papers or senior theses. I stuck around too, hanging out in the lab,
conducting interviews and providing some editorial assistance on papers. Midway through the academic year 2008-2009, Weiss accepted a job at MIT, and on June 15, 2009, the lab packed up, picked up and left, leaving a few members behind who had chosen not to start anew in Boston.

Weiss’ lab was pursuing a number of projects in synthetic biology simultaneously. Unlike Hecht’s lab, described below, in which a larger project was subdivided among students and postdocs, Weiss’ lab’s projects were mostly discreet attempts, requiring different strategies, different skills and different materials. Some projects relied heavily on mathematical models and computer simulations, while others were almost exclusively experiment-based. But Ron’s lab’s projects, to my knowledge, all involved genetic circuitry and created logic gates out of cells using chemical inputs and outputs. The circuits would hopefully produce predictable behavior. Here I briefly describe two projects in order to demonstrate the flavor of the work: The iGEM project of 2008 and the Turing patterns project. A note to the science weary reader: I have designed this dissertation so that sufficient information is available in subsequent chapters to follow the gist of arguments and discussions.

The goal of the iGEM project, now pursued by graduate students in Weiss’ lab, was to build something called an RS latch or toggle switch out of neuronal cells. An RS latch describes a circuit that has two stable states, and will remain in whatever state it was last left. It is a memory circuit, in so far as it can be used to store state information, and is a fundamental building block of digital electronic systems.

The iGEM team designed a toggle switch using pacemaker cells to excite two separate clusters of neuronal cells. Each cluster, when left to its own devices, secretes a
neurotransmitter that represses the production of a neurotransmitter in the other cluster. The two clusters thus cross-repress each other. When an external inhibitory input is applied to either cluster, the now inhibited cluster stops acting as a repressor. This means that the ‘uninhibited’ cluster is no longer being repressed (its repressor has been inhibited) and it begins to repress the inhibited cluster of cells. Now the external inhibitor may be removed and the system will remain in a steady state. Different neurotransmitters, which may act as excitatory or inhibitory inputs depending on receptors, are used in the circuit, schematized by the iGEM team on their team wiki as follows.

![Image 1: Biological Toggle Switch](http://2008.igem.org/Image:Toggle_Princeton.jpg)

---

The iGEM team first went about making the first batch of neuronal cells out of mouse embryonic stem cells. Stem cells are unspecialized, and can develop into many different kinds of cells. Through genetic engineering, researchers can differentiate the stem cells into the kind of tissue they need. In this case, the iGEM team used a virus which contains a plasmid to integrate a specific genetic sequence into the genome of the mouse cells. Among the genes pasted into the plasmid, the iGEM team inserted a green fluorescent protein and antibiotic resistance for delivery into the cell nucleus. Their goal is not to make glowing killer bacteria. Rather, in the initial phases of replicating the plasmids in bacteria, antibiotic resistance allows researchers to select only those bacteria that contain the desired genetic sequence. When a specific antibiotic is added to the bacteria, the ones that do not contain the desired sequence, and therefore do not possess the gene for antibiotic resistance, will die. The fluorescence is used to confirm that the desired sequence is present in cells that have been infected with the viral vector. Cells that contain the new genetic sequence will turn any number of colors with different properties, available for sale on-line. Aside from a confirmatory fluorescence, iGEMers finally appeal to the phenotype, to the appearance of cells to confirm their success. Their genetically modified cells look like neurons! On their team wiki, they gush “This is really cool.”
Image 2: Neuronal Cells Differentiated from Mouse Stem Cells.\textsuperscript{44}

Over the course of the summer, the iGEM students, along with their advisors, constructed a number of plasmids, and managed to differentiate neurons with a particular receptor. In the days before the jamboree, the presence of the receptor was confirmed in a set of exciting experiments. After the jamboree, the project migrated to the hands of a new graduate student.

A second project in the lab was concerned with producing Turing patterns with genetically engineered cells. Towards the end of his life, Alan Turing turned his attention

\textsuperscript{44} Princeton iGEM team, 2008: <http://2008.igem.org/Image:Results2_Princeton.jpg>
to chemistry and biology. In a very influential paper, he described how patterns in nature could arise out of a certain kind of chemical reaction. Turing patterns are built around something called a reaction-diffusion system. The system requires a chemical which acts as an “activator,” and turns on its own production, and another which acts as an “inhibitor” and inhibits production of the activator. The concentrations of these chemicals then vary in predictable ways across space, creating patterns, like the ones we see on giraffes and leopards and tigers. Different rates of diffusion form different patterns. Many different chemicals and contexts can fit the overall mathematical rendering of the system.

For a long time, there was doubt as to whether patterns in nature really followed Turing’s reaction-diffusion equations. That doubt was dispelled in the 1980s when scientists produced their own Turing patterns in the lab. In this way, scientists produced “true” Turing patterns. Though the simulations produced results that were strikingly similar to patterns found in nature, many scientists were careful not to conclude that they had found the mechanism behind natural patterns. In 2008, however, as I was doing my research, a Japanese team published results of a study on zebra fish, in which they had scarred the tissue of the fish, and watched the patterns develop according to what their simulation predicted. It is now thought that Turing patterns underlie many naturally occurring patterns. Weiss’ project involved engineering Turing patterns out of cells using cellular circuitry.45 The justification for the project in terms of far-off applications was the importance of cellular patterns for the construction of pretty much anything out of cells. A tooth is a pattern of cells. So is a liver. And so are leopard spots. It is also a

45 For a simple description of Turing patterns, see Brandon Keim, “Alan Turing’s Patterns in Nature, and Beyond,” Wired Science News for Your Neurons, February 22, 2011.
project that is heavy in computer models, a kind of activity and engagement with organic systems that Weiss valued very highly.

Discovering Synthetic Biology

“I’ve always been interested in computers,” explained Weiss, whose dad had worked for IBM, and had moved the family from Israel to Austin, Texas, when Weiss was fourteen to take a job with a software company. Weiss attended Brandeis, where he majored in computer science and economics. “I mean, I knew I wanted to definitely find out about computers. And I initially liked economics, but after a while I realized that it wasn’t my passion. Computers were my passion. So I ended up going to grad school for computer science.”

For his master’s degree at MIT, Weiss pursued projects on digital media and information retrieval, with which he could have continued for his doctorate. But he recalled that he didn’t find it to be “life fulfilling” research. For a time, he did research in a field called amorphous computing, so named by two of Weiss’ PhD advisors at MIT in a 1996 paper titled “Amorphous Computing Manifesto.” Amorphous computing looks at computations that involve millions, even billions of components with limited individual capacity, which are coordinated towards shared goals. Researchers in the field use naturally occurring examples of amorphous computations as models. They try to both
understand the local coordination of elements towards a global pattern, and integrate insights into new computational tools.

**RW:** I was looking at biology as a way to get inspired for how to program computers and I was doing all kinds of simulation and things like that. There was a point when I came to the conclusion that rather than wanting to look at biology as a way to get inspired for how to program computers, I actually want to reverse the arrow and say how can I look at computing and understand how to program biology. And at that point I teamed up with the person who ended up being my main advisor, Tom Knight, who’s actually one of the visionaries in synthetic biology. He’s one of the people who started the field. So I ended up having three advisors. And I helped him set up a wet lab in the computer science building. From then on I was working on bioengineering.

Weiss here described a Copernican Revolution-type inversion, a Eureka moment which meant changing course significantly. To get started, Weiss, his advisor and a fellow graduate student had to learn how to work with cells. They were completely unfamiliar with wet lab work. They started by taking some undergraduate courses in biology, reading papers and books and talking to people. Mainly, Weiss, explained, “we just picked it up as we went along… and just tried things. It was probably not the most efficient way to learn, but it’s kind of the MIT way. You can figure it out by yourself and you don’t need anybody else.” MIT, in Weiss’ accounts, was consistently cast as the Mecca of DIY. The institution, claimed Weiss, was supportive of innovative work done
under little or no surveillance. “At MIT they are very open. They like crazy ideas. Doing something nontraditional is something that people enjoy. You pretty much assume that you can figure it out by yourself and you don’t need anybody else.” Indeed, MIT was incontrovertibly the epicenter of engineering-based synthetic biology at its inception in the late 1990s and early 2000s. Weiss was a graduate student at the time, under the supervision of three advisors. His account of those early days is one of excitement and trepidation.

The first technical hurdle was the production of the first plasmid. Weiss remembered the frustration of those months, and the doubts which accompanied the daily lab work: “I was thinking to myself, maybe I shouldn’t have switched to do all this biology stuff.” At the same time, he described the actual lab work as soothing. He recalled having had to learn how to focus and check himself in order to do experiments. In computer science, he observed, if you make a mistake, you can go back and fix it. In biology, it doesn’t work that way. Mistakes usually mean having to repeat specific protocols or entire experiments. One has to be diligent and attentive. It took Weiss six months to confirm his first plasmid. A week later, a biologist the lab had hired for assistance arrived on the scene. Weiss realized the value of this new lab member quickly: “She was a biologist. An actual biologist. Which is something we should have done week one. We should have had an actual biologist.”

That first plasmid was a piece of what became a major part of Weiss’ dissertation project: a digital logic circuit made out of cells. This, along with an oscillator, digital memory, and cell-cell communication, were the ideas with which Weiss ventured into experimental work. The cell-cell communication project, Weiss argued, was directly in
line with his engineering background. First of all, he observed, for computer scientists, “networks are everywhere.” If they wanted to be able to build networks, it was clear that they would have to figure out how to control the way cells talk to each other. This was the obvious first step for a computer scientist working with organic systems. The digital logic circuit and the cell-cell communication together comprised the bulk of Weiss’ PhD thesis, which he completed in 2001.

Next, Ron went on the academic job market. He had spent nine years at MIT, and had thoroughly enjoyed being a graduate student, but his family had expanded, and the time had come to move on. His PhD thesis had been noticed as prominent, original work. Ron continued directly from MIT to Princeton’s electrical engineering department, where he had received tenure and continued to do research, teach and publish, while actively expanding his network of relations in the synthetic biology community. My year in Ron’s lab was his last year at Princeton. In their final group meeting at Princeton, I helped lab members pick the linoleum tiles for their new MIT lab.
Hecht’s lab

Some months after Ron’s lab had relocated, I met Michael Hecht, Professor of Chemistry. He had been slated to become the next master of Forbes College, where I, along with my partner, took part in a resident graduate student program. Weiss had suggested I speak to Hecht months earlier. Hecht had been part of Princeton’s bioengineering school initiative. He was a fringe member of the synthetic biology community, and a reluctant card carrying chemist, whose lab did truly exciting work with synthetic genes and proteins. Medium build, medium height, and in his early fifties, Hecht embodies professorial charm with fast speech and fast wit. His story telling acumen and convivial presence made for easy fieldwork. The first time we met, Hecht in fact interviewed me for reappointment to the residential program at the college. We talked about synthetic biology, about the big questions about life and the little questions about molecules, and visa versa. From there I started attending lab meetings regularly, interviewing his lab members, and hanging out in the lab.

Michael’s lab tackled two main projects: One on Alzheimer’s disease and one in synthetic biology. The lab had two postdocs, both of whom worked on version of the synthetic biology project. Icky, who was from Israel, worked with yeast. Betsy joined the lab weeks before I started attending lab meetings, having finished her PhD at Yale, and quickly became synthetic biology central command (I mean that in the best of ways). Maria and Mia were graduate student who worked on the synthetic biology project. Mia joined the lab sometime in the Spring of 2011, perhaps a casualty from another lab. Angela was the lone graduate student working on the Alzheimer’s project. Both projects had been around for a while, and had generated good results. They therefore comprised
whole experimental systems which offered up relatively discreet research questions to be
tackled by graduate students and postdocs. This was one major difference between Weiss
and Hecht’s labs. In Weiss’ lab, graduate student and postdocs were, for the most part,
working on unrelated projects. Michael’s lab, in contrast, had a team of researchers
tackling different parts of overarching projects. In both labs, undergrads contributed
significantly to the lab’s research projects. Sarah, Charlotte, Cara and Dani were all
undergrads working on the synthetic biology project, and they all made progress in their
particular areas of investigation.

At a cocktail party for the retirement of the old master of Forbes College, Michael
Hecht and I got a chance to catch up. His paper with students (undergrad and grad) on
their synthetic biology project had just circulated back to them with peer review
comments, and they had been busily crafting a response. It was my first look at the paper,
which was subsequently published in the Public Library of Science (PLoS), an open
access peer-review journal. An account of the paper gives a sense of the positioning of
Hecht’s lab’s work.

The paper begins with Hecht’s major point of entry into synthetic biology:
synthetic biologists are getting closer to constructing synthetic organisms from “off-the-
shelf” parts. The parts, however, have relied on sequences that already existed in nature.
Hecht’s team asks “Must the toolkit of life be so restricted?” The sequences of naturally
occurring proteins represent a tiny fraction of possible sequences. Natural selection, they
write, has whittled down the potential diversity to a relatively small collection of
sequences that sustain life. One may therefore suppose that these proteins that have been
selected for are in some respect special. Hecht’s team claims to pose this question
experimentally. Can their own sequences, which are not naturally occurring and were designed in the lab, support life?

The first step in asking this question experimentally is procuring the artificial genes encoding for sequences of amino acids. The proteins must fold into stable three-dimensional structures, so the sequences cannot be derived at random. Instead, a binary code strategy is used to make sure that stable structures are achieved. The position of polar and nonpolar elements is restricted but the content of those elements may vary within that framework. The result is a large collection of proteins with predictable shape and size.

In the section titled “results and discussion” the experimental design is more thoroughly described. To test the ability of the synthetic proteins to substitute for naturally occurring ones, Hecht’s group used specifically designed bacterial strains called auxotrophs. Auxotrophs are strains of bacteria with mutations that render them unable to perform essential functions for life. The auxotrophs used in these experiments are able to survive in enriched media (bacteria food) but not in nutrient poor media. Hecht’s group used the Keio collection of auxotrophs, which contains *E. coli* strains that have had single genes knocked out and were nonetheless able to survive on nutrient rich media. Twenty-seven auxotrophs were tested, first with a control gene (or empty vector), to make sure that they were indeed auxotrophs.

“As expected,” state the authors, the auxotrophs transformed with the empty vector failed to grow on nutrient poor media. The auxotrophs transformed with the synthetic gene sequences also failed to grow for the most part on nutrient poor media. Four of the auxotroph strains, however, were rescued by the synthetic proteins. The
authors conclude that the \textit{de novo} sequences can compensate for the deletions in the modified \textit{E. coli} strains.

For one of the auxotroph strains that were rescued, Hecht’s group isolated a particular novel protein that facilitated cell growth. In the other three cases, however, several sequences were able to rescue the strain, suggesting, claim the researchers, that the functions are “relatively easy to achieve.” What’s more, careful study of the synthetic proteins suggests that they are unlikely to be found in nature.

Since the synthetic proteins were neither selected through evolution, nor designed by computer, they are unlikely to perform as well as naturally occurring proteins. The experimental results show that the ability to rescue strains was dependent on high-level expression systems. As expected, strains expressing the natural protein grew better than ones with the synthetic proteins. The synthetic proteins did, however, enable some growth, while the same strains transformed with the empty vector failed to grow at all.

The next step in the analysis, as presented in this paper, is to establish through what mechanism the synthetic proteins are operating: are they providing the same function as the deleted gene or are they operating in some other way? The researchers proceed by a process of elimination, considering three different ways in which the rescue might be occurring, and then knocking each one out experimentally.

The next move in the paper proceeds a bit like a circus act. Having accomplished four separate feats, the researchers will now attempt to achieve all four at the same time! Having rescued four strains, each lacking one essential gene, Hecht’s group attempted to rescue a strain with all four genes knocked-out simultaneously. The resulting strain shows severely hampered growth even on rich media. The quadruple knock-out strain was
then genetically engineered to contain the genes for four synthetic proteins shown previously to individually rescue the one knock-out auxotrophs. While the control with the quadruple-knockout failed to grow on nutrient poor media, the strain transformed with the synthetic proteins achieved some growth. Hecht’s group concludes that synthetic proteins with no similarity to naturally occurring ones can substitute for a significant chunk of the *E. coli* genome. For Michael, this is a step on the road to synthetic life.

**Falling Into Synthetic Biology**

When I asked Michael Hecht how he became a scientist, he smiled wryly and said, “not the normal path.” He applied to Cornell as an intended psychology major. “I wanted to be a shrink,” he explained. He quickly discovered upon arriving at Cornell that a psychology major didn’t involve clinical psychology or therapy, and the plan was abandoned. Hecht took part in the college scholar program at Cornell, which freed him from having to choose a major or fulfill university requirements. After his sophomore year, he decided to take a break. So he went out to route 80 in New Jersey and hitchhiked around the country for a while, sending his Cornell undergraduate advisor the occasional postcard (“Here I am in the Grand Canyon!”). In November, it became too cold to keep up the tour, and Michael returned to Cornell. The fall semester was already underway, and the spring semester was three months away. It was flipping burgers or lab work. Michael turned to his undergraduate advisor, who called everyone he knew in the chemistry department.
Finally, he called the famed chemist Harold Scheraga, who agreed to have Hecht in his lab.

In Scheraga’s lab at Cornell, Hecht took part in work that was at the interface of chemistry and biology. He enjoyed the work, though he still wasn’t sure what he wanted to do for a living. After college, Hecht read James Watson’s textbook and was fascinated by the molecular biology of the gene. He described the book as a precipitant in his decision to go to graduate school in molecular biology, though he quickly added, “I really was pretty darn clueless.” Between Cornell and graduate school at MIT, Hecht spent some time driving a cab in New York and living in Alaska. His last summer in Alaska was spent in the very remote Arctic Wildlife Refuge. One week later, he was in a classroom at MIT, and thoroughly disoriented.

He eventually joined the lab of a young Professor who was doing early work in protein folding and protein engineering. “I had a really good graduate experience,” recounted Hecht, “because [my advisor] was amazing, and the project was amazing.” He finished graduate school, got married, and spent half a year driving around North America in a camper van, and the other half traveling in Nepal, Burma, Thailand, a little bit of Egypt and a lot of Israel. In the meantime, Hecht had applied for a postdoc with David and Jane Richardson at Duke. Jane Richardson originated the now ubiquitous ribbon diagrams for proteins. She had received a BA in philosophy with a minor in physics from Swarthmore, and an MA in philosophy from Harvard. She eventually joined her husband, David, in his work studying the three dimensional structure of the staphylococcal nuclease protein. Though she never pursued a degree in the sciences, she was awarded a MacArthur fellowship for work in biochemistry. Today, she is the
president of the Biophysical Society, and continues to run a lab at Duke with her husband. Hecht described Jane Richardson as a visionary, a word he chose carefully. “Jane… could actually visualize [proteins] as these ribbon diagrams. She could visualize the whole structure. And people used to say that the protein database was a computerized version of Jane.” After traveling around the world, Hecht landed in the Richardson’s lab as a postdoc.

After the postdoc, the Princeton chemistry department recruited Hecht. In his early years at Princeton, Hecht published an influential paper in Science on a technique for constructing proteins using binary codes of polar and non polar elements. The technique was included in a textbook the year after it was published. It was a big deal, on the heels of which Hecht received an offer from another university. He went up for early tenure at Princeton with some assurances of a positive outcome from department members, and was denied. This was the first in a series of disappointments stemming from an altogether unpleasant tenure process, which dragged on for years. Before that, he explained, “it was all just falling into things, it really was: undergrad, grad, postdoc, faculty, until all of a sudden, when tenure came around, it didn’t fall into place.” In narrating the tenure debacle, Hecht gets visibly uncomfortable. It’s not a time he likes to relive. Eventually, he received tenure, and the following six years were very pleasant and allowed him to focus on research. And this is when he started working on a project that he would later come to call a contribution to synthetic biology.

In his account of research projects, Michael’s career trajectory is a logical progression of questions that build naturally on one another. At Cornell, he had done protein folding in the lab of Harold Scheraga, and then at MIT he had experimented with
protein mutagenesis. “[At MIT] with Bob,” explained Michael, “we had mutated lots of amino acids. What’s the next conceptual step? You mutate every amino acid, i.e. design from scratch. So that was a logical step. So I did that with Dave and Jane Richardson.”

With the Richardsons, Hecht had designed one protein at a time. His new binary code for protein design allowed him to design whole libraries of proteins that folded predictably. “The proteins folded, I got tenure.” Next, the question was whether these binary coded proteins could function.

MH: So you do all sorts of biochemistry experiments to see if they can function. We did that. And then what I always knew because I trained in Bob’s lab at MIT as a biologist, I said, well, all that doesn’t matter, what really matters is, Do they function in vivo? And so for a long while we were trying to get them to function in vivo by taking a particular strain that was defective for a particular activity and getting it to rescue. And then one day I heard a seminar somewhere where somebody mentioned this Keio collection where you can get all the strains that are deficient for all the activities, and I said, that’s the way to do this, because if we’re trying to rescue one thing, it may or may not work, but if you have 4000 to try, something’s going to work. And so that’s what we did.

That was how Hecht’s lab had gotten involved in testing their synthetic proteins on the Keio collection. The stepwise motion of Hecht’s research journey all left their traces in the auxotrophs project: the protein folding, the protein design and engineering, and the
binary method for building protein libraries. Now they were testing the viability of these proteins as supporters of life.

All this came before synthetic biology had made a mark as an approach or aim or method to which researchers attached themselves. Then synthetic biology came along and, Hecht recalls, “I said, oh, we’re doing that.” He quickly adds, “but we’re doing more synthetic biology than they are.” It’s a sort of taunt that Hecht repeats often and which simultaneously marks out his lab’s research as different from the synthetic biological mainstream, and asserts its importance in that milieu. It is a taunt directed at the circuit based synthetic biology practiced by Ron Weiss and his engineer colleagues, and it is a critique of BioBricks as the basis of biological engineering “from scratch.” These engineers, argues Hecht, are using parts that are already given in nature, and then assembling them in novel ways. In other words, Weiss’ approach amounted to bricolage: repurposing parts that have been given in advance, stockpiling a set of pre-constrained tools and then combining them in different configurations according to the job at hand. If you want parts from which to build life “from scratch,” Hecht explains, you have to create novel genes which code for novel proteins. That’s what his lab is doing.

Hecht similarly critiques J. Craig Venter’s synthetic genome, described in the coming chapter. He took the press’ coverage of Venter’s work as a testament to the role of power and money in scientific credit. But in his view, Venter’s team had also failed to build something “from scratch.” They had merely replicated what already existed. All of the cells produced by the Venter transplants mimicked existing life forms. In a talk in 2011, Hecht argued the point:
MH: Several months ago, in the spring, there was this big press release and big
to do when Craig Venter and colleagues came out with what they called the
creation of an artificial cell. That was copying natural sequences and placing
them into a cell. It was synthetic in the sense that they knew the information
from biology, they copied it, and they put it in the cell. Not really synthetic
biology in my view.

Their own project, Hecht said, “is in principle way more profound than what Craig
Venter is doing because we’re using things made from scratch.” On another occasion
Hecht explained, “What we're talking about here is the possibility of going many steps
beyond that.” But he was also jaded by the difficulty of convincing others of the
profundity of the work. “I’m not sure they’ll see that,” he appended. “It has taken forever
to get that paper out, which has been kind of painful. Obviously it’s hard to be distant
from one’s own work. It’s been frustrating.”
Chapter Two

Denaturing A Discipline:

Knowledge, Complexity and Synthetic Biology

Of the three approaches which make up synthetic biology mentioned in the Introduction, it is the second approach involving synthetic genomes which has garnered the most media attention, both as a technological achievement and as a source of possible ethical and regulatory dilemmas. It therefore has played a major role in popularizing synthetic biology. That attention and popularization is in no small part due to the celebrity and notoriety of its chief practitioner, J. Craig Venter. Venter’s work began producing drastic results the year before I started my fieldwork, and continued to produce a buzz at regular intervals throughout my time in the two labs. Both Weiss and Hecht commented on Venter’s work, sometimes publicly. Venter’s work drew Weiss into the very public regulatory discussion. For Hecht, Venter was a powerful competitor benefitting from celebrity and resources. For the purpose of this thesis, I use a set of popular responses to Venter’s work to introduce the different sensibilities and aims of biologists and engineers. These aims are further used to unpack the pervasive charge that life is too complex to be built from scratch.

In the summer of 2007, the science press reported excitedly that Venter’s new Institute had transplanted a genome from one species of bacteria into another. The significance of the achievement was hard to pinpoint and prominent synthetic biologists...
commented—in the popular press, lest we miss the irony—that they hoped the importance of Venter’s gene transplant wouldn’t be exaggerated. For the most part, the synthetic biologists I spoke to, as well as those who volunteered or were asked to comment on the matter publicly, played down the importance of Venter’s work. But the genome transplant proved nonetheless a cultural and regulatory milestone for synthetic biology, significantly increasing its visibility. Since then, Venter’s team has made additional advances, each setting off a new wave of responses.

Venter’s legacy is still in the making. Perhaps the most prominent figure in the now decade old completion of the Human Genome Project, Venter’s pursuits consistently garner attention, and some amount of trepidation. He is the second most frequently mentioned scientist in a recent recombinant DNA textbook.46 His ventures have jump started or accelerated regulatory debates, partly because of his chomping-at-the-bit patent approach, and partly because the oft used descriptor “maverick” for his personal ambition and style does not fall in line with the requisite caution we like to see from scientists much less scientist entrepreneurs. The New York Times recently dubbed Venter “the Richard Branson of the Lab,” conjuring an image of millionaire adventurer CEOs seeking profit and changing the world in one stride.47 Forbes magazine, meanwhile, called Venter “the Bono of Genetics.”48 He is, no doubt, a charismatic figure, a force of sorts in the genomics world, and a popularly recognizable presence in a biological endeavor that has attempted for a century to erase the traces of the hands that have shaped it. Stephen

47 New York Times Topics, July 14, 2009
Hilgartner summarizes the sentiment aptly when he writes of Venter, “With his countless admirers and critics, he is probably the closest thing biology has to a Hollywood celebrity.”\(^{49}\) Taken together, these analogies tell us something about Venter’s status for commentators and colleagues alike: he is a figure to be apprehended by means of analogy.

Venter is also the poster child of the “mapping paradigm,” a legacy of the 20\(^{th}\) century genetics program. In their introduction to a volume on the metaphor of mapping in genetics, Rheinberger and Gaudilliere note that knowledge about the genetics of living beings has been organized around two metaphors: information and mapping.\(^{50}\) The mapping paradigm of the genome era, which produced linkage maps and high throughput gene sequencing remained remarkably stable in terms of overall approach in the last decade of the 20\(^{th}\) century, despite a Moore’s law scale of acceleration in output. Mapping consistently embedded certain kinds of questions, and produced particular kinds of information. At Celera Diagnostics, the company at which I did fieldwork in the early 2000s with Rabinow, the term “mapping paradigm” as a label for what had gone before them was used by company execs to differentiate their own research from the profitless drudgery of their predecessors. “Mapping paradigm” was an insult.

Venter founded The Institute for Genomic Research (TIGR) in 1992, after leaving the NIH on less than friendly terms. In 1995, TIGR announced they had sequenced the genome of the bacterium Haemophilus influenza, and the Institute has since remained at


the forefront of small microbe genome sequencing. Venter joined Celera—the private player in the genome sequencing race—in 1998 with the hopes of sequencing the entire human genome. His substantial research acumen at Celera was not matched by business success, and Celera’s stocks fell as investors doubted the profitability of genome sequences. Venter was fired in 2002 and subsequently returned to TIGR. He then launched three more non-profits: One focused on genomics research, another on synthetic biological research with the ultimate goal of producing renewable energy and biofuels, and finally one dedicated to procuring funding. Under Venter’s guidance, the three nonprofits merged in 2004 to form the new J. Craig Venter Institute (and again, the analogies proliferate, one observer having dubbed the center the closest thing to Willy Wonka’s chocolate factory that genome science has to offer), and absorbed the reluctant TIGR in 2006. 

The J. Craig Venter Institute, in turn, claims the lion’s share of Synthetic Genomics Inc., a company that seeks to discover and commercialize alternative fuel sources.

---

Step One

In Venter’s own estimation, the work on synthetic genomes produced the first noteworthy result in 2003, prompting heightened attention from Department of Energy (DOE) and the federal government. The DOE had in fact funded the research that led to the synthesis of Phi-X174, a bacteriophage that infects *E. coli*. Bacteriophages are viruses that specifically infect different kinds of bacteria. The synthetic bacteriophage project was a test bed for the technological and conceptual approach that would eventually be used to synthesize other, larger genomes. For Phi-X174 to be infective, its genome must be assembled with incredible accuracy. Failed attempts—and there were many—produced molecules of the right size that would not infect bacteria because the genetic sequence was off by a few base pairs. Interestingly, Venter insisted that the team sequence the failed attempts as well, to locate the errors in the genetic sequence, and to “take the guesswork out of the science.” The choice to track failures is evidence of ample resources, as well as easy access to sequencing machinery.

In the meantime, a team of microbiologists at SUNY Stony Brook, led by Eckard Wimmer, had successfully synthesized poliovirus. Wimmer had been the first to decode the genetic sequence of the virus in the early 1980s. In the early 2000s, he and his team followed the blueprint he himself had provided in constructing the synthetic virus.

In September of 2003, Venter’s team, led by prominent microbiologists Hamilton Smith and Clyde Hutchison, had successfully synthesized an infective Phi-174. The difference between Venter and Wimmer’s achievements was largely a temporal one.

Wimmer’s strategy took months; Venter’s could take about a week. The technique had been practiced and refined, and the synthesis of *Mycoplasma genitalium* was next on the agenda. In the meantime, the capacity to make viruses in the lab prompted concerns that contributed to the creation of the National Science Advisory Board for Biosecurity.

Nicolas Wade—mine canary of popular science—wrote of the J. Craig Venter Institute’s genome transplant in late June of 2007. In the intervening years, much had happened in the circuit-oriented synthetic biology. In 2003, Tom Knight and Drew Endy, both engineers by training, founded the Registry of Standard Biological Parts. Along side the registry, MIT held the first International Genetically Engineered Machine Competition (iGEM). Even with all of the buzz about MIT engineers and satellite operations all over the place, nothing hit the big time the way Venter’s next announcement did. Wade’s article tracked the first of a number of high profile press conferences followed by extremely technical publications in top peer review journals. Scientists at Venter’s institute purified the genome of one kind of mycoplasma and “transplanted” it into another. The new host cells then expressed proteins specified by the donor DNA.

The official journal publication explicitly coins the term “genome transplant” to describe the transfer of genetic material from one bacteria to another. The transplantation produces cells with the genotypic and phenotypic characteristics of the input cell. The exchange of genomes is achieved without any recombination, completely replacing one genome for another. Venter’s group gestured towards ethical and regulatory questions but insisted that, since so far they had used an existent genome, these initial steps didn’t pose any risks. Nevertheless, even this first step didn’t escape scrutiny owing to the very
artificiality of its products: Perhaps synthetic versions of organisms that already exist are particularly unpredictable. Besides, the Venter group was steadfastly adhering to a trajectory which quickly led away from genomes found in nature to fully synthesized human-made genomes. The call for regulatory attention was therefore immediate. In July of 2007, discussions of the need for governmental regulation echoed through the science press. “No formal regulations exist for synthetic biology, and some say that’s got to change, stat,” reported Brandon Keim for Wired Magazine.\(^5^3\)

Synthetic biologists had already launched into pre-emptive ethical discussions and had largely agreed to endorse self-regulation. Such discussions, weighted heavily by the interests of researchers who preferred not to be regulated upstream, tended to favor regulatory models that left them free to play. The formal NIH regulations followed by synthetic biologists stemmed from the recombinant DNA debates of the 1970s. Beyond these regulations, governmental organizations had engaged in some discussions of bioterror threats, but the conversations had not addressed synthetic biology as posing or raising a specific set of risks or questions. Prominent synthetic biologists piped up that these thirty-year old recombinant DNA regulations would do the job if tweaked enough to accommodate contemporary conditions without hindering research. Venter had perhaps foiled their plan. His reputation as an egotistical profit seeker and a mover and shaker of science, combined with the nature of the research produced a sense of regulatory urgency. His press conferences and press releases, on the heels of the DNA transplant from one type of bacteria to another, fanned the flame. His high-profile output was accelerating. His autobiography came out in late 2007, perfectly timed with a

scientific breakthrough/publicity stunt: the sequencing of an entire diploid genome belonging to, who else, J. Craig Venter. And so, around the same time, the pressure for government attention and regulation started to mount.

**Step Two**

In January of 2008 the J. Craig Venter Institute announced the creation of a synthetic genome from scratch. They had reached another milestone in their pursuit of synthetic life. Researchers at the Institute had stitched together 147 pages-worth of nucleotides, 582,000 base-pairs. The parts of this genome were provided to Venter’s team by commercial sequencers in 5000 to 7000 base-pair segments, to specifications of course. The key achievement was the method of gluing these segments together, which was cleverly left to the natural prowess of yeast. “We consider this second in our three-step process to create the first synthetic organism,” explained Venter. “What remains now that we have this complete synthetic chromosome…is to boot this up in a cell.”54 This second step opened the door to the construction of robotic cells. Now, the two steps would be put together. A synthetic genome “booted-up” in a bacteria would equal synthetic life. This, at least, was the favored narrative, the cinematic build-up coming out of the Venter camp and propounded by its enthusiasts.

The synthetic sequence itself was a slightly modified version of the *Mycoplasma genitalium* genome. The modifications contained coded messages, “watermarks,” exhibiting the human origin of the sequence. The “watermarks” were embedded in the letters for the amino acids coded in the DNA and were decoded by Wired Science in collaboration with government scientists, to reveal the names of the researchers on the project. Venter and colleagues had signed their creation.

**Step Three**

In 2008, when the creation of the synthetic genome was announced, Venter expressed his expectation that a successful genome transplant using a synthetic genome would be completed in a matter of months. But booting-up a synthetic genome in a bacterial cell turned out to be no walk in the park. In 2009, Venter’s group published the results of experiments which showed progress, and elaborated new techniques, but also reflected difficulties encountered along the way. First of all, the *Mycoplasma genitalium* took too long to grow. For every experimental attempt, researchers had to wait a month for results. Second, their synthetic genome was rejected by the host cells in their first attempts at bringing their experiments together. After racking their brains, Venter’s team figured out that proteins called restriction enzymes were getting in the way of the synthetic genome transplant. They also switched out *Mycoplasma genitalium*, opting instead for its larger and speedier cousin, *Mycoplasma mycoides*. 
It wasn’t until early 2010, therefore, that the experiments were successfully put together to create bacteria with synthetic genomes. By that time, two-and-a-half years of constitutive work had gone into producing living cells with synthetic genomes. The final attempts were approached with caution and hope. Daniel Gibson, a researcher on the Venter team, recalled the last try: “We all had a very good feeling that it was going to work this time. But we were cautiously optimistic because we had so many letdowns following the previous experiments.”

Indeed, in May of 2010, Venter announced that the first “synthetic cell” had been successfully engineered. A New York Times journalist wrote that Venter “Displayed all the savvy graciousness of an actor accepting an Academy Award.” The cells and their celebrity creator drew equal attention. The Venter team had managed to transplant their synthetic DNA into host bacteria, and those bacteria had produced proteins coded in their new DNA, and divided. The new engineered bacteria were almost identical to the natural version of the donor bacteria, with one crucial difference: the watermarks. Beyond the names of the researchers, the watermarks contained lofty quotes: “See things not as they are, but as they might be,” courtesy of Oppenheimer; “What I cannot build, I cannot understand,” written by the physicist Richard Feynman and quoted ceaselessly by synthetic biologists; and finally “To Live, to err, to fall, to triumph, and to recreate life out of life,” by James Joyce. Two physicists and a writer. Some public embarrassment accompanied the watermarks, when it was observed the Feynman had been misquoted. Caltech even sent Venter a picture of the board on which Feynman had scrawled, “What I

---

cannot create, I cannot understand” (emphasis added). Finally, Venter was forced to admit the mistake and has promised to “edit” the watermark.

The papers for all three major stages in the project trace mind bogglingly tedious procedures. Each step can be broken down into lists and lists of protocols, careful recipes that call for cooling, heating, centrifuging, measuring, purifying, etc., and often have to be tried and retried for better results. So many things can go wrong even with the standard kits provided by biotech companies. So many more things can go wrong with biological experiments in unknown territory. “This was a debugging process from the beginning,” Venter recalled. “99% of our experiments failed.”

**Biochemical Magic**

Right on the heels of the announcement of the second step in Venter’s synthetic life trilogy, in a sea of pronouncements and dramatic commentaries, Alexis Madrigal wrote a concise and illuminating piece comparing her own response to the Venter work, which reflected that of more engineering-oriented synthetic biologists, with that of her colleague, science writer Carl Zimmer. In “Artificial Life? Old News,” Zimmer wrote of his own less than exhilarated response to the Venter team’s second big announcement. First, Zimmer claimed that not since the Middle Ages had humans believed that there was

---

something fundamentally different between living matter and non-living matter. Furthermore, ever since the synthesis of the organic molecule urea, the barriers for building life have been technical rather than conceptual or philosophical. Second, Zimmer argued that we don’t stand to learn much from the Venter team’s shot at synthetic life. “When and if Venter’s team does create a viable synthetic life form, our ignorance will remain profound,” contended Zimmer.\textsuperscript{58} He wrote,

> There is a lot we don’t understand about life, of course, but Venter’s project isn’t going to answer all the questions. We are a long way from playing God. The scientists didn’t assemble the fragments of DNA by themselves, nor did they program robots to do so. Instead, they injected the fragments into \textit{E. coli}, and let the bacteria do the job themselves. Eventually, it turned out that \textit{E. coli} could only build up a quarter of the genome. The scientists don’t quite know why. So they injected those big chunks of Mycoplasma DNA into yeast. Lo and behold, the yeast were able to finish up the job for the scientists. They don’t quite know how the yeast did their own biochemical magic either…\textsuperscript{59}

Zimmer’s editorial pointed to the gaps in knowledge. Venter’s work would not reveal much of anything that wasn’t known before he undertook the project, and so the project is not worthy of its hype. His was a stock biologist’s response to the announcement of an engineering feat. The normative assumptions of biology hold that for research to be

\textsuperscript{59} Ibid.
meaningful, it has to explain something or describe a process, or help us understand something *about biology* that we did not previously understand. Zimmer therefore downplayed the importance of Venter’s work from a particular disciplinary perspective, and his lack of enthusiasm was a common feature of many biologists’ responses, for much the same reason.

Madrigal’s comments, on the other hand, outlined an entirely different way of conceptualizing achievements and assessing their importance in synthetic biology. Her response made clear that there were perhaps new rules to this new game, which of course borrowed heavily from engineering disciplines. The standards of assessment for synthetic biology, with their mimicry of engineering, meant that Venter’s work could be assessed according to a different set of norms. According to these norms, if it works, it works. Engineers seek to operationalize biology, at the same time black-boxing what they don’t understand, and frankly may not need to know. Madrigal explained,

> In Zimmer’s column, there is a purpose, a teleology, to the study of biology: “a deeper understanding of life.” But for many synthetic biologists, that’s not the primary point of their work. Synthetic biology is to biology what electrical engineering is to physics. In the latter case, both fields involve electrons, but they don’t necessarily have the same goals and can’t be measured with the same yardsticks. Instead of asking, “What have you learned?” or “What do we understand?” we can ask, “What have you made?” and, “How did you make it?”

---

The purported role of understanding comes up again and again in discussions of synthetic biology and represents a common formulation of the difference between biology and synthetic biology. Synthetic biologists want to build things and they want to be able to control what they build. One key for control without understanding is the method of black-boxing. As long as you know what comes out of a system, given a certain input, you don’t have to know the mechanism by which the system produces that output. Venter’s stitching yeast are a black box. They’re not sure how the yeast do what the yeast do, but they can use it to produce certain results. For synthetic biologists, contributions to a descriptive understanding of how biological systems work are at most, and despite some contestations, secondary, if not largely incompatible with an engineering approach.

Having staked out the engineering norms by which Venter’s achievement may be assessed, Madrigal’s engineer interlocutors still play down the scale of the achievement. As an engineering feat, they argue, the Venter genome is not all it’s cracked up to be. The method Venter’s team used is slow and expensive and therefore not conducive to large-scale production and integration into genomes. Chris Voigt, UCSF synthetic biologist, provided a whimsical analogy:

There’s this great computer in the MIT museum. There’s this one computer sitting in there and it is the most intricate woven set of wires. It looks like a rug almost but it was hand put together. That represents the last point when one person could sit there with Radio Shack components
and build the best computer in the world…\textsuperscript{61}

Madrigal concludes, “In other words, we just witnessed the end of the beginning for biological engineering.”\textsuperscript{62} There’s a good dose of irony in this conclusion, given Venter’s role in the race for the human genome sequence, and the subsequent eulogy the race inspired for sequencing and mapping techniques when the sequence didn’t translate into an obvious source of value-added. Venter is always somehow pushing a paradigm to its logical conclusion, and therefore his work seems always simultaneously cutting-edge and dated. The “watermarks” speak for themselves. The gesture is opposed to the ambitions of Weiss and some of his engineer colleagues. If all goes well with standardization, efficiency, and user-friendliness, they hope to disappear.

\textsuperscript{61} Chris Voigt, as quoted in Alexis Madrigal, 2008. 
\textsuperscript{62} Ibid.
A Synthetic Biologist’s Encounters with Biologists

Hoping to disappear from one’s work is an engineer’s fantasy. Scientists opt for invisibility over disappearance. For a long time, objectivity in the sciences meant that the traces of human subjectivity were carefully separated from the content of the scientific pursuit. In contrast, the kind of disappearance Ron dreamed of had to do with seamless, user-friendly function, modeled after the computer industry.

The problem for engineers like Weiss was that organic systems came rapped up with the disciplinary norms and expectations of those who had previously studied such systems. Additionally, while switches and oscillators provided a new set of tools with which to tinker with biology, they did not provide a full- fledged alternative (as yet) to existing descriptions of organic systems. The challenge, then, was to sort through biology on a need-to-know basis, and to deliberately displace biological thinking with what Weiss called “higher-level thinking” or “abstractions.” These were techniques for moving away from the discursive, formal and disciplinary apparatuses of molecular biology, and they came mostly from engineering disciplines.

For many biologists and critics, these were just attempts to bypass detail and complexity, and futile ones at that. What’s more, the kinds of aims which synthetic biologists set out for themselves were often nonsensical to audiences of biologists. The sensed tensions between biologists’ and engineers’ perceptions of the right level of detail and appropriate goals surfaced clearly in Weiss’ autobiographical narrative, in which he described his job talk at Princeton.

In general, Weiss’ PhD work was very well received by engineers and computer scientists. It made a splash, and was intuitively interesting to these audiences, even if
applications with therapeutic or practical value were still a long way off. In contrast, convincing biologists that there was any merit in programming cells proved a difficult task.

**RW:** A lot of people didn’t know why we were doing this. People didn’t understand what it means to program cells. But I remember when I was giving my PhD defense, the room was packed with people. A lot of them were computer scientists and the way I thought about things was actually very easy for computer scientists to understand, and that’s still true today. So I give in my talk a ten-minute brief introduction for understanding the vocabulary and some of the underlying concepts, but then people in computer science pick that up very quickly. So they may not know all the details but they understand why I’m trying to do this. They understand where I’m going with this. Engineers I think caught on pretty quickly. People who like to create new systems get this very quickly. But people who study natural systems have a hard time understanding why you would want to create new genetic circuits, for example. It’s been hard, you know, especially when you create small circuits that don’t do anything, that just flash on and off. It was really hard to convince biologists about that. That took a very long time—way beyond my PhD.

Weiss draws a fault line between people who like to produce new circuits and people who study natural systems. Flashing cells made sense if you like building things. But the issue was not necessarily that biologists didn’t understand why one would build
something, but rather why one would build something so useless. The scale of the systems produced by synthetic biologists was unintelligible to audiences of biologists. Weiss perceived it as an inability to extrapolate, to understand the implications of small-scale advances on a larger scale. But it is hard to divorce his perception of the causes of misunderstanding from the self-evident value which games and devices possess for communities of engineers.

Having gotten the cell-cell communication project and the digital circuit to work, Weiss went on the academic job market. He applied mainly to computer science departments. Many of these departments recruited biology professors to assess Weiss’ job talk. But the content of the talk largely concerned programming languages in biology, and was intended for computer scientists. Princeton’s was in fact the lone Electrical Engineering department in the mix.

Weiss reiterated his sense of resistance from biologists in the early days. He noted that if it were up to the biologists who were attending his job talks, he did not think he would have gotten a job. He recalled giving talks to biologists at MIT and Princeton around 2001-2002 that fell flat, in which he felt that the impetus for the work was simply not understood. I asked Weiss why it wasn’t just a matter of tying the likes of tic-tac-toe to some future application. “You would think it would be easy but it’s actually not that easy,” he answered.

**RW:** Especially within a scientific talk, you say all sorts of grandiose things, but then that doesn’t make a connection. And at the end of the day they say, well, regardless of what you say, what did you do? What circuit did you make? What
proteins did you use? What organism are you doing this in? A lot of times they cannot break away from looking at the details to understand the main concepts. Some people are able to do that very well, and a lot of people in biology have a hard time doing that. The fundamental problem in biology is that in order for people to understand systems a lot of times they have to keep in mind the properties of every individual protein. A lot of times it’s hard to abstract away from the details of the individual pieces, because the details matter.

Professor Jim Broach, Weiss’ main collaborator in the Molecular Biology department pointed out that, up to a certain point, synthetic biologists and biologists overlapped in their methods and aims. Beyond that point, their goals diverged. Biologists, he explained, did not understand why you would want to make a bull’s eye pattern out of cells, or program cells to play tic-tac-toe. Broach explained that synthetic biologists needed to get past the ‘talking dog’ phase. People were still impressed with the novelty of the fact that they taught a dog to talk, but at some point one had to start caring what he’s saying. In this sense, synthetic biology was still unproven. It also will have to have some value added to stay within engineering, Broach argued. If it becomes “tinkering for tinkering’s sake” somebody will get sick of footing the bill. Nonetheless, Weiss recalled that in 2005 he gave another talk to an audience of molecular biologists at Princeton. He described it as “one of the most fun talks he had ever given,” having felt that the audience was fully on board, engaged, and asking the right questions. Weiss argued that there was
a palpable shift in the way his work was received in the three years that elapsed between the first and second presentation for molecular biologists.

**Biological Knowledge as Troubleshooting**

Right as I was starting fieldwork in Weiss’ lab, I ran into one of his graduate students, Josh, in a parking lot in Princeton. We stopped to chat and I told him that I was a bit nervous about knowing enough biology to study high level molecular tinkering. Josh, a second year graduate student at the time, who had majored in applied physics at Caltech, replied, “You don’t have to know much biology to do this stuff.” I was slightly perplexed by this response at the time, but quickly realized that there was an entire nexus of issues nestled in this proclamation; one that related to a distinction introduced by Andrew Pickering in a piece on cybernetics.

There is a lab at Irvine run by one Garnet Hertz, reports Pickering. In this lab, researchers are building biological computers. What does that mean? In this case, it means a cockroach-controlled robot, in which the roach acts as the computer. Sensory feedback involving light tailored to the specifics of cockroach sensory apparatuses allow the cockroach to maneuver the robot. Below is an image of the robot, taken from the lab’s website. A roachbot, if you will:
For its inventors, the roachbot raises a question about will and world: Is the roach controlling or being controlled? For Pickering, the roachbot signals an alternative to the pervasive, uber-modern paradigm of technology as “the domination of nature through knowledge.”

He writes:

We can note that centuries of engineering and science go into the manufacture of valves, chips and electronic circuits in general. The human race has had to accumulate an enormous amount of material infrastructure, knowledge and expertise to put us in a position to build and program computers. Hertz instead needed to know *almost nothing* about roaches to build his machine. The roach remains, in a cybernetic phrase, a *black box*,

---

known only in terms of its performative inputs and outputs—it runs away from light, and that’s it. If the construction of a conventional robot entails ripping matter apart and reassembling it in accordance with our designs, hertz’s involves a different strategy: the entrainment of the agency of nature, intact into a human project, without penetrating it by knowledge and reforming it…We can see two different stances towards matter in play here: the conventional one that involves penetrating black boxes through knowledge, and the cybernetic one that seeks to entrain boxes that remain black into our world.65

The latter was one path that cybernetics followed, especially in its British variety. The hallmark of technology as a function of power/knowledge is design: the taking apart and reassembling of characterized components, understood through an analytic approach. Against such a paradigm, Pickering poses the mid-century work of Stafford Beer on automatic factories. Automatic factories, or ‘lights out’ factories, represented the dream of total automation: factories that not only mechanized production, but also moved materials around from machine to machine, making human involvement completely superfluous. Beer argued that in order for the automatic factory to adapt to changing market conditions, thereby avoiding extinction, it would have to have something of a brain. But just what kind of brain could this cybernetic factory have? Beer reflected,

65 Ibid., p. 472.
As the constructor of machines man has become accustomed to regard his materials as inert lumps of matter which have to be fashioned and assembled to make a useful system. He does not normally think first of materials as having an intrinsically high variety which has to be constrained...[But] we do not want a lot of bits and pieces which we have to put together. Because once we settle for [that], we have got to have a blueprint. We have got to design the damn thing; and that is just what we do not want to do.66

For Pickering, Beer’s words parse out the cybernetic vision from modern technoscience. The two constitute distinct paradigms. The former describes a more symbiotic relation between project and components (echoes of bricolage). “If a digital computer does not execute the operations we envisage for it, something has gone wrong. Biological computing entailed a much more symmetric relation between the human and the nonhuman—a ‘conversation’[...] a ‘compromise,’ in which human performances and goals, the specifics of management, were themselves liable to open-ended transformation...in relation to...performative black boxes.”67

The distinction is rich and illuminating, though it neglects the intrinsic mixed media of the roachbot. Perhaps, rather than dichotomies, we can speak instead in terms of a spectrum of hybridities. The cockroach is integrated into a machine that presupposes a thoroughly modern kind of technology involving domination through knowledge. What we have is indeed a hybrid being, not just in the sense of man and machine, but also in

67 Ibid., p. 485.
terms of the technological paradigm: part black box, part electronics. This hybridity between knowledge/power on the one hand, and what Pickering calls *entrainment*, is precisely the medley at play in synthetic biology, where cellular processes have replaced neural networks as the sources of regulation and feedback. But synthetic biology would be towards the other end of the spectrum from the roachbot: lots of power/knowledge aided by some *entrainment*. That is, there is no question that design and control are the desired end in synthetic biology, and symbiotic, *symmetrical* relations are somewhat precluded by domination through abstractions. One could then argue that it is precisely the case that synthetic biologists are digitizing natural computers: breaking them down into parts and building them up again according to a blueprint or design. This is certainly one fitting narrative. But it only gets us halfway there. Synthetic biology reflects the specter of a cybernetic vision through biological input/output performatives nestled in feedback networks. Hence Ron’s ultimate goal: systems that evolve and respond to changing environments. Or the common concern for the *entrainment* of “emergent properties.” Or Venter’s deployment of stitching yeast (and the attendant critic biologists consequently crying foul because the work wasn’t based on understanding), or Adam Arkin’s marveling at all the rich modules out there just ready to be played with, discussed in a later chapter.

The distinction also allows us to reflect on these relations of knowledge/power in the social sciences, and critique Pickering’s normative stance. Pickering’s distinction builds on Heidegger’s disillusioned take on technology. Pickering therefore poses *entrainment* as a slightly romantic potential stance vis-à-vis technology. If we try to map this distinction onto the operation of power and knowledge in social institutions, we find
that the two co-exist there too. Entrainment nuances the knowledge/power dyad. In fact, the hybridity of power/knowledge and *entrainment* in the operation of social institutions has created logics of governance that are fundamentally strategic and statistical. These logics seek to define a *norm* rather than understand the causes of behavior.\(^68\) That norm, like the projected sense responses of the cockroach, is then used to calibrate governance. This kind of power/entrainment has a scary edge to it (apparent in the living being strapped into the machine as its brain). The question then is how is the cockroach modified by its very engagement in the hybrid.

Another problem for such a hybrid vision is that black boxes can only remain black boxes as long as they can be made to interface with designed networks, and work consistently under diverse circumstances. When they don’t work, practitioners have a few options: tinkering, trial and error (which is a ubiquitous and oft lamented part of synthetic biological research in its current state) and research aimed at debugging through understanding. Understanding is then a *side effect*, unwanted knowledge which nonetheless goes into the stock of such knowledge (who hasn’t learned more than they ever wanted to know about an appliance that stopped working?) It would be better to get along without it.

The relationship between biological knowledge and synthetic biology, therefore, was a matter of contention. When Weiss’ advisor, Tom Knight, first started tackling biological systems at MIT, he filled his shelf with biology books, and attended molecular biology courses at MIT. His collaborators and heirs build a legend of the recent past for

their newly-formed discipline, which relies on biological knowledge, but then takes an unexpected turn:

Many engineers, often trained to deal with complex system, saw a new opportunity to join in biological research at the cellular and molecular levels.

But some who entered were taken aback at the complexity of natural biomolecular systems. Luckily, a few researchers learned enough molecular biology to realize that instead of directly grappling with nature’s inherent complexity, simpler synthetic gene ‘circuits’ could be manually crafted using the tools of genetic engineering. For example, genetic analogs of basic electronic circuits were engineered and natural organisms slightly rewired, with an optimism and excitement similar to tinkering with a radio and with resultant building of confidence and framing of new scientific questions.69

After these founding fathers (there were no mothers at the time) learned “enough” biology, they abstracted from it. They built a language to bypass complexity, either by black boxing it, or by focusing on higher level motifs and big picture properties of biological networks.

For Weiss, the discipline of biology had gotten bogged down in the details and had set the standard at that level, in place of other higher level approaches to organic

---

systems. The needs of synthetic biologists, and their run-ins with biological knowledge and knowers, made clear that the two disciplines were encountering some friction where they interfaced. Weiss was critical of biology as a discipline:

**RW**: There’s over emphasis on the details [in biology] and sometimes the big concepts are not emphasized. And there are some people who are trying to change that, for example here in Princeton, there is something very good going on with efforts for integrated science and technology modeling and high-level concepts in biology, as opposed to memorizing like the krebs cycle or something like that.

**TDC**: But you are dependent on those people too right?

**RW**: In general, I’m heavily, heavily dependent on the biological community to make discoveries which then I can use to engineer new things with new capabilities and making discoveries myself while at the same time engineering new capabilities. So without the knowledge that has been gained in biology none of this would be possible at all. Without biology there wouldn’t be any bioengineering.

**TDC**: But you think if they were to teach higher-level concepts they would continue to produce the kind of knowledge that you need to proceed?
**RW:** When you do things more abstractly it doesn’t mean that you don’t still collect the information. So you need to collect the underlying information to then be able to abstract from it, so I’m not saying not to do the collection, I’m saying, after you do the collection of the data, be able to abstract away from that about what’s really going on in the system. And I think there’s a general tendency now to try to do that. I think that if you compare the field now versus ten years ago there’s definitely changes. And there are definitely people who are leaders in trying to make that happen, trying to make that transition, so it’s in different places, it’s very uneven, and in different places you have the leaders that are pushing for that transition to happen. So the information needs to be there, the information needs to be accessible, but maybe a computer should store it or something like that.

**TDC:** As opposed to an undergrad brain.

**RW:** Right, which then goes away. It shouldn’t be like a folklore kind of passing of information by the fire using smoke signals and things like that. It should be a much more, I don’t want to say rigid, but structured activity of collecting data, putting it into databases, being able to access the data, having it be accessible in machine format so that you can have software that actually looks at the data and assembles it and allows you to make all kinds of claims or hypotheses that you can then test out using computation, using modeling, using abstractions.
Weiss’ position on the organization of biology highlights points of tension in the transmission and use of information, from biology to synthetic biology. James Collins, Professor of Biomedical Engineering at Boston University explained in a journalistic interview, “…Engineers are really good at building things. We can build things even if we don’t understand how every detail of the system works…” The viability of this view relies on the translation of a couple of basic engineering concepts to cells. Scalability and modularity are particularly important for synthetic biologists. Scalability refers to the capacity to put parts together into larger assemblies and still know what they’re going to do. Modularity means that parts contribute independently, yet predictably to a whole.

Work in Weiss’ lab, as in many synthetic biology labs, was oriented towards functionality, and functionality often relied on some knowledge of biology. But the exact balance differed for different practitioners, and different projects. Cil, who came from a molecular biology background, insisted that there was a fine balance between biology and engineering. She argued that some synthetic biologists too heavily entrenched in engineering failed to understand the importance of biological detail, which should ideally be integrated into designs, rather than ignored. She and Josh represented the two ends of a spectrum in Weiss’ lab.

Cil’s approach to biological knowledge, in practice, reflected the pressures and expectations of engineering. About five months into my fieldwork, I sat with Cil in the lab one Tuesday afternoon as she was running some experiments on neurons for the toggle switch project that had been differentiated two weeks earlier. We were staring at a short video of neurons which had been produced out of mouse stem cells, and given a

---

70 James Collins, as quoted in Breithaupt 2006.
dopamine agonist. At different times in the video, a number of neurons rapidly become brighter, and then lose brightness, producing a flash of light. Cil exclaimed, “that’s so cool!” I asked her what she thought that was and she said she has no idea. She then continued, “If I were in a biology lab, I would study that, run some experiments, try to figure out what it was.” The statement was delivered with a slight tone of longing. Cil’s curiosity would have to be quelled elsewhere. In Weiss’ lab, the experiments were goal-oriented, the desired effect was or was not achieved.

Cil had picked up an iGEM project from 2006. The project, which involved diabetes, had since become quite elaborate. Weiss had a diagram on his corkboard which tracked the different parts of the grand plan. Cil was pursuing one crucial thrust of it. The iGEM students, in their brainstorming sessions for the 2006 competition, had decided they wanted to create muscles out of stem cells that could move an object with a contraction. They were subsequently divided into two teams, and each team tried to produce one kind of muscle out of stem cells. At the same time, a postdoc in a collaborator’s lab suggested that the team pursue a project on pancreatic cells that produce insulin (beta cells), based on a paper that had just come out, in which researchers had managed to turn embryonic stem cells into insulin producing cells. The process described in the paper was long (about five weeks to go from stem cells to insulin producers) and required a whole host of growth factors, administered in a particular order over the course of cell differentiation. The iGEM leaders subsequently set to work, along with the team, trying to figure out how to devise a beta cell pathway.
Cil: So the students went and looked at papers and with our guidance they found some evidence that there are certain cell-fate regulators—cell-fate regulators are the transcription factors that push cells toward a particular fate—that could almost certainly push these cells to make insulin. So if we could somehow turn these cell-fate regulators on in the cell, possibly in a programmed fashion, maybe not, maybe we can get very high efficiency of cells that produce insulin. Because the paper that shows that you just add these cocktails of growth factors comes out with maybe ten to fifteen percent of the cells producing insulin. Not a very high efficiency, but it was a dramatic increase over stem cells that just randomly differentiated… And then after the summer ended, the experiments continued…

In the meantime, Cil’s funding had run out and her job was on the line. She remembered the constant specter of unemployment that summer, as she went about her work. One day, she received an email from Weiss, telling her to put together a presentation on the beta cell work. They had found possible funding. Cil described her lack of preparation and training for the task at hand. Biologists are usually specialized by tissue type or pathway, whereas the synthetic biologists in Weiss’ lab were more like jacks of all trades, as long as there was a literature to review and someone who would sell them the materials. Cil was a trained neuroscientist.

Cil: And so here was a neuroscientist who knew nothing about endocrinology told to become an expert in beta cell biology in five days to speak to experts on
beta cells. Just a little pressure. For my job. Because as soon as he said that I knew this was for my job… And so I was very grateful.

The presentation went well, and Cil secured the funding, and her job. And so the beta cells project became her primary project in the lab. After more journal research, Cil and Weiss came up with a plan to differentiate stem cells into beta cells in a two-step process.

Cil: So into the literature we go, and we look at what the iGEM students found, we looked up all the different papers involving the differentiation process and found that this process is almost certainly governed by gata factors: gata 4 gata 6, sox 17… So we had plasmids with gata 4 already so I started the process of making endoderm and it was pretty dramatic. We saw the cells turn from these nice little balls to what’s called a cobble stone effect. The cells almost look cuboidal and they’re bright green. And it is too cool. It’s just a really clear effect.

Though the results were exciting, the cells were differentiating into the wrong germ layer for pancreatic beta cells. Nonetheless, what Cil had done had been a first, and only later did she realize both its audacity, and its significance. A year and a half after she had done this work, Cil took a course on stem cells,

Cil: And they tell you that you can’t directly change stem cells into any of these tissues, and I’m like, yes you can. No you can’t, you have to serum starve them,
make them ball up, and then you can differentiate them. And I said, no, you
don’t have to do that. And they were in total shock. So our ignorance of typical
stem cell biology played to our advantage.

It was clear from the trajectory of the project, that biological knowledge played a crucial
role in enabling research. Without it, projects didn’t get off the ground. But Cil’s
description made clear that gaps in knowledge could produce unexpected results where
thorough research would produce doubt, if not discouragement.

This is distinctly a story of at least partial experimental success. It is the
exception. When constructing circuits in the lab, desired effects are rarely achieved on
the first few tries, and among the things that intervene between subsequent attempts is a
purported lack of knowledge of biology. Biological knowledge, and by this I mean
descriptive characterizations of biological systems and their dynamics, was amassed at
the outset, and then largely at impasses, through research in scientific journals. In effect,
research in molecular biology for these engineers often took the form of troubleshooting,
a term I heard used frequently when experiments failed to produce the desired behavior.
**Complexity**

Synthetic biology has a mixed track record in its short history. While practitioners have had some success with building circuit components, assembling these components into larger systems has proven quite the hurdle. The resistance of organic systems to predictable behavior in large assemblies is usually glossed as stemming from complexity: “We don’t have a systems level understanding,” “There are too many unknown interactions,” These are some formulations of the doubt which surrounds the field. One biologist offered this criticism of synthetic biology: “An engineer’s approach to looking at a biological system is refreshing but it doesn’t make it more predictable. The engineers can come and rewire this and that. But biological systems are not simple... And the engineers will find out that the bacteria are just laughing at them. If you have incomplete knowledge then it is highly possible that you are up for a few surprises.” Complexity is taken to be something that inheres in biological systems, which introduces discrepancies between the modularity of engineering parts and the performance of such parts made of cells. Synthetic biologists have likewise marveled at biology’s complexity. One group lamented, “Our understanding of physical laws and knowledge of material properties allow us to engineer bridges that do not collapse and car engines that convert energy into mechanical motion. Engineering biology, however, is different. Even the simplest bacterium comprises a system whose complexity is humbling.” Or you have, “However,
with all its complexity and interconnectivity, biology has for many years been recalcitrant to engineering manipulation.”

Complexity is therefore an issue both for my analytics and for my interlocutors’, and it raises two problems. The first is the tendency to see complexity as an independent quality, rather than a relational one, both in anthropological knowledge and in the kinds of knowledges anthropologists study. The second is the accompanying move towards more expertise on the part of the anthropologist or practitioner, with the goal of capturing more of that complexity. In my first pass at writing about my field experience with synthetic biologists, complexity became a self-referential, redundant concept, wherein my analytics showed that synthetic biologists’ usage of complexity was in fact complex. The problem was that there was no clear scheme by which to differentiate between the artifacts of my own analytics and the products and techniques of theirs. I attempt to unravel this redundancy by pointing to the underpinnings of complexity in synthetic biology, following Miyazaki and Riles’ lead in their piece, “Failure as an Endpoint.” In this piece, the authors deliver a strong critique of anthropology’s current analytic repertoire. They write,

…[T]he focus on “emergence,” “complexity,” and “assemblage” implicitly resigns itself to the fact that little can be known about the world except for the fact of complexity, indeterminacy, and open-endedness, since reality, in this view, is always emergent, indeterminate, and complex.” Anthropological analytical strategies, “in response to the apprehension of the endpoint of their own knowledge, […] retreat from

---

knowing. And they also retreat from the recognition of the failure of their own knowledge by locating indeterminacy and complexity ‘out there’…

In other words, complexity in this case is a consequence of the failure of analysis which hides in the purported complexity of what is to be explained. However, complexity is out there in the failure of our interlocutors’ analytics. In the setting of synthetic biology, therefore, the maneuver Riles and Miyazaki point to, in which complexity is located ‘out there,’ becomes itself a good ethnographic tool. It becomes the basis for an analogy between anthropological complexities and biological ones. When theories fail, they have effects. For anthropology, the analytical failure of theories has made complexity appear everywhere as a feature of social worlds. In synthetic biology, the failure of a certain set of representations of biological systems has also perhaps generated complexity as an effect.

Complexity talk with reference to biology is ubiquitous today, but its scope and ascription, I would like to suggest, are matters which require clarification. What is it, exactly, that’s so complicated? The laughing bacteria seem to suggest that it is organic systems in themselves which are complicated and evade the synthetic biologist. Another possibility, one that I find more compelling, is that theoretical and analytic failures have forced biologists to turn to a notion of complexity. A quick, admittedly simplistic, history will help clarify this point.

In the 1941, George Beadle and Edward Tatum famously formulated the one gene-one enzyme hypothesis. The hypothesis held that each gene is responsible for the

---

production of a single enzyme, and was greatly bolstered by the identification of DNA and the characterization of the double helix by Watson and Crick in the 1950s. Now the stage was set for finding direct correlation between sequences of DNA and sequences of amino acids in proteins. Indeed, in the 1960s, scientists were able to decipher the three-base pair code for a particular amino acid. The understanding of the genome now held that the genetic code was a sort of script from which life was read off quite linearly. By the 1970s, this elegant and simple model was thought to be missing a few key distinctions. Jacob and Monod proposed the existence of regulatory genes, whose main function it is to control when and how genes are transcribed and regulate protein production. In the following decades, a whole slew of regulatory functions were discovered in the genome. Far from one gene-one enzyme, we now had an entire arsenal of active genetic sequences that do not necessarily code for amino acids, and could be located far away from the enzyme-coding sequences they regulate. Furthermore, in the late 1970s, the discovery of split genes further undermined any notion of discrete sequences carrying out discrete functions. Genes are, frequently, not discrete units, have variable effects, don’t all code for proteins, and the list goes on. But that fact remains that, despite such difficulties, for a long time genes produced results, and provided some form of conceptual traction for tinkering with biological systems. Francois Jacob observed the enigmatic efficacy of genes, when, after a perusal of the central processes we don’t understand, he pointed to the astonishing achievements of genetic engineering and manipulation: “We have learned to cut and to knot DNA almost at will, to delete and to insert fragments. We know how to isolate certain structural genes, to mass-produce them and analyze their anatomy down to the last detail. All this work on recombinant
DNA is in a way the triumph of our one-dimensional biology. It provides a new tool both for the study of fundamental problems and for many aspects of applied biology.”

The mountains of data generated by high throughput gene sequencing, however, have solidified the sense that the simple model of genetics is deficient. It turns out humans and fruit flies have roughly the same number of genes, however those are counted, a fact that has been frequently cited to highlight the likely possibility that there is a lot going on in regions of DNA where geneticists previously thought nothing was going on. They are now haunted by that once common phrase, “evolutionary junkyard” referring to purportedly non-coding regions. “The result,” explains Margaret Lock, “is that we have entered an era, almost overnight, in which the “dark” parts of the genome are starting to fluoresce.”

Hannah Landecker notes that the life sciences today are shifting away from an emphasis on DNA. “Whether we identify this shift as postgenomics, metabolomics, proteomics, epigenomics, or stem cell biology, the linearity of the central dogma—DNA makes RNA makes protein—is being corrected by the elaboration of other complex temporal and spatial relationships between biological molecules that are ordered by the structure and function of the living cell.”

Indeed, in order to incorporate discoveries into an understanding of how genomes work, the account has had to increase significantly in complexity. But this complexity is deeply relational, and tied to the simplicity it has come to challenge, or even replace.

---

78 Landecker, p. 4.
Biological systems are complex in so far as they exceed and undermine the model of gene action around which molecular biology was built. The relational aspect of complexity drops out of the equation when geneticists and molecular biologists address the gaps between our knowledge of genetics and organic systems. The result is that the failure of simplicity in biology is now being naturalized and attributed to the complexity of organic systems. Complexity, which has so frequently been tied to depth, may have no phenomenal depth of its own in many of its deployments. We are left with always multiple complexities in relation to different norms and measures, or different failures of representation, and we lose complexity as a unitary phenomenon.

If we now come back to synthetic biology, we find that what is resistant to the language and knowledge practices associated with circuits, switches, oscillators and transistors, may not be some notion of organic systems in themselves, but rather an entire language and experimental apparatus which incorporates biological knowledge and the objects it posits, and renders them complex. In other words, biological disciplines have mapped out our knowledge of organic systems. Synthetic biology attempts to render organic systems through alternate tropes and methods. But in order to do this, its practitioners negotiate a place for this new descriptive language in relation to biological knowledge, on which they explicitly, but also inadvertently, build, through which its practitioners think, and within the confines of which they work. They, after all, engage in gene talk, and mine papers for information which, even in database form, would still be the outgrowth of the particular model which dominates the study of biology. In this sense, synthetic biologists of Weiss’ ilk are playing with a second order semiology. They are constructing a second order of significations out of an existing language that purportedly
describes the natural world as it is.

The language of gene-as-code is of course itself already operating at two levels: language and biology. In his short and sweet lectures on myth, Lévi-Strauss addressed the echoes of systematic coherence which fit different planes of reasoning. He commented on the genetic code, “[I]t is well known that, when the biologists and the geneticists had the problem of describing what they had discovered, they could do nothing better than borrow the language of linguistics and to speak of words, of phrase, of accent, of punctuation marks, and the like. I do not mean at all that this is the same thing; of course, it is not. But it is the same kind of problem arising at two different levels.” That consistency makes one system of thought, borrowed and bent like the pieces of a kaleidoscope, carve out new space with combinatorial agility. This is how order is introduced into the material of observation, and appears as though it has been there all along.

The sources of breakdown between synthetic biology and biology proliferate, perhaps, because the fit between circuits and cells or genes does not map onto representations, on the one hand, and the natural world, on the other. Biological knowledge therefore leaps up in the impasses not just because “there are too many unknown interactions” between cells in circuits, but because there are too many unknown interactions between the symbology of genes and the symbology of circuits, as two ways of organizing the world.

Let me return though to Josh’s proclamation: “you don’t have to know much biology to do this stuff.” Josh’s response was meant to sooth a nervous anthropologist.

---

My initial goal was to learn some biology as I went along, to supplement what I had learned doing a project on genetic diagnostics as an undergrad. The plan was to learn on my feet according to what I needed to know. But this intention to learn on my feet itself replicated an aspect of the specific condition of my field site. That is, it was more or less what everybody in Ron’s lab was engaged in. We were all involved in an attempt to find traction with our analytic and descriptive systems, applied to new domains, which were deeply saturated with existing understandings and practices.
Chapter Three

Changing Teleology

“Natural Selection, as we shall hereafter see, is a power incessantly ready for action, and is immeasurably superior to man’s feeble efforts, as the works of Nature are to those of Art.”

—Charles Darwin

In his sweeping history, The Age of Capital, Hobsbawm recounts the achievements in the sciences that characterized the third quarter of the 19th century. This period saw the publication of Darwin’s *Origin of Species*, a work which, Hobsbawm notes, provided a historical explanation in terms which were widely familiar and widely in use: “[T]he Darwinian theory of evolution was impressive, not because the concept of evolution was new—it had been familiar for decades—but because it provided for the first time a satisfactory explanatory model for the origin of species, and did so in terms which were entirely familiar even to non-scientists, since they echoed the most familiar concept of the liberal economy, competition.”

As Marx famously wrote in a letter to Engels,

---

It is remarkable how Darwin recognizes among beasts and plants English society with its division of labour, competition, opening up of new markets, “inventions,” and the Malthusian “struggle for existence.” It is Hobbes’s bellum omnium contra omnes,” and one is reminded of Hegel’s Phenomenology where civil society is described as “spiritual animal kingdom,” while in Darwin the animal kingdom figures as civil society.

It’s a great circle: Nature in the image of the human, and the human in the image of nature. Yet the difficulty with evolutionary theory always manifested itself in the conspicuous absence of a rational chooser in a theory of rational choice. Whereas Adam Smith’s invisible hand was a metaphorical aggregate of rationality, Darwin’s survival of the fittest sublimated rationality entirely in the evolutionary mechanism. The problem of teleology (and theology) was therefore written into evolutionary theory from the very beginning.

Moreover, the significance of the theory of evolution, notes Hobsbawm, lay in its breadth, which reached far beyond biology, to the ultimately definitive role of history over all of the sciences, misunderstood by Darwin’s contemporaries in terms of progress. Evolutionary processes applied to humans challenged barriers between humans and animals, culture and nature, and their related sciences, making evolution the historical process par excellence.\footnote{Ibid., p. 258.} For the West, many scholars have argued, evolution is one
constitutive myth of our times, alongside of the insistence that ours is a society without myth.\textsuperscript{83}

To call evolution a myth is not to challenge its claim to truth. It is to apprehend it as a certain kind of language embedded in and embedding ways of knowing and being in the world. Given the regnant paradigm of rational choice which has made the biological sciences so amenable to maximization think, and given that synthetic biology takes biology and design, and recombines them according to the double logics of design and entrainment discussed in the previous chapter, we may want to ask: How does synthetic biology recast the relationships between biology, teleology and design? This is a vast question, the answers to which figures into how living beings are deployed, sensed and theorized. The goal of this chapter is to tease out some concepts that help clarify how an intervention into the “natural world” is made meaningful. Taking a position or making something always implies a relation to something else; hence the importance of “context” or “history.” But the very act of positioning transforms relations, such that “context” and “history” are taken up in different ways, as motivation, rationalization, epochal proclamation or practical lesson. For synthetic biology, the theory of evolution and its history plays all of these roles. The key is that synthetic biologists employ a set of relational understandings with reference to evolution.

Broadly speaking, and rather schematically, the chief undertaking of synthetic biologists is to do something by human hands that hitherto was purportedly out of human control. This however raises a host of questions. Is the goal to mimic nature, improve on it, or side-step it? Is human comprehension of nature a prerequisite for such intervention?

\textsuperscript{83} See Latour, 1993.
or is it an impediment? In part these question depend on our picture of how our notion of nature itself does what it supposedly does. Does nature strive for and attain perfection? Is it a teleological process or a random one? Answers to these questions further resonate with the concerns of practitioners. For example, are the practitioners of this new field trying to build living elements “from scratch” and with a predetermined goal in mind, or are they bricoleurs, putting together existing biological material to fashion new and unexpected combinations? Questions like these arise not only for the observer of the new field; at some level they are part of the field, and mark fissures and uncertainties within it.

The stakes in this case are especially high since evolution has figured so centrally in American cultural politics. Jerry Coyne’s recent New York Times bestseller, *Why Evolution is True*, has one of those polemical titles that bring into view the dimensions of a national debate about the boundaries of religion and science.\(^{84}\) Coyne begins his book by citing one relatively recent, and relatively prominent court case which turns on this difference, and which concludes that the theory of evolution is as public school appropriate as chocolate milk, while creationism is not.

In 2005, the school board of Dover, Pennsylvania passed a resolution requiring biology teachers to read a statement that suggests Evolution is a flawed theory and recommends Intelligent Design as an alternative, along with a handy reference to an Intelligent Design textbook:

> The Pennsylvania Academic Standards require students to learn about
> Darwin’s theory of evolution and eventually to take a standardized test of

---

which evolution is a part. Because Darwin's Theory is a theory, it is still being tested as new evidence is discovered. The Theory is not a fact. Gaps in the Theory exist for which there is no evidence. A theory is defined as a well-tested explanation that unifies a broad range of observations. Intelligent design is an explanation of the origin of life that differs from Darwin's view. The reference book, Of Pandas and People is available for students to see if they would like to explore this view in an effort to gain an understanding of what intelligent design actually involves. As is true with any theory, students are encouraged to keep an open mind. The school leaves the discussion of the origins of life to individual students and their families. As a standards-driven district, class instruction focuses upon preparing students to achieve proficiency on standards-based assessments.

Some parents sued the school district, and the case went to court. Judge John E. Jones III who presided over the case, himself a conservative-George W. Bush appointed-churchgoing Christian, decided rather emphatically in favor of the parents.

That the statement is half-baked is evidenced by the way it accidentally suggests that there is no evidence for the existence of gaps in the theory of evolution (as a result of some shoddy grammar). But it also points to a highly tactical discursive shift in the way conservative causes and liberal ones are distinguished both formally and thematically. The statement conveys an anti-dogmatic stance and an endorsement of pluralism in the form of open-mindedness. Teaching evolution exclusively is then taken to be closed-
minded. Whether or not the discourse of pluralism and open-mindedness is here used in
good faith is hard to ascertain. But Coyne’s own response plays the role of hostile closed-
mindedness perfectly. With the legacy of the culture wars playing out discursively, his
metaphor of choice is military aggression, and he uses the Dover case to highlight that
“we” (scientists, I presume) are at war with Creationism: “Creationists…are establishing
footholds in other parts of the world, especially the United Kingdom, Australia, and
Turkey. The battle for evolution seems never-ending. And the battle is part of a wider
war, a war between rationality and superstition. What is at stake is nothing less than
science itself and all the benefits it offers to society.”85

Staunch proponents of evolution-as-fact were likewise roused when the National
Science Board deleted a section on American science literacy from its 2010 Science and
Engineering Indicators. While anti-evolutionism may not represent a strictly American
skepticism, the deleted section purportedly showed that it does hold a far above average
market share in American science literacy. According to the deleted text, in 2008, 45% of
Americans answered ‘true’ to the statement, ‘Human beings, as we know them today,
developed from earlier species of animals.’ The number of people who agree with this
statement is much higher in Japan (78%), Europe (70%), China (69%), and South Korea
(64%).

What bothered some scientists were the reasons for deleting the section. John
Bruer, philosopher, board member, and head survey question skeptic, pushed for the
deletion of the text, suggesting that the questions were too value charged to provide good
indices of scientific knowledge in the US. Louis Lanzerotti, who chairs NSB’s Science

85 Coyne, p. xiii.
and Engineering Indicators Committee, explained that the questions were "flawed indicators of scientific knowledge because responses conflated knowledge and beliefs."

Bruer points out that a 2004 study found 72% of Americans answered “yes” to the question regarding human’s animal ancestors when it had the caveat “according to the theory of evolution” tacked onto it. For some scientists, this observation missed the point by miles. The originator of the twenty-five year old survey questions, John Miller, who studies science literacy at Michigan State, responded, “Evolution and the big bang are not a matter of opinion…If a person says that the earth really is at the center of the universe, ... how in the world would you call that person scientifically literate?”

The NSB’s report is, after all, a government document about America and Americans. The deletion of the section on the big bang and evolution was therefore taken as an attempt to sweep an unfavorable representation of a nation under the rug. This was done by nationalizing the questions and their analytic reach in terms of a culturally specific nexus of distinctions between knowledge and belief. Whereas in Europe, questions about evolution suffice as indices of knowledge, in the U.S., the NSB committee claimed, these same questions index belief. Again, like in the statement by the Dover school board, an inclusive agenda built on national cultural leanings shows up as an attack on science, while scientists decry these cultural considerations as an attempt to cover up illiteracy.

The situation, as far as science education is concerned, brings to light multiple crossbreeds of knowledge, discourse and power. Theories, we have all been taught, are not strictly speaking ‘true,’ but rather better or worse, since facts, or interpretations of facts can corroborate theories, but never prove them. And, that theories have a certain
history of being replaced by better theories, is also pretty widely understood. Why then wouldn’t one answer “yes” only when the question is prefaced by the recognition that such an account is part of a particular—excellent—theory? But this is not how scientists like Coyne are reading such maneuvers, and this is not what leads them to claim that evolution is ‘true.’ For them, pluralism, open-mindedness, and national differences are ways of undermining the universality of science for the purpose of legitimizing religious accounts of the origins of life.

There is a further dimension to the politicization of evolution. It instantiates the fallacious notion that science is only political when it is debated in newspapers, or among opposing groups with clearly public interests. Like the sociobiology debates, which provided a highly politicized occasion to observe yet another permutation in the naturalization of bourgeois logics, there are rare moments when cultural politics open up the space for a critique of knowledge. More often we are caught resisting the implications of knowledge too deeply political, too deeply cultural to be felt. The pluralist tendencies of some of creationism’s champions represent a shift in the alignment of discourse and knowledge, and one that might well reverberate in scientific commitments. Let us not forget therefore that some of the most popular evolutionary theorists of our time were die-hard sociobiologists, who produced perhaps the most flattened view of cultural difference of our century.86

86 Sociobiology rested upon the questionable biological roots of altruism. Durkheim addressed the roots of altruism beautifully. For him social solidarity explained altruism. He wrote, “If the hypotheses of Darwin have a moral use, it is with more reserve and measure than in other sciences. They overlook the essential element of moral life, that is, the moderating influence that society exercises over its members, which tempers and neutralizes the brutal action of the struggle for existence and selection. Wherever there are societies, there is altruism, because there is solidarity.” Emil Durkheim, *The Division...
Making Life is Easy

One fine spring evening in Princeton, Michael Hecht participated in a panel on religion and science hosted by the Religious Life Council. The other participants were a neuroscientist and a philosopher of science, both faculty at Princeton. The panel discussion was attended largely by students from different religious groups around the university, and the evening’s event was convened with an Islamic prayer. Students were given the choice of kosher pizza, which soon ran out. I was uncomfortable. Ever religiously unmusical, to cite Weber’s famous self-description, a room full of religious prohibitions presents the ubiquitous risk of causing offense, and any anthropological sophistication I may have access to runs up against sheer human awkwardness. But there is also the truly unnerving sensation that “they” are more comfortable with difference than I am; that “they” know how to juxtapose and live with kosher pizza and Islamic prayer, where I, liberal, agnostic, Jewish American, can’t but efface differences and/or feel their presence as an impediment.

Michael Hecht, I suspect, was not uncomfortable. He certainly seemed at ease. In one of my first encounters with Michael, I discovered that he and his wife kept Shabbat, and ran a strictly kosher household. Michael was the only member of the panel to reference his own faith, by invoking the book of genesis and reciting a passage in

Hebrew. His presentation was a somewhat theologized version of his standard talk for the auxotrophs project.

Hecht began with the volume of all conceivable proteins, to set the stage for thinking about a universe of possibilities. Proteins are made of amino acids, and there are 20 different amino acids. Suppose, he says, each protein is 100 amino acids long (these would be small proteins). If so, then there are $20^{100}$ possible combinations. Now, he continues, suppose we were to make all of these sequences. Just one molecule of each. “Then the size of that flask would have to be not only larger than the known universe, it would have to be larger than $10^{23}$ known universes.

MH: … [L]et's play with this for a moment. So this is the theoretical possibility, some ridiculously huge number. A long comes Charlie here, right, and he does some evolution. And so Darwinian evolution says, okay, this is the starting material, we work on it for a few billion years and we get life. If you're a bacterium, there are only 4000 genes coding for 4000 proteins that sustain life. If you're human, there are maybe 20,000 genes that are spliced in different ways to make 100,000 proteins. I look at this and I say, wow, these must be really special. We started with a staggering number of possibilities. Evolution worked on them for billions of years, and you end up with these very small numbers...

But whether it was Charlie over here, or whether it was Elohim [our lord in Hebrew], whichever you may choose, or you may choose both, one way or the other we end up with a very small number of information, genes, or molecular machines that sustain life. So surely, these must be very special. They're
whittled down from a huge number. And so with that in mind, we ask the question, well are these so special? Are they really that special or can I just do that in the laboratory just like that. And if you can, it forces you to think about what is special and what is not, or how difficult something is to do.

Hecht then presented his lab’s research, which the reader is by now somewhat familiar with. Hecht’s lab takes auxotrophs, bacteria strains that have an essential function knocked out of their genome, and replaces that gene with a gene that codes for a synthetic protein from their protein library. The proteins are designed using a binary code method that is one of Hecht’s claims to fame. Through experiments, lab members isolate the synthetic proteins that can “rescue” the sickly bacteria. Hecht continues,

MH: Where things did not grow before, we now come in with parts constructed in the laboratory that enable life. They are unrelated to anything in the known biological system… and yet these simple things can functionally replace the deletion of th[e] [original proteins]. Can we now construct genomes from scratch? We've taken baby steps in that direction. We've just started. And in fact, it appears that a molecular parts kit need not be limited to those that exist in nature. This is a concept of synthetic biology. Instead of copying that which exists through Darwinian evolution or through creation, instead of copying those, we're now at the stage where we can devise novel ones entirely from scratch in the laboratory, and they don't just function as chemicals in a test tube,
they actually enable the growth of living systems that would not otherwise grow. And so it's really a step towards synthetic biology and artificial parts. And whether it's the Darwinian way of looking at life or the creationist way of looking at life, we now have an additional way of looking at it: that system of parts that sustains the growth of life need not be limited to that which is out there in the world around us.

Hecht and his group frame their research as a contribution to evolutionary theory. The crucial point of that contribution is a significant reformulation of the ease with which life could have been supported by unspecialized proteins. Hecht’s library of proteins is based on a standard design template that specifies structure and folding, not function. And the proteins are very small compared to the complex proteins they are replacing. If they can do the job, than a lot of simple, unspecialized proteins in that space of conceivable proteins could also have supported life.

Accordingly, Hecht et al. begin their paper (after a nod to Loeb) with an invitation and a challenge to synthetic biologists to look for parts outside of what evolution has provided for them. They write, “Until now… most advances in synthetic biology have relied on collections of parts—genes, proteins, and regulatory elements—derived from sequences that already exist in nature. Must the toolkit of life be so restricted? Natural sequences comprise only a miniscule fraction of the theoretical sequence space that is possible for genes and proteins.”

Any number of mutations could

have supported the evolution of living beings, and humans can be the authors of such mutations. When a graduate student in Hecht’s lab who had done a lot of work on the synthetic biology project graduated, lab members gave him a custom t-shirt. It read, “Making a Living is Hard, but Making Life is Easy,” and at the center of the t-shirt was a smiley face in a petri-dish. Where we thought there was one path, there were many. This perspective sets up the possibility of simplifying, circumventing and improving on natural biological processes, which in turn means changing the study of biology to include the biology of the possible. The miraculous dimensions of creation which were displaced onto the quasi-miraculous and unthinkable timeframes of evolution become intriguingly mundane.

Ron and Cil have similarly emphasized the possibility of improving on natural processes. In a review essay for Nature, titled “The Second Wave of Synthetic Biology,” Ron and Cil write, “Whether addressing an existing problem or creating new capabilities, effective solutions can be inspired by, but need not mimic, natural biological processes. Our new designs can potentially be more robust or efficient than systems that have been fashioned by evolution.” Ron and Cil’s review shows synthetic biologists explicitly seeking to improve on what’s out there, cutting out unnecessary complexity and expenditure of energy (Our own? The cell’s?), while making processes more resilient in the face of changing environments and other perturbations. This view picks up on the suboptimality of nature, and natural selection as bricolage. It is an outgrowth of a particular emphasis on the exigencies of evolution as a historical process that has been

---

89 Purnick and Weiss, pp. 410-422.
circulating along side of more teleological views of evolution, of its perfection and unsurpassable efficiency for more than a century. Genetic engineers have long been tweaking nature along normative lines, making it “better.” But they have worked from within the blueprints and processes that make up natural processes. In an ebullient New York Times piece on iGEM, John Mooallem writes, “Genetic engineers have looked at nature as a set of finished products to tweak and improve — a tomato that could be made into a slightly better tomato. But synthetic biologists imagine nature as a manufacturing platform: all living things are just crates of genetic cogs; we should be able to spill all those cogs out on the floor and rig them into whatever new machinery we want. It’s a jarring shift, making the ways humankind has changed nature until now seem superficial.”

**Biology Bricolage**

With the publication of Darwin’s *The Origin of Species*, two of Darwin’s distinguished contemporaries hailed the work’s greatest achievement in their comments. The achievements they identified, however, were opposed to each other. Darwin’s supporter, Thomas Huxley, wrote: ‘That which struck the present writer most forcibly in his first perusal of the “Origin of Species” was the conviction that Teleology, as

---

commonly understood, had received its deathblow at Mr. Darwin’s hands.” Meanwhile, Darwin’s great American Champion, the theologically minded Asa Grey, hailed the great achievement of the work its rehabilitation of teleology in Natural Science, “so that, instead of Morphology versus Teleology, we shall have Morphology wedded to Teleology”.

Darwin’s great work, accessible and puzzling at the same time, lays the foundation not for evolutionary thinking (there were others) but for the mechanism of natural selection. And natural selection could be tweaked to fit both a progressive march towards Nature’s perfection and a random array of ‘traits’ whose success reflected only relative advantage in changing environments. Beyond that, parts of the empirical record could be explained by a set of non-adaptive mechanisms, which could work in concert with natural selection. Darwin’s work reflects this flexibility. In Origin of Species, writes historian of evolutionary theory William Povine, “Darwin proposed at least seven distinct mechanisms of evolution, gradual natural selection operating upon small heritable individual differences being of course the most important…. In addition…Darwin proposed… four mechanisms of nonadaptive evolution.”

---

92 William Povine, “The Development of Wright’s Theory of Evolution: Systematics, Adaptation, and Drift,” Dimensions of Darwinism (Cambridge: Cambridge University Press, 1983) p.47. Origin contains 155 instances of the word “perfect” in all its permutations. See Timothy Shanahan, The Evolution of Darwinism (Cambridge: Cambridge University Press) p. 100. Darwin’s language conveys awe at the perfection of Nature, and the great evolutionary processes that lead to that perfection. He marvels at “how the innumerable species inhabiting this world have been modified, so as to acquire that perfection of structure and coadaptation which most justly excites our admiration.” He insists that “all corporeal and mental endowments will tend to progress toward perfection.” Timothy Shanahan contextualizes this emphasis on perfection by arguing that, “Indeed, it was the ubiquity of adaptation and astounding contrivances that
Later editions of *Origin* demonstrate an increasingly relativized understanding of perfection on Darwin’s part, in which, even if some absolute perfection is achieved, the conditions which selected that perfection would subsequently shift, leaving adaptations outmoded. One can see the development of Darwin’s thought in his treatment of the eye across several works. In an essay that preceded *Origin of Species*, Darwin claimed that “each eye throughout the animal kingdom is not only most useful, but perfect for its possessor.” By the 1859 publication of the *Origin of Species*, the eye represented imperfection in one of Darwin’s examples: “The correction for the aberration of light is said on high authority, not to be perfect even in that most perfect organ, the human eye.” Nature was falling from grace. Indeed, later in life Darwin admitted that in *Origin* he had failed to entirely dispel his own belief in the purposive creation of species.

*constituted the problematic for this theory: How to convincingly explain such features without appeal to divine agency?”* (Shanahan, p. 100). Thus Darwin posed an alternative to the prevailing belief in natural theology. But Darwin’s text clearly lends itself to both teleological and more stochastic readings. He folded vestigial and rudimentary organs into his account with ease and noted that not all biological structures could be explained in terms of adaptation.

It is doubtful whether Darwin’s use of the notion of perfection is entirely consistent in *Origins*. Shanahan’s struggle on this point betrays something of the difficulty of trying to iron out Darwin on this topic. Shanahan writes, “Throughout the *Origin* [Darwin] does not hesitate to describe living things as ‘perfect,’ in the sense that they rightly inspire admiration and wonder; but they are generally not so perfect that no improvements are conceivable. ‘Perfection’ in this sense does not preclude improvement” (Shanahan, 103). This notion of “perfection” is significantly impoverished. Shanahan argues that, over the course of his life, Darwin moved away from his “adaptationist” perspective. And in fact, there are some strong indication in *Origin* that Darwin does at times see “perfection” as a relative quality, applicable to local standards of fitness. So for example, Shanahan quotes: “Natural selection tends only to make each organic being as perfect as, or slightly more perfect than, the other inhabitants of the same country with which it has to struggle for existence. And we see this in the degree of perfection attained under Nature” Darwin, *Origin of Species* in Shanahan, p. 104).

93 Charles Darwin as quoted in Ruse, 117.
Stripping natural theology of its perceptual hold on nature’s perfection was a matter of seeing the world differently.\textsuperscript{94}

After Darwin’s death, the mechanism of natural selection was largely eclipsed by other mechanisms for a few decades. Then came the Modern Synthesis, the marriage of Mendelian genetics and evolutionary theory which proved hugely productive for evolutionary theory in general, and redemptive for the mechanism of natural selection in particular. So much so, that Stephen Jay Gould identified a “hardening of the Modern Synthesis” in a period spanning from the 1940s through at least the late 1970s. Gould writes, “As the synthesis developed, the adaptationist program grew in influence and prestige, and other modes of evolutionary change were neglected, or redefined as locally operative but unimportant in the overall picture.”\textsuperscript{95}

In 1979, Gould and Lewontin published a highly influential paper on the problem of adaptation and adaptationist reasoning in the study of evolution, titled “The Spandrels of San Marco and the Panglossian Paradigm: a Critique of the Adaptationist Programme.” Right out the gate, Gould and Lewontin assert that adaptationist thinking, “…based on faith in the power of natural selection as an optimizing agent,” had dominated evolutionary theory in the US for some 40 years.\textsuperscript{96} Gould and Lewontin,

\textsuperscript{94} For some of Darwin’s contemporaries, evolution and theism were compatible. Famous American botanist Asa Gray propounded a theory of “theistic evolution” in which God supplied the variations necessary for natural selection. Two Englishman independently put forward another version of “theistic evolution” in which God arranged the laws of development so as to fit his plans. But by the end of the 19th century “theistic evolution” all but died out.


instead, “support Darwin’s own pluralistic approach to identifying the agents of evolutionary change.”

Gould and Lewontin’s great intervention rests on central metaphor between evolutionary processes and the architectural exigencies of Byzantine architecture. Specifically, spandrels. Spandrels are the triangular spaces formed when a dome is mounted on rounded archways. The spandrels are a byproduct of a particular architectural form that are then decorated: “Each spandrel contains a design admirably fitted into its tapering space... The design is so elaborate, harmonious and purposeful that we are tempted to view it as the starting point of any analysis, as the cause in some sense of the surrounding architecture. But this would invert the proper path of analysis. The system begins with an architectural constraint: the necessary four spandrels and their tapering triangular form. They provide a space in which mosaicists worked...” The architectural form necessitates the spandrels, which are then “adapted” for decorative use. Gould and Lewontin’s point is that assuming that the spandrels are there for decoration would invert the order of affairs.

What Gould and Lewontin re-introduce is a notion of constraint, something that their adaptationist colleagues, they contend, had strayed away from. For Gould and Lewontin, adaptationist thinking takes our world to be the best of all possible worlds, and selection to be the cause of almost all morphology, function and behavior. Each species is molded and selected to fit its environment with precision and efficiency. Gould and

---

581-598. Quote from page 581. In anthropology, the 70s also saw the rise of adaptationist thinking, which had indeed inherited the legacy of functionalism. Anthropologists contributed explanations to sociobiology which posed cultural institutions as elaborate arrangements for distributing women and sustenance.

97 Ibid., p. 581.
Lewontin attribute this emphasis on perfection to the later thought of Wallace. The point of their critique is to both push for non-adaptive evolutionary processes and to emphasize the pressures exerted on adaptation that keep it from optimizing traits. They compare the adaptationist view to Voltaire’s Dr. Pangloss, for whom “Everything is made for the best purpose. Our noses were made to carry spectacles, so we have spectacles. Legs were clearly intended for breeches and we wear them.” Gould and Lewontin quip, “If blushing turns out to be an adaptation affected by sexual selection in humans, it will not help us to understand why blood is red.”

Gould and Lewontin identify two maneuvers that typify work undertaken under the “adaptationist programme.” First, such work divides organisms into ‘traits’ and construes those traits as optimally designed by natural selection. And, “Since the range of adaptive stories is as wide as our minds are fertile, new stories can always be postulated.” Second, competition between different traits which are seen to offer trade-offs are introduced, and the balance between traits is itself taken to be optimized. Importantly, write Gould and Lewontin, “Any suboptimality of a part is explained as its contribution to the best possible design for the whole. The notion that suboptimality might represent anything other than the immediate work of natural selection is usually not entertained.”

Gould and Lewontin provide an example: The information that accompanies the fiberglass Tyrannosaurus at Boston’s Museum of Science says “Front legs a puzzle: how Tyrannosaurus used its tiny front legs is a scientific puzzle; they were too short even to reach the mouth. They may have been used to help the animal rise from a lying

---

98 Voltaire’s *Candide*, as quoted in Ibid., p. 583.
99 Ibid., p. 593.
100 Ibid., p. 585.
position.”¹⁰¹ The charm of the example is its familiarity as a source of well-domesticated mystification. It delineates a specific space for explanation, but fills that space with wonder. We know those legs were just perfect for something, but we don’t yet know for what. Like the contemporary penchant for identifying “incentives” from the traces of lives lived inscribed in bodies or behavior such reasoning has been inculcated across the sciences and social sciences. The assumption is that something is being optimized, and we are left to discover what and how.

Yet, from the 1960s onward, another view of evolution grew along side this hardened synthesis, one that saw the stock from which adaptation pulls and on which selection exerts its pressure as constrained. This historical view in evolutionary theory renders species as inscription bearers, deep, hereditary palimpsests responsive to changing environments. Historical constraints and environmental pressures perform a complicated dance when it comes to selection and adaptation. Francois Jacob’s *The Possible and the Actual* is frequently viewed as a seminal text for this view. Jacob proposes an analogy between evolution and tinkering, relying on Lévi-Strauss’ distinction between engineering and bricolage. Jacob argues that, while evolution has often been compared to engineering (a comparison that led down the road to Dr. Pangloss), the comparison is flawed. Evolution requires the repurposing of existing parts. That is, evolution draws from a stock of existing components. And it does not proceed according to a set plan or aim. Whereas engineering uses parts that are specifically produced for the execution of a particular design, evolution draws on that which is already available, leftovers in an evolutionary junkyard. Now, Jacob insists that, while

¹⁰¹ Quoted in Ibid., p. 587.
no human action is commensurate with evolution, the best analogy available is *bricolage*, which he translates as tinkering.

While the engineer’s work relies on his having the raw materials and the tools that exactly fit his project, the tinkerer manages with odds and ends. Often without even knowing what he is going to produce, he uses whatever he finds around him, old cardboards, pieces of string, fragments of wood or metal, to make some kind of workable object. As pointed out by Claude Lévi-Strauss, none of the materials at the tinkerer’s disposal has a precise and definite function. Each can be used in different ways. What the tinkerer ultimately produces is often related to no special project. It merely results from a series of contingent events, from all the opportunities he has had to enrich his stock with leftovers.

Since Jacob recruited Lévi-Strauss along these lines, many have followed. The word ‘tinker’ shows up frequently in texts on evolution and signals both the lack of a plan, negating some notion of progress towards perfection, and the limitations put in place by an existing set of components from which new functions can be patched together. The repurposing of parts, and the lack of a plan open the door for inefficiency. It is history, and its etchings in bodies, which allows adaptation to account for sub-optimality.\(^{102}\)

\(^{102}\) Though Jacob’s book is concerned with myth, his use of the notion of bricolage has little to do with the analogy that animated Lévi-Strauss’ use of the term: that bricolage represents, on the practical plain, the operation of myth in the domain of thought. Mythical thought, for Lévi-Strauss, is a second order semiology, which produces ordered thought out of the remnants of historical events, the “debris of what was once a social
If evolution then amounts to a myth of teleology composed out of the remnants of old accommodations, synthetic biologists may be changing that myth. Synthetic biology treats biology as a first order semiology that can be recombined. It takes the discursive constitution of parts from evolutionary theory and creates out of them “abstractions” which can be seen as second order semiologies, systems of signs composed of the remnants of other systems of signs. A key component of this recombination is an emptying of meaning from the first order system, an impoverishment that was theorized in Roland Barthes’ work on myth.

In his famous essay, “Myth Today,” Roland Barthes proposes a theory of modern myth which, like Lévi-Strauss’, takes myth as a second order language. The sign of the discourse,” (Lévi-Strauss 1968:21). It is the recombination of this historical stockpile which is akin to bricolage and which produces myth. For Lévi-Strauss, “The elements which the ‘bricoleur’ collects and uses are ‘pre-constrained’ like the constitutive units of myth, the possible combinations of which are restricted by the fact that they are drawn from the language where they already possess a sense which sets limits on their freedom of maneuver;” (Lévi-Strauss 1968:19).

So much, points out Lévi-Strauss, is to some extent true also of science, “...if one takes into account the fact that the scientist never carries on a dialogue with nature pure and simple but rather with a particular relationship between nature and culture…” The difference between science and mythical thought lies in the scientists’ imperative to go beyond, indeed, break out of the constraints imposed by a particular relationship between nature and culture. Modern science, for Lévi-Strauss, is suspended on the precipice of the meaningless. It has taken this wager in the pursuit of a thoroughly cultural end that is changing the world. This should not, however, provide the baseline for the observation of others. It is a distinct, and thoroughly cultural end. To quote Ruth Benedict, “[Cultures] differ… because they are oriented as wholes in different directions. They are travelling along different roads in pursuit of different ends, and these ends and these means in one society cannot be judged in terms of those of another society, because essentially, they are incommensurable.” See Ruth Benedict, Patterns of Culture (Boston: Houghton Mifflin, 1934) p. 223.

All this goes beyond what Jacob gives voice to. There is a clear reason for this. Where the analogy falls apart for Jacob is in the conspicuous absence of an agent to whom signs can be rendered meaningful, and who can transform those signs. And so his invocation of bricolage is emptied of the significance given to it by Lévi-Strauss. Jacob cannot quite bring myth into the picture.
first order system is transformed into a “mere” signifier in mythical speech. Barthes gives an example: a Latin grammar book contains the phrase, “quia ego nominor leo.” The simple meaning of the phrase is apparent to the student, *because my name is lion*. But this sentence signifies something else. It announces: “I am a grammatical example meant to illustrate the rule about the agreement of the predicate.” The entire semiological system *my name is lion* becomes the signifier of “I am a grammatical example” which constitutes the global sign. For Barthes, the signifier of the myth is emptied of meaning in the mythical context in order for the grammatical example meaning to come to the fore: “The story of the lion must recede a great deal in order to make room for the grammatical example…But the essential point in all this is that the form does not suppress the meaning, it only impoverishes it, it puts it at a distance, it holds it at one’s disposal.” The similarity to Lévi-Strauss’ bricolage is clear. Both authors take myth as the restricted appropriation of meaningful signs. But the emphasis is shifted. For Lévi-Strauss, the signifier of myth is half-full, for Barthes, half-empty. For both, however, the mythologist makes something out of the myth in its structural analysis by identifying constituent parts and assembling the myth along syntagmatic and paradigmatic relations.

The emptying of signs was evident during a particular episode in Weiss’ lab involving the frustration of an undergraduate student. The iGEM team was trying to find a project. The project to be undertaken that particular summer took a long time to congeal relative to the previous year. Session after session of brainstorming and design yielded slow progress in the direction of a manageable goal with interest for as many team members as possible. While specific projects were student driven, Weiss and Cil, the key

---

104 Ibid., p. 118.
postdoc who contributed to mediating iGEM havoc, delineated certain areas of interest. There was a lot of subtle and not so subtle herding of interests involved, and this was the result of a shifting role for iGEM within Weiss’ lab. Between the first Princeton iGEM team submission in 2004 and the most recent, Weiss discovered that they could use the summer program as a jumping off point for further research. In fact, the last three iGEM projects are being pursued by graduate students or postdocs in the lab today. So, while the first submission consisted of some cells engineered to play the game Simon, submissions since have addressed complicated medical conditions such as cancer or diabetes. Cil, for example has inherited an iGEM project on diabetes from 2006. This meant that the standard and interests addressed by project goals had been changed to fit the standards of the lab more generally. Thus Cil and Ron kept certain boundaries in place as to what would be an acceptable iGEM team goal, having themselves delineated some basic interests in a project on neurons.

So neurons it was. Aside from the occasional interjection by an unruly or simply forgetful student to the contrary, nobody much contested this basic requirement. But then, the question still remained, what would they do with these neurons? After many iterations, two students submitted a design to the group that passed muster. The design was for a bi-stable switch made out of genetically engineered cells, described in Chapter One. A bi-stable switch allows the experimenter to change the system from one state to another. Unless a change is desired, the system remains in one of the two states. From his engagement in the discussion surrounding the idea, it was clear that Weiss liked the design for the bi-stable switch. Many of the students remained silent. Some were still wrapping their heads around the idea. Other team members actively struggled to
understand and refine the design by asking questions and thinking through possible problems and solutions. One student, Michael, who had been relatively quiet up to this point in the meeting in question, piped up to voice his frustration, which he now addressed to Weiss himself. Michael was a Molecular Biology major. He was also one of the originators of the switch that had now been more concretely proposed. But he was not entirely pleased. “What’s the point,” he asked, “of using neurons, which can make connections with thousand of other cells, to make such a simple device which only requires a few neuronal connections?” This question highlighted problems of purpose and of scale, of harnessing potential and of using the right tool for the job. One does not use a blowtorch to light a cigarette; one should not use neurons to make switches.

Weiss took the question seriously. It addressed a major point of articulation for synthetic biology, and represented a difference between biology and engineering. He argued that the idea of natural purpose presented a hurdle for synthetic biology that had its roots in biology. As we’ve seen, one of the mantras of synthetic biology is that the field need not be limited by what is already out there. How a brain currently works doesn’t necessarily have anything to do with what synthetic biologists can do when putting together modules.

Weiss’ position was ever-interdisciplinary, though engineering disciplines were always at the center of his formulation of the field. He highlighted the specific approach necessary for designing organic systems, which evaded strict biologists by virtue of requiring ‘design’ and escaped pure engineers because the substrate was organic:
RW: I think everybody in synthetic biology needs to think as a synthetic biologist. Meaning a little bit of computer science, a little bit of bioengineering, little bit of math, little bit of physics and it doesn’t work if you just think of things as a computer scientist, it doesn’t work. It’s just inappropriate.

TDC: So what goes wrong?

RW: Well you make certain assumptions about how these circuits work and they don’t. They just don’t work like that. And so you need to think about what’s the best way to construct a genetic circuit. It’s not exactly the way it works in a computer circuit.

TDC: Why is it not just a matter of knowing more biology?

RW: Well, yeah it is a matter of knowing more biology, understanding more about the biological substrate, but also it is important to understand that bioengineering is also different from biology. So your body, your cells, have a particular way of doing something and your genetic circuits are created in a particular way. That’s not necessarily the way that I should be creating genetic networks as a synthetic biologist. You don’t have to follow the exact rules that nature follows.
But the “rules” that Nature follows have a certain hold on us. We have built a science around them, complete with normative assumptions and expectations. Tom Boellstorff shows the same dynamics with relation to physics in his ethnography, *Coming of Age in Second Life*. Boellstorff’s ethnography of a virtual world begins with a scene-setting typical day in the (virtual) life of an avatar, written in second person to jog the imagination of the read: you teleport and fly, chat with people and shop for clothes and construction materials for that deck you’ve been meaning to add to your Second Life home. When you get to your virtual home, you go about building that deck: “In this virtual world a deck would stay up without poles, but like most people you create structures that accord visually with the laws of physics, more or less.”¹⁰⁵ This impulse to build poles in a gravity free world or keep neurons glued to their “traditional” tasks reveals the normative hold of a world. Imagination must be trained. Weiss’ position was that the idea of neurons could retain functional content without being normatively defined. That is, the notion that neurons *can* form lots of connections comes to replace the idea that neurons *should* form lots of connections. Their capabilities start to lose their hold.

The teleological view of evolution constituted a certain kind of myth. It, indeed, recombined the components of utilitarian social discourse to produce more and less progressive stories about the world and the things that populate it, complete with an origin story, as was observed by Marx, and later Haraway, Hobsbawm, Lévi-Strauss and Sahlins, among others. What were the constructs of natural selection if not composites with varying degrees of utility, whose utility in fact frequently delineated their status as

objects? But times change and myths change too. Some synthetic biologists who want to assemble biological parts are in the process of emptying biology of some of its first-order signification, and reassembling that impoverished semiology into other kinds of meaningful systems. Their derivative of Barthes’ grammatical example could contain textbook images with sets of genes assembled in a figure that announces, “I am a circuit.” The second order language, therefore, dismantles as it assembles. Like the study of myth, it composes relations of similarity and difference by combining paradigmatic and syntagmatic meanings, producing a productive intervention.\textsuperscript{106}

This gives a different sense to the “learning by doing” mantra which has come to justify synthetic biology in the absence of ground breaking technological results. This secondary imperative, to learn by putting things together, has become a major end for synthetic biology, captured in the endless incantation of the physicist Richard Feynman’s phrase “What I cannot create, I do not understand,” so prominently misquoted by Venter. The intended meaning often seems to be that biological knowledge will be an important byproduct of a bottom-up approach. Building biological systems will mean understanding how they work. Yet the phrase is delightfully ambiguous as a source of inspiration for synthetic biology. It lends itself to (at least) three interpretations. The first posits creating as a method for understanding. If we want to understand something, we have to create it.

\textsuperscript{106} See Claude Lévi-Strauss, “The Structural Study of Myth,” In Structural Anthropology (New York: Basic Books, 1963) p. 211. There is a layering effect in the study of myth. As Boon has observed, making sense of text is a productive intervention into a productive intervention, and is concerned with “constructing significance out of the interrelated sensory units comprising […] texts, which are themselves constructed out of the interrelated sensory units comprising experience as it is conceived.” James A. Boon, From Symbolism to Structuralism: Lévi-Strauss in a Literary Tradition (New York: Harper and Row, 1972).
The second points to the limits of our understanding, and those limits are radical. If we can only understand what we create, then our understanding is self-enclosed. Feynman thus echoes Kant, for whom, “Reason has insight only into what it itself produces according to its own plan.” Synthetic biology will never speak to the concerns of biology as we know it, because biology deals with “nature” understood precisely as that which is not created by us. A third, constructivist view holds that “nature” is also “created,” in the sense that it is made intelligible through human modes of engagement and understanding. This is the constructivist viewpoint. From this perspective, neurons, as the makers of millions of connections, and neurons that play tic-tac-toe are not separated through the difference between born and made.

**Constructivism in Action**

In *Representing and Intervening*, Ian Hacking ascribes his own form of realism to an exchange with a physicist friend, who was running experiments to detect quarks. The experimental procedure required that scientists alter the charge on a niobium ball. How did they do this? Hacking recalled, “‘Well, at that stage,’ said my friend, ‘we spray it with positrons to increase the charge or with electrons to decrease the charge.’” This becomes Hacking’s charming incantation for the real: “*So far as I’m concerned, if you*  

---

The story serves as an experiential anchoring point for a focus on experimentation in Hacking’s book, and the proposition that, “Reality has to do with causation and our notions of reality are formed from our ability to change the world.” The problem with Hacking’s formulation is that it doesn’t take into account all of the quasi-theoretical and yet overwhelmingly practical ways of intervening in a world—words, metaphors, models, ideal types, etc. In the sciences these kinds of interventions abound. There are therefore different kinds of theoretical entities, approximated with different kinds of images and different kinds of anchors, dredge up what they may. The use of a circuit, for example, doesn’t exactly confirm that a circuit exists, either in biology or in electrical devices.

Weiss’ bread was earned, and his intellect exerted through the pursuit of controlled practical goals with organic systems, on the order of spraying positrons and electrons. He ventured to make symbols work in predictable ways day in and day out. The fuel for this view, and its mode of expression, was an ambition and sense of possibility that relied on imagination, on the basis of which, it might even be argued, that in order to intervene in the world, one must not be too attached to the ultimate truth of ones materials, instruments and circumstances. This is perhaps the basis for intervention aimed at invention, in which reference must evolve along with aims (See Chapter Four).

This attitude evokes the convictions of the nineteenth century physicist Ernst Mach, who, through his influence on Jacques Loeb, played a key role in injecting biology

\[^{108}\text{Ibid., p. 23 (Italics in original).}\]
\[^{109}\text{Ibid., p. 146.}\]
\[^{110}\text{See Emily Martin, } \textit{Flexible Bodies: The Role of Immunity in American Culture from the Days of Polio to the Age of AIDS} \text{(Boston: Beacon Press, 1995). See also Evelyn Fox Keller, } \textit{Making Sense of Life: Explaining Biological Development with Models, Metaphors, and Machines} \text{(Cambridge, MA: Harvard University Press, 2003).}\]
with the aim of controlling life. Historian of science Philip Pauly describes Mach’s belief that scientific concepts were valuable in so far as they contributed to an “economy of thought.” And contribution to an “economy of thought” could be measured by the extent to which a concept facilitated prediction and control. These were the new tests of scientific concepts. Mach dismissed the search for “causal explanations of phenomena in terms of assumed entities such as atoms and forces.”\textsuperscript{111} He saw these as metaphysical searches for “the nature of things.” \textsuperscript{112} Pauly elaborates, “Mach believed that such entities, and even the concept of causation itself, were merely tools for dealing with the environment. Science, for Mach, was ultimately derived from and subordinate to technology…”\textsuperscript{113} Mach was thus concerned with concepts and theoretical entities as tools.

This concern is shared between science, engineering and social theory. In his much cited and discussed book, \textit{We Have Never Been Modern}, Latour takes anthropology as a model for dealing seamlessly with ‘nature-cultures.’ Except, that is, when it comes to our own—Western—way of life. The West, argues Latour, rather than seeing itself as a culture among cultures, relegates others to the category of culture, while claiming for itself knowledge that goes beyond cultural specificity:

\begin{quote}
We Westerners cannot be one culture among others, since we also mobilize Nature. We do not mobilize an image or a symbolic representation of Nature, the way the other societies do, but Nature as it is,
\end{quote}

\textsuperscript{112} Ibid., p. 43.
\textsuperscript{113} Ibid., p. 44.
or at least as it is known to the sciences—which remain in the background, unstudied, unstudiable, miraculously conflated with Nature itself.\textsuperscript{114}

Latour argues that the underlying assumption of a privileged science undid the very scholarship which sought to understand forms of knowledge other than our own as more than the faulty reasoning of feeble minds. Here, Lévi-Strauss’ \textit{The Savage Mind} serves as Latour’s example of anthropology’s predilection for resuscitated native thinking by bringing it as close as possible to science. Latour quotes Lévi-Strauss:

\begin{quotation}
In treating the sensible properties of the animal an plant kingdoms as if they were the elements of a message, and in discovering ‘signatures’—and so signs—in them, men have made mistakes of identification: The meaningful element was not always the one they supposed. But, without perfected instruments which would have permitted them to place it where it most often is—namely, at the microscopic level—they already discerned ‘as through a glass darkly’ principles of interpretation whose heuristic value and accordance with reality have been revealed to us only through very recent inventions: telecommunications, computers and electrons.”\textsuperscript{115}
\end{quotation}

For Latour, Lévi-Strauss’ recourse to science is a clear manifestation of what he terms ‘The Great Divide.’ Instead of dealing with a representation of nature which is socially


constructed, science deals with Nature as it is. If so, then all of Lévi-Strauss’ pronouncements on behalf of the ‘primitives’ boil down to: did they or did they not come to the same conclusions that science does? “Give the primitives a microscope, and they will think exactly as we do. Is there a better way to finish off those one wants to save from condemnation?” writes Latour. He continues, “For Lévi-Strauss…this new scientific knowledge lies entirely outside of culture.” The result is that if we are to take science to lie within culture too, we find that Lévi-Strauss has simply measured ‘the savage mind’ against the ‘sound knowledge’ of our own cultural construction.

Others have imputed to Lévi-Strauss this one-sidedness and exceptionalism. And Lévi-Strauss himself seems to lean in this direction in his often used example of the scientific confirmation of taxonomies based on the senses (smell is his sense of choice). But Derrida’s influential essay, “Structure, Sign and Play” produces a different reading. For Derrida, ethnocentrism is built into ethnology, as the condition of its possibility, and the question is not whether ethnology tries to escape the concepts at its disposal, but rather how “the relationship…to inherited concepts is thought.” Focusing on the Nature/Culture distinction, Derrida observes a vacillation in Lévi-Strauss’ usage of these concepts: “From the beginnings of his quest and from his first book, The Elementary Structures of Kinship, Lévi-Strauss has felt at one and the same time the necessity of utilizing this opposition and the impossibility of making it acceptable.” In other words, the limits of this conceptual opposition make themselves continually felt, and yet Lévi-Strauss does not banish these concepts. He in fact, uses them, as tools which may yet be superseded but for the time being still hold value. These concepts facilitate the decomposition of the order from which they arise:
[T]he…choice-which I feel corresponds more nearly to the way chosen by Lévi-Strauss consists in conserving in the field of empirical discovery all these old concepts, while at the same time exposing here and there their limits, treating them as tools which can still be of use. No longer is any truth-value attributed to them; there is a readiness to abandon them if necessary if other instruments should appear more useful. In the meantime, their relative efficacy is exploited, and they are employed to destroy the old machinery to which they belong and of which they themselves are pieces… Lévi-Strauss will always remain faithful to this double intention: to preserve as an instrument that whose truth-value he criticizes.116

This is most clearly achieved in Lévi-Strauss’ work on totemism, where totems oscillate between objective category and subjective construct, continually built up and knocked down.117 This is also the understanding of certain scientific constructs such as genes and circuits that I push for here. I once naively insisted to Weiss that in reference to biological systems, circuit talk was entirely metaphorical, and the question was whether

117 Like Clifford Geertz, Wendy Doniger has written that there have always seemed to be two Lévi-Strausses, “an appropriate fate for a man who has spent his life preaching that human beings tend to split everything into twos. Both of him are anthropologists, but they are different sorts of anthropologist. Like the mythical beast once described by Woody Allen, Lévi-Strauss has the head of a lion, and the body of a lion, but not the same lion.” Wendy Doniger in Claude Lévi-Strauss, Myth and Meaning: Cracking the Code of Culture (New York: Schocken books, 1995) p. vii.
the metaphor could be made to do something. He pointed out that circuit talk was entirely metaphorical when applied to computer circuits as well. Circuits, he insisted, didn’t exist any more in computers than they did in cells. The point was to specify the tools, and get some traction with a particular way of intervening in the world.

Much of social theory, like engineering abstractions, is made, and made to think with. Thus, for example, we often forget today that Weberian ideal types are posed as the researcher’s constructs in sociological research, abstractions that are not meant to correspond to particular instantiations of phenomena, nor to the average or mean of a set of related cases. Ideal types pick out, exaggerate and assemble. They are “ideal” not the sense of representing a perfect instance, a platonic form, but insofar as they operate in the realm of ideas. For Weber, they represent “the synthesis of a great many diffuse, discrete, more or less present and occasionally absent concrete individual phenomena, which are arranged according to those onesidedly emphasized viewpoints into a unified analytical construct...”¹¹⁸ These are tools for thought, which, in assembling, constitute a kind of intervention.

A Mythical Ontology

Weiss and some of his lab members were operating quite comfortably at the level of effective metaphors or models. But some of their colleagues have been more vocal in describing an ontology for genetic parts. Recently, review after review essay of current developments in synthetic biology mention the plethora of “parts” made available through evolution. These, we’re told, should be “leveraged” or “exploited.” A number of practitioners have particularly emphasized the richness of evolution as a source of “diversity and modularity.” Led by Berkeley synthetic biologist Adam Arkin, this group marvels at the modularity of natural systems. Arkin’s approach was recently described in another article by Hayden on the state of synthetic biology in 2011 for the magazine Nature. Hayden writes, “Bioengineer Adam Arkin…has pursued the idea that circuits can be made more reliably by basing parts on existing cellular components that already accomplish a certain function in the cell. Such ‘mother parts’ could be tweaked slightly to yield ‘families’ of parts with similar features that could carry out their functions independently and efficiently.”119 Underlying this view is a further naturalization of “parts” as evolutionary products. In their paper on synthetic biology, evolution and ecology, Adam Arkin and his group propose that synthetic biologists rely and build on the inherent compartmentalization and modularity of organisms:

Even if a gene function exists in nature, our ability to use it to engineer complex biological systems with new composite functions relies on the

modularity inherent in naturally evolved systems. Modular biological systems are composed of functional domains that can be individually swapped or altered to change the overall characteristics of the system. A number of compelling studies have demonstrated that modularity in biological systems arises under selection in a changeable environment, and modularity seems to have been selected because it makes ‘rewiring’ on an evolutionary timescale more effective. The ability to rewire natural biological systems makes nature a vast source of modular ‘parts’ for the synthetic biologist.\(^\text{120}\)

For Arkin et al., modularity confers adaptive benefit, and is therefore an evolutionary trait in the sense of being a purposive development. Modularity means that functional molecular domains can be swapped. Arkin et al. put “rewiring” and “parts” in quotes. The quotes formally mark off the metaphorical domain. But the boundaries of the metaphor are hard to draw. So, for example, “Modular biological systems are composed of” what? Of parts, of course. Modular parts. The authors replace ‘parts’ with their new definition. They assert that certain biological systems are composed of “functional domains that can be individually swapped or altered to change the overall characteristics of the system.” Parts are defined not by their evolutionary histories, nor are they understood purely in terms of function. Rather, they are characterized by a combination of function and interchangeability, which describes something one can do with a part on

an abstract scale, namely, swap it, as well as the function it will perform. A natural biological part is now something you can switch out or put in, tweak and tinker with. In other words, it’s now a modular engineering part. But Arkin et al. have also given that part an existence out there in the world. Swappable parts are now adaptations that thankfully fit neatly with what engineers want to do with biological systems. This amounts to the naturalization of a technology of living substance. The ‘nature,’ thus conceived, is a parts library. Following Feyerabend, “[w]e may even say that what is regarded as ‘nature’ at a particular time is our own product in the sense that all the features ascribed to it have first been invented by us and then used for bringing order into our surroundings.”\(^\text{121}\) That order comes to inhabit things and to delineate what we can do with them.

Old Circles, New Circles

To return to the opening discussion of this chapter, Marshall Sahlins posits a circularity undergirding the transformations of economy, natural science and society, unfolding over time. Sahlins’ structuralism transpires in history (so did Lévi-Strauss’, though perhaps not dialectically), as opposed to what Sahlins characterizes as the synchronic Saussurean structuralism from which anthropology so heavily pulled, and for whom “from the

perspective of a system of signs, the changes to which it submits will be fortuitous.”

But any necessary relations to a referent compromise the system. Thus, any values that carry over, “despite contemporaneous relations within the language” would hamper the coherence of the structure. Interpretation was one more locus of wiggle room for this kind of structuralism, since all events, all contingent factors, would have to be interpreted within a systematic frame. For Sahlins to encompass some form of history, some form of change in his analysis, transformation must come from within. He writes “The great challenge to an historical anthropology is not merely to know how events are ordered by culture, but how, in that process, the culture is reordered. How does the reproduction of a structure become its transformation.” And so the circularities proliferate. Thus Sahlins noted that while Darwinism circulated into society in the form of social Darwinism, it circled back into biology as “genetic capitalism.”

In the final, powerful chapter of his anthropological critique of sociobiology, Sahlins argues that discovering the “lineaments of the larger society in the concepts of its biology” is not the exclusive province of sociobiology: “…[W]e have been caught up in this vicious cycle, alternately applying the model of capitalist society to the animal kingdom, then reapplying this borgeoisified animal kingdom to the interpretation of human society.” The culprits of this dialectical motion are clear: “Adam Smith produces a social version of Thomas Hobbes, Charles Darwin a naturalized version of

123 Sahlins 2004, p. 5.
124 Ibid., p. 7.
126 Ibid., p. 101.
Adam Smith; William Graham Sumner thereupon reinvents Darwin as Society, and Edward O. Wilson reinvents Sumner as nature.”¹²⁷ And the stakes are huge: “What is inscribed in the theory of sociobiology is the entrenched ideology of Western society: the assurance of its naturalness, and the claim of its inevitability.”¹²⁸ Possessive individualism as the dominant theory has imbued nature with a set of calculative rationalities, and humans with that nature.

Sahlins describes a mythico-ideological dialectic, a means of perpetuating the subordination of social classes by way of measuring ‘truth’ in relation to nature, and calibrating nature in relation to society. Then the circle begins again. But when Sahlins inveighs against the totemic literalism of sociobiology, its sense of proximities and affiliations as natural taxonomical categories, it is unclear if what he longs for are different totems and different logics of association, or an escape from the intertwinement of nature and culture. In the final analysis, Sahlins contends, we seem to be caught. Caught, that is, not simply in this particular circle, with unerring faithfulness to *Homo economicus*. But caught in such circles in general. He writes, “We seem unable to escape from this perpetual movement, back and forth between the culturalization of nature and the naturalization of culture.”¹²⁹ The possibility of escape is not one I find compelling. In fact, the very notion of escape seems dubious. Sahlins crosses the line from demystification to transcendence. There is no asocial nature, some pure apprehension unsullied by meaning.

¹²⁷ Ibid., p. 93. For Sahlins, anthropologists have not been situated outside of this dialectic, having inherited and theorized a utilitarian view of individuals.
¹²⁸ Ibid., p. 101.
¹²⁹ Ibid., p. 105.
Instead of escape, what we can hope for, or fear, or study or think about is the possibility of a different natural-cultural order, one that, whether alongside of or instead of the maximizing individual, instantiates its own set of relations, distinctions and differences. Parts and modularity could be naturalizing the division of labor, transmuted into organic and mechanical solidarity, re-read as the swappable ‘parts’ of cells, and so reinforce the old circle while expanding its reach. They could also hint at a new biology, where the old had become so teleological, so mythologized, that we are afforded, if briefly, a breath of fresh air.
“Observationality is vague at the edges. There are gradations in an individual’s readiness to assent. What had passed for an observation sentence, say ‘that’s a swan’, may to the subject’s own surprise leave him undecided when he encounters a black specimen. He may have to resort to convention to settle his usage. We shall need now and again to remind ourselves thus of the untidiness of human behavior, but meanwhile we foster perspicuity by fancying boundaries.”\(^{130}\)

—W. V. Quine

**Indeterminacy and Experiments**

In *Toward A History of Epistemic Things*, Jans-Jörg Rheinberger discusses the necessity for conceptual indeterminacy in experimental work. He begins with Freud’s opening remarks on science and concepts in *Instincts and their Vicissitudes*.\(^{131}\) “What Freud


invites us to ponder here,” writes Rheinberger, “is the ineffable trace of scientific action or, as I would like to call it, the experimental situation. It appears as if the relationship of “deriving” ideas from the material of observation and of “imposing” ideas upon that material represented the focal point of the argument.” The “deriving from” and “imposing upon” relationship to which Rheinberger refers gradually articulates the basic concepts of a science. Importantly, the two acts are intricately tied, and co-constitutive, working as part of “one and the same movement.” Experimental systems “inextricably cogenerate the phenomena or material entities and the concepts they come to embody. Practices and concepts thus ‘come packaged together.’” This movement goes hand in hand with the duality of concepts, which are, at once, instruments of scientific investigation and the products of such investigation. The aim of all this simultaneous movement between loosely defined concepts and observation for Freud is not to settle on

In actual fact no science, not even the most exact, begins with such definitions. The true beginning of scientific activity consists rather in describing phenomena and then in proceeding to group, classify and correlate them. Even at the stage of description it is not possible to avoid applying certain abstract ideas to the material in hand, ideas derived from somewhere or other but certainly not from the new observations alone. Such ideas—which will later become the basic concepts of the science—are still more indispensable as the material is further worked over. They must at first necessarily possess some degree of indefiniteness; there can be no question of any clear delimitation of their content. So long as they remain in this condition, we come to an understanding about their meaning by making repeated references to the material of observation from which they appear to have been derived, but upon which, in fact, they have been imposed. Thus, strictly speaking, they are in the nature of conventions—although everything depends on their not being arbitrarily chosen but determined by their having significant relations to the empirical material, relations that we seem to sense before we can clearly recognize and demonstrate them. It is only after more thorough investigation of the field of observation that we are able to formulate its basic scientific concepts with increased precision, and progressively so to modify them that they become serviceable and consistent over a wide area.”

133 Ibid., p. 13.
134 Ibid., p. 28.
definitions, but rather to render the “sensed” explicit, and thereby make room for new observations, explains Rheinberger.\footnote{135}

Rheinberger focuses on the internal dynamics of experimentation. Experiments are for him, following Jacob, “machines for making futures.”\footnote{136} They are themselves involved in formulating the questions to which they, over time, delineate answers. They also represent a nexus of practices with reasoning dynamics of its own: “The dynamic body of knowledge, the network of practices structured by laboratories, instruments, and experimental arrangements, is a reasoning machinery in its own right.”\footnote{137} Since the dynamics of experimental systems generate their own internal effects, researchers are necessarily alienated from the objects of their investigation. “The scientist, as an authoritative speaker, is not the ultimate master of the game. But as a humble subject, he or she finds him- or herself captured in an inextricable relation of internal exclusion with his or her objects. He or she makes them, but only insofar as they make him or her make them.”\footnote{138} Thus, Rheinberger suggests, researchers are continually displaced by the products of their work.

Rheinberger’s characterization of the relationship between concepts and experimental systems starts in the middle of the lifecycle of concepts, but Freud directs us also to their beginning. Freud reminds us that the concepts which are imposed upon observations must come from somewhere other than the material of observation at hand. Vestiges of such concepts which do not derive from the experimental system under

\footnote{135}Ibid., p. 13.
\footnote{137}Rheinberger 1997, p. 20.
\footnote{138}Ibid., p. 226.
consideration persist within the “deriving from” and “imposing upon” relationship. That
is, initial formulations of concepts may have non-experimental lives as well: they
originate in prior, and by now conventional, usage.

These dynamics, for example, furnish an important part of Hannah Landecker’s
history of the concept of immortality, as it came to apply to the longevity of cells.
Landecker describes the technical means of achieving increased lifespan in cells,
introduced by Alexis Carrel in the early twentieth century. Carrel famously cultured
chicken heart cells that continued to beat in media for some hundred days. Carrel’s use of
the term immortality saturated laboratory practice with a set of colloquial connotations.
Landecker explains, “Calling cultures immortal only strengthened the perception of cells’
potential autonomy from the body. The spatial reorganization of cells, releasing them
from the bound of the original organism, also seemed to free cells of the body’s limited
lifespan.”139 In turn, the haunting image of hearts beating in jars fed back into the popular
imagination and helped delineate a notion of immortality, both in and outside of the lab.
Carrel’s immortal chicken hearts, mediated and mastered through human technical
intervention, in turn, captured the attention of the public and imbued immortality with the
tinge of technical intervention. This oscillation and its reverberations continued through
many iterations: “Immortality has, in the twentieth century, undergone several such
cycles of capture from colloquial language into scientific practice and back into an
altered state of cultural salience.” She therefore asks, “After its emergence as a tangible
scientific object, how did immortality’s cultural salience change?”140 This cultural
salience is not to be divorced from the production and delineation of a laboratory object.

139 Landecker, p. 69.
140 Ibid., p. 93.
It is not mere popular imagination, irrelevant to and outside of scientific practice. It too will feed back in. In the case of chicken hearts beating in glassware, the apparatuses of manipulation, the artificial environment that stopped or slowed the marking of cellular time, became both a new form of life, and a new form of science.\footnote{Ibid., p. 95.}

The concepts I describe in this chapter carry with them the baggage of such conventional usage. This is not to say that these terms somehow originate outside of experience, but only that they precede particular experimental systems and also exceed those systems, just as any object of observation “possesses significance that is not exhausted by our conception of any single aspect of it.”\footnote{Michael Polanyi, \textit{Duke University Lectures}, Fourth Lecture on “The Emergence of Man,” quoted in Rheinberger 1997, p. 23.} The meaning of conventional-turned-technical concepts is accordingly not exhausted by the experimental system and therefore cannot be understood solely with reference to that system. The same is true of their doubles: technical concepts which become conventional for a particular culture of reasoning.

“How do we define growth?” “What is a pattern?” This chapter describes how two labs, working on two very different projects which they both saw as contributions to the emergent field of synthetic biology, ran into questions of clarity and definition with respect to basic conceptual components of research projects. These basic concepts were steeped in conventional usage, the looseness of which emerged in the course of research. The need for conceptual clarity arose out of experimental indeterminacy, which expressed itself as trouble in interpreting the results of particular experiments: an “I know it when I see it” approach to key elements of project definition proved too loose when
applied to ambiguous experimental results. Practitioners were then impelled to tighten old concepts or elaborate new ones in order to interpret results, and devise a new interpretive map, complete with a new key. The order of events with regard to this conceptual clarification is crucial: projects change through intermittent iterations of experiments and interpretation. And they change at least partly because questions which seem perfectly suitable at the outset turn out to lack specificity with relation to experiments. Experimental systems provide the impetus for specifying meanings of concepts, and they frequently suggest some alternatives, but they do not in themselves determine the narrowing of conceptual ambiguity, nor can they, in one step, produce such technical abstractions that everyday language gets left behind. Rather, researchers make choices between alternative meanings which allow a range of interpretations. These choices are weighted with interests and reflect creative accommodations. Past choices may then be inherited as part of any experimental system, while new ones may eventually become part of the taken-for-granted of future laboratory practice.
Hecht’s Lab: “How do we define growth?”

Michael Hecht’s lab’s main project tested the function of synthetic *de novo* proteins in sick bacteria. Hecht saw this project as both a contribution to synthetic biology, and a challenge to its self-description. Whereas many synthetic biologists saw themselves as inventing new pieces from which to build synthetic life forms, Hecht argued in effect that these synthetic biologists were mostly bricoleurs engaged in rearranging and recombining naturally occurring components.

Hecht’s lab was committed to a different understanding of the term “from scratch.” For them, synthetic *de novo* proteins were a more convincing building block for circumventing Darwin than the standardized genetic parts found in most synthetic biology labs. To that end, Hecht lab members had designed a collection of 1,000,000 *de novo* proteins encoded by a library of synthetic genes (an artificial ‘genome’). For this project, lab members used special *E. coli* strains called auxotrophs. Auxotrophs are mutated *E. coli* strains that can grow when given rich media (what Hecht compares to chicken soup) but cannot grow on poor media (the equivalent of sugar water). Hecht’s lab tested their own proteins for their ability to compensate for the deleted functions in the auxotrophs. They managed to “rescue” four *E. coli* strains, “thereby demonstrating that novel sequences bearing little or no similarity to natural sequences can provide essential biological activities.” Then, lab members co-expressed several novel sequences in a strain of *E. coli* that had been modified to lack four essential functions at once (a “quadruple knockout”), and this resulted in growth. Remember the t-shirt? Making life is easy.
In mid June of 2010, Sarah, a rising senior, and Betsy, a new postdoc, present at a lab meeting together. They had both recently taken up the synthetic biology project, following the departure of a very talented graduated senior and a couple of graduate students who had been pursuing the work. The problem on this day is that the new team is having trouble replicating the results of experiments that had succeeded in the past for students who are long gone. A key control is producing ambiguous results with a number of different auxotroph strains. For the auxotrophs project to work, one set of controls has to show that these strains are in fact auxotrophs: sickly cells that rely on their environment for nutrients their healthy counterparts can produce. If they grow without Hecht Lab’s synthetic proteins, they are not candidates for illustrating the effectiveness of the synthetic proteins for compensating for deleted function. But several of the auxotrophs show what may be some growth on minimal media, which makes it hard to prove that the synthetic genes are solely responsible for growth.

Sarah and Betsy come up with a series of hypotheses as to why this growth is occurring, and experimentally knock these alternative explanations out one by one, as best they can. Was there contamination? Were they sloppy? Are the auxotrophs correct in the first place? Was the previous grad student just wrong? Are these actual colonies on the plates? Are the cells getting the nutrients they need from dead cells in their vicinity? Sarah explains that they have become incredibly meticulous about the way they run experiments in order to rule out human error and contamination. She now sees colonies everywhere, she laments, so that she no longer trusts her own perception and asks other lab members to look at her plates. Hecht asks her jokingly if she dreams about cell colonies at night too.
One question that emerges during the presentation and discussion is, “How do we define growth?” The colonies that are forming, if colonies they are, are tiny and fail to grow beyond this initial size. The lab members disagree as to whether to call this growth. Parameters are suggested: less than or equal to five colonies does not amount to growth. One lab member says that if these dots do not expand, they should be ignored. This solution is quickly rejected: the phantom colonies have caused too much unease, and occasioned too much work, to be ignored. Should there be an error margin for cells that are not supposed to be at all viable on minimal media? Dead (+/- 5)?

A couple of months later I interview Betsy and Sarah about the outcomes of their struggles with the definition of growth. In general, they tell me, meticulously sterile lab technique helped clear up some of the problems. Betsy explains that they made ad hoc adjustments based on particular auxotroph strains, but a global definition, so far, was not on the table. For example, they decided that one particular E. coli mutant, under their growth conditions, is not an auxotroph, since it grows quite well on minimal media. And obviously, Betsy points out, they can’t rescue something that grows fine on its own.

How had they avoided defining a threshold for growth? In the course of mulling over the controls that showed slight growth, Hecht, Betsy and Sarah had come up with a new category of experimental results. The claim with reference to the E. coli strains in this category was significantly weaker, but still worth making, they decided. The problem of the definition of growth was therefore deferred by the creation of a category for the indeterminate results.
**Betsy:** So we did also determine that there were a few [strains] that were growing after a really long time, like eight or nine days. So [there] was one that [Sarah] was working on that she determined that if it grows with no additional proteins in eight days, but we can get it to grow in two days with the additional protein, that’s still a phenotype. It’s a slightly different phenotype than we were originally looking for, which was a rescue of a total auxotroph, but we did sit down with Michael [Hecht] and talk about whether that was something worth pursuing and we decided it was. Obviously we have to be careful that we don’t imply that it’s rescuing a totally dead cell… It’s more like it’s making them healthier, as opposed to rescuing from the dead.

The introduction of the new phenotype, the “unhealthy” cell, involves a tradeoff, Betsy explains: sometimes, rather than complicate the claims, it’s better to just move on. But in this case, they decided that the colonies with the synthetic gene grow much faster, and fare better than the cells in the control.

Betsy goes on to explain that they had always left room for a margin of error of “one to two” colonies, but would not ignore “six-hundred colonies.” I ask Betsy whether they’ve had any border cases now that hey had created the new category and she says that so far they did not. And even with those strains, she explains, waiting those eight or nine days to see growth is an arbitrary choice. They could have waited seven, and missed the action, or thirty, in which case it’s possible that other purported auxotrophs would show growth. Nonetheless, she reflected, the question about the definition of growth would likely come up again.
Lab Precedent and Inherited Practice

One element that played an important role in how Betsy and Sarah understood the problems they encountered with defining growth was the inheritance of lab projects. For the auxotroph rescue project, Betsy and Sarah both insist that the questions that surrounded minimal cell growth arose because they were both new to the project. First, it’s a matter of experience with cells:

Sarah: I didn’t know if what I was seeing were crystals or colonies or fake colonies… And I wasn’t yet accustomed to looking at so many different types of colonies. Now I can tell if things are smaller than normal, or not the right color, or they’re growing in a weird amount of time, but back then I didn’t know what to compare it to.

In other words, identifying the correctness of procedures and results requires a trained eye and confidence.\textsuperscript{[143]} Cells require interpretation, and the more familiar the experimenter is with the range of possibilities, the more easily she makes sense of what she encounters in Petri dishes on a daily basis.

Cil, the postdoc in Weiss’ lab, for example, had extensive experience interpreting spots on Petri dishes. When describing results once, she gave voice to the fullness of the interpretive key used for identifying cells and their states, an accumulation of experience

\textsuperscript{[143]} For a thorough discussion of the emergence of the trained scientist-observer in the early twentieth century, see Lorraine Daston and Peter Galison, \emph{Objectivity} (New York: Zone Books, 2007) pp. 309-361.
and confidence that could only be achieved through first had familiarity with the variety of possible outcomes. In Cil’s beta cell project, when she had managed to differentiate stem cells beyond the endoderm phase, the resulting cells were grouped into “big, ugly, dark colonies.” These colonies were very hard to image on a microscope, because they looked like “a glob of death.” But she knew they were alive. I asked her how she knew this, and she explained,

Cil: Well there are several ways. You can actually use propidium iodide to look, and that stains live cells. Or you can just know. If you’ve looked at cells long enough you can tell what a dead cell is versus what a live cell looks like. And live cells are very three dimensional, you’ll see shadowing and you’ll see the color throughout the cytoplasm, whereas if they’re dead you’ll see punctuated color, because that’s chromosomes breaking, and breaking chromosomes have their own autophlorescence, so that complicates things, because you still see red and green but you have to look closer at the cells and really make sure that you’re seeing the fluorescents throughout.

Returning to Hecht’s’ lab, familiarity and training wiped out a considerable portion of the difficulty surrounding cell growth, but the value of such experience and training has to be adjusted to the problematic place of inculcation and inherited practice. That is, projects which predate a given experimenter carry with them a kind of user lock-in wherein interpretive schemes are inherited wholesale.
**Betsy:** So Sarah started shortly before I got here. I got here in late March. She hadn’t been here that long. I got here and we had only a slight overlap with another undergrad who had worked on the project and no overlap with any grad student. If there’s anyone to overlap with it’s [that undergrad], but you know, [the undergraduates] are not around as much. Especially her, she was finishing up her thesis, she would do stuff at night, so there wasn’t really a lot of overlap with her. So A) Sarah and I figured it’s probably human error. It’s probably our fault, right? and B) If, say, John (the previous graduate student on the project) had still been here he would have been like, oh, that’s normal, seen that with six other strains, don’t freak out. But I think we freaked out a little bit because we were new to the project, we were new to the lab.

The familiarization process with regards to labs and projects comes packaged together with the choices and standards of previous researchers. Such black boxes of practice are part of the acculturation of a particular lab.\(^{144}\) In some cases, such perpetuation is a conscious choice to maintain continuity of practice and context for experiments. Betsy recounted a case of user lock-in in Hecht’s lab with regards to the temperature at which lab members grow *E. coli*. One grad student, years ago, had decided to grow the bacteria at a lower temperature than the standard 37 degrees Celsius because of some research he had done on toxicity. Since then, everyone had had to continue the practice for the sake of continuity and consistency. In the case of strange growth patterns

in purported auxotrophs, Betsy and Sarah bring into view the decisions that may come packaged together with a project in unreasoned form.

At a Forbes reception in the Fall, I discussed the transition difficulties with Michael Hecht. I suggested that inherited arbitrary decisions smoothed research along, and were upended, and revealed by discontinuity in the pursuit of an experimental system. To me, this was a provocative thing to say, because it suggested a grid of arbitrary decisions underlying a seemingly rational set of experimental principles. He took it in stride, and said he regretted not having insured greater overlap such that Sarah would have been trained to those standards. Months later, as the same set of transitions loomed again with the end of the academic year, Hecht ran a series of lab meetings to make sure that projects transitioned smoothly to another generation of juniors and seniors and graduate students. Students who were entirely new to the auxotrophs project would test some as yet untested auxotrophs. This had now become the standard lab competence and familiarization building exercise. Joe, a new researcher in the lab, was next up for these entry-level experiments. “If you’re not sure if they’re colonies or not, call Sarah at 3AM,” joked Hecht.
Weiss’ Lab: “What is a pattern?”

In Weiss labs’ Turing patterns project, a similarly definitional question had been prompted by experimental results that occupied a grey area. The initial goal of the project was to produce patterns with cells, and when a round of experimental results showed variance from the controls, but still left something to be desired with respect to making patterns, the definition of a pattern became a major issue. But “pattern” wasn’t the only concept that underwent significant renovation over the time I followed this project. The inspirations for the patterns, giraffes and leopards, were also brought into question.

The Turing project involved cellular pattern formation through cell-to-cell communication with the aim of generating patterns like those found in nature. To this end the experimenters used a synthetic gene circuit that “directs an initially homogenous lawn of genetically engineered bacteria to spontaneously produce Turing-like patterns of fluorescent spots with spatial scale much larger than that of a single cell.”145 The patterns are achieved through selective activation and repression of gene expression. Weiss’ presentation of this project, iterations of which I observed over the course of the year, started with pictures of cheetahs, zebras, giraffes, and fish, animals whose elegant coats or scales did not immediately strike the uninitiated with therapeutic possibilities. Eventually, the applicability of the patterning goal became clear: bodies are replete with detailed cellular architectures in which local interactions produce global patterns. If synthetic biologists want to construct functional parts for therapeutic purposes through genetic engineering, patterning will have to be part of this story.

As previously described, Weiss is part of a community of synthetic biologists who use pieces of DNA to construct staples of electrical engineering, such as “switches,” “oscillators,” and “transistors.” Thus, all of Weiss’ lab members were expected to speak the language of Boolean logic, and most were required to do some modeling. Unlike Hecht’s lab, Weiss’ lab used mathematical models in almost all of their projects as a first step towards proving the achievability of a project, and as a way of calibrating experiments along the way. Between a design goal, a model, and an experimental system, an elaborate negotiation must take place. The closer the experiment is to the model, the more the engineer can know about what sorts of changes will have what sorts of effects. Ideally, notes Weiss, the model and the experiment produce the same results. This, he explains, never happens. The model and the experiment are therefore herded towards a point of convergence. But not any convergence will do. The engineering goal delineates where the two should converge. Successive iterations may bring the experiment and the model into greater agreement while the two slowly stray away from the engineering feat to be accomplished, in which case the ambiguity of the goal is key. In the case of the Turing patterns the model and the experiments may look more and more alike, and yet raise questions about the definition of patterns. The convergence between models and experiments must occur at a particular point, a point that requires some notion of what counts as a pattern, and some justification for that notion.

**RW:** So there’s a goal, and then how quickly can the model get to the goal, and how quickly can the experiment get to the same goal and how do they converge.
**TDC:** yeah but you don’t want to get too far from the goal. So the problem with the Turing patterns is partly how far from the goal is it going to get before they converge?

**RW:** Yeah.

**TDC:** I mean it’s going to look like a big tomato, and you’re trying to make leopards…

**RW:** right, so yes, you want the convergence to be at a particular point in space, and I would say that that’s true for almost every one of the projects.

After several iterations of design and experiments, the experimental results did not match up satisfactorily with the perfect circles of the model. The results did, however, outperform a key control meant to convince the relevant scientific audience that the patterns were generated by the designed genetic circuits through cell-cell communication, rather than through spontaneous individual decision making in cells. The control involving the independent cell-fate decisions produced a very different distribution of cells than did the one with the Turing pattern circuit. Thus, whether the attempt at Turing patterns in the lab is robust enough to be called a pattern, it is at least viably demonstrating a different process and a different outcome than the one observed in the control.
Weiss didn’t ask the question, What is a pattern? at the outset. But it became a pillar of the project and a source of wiggle room, at the same time. Weiss called it “the million dollar question.”

I saw Weiss present this project a few times. The last presentation for which I was present was given in Weiss’ graduate seminar in the Spring semester. The presentation, again, opened with the slide of animals, presumably in their natural habitats: national geographic-type images, coats shining in the sun, scales glistening under water. In this version, the presentation was followed by an intriguing coda. The giraffe reappears, but this time, in a different guise. “If you look closely at the giraffe,” Ron points out, “the

---

Image 4: Synthetic Turing Patterns a) experimental result with genetically engineered cells; b) a collection of controls.\(^\text{146}\)

\(^\text{146}\) Ibid.
domains vary and the pattern is not uniform.” This observation emerges from the experiment. Having produced a pattern that does not accurately reproduce the perfect circles and spacing of the model, Weiss goes back to the biological occurrence of the system his lab seeks to approximately reproduce. And now, the giraffe is reexamined in light of the experiment, an asymmetrical and irregular example of contingency in the form of noise. The giraffes and leopards then bolster the decision to try a different model and help explain the experimental result by suggesting “this is how biological systems work.” Ian Hacking notes that “[o]ur preserved theories and the world fit together so snugly less because we have found out how the world is than because we have tailored each to the other.”147 In this case, we see how “the world” is subject to reinterpretation in light of scientific results. When we discussed this, Weiss pointed to the imperfections in the giraffes on his corkboard. The shift exemplifies the tentative place of perception. It can be made to play for both teams. That is, whereas the original model seemed like a good fit before the experiment, after the results of the experiment are analyzed, the giraffe itself is reassessed. A new interpretation of the natural example thus helps arbitrate differences between the model and the experiment, and the experiment tentatively wins. A stochastic model is introduced.

Image 5: Models of Synthetic Turing Patterns. a, b original deterministic model; c, d stochastic model.  

The giraffe and the leopard do not show up in the paper on the Turing patterns project. They are the stuff of power point presentations. Their role as tools of persuasion falls out of the formal record. And yet the animals are an important part of the conceptual arsenal of this project, both for thinking through problems and for convincing others. The public record is remarkably skewed towards agent-free science, divorced from notions of persuasion, experience and choice. As David Gooding has observed, “scientists themselves write such agency out of the narratives they publish in papers and texts, along with many other things they have used or produced along the way.”

148 Ting Lu, David Karig and Ron Weiss, Draft.
A Major Concept in Flux

In her book, *The Century of the Gene*, Evelyn Fox Keller argues that the concept of the gene is underspecified, and connotes different things in different contexts. Keller’s narrative takes the reader from the one gene-one enzyme theory of genetics, through the increasing difficulty of pinning down what exactly genes are, or, for that matter, what they do. Decades of research have shown that far from a one-to-one mapping of genes to proteins, genes are frequently discontinuous strands of DNA that may code for different properties in different contexts. Keller introduces the possibility that genes are on the verge of outliving their usefulness as ways of organizing biological knowledge, and thinking about development and heredity. Yet genes are pervasive in what Keller terms *gene talk*: genes are deeply entrenched in popular ideas of biology, as well as within research communities. And, she argues, this state of affairs doesn’t necessarily get in the way of experimental work:

The first point to be made is obvious to any working biologist (and probably to any working scientist): there may be “no single fact of the matter about what the gene is,” but neither is there necessarily a problem in such a state of affairs. Indeed, the sort of definitional difficulties
historians and philosophers worry about rarely if ever impede biologists in their day-to-day usage of the term.\textsuperscript{150}

In fact, Keller points out, such definitional under-determination may be essential for communicating between experimental systems, subfields, and scientific knowledge communities.\textsuperscript{151} Where I disagree with Keller is in the identification of how far into the experimental scene one has to go in order to find specificity. In her view, definitions and meanings must be specific and precise for a particular experimental context. She writes,

\begin{quote}
Where precision is necessary (and absolutely so) is in particular laboratory practices. Moreover, it is from the specificity of the experimental context in which they are invoked that technical terms acquire the precision they need. Terms like gene may be subject to a variety of different meanings; but locally, misunderstanding is avoided by the availability of distinct markers directly and unambiguously tied to specific experimental practices. Within that practice, the marker has a clear and unambiguous reference.\textsuperscript{152}
\end{quote}

I have tried to show that even within a specific experimental setting, terms are deeply in flux. It is through the experimental process that meanings are forged, but this means that they shift through iterations of research and interpretation. Precision is therefore an

\begin{flushright}
\textsuperscript{152} Keller 2002, p. 140.
\end{flushright}
artifact of experimental practice, something that pops up at the end, and looks as though it has been there all along.
“We must not forget that the laboratory itself constitutes a new environment in which life certainly establishes norms whose extrapolation does not work without risk when removed from the conditions to which these norms relate. For the animal and for man the laboratory environment is one possible environment among others (...) for the living being apparatuses and products are the objects among which it moves as in an unusual world. It is impossible that the ways of life in the laboratory should fail to retain any specificity in their relationship to the place and moment of the experiment.”

—Georges Canguilhem

The Move

In March, 2011, Hecht’s lab was transitioning from the old chemistry building to the new one on the Princeton campus. The transition period was still in full swing, and reverberated through different scales of lab members’ lives. Hecht’s lab was one of the last to be moved from the old chemistry building. (Old) Frick Laboratory is a gothic style building with arches and vaults, named for Henry Clay Frick, turn of the century.

industrialist, chairman of the Carnegie Steel Company, and noted arts patron. Frick’s initial interest lay in funding a law school for Princeton in the 1910s, but his support was redirected to the construction of a chemical laboratory. Plans for construction hit a snag when Henry Frick saw the price tag of the project, supposedly, and ironically inflated by the price of steel. Frick died in 1919, leaving the university a sizeable gift for discretionary use. Other needs having been found more pressing, Frick’s donation was not ultimately used in the construction of the laboratory, which was financed by the Princeton University Fund and constructed in 1929. Recalling his interest in the project, the trustees voted to name the new building after the great American industrialist.

Behind Frick stands Hoyt, the very functional and unremarkable 1960s extension of Frick Laboratory, which contains many more labs, and is connected to Frick by walkways and hallways. Hecht’s office and lab were located in Hoyt, while lab meetings took place in Frick. Frick was slated for massive renovations that would supposedly transform it into the home of the economics department. The name Frick was to migrate down the hill to the new site of the chemistry building, leaving room for a new donor’s name on the face-lifted Old Frick.

Construction of New Frick was completed in late 2010. Its façade hangs back from Washington Road, down the hill from Old Frick. On a cloudy day, New Frick looks slightly dour, aged before its time. Covered in dark, grate-like panels that filter light into the building, the exterior manifests some of the hallmark industrial banality of 1960s university buildings, with a key difference. The 216 panels covering the façade are in fact photovoltaic panels, power generators that simultaneously shade the glass-roof covered
interior atrium. And what an atrium. Four tall stories high, twenty-seven feet wide, this large hollow interior is spanned by pedestrian bridges on all levels, while aerial bubble sculptures riffing on molecular themes hang from the glass roof. On both sides, this atrium, which spans the length of the building, is bordered by glass beyond which one sees workspaces: labs on one side, group rooms, offices and classrooms on the other. The ideology of transparency reaches the pitch of parody as the notoriously mundane day-to-day labor of science is forced into view. Forced, because transparency is embedded in design, and not subtly so.

Transparency is formally couched as a way of facilitating collaboration in New Frick. But transparency lends itself all too easily to surveillance, as “visibility as a conduit for knowledge is elided with visibility as an instrument for control.” As I wandered around the 3rd floor one Sunday afternoon, I focused my attention on the graduate student offices across the atrium, one floor below me. I zoomed in: One graduate student at his cubicle, his back and shoulders visible, ergonomic Danish swivel chair (this is Princeton after all) turned slightly away from the window. His computer screen was directly in my line of vision. If he turned his laptop to obscure my view, his screen would be visible to his associates in the room. All this would be perhaps slightly creepy, but on the whole irrelevant, if it were not for a particular history of student surveillance in the sciences, complete with its own set of stereotypes and mythologies. Hecht once explained to me that organic chemists have the reputation of being the most brutal when it comes to

---

154 I was told that the architect, at the dedication event, explained that this large central space was intended as a sort of street. But the hum of HVAC and the hollow acoustics have their own aesthetic effects which are not so easily curbed.

demanding long hours and making sure that they get them. He recalled a colleague once telling him that his PhD advisor and lab head had expected sixteen hour work days from his graduate students. The colleague in question would show up in the morning, drop off his backpack and go get breakfast. One day, the advisor pulled him aside and said, “Your backpack doesn’t run experiments.”

But the meanings of transparency proliferate beyond the (un)intended consequences of surveillance. The new building is the direct result of close ties between Big Pharma and university-based basic research. Edward Taylor, Professor of chemistry (now emeritus), approached global pharmaceutical giant Eli Lilly with a possible cancer drug a couple of decades ago. This was the beginning of a relationship that has been variously described as productive, close and fruitful in Princeton’s wide circle of student, institutional and alumni publications. The patent for the resulting drug, Alimta, which is exclusively manufactured and marketed by Lilly, belongs to Princeton, and the building had been financed entirely through royalties.

The Lilly license is irrevocable, and exclusive for the lives of the patents. The patents were, in fact, set to expire in 2011, but were extended by the US Patents and Trademark office to 2015, as a result of delayed approval by the FDA. Princeton and Eli Lilly have teamed up as co-plaintiffs on at least three occasions to impede the production of generic and variable versions of the drug. The extension and generics litigation angered critics, who questioned Princeton’s incentives. According to Lilly’s financial statements, the company sold $1.15 billion of Alimta in 2008 alone. Princeton is contractually entitled to royalty payments equaling a “single-digit” percentage of net

---

sales. While Princeton, with true preppy propriety, has declined to discuss its paycheck from the drug, the new building’s price tag was estimated at a whopping $298 million—still far less than the estimated cost of R&D for the drug. Of our new problems, it’s an old one. Drugs are extremely expensive to develop, necessitating work and time and resources and more resources. Big pharma blocks availability through generics or patent infringement in order to maximize profit. But, this line of reasoning holds, profit is what motivates research. There’s a lot more to say about this, and many experts who have and will continue to say it. But what happens when an institution of higher learning—a non-profit committed to the betterment of humanity or some such rhetoric—is a litigant and on the side of Big Pharma?

One result is some awkward architectural transparency that conceals as much as it reveals.¹⁵⁷ Universities combine heterogeneous logics and esthetics, which continue to change over time. In this case, a mix of audit culture and corporate transparency produces such esthetic effects. As I ascended the zigzagging staircase to the third floor, I recognized Hecht lab members across the atrium, through the glass. I waved at Maria and was surprised to find out how quickly she waved back.

Moving, Hecht will tell you, is a drag. He’s done it twice this year: once from home to Forbes College, and once from Frick to Frick. The relocation to New Frick required a fair amount of preparation from lab members. Packing, labeling, more labeling, finishing experiments and cleaning up workspaces, then unpacking in a new lab with different dimensions, different layout, setting up, getting oriented, mixing new compounds and culturing new cells. Weeks of potential research time go by in this way.

¹⁵⁷ Strathern 2000, p. 310.
Furthermore, in Hecht’s lab’s case, the excitement over new fancy facilities was significantly tempered by institutional politics, overt hierarchies and priorities. Hecht’s lab was actually losing space in the move, even though New Frick was designed to accommodate many more benches than old Frick and Hoyt combined. Much of the better real estate (closer to the windows) was intended to attract future faculty, and so, for the time being, was empty. Since their final destination on the 3rd floor was not yet ready for occupancy, Hecht’s lab was put in temporary workspace nearby, scientist squatters, defiantly stretching their workspace beyond the allotted ten benches. “For now,” Betsy showed me, “We’ve taken over this space.” She points to the benches around her, as we start wandering towards the lab meeting.

**Mobility Redux**

At the lab meeting, Sarah presented her most recent round of work that was to soon culminate in her senior thesis. Sarah, I discovered, like many of her colleagues, had been unable to replicate results in the new surroundings. Experiments had just stopped working, experiments whose results had already been published. Sarah’s presentation therefore was a little deflated in tone and uncharacteristically short. She had been running negative controls on CisN, one of the newest confirmed rescues that she had discovered. The negative controls—auxotrophs transformed with a control gene—were not growing, which, under normal circumstances, would be a good sign. Cells in the negative control
are not supposed to grow. But now, Sarah explained, she didn’t trust the controls because the experiments didn’t grow either. What if the controls aren’t growing for the same reasons that the experiments with the lab’s synthetic proteins aren’t growing? Sarah had learned to trust her reliable experiments. They had become a logical baseline for other sorts of expectations in the experimental system. She was convinced by the results to the point where, if they didn’t work, then the controls were highly suspect. A world, turned on its head, no longer produces readable signs.

In one of his more didactic lab meeting moments, Michael explained to the group that in order to do these experiments, you need to have a floor and a ceiling (he drew two dashes on the board, one hip-height, one head-height). The floor is usually provided by the negative control. It is that “death+/−5”. The ceiling is the positive control or the growth that would be expected under “normal” circumstances. Years into the auxotroph research, these were still malleable categories since parts of the experimental system were not yet fleshed out. So, for example, one of the things on the lab’s agenda, was to grow the auxotrophs with their wild type protein—the one that had been deleted—added back in. This would help construct that ceiling. Floors and ceilings are experimental bearings. And in Sarah’s case, the floor fell out from under her.

After the meeting, Maria, Betsy, Michael and I sat down for a quick catch-up in one of the common areas. Seated in a modernist style brand new pea green armchair, Michael, with characteristic humor, explained the current state of anxiety in the lab. He scribbled three names of what I took to be synthetic proteins on a piece of paper, with their distinctive capitalization at the first and the last letter. Lab members, he explained, had thought that CisN and CisD had rescued the auxotrophs. They were now hoping that
it wasn’t “HoyT.” Hoyt, meaning the previous lab building. There was something marvelously paranoid in the kind of thinking this microscopic macrofailure engendered. Was it the air? Was it the water? The cells? Were they damaged? Did something thaw in the move? Something freeze that wasn’t supposed to? Worst of all, what if Hoyt was contaminated, and represented a special environment for growth? What if the water there, the air there was what facilitated growth in sickly auxotrophs, rather than synthetic genes? In an interview, Betsy provided a more detailed account.

**Betsy:** We have these five strains where we have synthetic proteins that rescue them. CisD was a real auxotroph up there [meaning in Hoyt], and [Sarah] got these rescues, and it was all good, and it was a fifth one, yay! And so now we have these five strains. Very replicable that they rescue on minimal media. We come down the hill, Sarah is working with CisD, Charlotte’s working with SerB, Maria is working with GlitA, and I’m sort of doing stuff with all of them, and suddenly we start noticing that we’re not getting rescue where we should, so our positive controls are not growing. And so we’re like, this is unfortunate. I mean because most things are still the same. We still have the same bottle of salt; we still have the same glassware. The one thing that could have been the obvious problem would be the water. The issue would probably have been, because it wasn’t that we were getting extra growth, it’s that we were getting NOT growth, the issue would be more that this water was purer than the water up there, and that there were maybe some small amounts of contaminants up there that were helping it grow. For instance, Michael was thinking that maybe there was a
problem in the pathway to an amino acid, and it’s an amino acid that’s in itself. So it’s almost like a chicken and egg problem thing, where you need a little bit to even make the protein. So he was thinking there may have been a tiny bit of that in there, there was just enough to help it get started and now if we don’t even have that tiny bit, you know, I don’t know…

Talia: So how long did it take for you to notice that there was a problem?

Betsy: Probably a few weeks. Because I mean the first week after we didn’t really do any work or anything, and then we start getting up and you have to remake your plates, you have to remake everything, by the time you’re even doing experiments it’s probably been about two or three weeks, so I would say probably almost a month, honestly. So now it’s probably been two to three weeks since we had a panic attack. So Sarah and Charlotte and I talked about this, and then I decided to take all five strains and just test them.

Talia: So nothing grew? Not even the positive controls? You guys just couldn’t grow anything?

Betsy: Okay, so, with the caveat of I did not include the positive positives, which is the natural protein. And certainly non auxotrophic cells will still grow on minimal media.
**Talia:** Okay, just checking if maybe you have lethal water.

**Betsy:** Right, exactly. Because you can think of that too. I mean, maybe it’s a weird pH, or something. And also things are growing totally fine on rich media. So when they told me this I didn’t want to go panic Michael, it was like a Friday or something, so I plated all these things and by Wednesday group meeting none of them had grown. And they should have grown in five days. So we bring it up at group meeting. We had a whole conversation and raised the possibility of killer phage or something, but that seemed unlikely because our things were growing in rich media, and it should just kill everything. And so, I don’t know, I was literally having a panic attack, because this is kind of what I’m working on right now, so if it doesn’t work, at least my name is not on the paper [laughs]. I’m pretty sure Michael was having more of a panic attack than me. So I decided to test them again, sometimes weird things happen, okay. So I realize I am out of this one reagent, IPTG. So I’ve just used the last bit. So I had made up IPTG when I started, back in March last year and I had finally just run out of it. So I make more, so I have these new plates and I plate… I think I plated all of them again… I did not plate CisD because I didn’t have CisD cells, so I didn’t do that. I did the regular four, and three of them grew. And I’m like, Yes! SerB still did not grow.

**Talia:** Had everybody been using your IPTG?
Betsy: No. So I mean that’s an iffy thing right there, right, because it’s like, that may have been an issue for mine but you know, I mean Sarah started about the same time I did and I think she made up about the same amount of IPTG so I don’t know. So I asked her to check and see how old hers was too. My other hypothesis was possibly that if things thaw during the move that could have been the final straw for it. It’s already really old, and it was doing okay but then it’s thawed and now it’s dead. So that could have been an issue for everybody when we moved. I talked to Icky about it and we were already using a pretty low concentration. So there’s sort of a curve with normal proteins that, if this is IPTG concentration, after a certain point more isn’t really going to help. But where we were using it was already on the slope where a little bit more makes it grow a lot faster, so if we’re already on the slope a little bit less could really be, you know…It’s already so touchy, you know, so it seemed like a possibly reasonable conclusion.

Nonetheless, tensions had eased up a bit by the time of the meeting, Betsy explained. After a gloomy couple of weeks, some of the auxotrophs with their synthetic protein gene grafts had grown. But the first weeks were tense.

At a lunch in Forbes, later in the week, Hecht gave a different account of the mysterious difficulties with experiments. He explained that, second to a death in the family, moving is considered the most stressful activity. He pointed out, in loaded jest, that the *E. coli* in his lab that survived the key experiments experience millions of deaths in the family under *normal* circumstances. On top of that they were suddenly moved! The
cells, like the humans who had just migrated down the hill, were overloaded. It was all too much.

**In the Shadow of Method**

The under-determination of concepts during the experimental process discussed in the previous chapter challenges clearly delineated scientific methods. It therefore raises all sorts of problems for the consistency of experimentation over time, which in turn affects the stability of replication and hypothesis testing. Such hallmarks of scientific method prove difficult to pin down, both because of this inherent conceptual wiggle room, and because replication is both less comprehensively practiced (or rewarded) and contains multiple facets which do not come up in elementary textbooks. The variance of actual lab practice in relation to the methodological ideal of the scientific method has been observed repeatedly by Science and Technology scholars. H. M. Collins’ elegant *Changing Order* presents a case in point. Collins observes that replication is hardly ever practiced in the sciences. He writes,

> Press scientists and in the last resort they will defend the validity of their claims by reference to the repeatability of their observations or the replicability of their experiments. This is usually a matter of their *potential* repeatability…Only in exceptional circumstances is there any reward to be
gained from repeating another’s work…A confirmation, if it is to be worth anything in its own right, must be done in an elegant new way or in a manner that will noticeably advance the state of the art. Thus, though scientists will cite replicability as their reason for adhering to belief in discoveries, they are infrequently uncertain enough to need, or to want, to press this idea to its experimental conclusions. For the vast majority of science replicability is an axiom rather than a matter of practice.\footnote{H. M. Collins, \textit{Changing Order} (London: Sage Publications, 1985) p. 19.}

Within labs, replication is a matter of disciplinary norms, I was told. Chemists, Betsy explained, did all their experiments in duplicate and triplicate. But Collins here refers to replication between different labs. Indeed, during my time in both Ron and Michael’s labs, their attempts to replicate others’ work were limited to experimental systems that could be turned into technologies for achieving something in their own experimental systems. That is, replication of published results from other labs was only practiced with reference to lab techniques that could be built on. And frequently, these replication exercises proved difficult. Especially, when experimental systems require enormous amounts of specialized equipment, bought for and tailored to specific projects. Thus, replication from one lab to the next is a matter of resources and equipment balanced against unfavorable research incentives. Problems with such replication can have many sources (e.g. inexperience, different materials, faulty equipment, special conditions). So for example, a student in Hecht’s lab was trying to replicate a set of experiments whose successful results had been published at another facility. She wanted
to build on these results, and use them as a technique in her own research. She spent months tweaking and tinkering with the system, frustrated at her inability to replicate the results. Finally, she packed her bags and went to visit the lab that had originated the work. She stayed for a month, observing and participating in performing experiments she had failed to replicate at home. The system worked. She then went back to Hecht’s lab. And again, nothing. Finally, after about six months, she figured out the problem: Hecht’s lab’s ultraviolet light was faulty.

The question then arises how reliance on the possibility of replication survives in spite of its rare practical application. This is especially pertinent given the “talking cure” frustrations of science studies scholars who have been puzzled by the rigidity of scientific ideologies which have been seemingly unaffected by observations of inconsistency in practice. Between the science wars and the present, a sentiment seems to have been brewing among science studies scholars that the practical account of science rendered there in great detail hasn’t made a dent in scientists’ thinking about what they do and how they do it. From an anthropological perspective, stomping one’s foot because the native doesn’t listen seems dubious. Sure, today anthropologists and their academic relatives are more and more imbricated in policy discussions frequently concerning their own societies, such that expecting or hoping for an audience in one’s interlocutors is not preposterous. Nonetheless, there may be something to be gained from that more traditional anthropological perspective which seeks to understand relations and relations among relations, rather than transform them in one go. This is hardly an endorsement of another study of the science wars, what they were, and to what they gave way (books and books have been written on the subject). Nor is it a call for the return of a disengaged,
spectatorial stance in which whatever the native does is what natives do. It is in fact premised on an encounter between science studies scholars and scientists in which the frustration of both sides is taken as an anthropological problem. What we learn is that we may need a model of how the idol of method squares with the patterns of practice.

There are two paths we can take to understand rarely invoked methodological pillars better. The first rationalizes their existence according to some logic of practical utility, albeit not the practical utility that furnishes the explicit justification for such practices. For this kind of explanation, I follow Collins’ legal analogy and relate it to the work of some legal scholars. But there is another way of understanding method, one that draws on Durkheim’s rich characterization of religious rites, and dispenses with rationality—it in fact takes the inefficaciousness of rationality as a starting point—in favor of group effervescence and holism.

Collins contends that replication, “in a manner of speaking, is the Supreme Court of the scientific system.” His point is that replication upholds what Robert Merton called “the norms of universality.” “Anybody, irrespective of who or what they are, in principle ought to be able to check for themselves through their own experiments that a scientific claim is valid,” writes Collins. Note Murton’s language: replication upholds norms of universality, rather than buttressing universality itself. Let’s run with the legal analogy. In a famous 1979 contribution to the field of legal theory, Robert Mnookin and Lewis Kornhauser observed that divorce proceedings often took place “in the shadow of the law.” Theirs is an endorsement of such an arrangement in which couples operate astride of legal codes with the hopes of avoiding litigation. Most divorcing couples, they

---

159 Ibid., p. 19.
160 Ibid., p. 19.
observe, never go to court. Nevertheless, Mnookin and Kornhauser argue that the possibility of litigation is crucial to the private negotiations that constitute most divorce proceedings. Thus their aim is to “develop a framework within which to consider how the rules and procedures used in court for adjudicating disputes affect the bargaining process that occurs between divorcing couples outside the courtroom.”\(^\text{161}\) The authors are interested in the specific bargaining positions of divorcing couples and the way possible recourse to the law contributes to strategic considerations. They write, “…the outcome that the law will impose if no agreement is reached gives each parent certain bargaining chips—an endowment of sorts.”\(^\text{162}\) Mnookin and Kornhauser’s formulation makes clear that between following rules and disregarding rules, there are other forms of relations that are adjacent and accountable to a particular set of norms, but separate from them. If we imagine the scientific method as a set of rules by which results may be assessed—as Collins puts it, the Supreme Court of the scientific system—the \textit{possibility} of appealing to replicability, for example, is what gives shape to scientific practice, rather than replication itself. Scientists operate “in the shadow of” the scientific method, aimed at the norm of universality.

Moving away from rationality, the persistence of behavior despite rational assault or disenchantment fits remarkably well with Durkheim’s characterization of the underpinnings of religious practice. For Durkheim, the failure of rationalization is exactly that which allows us to apprehend how religion is acculturated as a set of predispositions.


\(^\text{162}\) Ibid., p. 968.
Like in the case of science, the failure of the “talking cure” opens up other explanatory avenues. Durkheim writes:

Especially among the most cultivated peoples and milieu, we often come upon believers who, while having doubts about the specific power ascribed by dogma to each rite taken separately, nonetheless persist in their religious practice. They are not certain that the details of the prescribed observances can be rationally justified, but they feel that it would be impossible to emancipate themselves from those without falling into moral disarray, from which they recoil. Thus the very fact that faith has lost its intellectual roots among them reveals the profound causes that underlie it. This is why the faithful are in general left indifferent by the facile criticisms that a simplistic rationalism has sometimes leveled against ritual prescriptions. The true justification of religious practices is not in the apparent ends they pursue but in their invisible influence over consciousnesses and their manner of affecting our state of mind.\(^{163}\)

The component parts of method may be subject to rational assault, but the entire complex of “rites” rests on something other than the justification of the parts, and this something other holds the parts in place. Replication, as a such a “rite,” answers to the necessities of a whole network of actions, practices and exercises of proof that authorize and produce knowledge and its norm of universality, with its “invisible influence over

consciousnesses.” That replication plays this role unconvincingly is therefore beside the point. Its practitioners are, in a sense, primed, and they recoil from dismantling normative horizons.

If scientific methods of the kind one finds in textbooks are at best ideals, and at worst formalities, how do practitioners find readable signs in experiments on a daily basis, signs that produce doubt or belief? An answer to this question may lie in the myriad practices which lie outside of formal methods but constitute quotidian lab life. Thus, instead of formal replication, for example, we find diverse forms of repetition that do not fit neatly into textbook classifications. Repetition occurs in different contexts, and cuts across previous tries in interesting ways. As Collins contends, no experiment is convincingly confirmatory if it is an identical copy of a previous experiment. Collins observes that confirmation does not involve making an indistinguishable copy of a primary experiment. In order for a second try to be in some sense confirmatory, some differences have to be introduced between attempts.

For an experiment to be a test of a previous result it must be neither exactly the same nor too different. Take a pair of experiments—one that gives rise to a new result and a subsequent test. If the second experiment is too like the first then it will not ad any confirmatory information. The extreme case where every aspect of the second experiment is literally identical to the first is not even a separate experiment. Under these
circumstances the second experiment would amount to no more than reading the first experimental report for a second time.\textsuperscript{164}

Collins further argues that the greater the difference between the initial success and its predecessors, the more confirmatory the latter attempts will be. Up to a point. If the predecessors are confirmatory by means of “a skeptical fairground gypsy who had generated the confirmatory result by reading the entrails of a goat!” then, says Collins, the confirmation is less convincing.\textsuperscript{165} There’s lots of room for culture (people, not cells) here, but Collins’ point is that confirming a result in the science practiced around us, power dynamics and all, is a matter of a fine balance between copying an initial success and changing things up.

Replication can perhaps be productively subsumed, then, into a whole host of practices which inscribe forms of continuity and discontinuity in the making of scientific knowledge. These practices frequently fall outside of the methodological tenets, outside of reported experimental contexts but do more for belief and doubt than the dues-paying practices which help publish papers and bolster the norm of universality. Discontinuity can be achieved through changes of location, or people, or project details. It can be purposive or accidental, generative or destructive. Continuity may also operate on different levels, from individual practitioners to whole labs. Experimentation is then a context for deploying similarities and differences towards knowledge ends. And we may observe such medleys of continuity at work in the daily lives of lab members, as much as in the more lofty methodological tenets that purportedly bolster or authorize the results of

\textsuperscript{164} Collins, p. 34.
\textsuperscript{165} Ibid, p. 35.
research. Such continuities and discontinuities deeply impact practitioners who live with and among their experimental systems, producing doubt or confidence, and often much frustration, felt in the pit of the stomach, as much as in that proverbial center of rationality, the head. This adds, perhaps, a second dimension to the notion of “the shadow of method,” in which knowledge, belief and doubt are shaped and felt through a spectrum of practices which lie just outside of explicit method.

Where difference or discontinuity therefore occurs regularly and causes regular problems is in one and the same network of credit—where projects change hands from one graduate student to another or labs move from one building to the next. Projects change hands frequently in most labs. Graduate students go on to become postdocs, and postdocs (hopefully) find jobs. New graduate students and postdocs frequently inherit projects in different stages of research from their lab elders, and they do so often with hardly any overlap. There were specific conditions which intensified the effects of project mobility in Hecht and Weiss’ labs. Princeton is an undergraduate research-intensive institution, with Juniors and Seniors completing two major research projects. This means that the circulation of bodies and expertise is a constant factor in the course of research.

A striking feature of Weiss’ account of his lab’s projects was the influence of individual graduate students or postdocs on the progress of research. Like Sarah, the undergraduate in Hecht’s lab, who reached an impasse and then surmounted it with experience and standard setting, each practitioner encounters a project with his or her own senses, skills, and intuitions, along with a set of inherited components. Therefore, when projects changed hands, they didn’t always continue to work, mostly for unknown reasons. Certain projects circulated like hot potatoes, stalling in some hands, flourishing
in others. And at each transition, some skills are passed between practitioners, some modes of work, approaches and techniques, while others are rethought.

In Ron’s lab, there was a cell-cell communication project for mammalian cells. If accomplished, the results of this project would contribute to the success of other projects, since the ability to coordinate action in mammalian cells underlies many other projects in the lab. The project had been launched around 2004 by a graduate student, Malcolm, who had since received his doctorate. He had gotten it to work and had even managed to further optimize it to the point that it was working well. “Almost as well as bacteria,” notes Ron. The project involved some cells sending signals and some receiving signals. The two sides approached separately. Malcolm had managed to get the receiver side working very well (“He’d done it several times”). On the sender side, Malcolm had shown the ability of mammalian cells to synthesize the desired protein. “It wasn’t a lot,” Ron recalled, “but it was enough to get me excited about it.” But after Malcolm left, another graduate student in the lab took over the project, and, so far, had been unable to replicate Malcolm’s results. In fact, the new graduate student on the project wasn’t having any luck with either the receivers or the senders, even though the new graduate student was in touch with Malcolm.

RW: On the receiver side I’m almost shocked that we can’t replicate it. And on the sender side, I wasn’t exactly sure whether it works or not, he hasn’t been able to replicate that either, and so he’s working on it…
The process of transition from one lab member to another with respect to any particular project was a gradual one, Ron explained. First, new members were familiarized with lab techniques through work that would seldom become their primary focus in the lab. Lab members then most often picked up some piece of an existing project. There were exceptions. Saurabh, for example, had started the project he was working on, and Josh was juggling a number of projects, at least one of which he had come up with independently. The rest of the graduate students, however, were working on projects that predated their tenure in the lab. Ernesto had inherited Conway’s game of life from a former lab member. Ting had inherited the Turing patterns project from another graduate student. Ana was working on two projects, both of which had been previously pursued in the lab: a protein toggle switch and a cancer project stemming from iGEM 2007. Yin Xing had picked up the neuronal iGEM project from the preceding summer, and Cil was working on the diabetes project which had originally been hatched for iGEM 2006.

When projects changed hands, Ron explained, they were rarely overhauled or approached in completely new ways. Rather, for the most part, these were chances to introduce smaller changes in circuit connections, choices of genes and so on. Josh’s original project, again, was somewhat of an oddball in this respect. An attempt to achieve cell-cell communication using virus particles, Josh’s project represents a somewhat different configuration since the goal preexisted his project. But this didn’t quite constitute a change of approach, since both approaches were being actively pursued in the lab at the same time. The cancer project, now headed by Ana, represents a more concrete exception, and served as an example of a seemingly fundamental change in approach. Ron recalls having been generally displeased with the approach to the cancer project.
taken by the 2007 iGEM team, which, he noted, had never been modeled. During the summer of 2008 Ron had helped put together an NSF event at Princeton, which I attended, at which a colleague gave a talk which Ron described as “really cool,” high praise from an engineer. The presentation provided the spark for their own new approach to the cancer project. Since then, a graduate student who was working on the project left the lab, a prospective postdoc had figured out some of the DNA sequences and thermodynamics but was waiting for funding to come through in order to be able to join Weiss’ lab, an undergraduate junior had done junior intensive work simulating the new method, and two other undergrads had done some more simulations. Ana, a very recent addition, was working towards the experiments.

Ron’s lab also had a few projects on the back burner, which had been stalled by difficulties and the ever-disruptive circulation of lab personnel. First, Christian’s departure had meant the temporary stalling of one particular project. Ron explains, “One dormant project that we kind of hit a wall on was another cell-cell communication project, but this time secreting proteins. So getting a protein outside a cell and then into another cell. Christian worked on that. And we kind of hit a wall. We actually have been able to demonstrate a lot, I think we demonstrated some secretion and we demonstrated some internalization on the receiver side. And I think Christian was making good progress and then he just left. You know I’m not sure that he would have necessarily been done by now but he definitely was making real progress.”
Back on Track

Back in Hecht’s lab, a couple of weeks after the tense lab meeting that had followed the move, a new grad student presented her work on the auxotrophs project in a lab meeting. By *new*, I mean to the lab, not to the chemistry department. Mia had been a grad student with another group, but had absconded. Her presentation therefore detailed another occasion of routine replication of experiments: competence building. Mia was an organic chemist. She was learning to do experiments in biology in Hecht’s lab by replicating auxotroph experiments and also testing a few new rescue contenders. Mia had managed to replicated experiments and grow auxotrophs with synthetic proteins again after the setbacks of the past month. Hecht stopped her to point out the significance of this result. They were back on track. Lab members already knew this. Being at the bench means having access to gradual transitions, to a scattered and staggered set of experimental developments as they unfold. But Hecht receives messages one step removed from the bench, presented in slides, or in meetings. By the time of this lab meeting, the fact that key experiments seemed to be working again was old news to the bench scientists in Hecht’s lab.

Half way through her presentation, Mia revealed some new results. She had tested a new auxotroph which had not grown with the control gene but did grow with the synthetic library of proteins. Hecht at this point, intervened in a peculiar way, along the lines of something we had discussed once before. He had once told me that he was reassured every time a new lab member was convinced that what they were seeing in the
lab was real, that it really was their proteins that were responsible for the rescue. He described this being convinced like a conversion experience. This was a question of hunches and intuitions and beliefs of people who had gotten their hands dirty in the experiments. Hecht now focused his attentions on Mia’s intuitive sense, and asked her for her gut feeling: Was the new auxotroph being rescued by synthetic genes? She answered in the affirmative, with conviction. Yes, she was convinced by the results. And Hecht made sure to add that he was reassured.

As I walked back to the lab with Betsy and Maria, Betsy explained that the lab was moving again, this time to their permanent home in New Frick. And this time, she was going to move as much as she could herself, and would encourage others to do the same. Indeed, when the move rolled around, everyone was onboard. After the mystery growth freeze and its traumatic effects the lab members took it upon themselves to move their belongings. Of course, this time, they were moving 100 feet.

The first time I visited them in their new space, I didn’t realize the change. Lab space in New Frick is almost indistinguishable. “So when’s the big move?” I asked. “Done. This is it,” answered Betsy.
Conclusion: Anthropology and Emergence Revisited

The concern with temporality pervades work on technology. There are many reasons for such concern, though perhaps dominant among them is an impetus to diagnose transformations. I have, in this thesis, taken up this dubious challenge in relation to a number of topics (e.g. changing teleology and the production of myth) and so to conclude, I would like to problematize the relationship between temporality and technology.

The study of technology frequently produces temporal orders. These orders are based on the premise that the technological shifts of today are producing effects. The task of the observer, then, is to figure out what those effects are through shifts in relations, in practices, in ways of being and of knowing, and to diagnose forms in formation.

Somewhat ironically, this state of heightened temporal differentiation (we are always mid-transformation between yesterday and tomorrow) reproduces the ahistoricity of anthropology’s antecedents and earlier instantiations. A hyperactive concern with transformation makes change the new stasis, such that the contemporary perpetuum mobile of transformation starts to look like a static cultural order. The culture in question is a culture of anthropological knowledge production comfortably dressed on top of an often future-oriented technological culture, which bows to the normative demands of investor relations and grant writing genres. These genres call for ever-greater horizons of
promise and profit. But such future-orientatedness is certainly not the only temporality one meets among scientists and engineers. Thus, for example, Ron Weiss’ vision of a future in which a public uses biological machinery just as they now do computers is one future orientation that serves to ground certain practices and justify approaches along a road to a goal. But just as importantly, Weiss, Hecht, and their colleagues delve into projects which likely bear little impact on tomorrow but are, to them, decidedly “cool,” and produce effects in the register of the tinkerer’s delight: making something, making it work, making it do something, exhibiting control. The rhetoric may be of the “one small step for man…” variety, but the pathos is mostly divorced from the promise of tomorrow.

So to reset the temporal order, we may want to assert that technology is not inherently oriented toward any one temporality. Orienting technology temporally is a way of doing something, or making something in the domain of knowledge (as well as profit). It is not a given, neither for the scientist or engineer, nor for the anthropologist.

If we take this as our starting point, we can make some distinctions among approaches which have made emergence the target for anthropologists interested in technology. What do anthropologists do with temporality and technology? One common orientation, which is associated with Foucauldian circles, and allows the present to be apprehended as a problem, is the history of the present. This requires detailed historical and archival work to supplant ethnographic modes of engagement. The ethnographic mode then supplies the problems which the archival and historical work is meant to account for. Thus, Rabinow writes the following characterization of the history of the present: “Minimally, one can say that it is (a) diagnostic of a current problem, (b) primarily genealogical in its elaboration, (c) not focused in its substantive discussion on
contemporary instances. The history of the present is neither properly historical, nor sociological nor ethnographic.” There is much strong scholarship of this sort coming from trained anthropologists. The problem is that, if we insist on tying anthropology to fieldwork and the ethnographic present, this work is, for better or worse, somewhere on the fringe of the discipline. Fieldwork for such scholarship confirms the existence of problems in the present. It does not supply the interpretive key. If anything, it obscures it, as the repressive hypothesis, for Foucault, obscured the proliferation of sexual discourse in the 19th century, in *The History of Sexuality V. 1*. The same sort of problem arises, for Marxist theorists, so that, Carol Greenhouse writes, “…[W]hile Marxist and Foucauldian theorists argued about the nature of state power, they framed the question… as an interpretive problem arising from beyond the immediate ethnographic milieu—its putative non-appearance at the level of description belying its urgency.”

Where an orientation to the future has figured most dominantly for anthropologists is in claims to relevance through the peremptory and preparatory. The results have entangled epistemological and political impasses, confusing an already confused set of constraints. That is, there are some major problems with notions of relevance tied to policy and the public sphere, in which anthropological insights are to produce value added beyond academic circles. The terms of such relevance merge with the discourse of governance, producing a double bind for anthropologists: reinforce the terms or stay “irrelevant,” limiting your reach to your disciplinary community, a

---

disciplinary community that is itself continually undergoing a crisis of relevance.\textsuperscript{168} Hence, anthropologists either take up the “social consequences” line of work, which is meant to isolate “risk” and promote “preparedness,” fight for new languages and new framings, or stay at home in their intellectual circles.\textsuperscript{169}

Novelty as difference, as discussed in the Introduction, provides us with another way of working with time. Its orientation is in an important sense the present and the past, but it achieves this orientation by operationalizing the future and the possible. So, for example, in my work, transformations prove useful to think with about the taken for granted modes of being yesterday and today. This falls in line with Rabinow’s imperative to figure out what difference today makes with reference to yesterday and tomorrow. Biology, evolutionary theory, contemporary mythology, scientific religiosity, these are all domains which become thinkable against the backdrop of possible futures and contemporary transformations. But those futures and transformations, as I’ve taken them up, are components of an \textit{imaginary world}. This approach leans on observation and involves a contingent relation to the futures it posits. Futures, ambitions, dreams and aspirations become heuristics for thinking about difference, rather than concrete predictions of futures in the making, possible worlds whose difference, in relation to

\textsuperscript{168} Carol Greenhouse points out that these dynamics were part of the reason that writings on race were conspicuously absent from American ethnography in the 1990s. Race had been discredited, and euphemized with “ethnicity” and even “culture.” But this created a vacuum where “the specificity of the connection between race and the U.S. state,” remained largely unaddressed (Greenhouse 2011: 42).

\textsuperscript{169} This imperative to deploy the public discourse or stay quiet is sometimes reinforced within field sites. Early on in my fieldwork I tried to understand some of the work being done around me in terms of poetics, a concept that circulates widely in the humanities, and also came to prominence in anthropological works of the 90s. I attempted to explain the notion of engineering poetics to one of my interlocutors, who got a mean case of the giggles at the very mention of the word in relation to her work.
today, makes today intelligible. These possible worlds are made. They themselves are the result of *bricolage* where elements found in the present could be recomposed in a number of different ways.
Bibliography

Andrianantoandro, Ernesto, Subhayu Basu, David K. Karig and Ron Weiss

Angier, Natalie

Baker, Monya

Barthes, Roland

Benedict, Ruth

Bethell, Tom

Boellstorff, Tom

Boon, James A.
1983 Other Tribes, Other Scribes: Symbolic Anthropology in the Comparative Study of Cultures, Histories, Religions and Texts. Cambridge: Cambridge University Press.

Borneman, John, and Abdellah Hammoudi

Breithaupt, Holger
2006 The Engineer’s Approach to Biology. EMBO Reports. 7(1): 21-23.
Brenner, Steven A. and A. Michael Sismour

Brettell, Caroline B.

Butkus, Ben

Canguilhem, Georges

Carnap, Rudolf

Clifford, James

Collins, H. M.

Collins, James J., Drew Endy, Clyde A. Hutchison III and Richard J. Roberts

Coyne, Jerry

Creager, Angela N. H., Elizabeth Lunbeck, and M. Norton Wise.

Darwin, Charles

Daston, Lorraine, and Peter Galison
Dawkins, Richard  

De Lorenzo, Victor  

Derrida, Jaques  

Dreyfus, Hubert L. and Paul Rabinow  

Durkheim, Emile  

Elowitz, Michael and Wendell A. Lim  

Emmeche, Claus  

Evans-Pritchard, E. E.  

Federal Register, Presidential Documents  

Feyerabend, Paul  

Fisher, Michael A., Kara McKinley, L. H. Bradley, S. R. Viola and Michael Hecht  
2011  De Novo Designed Proteins From a Library of Artificial Sequences Function in Escherichia Coli and Enable Cell Growth. PloS One. 6(1)

Fleck, Ludwik  
Foucault, Michel

Franklin, Sarah and Margaret Lock

Fritz, Brian R., Laura E. Timmerman, Nichole M. Daringer et. al.

Galison, Peter

Geertz, Clifford

Gerchman, Yoram and Ron Weiss

Gershon, Ilana and Janelle S. Taylor
Gooding, David  

Gould, Stephen Jay  

Gould, Stephen Jay, and R. C. Lewontin  

Greenhouse, Carol  

Hacking, Ian  

Hammoudi, Abdellah  

Handler, Richard  

Haraway, Donna J.  
Harper, Matthew

Hayden, Erika Check

Hayden, Erika Check and Heidi Ledford

Helmreich, Stefan

Hilgartner, Stephen

Hobsbawm, Eric

Hopkin, Karen

Jacob, François

Jaffe, Alexandra

Keim, Brandon

Keller, Evelyn Fox

Kelty, Christopher M.


Khalil, Ahmad S., and James J. Collins

Kuhn, Thomas S.

Kwok, Roberta

Landecker, Hannah

Latour, Bruno
1993  We Have Never Been Modern. Cambridge: Harvard University Press.

Latour, Bruno and Steve Woolgar
1986  Laboratory Life: The Construction of Scientific Facts

Law, John and Annemarie Mol

Lederman, Rena

Lévi-Strauss, Claude
1985  The View from Afar. Chicago: Chicago University Press.
Liang, Jing, Yunzi Luo, and Huimin Zhao
2011 Synthetic Biology, Putting the Synthesis into Biology. Systems Biology and Medicine (January/February 2011) v.3.

Lock, Margaret,

Lu, Timothy K.

Lu, Ting, David Karig and Ron Weiss.

Madrigal, Alexis

Malinowski, Bronislaw

Marcus, George E.

Martin, Emily
1995 Flexible Bodies: The Role of Immunity in American Culture from the Days of Polio to the Age of AIDS. Boston: Beacon Press.

Miyazaki, Hirokazu and Annelise Riles

Mnookin, Robert H., and Lewis Kornhauser
Mooallem, John

Moya, Andres, Natalio Krasnogor, Juli Pereto and Mparo Latorre

Mukherji, Shankar and Alexander Van Oudenaarden

Pálsson, Gísli

Pauly, Philip J.

Pickering, Andrew

Porcar, Manuel

Potthast, Thomas

Povine, William B.

Purnick, Priscilla E. M., and Ron Weiss
Quine, W. V.

Rabinow, Paul

Rabinow, Paul, and Gaymon Bennett
2011 Designing Human Practices: An Experiment with Synthetic Biology. (Forthcoming).

Rabinow, Paul and Talia Dan-Cohen

Rabinow, Paul and George Marcus, with James D. Faubion and Tobias Rees

Rapp, Rayna

Regis, Ed

Rheinberger, Hans-Jörg

Rheinberger, Hans-Jörg, and Jean-Paul Gaudillière
Riles, Annelise  

Rothschild, Lynn J.  

Ruse, Michael  

Sahlins, Marshall  

Shanahan, Timothy  

Shapin, Steven  

Skerker, Jeffrey, Julius Lucks, and Adam Arkin  

Strathern, Marilyn  

Swaby, Rachel  

Venter, J. Craig  

Wade, Nicolas  
Weber, Max

Zhang, Hong, and Taijiao Jiang

Zimmer, Carl